

**The Wired Wilderness:
Electronic Surveillance and Environmental Values in Wildlife Biology**

by

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M.A., Psychology
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Submitted to the Program in Science, Technology and Society
in Partial Fulfillment of the Requirements for the Degree of

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Abstract

In the second half of the twentieth century, American wildlife biologists incorporated Cold War-era surveillance technologies into their practices in order to render wild animals and their habitats legible and manageable. One of the most important of these was wildlife radio-tracking, in which collars and tags containing miniature transmitters were used to locate individual animals in the field. In addition to producing new ecological insights, radio-tracking served as a site where relationships among scientists, animals, hunters, animal rights activists, environmentalists, and others involved in wildlife conservation could be embodied and contested. While scholars have tended to interpret surveillance technologies in terms of the extension of human control over nature and society, I show how technological, biological, and ecological factors made such control fragmentary and open to reappropriation. Wildlife radio-tracking created vulnerabilities as well as capabilities; it provided opportunities for connection as well as for control. I begin by showing how biologists in Minnesota and Illinois in the early 1960s used radio-tracking to establish intimate, technologically-mediated, situated relationships with game animals such as ruffed grouse, which they hoped would bolster their authority vis-à-vis recreational hunters. I then show how the technique was contested by environmentalists when biologists applied it to iconic “wilderness wildlife” such as grizzly bears in Yellowstone National Park in the 1960s and 1970s. One way for biologists to render radio-tracking acceptable in the face of such opposition was to emphasize its continuity with traditional practices, as they did in a radio-tagging study of tigers in Nepal in the 1970s. Another way was to shift to less invasive techniques of remote sensing, such as the bioacoustic surveys of bowhead whales off Alaska's Arctic coast that were conducted in the 1980s after a proposal to radio-tag whales was rejected by marine mammalogists and Iñupiat whalers. Finally, wildlife biologists could reframe radio-tracking as a means for popular connection rather than expert control, as they did by broadcasting the locations of satellite-tagged albatrosses to schoolchildren, gamblers, and the general public via the Internet in the 1990s and early 2000s.

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INTRODUCTION THE POLITICS OF SURVEILLANCE IN WILDLIFE BIOLOGY

On a humid evening in the summer of 2006, I joined a small team of researchers to search for a locally threatened species of turtle in the Great Meadows National Wildlife Refuge, which stretches along twelve miles of the Concord and Sudbury Rivers in Boston's northwestern suburbs. Several years earlier, the town of Concord, concerned by evidence that the Great Meadows population of Blanding's turtles had declined dramatically since the 1970s, had hired a local environmental consulting company to determine the cause of the decline and to protect the remaining turtles from both human and nonhuman threats. During the height of the nesting season, which was then just coming to an end, field assistants working for the company had carefully monitored the turtles' use of the habitat, recorded causes of mortality, and set up protective fences around nests. The amount of resources invested in the project were small, but its existence reflected an important shift that had taken place in the practice of wildlife conservation in the United States since the 1940s, when the refuge had been established on the site of a private duck hunting reserve. While many of the wildlife managers who administered the Fish and Wildlife Service's national network of refuges continued to be oriented toward the mission of protecting and restore hunting opportunities that motivated their predecessors in the mid-twentieth century, they were now also concerned with protecting animals such as the Blanding's that few hunters had ever dreamed of pursuing. The Service shared this new project of protecting endangered species, which came to prominence in the 1970s, with a variety of other federal, state, local, nongovernmental, and international institutions, including the town's Natural

Resources Commission and the nonprofit Concord Land Preservation Trust. Fifty years earlier, few people had known or cared about the Blanding's population of Great Meadows; now it was the focus of special town committees and a candidate for listing under the federal Endangered Species Act.

The broad transformation of environmental attitudes that the attention to this relatively nondescript animal represents has been the subject of a great deal of scholarship in environmental history, but another, arguably equally important transformation that accompanied it has been largely neglected. Between 1944, when the Great Meadows National Wildlife Refuge was established, and 2006, when my visit took place, wildlife biologists and managers partnered with engineers to develop an array of new surveillance technologies, many of them adapted from military applications, for the purpose of monitoring and managing populations of wild animals. One such technology, wildlife radio-tracking, was the methodological foundation of the research being conducted on the Great Meadows Blanding's population and the reason that I had joined the team for an evening. By attaching miniature radio transmitters to the turtles' shells, each set to a unique frequency, the researchers had transformed these well-camouflaged animals into individuals who could be located and identified through thick brush from hundreds of yards away. Radio-tracking not only made it possible to construct detailed maps of the animals' movements throughout their Great Meadows refuge, but also allowed researchers to intervene when those movements threatened the turtles' survival. Breeding-age females who traveled away from the wetlands at the heart of the refuge in search of dry land in which to dig their nests were at risk because those movements often brought them to the heavily-trafficked roads at the refuge's borders. Radio-tracking made it possible to relocate turtles who were dangerously close

the roads and to protect their nests once the eggs had been laid. Since the invention of radio-tracking around 1960, it has become essential to many similar projects to understand, manage, and conserve populations of wild animals around the world. Radio-tracking is one of the technical practices in which environmental ideals have been embodied, contested, and transformed.

Despite the importance of radio-tracking and other field practices to the work of wildlife biologists and managers and the broader project of wildlife conservation of which they are a part, such practices have received little attention from historians and scholars of science and technology studies. To the extent that radio-tracking has been studied, it has been understood either as a symbol of the ironies of modern environmentalism or as a tool of legibility, surveillance, discipline, and control. The environmental historian Thomas Dunlap, for example, concludes his history of the twentieth-century shift from predator control to predator protection in the United States with the image of a radio-collared wolf. “That science, a complex result of a sophisticated culture, guides our appreciation of the primitive is ironic but not surprising,” he writes. “Americans are awed by science, have a passion for gadgets, and are deeply nostalgic about the frontier past. We have as a society often turned to modern means to preserve old virtues and values. The wolf with the radio collar, providing data for scientists to use in reestablishing the primitive ecosystems of North America, may be the perfect symbol of our efforts to come to terms with our knowledge of nature's order, our power over it, and our need to preserve our mythic past.”¹ Similarly, John MacKenzie ends his study of hunting and wildlife conservation in the British Empire with the image of wildlife radio-tagging as simultaneously the culmination

1 Thomas R. Dunlap, *Saving America's Wildlife: Ecology and the American Mind, 1850-1990* (Princeton: Princeton University Press, 1991), 176.

and inversion of colonial hunting practices—an irony epitomized by the Duke of Edinburgh's visit to Nepal in 1986, where twenty-five years earlier he had participated in a tiger hunt. “In that year the queen and the Duke again visited Nepal,” MacKenzie writes, “but the nearest they came to big-game shooting was to witness the tranquillising of a rhino called 'Philip' so that it could be fitted with a radio device. Here was the perfect symbol for the replacement of the hunting by the conservation ethos, imperial power by post-colonial environmental concerns.”²

While Dunlap and MacKenzie are right to note the ironies of radio-tagging as it was practiced in the 1980s, by focusing on the symbolism of a well-developed practice they missed an opportunity to tell a more complex and contingent history. As I show in this dissertation, “perfect symbols” of the ironies of modern conservation were made, not born. Radio-tracking in the form that Dunlap and MacKenzie noted it in the late 1980s was the result of several decades of contestation in which its ultimate meaning—what exactly it would serve as the “perfect symbol” for—had been up for grabs.³

Other scholars have understood wildlife radio-tracking primarily as a means of extending the techniques of discipline and legibility that Michel Foucault, James Scott, and others have described for human societies into nature, the wilderness, or the wild.⁴ From this perspective, there is nothing particularly ironic about wildlife radio-tracking; it simply represents the logical extreme of a project of rationalizing the world that has been underway for at least two centuries. Tools that were used to transform diverse forests into monocultures in the late eighteenth century

2 John MacKenzie, *The Empire of Nature: Hunting, Conservation, and British Imperialism* (New York: Manchester University Press, 1988), 310.

3 As Donna Haraway writes, “Natural-technical objects of knowledge are contested; they are the product of social engagement, in and out of the perimeters of science”; Donna J. Haraway, *Primate Visions: Gender, Race, and Nature in the World of Modern Science* (New York: Routledge, 1989), 111.

4 Michel Foucault, *Discipline and Punish: The Birth of the Prison* (New York: Pantheon Books, 1977); James C. Scott, *Seeing Like a State: How Certain Schemes to Improve the Human Condition Have Failed* (New Haven: Yale University Press, 1998).

were, by the end of the twentieth century, being used to maintain and restore carefully managed ersatz wildernesses. Radio-tracking gave wildlife biologists and managers the same kind of control over their often-elusive, mobile subjects that surveys and techniques for estimating board-feet had given scientific foresters over their more stationary subjects or that censuses had given states over their human subjects. For environmentalist writers such as Charles Bergman, the continuity of radio-tracking with this long history of rationalizing nature and society was a primary reason for opposing it: “The methods we have used to save endangered species must fail,” he argued in *Wild Echoes* in 1990, “because the scientific approach to animals is part of the cultural mentality that created endangered species.”⁵ Similarly, Donald Worster, although he does not write specifically about radio-tracking in *Nature's Economy*, distinguishes between “Arcadian” and “imperial” traditions of ecological thinking, the former characterized in part by “direct contact with the living organism in its natural environment,” the latter by “extensive reliance on elaborate instruments of measurement.”⁶ In *Primate Visions*, Donna Haraway describes biotelemetry as enacting a “high-tech narrative of remote control” that drew on Cold War military ideologies of communications, control, command, and intelligence and transformed the relationship between scientist and animal into a problem of coding.⁷ For Gregg Mitman, wildlife telemetry projects in Yellowstone National Park and the Upper Midwest in the 1960s reflected these Cold War, cybernetic ideologies as well as a “transcendent vision” of nature as an object of study, management, and re-creation by an invisible, all-knowing wildlife biologist.⁸

5 Charles Bergman, *Wild Echoes: Encounters with the Most Endangered Animals in North America* (Anchorage: Alaska Northwest Books, 1990), 82.

6 Donald Worster, *Nature's Economy: A History of Ecological Ideas* (New York: Cambridge University Press, 1994), 18.

7 Haraway, *Primate Visions*, 108.

8 Gregg Mitman, “When Nature Is the Zoo: Vision and Power in the Art and Science of Natural History,” *Osiris* 11 (1996): 117-43.

Michael Lewis, writing about the resistance to the use of wildlife radio-tagging in India, pointed out that the technique's association with Cold War military and intelligence work was both real and easily overestimated by the popular press, but in either case it was tied sufficiently closely to fears about American surveillance of the human residents of India to render its use extremely controversial.⁹

All of these scholars rightly highlight the links between radio-tracking and projects of social and natural control, but in doing so they tend to ignore the other meanings that the technology embodied as well as to exaggerate the extent to which it was actually able to produce the total surveillance and control that putatively motivated its use. Like Scott, who argues that, even though scientific forestry was incapable of achieving its goal of total rationalization, “the critical fact is that it did partly succeed in stamping the actual forest with the imprint of its designs,” these scholars have tended to attribute to radio-tracking an abstract essence of surveillance and control that remains the “critical fact” about the technology even if, in practice, it was often contested and never achieved.¹⁰ As I show in this dissertation, the fact that total control was unachievable for both political and technical reasons was an equally “critical fact” about the use of surveillance technologies in wildlife conservation. Radio-tagging could be and was contested and appropriated for other purposes.

Not all technologies are equally available for contestation and appropriation. The contestability and appropriability of radio-tracking depended primarily on two factors: the requirement that it be embodied in a particular material form and the fact that it was most often deployed in field sites and on animals to which scientists did not have exclusive claim. Radio

⁹ Michael Lewis, *Inventing Global Ecology: Tracking the Biodiversity Ideal in India, 1945-1997* (Hyderabad: Orient Longman, 2003), 107.

¹⁰ Scott, *Seeing Like a State*, 19.

collars and tags were much more than abstract means of making animals identifiable and locatable. They were assemblages of transistors, circuits, batteries and solar panels, wires and antennas, and straps of leather, rubber, or cloth that had to be physically attached to a wide variety of animal bodies, almost always under field conditions that lacked the comfort and convenience of the laboratory. Similarly, radio waves were much more than an abstract signal of location; they were a physical form of energy that bounced and diffracted in confusing ways across the landscape. The materiality of the technique forced scientists to become “intimately acquainted,” as one of the first developers of the technique put it in the mid-1960s, with individual animals and the landscapes they inhabited. It also made them vulnerable to outside critique and intervention; although only properly-equipped scientists could detect the radio signals emitted by the tags, anyone could see, photograph, or even remove the assemblage of physical materials that made such signals possible. Many radio-tagged animals have been legally shot by hunters; many others have been photographed by tourists, not all of whom have appreciated their juxtaposition of primitive nature and industrial civilization. Such encounters between non-scientists and materially embodied technologies of legibility were especially likely because wildlife biologists usually conducted their research in national parks, forests, wildlife reserves, and other public lands that were open to alternative uses ranging from hiking and nature photography to logging and subsistence hunting. As Robert Kohler writes, “the social complexity of nature is a mixed blessing. It makes field work a more diversely meaningful experience than work in labs But social diversity also compromises field biologists' credibility and social standing.”¹¹ Unlike the early and mid-twentieth century ecologists and evolutionary biologists whose “practices of place” Kohler describes, however, wildlife biologists did not have the

11 Robert E. Kohler, “Place and Practice in Field Biology,” *History of Science* 40 (2002): 189-210, on 193.

flexibility of choosing the research site that would best answer their theoretical questions. They had to create what might be called “practices of mobility” that allowed them to establish credible knowledge about animals who moved unpredictably across diverse landscapes and whose proper management or conservation hinged on the results of their research.

While doing so, wildlife biologists also had to grapple with the changing institutional, legal, and cultural context for their work in the second half of the twentieth century. Wildlife management emerged in the United States in the 1930s as an alliance among wildlife managers working for state and federal government agencies, academic biologists mostly employed by the large midwestern land-grant universities, and recreational hunters and the arms and equipment industries who supplied them.¹² The development of various new surveillance technologies during World War II and the early Cold War offered wildlife biologists a means of bolstering their epistemological authority vis-à-vis both frontline managers and hunting interests. To adapt these techniques to their purposes, they had to go beyond existing institutional arrangements, such as the limited funds available through the federal government's excise tax on sporting arms and ammunition, and attract support from Cold War organizations such as the Office of Naval Research and the Atomic Energy Commission. By the late 1960s, however, the popular environmental movement had begun to challenge wildlife biologists' focus on huntable species of “game,” the antiwar movement and a federal budget crunch had begun to undermine the appeal and availability of military support, and the animal rights movement had begun to

12 On the origins of wildlife management, see Christian C. Young, *In the Absence of Predators: Conservation and Controversy on the Kaibab Plateau* (Lincoln: University of Nebraska Press, 2002); Thomas R. Dunlap, *Saving America's Wildlife: Ecology and the American Mind, 1850-1990* (Princeton: Princeton University Press, 1991); Susan Flader, *Thinking Like a Mountain: Aldo Leopold and the Evolution of an Ecological Attitude toward Deer, Wolves, and Forests* (Madison: University of Wisconsin Press, 1994); Curt Meine, *Aldo Leopold: His Life and Work* (Madison: University of Wisconsin Press, 1988); James Trefethen, *An American Crusade for Wildlife* (New York: Winchester Press and the Boone and Crockett Club, 1975).

challenge the use of invasive research techniques. Having successfully appropriated Cold War surveillance technologies for the purpose of producing abundant game populations in the context of professional infrastructure designed in the 1930s, wildlife biologists now found themselves in a different and much more contentious political environment. In this dissertation I show how changes in field practice were a central part of their response to these challenges. Specifically, I argue that challenges to radio-tagging inspired several strategies of adaptation. These included efforts to hybridize radio-tracking with traditional forms of interaction with wild animals, the increasing use of noninvasive but pervasive means of wildlife surveillance, and a reframing of radio-tagging as a means of public connection rather than as a tool of expert control. All of these strategies were part of what Peter Alagona has described as the successful wedding of “environmentalists' concern for wild nature with scientific respectability and technological expertise” by the new field of conservation biology in the 1980s.¹³

During an evening of searching in the Great Meadows National Wildlife Refuge, I never saw a Blanding's turtle, but I heard one on the radio. After a brief walk along the trails during which we relied on nothing but our eyes to find turtles, without success, we stopped near the municipal wastewater treatment facility near the border of the refuge to try to detect some of the tagged turtles using the radio-tracking gear. At first, holding a receiver tuned to the frequency of one of the tags in my hand and turning in a circle with the antenna pointed toward the horizon, I picked up nothing but the whine of interference coming from the direction of the Hanscom Air Force Base, home of the Electronic Systems Center, whose mission is to provide the Air Force with “the latest in command and control and information systems.”¹⁴ In the midst of a nature

13 Peter S. Alagona, “Biography of A 'Feathered Pig': The California Condor Conservation Controversy,” *Journal of the History of Biology* 37 (2004): 557-83.

14 Hanscom Air Force Base, “Units,” <http://www.hanscom.af.mil/units/>, retrieved on 22 April 2008.

reserve, just a few miles from where Henry David Thoreau had lived in a one-room shack a century and a half earlier, reminders of the Cold War's ongoing legacies proved impossible to ignore.¹⁵ Because of the complex landscapes in which it is used and the variety of situations into which tagged animals get themselves, radio-tracking is heavily dependent on local, practical knowledge. At the recommendation of one of the researchers I turned the Yagi antenna ninety degrees so that its tines were perpendicular to the ground instead of parallel to it; now, with the antenna pointed toward the large pond at the heart of the refuge, I could hear a faint beep through the static. The researchers noted the location down and moved on to the next frequency. During an evening of searching amidst the sounds of buzzing insects, the hum of distant traffic, and the crackling static of the radio-tracking receiver, the sound of that beep was the only evidence I had that Great Meadows still harbored the Blanding's turtle.¹⁶ Later, as we walked along trails,

15 Although Thoreau was familiar with the Blanding's turtle, the species was relatively uncommon in Concord even in his day; see R. Heber Howe, Jr., "The Second Record for Blanding's Turtle in Concord, Mass.," *Science* 34 (1911): 272. After killing one for study, which he later donated to the Boston Society of Natural History, Thoreau wrote in his journal for August 18, 1954: "I cannot excuse myself for this murder, and see that such actions are inconsistent with the poetic perception, however they may serve science, and will affect the quality of my observations. I pray that I may walk more innocently and serenely through nature"; Bradford Torrey and F. B. Sanborn, eds., *The Writings of Henry David Thoreau* (Boston: Houghton, Mifflin and Company, 1906), on 452. On this passage of Thoreau's journal and his attitude toward hunting in general, see Thomas L. Altherr and John F. Reiger, "Academic Historians and Hunting: A Call for More and Better Scholarship," *Environmental History Review* 19 (1995): 39-56; Thomas L. Altherr, "Chaplain to the Hunters': Henry David Thoreau's Ambivalence Toward Hunting," *American Literature* 56 (1984): 345-361. Thoreau's negative reaction was clearly to the death of the individual animal and its impact on his own sensibility rather than to the nonexistent threat to the species. As Donald Worster writes, Thoreau, unlike late-nineteenth-century conservationists who were concerned with the efficient utilization of nature's limited resources, was "exhilarated at the prospect of excess and rebelled against the idea of a stingy natural order"; Donald Worster, *Nature's Economy: A History of Ecological Ideas Studies in Environment and History* (New York: Cambridge University Press, 1994), on 64. On Thoreau's place in American literature, see Leo Marx, *The Machine in the Garden: Technology and the Pastoral Ideal in America* (New York: Oxford University Press, 1964). On his ideas about environment and conservation, see Lawrence Buell, *The Environmental Imagination: Thoreau, Nature Writing, and the Formation of American Culture* (Cambridge, MA: Harvard University Press, 1995); Roderick Nash, *Wilderness and the American Mind* (New Haven: Yale University Press, 1967). Brian Donahue has argued that the deforested landscape that Thoreau encountered was the product of early-nineteenth century economic development, which displaced more sustainable mixed agricultural systems of the colonial period and early republic; Brian Donahue, *The Great Meadow: Farmers and the Land in Colonial Concord* (New Haven: Yale University Press, 2004)

16 I am grateful to Timothy Beaulieu for inviting me to accompany himself and several other members of the research team in Great Meadows. The radio-tagging study is described in a report to the U.S. Fish and Wildlife Service by Bryan Windmiller and Ian Ives: "Conservation and Management of the Great Meadows Population of Blanding's Turtles, (*Emys blandingii*), in Concord, Massachusetts: Year-End Report on Accomplishments and

scrambled up dirt piles, and squelched through marshland in a fruitless search for additional turtles, one of the researchers mentioned with frustration a local woman who had recently discovered a radio-tagged turtle along one of the trails. Believing the sluggish turtle to be in distress, the woman had removed the tag and turned it in to refuge authorities. Nearly fifty years after the invention of radio-tagging, the materiality of the technique continues to provide opportunities for non-scientists to challenge the work of biologists in the field.

Outline of the Argument

In Chapter 2, I show how a group of wildlife biologists and engineers in Minnesota and Illinois developed wildlife tracking into a workable field technique in the late 1950s and early 1960s. Though these biologists were not the only ones working with the technique, they were among the first to deploy radio tags on animals in the field and to publish designs for tags that were widely emulated, and they played important roles in developing the technical and institutional frameworks within which the invention of radio-tagging took place. I argue in this chapter that one of the primary motivations for transforming the technique from an experimental curiosity to a routine tool was to bolster wildlife biologists' epistemological authority in the face of challenges from recreational hunters, who remained skeptical of this young profession's ability to improve hunting opportunities. I further argue that, although some of these biologists were initially motivated to pursue the technique by ideals of transcendence and mechanical objectivity, they soon shifted their focus to the intimate, technologically-mediated, locally-situated relationships that radio-tagging both enabled and required. The University of Minnesota wildlife

Activities During the 2005 Field Season With Proposed Objectives and Activities for 2006," dated April 18, 2006.

biologist William Marshall, for example, justified his initial application to the National Science Foundation in 1959 for funding to track ruffed grouse in northern Minnesota by claiming that existing techniques were “antiquated” in light of recent advances in the physical sciences and that radio-tagging would allow scientists to collect objective data by removing themselves from direct contact with the animals they studied. Five years later, however, he was describing the project in very different terms. In his final report to the National Science Foundation, he explained that the radio-tagging system “has one basic attribute. The biologist becomes, in a very real sense, intimately acquainted with the animal carrying the transmitter and also with the habitat it is occupying, as the work proceeds. Thus, he can do a great deal of qualitative interpretation on the spot adding to his understanding of the complex conditions encountered in the field.”¹⁷ While the National Science Foundation review panel was not impressed by the “intimate,” “qualitative,” and “complex” nature of the knowledge that radio-tagging produced, it was these qualities that helped wildlife biologists achieve what was arguably their primary goal: a privileged and exclusive relationship to wild animals.

In Chapter 3, I show how this exclusive new form of relationship met its first serious resistance when wildlife biologists attempted to enact it in national parks and wilderness areas. I argue that the materiality of radio-tagging and its use in field sites upon which non-scientists had legal, economic, and cultural claims produced opportunities for non-scientists to gain a stronger voice in the practice of wildlife biology. In the early 1960s, National Park Service administrators encouraged wildlife biologists to conduct research in Yellowstone National Park in the hope that novel techniques such as radio-tagging would help resolve problems of ungulate overpopulation

17 William H. Marshall, “Final Report—Development and Use of Radio Telemetry Techniques to Study Ruffed Grouse” (1967), Box 5, William H. Marshall Papers, 1947-1982, University of Minnesota Archives, Elmer L. Andersen Library, Minneapolis, MN.

and the increasingly frequent and dangerous interactions between humans and bears. But by the late 1960s, in the context of a wilderness movement that idealized spaces where human influences were invisible and a Park Service that was increasingly oriented toward reproducing “vignettes of primitive America,” radio-tagging came to seem more like an unwanted intrusion of industrialized society than like a method for preserving the nation's frontier past. When wildlife biologists Frank and John Craighead clashed with the National Park Service over the management of Yellowstone's grizzly population in the late 1960s, their use of the modern, hands-on techniques of wildlife biology, especially radio-tagging, proved to be as much a political vulnerability as a source of epistemological authority. Radio-tracking allowed wildlife biologists to claim an unequalled understanding of the movements and behaviors of Yellowstone's grizzlies, but the fact that it depended on attaching highly visible collars and tags to the animals provided opportunities for non-scientists to challenge their work, especially as the popular environmental movement gained steam over the course of the 1960s. The Yellowstone administration, with the support of influential wilderness activists, used the tags as both justification and means for terminating the Craigheads' research in the park. As Superintendent Jack Anderson told John Craighead, even if only one percent of Yellowstone's visitors saw a marked bear, there would still be twenty-five thousand people each year who were “short-changed in seeing the grizzly as it occurs naturally.”¹⁸ After Anderson instructed Yellowstone's rangers to remove the tags whenever possible, the Craigheads' privileged, intimate, technologically-mediated relationship to the park's grizzlies began to vanish, tag by tag. Although the Craigheads were forced to conclude their research on grizzlies in Yellowstone in

18 Jack K. Anderson to John J. Craighead, 9 February 1971, Box N-112, Natural and Social Sciences Records, Yellowstone National Park, Heritage and Research Center, Gardiner, MT.

the early 1970s, the radio-tagging techniques they had pioneered were soon back in use. After Yellowstone's grizzlies were designated as “threatened” under the Endangered Species Act in 1975, it became politically difficult for the Park Service to prohibit the use of the most advanced technique available for preventing the extinction of the population. The key difference between the use of the technique by the Interagency Grizzly Bear Study Team in the 1970s and its use by the Craigheads in the 1960s was that the Park Service now had a much stronger voice in discussions of exactly how, when, and where it could be used. Wildlife biologists were no longer able to monopolize the intimate relationships that radio-tagging made possible.

In Chapter 4, I show how American and Nepalese wildlife biologists used radio-tagging to conduct an influential study of tiger behavior in Royal Chitwan National Park in the 1970s, despite the opposition of European conservationists and an ecotourism company operating in the park. In India, where American researchers had initially hoped to carry out the study, the same sources of opposition had combined with a stronger and more nationalistic forest service to prevent the study from going forward. In this chapter I argue that Nepalese researchers, eager to use the technology to bolster their epistemological authority in the international conservation community, defused the conflict by hybridizing Cold War surveillance technologies with the traditional practices and pageantry of the colonial and aristocratic hunt. At the beginning of the project, which was organized by the Smithsonian Institution and funded by the U.S. branch of the World Wildlife Fund, the managers of the Tiger Tops Jungle Lodge saw the aesthetic impact of collaring tigers as a potentially fatal blow to their bottom line. As the lodge's research director argued in a letter to the Nepalese forestry department in the fall of 1973, a few months before the project began, “a tiger with a radio hanging around its neck is no longer an attraction.”¹⁹ At the

19 Charles McDougal to Secretary of Forests, His Majesty's Government of Nepal, 8 September 1973, Box 24,

same time, the international headquarters of the World Wildlife Fund in Switzerland opposed the project as a waste of the funds raised through its “Operation Tiger” campaign, which its leadership believed were better spent on guns, fences, rangers, and land acquisition. When WWF-US decided to fund the project against WWF-International's opposition, it led to a major schism within the organization that nearly resulted in a permanent separation. Over the course of the 1970s, however, the Nepalese researchers and their American partners successfully hybridized radio-tracking with the practices of the hunt—the skilled trackers who located tigers in the tall grasses and forests of the Nepal terai, the lines of elephants used to drive them from hiding, the long sheets of white cloth used to direct them toward a shooter hiding in a tree blind. By the early 1980s, participation in the tranquilizing and radio-tagging of a tiger, leopard, or rhinoceros had become a highlight of VIP visits to the park. When Russell Train, the president of WWF-US, visited the project in Chitwan in 1981, he noted in his journal that he while he was glad that the wildlife “massacres” of the colonial era were “a thing of the past, a sad aspect of the matter is that the training and keeping of elephants is a rapidly disappearing art. Our tiger project is one of the last—perhaps even the last—examples of their utilization in this fashion.”²⁰ The fact that British royalty and American elites could understand radio-tagging as simultaneously a perpetuation of colonial traditions and a symbol of postcolonial environmentalism in the mid-1980s was largely the result of the work of Nepalese biologists such as Kirti Man Tamang and Hemanta Mishra and their American partners during the previous decade.

Smithsonian Institution, Assistant Secretary for Science Records, 1963-1978, Record Unit 254, Smithsonian Institution Archives, Washington, DC.

20 Russell E. Train, Journal Entry, 6-9 February 1981, Box 36, Russell E. Train Papers, Library of Congress, Washington, DC; Hemanta R Mishra to Kathryn S. Fuller, 18 July 1994, and Hemanta R. Mishra to Russell E. Train, 1 August 1994, Box 13, Russell E. Train Papers, Library of Congress, Washington, DC. On the genealogical links between colonial big-game hunting and postcolonial wildlife conservation, see John MacKenzie, *The Empire of Nature: Hunting, Conservation, and British Imperialism* (New York: Manchester University Press, 1988).

In Chapter 5, I show how marine mammalogists and Iñupiat whalers joined forces to halt a proposed radio-tagging study of bowhead whales in the late 1970s. I argue that, in the wake of the kinds of controversies over radio-tagging that had led to the Craigheads' eviction from Yellowstone and that prevented the use of the technique to study India's tigers, wildlife biologists began to police their own use of invasive research techniques in controversial situations in order to avoid a further erosion of their authority. As a result, the more cautious members of the wildlife biology community found themselves allied with non-scientific groups who opposed radio-tagging. In the case of the bowhead whale, scientists at the Naval Arctic Research Laboratory in Barrow, Alaska, proposed radio-tagging the whales in the late 1970s as part of a broader “Project Whales” aimed at improving uncertain and controversial population estimates. Knowledge of the whales' migratory routes would, they argued, help to resolve an increasingly tense stand-off between indigenous whalers and the International Whaling Commission, which had declared a moratorium on the killing of bowheads in 1977. However, marine mammalogists believed the proposed study was too risky in both scientific and political terms. As one program officer at the Office of Naval Research, which had been the primary funder of marine mammal radio-tagging since the early 1960s, put it, “a hurry-up tagging program may be premature and if the data return is poor may so arouse vocal members of the community as to impede continued long-term development.”²¹ For leaders of the Iñupiat whaling community, the invasive nature of the radio-tagging project—the material requirement, using the techniques available at the time, of attaching the tag using either a harpoon or a shotgun, an operation unlikely to succeed without the help of practiced whalers—provided an opportunity to gain a measure of control over

21 Ronald C. Tipper to John J. Kelley, 15 December 1978, Records of the Naval Arctic Research Laboratory, Ca. 1940's to 1980's, Accession 89-188, Elmer E. Rasmuson Library, University of Alaska, Fairbanks.

scientific work on the North Slope. In the context of federal budget cuts for military research, the Navy was planning to close the laboratory by the end of the decade; the NARL leadership knew that support from the Iñupiat community was critical to continuing the laboratory now that Cold War-era justifications for its existence were no longer compelling. For these reasons, the tagging project was never conducted. Instead, experimental radio-tagging work was conducted with the gray whales of Baja California, and noninvasive bioacoustic techniques became the most important means of improving estimates of the bowhead population. Many of the scientists associated with Project Whales were soon employed by or consulting for the native corporations of the North Slope. In the context of environmental, animal rights, and indigenous rights movements with growing legal and cultural clout and the end of the golden age of Cold War science funding, choosing not to tag was sometimes the best way for wildlife biologists to maintain both their epistemological authority and their jobs.

Scaling back the use of radio-tagging was not the only viable response to this changing context, however. Chapter 6 shows how wildlife biologists used radio-tags that could be tracked by satellite in combination with the World Wide Web to engage public interest, raise funds, and change policy towards endangered albatross species in the 1990s and early 2000s. I argue that wildlife biologists responded to challenges to their authority based on the materiality of radio-tagging by reframing it as a means of establishing intimate, technologically-mediated relationships between endangered animals and members of the public—in particular, a “public” now defined as the audience accessible through the Internet. For albatrosses, which were highly endangered by longline fisheries on the high seas but for whom vocal and committed constituencies were lacking, new methods of establishing such connections were especially

appealing to conservationists and biologists. Satellite tagging suited this democratizing turn especially well because, although the initial tagging of the animal still depended on the scientist's privileged access and technical expertise, the tracking itself was carried out by a fully automated system. By removing the scientist as a mediating term, satellite-tagging and the World Wide Web made wildlife biologists into enablers of intimacy with wild animals rather than its sole possessors; they became specialists who produced connections rather than experts who monopolized authority. One of the earliest and most successful of these projects was the Albatross Project by David Anderson of Wake Forest University, who distributed daily updates on the movements of satellite-tagged albatrosses to tens of thousands of schoolchildren and members of the public in the late 1990s. Stressing the fact that the public received the same raw data as the scientists, Anderson told one journalist in 1998, "There's no filter."²² In addition to changing the role of the scientist, satellite-tagging also made it possible to generate sympathy for animals less charismatic than grizzly bears, tigers, or whales, such as the endangered albatrosses whose deaths as bycatch in longline fisheries became a focus of marine conservation advocacy in the 1990s. By following the foraging journeys of "their" albatrosses, schoolchildren were given an opportunity to identify with creatures who, like themselves, would be forced to make hard choices in the face of global economic and environmental change. This strategy was not limited to schoolchildren, either; in an explicit attempt to appeal to adults, the British betting company Ladbrokes and the nonprofit Conservation Foundation sponsored a "Big Bird Race" in 2004 in which gamblers could wager on the progress of individual satellite-tagged albatrosses as they migrated from breeding colonies in Tasmania to feeding grounds of the coast of South Africa.

²² Scott Lafee, "Public Has Opportunity to Help Science Take Flight," *San Diego Union-Tribune*, 18 February 1998.

Now that wild animals could be seen as representing participation in the modern world rather than an escape from or foil to it, and hands-on techniques such as radio-tagging could be seen as a means of popular connection as well as elite control, the kinds of opposition to radio-tagging that had flourished in the period from the mid-1960s to the mid-1980s became history.

2

A BIRD IN THE HAND **TRACKING RUFFED GROUSE IN MINNESOTA, 1957-1967**

Although well into his eighties and weakened by a serious illness, Dwain Warner was still an enthusiastic and vibrant presence when I met him at his home in rural Minnesota in the summer of 2004. It was easy to see how, as an ornithologist at the University of Minnesota more than four decades earlier, he had been able to inspire his colleagues and students to help him pursue what was at the time an ambitious goal—a way of continuously monitoring wild animals in their natural environments using some of the new surveillance technologies that been developed during (and, for the most part, for) World War II and the early Cold War. Warner's home was a short drive from the Cedar Creek Natural History Area, a University of Minnesota field site for ecological research where he had partnered with a younger ecologist named John Tester and an electrical engineer named William Cochran to build an innovative automatic wildlife radio-tracking system in the early 1960s. Warner had begun collaborating with engineers at the university's Institute of Technology in the mid-1950s, but it was the launch of a Russian satellite carrying a live dog late in 1957 that shifted the radio-tracking project into high gear. As Warner remembered it, Athelstan Spilhaus, an oceanographer with close ties to the U.S. Navy as well as being the high-modernist dean of the Institute of Technology, provided crucial support for the idea when most of his colleagues at the Minnesota Museum of Natural History had dismissed it as unworkable. “I got this idea,” Warner recalled telling Spilhaus in late 1957 or early 1958. “The Russians put the dog Sputniks up there, and they're having telemetered back to the Earth both physiological and ecological data on the capsule, and physiological ... data on the dogs.

We haven't even done that out the window here.”¹ Inspired by Cold War competition, Warner dreamed of rendering the natural world as knowable as the inside of a space capsule. Like Operation Igloo White, the U.S. Air Force surveillance system deployed in Southeast Asia almost a decade later, Warner's vision of radio-tracking reflected what Paul Edwards has called the “closed-world discourse” of Cold War culture.² More specifically, as Gregg Mitman and Donna Haraway have both argued, the development of wildlife radio-tracking in the early 1960s reflected a new, postwar vision of nature made manageable through advanced technologies of surveillance and control. In this transcendent vision, the wildlife biologist would be all-knowing, invisible, and empowered to recreate an Edenic nature that would serve as a foil and an escape from an increasingly fallen world.³

Even the most transcendent visions require engineers, however, and Warner's experience studying the ornithology of neotropical migrants had ill-prepared him to work with transistor-based radio circuits or to raise the kinds of funds that would be necessary to make wildlife radio-tracking into a reality. Cold War discourse was pervasive in late-1950s America, but Cold War institutions, patronage, technologies, and ideologies did not come in a single, neat package that could be transformatively applied to fields as seemingly distant from military concerns as ornithology. Despite the technical assistance of literal rocket scientists in the laboratory of Ernst Eckert at the University of Minnesota, Warner's project made only limited progress until he and Tester hired Cochran away from the University of Illinois in 1963. At Illinois, Cochran had been simultaneously designing radio beacons for ionospheric research on artificial satellites and

1 Dwain W. Warner, interview conducted in August 2004.

2 Paul N. Edwards, *The Closed World: Computers and the Politics of Discourse in Cold War America* (Cambridge, MA: MIT Press, 1996).

3 Gregg Mitman, “When Nature *Is* the Zoo: Vision and Power in the Art and Science of Natural History,” *Osiris* 11 (1996): 117-143; Donna J. Haraway, *Primate Visions: Gender, Race, and Nature in the World of Modern Science* (New York: Routledge, 1989).

building radio-tags to be used on ducks and rabbits for biologists at the Illinois Natural History Survey. Trained by the U.S. military in radio engineering during the Korean War, Cochran's expertise was partly a product of the Cold War, but it was not reduceable to it. As Cochran's former boss at the University of Illinois told me when I visited him and Cochran in the summer of 2006, engineers with Cochran's experience were a dime a dozen, but Cochran had an unusual talent for transistor circuit design—and especially for the kinds of small, robust, and efficient transmitters that were useful for tracking artificial satellites and wild animals alike. Positioned outside of the military and major defense contractors and genuinely interested in scientific questions of bird migration, Cochran was able to reappropriate Cold War technologies in ways that engineers more firmly situated within Cold War institutions could not. Similarly, the Cedar Creek radio-tracking project depended on a mix of patronage that included but was not limited to the Sputnik-inspired expansion in federal science funding of the late 1950s. Initially, despite Spilhaus's support and the fact that both the Office of Naval Research and the National Science Foundation were actively supporting other wildlife radio-tracking projects, both agencies refused to fund Warner's proposal. (Warner recalled NSF's George Sprugel telling him that the proposal had a “snowball's chance in hell in Washington.”) Instead, Warner turned to the Louis and Maud Hill Foundation, a Minnesota-based philanthropy founded on the wealth of late nineteenth-century railroad tycoon and conservationist James Hill. Though soon complemented by additional funding from the Atomic Energy Commission and the National Institutes of Health, it was this initial seed money that made the Cedar Creek radio-tracking project possible. For the developers of wildlife radio-tracking, the Cold War was a set of appropriable resources rather than a totalizing discourse.

The transcendent vision of nature that, in combination with Cold War surveillance technology, inspired the invention of wildlife radio-tracking was also less influential and less totalizing than it seems at first glance. As biologists and engineers grappled in the field with the materiality of the tags, the limits of human and animal bodies, and the complex propagation of radio signals across landscapes, they largely abandoned dreams of transcendence in favor of the embodied, technologically-mediated, situated intimacy with wild animals and their habitats that the technique both required and enabled. This shift is apparent in the detailed correspondence, field notes, and progress reports that I found in the papers of William H. Marshall at the University of Minnesota archives, which provide insight into changes in field practice that were often elided in the published literature. In addition to serving as the director of the Cedar Creek Natural History Area where Warner, Tester, and Cochran built their automatic radio-tracking system, Marshall had launched his own radio-tracking study of ruffed grouse at the Cloquet Forest Research Center in northern Minnesota. At the beginning of the project in 1959, Marshall and the Honeywell engineers with whom he worked designed the system so that researchers would spend as little time in the field as possible. Almost immediately, however, it became apparent that the system as designed could not work. By the end of the project in 1965, researchers were conducting almost all of their tracking on foot, immersed in the same landscape as the animals they studied. In 1966, when Marshall wrote up his final report to the National Science Foundation, he emphasized that the system's "basic attribute" was that it allowed scientists to become "intimately acquainted" with animals and their habitats.

The efforts of wildlife biologists such as Warner, Marshall, and Tester to achieve a transcendent vision of nature by appropriating Cold War surveillance technologies had been

motivated in part by professional concerns. New technologies of legibility would, they hoped, solidify the epistemological authority of wildlife biologists vis-à-vis the hunters and other special interest groups who often contested their claims about the hunting regulations and habitat improvements that would produce healthy populations of wild animals. Setting aside the quest for transcendence did not, however, mean that radio-tracking could not fulfill this authority-generating role. As Marshall realized in the course of his grouse-tracking project, technologically-mediated intimacy could also serve as a source of epistemological authority. A scientist with a radio-tracking receiver had privileged access to the private lives of the wild animals he or she had tagged—a kind of connection to which no hunter could lay claim. Even when an animal was not being actively tracked, its collar or tag identified it as the property, in epistemological if not legal terms, of the scientist who had marked it. Not all biologists agreed that the kind of hands-on engagement required for radio-tracking produced legitimate knowledge; the reviewers at the National Science Foundation who denied Marshall's grant renewal application in 1965, for example, dismissed it as “nothing more than natural history.” But many did. Although wildlife biologists would soon find that radio-tagging brought new vulnerabilities as well as new powers, such claims to intimacy and ownership went largely uncontested at first, especially when they were based on work conducted in dedicated research sites such as the Cedar Creek Natural History Area or the Cloquet Forest Research Center. “Closed-world discourse” and “transcendent visions” influenced radio-tracking, but they did not define it, for the simple reason that neither were sufficient to produce a working technology under actual field conditions.

Wildlife Managers and Manageable Wildlife

As a profession distinct from forestry or zoology, wildlife management had been established only in the 1930s, and in the postwar years it was still shaky on its feet. Despite an increase in funding and student enrollments after World War II, the professional leaders of the discipline of wildlife management remained insecure about the status of their would-be profession. In the early 1950s, student enrollments and job opportunities dropped or leveled off as the influx of GI Bill students subsided and the conflict in Korea drew young men back onto the battlefield.⁴ By 1955, the Wildlife Society's employment committee, chaired by William Marshall, judged the profession to be at a "saturation point."⁵ Two years later, reviewing what he considered to be a stellar report on gray squirrels in West Virginia, one wildlife biologist suggested that the authors might have been better off professionally if they had "developed skills in the arts of carpentry, plumbing, or house-painting."⁶ The professional anxieties of wildlife managers and the low-status social groups against which they defined themselves were summed up succinctly by Justin Leonard in a 1953 speech to the Midwest Wildlife Conference: "Despite the very real contributions research has already made to fish and game management a significant portion of our citizenry still has greater regard for the management ideas of the Indian guide or the unlettered backwoodsman than for those of the professionally competent fact finder."⁷

4 Merle A. Gee, Laurence E. Riordan, William F. Sigler, Harold V. Terrill, and Lansing A. Parker, "Enrollment, Graduation and Jobs: Report of the Employment Committee, March 1953," *Journal of Wildlife Management* 17 (1951): 355-360.

5 William H. Marshall, Victor E.F. Solman, and Shaler E. Aldous, "Report of the Employment Committee, March 1955," *Journal of Wildlife Management* 19 (1955): 395-97.

6 Durward L. Allen, Review of Hans G. Uhling et al., *The Gray Squirrel in West Virginia* (Charlseton, WV: Conservation Commission of West Virginia, Division of Game Management, 1956), *Journal of Wildlife Management* 21 (1957): 107-108, on 108. Allen was the author of a popular summary of the principles of wildlife management; Durward L. Allen, *Our Wildlife Legacy* (New York: Funk and Wagnalls, 1954).

7 Justin W. Leonard, "The Future of Conservation Research," *Journal of Wildlife Management* 19 (1955): 1-7, on 1.

One response to this crisis of confidence was to suggest that wildlife biologists needed to engage more directly with American culture and politics—to become, as Olaus Murie put it, “responsible citizens.”⁸ Like Aldo Leopold in his later years—the Leopold of *Sand County Almanac* rather than the Leopold of *Game Management*—Murie argued that wildlife managers could not afford to retreat into technical expertise.⁹ “Thoughtful people are trying to understand our place in Nature, trying to build a proper social fabric, groping for a code of ethics toward each other and toward nature,” he wrote in the *Journal of Wildlife Management* in 1954. “We have a choice as a profession: We may be content to expertly tinker with the wildlife machine to keep it alive somehow; or we can give our profession the dignity and importance it deserves and help the public interpret the facts so as to contribute in man's struggle to find himself.”¹⁰ Others argued similarly that only the development of what Leopold had called an “ecological conscience” would ensure that the American public followed the advice of the “wildlife technicians.”¹¹ More prosaically, prominent wildlife biologists such as Gustav Swanson, chairman of Cornell University's Department of Conservation, suggested that wildlife managers in training need to learn more about law and administration, despite the widespread impression

8 “More and more the modern scientist is taking part in the molding of our culture, whatever his particular discipline. In other words, he makes value judgments in the light of modern needs. He has become a responsible citizen”; Olaus J. Murie to Thomas R. Evans, 19 April 1956, Box 5, Olaus J. Murie Papers, CONS 90, Denver Public Library, Denver, Colorado.

9 See also Paul Errington, “In Appreciation of Aldo Leopold,” *Journal of Wildlife Management* 12 (1948): 341-50.

10 Olaus J. Murie, “Ethics in Wildlife Management,” *Journal of Wildlife Management* 18 (1954): 289-293.

11 Francis C. Evans, Review of Paavo Voipio, *Evolution at the Population Level With Special Reference to Game Animals and Practical Game Management* (Helsinki: Game Research Institute, Finnish Foundation for Game Preservation, 1950), *Journal of Wildlife Management* 15 (1951): 229-231, on 229. Similarly, in 1957, Clarence Schoenfeld wrote: “A generation ago, we were confident that once we had collected a body of scientific knowledge about wildlife crops and cropping, all would be well. Today we realize that wildlife management cannot function in America without public support, or at least without public sufferance, and that the development of a favorably climate of public opinion must accompany or even precede the management of game”; Clarence A. Schoenfeld, “Public Relations Aspects of Wildlife Management,” *Journal of Wildlife Management* 21 (1957): 70-74, on 70.

that such subjects were “dull and somewhat evil.”¹² On a rhetorical level, at least, the idea that technical expertise was the easier half of wildlife management was widespread.

In classrooms, field sites, and meetings of state fish and game commissions, however, scientific and technical authority continued to overpower softer considerations of ethics, culture, or public relations. In 1947, for example, Clarence Cottam, who had just completed a brief term as Chief of the Division of Wildlife Research for the U.S. Fish and Wildlife Service, wrote that there was “no greater need than that decisions and actions pertaining to research and management be based upon biological facts rather than political expediency. Experience should have taught us by now that among the worst enemies of research—and, in fact, of all wildlife and fishery resources—are expedient, patronage-seeking partisan politicians of all brands.”¹³ With “biopolitics” figured as the enemy of effective wildlife management, training and practice increasingly leaned toward the adoption of sophisticated technologies that would distance wildlife managers from the “Indian guide” and the “unlettered backwoodsman.” Lending strength to this approach was the fact that Leopold and others had urged an increased focus on research in 1940s, when it became apparent that existing wildlife management theories were inadequate. As Leopold put it to A.D. Middleton, a member of Charles Elton's Bureau of Animal Ecology, in 1945, “my group has moved very definitely in the direction of fundamental population research, as against applied management techniques. Reason: management techniques didn't work except in a few species.”¹⁴ After Leopold's death in 1948, his students and his son A.

Starker Leopold largely abandoned the cultural program epitomized by *Sand County Almanac*

12 Gustav Swanson, Review of William F. Schulz, Jr., *Conservation Law and Administration* (New York: Ronald Press Company, 1953), *Journal of Wildlife Management* 19 (1955): 312.

13 Clarence Cottam, “Some Improvements Needed in Wildlife Research,” *Journal of Wildlife Management* 11 (1947): 339-347.

14 Aldo Leopold to A.D. Middleton, 14 December 1945, Box 2, Series 9/25/10-1, Correspondence, Aldo Leopold Papers, 1903-1948, Archives and Records Management, University of Wisconsin, Madison.

and focused their efforts on continuing Leopold's efforts to professionalize wildlife management and to strengthen its claims to scientific legitimacy. In 1954, for example, one of Leopold's former students argued that even though Leopold had referred to wildlife management as an “art,” no one had been more responsible than he for transforming it into a “science.”¹⁵ But transforming Leopold's art into science entailed an active misinterpretation of his thinking, one that would simmer unnoticed until an equally egregious misinterpretation made him a sentimental icon of the popular environmental movement in the 1960s.¹⁶

For those who saw technical sophistication as the solution to wildlife management's woes, physical scientists and engineers represented a model to be emulated.¹⁷ For Leonard, who was so concerned to distance himself and his colleagues from Indian guides and uneducated backwoodsman, the relevance of physicists—“today's unquestioned aristocrats, who thirty years ago were considered the most impractical and unworldly of pure scientists”—to the professional anxieties of wildlife managers was clear.¹⁸ Similarly, Fairfield Osborn, director of the New York Zoological Society and son of the famous paleontologist, conservationist, and eugenicist Henry Fairfield Osborn, bemoaned the fact that, for the moment, “the physicist, the chemist, or the engineer is in the driver's seat and the voice of the biologist or zoologist is scarcely heard.”¹⁹

Glen Cole, a wildlife biologist at the Montana Fish and Game Department in the 1950s who would later play a key role in debates over whether grizzly bears should be radio-tagged in

15 Robert A. McCabe, “Training for Wildlife Management,” *Journal of Wildlife Management* 18 (1954): 145-149.

16 Only in the late 1980s would the two Leopolds that had been divided in the decade after his death—the sentimental land ethicist of *Sand County Almanac* and the hard-nosed wildlife technician of *Game Management*—begin to be reunited; J. Baird Callicott, *In Defense of the Land Ethic: Essays in Environmental Philosophy* (Albany, NY: State University of New York Press, 1989).

17 See John L. Rudolph, *Scientists in the Classroom: The Cold War Reconstruction of American Science Education* (New York: Palgrave, 2000).

18 Justin W. Leonard, “Research Man vs. Administrator—The Research Man's Viewpoint,” *Journal of Wildlife Management* 13 (1949): 237-244.

19 Fairfield Osborn, “Man Can Not Stand Alone,” *Animal Kingdom* 54 (1951): 1.

Yellowstone, was one of the aggressive promoters of this new strategy of professionalization. Writing in a bulletin targeted toward Montana's hunters and fisherman, Cole explained that game management had recently become “a full-fledged science.”²⁰ In another bulletin in the same series, Cole appealed to the technological spirit of the age: “In this age of missiles and Sputniks, the use of research results and a scientific approach should be something the public demands in a deer management program.”²¹ As students trained in the postwar years rose into positions of responsibility in the 1950s and 1960s, such ideas began to influence everyday practice as well as the high-level discussions of the leaders of the field.²²

The sense that new tools were crucial to wildlife biologist's professional status was reflected in a speech given late in 1959 by James Kimball, director of research for the Minnesota Department of Conservation's Game and Fish Division. Invited by Marshall to say a few brief words of introduction and welcome at the Midwest Wildlife Conference, Kimball instead delivered a spirited attack on the value of wildlife research to the wildlife managers who determined bag limits, open seasons, and refuge policies, and to the hunters whose taxes paid their salaries and research budgets. For too long, Kimball argued, wildlife managers had been forced to make life-or-death decisions about wildlife populations on the basis of guesswork.

20 Glen F. Cole, *Montana Deer Management: Where Do We Go From Here?*, Montana Fish and Game Department Information Bulletin No. 1 (Helena, MT: Fish and Game Department, c. 1958), 3.

21 Ibid, 6. In a later issue of the same bulletin series, which was target at Montana hunters, Cole advocated aggressively for the professional authority of wildlife biologists: “Recommendations by professional game men occasionally conflict with those by public groups. Such conflict does not always win popularity contents for the game manager or biologist. However, it should be recognized that a professional game man would not be doing his job if he altered his recommendations simply to obtain a favorable public reaction”; Glen F. Cole, *Game Management Based on Facts*, Montana Fish and Game Department Information Bulletin No. 9 (Helena, MT: Fish and Game Department, n.d.), 1.

22 By the late 1960s, according to one observer, the management of wildlife refuges was coming under the control of a new, technically-minded generation: “Vacancies, by and large, are being filled with young men who have more technical knowledge. They want to test ideas, band birds, and learn bow to apply biological knowledge accumulated on wildlife and land and water uses”; Laurence R. Jahn to Ira N. Gabrielson, 24 July 1967, Carton 5, A. Starker Leopold Papers, MSS 81/61c, Bancroft Library, University of California, Berkeley

Despite the establishment of various federal and state programs to promote wildlife research since the late 1930s, researchers—whether they were academic biologists such as Marshall and Warner, employees of the state fish and game agencies such as Kimball and his staff, or employees of the U.S. Fish and Wildlife Service—had done little to make the jobs of managers easier. Only when biologists had proven their worth to hunters would they be justified, Kimball argued, in asking for additional funding for research. His speech was, needless to say, controversial among the researchers in his own department. In the gloomy landscape that Kimball depicted, they were, however, some bright spots—notably, the wildlife radio-tracking projects that Marshall and Warner had just launched. The new technologies of surveillance, information, and communications spawned by Cold War competition offered a way to bolster the relevance and credibility of wildlife biologists, but only, Kimball argued, if wildlife biologists realized the urgency of the situation. “We must have greater knowledge and we must have it soon. Where do we find a Russian Sputnik to shatter our complacency?” he asked his audience of researchers. “America cannot afford to have you content or complacent. The growing need for wholesome outdoor recreation is overwhelming us. Only you can solve the problem. There is no time for you to relax.”²³

The Grouse Project

William Marshall was typical of the generation that spanned the founding of the wildlife management profession in the 1930s and its dramatic expansion and transformation after the war.

23 Kimball sent a letter of explanation to the researchers in his department—but not a retraction of his criticisms of wildlife research—two weeks later; see James W. Kimball to All Research Personnel, 18 December 1959, and attached manuscript of James Kimball, “What the Wildlife Administrator Wants From Research,” delivered on 7 December 1959 at the Twenty-First Midwest Wildlife Conference, Minneapolis, MN, Box 1, Minnesota Conservation Department Game & Fish Division, Research and Planning Section, Administrative Files, 1957-1966, Minnesota Historical Society, St. Paul, MN.

In his peripatetic twenties, he had received broad exposure to the United States' wildlife populations and the federal agencies that managed them. As a college student at the University of California, Berkeley, he worked as a weekend nature guide for the National Park Service and as a laborer removing invasive shrubs for the Forest Service, and after graduating in 1933 he worked on Forest Service projects in Arkansas and the Northeast, assessed waterfowl in Florida and Utah for the Biological Survey, and studied pine marten, grouse, and other wildlife in Utah, Idaho, and Montana for the Fish and Wildlife Service. By the time he received his PhD from the University of Michigan in 1942, at age 30, Marshall had worked in the South, the Northeast, the Midwest, the Pacific Coast, and the interior West, and he had made contacts with the wildlife community that would serve him in good stead throughout the rest of his career. After spending 1943-44 working for the War Food Administration in Idaho, his career as an academic wildlife biologist began in earnest in 1945, when he took a professorship in economic zoology at the University of Minnesota. By the mid-1950s he had achieved a prominent position in Minnesota's conservation community, serving as president of the Minnesota Academy of Sciences in 1954-55 and helping run the university's summer biology session at Lake Itasca.²⁴

Marshall's research in Minnesota focused on the ruffed grouse, a mid-sized “upland game bird” common to the northern forests of North America and a favored target of hunters. Marshall's research on the grouse population in the Cloquet Forest Research Center was firmly in the Leopoldian tradition. Located some twenty-five miles west of Duluth, the 3,300-acre research forest was established by the University of Minnesota in 1909 on land that had formerly

²⁴ Marshall's work at Itasca was funded by the National Science Foundation and the Hill Family Foundation, a St. Paul-based philanthropic organization founded with money from James J. Hill's Great Northern Railroad, and he received federal funds through the Minnesota Department of Conservation for research on ruffed grouse. For details of Marshall's career, see “Curriculum Vitae,” n.d. (1978?), William H. Marshall Biographical File, University of Minnesota Archives, Elmer L. Andersen Library, Minneapolis, MN.

been part of the Fond du Lac Indian Reservation.²⁵ In the 1930s, at the recommendation of Leopold, the university sponsored a study of the Cloquet forest's grouse population that was modeled after Herbert L. Stoddard's paradigm-setting study of bobwhite quail.²⁶ Marshall had continued this research from the late 1940s to 1956, when he received a new grant through the Pittman-Robertson Act to study the relationship between forest management practices and ruffed grouse populations at the Cloquet Forest Research Center.²⁷ The project was led in the field by Gordon W. Gullion, a wildlife biologist who had previously studied grouse and other upland game birds in Nevada. Despite the decades of intensive study of the Cloquet population with standard field methods—trapping, tagging, weighing, observing behavior, conducting autopsies—critical questions about ruffed grouse remained unanswered. In particular, little was known about the causes of cyclic fluctuations in the population or about the decline of grouse numbers in forests that had been clear-cut early in the late nineteenth and twentieth centuries for lumber and had since been managed for paper pulp.²⁸ Like other wildlife biologists, Marshall was

25 “History of the Cloquet Forest Research Center,” <http://www.cnr.umn.edu/cfc/about/history.html>, retrieved on 8 March 2008.

26 Aldo Leopold's encouragement of ruffed grouse research in Minnesota is described in David L. Hansen, *A Century of Research in Natural Resources* (St. Paul, MN: University of Minnesota, Minnesota Agricultural Research Station, College of Natural Resources, 2003); also available at <http://www.maes.umn.edu/CenturyofResearch.asp>, retrieved on 6 March 2008. The influence of Leopold and Stoddard on wildlife research in the 1930s is described in Thomas R. Dunlap, *Saving America's Wildlife: Ecology and the American Mind, 1850-1990* (Princeton, NJ: Princeton University Press, 1991): 71-74. The paradigmatic work was Herbert L. Stoddard, *The Bobwhite Quail: Its Habits, Preservation, and Increase* (New York: Charles Scribner's Sons, 1931).

27 On the history of the Pittman-Robertson Act, see Harmon Kallman, ed., *Restoring America's Wildlife, 1937-1987: The First 50 Years of the Federal Aid in Wildlife Restoration (Pittman-Robertson) Act* (Washington, DC: U.S. Department of the Interior, 1987).

28 For an overview of standard field methods in the 1950s, see Henry S. Mosby, ed., *Manual of Game Investigational Techniques* (Blacksburg, VA: The Wildlife Society, 1960). Fluctuations in Cloquet's grouse population from the late 1930s to late 1950s are described in G. W. Gullion, R. T. King, and W. H. Marshall, “Male Ruffed Grouse and Thirty Years of Forest Management on the Cloquet Forest Research Center,” *Minnesota Journal of Forestry* 33 (1962): 617-622. On the Pittman-Robertson-funded ruffed grouse and forest management project that began in 1956 under Marshall and Gullion's direction, see the file on “Forest Wildlife Relations, 1956-1962,” State Documents, MN 3390 FWR, University of Minnesota Entomology, Fisheries and Wildlife Library, St. Paul, MN.

painfully aware of the limitations of existing census or survey techniques for answering questions about population fluctuations. Starting in the late 1940s, he and his colleagues and students began focusing increasingly on physiology, life history, and behavior of grouse, using the new tags, traps, and other tools that were being described in every issue of the *Journal of Wildlife Management* as well as developing new methods of their own. By 1959, however, the continued decline of the ruffed grouse population of northern Minnesota continued to mystify Marshall, Gullion, and their colleagues, and to frustrate hunters and wildlife managers. The situation was ripe for a new technique that would provide detailed information about where individual grouse looked for cover, what they ate during the long winter, and why they died—and would do all of this without disturbing the bird's behavior to the point that it was no longer representative of the population in its natural state.

Wildlife telemetry was made possible by the transistor, a technology whose invention in 1948 and subsequent refinement owed much to Cold War demands for light-weight control and communications systems for missiles, satellites, and military aircraft. Over the course of the 1950s, as transistors became more affordable, reliable, and efficient, and as they began to enter the civilian consumer market through devices such as the transistor radio, scientists and engineers began to experiment with using them to study life in the laboratory and in the field. Compared to previous radio technologies, transistor-based transmitters were dramatically smaller, lighter, and less power-hungry. They thus opened up the possibility of transforming instruments that had previously been confined to the laboratory or to fixed field stations into instruments that could be carried into and around field sites, or even attached to mobile animals and people. By using such devices—as well as radar, radioactive tracers, computers, and the

plethora of other new instruments that had been developed for World War II and the Cold War—biologists hoped to render nature legible and, by doing so, to make themselves indispensable to the practice of wildlife management.²⁹

The sophistication of the technology involved was one of the appealing characteristics of radio-tracking; it promised to help distance wildlife biologists from the hunters and trappers who so often disputed their assessments of the health of wildlife populations and the actions necessary to protect it. But it also posed a challenge. Electrical engineering was not part of the curriculum that Marshall had followed to become one of the leaders of the wildlife management profession in the Upper Midwest, nor were institutional links between wildlife management and engineering abundant. Transforming initial experimental efforts into a reliable field technique required the creativity of a few bridging individuals, the support of institutions (some of them new), and a heavy infusion of capital from sources not usually tapped by wildlife biologists. The Navy played an especially important role in the development of wildlife telemetry, directly sponsoring some of the earliest attempts to use radio transmitters to monitor the physiology and behavior of wild animals. One reason for the Navy's interest was the legacy of its role as the federal government's primary science-funding organization in the decade after World War II, but there were other, more particular reasons that kept the Office of Naval Research actively involved in wildlife radio-telemetry for many decades after its funding of other research fields been taken over by the National Science Foundation, the National Institutes of Health, the Atomic Energy Commission, and other agencies within the Department of Defense.

²⁹ For an example of unbridled technological enthusiasm among wildlife biologists in the 1960s, see Howard J. Stains, "Automation in Wildlife Research—Present and Future," *Transactions of the Twenty-Eighth North American Wildlife Conference* (1963), 262-268.

The first account of wildlife radio-tracking published in a peer-reviewed journal came out of a study conducted at the U.S. Navy Medical Research Institute in Bethesda, Maryland, in 1957-1958. Cobert D. LeMunyan and John J. Christian of Johns Hopkins University had found it difficult to study the population dynamics of woodchucks because the burrowing rodents could not easily be relocated on their 400-acre study plot. Inspired by the use of radio telemetry in military studies of the physiology of jet pilots and in medical studies of digestion, they worked with engineers at the Naval Research Laboratory in Washington, D.C., to build transmitters that could be surgically implanted in the animals. At the time, the Naval Research Laboratory was developing radio transmitters and tracking technologies for the Vanguard series of satellites. The signals emitted by the implanted transmitters proved to be detectable from a distance of eighteen yards even when the woodchucks were within their burrows. Christian and LeMunyan's main reason for experimenting with radio-tracking was to improve their understanding of populations dynamics, but their work was also intended to served military purposes of understanding and controlling rodent vectors of disease. Although their research group did not pursue the technique further, their article in *The Journal of Wildlife Management* in 1959 was widely cited in the early wildlife telemetry literature and helped spark interest in the technique among wildlife biologists with more tenuous connections to the military-industrial complex.³⁰

30 The radio-tracking system is described in Cobert D. LeMunyan, William White, Ernest Nyberg, and John J. Christian, "Design of a Miniature Radio Transmitter for Use in Animal Studies," *Journal of Wildlife Management* 23 (1958): 107-110. Christian and LeMunyan also collaborated on a laboratory study of crowding in mice; see John J. Christian and Cobert D. LeMunyan, "Adverse Effects of Crowding on Reproduction and Lactation of Mice and Two Generations of Their Progeny," *Endocrinology* 63 (1958): 517-529. Christian had a dual appointment at the Johns Hopkins School of Hygiene and Public Health, where he worked in the laboratory of his mentor David E. Davis; see John J. Christian, "In Memoriam: David E. Davis, 1913-1994," *Auk* 112 (1995): 491-492. Davis and Christian were interested in the impact of high-density urban living on social behavior.

A Navy-sponsored collaboration with more lasting consequences grew out of the work of the Philadelphia-based American Electronics Laboratories on devices for remotely monitoring the physiology of Navy sailors and aviators. In 1956, Orr Reynolds, the director of ONR's biology branch, received two nearly simultaneous requests for a miniature radio thermometer. One came from Carl Eklund, a wildlife biologist with the National Research Council who was preparing to spend the International Geophysical Year in Antarctica, and the other from the Navy Clothing Supply Office, which wanted to measure the efficacy of immersion suits designed to keep pilots warm in ocean waters. In December 1957, Eklund and a Navy electronics technician used the radio thermometer to study the temperature of the incubating eggs of a South Polar skua and an Adelie penguin from a mobile receiving station.³¹ This initial foray into wildlife telemetry on the part of the Office of Naval Research laid the groundwork for further projects that continued into the mid-1960s. John Busser, the AEL engineer who designed the radio thermometer used by Eklund, went on to become an important figure in the development of biological telemetry, including wildlife telemetry. In the early 1960s he designed implantable transmitters for a study of woodchuck movements by H. Gray Merriam, a student of population ecologist LaMont C. Cole at Cornell University, and a pigeon-tracking transmitter for Sidney R. Galler, who succeeded Orr Reynolds as director of ONR's biology branch. Support for wildlife telemetry from ONR remained strong through 1965, when Galler left the Biology Branch to take a position as Assistant Secretary for Science at the Smithsonian Institution. Galler, whose support

31 The use of the thermometer in studying the immersion suit was reported in the *Naval Research Review* in 1957; its use for studying incubating eggs was reported in *American Scientist* in 1959. John H. Busser and Marion Mayer, "Radio Thermometer That Fits in a Penguin Egg," *Naval Research Reviews* (June 1957): 9-13; Carl R. Eklund and Frederick E. Charlton, "Measuring the Temperatures of Incubating Penguin Eggs," *American Scientist* 47 (1959): 80-86. After returning from Wilkes Station in 1958, Eklund became head of the Polar Research Division of the Army Research Office. For more on research at Wilkes Station during the International Geophysical Year, see Carl R. Eklund and Joan Beckman, *Antarctica: Polar Research and Discovery during the International Geophysical Year* (New York: Holt, Rinehart and Winston, 1963).

for wildlife telemetry was based on an interest in biological mechanisms of orientation and navigation that he had held since at least the early 1950s, encouraged and funded the first major symposium on biotelemetry at the American Museum of Natural History in 1962. The Navy thus served as a major conduit for cutting-edge electronics to make their way into wildlife research in the 1950s and early 1960s.³²

The Military-Industrial-Ecological Complex

While direct connections with engineers within the Department of Defense, particularly the Navy, was useful for wildlife biologists interested in Cold War surveillance technologies, it did not turn out to be necessary. Starting in the late 1950s, federal and state wildlife agencies contracted the services of defense contractors such as the Minneapolis-Honeywell Regulator Company's Ordnance Division in Washington State, American Electronics Laboratories in Pennsylvania, and Philco's avionics branch in California to build telemetry devices for studying the impact of dam-building, logging, and other forms of natural resource exploitation on populations of wild animals. One of the earliest successful applications of transistor-based telemetry—though it was acoustic telemetry rather than radio telemetry—took place in 1956,

32 S. R. Galler and C. E. Fix, "Animal Tracking Gone Modern," *Naval Research Review* (August 1961):11-14. On Merriam's woodchuck telemetry study and Busser's role in it, see H. Gray Merriam, "Telemetering as an Aid to Population Studies," *Bulletin of the Ecological Society of America* 39(1958): 104; H. Gray Merriam, "Problems in Woodchuck Population Ecology and a Plan for Telemetric Study," Unpublished PhD Thesis (Cornell University, 1960); H. Gray Merriam, "Low Frequency Telemetric Monitoring of Woodchuck Movements," in *Bio-Telemetry: The Use of Telemetry in Animal Behavior and Physiology in Relation to Ecological Problems*, edited by Lloyd E. Slater (New York: Pergamon Press, 1963): 155-171; H. Gray Merriam, "Temporal Distribution of Woodchuck Interburrow Movements," *Journal of Mammalogy* 47 (1966): 103-110. On Busser's pigeon-tracking transmitter, see Lowell Adams, "Updates," *Wildlife Telemetry Newsletter* 1 (January 1962): 1-2. Based on circumstantial evidence linking a survey of Pacific birds to biological weapons research, Roy MacLeod has suggested that Sidney Galler helped coordinate "a highly problematic *mésalliance* between science and secrecy during the height of the Cold War" after he left ONR in 1965 to become Assistant Secretary for Science at the Smithsonian Institution; see Roy MacLeod, "Strictly for the Birds': Science, the Military and the Smithsonian's Pacific Ocean Biological Survey Program, 1963-1970," *Journal of the History of Biology* 34 (2001): 315-352.

when engineers at Honeywell built an acoustic tag for tracking salmon at the request of the National Marine Fisheries Service. The government fisheries scientists used the ultrasonic transmitters to track commercially important coho and chinook salmon as they migrated along the Columbia River at a time when the government was attempting to mitigate the impact of the dams on salmon through the use of hatcheries.³³

This study of salmon indirectly led to Marshall's involvement with wildlife radio-tracking. In December 1957, the Minneapolis office of the U.S. Fish and Wildlife Service approached Honeywell about the possibility of applying the company's "electronics know-how" to wildlife management.³⁴ Among the technologies identified in the series of discussions that followed was a miniaturized radio transmitter that could be used to track wild animals in their natural environments.³⁵ The idea may have been inspired by the November 1957 launch of the second Sputnik module and its payload of a live, remotely monitored dog, as Warner later recalled. In the fall of 1958, the USFWS regional office in Minneapolis began contacting local wildlife researchers to determine whether they had any interest in collaborating with Honeywell on such a device.³⁶ Marshall submitted an initial grant proposal to the National Science

33 On the management of salmon on the Columbia River, see Joseph E. Taylor, *Making Salmon: An Environmental History of the Northwest Fisheries Crisis* (Seattle: University of Washington Press, 1999); Richard White, *The Organic Machine: The Remaking of the Columbia River* (New York: Hill and Wang, 1995), 89-113. For contemporary accounts of sonic tracking of salmon from the mid-1950s to late 1960s, see Parker S. Trefethen, "Sonic equipment for tracking individual fish," *Special Scientific Report—Fisheries 179* (Washington, D.C.: United States Fish and Wildlife Service, 1956): 1-11; Parker S. Trefethen, J. W. Dudley and M. R. Smith, "Ultrasonic Tracer Follows Tagged Fish," *Electronics* 30 (1957):156-160; John T. Frye, "Carl and Jerry: Fish-Sniffing," *Popular Electronics* 8 (May 1958); J. H. Johnson, "Sonic Tracking of Adult Salmon at Bonneville Dam, 1957," *Fishery Bulletin* 176 (1960).

34 The term "electronics know-how" was used by Van Bearinger, the associate director of research at Honeywell, in a letter to the director of the Montana Department of Fish and Game explaining Honeywell's involvement with William Marshall in developing wildlife radio-tracking; Van W. Bearinger to W.J. Everin, 6 January 1959, Box 5, William H. Marshall Papers, 1947-1982, University of Minnesota Archives, Elmer L. Andersen Library, Minneapolis, MN.

35 R. W. Burwell to William H. Marshall, 27 October 1958, Box 5, William H. Marshall Papers, 1947-1982, University of Minnesota Archives, Elmer L. Andersen Library, Minneapolis, MN.

36 It is unclear why they settled on Marshall—rather than, for instance, Warner, who had significantly more experience and interest in collaborating with engineers—except that Marshall had long been involved with

Foundation in January 1959 for funding to develop a wildlife radio tag in partnership with Honeywell, which would be used to study ruffed grouse in the Cloquet Research Forest. “Field conditions are ideal in that it will be possible to work with a known group of birds on known terrain under controlled conditions using well qualified personnel,” Marshall wrote.³⁷

At NSF, the proposal was assigned to the Environmental Biology Branch, then headed by George Sprugel, Jr., a zoologist who had been employed by NSF since 1953 and who had funded some of Marshall's earlier research. By the time he received the proposal, Sprugel had already heard enough about wildlife radio-tracking to warn Marshall about the risks of experimenting with cutting-edge technology and to suggest that Honeywell's price for engineering services was too high. “It seems that people are going off in all directions on the business of electronic tracking of animals,” he wrote.³⁸ Marshall replied that Honeywell's engineering expertise was well worth the cost and that the wildlife radio-tracking projects in progress elsewhere were using different methods, none of which overlapped exactly with his own.³⁹ This reply seems to have assuaged Sprugel's concerns. In March Sprugel wrote to let Marshall know that the project had been approved “on the basis of scientific merit, contingent upon the availability of sufficient funds.”⁴⁰ All that remained was to determine whether NSF had enough money in the bank to

applied wildlife management. R. W. Burwell to James Kimball, 23 September 1958, and R. W. Burwell to William H. Marshall, 27 October 1958, Box 5, William H. Marshall Papers, 1947-1982, University of Minnesota Archives, Elmer L. Andersen Library, Minneapolis, MN. Marshall was on close terms with Fish and Wildlife Service staff in Minneapolis, particularly Jack Berryman, see Clarence Cottam to William H. Marshall, 11 November 1958, Box 5, William H. Marshall Papers, 1947-1982, University of Minnesota Archives, Elmer L. Andersen Library, Minneapolis, MN.

37 Marshall submitted the proposal to the University of Minnesota treasurer for approval on January 12, 1959; see William H. Marshall to Laurence R. Lunden, 12 January 1959, and attached proposal: “Development and Use of Short Wave Radio Transmitters to Trace Animal Movements,” Box 5, William H. Marshall Papers, 1947-1982, University of Minnesota Archives, Elmer L. Andersen Library, Minneapolis, MN.

38 George Sprugel to William H. Marshall, 27 January 1959, Box 5, William H. Marshall Papers, 1947-1982, University of Minnesota Archives, Elmer L. Andersen Library, Minneapolis, MN.

39 William H. Marshall to George Sprugel, 2 February 1959, Box 5, William H. Marshall Papers, 1947-1982, University of Minnesota Archives, Elmer L. Andersen Library, Minneapolis, MN.

40 George Sprugel to William H. Marshall, 16 March 1959, Box 5, William H. Marshall Papers, 1947-1982, University of Minnesota Archives, Elmer L. Andersen Library, Minneapolis, MN.

support the project immediately or whether Marshall would have to wait until the 1960-1961 fiscal year. By mid-June 1959 the funding situation had been clarified, and in July the project officially began with the deposit of \$18,300 to a University of Minnesota account. Nearly \$10,000 of it was to go directly to Honeywell for the design and manufacture of six miniaturized radio transmitters.⁴¹

Marshall's impatience to get the project started is evident in his correspondence from the first half of 1959, when he was still waiting for the final word from Sprugel. In April, Jack L. Seubert of the South Dakota Game, Fish and Parks Department wrote to ask Marshall if he would be interesting in cooperating in the development of radio-tracking. Instead of agreeing, Marshall demanded to know how Seubert had found out about what he had assumed was a confidential proposal to NSF. The tension was somewhat relieved when Seubert reminded Marshall that the project had been described in a recent Fish and Wildlife Service newsletter, but the episode is a telling sign of Marshall's desire to stay ahead of the competition and his anxiety about the collaboration with Honeywell, which grew as Honeywell dragged its feet on the project.⁴² Although Honeywell had submitted a formal proposal to build the transmitters in January, the company took until the end of October to assemble an engineering team, and it was only in November that two Honeywell engineers came out to visit the research site—something Marshall had been requesting since March.⁴³ The Federal Communications Commission also

41 George Sprugel to William H. Marshall, 15 June 1959, Box 5, William H. Marshall Papers, 1947-1982, University of Minnesota Archives, Elmer L. Andersen Library, Minneapolis, MN.

42 William H. Marshall to John L. Seubert, 6 April 1959, John L. Seubert to William H. Marshall, 1 April 1959; and William H. Marshall to John L. Seubert, 23 April 1959, Box 5, William H. Marshall Papers, 1947-1982, University of Minnesota Archives, Elmer L. Andersen Library, Minneapolis, MN. John Tester told me that he remembered Marshall as an “independent operator” and “almost a little bit secretive” about the radio-tracking work at Cloquet; interview with John R. Tester, August 2004.

43 William H. Marshall to John B. Moyle, Jack Berryman, and Finn Larsen, 23(?) March 1959; William H. Marshall to Roy H. Malm, 26 March 1959; William H. Marshall to R. J. Boyle, 25 August 1959; William H. Marshall to William Currie, 12 November 1959, Box 5, William H. Marshall Papers, 1947-1982, University of Minnesota Archives, Elmer L. Andersen Library, Minneapolis, MN.

proved more recalcitrant than expected, initially rejecting Marshall's application for a license to use the transmitters, which the FCC insisted on regulating as “experimental radio stations.” The license finally came through on March 17 with only one string attached: Field workers would need shotguns and the authority to use them in the Cloquet Forest Research Center, an official state game refuge, so that if these radio stations (i.e., grouse) interfered with other radio equipment, they could be quickly taken off the air (i.e., shot).

The necessary permits arrived from the Minnesota Department of Conservation in April, just in time for the breeding season, when male ruffed grouse advertise their prowess by staking claim to fallen logs and rapidly flapping their wings to create a distinctive drumming sound.⁴⁴ The timing mattered because male grouse were easiest to capture during the drumming season, when the desire to rid their chosen drumming log of competitors—or even of their own reflection in a mirror placed strategically inside a wire trap—overcame their usual wariness. The Honeywell transmitters also arrived in early April along with Charles D. Canfield, the Seattle-based Honeywell engineer in charge of the project. Over the next ten days Canfield, Marshall, Gullion, and graduate students Robert Seabloom and Robert Schwab set up the receiving system at Cloquet using a domestic chicken to test the transmitters. The major challenge they faced was installing the two receiving antennas, one of which was to be located at the top of a 90-foot fire tower. By late April, after Canfield managed to wire an antenna at the top of the fire tower to receiving gear at its base and initial testing had been complete, the system was ready for use on a live, free-flying grouse.⁴⁵

44 William H. Marshall to James Kimball, 24 March 1960 and John B. Moyle to William H. Marshall, 6 April 1960; “Special Permit No. 231,” 31 March 1960, signed by James W. Kimball, Box 5, William H. Marshall Papers, 1947-1982, University of Minnesota Archives, Elmer L. Andersen Library, Minneapolis, MN.

45 After Canfield returned to Seattle, Marshall convinced the president of the University of Minnesota to send a letter to the president of Honeywell commending Canfield for a job well done; William H. Marshall to James L. Morrill, 25 April 1960; James L. Morrill to Paul B. Wishart, 28 April 1960, Box 5, William H. Marshall Papers,

Intimate Acquaintances

Marshall's plan was to use two fixed receiving stations, one at the fire tower and the other at the research center's headquarters, to triangulate the location of each transmitter-equipped grouse. His hope was that, aside from the minor discomfort of wearing a radio transmitter, the grouse would remain completely undisturbed by the researchers, who would follow their every move from the secrecy of the receiving stations.⁴⁶ In practice, neither the transmitters nor the receiving system worked as intended, though it took several weeks of experimenting to discover the fact. The first and most serious problem was the weight and shape of the transmitter. The design specifications had called for a total transmitter package—transistors, batteries, wires, and casing—weighing one ounce, or about 28 grams. But in order to achieve a battery life of more than a month and a transmitting distance of more than a quarter of a mile, the Honeywell engineers had increased the weight to about 1.7 ounces, or about 48 grams. The average grouse at Cloquet weighed between 500 and 600 grams, which made the packages almost ten percent of the bird's weight.⁴⁷ To make matters worse, the Honeywell engineers had encased the transmitter and batteries in a sharp-edged rectangular box, out of which a stiff antenna pointed sharply upward. This clunky contraption was to be strapped to the back of the grouse using a harness that had been designed to carry flat, lightweight tags used for visual identification.⁴⁸

1947-1982, University of Minnesota Archives, Elmer L. Andersen Library, Minneapolis, MN.

46 Marshall explicitly linked using the fixed stations and not disturbing the birds in a letter to Canfield in December 1959: "We would like to remain with the original tracking scheme of two antennae on towers rather than use a portable device; carrying the latter around would cause disturbance to the birds"; William H. Marshall to Charles Canfield, 10 December 1959, Box 5, William H. Marshall Papers, 1947-1982, University of Minnesota Archives, Elmer L. Andersen Library, Minneapolis, MN.

47 For comparison, most guides to radio-tracking birds today recommend a maximum of three percent of body weight; see Robert Kenward, "Radio-Tagging," in William J. Sutherland, Ian Newton, and Rhys E. Green, eds., *Bird Ecology and Conservation: A Handbook of Techniques* (New York: Oxford University Press, 2004)

48 These visual tags are described in Gordon W. Gullion, Robert L. Eng, and John J. Kupa, "Three Methods for Individually Marking Ruffed Grouse," *Journal of Wildlife Management* 26(1962): 404-407.

The result of the excess weight and awkward design of the tag was the nearly complete failure of the experimental system. The first grouse was captured, radio-tagged, and released at the end of April. After a few days of successful tracking, the signal suddenly disappeared completely. Neither the grouse nor the transmitter were recovered, even after Gullion and Schwab used a portable antenna and receiver that had been designed by a local radio engineer named Sidney L. Markusen to search the study area by foot. Ten days later they captured, radio-tagged, and released a second male grouse, and again the signal lasted for several days before disappearing. This time the grouse and transmitter—which were dead and barely transmitting, respectively—were found using the portable receiver only 150 feet from the point where the tagged bird had initially been released. Initially the cause of death was unclear and Gullion wrote a long letter to Marshall explaining that the bird must have died of exhaustion. A closer look revealed severe bruising at the base of one wing, most likely caused when the antenna caught on a low-lying branch as the grouse attempted to fly under it.⁴⁹ The failure of these initial attempts forced the Grouse team back to the drawing board. Gullion, in particular, who had the most experience trapping and handling grouse, was convinced that extensive testing of different harness designs on captive birds was necessary before tracking birds in the field could be attempted again. “We are dealing with an organism which I suspect is a pretty nicely balanced aerodynamic design—and one that may be quite easily upset by fairly slight modifications of airflow patterns,” he explained to Marshall.⁵⁰ Gullion proposed six alternative harness designs

49 Gordon W. Gullion to William H. Marshall, 18 May 1960 and 29 May 1960,, Box 5, William H. Marshall Papers, 1947-1982, University of Minnesota Archives, Elmer L. Andersen Library, Minneapolis, MN. On May 16, Honeywell's in-house paper published an article claiming that the radio-tagged grouse were “[a]pparently unmindful of their load of instruments”; “Tiny Space-Age Radios Track Grouse,” *Honeywell World* (16 May 1960): 8, in Box 5, William H. Marshall Papers, 1947-1982, University of Minnesota Archives, Elmer L. Andersen Library, Minneapolis, MN.

50 Gordon W. Gullion to William H. Marshall, 20 May 1960, Box 5, William H. Marshall Papers, 1947-1982, University of Minnesota Archives, Elmer L. Andersen Library, Minneapolis, MN.

and suggested testing them on captive chukar partridges, which were similar in size to grouse but much better at surviving in captivity.⁵¹

Like the transmitters, the receiving system had also failed. The station at the research center's headquarters experienced so much radio interference from car ignition systems that it was almost impossible to get a steady reading. The problem was solved by moving the receiving station to less-trafficked fish house three quarters of a mile away, but then a second problem emerged that threw the whole idea of using fixed stations into question.⁵² In order to achieve maximum power for the transmitted signal, the length of the transmitting antenna had to be a significant fraction of the wavelength of the transmitter. For this reason Honeywell's engineers had recommended a relatively high frequency, between 150 and 250 million cycles per second, that would allow the transmitting antenna to be as short as eight inches without sacrificing much efficiency. But as the frequency increased and the antenna length decreased, so did the ease with which the transmitted signals could be diffracted or reflected by vegetation, buildings, and other topographical features. From a fixed station, it was nearly impossible to distinguish between these distorted signals and the true location of the transmitter. An added disadvantage of the fixed stations was that when signals weakened, as they had in the case of the radio-tagged grouse that flew into the tree, there was no way to strengthen the signal by moving the receiver closer to the transmitter. As a result, the fixed receiving stations, which had been intended to minimize the disturbance the researchers caused the grouse, also minimized the amount of reliable data they could collect. Radio-tracking was not the disembodied ability to locate individual wild animals in

51 Gordon W. Gullion to William H. Marshall, 18 May 1960 and 29 May 1960, Box 5, William H. Marshall Papers, 1947-1982, University of Minnesota Archives, Elmer L. Andersen Library, Minneapolis, MN.

52 "Progress Report, NSF Grant GS 8644: Development and Use of Short Wave Radio Transmitters to Trace Animal Movements," Box 5, William H. Marshall Papers, 1947-1982, University of Minnesota Archives, Elmer L. Andersen Library, Minneapolis, MN.

the field that Marshall and others had initially imagined it as, but rather an embodied practice that depended on easily disrupted relationships between biologist, animal, equipment, and landscape.

Within a few weeks of its deployment, it was clear that the system originally conceived as a way of producing a transcendent, objective vision of grouse behavior would not work. The grouse and the forest, two critical components that could not be tested in the lab, had failed to play their assigned roles. The grouse had rejected radio-tagging by dying, while the forest had thrown up a screen of distortions and red herrings. Getting the system to work would require the creation of new field practices appropriate to the biology and behavior of the animal and to the radio characteristics of the landscape. With the drumming season over in mid-May, however, there was no easy way to capture male birds, so for the time being further tests on grouse were out of the question. Instead, Gullion and Schwab began testing the remaining transmitters on porcupines—relatively large, slow-moving rodents that, despite their formidable quills, were easy for a properly equipped wildlife researcher to handle. Between May 22 and June 13 they captured, radio-tagged, and released three female porcupines, including one mother-daughter pair. The goal was not to use radio-tracking as a tool to understand the porcupine, as they had hoped to do with the grouse, but to use the porcupine as a tool to understand radio-tracking.⁵³ For that purpose, the animal was nearly ideal. Unlike ruffed grouse, porcupines could carry a 50-gram transmitter package with little discomfort (and almost no chance of an unexpected crash-

⁵³ For a description of porcupines as tools for perfecting radio-tracking, see Gordon W. Gullion to Phillip J. Tichenor, 13 July 1960, Box 5, William H. Marshall Papers, 1947-1982, University of Minnesota Archives, Elmer L. Andersen Library, Minneapolis, MN. Gullion's skepticism about the inherent value of studying porcupines is captured in this comment from one of his letters to Marshall: "We're making good use of the technique now—gathering worthwhile information on the movements of a pretty important (?) [*sic*] mammal of the forested areas"; Gordon W. Gullion to William H. Marshall, 12 July 1960, Box 5, William H. Marshall Papers, 1947-1982, University of Minnesota Archives, Elmer L. Andersen Library, Minneapolis, MN.

landing). After the first porcupine to be radio-tagged was captured on May 22, Gullion and Schwab spent four days testing different harness and collar arrangements, eventually settling on a store-bought dog harness. The porcupine seemed to adjust to the arrangement happily. Like grouse, porcupines tended to split their time between trees and the ground. This serendipitous behavioral resemblance between two otherwise entirely dissimilar creatures meant that lessons learned by radio-tracking “porkies” could easily be applied to grouse once the appropriate transmitters and harnesses had been developed—or at least so Gullion, Schwab, and Marshall hoped.⁵⁴

On June 12, the wiring connecting the antenna at the top of the fire tower to the receiving set at its base broke, but the accident had little effect on field work. By that time the researchers were using portable, hand-held receivers and antennas almost exclusively. With the coming of summer, the deciduous shrubs and trees at the Cloquet Forest Research Center had produced a bewildering array of reflective surfaces—leaves—that increased the range of the signals but made their exact location harder to pinpoint. Even with the portable locators, it was not always easy to find a radio-tagged porcupine. With the fixed stations it was hopeless. On June 7, in the first of two internal reports produced over the summer, Gullion and Schwab wrote, “Subject to further testing, it is our belief that under current conditions of forest leafage it will be impossible to obtain trustworthy information on the location and movements of animals carrying radios, based on directional bearings taken from fixed stations.”⁵⁵ By early July they had identified two species of evergreens, white spruce and Norway pine, as most likely to create misleading radio

54 Gordon W. Gullion to Phillip Tichenor, 13 July 1960, Box 5, William H. Marshall Papers, 1947-1982, University of Minnesota Archives, Elmer L. Andersen Library, Minneapolis, MN.

55 Gordon W. Gullion and Robert Schwab, “Plotting Animal Locations by Means of a Miniature Radio,” unpublished manuscript, unpaginated (3pp), 7 June 1960, Box 5, William H. Marshall Papers, 1947-1982, University of Minnesota Archives, Elmer L. Andersen Library, Minneapolis, MN.

signals when combined with the diffusive effects of deciduous foliage. The only solution was to learn the landscape and the equipment well enough that “signal bounce” could be recognized when it occurred, and then to reposition the receiver until several accurate bearings that allowed the location of the transmitter to be triangulated were achieved.⁵⁶ Contrary to the initial plans, which had called for plotting the locations of radio-tagged animals on the basis of triangulation from fixed stations, Gullion and Schwab used the portable receivers primarily to locate the porcupines by sight. This was partly because they wanted to verify the accuracy of the technique, but also because critical details about the porcupines' behavior—their preference for certain kinds of trees, for instance—could only be determined the old-fashioned way: by looking.

In order to use the technique successfully field workers needed to relearn the landscape in terms of its radio properties, while also creating an infrastructure of receiving stations that enabled them to clarify confusing signals. The Grouse Project used a combination of fixed stations, semi-permanent antenna placements, and car-mounted and hand-carried receivers and antennas to gain the best coverage with the least effort. The particular shape that this infrastructure took depended on the local environment: the reflective and absorptive properties of the vegetation, the distribution of roads, the topography of the landscape. The Grouse Project's difficulties with diffuse, echoing signals were exacerbated by its use of high-frequency transmitters (150 million cycles per second, as compared to 27 million cycles per second for most projects), but it was lucky in the sense that the topography of the Cloquet Forest Research Center was nearly flat and the animals it studied had limited mobility. Researchers working in

56 Gordon W. Gullion, “Further Observations on the Problems of Determining the Locations of Animals Carrying Miniature Radio Transmitters,” unpublished manuscript, unpaginated (7pp), 9 July 1960, Box 5, William H. Marshall Papers, 1947-1982, University of Minnesota Archives, Elmer L. Andersen Library, Minneapolis, MN.

mountainous regions or with wide-ranging animals could face similar problems even when using lower-frequency signals, which were less easily reflected and absorbed by vegetation.⁵⁷

After the porcupines were recaptured and their transmitters removed in July 1960, the project entered a period of hibernation that lasted nearly a year. The main reason was Marshall's departure for New Zealand, where he had a Fulbright fellowship to spend ten months studying the ecology of introduced stoats and ferrets.⁵⁸ But the project also needed to regroup after disappointing results of the spring and summer. Back in Minnesota, Gullion and Schwab wrote up a paper on the porcupine tracking while Markusen, the radio technician in Cloquet who had replaced Honeywell as the project's source of engineering expertise, tried to build transmitters that would be light enough for grouse.⁵⁹ Markusen's involvement with the project had begun in April, when he designed the portable receivers that had allowed the team to relocate the tagged grouse that had flown into a tree and had proved essential to understanding the problem of signal bounce in the porcupine study. With his workshop only a short drive from the Cloquet Forest, Markusen was able to participate in the project in a way Honeywell's Seattle-based engineers

57 On the Grouse Project's problems with signal bounce and diffusion, see Gordon W. Gullion and Robert Schwab, "Plotting Animal Locations by Means of a Miniature Radio," unpublished manuscript, unpaginated (3pp), 7 June 1960, and Gordon W. Gullion and Robert Schwab, "Further Observations on the Problems of Determining the Locations of Animals Carrying Miniature Radio Transmitters," unpublished manuscript, unpaginated (7pp), 9 July 1960, Box 5, William H. Marshall Papers, 1947-1982, University of Minnesota Archives, Elmer L. Andersen Library, Minneapolis, MN.

58 Mustelids (members of the weasel family) were seen as important subjects of research because they had decimated certain of New Zealand's local fauna after being introduced via fur farms earlier in the century. Marshall was placed at the Animal Ecology Division of the Division for Scientific and Industrial Research in Wellington; see J. Manuel Espinosa to William H. Marshall, 2 November 1959, Box 2, William H. Marshall Papers, 1947-1982, University of Minnesota Archives, Elmer L. Andersen Library, Minneapolis, MN.

59 William H. Marshall, Gordon W. Gullion, and Robert G. Schwab, "Early Summer Activities of Porcupines as Determined by Radio-Positioning Techniques," *Journal of Wildlife Management* 26 (1962): 75-79. Markusen's initial work on a new radio tag was funded with the several thousand dollars that remained in the National Science Foundation account after the porcupine research had been completed and Honeywell had refused to collaborate further; see Robert Seabloom to William H. Marshall, 9 November 1960, Box 5, William H. Marshall Papers, 1947-1982, University of Minnesota Archives, Elmer L. Andersen Library, Minneapolis, MN.

never had, experiencing field conditions personally and fixing or improving the equipment from day to day.⁶⁰

Relations with Honeywell had gone precipitously downhill in June, when Canfield and other Honeywell employees associated with the project stopped responding to Marshall's letters—letters that became more urgent as the problems with the fixed stations became clearer and as the deadline for submitting a renewal application to NSF approached.⁶¹ It was only in August, when Marshall personally visited Honeywell's main offices in Minnesota, that he learned that Canfield had been transferred to a military project and that the company had decided that wildlife radio-tracking had too little commercial potential to pursue further. After his visit to Honeywell, Marshall wrote a letter to Sprugel explaining the company's withdrawal. “The problem here is that for a company so large this was a pretty small project,” he wrote. “I feel we would have been better off in many ways with a smaller outfit”⁶² Here, as in several other early attempts to develop wildlife radio-tracking equipment, the lone electronics engineer with limited resources but a great deal of flexibility and a close relationship with field workers proved far more useful to biologists than a large corporation, regardless of the technical expertise and resources it could muster.⁶³

Notification of a second round of NSF funding came in the spring of 1961, while Marshall was still in New Zealand. The \$21,000 award was only half of what Marshall had

60 Gordon W. Gullion to William H. Marshall, 7 July 1960, Box 5, William H. Marshall Papers, 1947-1982, University of Minnesota Archives, Elmer L. Andersen Library, Minneapolis, MN.

61 William H. Marshall to Charles Canfield, 1 June 1960, 6 June 1960, and 25 July 1960; William H. Marshall to R. A. Crinkley, 6 June 1960 and 22 July 1960, Box 5, William H. Marshall Papers, 1947-1982, University of Minnesota Archives, Elmer L. Andersen Library, Minneapolis, MN.

62 William H. Marshall to George Sprugel, 22 August 1960, Box 5, William H. Marshall Papers, 1947-1982, University of Minnesota Archives, Elmer L. Andersen Library, Minneapolis, MN.

63 For a contemporary account of the disadvantages of working with large corporations, see Robert J. Jeffries, “The Frontiers of Telemetry Technology,” in *Bio-Telemetry: The Use of Telemetry in Animal Behavior and Physiology in Relation to Ecological Problems*, edited by Lloyd E. Slater (New York: Pergamon, 1963): 65-74.

requested in his application of September 1960, which meant that further work on porcupines, as well as some proposed research on female grouse, would have to be dropped. But it was enough for the main study of male breeding behavior to continue.⁶⁴ In August 1961, a few weeks after his return to Minnesota from New Zealand, Marshall called a meeting at Cloquet of everyone involved with the Grouse Project. They spent the morning discussing the project and the afternoon testing Markusen's new, lighter transmitters out in the field, with promising results.⁶⁵ Testing continued until February, when three grouse were trapped and equipped with transmitters and redesigned harnesses. The weather at Cloquet in February 1962 was typically inhospitable, with temperatures hovering around 0 degrees Fahrenheit, but the new harnesses and transmitters seemed to have little impact on the birds' condition. One of the radio-tagged birds, a juvenile female, was recaptured eight days after its release and found to have a small callus on its left knee where the harness had rubbed against it; otherwise it "appeared robust and healthy and struggled in the manner of a healthy grouse." The fact that it had lost weight was deemed normal given the season. Two days after the bird was re-released, it was killed by an owl which, perhaps disconcerted by the attached electronics, had removed the grouse's head but left the body intact. A post-mortem revealed that the bird was in "normal condition": the flight muscles appeared "normal," the organs appeared "normal," and the crop contained hazel buds and catkins "which are normal foods at this season." The NSF progress report submitted in March concluded that the

64 William H. Marshall to Alexander C. Hodson, 11 April 1961; Alexander C. Hodson and Clinton T. Johnson to George Sprugel, 11 April 1961; William H. Marshall to Alexander C. Hodson, 12 April 1961, Box 5, William H. Marshall Papers, 1947-1982, University of Minnesota Archives, Elmer L. Andersen Library, Minneapolis, MN.

65 Marshall and Seabloom carried the transmitters along roadways near the research center headquarters while the rest of the crew tried to track them using portable locators; John Weigand, Field Notes, Box 5, William H. Marshall Papers, 1947-1982, University of Minnesota Archives, Elmer L. Andersen Library, Minneapolis, MN.

transmitter package allowed grouse “to continue their normal pattern of life.” For the moment, the problem of adjusting the transmitter to the animal seemed to have been solved.⁶⁶

The receiving system, too, was much improved. Gullion and Schwab had ruled out the use of fixed stations because of problems with signal bounce, but relying on portable locators alone, as they had done with the porcupines, also seemed unfeasible. It simply took too much time to search for the transmitters on foot, particularly since the small handheld antennas could pick up signals only at close range. The solution to this dilemma came from researchers at the Illinois Natural History Survey. At the meeting of the American Society of Mammalogists in 1961, one of the graduate students involved in the Grouse Project had heard Rexford D. Lord describe a system of “temporary directional antennas” that he and electrical engineer William W. Cochran were using to track cottontail rabbits. Their idea was to blanket the study area with a network of mid-size antennas—too large for a single person to carry by hand, but not so large that they could not be readily moved—to which receivers could be attached when needed. Once these antennas had been used to triangulate the general location of a radio-tagged bird, hand-held antennas and receivers could be used to pinpoint the exact location, if desired. The system was rounded out by a car-mounted receiver for locating grouse that had strayed beyond the area that could be covered on foot.⁶⁷

66 “Progress Report: Development and Use of Short Wave Radio Transmitters to Trace Animal Movements, August 1960-February 1962,” 1 March 1962, Box 5, William H. Marshall Papers, 1947-1982, University of Minnesota Archives, Elmer L. Andersen Library, Minneapolis, MN.

67 “Progress Report: Development and Use of Short Wave Radio Transmitters to Trace Animal Movements, August 1960-February 1962,” 1 March 1962, Box 5, William H. Marshall Papers, 1947-1982, University of Minnesota Archives, Elmer L. Andersen Library, Minneapolis, MN.

Networks of Expertise

The fact that a crucial set of improvements to the Grouse Project's system came from another research group is an indicator of the accelerating pace of work on wildlife radio-tracking in the early 1960s. From the mid-1950s to the end of the decade, wildlife biologists attempted to use techniques originally developed for remotely monitoring the physiology of jet pilots or for tracking satellites to complement their traditional methods of studying unconfined wild animals in their natural environments. They had limited success, in part because what was being experimented with during this early period was not just the technology—the use of particular circuit designs, attachment methods, and tracking systems—but also the modes of collaboration between wildlife biologists and those who possessed the electronics expertise and resources they needed. Too focused on the abstract capabilities that they hoped the technology would provide, both biologists and engineers lost sight of need to carefully articulate bodies, technologies, landscapes, and institutions. Program officers at agencies such as NSF and ONR were particularly aware of how the technology's development was being hampered by lack of coordination, but so were wildlife biologists who lacked the resources to develop radio-tracking on their own—a category that included the vast majority of those interested in the technique. The first real steps toward more coordination were taken in the spring of 1960 when, at the urging of South Dakota wildlife biologist Jack Seubert, a group of wildlife biologists interested in radio techniques met at the annual meeting of the Wildlife Society in Dallas. Through its publications and meetings, the Wildlife Society helped create a hybrid space in which wildlife biologists, amateur radio experts, and electrical engineers could exchange information. Following the 1960 meeting on radio techniques, the Wildlife Society established a new “radio tracer branch” of its

Wildlife Techniques Committee, which was responsible for producing the society's *Manual of Game Investigational Techniques*.

The Radio Tracer Branch was soon renamed the Radio Tracking Branch, in order to distance it from work on radioactive tracers and to make its affinities with satellite tracking more evident, and then the Wildlife Telemetry Committee, in order to encompass both location tracking and remote physiological monitoring. It consisted of Frank Craighead, Lowell Adams, and Marshall, who served as the committee's chairman. Because of their work on grizzly bears at Yellowstone and their frequent appearance in the pages of *National Geographic* and other national media, Frank Craighead and his brother John Craighead were probably the most high-profile figures among early developers of radio-tracking. Lowell Adams was a biologist affiliated with the University of California at Berkeley who was enthusiastic about wildlife telemetry but never published original research in the field; his main contribution was serving as editor of the telemetry committee's newsletter. The newsletter provided an informal forum for progress reports and descriptions of annual meetings, while two other Wildlife Society publications provided more formal venues for reports on wildlife telemetry. Peer-reviewed methodological and scientific articles appeared in *The Journal of Wildlife Management*, while textbook-like introductions to the topic were published in the editions of the Wildlife Society's techniques manual that appeared in 1963 and 1969.⁶⁸

Despite the success of the Wildlife Society in creating venues favorable to the development of wildlife telemetry, its ability to forge links between wildlife biologists and engineers was limited. Two other organizations were more capable of forging such links: the

68 Henry S. Mosby, ed., *Wildlife Investigational Techniques* (Ann Arbor, MI: Wildlife Society, 1963); Robert H. Giles, Jr., ed., *Wildlife Management Techniques* (Washington, DC: Wildlife Society, 1969).

Office of Naval Research and the American Institute of Biological Sciences. In addition to its early support of work on wildlife telemetry at the American Electronics Laboratories, the Office of Naval Research sponsored a symposium in 1962 that brought together wildlife biologists, laboratory biologists, and electrical engineers to discuss bio-telemetry, a broad area that included the use of telemetry in the lab and in the field to study both humans and animals. The American Institute of Biological Sciences played a smaller role in the initial development of wildlife telemetry than ONR had, but its involvement increased over the course of the 1960s. It co-sponsored several symposiums and published a special issue of its journal *Bioscience* in 1965 that focused on biotelemetry and included a strong focus on wildlife applications. Around the same time it established a BioInstrumentation Advisory Council, headed by engineer John Busser, which partly took over the functions of the Wildlife Telemetry Committee. The Wildlife Telemetry Committee effectively stopped operating in 1966 and was officially disbanded in 1967, when the last issue of its newsletter appeared.⁶⁹

While the Wildlife Society, ONR, and AIBS did much to improve communications among early developers of wildlife telemetry, and ONR funded a significant amount of early work in biotelemetry, the bulk of the funding for wildlife radio-tracking projects came from other institutions. Some money came from the federal government to state-affiliated researchers via the Pittman-Robertson Act, but few wildlife telemetry projects, especially early on when development costs were high, could depend on funding from the wildlife research community

⁶⁹ On the activities of the BioInstrumentation Council, see John Busser, "Instrumentation for Field Biology," *BioScience* 17 (1967): 340-342; John Busser, "Energy Sources and Batteries," *BioScience* 17 (1967): 643-646; John Busser, "Biotelemetry: Pro, Con, and How," *BioScience* 18 (1968): 323-326; John Busser, "The Biotechnician," *BioScience* 18 (1968): 433-435; John Busser, "Some Problems and Some Answers From the 1967 Mailbag," *BioScience* 18 (1968): 41-44; John Busser, "Tools and Techniques for the Marine Biologist," *BioScience* 18 (1968): 957.

alone. Most projects depended on a mix of funding from state and federal wildlife agencies, private philanthropies, federal science agencies (both military and civilian), and universities.

By the mid-1960s, a variety of developments had helped create hybrid spaces within which engineers and biologists could successfully collaborate to develop wildlife telemetry. But the ideal of cooperation was continually in tension with competitiveness among researchers eager to get credit for advancing the technique, as Marshall's hesitant response to Seubert's call for "more coordination" illustrated. After Marshall showed a film about the Grouse Project's field practices at Des Moines in 1962, requests for equipment and design specifications flooded in to Markusen. While Marshall had been happy to share his groups' progress reports with other groups, he urged Markusen not to volunteer his services without payment and not to take on jobs that would interfere with providing equipment for the Grouse Project.⁷⁰ A proposed collaboration to compare the use of different radio-tracking frequencies between Marshall's group and a parallel project at the Cedar Creek Natural History Area seems never to have gotten off the ground. By the mid-1960s, the flow of progress reports sent to the editors of the *Wildlife Telemetry Newsletter* had slowed to a trickle. In part this was a consequence of the technology's maturation, but it is also an indicator that researchers were keeping their technical advances to themselves until they could be published in more prestigious forums.⁷¹

70 On not doing the work for free, see William H. Marshall to Sidney L. Markusen, 1 May 1962; on not allowing it to interfere with Grouse, see William H. Marshall to Sidney L. Markusen, 18 March 18, 1963, Box 5, William H. Marshall Papers, 1947-1982, University of Minnesota Archives, Elmer L. Andersen Library, Minneapolis, MN.

71 In the penultimate issue of the *Wildlife Telemetry Newsletter*, after having received only a few submissions of progress reports or other news items, Marshall wrote: "In your chairman's opinion, these contributions represent an infinitesimal percentage of the total mailing list and of operating telemetry projects known to him by hearsay [I]t is time for workers using radio-telemetry techniques to present data which will stand on scientific merit in recognized society meetings and/or publications"; William H. Marshall, "Closing Editorial," *Wildlife Telemetry Newsletter* 5 (December 1966): 8.

“After the Grouse, the Spouse?”

The Grouse Project attracted national media attention almost even before it had begun. In 1959, James Kimball of the Minnesota Department of Conservation prepared a press release that generated coverage in a number of small papers in Minnesota as well as the Minneapolis and St. Paul dailies and the national sportsman's magazine *Outdoor Life*. The publicity was, Marshall told his partners at Honeywell, “self generating and in fact almost ran away from me,” although Marshall was hardly shy of media attention.⁷² Even before the first radio-tag had been attached, editors from national magazines such as *Life* and *Sports Illustrated* and producers of television programs were planning to send reporters and cameramen to Cloquet to capture the story.⁷³ In the summer of 1960, as the porcupine-tracking study was underway, Marshall cooperated with the public relations staff at the University of Minnesota to conduct a demonstration at the Cloquet Forest Research Center and urged Gullion to provide detailed information about the project to be forwarded on to the press. The announcement inviting the press to Cloquet described the study as “one of the first examples of space-age techniques for wildlife research” and suggested that, though it was being developed initially for ruffed grouse, it would “eventually be used for many other species.”⁷⁴ Much of the resulting coverage emphasized the similarities between the techniques used to track animals and satellites, and a smaller but still significant number noted the possibility that studies of animal orientation and navigation could eventually improve missile

72 William H. Marshall to D.R. Bishop, 25 August 25, Box 5, William H. Marshall Papers, 1947-1982, University of Minnesota Archives, Elmer L. Andersen Library, Minneapolis, MN.

73 William H. Marshall to Milton Stenlund, 24 March 1960, Box 5, William H. Marshall Papers, 1947-1982, University of Minnesota Archives, Elmer L. Andersen Library, Minneapolis, MN.

74 Phillip J. Tichenor to William H. Marshall, 7 July 1960; Gordon W. Gullion to Phillip J. Tichenor, 11 July 1960; Phillip J. Tichenor, Press Release for Demonstration on July 20, 1960, 13 July 1960; Phillip J. Tichenor to Gordon W. Gullion, 13 July 1960; Box 5, William H. Marshall Papers, 1947-1982, University of Minnesota Archives, Elmer L. Andersen Library, Minneapolis, MN.

guidance systems, a theme that ONR's Sidney Galler often emphasized.⁷⁵ The Minneapolis-Honeywell Regulator Company reported in its company magazine that the project's techniques were "similar to those used in tracking missiles and orbiting space vehicles."⁷⁶

Most coverage was positive, but expressions of unease about the possible implications of the technology were common as well. In many cases, the Grouse Project was presented as amusing trivia, nothing more than a striking photograph and a brief caption, but even then the implications were not entirely positive. Above the caption "Every Right to Grouse," the *Columbia* (South Carolina) *Record* published a photograph of Marshall holding a radio-tagged grouse with a black hood over its head.⁷⁷ Many commentators found the transition from technologies for monitoring wildlife to technologies for monitoring humans easy to make. Even supporters found it hard to avoid commenting on the thin line between animal and human surveillance. After visiting Cloquet in April 1960, Orville Freeman, the conservation-minded governor of Minnesota, joked that the project represented an "invasion of privacy."⁷⁸ Tongue-in-cheek columns and editorials expressed concern that suspicious housewives would soon be using the technique to keep tabs on wayward sons and unfaithful husbands. In 1960, for example, Readers Digest reprinted an editorial from the Toronto *Financial Post* titled, "After the Grouse, the Spouse?" which had been written in response to a UPI wire report about the Grouse Project. "It's a well-known fact that scientists first try out on animals those things eventually destined for humans," the *Financial Post's* editorial staff wrote. "It's only a matter of time until some entrepreneur offers housewives a similar contraption that can easily be attached to wandering

75 For an example of improved missile guidance as one of the benefits of animal tracking, see Jack Wilson, "Secrets of the Birds May Guide Missiles," *Minneapolis Tribune*, 7 September 1961.

76 "Tiny Space-Age Radios Track Grouse," *Honeywell World*, 16 May 1960.

77 "Every Right to Grouse," *Columbia* (South Carolina) *Record*, 6 June 1960.

78 "What's Up by Rupp: Freeman Views Project as 'Invasion of Privacy'," *Duluth News-Tribune*, 24 April 1960.

husbands. Just think of the little woman crouched over her monitoring set tracing the incoming beeps as her husband orbits around town.”⁷⁹ In one cartoon published alongside an editorial in the *St. Paul Dispatch* by Lewis Patterson in March 1960, a transmitter-equipped male grouse confessed to a female companion, “Can't get over this feeling I'm being watched. Hope it isn't my wife!”⁸⁰ In 1963, responding to a proposal by Warner to track wildlife by satellite, Patterson found the Orwellian implications of such a system hard to ignore: “[I]t appears the animal kingdom is about to have a Big Brother in the sky.”⁸¹ Such themes continued to reappear in the popular press even after the initial novelty of radio-tracking had faded. In 1969, responding to the announcement of an attempt to track an elk (named Moe) using the Nimbus 3 satellite, the editorial page of the *Washington Post* echoed Patterson's Orwellian concerns: “No benefit, however, comes without its disadvantages. If Nimbus 3 can tell all about Moe, Nimbus 10 or 50 or 100 can tell all about a ship or an airplane or, for that matter, a person. All it would take would be a bug attached in the right place. Thus, while Moe is clearly a Very Important Elk, we're not sure whether to hail him if he lives up to his assignment (and brings 1984 a step closer) or blows it (and postpones that day a bit longer).”⁸²

The sport-hunting press carried some positive coverage of the new technique; in August 1959, for example, *Outdoor Life* published a short, glowing account of the Grouse Project and other radio-tracking projects in South Dakota, Montana, and Ontario, echoing scientists' claims

79 “After the Grouse, the Spouse?” *Toronto Financial Post*, n.d., reprinted in *Reader's Digest*, Box 5, William H. Marshall Papers, 1947-1982, University of Minnesota Archives, Elmer L. Andersen Library, Minneapolis, MN.

80 Lewis Patterson, “Wiretapping of Grouse to Bare Intimate Secrets,” *St. Paul Dispatch*, March 25, 1960; “Where Did You Go? Out,” *Richmond News Leader*, July 19, 1960.

81 Lewis Patterson, “U Prof Envisions Satellite to Track Animal Migration,” *St. Paul Dispatch*, 22 March 1963. Patterson's article erroneously claimed that it was the October 1962 snow geese study that had cemented the need for satellite tracking in Warner's mind. In fact, Warner had submitted a preliminary proposal to NASA in April, some six months earlier. In 1963, mentions of tracking animals by satellite appeared in a number of general articles on wildlife radio-tracking in the popular press; see, e.g., “Tracking Animals by Radio,” *Current Science* 48 (May 1963): 247; “Wildlife at the Mike,” *Read Magazine* 13 (September 1963): 24-25.

82 “Moe the Bugged Elk,” *Washington Post*, 12 April 1969.

that the “basic purpose is to increase the game supply and improve hunting.”⁸³ But many writers were skeptical that such research was a wise way to spend the excise taxes and license fees gathered from hunters. In July 1962, Olin L. Kaupanger of the Minnesota Emergency Conservation Committee—a sport-hunting group that often opposed the policies of the Minnesota Department of Conservation—wrote to Kimball to express his opinion about the uselessness of academic research into wildlife, including “radio-sending devices.”⁸⁴ That December, the Minneapolis *Star Tribune* published a column that included criticisms of the grouse-tracking project as part of a broader critique of the ignorance of wildlife biologists: “Grouse can't be helped under any known program (the biologists' radio experiment to track grouse failed) so not much can be done there.”⁸⁵ Marshall protested to the managing editor, with the result that a more positive column by Kimball of the Minnesota Department of Conservation was published in March 1963 defending the importance of research in general and the Grouse Project in particular: “Infinitely more important than this particular study is the development of radio-telemetry, a technique which can lead to better understanding the intricate lives of wild animals. Progress is based on knowledge, whether it be in the field of rocketry or wildlife management.”⁸⁶

83 Ben East, “More Beep-Beep,” *Outdoor Life* 124 (August 1959): 26. East also ghost-wrote an article on the grouse project that was published as William H. Marshall, “What's On Your Mind? Radio Tracking of Grouse,” *Outdoor Life* 126 (August 1960): 4. East thanked Marshall “for your willingness to use your name on the letter” in Ben East to William H. Marshall, 7 June 1960, Box 5, William H. Marshall Papers, 1947-1982, University of Minnesota Archives, Elmer L. Andersen Library, Minneapolis, MN.

84 Olin L. Kaupanger to James W. Kimball, 23 July 1962, Marshall, Box 5

85 The article by Jim Peterson, which appeared on December 29, 1962, was quoted in correspondence between Marshall and the *Star Tribune*'s managing editor; William H. Marshall to Daryle M. Feldmeir, 26 March 1963; Daryle M. Feldmeir to William H. Marshall, 27 March 1963, Box 5, William H. Marshall Papers, 1947-1982, University of Minnesota Archives, Elmer L. Andersen Library, Minneapolis, MN.

86 Jim Kimball, “Electronics Permits Intimate Insights Into Animal Life,” *Minneapolis Star Tribune*, 22 March 1963.

“Nothing More Than Natural History”

Because radio-tracking sometimes provided information that was impossible to gather by any other means, wildlife biologists were never able to dismiss entirely the question of whether the tagged animals were behaving normally. They were trapped in an “experimenter's regress” with few or no ways of independently validating their results.⁸⁷ In trying to ensure that animals were still behaving naturally, on the one hand, but that the attachments were secure enough not to be removed, on the other, scientists were forced to partly reinvent radio-tracking for each species.⁸⁸ Not all such reinventions were successful. In the early 1960s, the Grouse Project was still seen as one of the projects most likely to lead to scientifically significant results.⁸⁹ By the spring of 1962, the combination of lighter, better-fitted transmitters and a flexible, multi-faceted infrastructure for receiving radio signals enabled research to begin in earnest. Over the next three years, until the termination of field work in the fall of 1965, a rotating crew of graduate students and research assistants used the radio-tracking system to study male breeding territories, winter movements and food sources, female nesting behavior, and the break-up and dispersal of broods.⁹⁰ By the end of 1963 the transmitter package weight was down to about 20 grams and the harness had been significantly improved. In the spring of 1960, the stiff, vertical antennas that had initially been supplied by Honeywell had been replaced with flexible, whip-like wires. These new antennas proved less likely to break or to get caught on vegetation, and they produced an

87 On the experimenter's regress, see Harry M. Collins, *Changing Order: Replication and Induction in Scientific Practice* (Chicago: University of Chicago Press, 1992).

88 L. David Mech, Valerian B. Kuechle, Dwain W. Warner, and John R. Tester, “A Collar for Attaching Radio Transmitters to Rabbits, Hares, and Raccoons,” *Journal of Wildlife Management* 29 (1965): 898-902.

89 Lowell Adams to John Olive, 30 December 1963, Box 3, William H. Marshall Papers, 1947-1982, University of Minnesota Archives, Elmer L. Andersen Library, Minneapolis, MN.

90 “Final Report—Development and Use of Radio Telemetry Techniques to Study Ruffed Grouse,” Box 5, William H. Marshall Papers, 1947-1982, University of Minnesota Archives, Elmer L. Andersen Library, Minneapolis, MN.

additional, unexpected benefit. When a radio-tagged grouse moved, the wire changed shape, causing the signal strength to vary. These variations could be used to determine from the radio signal alone whether the animal was resting, walking, flying, feeding, or drumming.⁹¹

Even though the new harness and transmitter package were significant improvements over the old, not all grouse tolerated them well. In fact, one result of the radio-tagging project was a new kind of psychologization of individual grouse. The Grouse Project progress report to the National Science Foundation for 1963 noted that, out of 15 birds radio-tagged that year, three “exhibited an abnormal behavior which was quite probably due to an aversion for the packages they carried.” Bird #1817, a juvenile female, moved erratically after being tagged on March 4, 1963, refusing to flush from a dense stand of balsam fir for three days and then returning to the area where it had been trapped. By March 22, when it was recaptured and its transmitter removed, it had lost more than 15 percent of its original weight. “It is our opinion that #1817 is a bird who is behaviorally unsuited for carrying a radio,” the report concluded. This new category of “behaviorally unsuited” birds allowed Marshall and his students to cordon off, for the purposes of data analysis, those animals that were clearly affected by the instrumentation from those that “appeared normal.” But it begged the question of whether the “normal” birds also were being affected by the harnesses, only in more subtle ways.⁹²

If individual grouse sometimes created unexpected challenges for data collection, so did the grouse population as a whole. In the winter of 1963-1964, the birds became so scarce at Cloquet that they were nearly impossible to trap. Rather than let students and equipment go idle

91 William H. Marshall and John J. Kupa, “Development of Radio-Telemetry Techniques for Ruffed Grouse Studies,” *Transactions of the North American Wildlife and Natural Resources Conference* 28 (1963): 443-456.

92 “Progress Report: Studies of Movements, Behavior and Activities of Ruffed Grouse Using Radio Telemetry Techniques, 1963,” 20, Box 5, William H. Marshall Papers, 1947-1982, University of Minnesota Archives, Elmer L. Andersen Library, Minneapolis, MN.

until the population recovered, Marshall endorsed a plan to study animals that, until then, had been significant to the Grouse Project only because of their proclivity for entering traps and trampling any grouse unlucky enough to be inside. Between February and May 1964, John Tilton, a research assistant on the project, captured, radio-tagged, and tracked sixteen snowshoe hares. The results of his efforts were disappointing. The hares confined themselves to relatively small ranges, covered most of their range within hours, and were most often discovered hiding under fallen trees and brush. In contrast to grouse, it proved impossible to use variations in the strength of the signal to determine the hares' behavior remotely. Resting, walking, and feeding all sounded alike. In March, the hares' summer pelage started to come in and they chewed off the modified plastic grouse harnesses that had been used to attach the transmitters, forcing Tilton to re-trap and re-tag them. This detour into snowshoe ecology confirmed that the technique could be adapted to species other than grouse and porcupines, but it also showed that the results were not always worth the effort.⁹³

The basic elements of the receiving system remained unchanged through 1965, but as the team's research goals shifted, so did the importance of each component. A study of the break-up of grouse broods in the summer and fall of 1964 forced Geoffrey Godfrey, the graduate student in charge of the study, to work in an area of the forest that was largely inaccessible by car. That rendered the car-mounted receiver unusable, but it also made it difficult both to set up and to access the 20-foot-tall temporary antennas. Consequently Godfrey depended almost entirely on handheld receivers, using a sparse, wide-spread network of temporary antennas only as a back-up system to relocate members of the brood after they had dispersed.⁹⁴

93 "Progress Report: Studies of Movements, Behavior and Activities of Ruffed Grouse Using Radio Telemetry Techniques, 1964," Box 5, William H. Marshall Papers, 1947-1982, University of Minnesota Archives, Elmer L. Andersen Library, Minneapolis, MN.

94 Ibid.

After the initial National Science Foundation grant, which covered the period from July 1959 to June 1960, Marshall had received two additional awards to fund the Grousar Project: one for \$21,000 for the period from July 1961 to June 1963, and another for the period from July 1963 to June 1965. None of the proposals upon which these awards were based had been particularly well fleshed-out—Sprugel had demanded a supplement to the proposal in 1963—but, with Sprugel's support, funding had always come through.⁹⁵ By 1965, however, the situation had changed. As the result of a new NSF policy of rotating program directors, Sprugel had left for a position in the National Park Service in the fall of 1964 to be replaced by John S. Rankin, Jr., a marine biologist with no special commitment to Marshall or the Grousar Project.⁹⁶ Other things had changed as well. Radio-tracking was an increasingly well-established technique, and it was hard to argue that the research taking place at Cloquet was groundbreaking, especially with a more technically sophisticated project underway virtually next door at the Cedar Creek Natural History Area. Finally, six years of NSF funding for the Grousar Project had produced relatively few peer-reviewed publications, most of them concerned with the development of the technique rather than with new scientific findings.

These were the surface issues that contributes to the end of the project, but there were also deeper problems. The anonymous reviewers of the Marshall's 1965 renewal application criticized the project on several grounds: its paucity of scientific findings, its use of an expensive technology where simpler ones would have sufficed—the reviewer suggested binoculars—and the sloppiness of the proposals themselves. But the reviewers' most damning criticism was that

⁹⁵ George Sprugel to William H. Marshall, 16 January 1963; William H. Marshall to George Sprugel, 22 January 1963, Box 5, William H. Marshall Papers, 1947-1982, University of Minnesota Archives, Elmer L. Andersen Library, Minneapolis, MN.

⁹⁶ Josephine K. Doherty to William H. Marshall, 7 October 1964; John S. Rankin to William H. Marshall, 27 August 1965, Box 5, William H. Marshall Papers, 1947-1982, University of Minnesota Archives, Elmer L. Andersen Library, Minneapolis, MN.

the project was not really science at all. “This is nothing more than natural history,” one wrote. Another wondered why the Fish and Wildlife Service was not funding the project, which clearly had important implications for wildlife management—the implication being that this was thus not the kind of “basic science” NSF should be funding. All three reviewers recommended against renewal.⁹⁷ Official notification of the decision arrived in early April 1965.⁹⁸ This turn of events seems to have taken Marshall by surprise. He continued to see the study as a pioneering effort with important scientific implications and tried unsuccessfully to convince NSF to reconsider its decision. Trapping and radio-tagging continued through the summer, but by mid-August there was no money left in the account to pay the salaries of graduate students and research assistants, which had always been the project's biggest expense. The last day of NSF-funded fieldwork took place on August 13, 1965.⁹⁹

Marshall was not quite ready to let go, however. In September he wrote Gullion to ask him for his opinion about resubmitting the proposal to NSF. Gullion's reply was ambivalent but clear: While he would be willing to participate if the grant were funded, his personal feeling was that the project could benefit from some time off. Too much data had been collected but not analyzed; no one really knew what had or had not been accomplished.¹⁰⁰ More importantly, Gullion had begun to doubt the validity of much of the data collected by the project. As early as May 1960, in a letter to Marshall about the death of the second radio-tagged grouse—the one

97 “Proposal Evaluation Sheet,” Box 5, William H. Marshall Papers, 1947-1982, University of Minnesota Archives, Elmer L. Andersen Library, Minneapolis, MN.

98 Harve J. Carlson to William H. Marshall, 1 April 1965, Box 5, William H. Marshall Papers, 1947-1982, University of Minnesota Archives, Elmer L. Andersen Library, Minneapolis, MN.

99 “Progress Report: Studies of Movements, Behavior and Activities of Ruffed Grouse Using Radio Telemetry Techniques, 1965,” Box 5, William H. Marshall Papers, 1947-1982, University of Minnesota Archives, Elmer L. Andersen Library, Minneapolis, MN.

100 Gordon W. Gullion to William H. Marshall, 10 September 1965, Box 5, William H. Marshall Papers, 1947-1982, University of Minnesota Archives, Elmer L. Andersen Library, Minneapolis, MN.

that had flown into a tree—Gullion had expressed serious concerns about the effects of trapping and tagging on the grouse population as a whole. “The fact that our populations have remained relatively stable for three years here on the forest may be the direct effect of 'too many biologists', and have nothing to do with forestry practices or other normal population behavior,” he wrote.¹⁰¹ In the fall of 1965, the same concerns resurfaced. In his initial response to Marshall's suggestion of resubmitting the grant, written in early September, Gullion limited himself to questioning the use of radio-tracking to study drumming males, many of which seemed to drum less frequently after being tagged. But in a second letter, written in October after one of Marshall's graduate students proposed an extensive radio-tracking study at Cloquet, he was more critical, wondering “just how much experimenting can be done with birds in a population where we are attempting to measure the response of the total population to changes in the forest environment? Normal trapping and banding raises the loss rate appreciably, and the additional handling and retrapping attendant to the radio technique increases this even more, then there is the problem that a significant proportion of the birds (mostly males) will not carry the radios. It boils down to simply a matter of whether we want to learn a good deal about grouse life history at the expense of evaluating their overall responses to forest management, or is the effect of forest management on the population our major concern?”¹⁰² Radio-tracking might be a useful tool for studying the individual lives and behaviors of ruffed grouse, Gullion argued, but not for understanding the relationship between grouse populations and forest management practices. Gullion's response seems to have dampened Marshall's enthusiasm for continuing the study.

Instead he completed his final report to NSF and urging his students to turn the data that were

101 Gordon W. Gullion to William H. Marshall, 29 May 1960, Box 5, William H. Marshall Papers, 1947-1982, University of Minnesota Archives, Elmer L. Andersen Library, Minneapolis, MN.

102 Gordon W. Gullion to William H. Marshall, 4 October 1965, Box 3, William H. Marshall Papers, 1947-1982, University of Minnesota Archives, Elmer L. Andersen Library, Minneapolis, MN.

still “locked up in thesis” into publishable articles.¹⁰³ The official end came in May 1966, when Marshall received word that the project's university account had been terminated.¹⁰⁴

Between 1959 and 1966, Marshall, Gullion and the other field workers at the Cloquet Forest Research Center transformed radio-tracking from a theoretical possibility into a practical field technique, significantly altering the original vision in the process. What had appeared in Marshall's initial NSF grant proposal of January 1959 as a technique for producing quantitative, objective knowledge about animal numbers and movements had been transformed over the course of six years into a way of becoming, in Marshall's words, “intimately acquainted” with an animal, its habitat, and the complex relations between them. As he wrote in his final report to the National Science Foundation, “The system has one basic attribute. The biologist becomes, in a very real sense, intimately acquainted with the animal carrying the transmitter and also with the habitat it is occupying, as the work proceeds. Thus, he can do a great deal of qualitative interpretation on the spot adding to his understanding of the complex conditions encountered in the field.¹⁰⁵ Instead of freeing Marshall and his students from the need to deal with the idiosyncrasies of the study organism and the environment in which it lived, radio-tracking had generated a new set of requirements, ranging from the need to adapt the transmitter package to the study organism to the need to learn how radio signals propagated across the landscape. Ironically, in the specific case of the Grouse Project, the transformations required to make the technique work had convinced both internal critics, such as Gullion, and external critics, such as

103 William H. Marshall to John Kupa, Robert Brander, Geoffrey Godfrey, Ronald Barrett, and Philip Schladweiler, 5 April 1966, Box 5, William H. Marshall Papers, 1947-1982, University of Minnesota Archives, Elmer L. Andersen Library, Minneapolis, MN.

104 “Notice of Termination of Account,” 18 May 1966, Box 5, William H. Marshall Papers, 1947-1982, University of Minnesota Archives, Elmer L. Andersen Library, Minneapolis, MN.

105 “Final Report—Development and Use of Radio Telemetry Techniques to Study Ruffed Grouse,” p. 5, Box 5, William H. Marshall Papers, 1947-1982, University of Minnesota Archives, Elmer L. Andersen Library, Minneapolis, MN.

the NSF reviewers, that it was not worth supporting. In scientific terms, they were most likely right. The project was not, however, a failure, nor did Marshall see it as such.¹⁰⁶ In his final report to NSF, submitted in February 1967, Marshall stressed the usefulness of the technique for “attack[ing] a specific phase in life history or one aspect of the ecological and behavioral problems involved” and pointed as evidence to new discoveries about the winter movements, breeding behavior, nesting behavior, and brood break-up and dispersal of ruffed grouse. But for the most part he emphasized technical developments rather than scientific findings, noting that the equipment developed by the project was being used by fifteen other research groups, and that several hundred copies of each progress report had been sent to researchers in the U.S. and other countries.¹⁰⁷

Marshall's dreams of transcendence quickly dissolved in the reality of intimate acquaintance and embodiment required to make the technique of wildlife radio-telemetry work with specific wild animals under real field conditions. The result was not a return to the “antiquated” techniques of observational natural history Marshall had hoped radio-tracking would replace, but rather the creation of new, privileged, technologically-mediated relationship between wildlife biologists and the animals they studied. Seeking to remove themselves from the scene of the action in order to gain an objective “view from nowhere,” to manage nature so that it would represent a slice of Eden or a “vignette of primitive America,” wildlife biologist instead established new connections, new capacities, and new entanglements with local landscapes and

106 “This particular radio-telemetry system is now being used in at least a dozen other projects in the country and I feel that the National Science Foundation support has really been a very important factor in the development of this technique in ecological research,” he wrote; William H. Marshall to Harve J. Carlson, 13 April 1965, Box 5, William H. Marshall Papers, 1947-1982, University of Minnesota Archives, Elmer L. Andersen Library, Minneapolis, MN.

107 “Progress Report: Studies of Movements, Behavior and Activities of Ruffed Grouse Using Radio Telemetry Techniques, 1965,” 4, Box 5, William H. Marshall Papers, 1947-1982, University of Minnesota Archives, Elmer L. Andersen Library, Minneapolis, MN.

individual bodies. In order to preserve the possibility of a primal encounter with nature and wildness for hunters in Minnesota's north woods, Marshall and his colleagues became experts in designing backpacks for grouse and predicting the way radio waves would diffract around stands of Norway pine.¹⁰⁸

108 Bruno Latour, *We Have Never Been Modern*, translated by Catherine Porter (Cambridge, MA: Harvard University Press, 1993).

3

THE POETRY OF WILDERNESS **TRACKING GRIZZLY BEARS IN YELLOWSTONE, 1959-1972**

Hayden Valley lies near the geographical center of Yellowstone National Park along a portion of the Great Loop Road that connects the Grand Canyon of the Yellowstone to Yellowstone Lake. It is known for its abundant wildlife, but when I visited the valley on an overcast day late in the spring of 2007 I was looking for something less scenic: the remains of the old Trout Creek garbage dump, which had been closed in the early 1970s as part of a campaign to reduce the dependence of the park's grizzly bears on human handouts. Despite finding the disused and overgrown gravel road that once led to the dump—and in it, a pull tab from a 1960s-era soda can that testified to its former function—my hiking companions and I saw neither grizzly bears nor garbage. A few miles down the Great Loop Road, however, we encountered a crowd gathering at the Mud Volcano pullout to gawk at a young male grizzly who had chosen a patch of grass near the pullout's boardwalk as a resting spot. Peering around the nature photographers with massive, tripod-mounted telephoto lenses who had pushed their way to the front of the crowd, we could see that the grizzly was wearing a radio collar. As the park ranger who arrived on the scene a few minutes later explained, the bear was a repeat offender whose excessive fondness for scenic geysers and other places where humans gathered required the careful monitoring that the collar made possible. Keeping a wary eye on the bear while urging us back from the edge of the boardwalk toward where “the safer people are,” the park ranger was enforcing a separation between human and wild that had become increasingly rigid over the course of the twentieth century, as Alice Wondrak Biel and other historians of the

national parks have shown.¹ In order to maintain the wildness and naturalness of the Yellowstone grizzly population under conditions of increasing human visitorship, both bears and humans have been subjected to an array of disciplinary technologies. Humans that transgress the dividing line that separates them from wild nature by feeding, harassing, or killing the bears are subject to warnings, fines, eviction from the park, and criminal charges; bears that transgress the same line are trapped, tagged, relocated, monitored, and, if they prove to be dangerous recidivists, killed. Since its introduction to the park in the early 1960s, radio-tracking has been a central piece of this program for producing and disciplining the wild.

By most measures, this approach has been successful. Just a few months before my visit to the park, the U.S. Fish and Wildlife Service announced that the Yellowstone grizzly population had recovered to the point that it would no longer be considered “threatened” under the Endangered Species Act, as it had been since 1975.² It has not, however, been uncontested. As we drove away from the Mud Volcano, one of my hiking companions mentioned that he would have much preferred seeing a grizzly during our hike in Hayden Valley than at the pullout. Peering over a wooden railing in the midst of a crowd at a bear with a collar, we might as well, he pointed out, have been at zoo. My friend's concerns reminded me of a letter from wildlife biologist Adolph Murie that I had found found a week earlier in the archives of the American Heritage Center at the University of Wyoming. In 1962, the Park Service leadership asked Murie for his opinion about a proposal to radio-collar grizzlies in Mount McKinley National Park, the

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- 1 Alice Wondrak Biel, *Do (Not) Feed the Bears: The Fitful History of Wildlife and Tourists in Yellowstone* (Lawrence: University of Kansas Press, 2006). See also Richard West Sellars, *Preserving Nature in the National Parks: A History* (New Haven: Yale University Press, 1997); James A. Pritchard, *Preserving Yellowstone's Natural Conditions: Science and the Perception of Nature* (Lincoln: University of Nebraska Press, 1999).
 - 2 U.S. Fish and Wildlife Service, “Grizzly Bears; Yellowstone Distinct Population; Notice of Petition Finding; Final Rule,” *Federal Register* 72 (29 March 2007): 14866-14938, http://www.fws.gov/mountain%2Dprairie/species/mammals/grizzly/FR_Final_YGB_rule_03292007.pdf, retrieved on 28 March 2008.

spectacular wildlife and wilderness reserve in central Alaska that would be expanded and renamed Denali National Park and Preserve in 1980. Murie's response was to condemn the proposal in the strongest terms, as he continued to do whenever similar proposals came to his attention until his death in 1974. Much of his opposition came from having witnessed the use of wildlife radio-tracking in Yellowstone by the twin brothers Frank and John Craighead, who had begun an aggressive research program of trapping, tranquilizing, and tagging the park's bears in 1960. "When we think of Yellowstone grizzlies we do not think of wilderness animals, but rather of radios, anesthetized bears, and general manhandling," he explained to the superintendent of Oregon's Crater Lake National Park, where he was then stationed.³ To allow the same thing to happen to Denali's bears would, as he told the Park Service's chief scientist several years later, "destroy the very essence of the poetry in wilderness, for the grizzly, along with the wolf, is a symbol of wild country."⁴ As Gregg Mitman has shown, Murie's concerns about the transformation of the national parks into tightly managed, zoo-like enclosures, which he shared with his brother and fellow wildlife biologist and wilderness advocate Olaus Murie, reflected an aesthetic that had been nurtured during the interwar years—an aesthetic in which the disciplinary technologies of postwar wildlife managers had no place.⁵ For the Craighead brothers, in contrast, who were also active members of the wilderness movement, these technologies were just what was needed to defend wildlife against an increasingly intrusive industrial society. At least partly for political reasons, such disagreements over the meaning and management of wilderness were

3 Adolph Murie to Regional Chief of Interpretation through Superintendent of Crater Lake, 5 March 1962, Box 2, Murie Family Papers, Series I: Adolph Murie Files, 1834-1982, American Heritage Center, University of Wyoming, Laramie.

4 Adolph Murie to George Sprugel, 11 January 1965, Box 2, Murie Family Papers, Series I: Adolph Murie Files, 1834-1982, American Heritage Center, University of Wyoming, Laramie.

5 Gregg Mitman, "When Nature *Is* the Zoo: Vision and Power in the Art and Science of Natural History," *Osiris* 11 (1996): 117-143.

subdued in the decade preceding the passage of the Wilderness Act of 1964, but in the decade following it they became the subject of intense, sometimes bitter, and often very public debate.

One of the most bitter and public of these debates was over the management of Yellowstone's grizzlies—in particular, over the Park Service's decision in the late 1960s to shut down Trout Creek and other garbage dumps within the park where bears had been gathering to feed during the summer months for decades. The decision was in tune with the Park Service's broader turn toward “natural management,” but in the Craigheads' eyes a sudden closure of the dumps risked driving an already-fragile grizzly population to extinction. They believed that the bears' search for alternative food sources would bring them to picnic areas and roadsides, where encounters with humans were more likely to have fatal outcomes than at the dumps, to which only scientists and park employees had access. Unable to convince the Park Service to phase out the dumps slowly, the Craigheads brought their case to the popular press, where their use of radio-tracking helped bolster their claims to being the world's top grizzly experts. Since the Park Service had invited them into Yellowstone to study grizzlies in 1959, the Craigheads had used traps, tranquilizers, ear-tags, colored tassels, radio-collars and other hands-on research methods to track the movements of bears throughout the greater Yellowstone ecosystem. As in other early radio-tracking projects, the Craigheads' initial hopes of using radio-tracking to obtain a transcendent vision of nature had quickly given way to a recognition that the technique's great strength was its ability to produce an intimate, technologically-mediated, situated-but-mobile relationship between wildlife biologists and wild animals. The Craigheads' highly visible tags and collars both enacted and symbolized their unique relationship with Yellowstone's grizzlies.

In the context of the popular environmental movement of the late 1960s and under the uncontrolled field conditions of a national park, however, the material basis of the Craigheads' intimate relationship to grizzlies became a political vulnerability as well as a source of epistemological authority. In the late 1960s, resenting the Craigheads' attempts to undermine Park Service policy, the Yellowstone administration began attacking their use of wildlife tags as a violation of national park ideals, just as Murie had at the beginning of the decade. In the early 1970s, as the crisis reached its peak, Yellowstone superintendent Jack Anderson ordered rangers to begin removing the Craigheads' tags. With each tag that was removed, the Craigheads' claim to having a uniquely intimate knowledge of the park's grizzlies was weakened.

The Park Service's ability to undermine the Craigheads' epistemological authority by removing wildlife tags was made possible by a wilderness movement that privileged an aesthetic of naturalness and a hands-off approach to wilderness management. After the passage of the Endangered Species Act of 1973 and the listing of the Yellowstone grizzly population as “threatened” in 1975, however, the winds shifted. The Endangered Species Act gave enormous rhetorical, financial, and legal support to the claim that the prevention of species extinction was one of the primary responsibilities of the Park Service and other federal land management agencies. The kinds of aesthetic and spiritual values that had motivated Murie's opposition to radio-tagging and that Anderson and other Park Service leaders had used to terminate the Craigheads' research in Yellowstone paled next to arguments that “extinction is forever.” Although the Craigheads never resumed their work in the park, radio-tagging had made a full return by the mid-1970s under the auspices of the Interagency Grizzly Bear Study Team, a cooperative effort among relevant federal and state agencies. Even in Mount McKinley National

Park, where Murie had vociferously and successfully opposed wildlife tagging until his death in 1974, a radio-tagging project of caribou was approved in 1977. As in Yellowstone, the key arguments in favor of tagging in McKinley were framed in biological terms of endangerment and extinction rather than in terms of spiritual or aesthetic values. Today radio-tagging is used throughout the national park and wilderness systems and the fact that a Park Service biologist and wilderness advocate as prominent as Adolph Murie passionately opposed it has been forgotten and even actively obscured. When I visited Denali's Murie Science and Learning Center a few weeks after seeing the radio-collared bear in Yellowstone, I found an exhibit celebrating present-day biologists' use of radio-tagging to study the park's wolves as a direct continuation of Murie's legacy of scientific research on wildlife. Needless to say, the fact that Murie had seen tagging as a "blemish on the wilderness" went unnoted.⁶

Artificial Conditions in the National Parks

The Craigheads had begun their careers in the late 1930s by publishing an article on falconry in *National Geographic Magazine*. They were charismatic, photogenic, and equally enthusiastic about the science of wildlife biology and the frontier lifestyle, both of which they promoted in a series of articles in *National Geographic* during the 1950s and 1960s.⁷ While Frank Craighead was itinerant, moving from position to position with funding from various

6 In a 2005 article in the *International Journal of Wilderness*, Ingrid Nixon, education coordinator for the Murie Science and Learning Center, touted the center's use of high technology for education and research, including a wireless network that extended along 40 miles of the park highway and made it possible "to communicate from the field ... to anywhere in the world via the Internet." In addition, with support from the Office of Naval Research, the park was developing radio-tracking collars that would make it possible "to track wildlife 24 hours per day, 7 days a week." Like the exhibits I saw in the summer of 2007, Nixon's article represented technological developments that Murie had passionately opposed during his lifetime as the continuation of Murie's own work; Ingrid Nixon, "Science and Learning the Alaska Wilderness," *International Journal of Wilderness* 11 (August 2005): 35-36, on 35.

7 Frank Craighead and John Craighead, "Wildlife Adventuring in Jackson Hole," *National Geographic Magazine* 109 (January 1956): 1-36.

sources over the course of his career, John Craighead settled down in the Northern Rockies, becoming director of the Montana Cooperative Wildlife Research Unit in 1952. Like Dwain Warner and William Marshall in Minnesota, the Craigheads saw space-age technology as a solution to problems of accuracy, objectivity, and credibility in wildlife biology. They embraced and invented new hands-on research techniques ranging from dyeing to tagging to tattooing as means of achieving a quantitative, objective survey of animal behaviors that had hitherto been studied only impressionistically. Some of their technical innovations and much of their philosophical approach to wildlife management seems to have come directly from their experience of falconry, a sport which they helped to revive in the United States, in which the instincts of a wild bird are temporarily redirected to suit the purposes of a human master.⁸ Although the Craigheads embraced the metaphor of the “laboratory” for describing their field sites, they were also passionate advocates of wilderness preservation, and it was the figure of the falcon—a animal that remains wild despite human control—that reconciled the two.⁹

Over the course of the 1950s, John Craighead discussed the possibility of conducting a broad-ranging study of grizzly bears with state fish and game agencies and with the National Park Service, culminating in a decision to focus the study on Yellowstone National Park in 1959. By inviting the Craighead twins into Yellowstone, the Park Service was creating an opening for the philosophies and practices of the new, technologically-oriented wildlife management in its

8 One of their colored neckbands for identifying geese was based on a falconer's knot, as was an ear-tag they later applied to grizzlies; see John J. Craighead and Dwight S. Stockstad, “A Colored Neckband for Marking Birds,” *Journal of Wildlife Management* 20 (1956): 331-332; John J. Craighead and Dwight S. Stockstad, “Color Marker for Big Game,” *Journal of Wildlife Management* 24 (1960): 435-438. The use of the jess-type tag for grizzlies was discussed in a meeting on June 1957; see Minutes, “Preliminary Planning Meeting for Proposed Grizzly Bear Study,” 7 June 1957, Box 3, University of Montana Department of Zoology Records, Record Group 60, K. Ross Toole Archives, University of Montana, Missoula.

9 Robert E. Kohler, *Landscapes and Labscapes: Exploring the Lab-Field Border in Biology* (Chicago: University of Chicago Press, 2002).

flagship park. Despite its traditional wariness of wildlife management, which too often seemed to privilege the interests of hunters over other potential users of natural areas, the Park Service was eager to have the Craigheads focus their grizzly research on national parks.¹⁰ They were unable to offer the Craigheads any funding, however, and during their first several years in Yellowstone, the Craigheads were forced to cobble together support from a variety of sources.¹¹

Despite the lack of financial support and the potential for conflict with the park administration, Yellowstone was an ideal location for grizzly research. The same factors that had led to growing conflict between visitors and wildlife in the postwar years—the increasing number of tourists and the absence of hunting, which made animals within the park far less wary of humans than those outside—had also created excellent logistical conditions for research on the large, dangerous, wide-ranging, omnivorous grizzlies. In the summer of 1958, Yellowstone's chief naturalist gave John Craighead a tour of the Trout Creek dump in Hayden Valley, near the geographical center of the park. During the summer months, the dump had become the site of a remarkable concentration of grizzlies in open ground, where conditions for observation were nearly ideal.¹² It was the dump and the “artificial conditions” for science that it provided, rather

10 Chief Park Naturalist to Superintendent, Yellowstone National Park, 8 December 1958, Box N-91, Natural and Social Sciences Records, Yellowstone National Park, Heritage and Research Center, Gardiner, MT; see also John J. Craighead to C. Gordon Fredine, 1 October 1958; C. Gordon Fredine [Acting Chief Naturalist] to John J. Craighead, 19 November 1958, Chief, Division of Interpretation [Ronald F. Lee] to Regional Director, Region Two, 11 March 1959; Acting Regional Chief of Interpretation [Paul L. Beaubien] to Superintendent, Yellowstone National Park, 18 March 1959; Superintendent, Yellowstone National Park, to Regional Director, Midwest Region, 15 December 1958, Box N-371, Natural and Social Sciences Records, Yellowstone National Park, Heritage and Research Center, Gardiner, MT.

11 John J. Craighead to Lemuel A. Garrison, 23 March 1959, Box N-371, Natural and Social Sciences Records, Yellowstone National Park, Heritage and Research Center, Gardiner, MT.

12 John J. Craighead to C. Gordon Fredine, 1 October 1958, Box N-371, Natural and Social Sciences Records, Yellowstone National Park, Heritage and Research Center, Gardiner, MT; Chief Park Naturalist to Superintendent, Yellowstone National Park, 8 December 1958, Box N-91, Natural and Social Sciences Records, Yellowstone National Park, Heritage and Research Center, Gardiner, MT; Minutes of the National Academy of Sciences, National Park Service Research Committee, Yellowstone-Grand Teton National Parks, June 13-16, 1963, Box 25, Carl Leavitt Hubbs Papers, MC 5, Scripps Institution of Oceanography, La Jolla, CA. Craighead had been informed about the dump's bears as early as 1954, when he asked then-superintendent Edmund B. Rogers for advice on where to obtain motion pictures of grizzlies, but it was only during his visit with Condon

than the park's natural conditions, that convinced Craighead to conduct his grizzly study in Yellowstone.¹³

The Craigheads began experimenting with field methods for handling grizzlies in Yellowstone in the summer of 1959. By the end of the season they had captured thirty grizzlies using traps and syringe darts loaded with immobilizing drugs, mostly in the area around the Trout Creek dump.¹⁴ Field work began in earnest in the summer of 1960, when the Craigheads trapped forty-seven individual grizzlies, of which three died from an overdose of the succinylcholine chloride and pentobarbital sodium used for immobilization.¹⁵ Forty-four bears, however, were successfully captured, marked, and released after being weighed, measured, and having blood samples and dental impressions taken. The bears were also tattooed on the upper lip and foreleg and marked with aluminum ear tags and brightly colored plastic tags, ropes, or streamers looped through slits cut into their ears or neck scruffs. Other samples and measurements were occasionally taken by outside researchers; one lactating female, for example, was milked by a biologist from the University of Minnesota. In the summer of 1960, the Craigheads also began testing dummy radio-tag packages for durability, though the radio-tracking equipment and the necessary permits from the Federal Communications Commission

that the site's promise for research became apparent; Edmund B. Rogers to John J. Craighead, 30 June 1954, Box N-91, Natural and Social Sciences Records, Yellowstone National Park, Heritage and Research Center, Gardiner, MT.

13 John J. Craighead to C. Gordon Fredine, 1 October 1958, Box N-371, Natural and Social Sciences Records, Yellowstone National Park, Heritage and Research Center, Gardiner, MT. As early as July 1959, Craighead was already thinking about ways to close the dumps without driving hungry bears into campsites; John J. Craighead to Lemuel A. Garrison, 9 July 1959, Box N-91, Natural and Social Sciences Records, Yellowstone National Park, Heritage and Research Center, Gardiner, MT.

14 *Annual Report of the Montana Cooperative Wildlife Research Unit, 1 July 1958-30 June 1959* (Missoula, MT: Montana Cooperative Wildlife Research Unit, 1960), 21-24.

15 There were strong parallels between the Craigheads' use of immobilizing drugs in Yellowstone and similar experiments in South Africa at the time, but the direct connections were slight; see G.F. Baggley to John J. Craighead, 12 April 1963, Box N-378, Natural and Social Sciences Records, Yellowstone National Park, Heritage and Research Center, Gardiner, MT.

were not yet in hand.¹⁶ By the end of 1960, a total of seventy-seven grizzlies had been marked, amounting to perhaps one of every four grizzlies in Yellowstone.¹⁷

Radio-tracking promised to allow the Craigheads to follow individual grizzlies from the dumps to the rest of the park and beyond. Although John Craighead coordinated the study as a whole, Frank Craighead took the lead in developing radio-tracking equipment with the help of Hoke Franciscus, a childhood friend and radio enthusiast from Pennsylvania, and Joel Varney, an engineer at the Philco Corporation's avionics division in Palo Alto. In 1960, Frank Craighead had attended the first session on "radio-tracing" at the meeting of the Wildlife Society (organized by Jack Seubert of South Dakota) and had been elected to the Society's new Wildlife Telemetry Committee (chaired by William Marshall). The Craigheads began testing their radio-tracking gear at Yellowstone later that year. On October 26, 1960, they captured a grizzly sow in the hope of attaching their first radio tag, but found that her pre-hibernation weight gain had made her neck too thick for the collar—a full twenty-nine inches in circumference. Unable to tag a free-ranging bear, they instead used the immobilized bear as a mannequin, testing the impact of various placements and orientations of the collar on the emitted signals. Although the tests left the Craigheads optimistic about the equipment, it was only in September 1961, nearly a year later, that the first two grizzlies in Yellowstone were fitted with working radio collars.¹⁸ It took until in 1963 for the tracking system to become fully operational. Although radio tracking could

16 P.E. Smith to Superintendent, Yellowstone National Park, 19 August 1960; P.E. Smith to National Park Service Chief of Design and Construction, 23 August 1960; Acting Chief Engineer to Chief, Western Office, Division of Design and Construction, National Park Service, San Francisco, CA, 31 August 1960; P.E. Smith to Superintendent, Yellowstone National Park, 12 October 1960, Box N-371, Natural and Social Sciences Records, Yellowstone National Park, Heritage and Research Center, Gardiner, MT.

17 Superintendent, Yellowstone National Park, to Director, National Park Service, 13 October 1961, Box 319, National Park Service, Record Group 79, National Archives and Records Administration, College Park, MD.

18 Yellowstone National Park Monthly Report for October 1960, 6 December 1960; Yellowstone National Park Monthly Report for October 1961, 24 November 1961, Box 319, National Park Service, Record Group 79, National Archives and Records Administration, College Park, MD.

be used to locate grizzlies from a central observation station without ever coming in sight of the animals, the Craigheads were far more interested in using it to facilitate first-hand behavioral observations.¹⁹ Like many of the pioneers of wildlife telemetry, they saw Cold War surveillance technologies as ways of extending and improving, rather than replacing, the natural-history-like observational studies in which they had been trained.

Because it captures the combination of wilderness advocacy and technological enthusiasm that characterized the Craigheads' work, Frank Craighead's description of the tagging of bear Number 40 in his 1976 book *Track of the Grizzly* is worth quoting at length:

Beep, beep, beep, full of portent and meaning, the repetitive metallic pulse came in loud and clear on the crisp fall air. The sound had nothing of wildness about it. No deep primitive instinct of the chase stirred in us at the sound, nor did it evoke a feeling of oneness with nature. Yet this beeping coming to us in the vastness of Hayden Valley thrilled us as few sounds ever had. The vibrant pulsing signal, though new to the Yellowstone wilderness, told us that we were in communication with the grizzly we identified as bear Number 40, just as surely as the distant honking told us that the Canada geese were on the wing. But the beep was more specific than the honk of the goose or the guttural caw of the raven, for it emanated from one particular grizzly bear somewhere within the three thousand square miles of the park. Hearing this sound meant that we were monitoring the first free-roaming grizzly sow to be tracked by radio.

Number 40's debut as a free-roaming electronic instrument of science took place that day.²⁰

19 John Craighead stressed the importance of first-hand visual observation in his presentation to the National Academy of Sciences committee that visited Yellowstone in June 1963: "One of the major purposes is to locate an individual bear and move in to make types of observations that we want to make, rather than hunting over a tremendous area"; Minutes of the National Academy of Sciences National Park Service Research Committee, Yellowstone-Grand Teton National Parks, June 13-16, 1963, Box 25, Carl Leavitt Hubbs Papers, MC 5, Scripps Institution of Oceanography, La Jolla, CA. Such observations were as important to the Craigheads as the "transcendent vision" emphasized in Gregg Mitman, "When Nature *Is* The Zoo: Vision and Power in the Art and Science of Natural History," *Osiris* 11 (1996): 117-143.

20 Frank C. Craighead, Jr., *Track of the Grizzly* (San Francisco: Sierra Club Books, 1979), 14.

In Craighead's account, though the radio signal has “nothing of wildness about it,” it does not destroy or contaminate the Yellowstone wilderness; it is simply “new to” it. Though it stirs no “deep primitive instinct of the chase,” it is just as, if not more, thrilling than the animal cries that might do so. The “vibrant pulsing signal” is similar to the “distant honking” of geese in telling the scientist about nature, but it is more immediate and more specific. Rather than violating the wilderness, it provides a new means of connecting to it. Although visual metaphors were important elsewhere in the Craigheads' writings, here it is hearing that is central—a kind of hearing that was mediated by Cold War technologies and situated within a particular soundscape. Only biologists equipped with the proper tools and training could understand the “portent and meaning” of this new kind of call. Radio-tracking created a new kind of immediacy between wildlife biologists and wild animals that was, at the same time, mediated in such a way as to exclude non-scientists.²¹

Although the Park Service provided little more than logistical support during the first few years of the Craigheads' study, it proudly touted its support of the use of “modern devices” for wildlife research.²² Initially, Yellowstone superintendent Lemuel A. Garrison was a strong supporter of the project and encouraged park rangers to assist the Craigheads in tagging bears.²³ In 1962, he described the grizzly project as “splendid work” to the director of the Park Service's Midwest region.²⁴ Garrison continued to support the project through the end of his term as

21 On mediation and/of soundscapes, see Jay Bolter and Richard Grusin, *Remediation: Understanding New Media* (Cambridge: MIT Press, 1999); Emily Ann Thompson, *The Soundscape of Modernity: Architectural Acoustics and the Culture of Listening in America, 1900-1933* (Cambridge, Mass.: MIT Press, 2002).

22 Memo for the Press from Yellowstone National Park, 21 August 1959; Memo for the Press from Yellowstone National Park, 20 October 1961, Box N-371, Natural and Social Sciences Records, Yellowstone National Park, Heritage and Research Center, Gardiner, MT.

23 Lemuel A. Garrison to Acting District Rangers, n.d. [1959], Box N-57, Natural and Social Sciences Records, Yellowstone National Park, Heritage and Research Center, Gardiner, MT; Superintendent, Yellowstone National Park, to All Park Personnel, 9 August 1959, Box N-91, Natural and Social Sciences Records, Yellowstone National Park, Heritage and Research Center, Gardiner, MT.

24 Superintendent, Yellowstone National Park [Lemuel A. Garrison], to Regional Director, Midwest Region, 18

superintendent in 1964.²⁵ The fact that the Craigheads were outside researchers, which the Park Service would soon come to see as a major problem, was initially seen as a boon. In 1963, for example, George Hartzog, director of the National Park Service, suggested that the Craigheads be hired to conduct a study of Yellowstone's apparently declining black bear population in order to head off any potential criticism of research by National Park Service biologists as being biased in the park's favor. (The Craigheads declined and the project was taken on by the U.S. Fish and Wildlife Service's Denver Wildlife Research Center.)²⁶ After the Craigheads reported their initial success with trapping, tranquilizing, tagging, and translocating bears, the Yellowstone administration began to incorporate the techniques into their grizzly bear and black bear management practices. As early as April 1960, after the Craigheads had submitted their initial report to the Park Service, the regional chief of interpretation emphasized the utility of these new techniques for managing “troublesome black and grizzly bears” to the Yellowstone superintendent.²⁷ By the summer of 1961, the superintendent was promoting the technique to administrators of other natural areas who had heard about the Craigheads' work through scientific conferences, the popular press, or word of mouth.²⁸ Bears that frequented campsites or begged for food at roadsides could be trapped, immobilized, tagged, and transported to remote

October 1962, Box N-190, Natural and Social Sciences Records, Yellowstone National Park, Heritage and Research Center, Gardiner, MT.

25 John J. Craighead to Lemuel A. Garrison, 30 November 1964; Lemuel A. Garrison to John J. Craighead, 29 December 1964; John J. Craighead to George B. Hartzog, 1964; Ben H. Thompson to John J. Craighead, 1964, Box N-378, Natural and Social Sciences Records, Yellowstone National Park, Heritage and Research Center, Gardiner, MT.

26 Director, National Park Service, to Assistant Director Price, 10 May 1963, Box 2324, National Park Service Records, Record Group 79, National Archives and Records Administration, College Park, MD.

27 Regional Chief of Interpretation to Superintendent, Yellowstone National Park, 1 April 1960; Yellowstone's chief naturalist was also enthusiastic about the technique's potential to improve bear management; Robert M. McIntyre to John J. Craighead, 16 March 1960, Box N-371, Natural and Social Sciences Records, Yellowstone National Park, Heritage and Research Center, Gardiner, MT.

28 The Superintendent told one inquirer, “So far we haven't killed a black bear that we didn't want to”; Superintendent, Yellowstone National Park, to Coleman C. Newman, 31 July 1961, Box N-371, Natural and Social Sciences Records, Yellowstone National Park, Heritage and Research Center, Gardiner, MT.

areas of the park, and then monitored to determine whether they would resume their bad habits. Long before the Craigheads began to draw scientific conclusions or formulate management recommendations based on their research, the techniques they developed had transformed bear management in Yellowstone.

“Laboratoryizing the Wilderness”

As Robert Kohler has argued, the laboratory became an important model and metaphor for field biologists in the twentieth century.²⁹ In the context of the campaign that culminated in the Wilderness Act of 1964, it also served as an important political tool. The argument that wilderness areas could serve as “laboratories” for ecological science was an important complement to arguments for wilderness preservation based on aesthetic, cultural, or spiritual factors. Such arguments were the focus of the Sierra Club's Sixth Wilderness Conference in 1959, the proceedings of which were published under the title *The Meaning of Wilderness to Science* under the editorship of David R. Brower, the club's high-profile executive director.³⁰ For policymakers who did not share wilderness advocates' enthusiasm for untrammelled nature, the argument that research in wilderness areas might eventually boost agricultural productivity or fight disease was powerful.³¹ However, behind the united front presented in legislative hearings and public forums, there were deep-running tensions within the wilderness movement between those who privileged scientific or utilitarian arguments and those who privileged aesthetic,

29 Robert E. Kohler, *Landscapes and Labscapes: Exploring the Lab-Field Border in Biology* (Chicago: University of Chicago Press, 2002).

30 David R. Brower, ed., *The Meaning of Wilderness to Science* (San Francisco: Sierra Club Books, 1959).

31 In 1958, the chair of the University of Montana's Department of Zoology wrote to one of his state's senators in support of the wilderness bill, arguing that large undisturbed wilderness should be preserved as study areas for “future students of ecology”; Philip L. Wright to James E. Murray, 31 October 1958, Box 22, University of Montana Department of Zoology Records, Record Group 60, K. Ross Toole Archives, University of Montana, Missoula.

cultural, or spiritual arguments. The Muries, though scientists by training and profession, strongly favored the latter, which allied them with Brower and brought them into conflict with the Craigheads and other scientists who supported the fight for wilderness but did not hesitate to fill the wilderness with scientific instruments. Brower had argued forcefully for the importance of wilderness to science since at least the early 1950s, but he opposed anything that might sully pristine scenery and was deeply skeptical of scientists' reliability as allies in the wilderness movement.³² In 1960, Brower explained his position to Olaus Murie: "I rebel at the pervasiveness of Management; one trouble is that the managers have corralled all the experts leaving no one on the outside, free of professional conflicts of interest, to tell the managers when to restrain their compulsion to manage. For example, they can now argue that since Dr. Teller's fallout is everywhere, they too must manage everywhere to try to offset what Teller has upset—leaving nothing unmanipulated anywhere."³³ In addition to disliking their proclivity for management, Brower mistrusted scientists' pretension to objectivity, as he explained to the Sierra Club's disgruntled science advisor in 1963: "when I remember the many controversies in which scientists have testified objectively on both sides of an argument and in direct contradiction to each other, and when I remember how scientists will come to the Sierra Club with pleas for us to save something for them to research in and they turn out to be unwilling to sign their names to arguments justifying the preservation—well I guess I just see stardust get mixed with something to produce the clay most feet are made up out of, including scientists'."³⁴ Wilderness was indeed

32 David R. Brower to Thane Raney, 16 April 1951, Carton 91, Sierra Club Records, 71/103c, Bancroft Library, University of California, Berkeley.

33 David R. Brower to Olaus J. Murie, 30 June 1960, Carton 92, Sierra Club Records, 71/103c, Bancroft Library, University of California, Berkeley; Bill McKibben, *The End of Nature* (New York: Anchor Books, 1990).

34 Brower's correspondent, Milton Hildebrand, stepped down from chairmanship of the Sierra Club's Natural Science Committee the next year out of frustration with what he saw as the club's—especially Brower's—politicization of science; David R. Brower to Milton Hildebrand, 17 September 1963; Milton Hildebrand to Board of Directors of the Sierra Club, 26 August 1964, Carton 42, Sierra Club Records, 71/103c, Bancroft

meaningful to science, but the meaning was far more ambivalent than wilderness advocates usually admitted in public.

The Muries saw themselves as defenders not only of the spiritual values of wilderness but also of the role of national parks in maintaining American culture. In response to a controversy in the early 1960s over whether public hunting should be used to control burgeoning ungulate populations in Yellowstone, Adolph Murie defended the park's prohibition on hunting in broad cultural terms. "There are scientific and other values in parks, but in my opinion the most fundamental values are in the realm of the esthetic and the spiritual," he wrote. "Our park ideals are an expression of the best in us. Our better instincts are given free play, and we have an opportunity to show tolerance and kindness toward our fellow creatures. This, I believe is uplifting to the human race."³⁵ Though less harmful than hunting, hands-on research also threatened the park's ability to uplift the human race. In 1960, as the Craigheads' radio-tagging project was just beginning in Yellowstone, University of Alaska biologist Frederick C. Dean applied to the Arctic Institute of North America for funding to tag grizzlies in Mount McKinley National Park. Although Adolph Murie had previously supported Dean's work on grizzlies in the park, he pulled as many strings as he thought he safely could to stop the study from going forward, including recruiting the help of his brother Olaus Murie, one of the leaders of the Wilderness Society. When the Arctic Institute wrote him to ask for his advice on whether they should fund the study, Murie recommended strongly against it. He also wrote a memo to Park Service leadership explaining his opposition to this project and to tagging in the parks in general, but, afraid that criticizing a project that Park Service administrators had already approved would

Library, University of California, Berkeley.

35 Adolph Murie to Anthony Wayne Smith, 27 January 1963, Box 21, Murie Family Papers, Series I: Adolph Murie Files, 1834-1982, American Heritage Center, University of Wyoming, Laramie.

jeopardize his own ability to work in McKinley, he did not immediately send it. For its own reasons the Arctic Institute decided against funding Dean's project, and when Murie spoke to Dean about it in person in Alaska in the summer of 1961, Dean's enthusiasm for the project seemed to have waned. Writing to Conrad L. Wirth the next spring, Murie reported that Dean "agreed, indeed suggested himself, that certain kinds of scientific work would not be suitable in a natural area, because of the disturbance it would cause."³⁶ Later in 1962, however, a new proposal for tagging McKinley's grizzlies was brought forward. This time, when the Park Service leadership requested Murie's opinion on "the use of anesthesia as a means for live capture for tagging and/or subdermal placement of radio transmitters" for McKinley's grizzlies, he did not hesitate to express his opposition. Radio-tagging might contribute some supplemental information about grizzlies in a few areas, he wrote, but "the need is not there." Such a study, moreover, would have a "contradictory effect on the McKinley wilderness which we are trying to maintain at a higher and purer spiritual and esthetic level than is possible in such parks as Yellowstone and Yosemite."³⁷

For the moment, the Park Service decided against going forward with the study. The issue re-emerged in late 1964, however, two years after Olaus Murie's death and just before Adolph Murie's retirement from the Park Service. When Adolph heard from a colleague that Dean was once again proposing to tag grizzlies in McKinley, he fired off another round of letters to fellow conservationists and Park Service administrators. In the letter to Freeman Tilden in which he

36 Olaus J. Murie to Conrad L. Wirth, 4 April 1962, Box 1, Series 2, Wilderness Society Records, CONS 130, Denver Public Library, Denver, CO. The Arctic Institute of North America turned down the study for its own reasons: Robert W. Mason to Adolph Murie, 24 January 1961, Box 6, Murie Family Papers, Series I: Adolph Murie Files, 1834-1982, American Heritage Center, University of Wyoming, Laramie.

37 Regional Field Biologist [Adolph Murie] to Regional Chief of Interpretation through Superintendent, Crater Lake, 5 March 1962, Box 2, Murie Family Papers, Series I: Adolph Murie Files, 1834-1982, American Heritage Center, University of Wyoming, Laramie.

accused hands-on scientific research of “destroying the very essence of the poetry of wilderness,” Murie framed his opposition to grizzly tagging in terms of a battle over the future of the national parks between advocates of “management” and advocates of “preservation.” Notable among the advocates of management was A. Starker Leopold, who had chaired an advisory committee on wildlife management in the national parks that released the influential “Leopold Report” in 1963 calling for national parks to be managed as “vignettes of primitive America.”³⁸ Murie also wrote to Sprugel, the Park Service's chief scientist, urging him to deny permission for Dean's proposed study. (Murie was perhaps unaware that Sprugel, in his former position as program officer for environmental biology at the National Science Foundation, had provided significant funding for the Craigheads' radio-tagging work in Yellowstone.) Sprugel gave Murie a respectful but noncommittal reply.³⁹ Although Murie tended to emphasize aesthetic and cultural arguments against tagging, he also provided a practical argument. His own observational work on grizzlies had already provided all the information the Park Service needed to protect grizzlies, he argued, and further research would, in addition to undermining the park's “wilderness character,” be a waste of resources. As Richard G. Prasil pointed out to Murie, this utilitarian argument—which Prasil thought was far more likely to succeed than aesthetic arguments—was weakened by the fact that Murie's grizzly findings had yet to be published (and in fact would only be published posthumously).⁴⁰

38 Although Adolph Murie was happy with many of the specific policy recommendations of Leopold Report, he was disappointed with its “more intangible park philosophy”; Adolph Murie to Richard Prasil, 4 January 1964, Carton 92, Sierra Club Records, 71/103c, Bancroft Library, University of California, Berkeley.

39 Adolph Murie to George Sprugel, Jr., 11 January 1965, and George Sprugel, Jr. [Chief, Division of Natural Sciences] to Adolph Murie, 15 January 1965, Box 2, Murie Family Papers, Series I: Adolph Murie Files, 1834-1982, American Heritage Center, University of Wyoming, Laramie.

40 Adolph Murie to Paul Brooks [Editor in Chief, Houghton Mifflin], 25 April 1965, Box 2, Murie Family Papers, Series I: Adolph Murie Files, 1834-1982, American Heritage Center, University of Wyoming, Laramie.

Nonetheless, in part because of Murie's opposition, Dean never carried out his proposed tagging study in Denali. Until his death in 1974, Adolph Murie continued to oppose the tagging of wildlife in the national parks, especially the “wilderness wildlife” of Mount McKinley. As he pointed out in one of his letters opposing Dean's proposal, he himself had tagged hundreds of rodents in the course of his research; it was the use of such methods in national parks, not their use in general, that offended him. That his opposition to tagging was not motivated solely by a proprietary feeling toward McKinley's grizzlies or by concern for endangered species is evidenced by his advice to Michael J. Gawel, a graduate student in Yale's School of Forestry who sought his blessing in 1969 for a study that would have involved tagging McKinley's abundant arctic ground squirrels. Murie replied that he would have been happy to endorse the project “if it did not involve *tagging animals* in a *national park*.” The tagging that Gawel had proposed, “though less obvious than the flamboyant gadgetizing of larger animals, nevertheless would inflict the same type of blemish to the wilderness spirit, and would serve as an entering wedge for this type of research within the park.” If such studies were allowed to proliferate, they would “cheapen the esthetics of this wilderness park” and transform its “charm and inspirational qualities” into a “mundane laboratory atmosphere.” Fortunately, Murie concluded, although tagging had become a real threat to McKinley, opposition to such despoliation of the parks seemed to be on the rise.⁴¹

In contrast to the vigor of his opposition to wildlife tagging in Mount McKinley National Park, Murie's resistance to grizzly tagging in Yellowstone was muted. It was clear to him that radio-tagging was only one part, and not the most important part, of a more fundamental problem with the park. Under the impetus of the Mission 66 infrastructure development program,

41 Adolph Murie to Michael Gawel, 24 February 1969-02-24, Box 2, Series I, MFP-AHC

Yellowstone had, in their eyes, become even more artificialized and overdeveloped than it had already been, with deleterious consequences for park wildlife. There is a palpable contempt in his writings and in those of certain other wilderness advocates for the Yellowstone bears that had been transformed into “bums” and “beggars” along roadsides, in campgrounds, and at garbage dumps.⁴² As Alice Wondrak Biel has documented, opposition to human interaction with bears in the parks hardened dramatically in the second half of the twentieth century.⁴³ Only a few marginalized hold-outs such as former Yellowstone superintendent Horace Albright continued to argue that human feeding of bears had a place in park policy.⁴⁴ For Murie, the problem in Yellowstone was not tagging but the garbage produced by too many visitors in combination with ineffective park policies, which both provided the opportunity for and overshadowed hands-on wildlife research practices. By the late 1960s, his distaste for such practices was shared by a much broader and more vocal group of critics who came to see the Craigheads work as a violation of national park ideals.

42 In 1969, Charles E. Olmsted, a member of the committee brought in under A. Starker Leopold to review the Craigheads criticisms of Yellowstone's bear management policies, pointed out the oddness of a sentence in the draft report that read, “roadside begging by black bears, while against Park regulations, has persisted to the extent that many individuals have become confirmed panhandlers.” Park regulations, he noted, were not intended to directly regulate bear behavior; Charles E. Olmsted to A. Starker Leopold, 26 September 1969, Carton 5, A. Starker Leopold Papers, MSS 81/61c, Bancroft Library, University of California, Berkeley. See also Harold C. Bradley to Lowell Sumner, 23 February 1959, Carton 91, Sierra Club Records, 71/103c, Bancroft Library, University of California, Berkeley; South District Naturalist [J. Halladay] to Chief Park Naturalist, 14 July 1975-07-14, Box N-39, Natural and Social Sciences Records, Yellowstone National Park, Heritage and Research Center, Gardiner, MT.

43 Alice Wondrak Biel, *Do (Not) Feed the Bears: The Fitful History of Wildlife and Tourists in Yellowstone* (Lawrence: University of Kansas Press, 2006).

44 Albright had been superintendent of Yellowstone in 1919-1929 and director of the Park Service 1929-1933. In 1962, he argued that the abandoned practice of evening feeding of bears in Yellowstone had “helped to keep bears away from the roads, made them less importunate, kept them out of campgrounds, etc.”; he also argued that policies meant to keep bears completely away from roads and areas where they might be seen by humans would take away one of the most important reasons that people visited the parks; Horace Albright to Ira N. Gabrielson, 11 September 1962; see also Horace Albright to Ira N. Gabrielson, 13 September 1962; Ira N. Gabrielson to Horace Albright, 16 October 1962, Ira N. Gabrielson Papers, CONS 37, Denver Public Library, CO.

Hatred Which is Not Rational

On June 14, 1963, a National Academy of Sciences committee on national park research policy visited Yellowstone and heard John Craighead give a presentation on the grizzly bear project. Craighead, accompanied by graduate assistant Maurice Hornocker, reported that the radio-tracking system had been perfected the year before. In the discussion after Craighead's talk it became apparent that there were some skeptics in the audience. "Is your main business to produce more grizzly bears for hunters?" one committee member asked. "Would it be more difficult for you to do it somewhere else?"⁴⁵ Though they had initially welcomed them with open arms, park administrators were beginning to wonder whether the Craigheads' shared the National Park Service's priorities and ideals. By the mid-1960s, influential Park Service administrators and outside commentators had begun to share—and, more importantly, to make public—the Muries' opposition to hands-on, technologically intensive, and highly visible wildlife research in the parks. The initially positive relationship between the Craigheads and the Park Service began to deteriorate in 1963, just as the Craigheads were beginning their radio-tracking work in earnest. The Craigheads' position was weakened by the Park Service's success in strengthening its internal science capabilities and by the fact that the Park Service had begun to contribute financially to the grizzly project. Although Park Service administrators were still hesitant about dictating conditions to the Craigheads since their Park Service contribution never amounted to a major proportion of the funds the Craigheads were spending on the project, they lost much of the deference with which they had initially approached the technically sophisticated and well-funded outside researchers.⁴⁶ The Park Service's growing confidence emerged most clearly in

45 Minutes of the National Academy of Sciences, National Park Service Research Committee, Yellowstone-Grand Teton National Parks, June 13-16, 1963, Box 25, Carl Leavitt Hubbs Papers, MC 5, Scripps Institution of Oceanography, La Jolla, CA.

46 Hesitation to intervene in the Craigheads' project was expressed in Chief, Branch of Wildlife Management, to

negotiations over the Craigheads' research contracts, which grew increasingly tense over the course of the 1960s.⁴⁷ The Craigheads accused the Park Service of using restrictive contract clauses to gradually take control of the grizzly project, while the Park Service began to suspect that the Craigheads, rather than conducting the time-limited and policy-relevant study they had initially been invited to carry out, had decided to make Yellowstone into their own permanent laboratory and training ground for graduate students.⁴⁸

In the summer of 1967, when the Craigheads submitted a report to the Park Service summarizing their findings and recommendations for grizzly management, the long-simmering tensions between the two parties finally boiled over. Before 1967, the Craigheads' publications on grizzlies had been limited to progress reports and conference papers, many of them focused on techniques.⁴⁹ In 1966, however, John Craighead took a year's leave from the Montana Cooperative Wildlife Research Unit to write up the study's results and management recommendations. When he finally submitted the report, the Yellowstone leadership deemed it faulty on two counts. First, it argued for grizzly management policies that violated the basic

Director, National Park Service, through Assistant Director Price, 7 June 1963, Box 2324, National Park Service Records, Record Group 79, National Archives and Records Administration, College Park, MD. By 1966, much of that hesitation was gone; see Walter H. Kittams, Status Report, Grizzly Bear Ecology, Project YELL-N-7, 17 February 1966, Box N-175, Natural and Social Sciences Records, Yellowstone National Park, Heritage and Research Center, Gardiner, MT.

47 Lemuel A. Garrison to John S. McLaughlin, 14 October 1964, Box N-91, Natural and Social Sciences Records, Yellowstone National Park, Heritage and Research Center, Gardiner, MT. See also Superintendent, Yellowstone National Park [John S. McLaughlin] to Regional Director, Midwest Region [Lemuel A. Garrison], 20 October 1964; John J. Craighead to Walter H. Kittams, 30 November 1964; Walter H. Kittams to John J. Craighead, 29 December 1964, Box N-378, Natural and Social Sciences Records, Yellowstone National Park, Heritage and Research Center, Gardiner, MT.

48 John S. McLaughlin to Regional Director, Midwest Region [Lemuel A. Garrison], 20 October 1964; John J. Craighead to John S. McLaughlin, 28 June 1965; Park Management Biologist to Chief Park Ranger, 5 May 1964, Box N-91, Natural and Social Sciences Records, Yellowstone National Park, Heritage and Research Center, Gardiner, MT.

49 In 1964, for example, the Park Service found itself in the unusual position of considering grizzly management recommendations the Craigheads had included in a presentation to the Instrument Society of America; Acting Assistant Regional Director, Midwest Region [Edwin C. Alberts], to Director, National Park Service, 27 February 1964; Walter H. Kittams to John J. Craighead, 3 March 1964, Box N-378, Natural and Social Sciences Records, Yellowstone National Park, Heritage and Research Center, Gardiner, MT.

philosophy of the national parks—namely, hands-on management and gradual rather than immediate closure of the park's open-pit dumps. Second, it failed to provide adequate data to support its claims. With the unprecedented killing of two women by grizzlies in separate incidents in Glacier National Park that year, the report came at a time when public attention, even more than usual, was focused on grizzly management in the parks.⁵⁰ In the fall of 1967, after several months of uncertainty and debate within the Yellowstone administration, the superintendent decided to allow the Craigheads to continue their research—but only under the “strict surveillance and guidance” of one of the three new research biologists the park had hired earlier that year.⁵¹ Glen F. Cole, formerly a wildlife biologist at the Montana State Game and Fish Department, was given the responsibility of supervising all research in the park, including that of the Craigheads.⁵² Compounding the insult was the fact that the Yellowstone administration allowed a concession operator to destroy the buildings that the Craigheads and their students had been using as their headquarters.⁵³ By 1969, the grizzly project had become so controversial and gotten so much attention in the press that one University of Colorado student wrote to

50 John S. McLaughlin to Superintendent, Yellowstone National Park, 21 August 1967, Box N-172, Natural and Social Sciences Records, Yellowstone National Park, Heritage and Research Center, Gardiner, MT.

51 Superintendent, Yellowstone National Park, to Director, National Park Service, 16 August 1967, Box N-176, Natural and Social Sciences Records, Yellowstone National Park, Heritage and Research Center, Gardiner, MT.

52 John S. McLaughlin to John J. Craighead, 25 July 1967, Box N-92, Natural and Social Sciences Records, Yellowstone National Park, Heritage and Research Center, Gardiner, MT.

53 John J. Craighead to Robert R. Lovegren, 11 May 1967; John S. McLaughlin to John J. Craighead, 23 May 1967, Box N-176, Natural and Social Sciences Records, Yellowstone National Park, Heritage and Research Center, Gardiner, MT. The Park Service also initiated a study of grizzlies in Glacier National Park without informing the Craigheads, much less consulting them. John J. Craighead to Keith Neilson, 1 April 1968, Box N-172, Natural and Social Sciences Records, Yellowstone National Park, Heritage and Research Center, Gardiner, MT. In 1968, advising Philip Wright of the University of Montana's Zoology Department on obtaining outside grants, John Craighead wrote: “There are a lot of advantages to grants but there are a lot of problems, as you know, associated with obtaining and keeping them going”; John J. Craighead to Philip L. Wright, 14 March 1968, Box 8, University of Montana Department of Zoology Records, K. Ross Toole Archives, University of Montana, Missoula.

Superintendent Anderson to tell him that he was planning to use it as the subject of his thesis on “the role of scientific information in the administrative decision-making process.”⁵⁴

The Craigheads' Yellowstone work had been the focus of a great deal of laudatory media attention since its beginning in 1959—coverage which the Park Service had supported as a means of burnishing its own reputation for supporting science and as a way for the Craigheads to bring more research money into the park.⁵⁵ In the late 1960s, however, the coverage became significantly less positive and the Park Service became much more critical of the way the Craigheads were representing their work to the press. Ironically, the criticism was, to a large extent, a backlash against the Craigheads' successful efforts to depict their work in the media as the epitome of technological modernism. Increasing skepticism toward technological fixes combined with the increased strength of wilderness and animal rights activists to make hands-on wildlife research in the national parks particularly risky in terms of public relations. As early as 1966, these risks were apparent to George Sprugel, the National Park Service's chief scientist, who noted in his comments on a draft article about research on black bears in Yellowstone that the researcher's use of the word “torture” to describe certain research procedures, however tongue-in-cheek, might provoke undesirable reactions if it came to the attention of “ardent animal lovers.”⁵⁶ The Craigheads were also aware of the risks of presenting their work to the

54 Ronald E. Lambertson (Boulder, CO) to Jack K. Anderson, 24 November 1969-11-24, Box N-196, Natural and Social Sciences Records, Yellowstone National Park, Heritage and Research Center, Gardiner, MT.

55 A National Geographic movie crew visited Yellowstone in late August and September 1961 to film the Craigheads with the endorsement of the park administration; Yellowstone National Park Monthly Report for August, 15 September 1961; Yellowstone National Park Monthly Report for September, 17 October 1961, Box 104, National Park Service Records, Record Group 79, National Archives and Records Administration, College Park, MD. The chief Yellowstone naturalist also praised the Craigheads for publishing an article about their work in *National Geographic Magazine* because it might help them raise additional funds; Robert M. McIntyre [Chief Park Naturalist] to John J. Craighead, 17 May 1960, Box N-91, Natural and Social Sciences Records, Yellowstone National Park, Heritage and Research Center, Gardiner, MT.

56 George Sprugel, Jr., to Fred A. Glover, 22 July 1966, Box N-175, Natural and Social Sciences Records, Yellowstone National Park, Heritage and Research Center, Gardiner, MT.

public. In June 1967, a few months before the broadcast of a National Geographic Society television special about their grizzly research—*Grizzly!* aired for the first time on November 1, 1967—John Craighead assured the Yellowstone superintendent that “no one recognizes the hazards involved in presenting a film of this kind to the general public any more than Frank and I do.”⁵⁷ Similarly, Craighead felt it necessary to reassure the president of the University of Montana that although “we had reservations regarding publicizing our work to the extent that is necessary for a TV program,” the final product appeared to be sound.⁵⁸

Craighead's concerns about *Grizzly!* proved well-founded. Although the president of the National Geographic Society reported to John Craighead that the film had made a positive impression on George Hartzog and other Park Service staff when shown in a special viewing in Washington in August 1967, many others found its depictions of the Craighead brothers in the act of darting, tagging, tattooing, and radio-tracking bears upsetting.⁵⁹ The writer and filmmaker Lois Crisler, for example, wrote to Adolph Murie to ask him whether he shared her feelings about the show, which struck her as sensationalistic and misleading: “That bear panting violently—was it killed? The cub 'restored to its mother'—just how? Walking with a receiver held out in the hand—how far? to what avail? All information given had already been found by you,” she told Murie, “without drugs or markers.”⁶⁰ Crisler and Murie kept their criticisms private or at

57 John J. Craighead to John S. McLaughlin, 28 June 1967, Box N-176, Natural and Social Sciences Records, Yellowstone National Park, Heritage and Research Center, Gardiner, MT.

58 John J. Craighead to Robert Pantzer, 20 April 1967, Box N-176, Natural and Social Sciences Records, Yellowstone National Park, Heritage and Research Center, Gardiner, MT.

59 Neil Bibler to John J. Craighead, 9 September 1967, Box N-176, Natural and Social Sciences Records, Yellowstone National Park, Heritage and Research Center, Gardiner, MT. Bibler was writing in part to reassure Craighead that a National Geographic press release about the documentary that included criticism of Yellowstone's grizzly management policies had not seriously damaged the Park Service leadership's view of the Craigheads; John J. Craighead to Neil Bibler, 22 August 1967, N-176, Natural and Social Sciences Records, Yellowstone National Park, Heritage and Research Center, Gardiner, MT.

60 Lois Crisler to Adolph Murie, n.d. [1967-1968], Box 1, Murie Family Papers, Series I: Adolph Murie Files, 1834-1982, American Heritage Center, University of Wyoming, Laramie.

least confined to tight professional and personal networks, but other critics were more public. In 1967, for example, in his “Wood, Field and Stream” column in the *New York Times*, Oscar Godbout complained about the impact the “priests of technology” were having on wildlife: “A hunter likes to think of the game he has shot as authentically wild, untouched by human hands. But too often it's been manhandled. I'm waiting for the day the porpoises capture a scientist, put ear tags in him, hang a transmitter around his neck and send him back from where he came.”⁶¹ Another critic, the Wyoming-based nature filmmaker Walter Berlet, took his concerns on the road. Early in 1967, before “Grizzly!” had aired, Berlet began criticizing the Craigheads' radio-tagging work in Yellowstone in public lectures that he gave along with showings of “The Living Wilderness,” a film that he and his wife had made for the National Audubon Society in 1960.⁶² John Craighead took Berlet's public criticisms seriously. In March 1967, he wrote to Berlet to tell him that he had been concerned to hear that Berlet's lectures included the statement, “While [the Craigheads] would have you believe that this program of putting radios around the necks of the grizzlies is to save them, actually this will seal their doom.”⁶³ When Berlet refused to back down, Craighead wrote again, this time more explicitly urging Berlet to cease his criticisms: “Until such time as you can meet with me for a detailed explanation of the program, I would hope that you would refrain from expressing your views in public even though you may

61 Oscar Godbout, “Animals and Fish Find It Hard to Keep Ahead of Scientific Advances, *New York Times*, 10 February 1967.

62 One fan of the Craigheads' work who attended Berlet's lecture sent an irate letter to the executive director of the Massachusetts Audubon Society: “If a few bears had to suffer the indignity of wearing a collar with a small radio attached so that the entire species could be saved, then this is a small price to pay indeed. If the idea of using our technological skills to save wildlife is so horrendous, then we should give up bird banding, fish tagging and all other wildlife management programs. No grizzlie [*sic*] was hurt by having to wear this radio and all have been removed. If we can use science to save wildlife instead of destroy it, as in the past, then why the complaints”; Peter Arnold to Allan Morgan, 23 February 1967, Box N-176, Natural and Social Sciences Records, Yellowstone National Park, Heritage and Research Center, Gardiner, MT.

63 John C. Craighead to Walter E. Berlet, 8 March 1967, Box N-176, Natural and Social Sciences Records, Yellowstone National Park, Heritage and Research Center, Gardiner, MT.

not change them. I can assure you that your actions, which no doubt have been well intentioned, have only contributed to placing greater jeopardy the very animal you profess to feel so concerned about.”⁶⁴ When his attempts to convince Berlet failed—Berlet replied that no less a luminary in the wilderness movement than Olaus Murie had shared his opinion when a similar study had been proposed in McKinley National Park⁶⁵—Craighead turned to Yellowstone superintendent John McLaughlin, who refused to intervene.⁶⁶ Appeals to senior figures associated with the Audubon Society, such as Carl Buchheister and Charles H. Callison, similarly failed.⁶⁷ Having assiduously courted attention from the press throughout their careers, the Craigheds now found their own tools used against them.

From 1967 onward, the Craigheds were on the defensive. It was a startling reversal from the beginning of the decade, when the Park Service had practically begged them to apply their “modern devices” to the study of Yellowstone's wildlife. After Jack K. Anderson became superintendent in 1967 and the Yellowstone management rejected the management recommendations that the Craigheds had presented over the summer of 1967 as the result of their eight-year research project, the relationship between the researchers and the park turned openly antagonistic. Getting permission to tag grizzlies or other park wildlife became difficult. In October 1967, for example, Cole denied the Craigheds' request to radio-tag several of Yellowstone's elk as part of an Atomic Energy Commission-funded study of “western big game.”

64 John C. Craighead to Walter E. Berlet, 31 May 1967, Box N-176, Natural and Social Sciences Records, Yellowstone National Park, Heritage and Research Center, Gardiner, MT.

65 Walter E. Berlet to John C. Craighead, 25 May 1967, Box N-176, Natural and Social Sciences Records, Yellowstone National Park, Heritage and Research Center, Gardiner, MT.

66 John S. McLaughlin to John C. Craighead, 14 September 1967, Box N-176, Natural and Social Sciences Records, Yellowstone National Park, Heritage and Research Center, Gardiner, MT.

67 John C. Craighead to Carl Buchheister, 8 March 1967; John C. Craighead to Charles H. Callison, 20 April 1967, Box N-176, Natural and Social Sciences Records, Yellowstone National Park, Heritage and Research Center, Gardiner, MT.

Alternatives to conspicuous marking must be sought, he told them, “if research itself is not to become a contributing factor to the loss of park integrity as a natural area.”⁶⁸ Furthermore, Cole argued, highly visible tagging in locations where animals were often viewed and photographed by visitors could potentially trigger a backlash that would make it impossible even to use inconspicuous tags in remote areas of the park. In 1969, the Craigheads reluctantly signed a new memorandum of agreement stating that certain scientific practices were not compatible with the park's status as a natural area, and they agreed to cooperate in removing conspicuous markers and to end the grizzly study within two years.⁶⁹

The Yellowstone administration's opposition to radio-tagging was partly motivated by a thoroughly reciprocated personal dislike for the Craigheads—in 1972, one observer explained the grizzly controversy to Congressman John Dingell as the product of “hatred which is not rational”—as well as by resentment over the Craigheads' use of the popular press to turn public opinion against the park administration.⁷⁰ At one point, A. Starker Leopold advised the Park Service against accepting one of the Craigheads' proposals not because the proposal was flawed but because the Craigheads had proved so difficult to work with in the past.⁷¹ It is also clear that

68 Supervisory Research Biologist [Glen F. Cole] to Superintendent, Yellowstone National Park, 16 October 1967, Box N-176, Natural and Social Sciences Records, Yellowstone National Park, Heritage and Research Center, Gardiner, MT.

69 John Craighead suggested that the grizzly study would continue on a limited basis for two more years in January 1969, and a memorandum of understanding to that effect was signed in April; John J. Craighead to Jack K. Anderson, 14 January 1969, Box N-92, Natural and Social Sciences Records, Yellowstone National Park, Heritage and Research Center, Gardiner, MT; Jack K. Anderson to John J. Craighead, 7 April 1969; John J. Craighead to Jack K. Anderson, 14 April 1969, Box N-196, Natural and Social Sciences Records, Yellowstone National Park, Heritage and Research Center, Gardiner, MT.

70 Nathaniel P. Reed to John Dingell, 26 October 1972, Box N-36, Natural and Social Sciences Records, Yellowstone National Park, Heritage and Research Center, Gardiner, MT.

71 Reviewing the Craigheads request to conduct a test of a satellite transmitter for tracking elk in Yellowstone, Leopold wrote: “I find nothing specifically objectionable about this research proposal except for the fact that it originates with the Craigheads who have been so singularly uncooperative with other projects in the past.” Anderson replied that he agreed that the study would best be carried out outside the park, but he also noted that he was in “a rather delicate position because we are, in fact, promoting research in the park”; A. Starker Leopold to Jack K. Anderson, 22 June 1970; Jack K. Anderson to A. Starker Leopold, 26 June 1970, Box N-118, Natural and Social Sciences Records, Yellowstone National Park, Heritage and Research Center, Gardiner, MT.

some of the National Park Service's own biologists continued to believe that radio-tagging was a useful technique for studying and managing park wildlife.⁷² Nonetheless, opposition to the Craigheads' work also reflected a genuine distaste for the aesthetics of collars, tags, and streamers on park wildlife, which extended beyond the Craigheads to work by other outside researchers as well as Park Service biologists.⁷³ As the 1972 centennial of Yellowstone's establishment approached, Anderson and Cole stepped up their pressure on the Craigheads to remove tags from elk and grizzlies and to shift any new radio-telemetry work out of the park. In July 1970, Anderson told John Craighead that he intended for most of the park's conspicuous wildlife tags to be removed by the centennial, when the park would host scientists, conservationists, and park administrators from around the world.⁷⁴ Craighead replied that the Park Service was exaggerating the extent of the problem, and that banning the technique would only retard the progress of science in what he called “one of the finest outdoor laboratories for ecological research that exists anywhere in the world”—a turn of phrase that must have

72 In 1968, one biologist even suggested to an outside researcher that his proposed study of coyotes in the park would be more likely to succeed if it used radio-tracking; William J. Barmore to Frank Clark, 13 May 1968, Box N-172, Natural and Social Sciences Records, Yellowstone National Park, Heritage and Research Center, Gardiner, MT. On the other hand, Acting Yellowstone National Park supt told a potential researcher that trapping, tagging, or handling of Yellowstone National Park wolves was out of the question; Gary E. Everhardt [Acting Superintendent, Yellowstone National Park] to S. Coret (Portland, OR), 6 October 1971, Box N-116, Natural and Social Sciences Records, Yellowstone National Park, Heritage and Research Center, Gardiner, MT.

73 Superintendent, Yellowstone National Park [Jack K. Anderson], to Regional Director, Midwest Region, 1968, Box N-172, Natural and Social Sciences Records, Yellowstone National Park, Heritage and Research Center, Gardiner, MT; Jack K. Anderson to Peter T. Bromley, 19 December 1969, Box N-196, Natural and Social Sciences Records, Yellowstone National Park, Heritage and Research Center, Gardiner, MT; Jack K. Anderson to R.E. Moore, 20 May 1971, Box N-116, Natural and Social Sciences Records, Yellowstone National Park, Heritage and Research Center, Gardiner, MT. When a group of researchers suggested establishing a research station in the town of West Yellowstone, just outside the park borders, Anderson urged them to coordinate with the park administration so that outside researchers would not “unknowingly conduct studies that detract from the integrity of natural ecosystems”—by, for example, “using park wildlife as experimental animals to test telemetry equipment or immobilization drug dosages.”; Jack K. Anderson to Regional Director, Midwest Region, 6 March 1968, Box N-172, Natural and Social Sciences Records, Yellowstone National Park, Heritage and Research Center, Gardiner, MT.

74 Jack K. Anderson to John J. Craighead, 8 July 1970, Box N-91, Natural and Social Sciences Records, Yellowstone National Park, Heritage and Research Center, Gardiner, MT.

confirmed to Anderson the Craigheads' failure to comprehend the essence of the national park ideal.⁷⁵

While personality conflicts and disagreements over grizzly management were the main reasons behind the Yellowstone administration's desire to evict the Craigheads, it was tagging that gave them the means to do so. In 1968, A. Starker Leopold—a prominent wildlife biologist at the University of California-Berkeley who had briefly served as the National Park Service's chief scientist in 1966-1967—volunteered to help mediate the increasingly “hostile situation” that had evolved between the Craigheads and the Yellowstone administration and which was beginning to tarnish the Park Service's reputation in the national media.⁷⁶ Cole and the Yellowstone administration turned to Leopold in the hope that his undeniable professional authority would counteract the Craigheads' charisma; in November 1969, frustrated by the Craigheads' success at gaining the ear of the national media, Cole wrote to Leopold to confess that “this kind of biopolitics is beyond me.”⁷⁷ Leopold's initial goal seems to have been to allow the Craigheads to terminate their grizzly project smoothly and without further interference from the park administration, but from the Craigheads' perspective, his intervention represented an unsolicited attack on their work by a fellow wildlife biologist who held a prominent place in the wildlife management field more because of his famous father, Aldo Leopold, than because of any achievements of his own.⁷⁸

75 John J. Craighead to Jack K. Anderson, 24 July 1970, Box N-92, Natural and Social Sciences Records, Yellowstone National Park, Heritage and Research Center, Gardiner, MT.

76 A. Starker Leopold to George B. Hartzog and John Gottschalk, 29 July 1968; George B. Hartzog to A. Starker Leopold, 5 August 1968, Box N-172, Natural and Social Sciences Records, Yellowstone National Park, Heritage and Research Center, Gardiner, MT.

77 Glen F. Cole to A. Starker Leopold, 10 November 1969-11-10, Carton 5, ASL

78 A. Starker Leopold to Sigurd F. Olson, Charles E. Olmsted, and Stanley Cain, 20 August 1968, Box N-172, Natural and Social Sciences Records, Yellowstone National Park, Heritage and Research Center, Gardiner, MT. In the oral history interview on file at the Yellowstone Heritage Center, conducted three decades after the events in question, John Craighead's bitterness toward Leopold, the mediocre biologist and “son of a bitch” who helped evict him from Yellowstone, is palpable; John J. Craighead Oral History Interview, Yellowstone National Park,

This was not the first time that Leopold and the Craigheads had come into conflict; in a bitter dispute in the early 1960s over whether hunters should be allowed into Yellowstone in order to reduce burgeoning elk populations, the Craigheads had sided with the hunters, while Leopold had sided with the Park Service.⁷⁹ In any case, the ad-hoc advisory committee that Leopold chaired ultimately sided with the Park Service, advising an immediate closure of the dumps and firm limits on scientific practice within the park. In the summer of 1971, the Craigheads learned that the ear tags they had placed on three of the park's grizzlies had been removed. John Craighead wrote to Anderson to protest this act of scientific sabotage and pointed out that, in his estimation, only one percent of visitors ever saw a marked bear. Recognizing that opposition to tagging risked creating an image of the Park Service as anti-science, Anderson apologized for the removal of the tags. He claimed that park rangers had misunderstood his instructions to remove only the brightly colored plastic streamers and that the Craigheads were being asked to terminate their study because of their relations with the press, not because of their research methods. But he also stressed the importance of finding replacement for “the conspicuous marking of park wildlife that is seen and photographed by Yellowstone visitors” and

Heritage and Research Center, Gardiner, MT.

79 Olaus Murie supported hunting but had a dim view of most of its practitioners in modern America; as he explained to Brower in the context of the debate over public hunting in the national parks, “In all phases of American life we stress the importance of size, numbers, grandeur, masses—rather than quality of human experience. ... I feel that the harm done in modern American hunting is not so much to the animals involved as it is to the human spirit, and so to our declining culture”; Olaus J. Murie to David R. Brower, 18 June 1960, Carton 92, Sierra Club Records, 71/103c, Bancroft Library, University of California, Berkeley. Elsewhere he was more blunt: “Sportsmanship today is becoming mere killing, and is at a low point”; Olaus J. Murie to L.C. Binford, 4 October 1962, Box 2, Olaus J. Murie Papers, CONS 90, Denver Public Library, Denver, CO. Brower had drawn an enormous amount of fire for his support of public hunting; his argument was that an alliance with hunters would allow for an expansion of national parks and wilderness areas that would otherwise be impossible. He used the same argument that he would deploy again in the early 1980s with regard to the California condor: “Game populations can be restored. The wilderness value of badly disrupted habitat cannot be. I think the greatest harm we can do the future is to fail to include enough area w/in protective boundaries. We may need to relax a while about any restorable disruption by man in order to get those boundaries drawn. Save the wild habitat, and the wildlife will fill it again”; David R. Brower to Henry M. Weber, 2 June 1960, Carton 92, Sierra Club Records, 71/103c, Bancroft Library, University of California, Berkeley

pointed out that one percent of Yellowstone's annual visitors amounted to twenty-five thousand people who were, as he put it, “short-changed in seeing the grizzly as it occurs naturally.”⁸⁰ In the fall of 1971, the Craigheads' twelve years of research in Yellowstone formally came to an end when they refused to sign a new agreement that would have placed even tighter restrictions on their research.⁸¹

The Risks of Engagement

Even outside the national parks, the Craigheads began to encounter resistance to the use of tagging and other invasive techniques. In the mid-1960s, they had become interested in using satellites to study animal migration, particularly the migration of elk into and out of Yellowstone, a project they pursued in collaboration with Helmut Buechner, a wildlife biologist and ecologist at the Smithsonian Institution, and Charles Cote, a satellite engineer at NASA's Goddard Space Flight Center. Although Yellowstone was their first choice as a site for attaching the satellite collar to an elk, both the Yellowstone superintendent and the superintendent of nearby Grand Teton National Park denied them permission. They finally resigned themselves to carrying out the study in the National Elk Refuge in Jackson Hole, where elk were far more wary of humans.

The scientific and management justifications for carrying out the study were shaky; elk migration into and out of Yellowstone National Park had become an important and controversial issue as the elk population in the park had soared, but satellite tracking offered only marginal improvements on other methods and at an astronomically inflated price. The main reason to

80 Jack K. Anderson to John J. Craighead, 9 February 1971; John J. Craighead to Jack K. Anderson, 13 July 1971; Jack K. Anderson to John J. Craighead, 20 August 1971, Box N-112, Natural and Social Sciences Records, Yellowstone National Park, Heritage and Research Center, Gardiner, MT.

81 Robert M. Linn to Mike Mansfield, 3 November 1972; George B. Hartzog to John Gottschalk, 11 August 1971, Box N-36, Natural and Social Sciences Records, Yellowstone National Park, Heritage and Research Center, Gardiner, MT.

conduct the study was to convince NASA to invest in animal tracking by satellite. In January 1969, Buechner submitted a proposal for elk and caribou tracking by satellite to NASA.⁸² The study would use the Interrogation Recording and Location, or IRLS, system that was scheduled to be carried aboard Nimbus 3, an experimental weather satellite. The IRLS system had been designed for meteorological and oceanographic platforms, not for animals, so the collar would be many times heavier than the conventional radio-tracking collars with which the Craigheads had experience. Elk were appealing test subjects because they were large, strong, and relatively docile, but even so, it was unclear whether they would tolerate the weight. At an early conference on the possibility of animal tracking by satellite that was held at the Smithsonian in 1966, Frank Craighead expressed doubts about whether “elk, caribou or polar bears can and will tolerate a 25 pound instrumentation package, at least for any length of time.”⁸³ Initially the Craigheads hoped that the collar could be engineered to weigh as little as 10 or 15 pounds, but it turned out that the 25-pound estimate had been correct. In June 1969, after Radiation engineers told him that the collar would have to weigh at least 20 pounds, John Craighead told Buechner that “I think we have a real problem on our hands If they are unable to cut the weight significantly, it means we will have to test this collar assembly for at least a two-week period, and frankly I am not at all optimistic... I am afraid that a 20-pound collar will not be tolerated by a cow elk, or at best it will interfere considerably with her activities and behavior.”⁸⁴ That fall, Cote and the Craigheads

82 Helmut K. Buechner, Grant Proposal to NASA, “Satellite (IRLS) Tracking of Elk in Yellowstone National Park and Caribou in Alaska,” 21 January 1969, Box 20, Office of Environmental Sciences, Ecology Program Records, 1965-73, Record Unit 271, Smithsonian Institution Archives, Washington, DC.

83 Frank C. Craighead, “The Prospects for Using Nimbus B Satellite for Studying Large Land Animals,” in *Some Prospects for Using Communications Satellites in Wild Animal Research* (Washington, DC: American Institute of Biological Sciences, 1966): 4-5, in Box 20, Office of Environmental Sciences, Ecology Program Records, 1965-73, Record Unit 271, Smithsonian Institution Archives, Washington, DC.

84 John J. Craighead to Helmut K. Buechner, 17 June 1969, Box 59, Helmut K. Buechner Papers, Record Unit 7279, Smithsonian Institution Archives, Washington, DC. A month later, Craighead wrote again to Buechner with a breakdown of collar weight from Radiation; “as you can see we have a lot of work ahead of us”; John J. Craighead to Helmut K. Buechner, 15 July 1969, Box 59, Helmut K. Buechner Papers, Record Unit 7279,

tested a 23-pound mockup on a female elk in the National Bison Range near Moiese, Montana. After wearing the collar for 90 days, the elk showed some rubbing of hair and skin, but otherwise tolerated it “surprisingly well.”⁸⁵ After this successful dress rehearsal, the team decided to go ahead with the planned test of an operational collar on a free-ranging elk at the National Elk Refuge.

The Craigheads had been attuned to the public relations aspects of research on wild animals ever since they wrote their first article about falconry for *National Geographic* in the late 1930s, and they tried to control the publicity around the elk-tracking project as much as possible. They were especially intent on doing so because the experiment was, despite its claims to producing scientific knowledge, fundamentally an exercise in publicity. The scientific stakes were low, but a successful demonstration might convince NASA that its own post-Apollo image could be burnished by animal tracking. However, the involvement of multiple agencies and the distance of Wyoming from Washington made it far more difficult for the Craigheads to control the flow of information than it had been in the case of their Yellowstone grizzly project, which itself had begun to run into serious trouble in the late 1960s. The first sign of problems with the elk study was a Goddard press release that claimed the elk would be collared in Yellowstone National Park; the problem was that the release was issued in the spring of 1969, well before the Craigheads had reached any kind of agreement with the National Park Service, which had become increasingly unhappy with the Craigheads.⁸⁶ More adverse publicity arrived on November 20, 1969, when the *Christian Science Monitor* published a critical article about the

Smithsonian Institution Archives, Washington, DC.

85 Helmut K. Buechner, “Satellite Tracking of Elk: Status Report for the Annual Report of the National Air and Space Council,” 19 November 1969, Box 20, Office of Environmental Sciences, Ecology Program Records, 1965-73, Record Unit 271, Smithsonian Institution Archives, Washington, DC.

86 The Goddard news release is referred to in Helmut K. Buechner to George J. Jacobs, 16 December 1969, Box 58, Helmut K. Buechner Papers, Record Unit 7279, Smithsonian Institution Archives, Washington, DC.

study that began, “Space scientists have a few things to learn about biology.”⁸⁷ John Craighead urged Buechner to rein in NASA's press office, which he assumed was responsible for the misleading article. “If they continue to release news items in such an irresponsible way we will find it difficult to make information available,” he wrote. “Frankly, I think that we might even go so far as to say that if this continues we may have to discontinue the project.”⁸⁸ Buechner was less sure than Craighead that NASA was to blame for the *Christian Science Monitor* coverage, but he promptly forwarded the article to George Jacobs at NASA along with a letter that recounted the troubled earlier history of publicity for the project and asked for his assistance in correcting such inaccurate and damaging reports. Both the scientific reputation of the principal investigators and the delicate agreements they had forged with agencies such as the National Park Service depended on it, he wrote.⁸⁹ In the wake of this incident, the Smithsonian's director of public affairs strongly urged Buechner to hand over all publicity responsibilities to his office, which would be able to coordinate more closely with NASA's public affairs people.⁹⁰

For the Smithsonian, the project was part of a broader effort to get involved in applied environmental issues—an effort motivated at least partly by the need to acquire new sources of funding as military sources began to dry up and the golden age of federal science seemed to be coming to an end. At a meeting of Smithsonian staff to discuss animal tracking in November 1969, Galler recommended that the Smithsonian play a leading role in developing satellite tracking in partnership with NASA, but he also suggested that Smithsonian staff “not toot our

87 “Scrappy Moe Tunes Out NASA,” *Christian Science Monitor*, 20 November 1969.

88 John J. Craighead to Helmut K. Buechner, 10 December 1969, Box 58, Helmut K. Buechner Papers, Record Unit 7279, Smithsonian Institution Archives, Washington, DC.

89 Helmut K. Buechner to John J. Craighead, 10 December 1969, and Helmut K. Buechner to George Jacobs, 16 December 1969, Box 58, Helmut K. Buechner Papers, Record Unit 7279, Smithsonian Institution Archives, Washington, DC.

90 Frederic M. Philips to Helmut K. Buechner, 17 December 1969, Box 58, Helmut K. Buechner Papers, Record Unit 7279, Smithsonian Institution Archives, Washington, DC.

horn or proclaim ourselves as a national center for animal tracking.”⁹¹ Initially, on January 7, 1970, the Smithsonian announced that an opportunity for the media to view the collared elk would be available in late February 1970, when an informal conference would be held at Jackson Hole.⁹² But the plan was changed after a special meeting of Smithsonian staff involved with the project was held a few days later to discuss publicity. Galler sent a memo describing the strict policies to be followed: no press releases during the initial attempt to tag an elk with an operational collar; no one invited to the field experiment who was not directly connected with the project; no press releases by invited guests; a conference to be held only after a successful test had concluded, rather than during it, probably at the Goddard Space Flight Center near Washington, DC, rather than at the field site in Jackson Hole. The elk tracking experiment was, Galler explained, “an excellent opportunity to demonstrate the Institution's interests and abilities to conduct research in ecology and animal behavior leading to the improvement of conservation techniques.”⁹³ Conversely, a failure might demonstrate the Smithsonian's unsuitability for such efforts. Worried that it would be impossible to maintain secrecy around the project for ten days after the planned collaring on February 19, 1970, Frank Craighead suggested a brief news release on the day of the test to placate members of the press.⁹⁴

In the event, the collaring did not go smoothly. The Craigheads hoped to recapture a female elk who had previously been used to test the dummy collar, but after an hour of chasing

91 “Minutes of Meeting Regarding Animal Tracking by Satellite, 12 November 1969,” Box 20, Office of Environmental Sciences, Ecology Program Records, 1965-73, Record Unit 271, Smithsonian Institution Archives, Washington, DC.

92 “Satellite Tracking of Elk,” 7 January 1970, Box 58, Helmut K. Buechner Papers, Record Unit 7279, Smithsonian Institution Archives, Washington, DC.

93 Assistant Secretary (Science) [Sidney R. Galler] to See Distribution [Philips, Reed, Wallen, Buechner, Jenkins, Aron, Maxwell, and Weiffenbach], copied to S. Dillon Ripley, 16 January 1970, Box 58, Helmut K. Buechner Papers, Record Unit 7279, Smithsonian Institution Archives, Washington, DC.

94 Helmut K. Buechner to Dale Jenkins, 9 February 1970, Box 58, Helmut K. Buechner Papers, Record Unit 7279, Smithsonian Institution Archives, Washington, DC.

the herd they were unable to get a clear shot at her with the dart gun. Instead they hit a different female elk; since she appeared to be healthy, they decided to proceed with collaring her. When “Monique,” as the replacement elk was dubbed, died of pneumonia less than a week later, the resulting news coverage was exactly the opposite of what the researchers, NASA, and the Smithsonian had hoped for.⁹⁵ Pneumonia was a relatively common disease in the elk herd, but even the Craigheads admitted that the stress of collaring had likely helped make the disease fatal.⁹⁶

Monique was not, of course, the first animal that the Craigheads had accidentally killed in the course of their research; a number of grizzly bears in Yellowstone, for example, had died in the early 1960s while they were still experimenting with tranquilizing and tagging techniques. Unlike these earlier cases, however, the experiment with “Monique” had been expressly designed to attract media attention. Moreover, attitudes toward scientific research on animals had shifted dramatically between the early 1960s and the early 1970s. Many more people were willing to question whether the promised results of research justified its means, and groups representing such concerns had grown much more vocal. In the spring 1970 issue of the Defenders of Wildlife newsletter, editor Mary Hazel Harris included a photograph of the collared elk apparently struggling to stand. The headline was: “‘Monique’ Death Ends Project.”⁹⁷ Critical letters poured in to the Smithsonian. The letters, most of which were from women, attacked the study primarily

95 Helmut K. Buechner, “First Progress Report on ‘Satellite (IRLS) Tracking of Elk,’” 14 October 1969; Helmut K. Buechner to George Jacobs, 12 March 1970; John J. Craighead, Frank C. Craighead, Charles E. Cote, and Helmut K. Buechner, “Second Progress Report on ‘Satellite (IRLS) Tracking of Elk,’” 8 April 1970; Box 59, Helmut K. Buechner Papers, Record Unit 7279, Smithsonian Institution Archives, Washington, DC.

96 Frank C. Craighead, John J. Craighead, Charles E. Cote, and Helmut K. Buechner, “Satellite and Ground Radiotracking of Elk,” 23 October 1970, Box 20, Office of Environmental Sciences, Ecology Program Records, 1965-73, Record Unit 271, Smithsonian Institution Archives, Washington, DC.

97 Mary Hazel Harris, “‘Monique’ Death Ends Project,” *Defenders of Wildlife News* (March 1970): 19, Box 58, Helmut K. Buechner Papers, Record Unit 7279, Smithsonian Institution Archives, Washington, DC.

but not exclusively on humanitarian grounds. One sixth-grade class in Pennsylvania whose teacher had learned about the study through the magazine of the American Anti-Vivisection Society wrote collectively to describe the study as “very cruel.”⁹⁸ Some letters also questioned the study's technical sophistication; Mary Vance of Albany, NY, for example, suggested that the researchers use “more modern methods, perhaps transistors to condense or lighten the weight of this equipment.”⁹⁹ A common rhetorical technique was to ask how the scientists would have felt if they were the ones being collared, whether by the animals or, as Dorothy Keyser of Waukegan, MI, suggested, by extraterrestrials: “Maybe someday some beings from another world who have conquered space will come down here to Earth and catch you and use *you* for an experiment. How would you like that?”¹⁰⁰ Several letters included religious themes, either accusing the scientists of using methods that “[n]o Christians should want to”¹⁰¹ or denying the scientists' right to “play 'God’”.¹⁰² One writer linked her distaste for the study to concerns about law and order that were helping to drive the conservative revival of the 1970s: “If the Government must test these devices, why not put them on the necks of thousands of criminals who are free to roam about killing and robbing innocent folks?”¹⁰³ The Animal Welfare Institute reprinted a letter from one professor of entomology, Richard B. Selander of the University of Illinois, who had

98 Mrs. R. Thorn (Leola, PA) to Helmut K. Buechner, with attached letter from members of sixth-grade class, n.d. (spring 1970), Box 58, Helmut K. Buechner Papers, Record Unit 7279, Smithsonian Institution Archives, Washington, DC.

99 Marie Vance (Albany, NY) to S. Dillon Ripley, 29 June 1970, Box 58, Helmut K. Buechner Papers, Record Unit 7279, Smithsonian Institution Archives, Washington, DC.

100 Dorothy Keyser (Waukegan, MI) to Helmut K. Buechner, 29 October 1970, Box 58, Helmut K. Buechner Papers, Record Unit 7279, Smithsonian Institution Archives, Washington, DC. Another writer asked, “Why not put such a collar on yourselves[?]”; Mrs. S.N. Levens to S. Dillon Ripley, 26 October 1970, Box 58, Helmut K. Buechner Papers, Record Unit 7279, Smithsonian Institution Archives, Washington, DC.

101 Mrs. S.N. Levens to S. Dillon Ripley, 26 October 1970, Box 58, Helmut K. Buechner Papers, Record Unit 7279, Smithsonian Institution Archives, Washington, DC.

102 Joanne J. Rongo (Providence, RI) to Helmut K. Buechner, 9 November 1970, Box 58, Helmut K. Buechner Papers, Record Unit 7279, Smithsonian Institution Archives, Washington, DC.

103 Catherine E. Heppe to S. Dillon Ripley, 24 November 1974, Box 20, Helmut K. Buechner Papers, Record Unit 7279, Smithsonian Institution Archives, Washington, DC.

criticized the elk study as an “awkward, cruel, and nonsensical 'experiment',” a “glorified high school Science Fair project,” and a “grandstand play for publicity” in the pages of central Illinois' *News-Gazette*.¹⁰⁴ The Secretary of the Smithsonian, S. Dillon Ripley, was forced to defend the project to members of Congress who had forwarded letters from their constituents to the Smithsonian or the Department of Interior.¹⁰⁵ The story grew to the point that the president of the Wildlife Society wrote to Buechner in March to ask for information that he could use to response to numerous telephone calls relating to television publicity about “the public's reaction to the transmitter elk which recently died (?) out west.”¹⁰⁶

After a more successful test in April, the tone of the press coverage improved somewhat; the *Jackson Hole News*, for example, published a short, factual article next to the artist's representation of the collared elk that had been distributed by Radiation, Inc., and a NASA photo of the Nimbus satellite.¹⁰⁷ When Buechner contacted the leadership of Defenders of Wildlife about publishing a more sympathetic follow-up to the original item that had provoked so much protest, he found the organization split between Harris, whose humanitarian focus mirrored that of many of the organization's grassroots supporters, and Victor Cahalane, who represented the organization's conservationist side, which sought to reform predator control policies and trapping

104 Richard B. Selander, “Protest Against Treatment of Elk,” *Animal Welfare Institute Information Report* 10 (April-June 1970): 3.

105 S. Dillon Ripley to Representative Charles E. Bennett, 11 December 1970, Box 59, Helmut K. Buechner Papers, Record Unit 7279, Smithsonian Institution Archives; Mike Mansfield to George Hartzog, 10 March 1970; Edward A. Hummel to Mike Mansfield, 30 March 1970, Box 58, Helmut K. Buechner Papers, Record Unit 7279, Smithsonian Institution Archives, Washington, DC. Mansfield's constituent's letter protesting the death of Monique astutely linked the incident to the senator's well-known campaigns against wasteful government spending: “I hope you are successful in your efforts to do away with some of the nonessential agencies and bureaus that were created in the past”; Robert A. Posey to Mike Mansfield, 28 February 1970, Box 58, Helmut K. Buechner Papers, Record Unit 7279, Smithsonian Institution Archives, Washington, DC.

106 Fred G. Evenden to Helmut K. Buechner, 5 March 1970-03-05, Box 59, Helmut K. Buechner Papers, Record Unit 7279, Smithsonian Institution Archives, Washington, DC.

107 “New Satellite for Elk,” *Jackson Hole (Wyoming) News*, 23 April 1970, Box 58, Helmut K. Buechner Papers, Record Unit 7279, Smithsonian Institution Archives, Washington, DC.

practices without losing the support of hunters and wildlife managers.¹⁰⁸ In July 1970, Buechner told Cahalane that the Smithsonian had received about one hundred letters in response to the Defenders of Wildlife newsletter article and asked him to publish a letter from the researchers and a sympathetic article written by E. Raymond Hall “for the benefit of those readers who have strong feelings on the matter and have not written in.”¹⁰⁹ By December 1970, however, Frank Craighead had concluded that further publicity was inadvisable. Reporting to Buechner on a conversation he had had with Craighead, river guide and nature author Verne Huser wrote, “I guess he's afraid that so many people are upset about it already, that any further publicity will just add to the upset.” As the recent “SST victory” had demonstrated, Huser went on, anti-technological attitudes were on the rise.¹¹⁰ Buechner and Ripley continued to receive indignant letters of protest from people who had belatedly come across the DOW news item even though no further elk were collared. In 1972, for example, Katherine Aspinall of Worcester, MA, wrote: “What savages you people become hiding behind the name of science” (“P.S. Who are you

108 E. Raymond Hall was a distinguished senior mammalogist who had earned his PhD in zoology under Joseph Grinnell in 1928, served as curator of mammals at Berkeley's Museum of Vertebrate Zoology from 1927 to 1944, and was chairman of the Department of Zoology and director of the Museum of Natural History at the University of Kansas from 1944 to 1967. His obituary in the *Journal of Mammalogy* described him as “one of the leading mammalogists the New World has produced”; James S. Findley and J. Knox Jones, Jr., “Eugene Raymond Hall: 1902-1986,” *Journal of Mammalogy* 70 (1989): 455-458, on 455. Hall had written an alternative and far more favorable account of the elk satellite study for publication in the Defenders of Wildlife Newsletter; see E. Raymond Hall to Mary Hazel Harris, 30 March 1970, Box 58, Helmut K. Buechner Papers, Record Unit 7279, Smithsonian Institution Archives, Washington, DC. Sidney Galler wrote to Hall in August 1970 to express his “respect and admiration” for Hall's “balanced presentation” of the elk incident; he told Hall that his article, which was never published, was “a wonderful example to illustrate a talk that I have been presenting here of late under the title, 'Honesty in Scientific Activism.' I wish that more of our colleagues would exercise the same care in their public pronouncements that they exercise in developing their research projects”; Sidney R. Galler to E. Raymond Hall, 27 August 1970, Box 58, Helmut K. Buechner Papers, Record Unit 7279, Smithsonian Institution Archives, Washington, DC. For more discussion of the elk incident, Victor H. Cahalane to Helmut K. Buechner, 26 July 1970; E. Raymond Hall to Victor H. Cahalane, 20 July 1970, Box 58, Helmut K. Buechner Papers, Record Unit 7279, Smithsonian Institution Archives, Washington, DC.

109 Helmut K. Buechner to Victor H. Cahalane, 14 July 1970, Box 58, Helmut K. Buechner Papers, Record Unit 7279, Smithsonian Institution Archives, Washington, DC.

110 Verne Huser to Helmut K. Buechner, 6 December 1970, Box 58, Helmut K. Buechner Papers, Record Unit 7279, Smithsonian Institution Archives, Washington, DC.

torturing to death this week?”).¹¹¹ Looking back on the event in 1972, Buechner largely blamed NASA's mishandling of the publicity for the protest. The letters, he explained to a Swiss collaborator with whom he hoped to conduct a satellite-tracking study of elephants in East Africa, “were amazingly stereotyped, indicating a response from a certain type of person in society.”¹¹²

In the wake of the troubled elk project, NASA's George Low convened a meeting in Washington to discuss the future of animal tracking by satellite. Buechner, Galler, and two other Smithsonian staff members were in attendance, as well as the head of the U.S. Fish and Wildlife Service, John Gottschalk, and Dwain Warner. Among the NASA representatives were Cote and George Jacobs, chief of physical biology in the Bioscience Program, who had been one of the main NASA advocates of the project, as well as Orr Reynolds, who in the late 1950s had sponsored several early biotelemetry projects while working at ONR with Galler.¹¹³ The researchers at the meeting worked hard to convince Low of the importance of the technique, usually by linking it to environmentalist concerns. In his introductory statement, for example, Galler emphasized the importance of satellite tracking for studying rare or endangered species whose movements carried them across geopolitical borders.¹¹⁴ Buechner stressed that “we are currently in the most critical period of time in man's history with respect to his environmental

111 Katherine M. Aspinall (Worcester, MA) to S. Dillon Ripley, 11 June 1972, Box 58, Helmut K. Buechner Papers, Record Unit 7279, Smithsonian Institution Archives, Washington, DC.

112 Helmut K. Buechner to Walter Leuthold, 26 June 1972, Box 58, Helmut K. Buechner Papers, Record Unit 7279, Smithsonian Institution Archives, Washington, DC. When I interviewed Charles E. Cote at his office in the spring of 2007, he showed me a several-inch-thick bound volume of photocopied newspaper articles about the elk project, almost all of it negative, that one of NASA's press relations people had assembled.

113 The meeting with Low was taped and transcribed for distribution; “Transcript of Proceedings: Presentation on Animal Tracking to Dr. Low, Washington,” 4 June 1970, Box 59, Helmut K. Buechner Papers, Record Unit 7279, Smithsonian Institution Archives, Washington, DC. For the meeting agenda and a list of attendees, see Helmut K. Buechner to Sidney R. Galler, I. E. Wallen, and Dale W. Jenkins, 5 June 1970, Box 59, Helmut K. Buechner Papers, Record Unit 7279, Smithsonian Institution Archives, Washington, DC.

114 “Transcript of Proceedings: Presentation on Animal Tracking to Dr. Low, Washington,” 4 June 1970, pp6-7, Box 59, Helmut K. Buechner Papers, Record Unit 7279, Smithsonian Institution Archives, Washington, DC.

relationships”¹¹⁵ and that satellite technology was particularly useful for “the study of animals in an open ocean, in desert areas, in huge open grasslands, and in the arctic regions.”¹¹⁶ Gottschalk stressed the usefulness of satellite tracking for studying migratory birds or for getting “a truly comprehensive international picture of what is probably an international resource,” the polar bear.¹¹⁷

Low's interest, however, was less in these general justifications for animal tracking by satellite than in what exactly had gone wrong with the elk study. In his presentation on the technical aspects of the study, Cote admitted that it had been a “mistake” to expand the study beyond a minimal, two-week demonstration. The decision to conduct an actual migration study had been made, Cote explained, because of the sense that “the success of the future in animal tracking [was] contingent on this experiment.” One of the consequences of that sense of the importance of the study was that the package weight had tripled, not only because an increased battery life was necessary but also because “no company ... will commit their reputations to a minimal package on those requirements.”¹¹⁸ At the end of the two-hour meeting, noting that of the two elk that had been collared, one elk had died and the other's transponder had gone off-line when the collar unexpectedly rotated around its neck, Low commented that “if it's worth doing, it's worth doing well. And the results to data have—I think it's been less than we'd like to see in some other programs.”¹¹⁹ Low's criticism provoked a defense by Cote: “if we're going to live in this environment where every time we lose an animal we're dead, we aren't going to achieve these sort of things.” Galler pointed out that the elk-tracking team was in the “hot seat” with

115 Ibid., 8.

116 Ibid., 10.

117 Ibid., 26.

118 Ibid., 35.

119 Ibid., 52.

regard to attitudes of the general public as well as scientists, who had long been skeptical of NASA's commitment to biology, and he confessed that he was partly to blame for having pointed out to the researchers “that if we failed on this initial experiment, it would add fuel to the fire of developing an additional kind of negative response.”¹²⁰ The public relations failures of the elk project did not put an end to NASA's involvement in animal tracking by satellite, but they did change their nature significantly.¹²¹ For the elk project, NASA had supported the development of the collar and encouraged Cote to become involved in fieldwork. After it became apparent how risky fieldwork with animals could be, NASA limited its support to data collection and analysis.

Radio-tagging had been meant to raise wildlife management above the “biopolitical” fray; now it, too, had become political. Wildlife biologists and ecologists often saw themselves as part of—or even the instigators of—the environmental movement, but as members of the scientific and technical elite they could also become its targets. Despite his enthusiasm for high-tech wildlife research, Buechner consciously opposed himself to “hard-core scientists” and technologists.¹²² In 1969, when a professor of cybernetics name T.C. Helvey sought Buechner's aid in gaining the Smithsonian's sponsorship of a seminar on urban planning, Buechner refused. Helvey's “technological approach,” he replied, was likely to lead to “regimented, dehumanized habitations for man. ... I would rather see a biological and humanistic approach, the technological approach being secondary.”¹²³ As the environmental movement gained strength,

120 Ibid., 53-54.

121 Charles Cote told me that he believed that NASA's withdrawal had little effect on the development of animal tracking by satellite because the necessary technology would not have been available until the late 1970s in any case; Interview with Charles E. Cote. October 2007.

122 Helmut K. Buechner, Evaluation for Science Magazine of R.A. Vaer, Jr., “Church, University, and Environmental Values,” n.d. [1972], Box 31, Helmut K. Buechner Papers, Record Unit 7279, Smithsonian Institution Archives, Washington, DC.

123 Helmut K. Buechner to T.C. Helvey, 6 January 1969, Box 21, Helmut K. Buechner Papers, Record Unit 7279, Smithsonian Institution Archives, Washington, DC.

however, ecologists such as Buechner found themselves often lumped in with the hard-core scientists they opposed. The irony of the situation was epitomized in 1970, when Buechner, as the Smithsonian's chief ecologist, was asked by the editors of *Science* to review an article on "American Institutions and Ecological Ideals" by Leo Marx, a professor of English and American studies at Amherst College. In the final lines of his article, Marx suggested that if the environmental crisis continued, the scientific community that had hitherto "enthusiastically placed its skill in the service of business and military enterprise" might begin to discourage its members from "serving the violators." Buechner recommended publishing Marx's article, but only after removing its occasionally "biased and reproachful" tone: "The last section is excellent, but the sarcasm in the last sentence weakens the author's position," Buechner wrote in his evaluation. "Not all scientists serve the violators."¹²⁴

Means and Ends

In the last few years of his life, Adolph Murie had reasons to be hopeful that hands-on research on wildlife, the "blemish to the wilderness spirit" against which he had been fighting for the previous decade and a half, was on the decline.¹²⁵ In 1974, the year of his death, radio-tagging had not yet tarnished the wilderness character of Mount McKinley National Park, soon to become Denali National Park and Preserve. The Craigheads, whom Murie greatly admired but

124 Helmut K. Buechner, n.d. [1970], Evaluation for Science Magazine of Leo Marx, "American Institution and The Ecological Ideal," Box 31, Helmut K. Buechner Papers, Record Unit 7279, Smithsonian Institution Archives, Washington, DC; Leo Marx, "American Institutions and Ecological Ideals," *Science* 170 (1970): 945-952.

125 Just before his death in August 1974, Murie wrote to the superintendent of Grand Teton National Park that "[i]t appears to many of us that in national parks wildlife management should be held to the minimum necessary." Adolph Murie to Gary Everhardt, 18 July 1974, Series 2, Box 2, Wilderness Society Records, CONS 130, Denver Public Library, Denver Colorado. Murie also expressed his opposition to tagging in a manuscript on grizzlies that he worked on in the 1960s but that was only published posthumously; Adolph Murie, *The Grizzlies of Mount McKinley* (Washington, DC: U.S. Government Printing Office, 1981).

whose research practices had, in his opinion, violated fundamental national park ideals, had been forced out of Yellowstone as part of a broad shift in policy toward less artificial research and management techniques and a stronger internal science capacity within the National Park Service. Outside as well as inside the parks, a newly attentive public was carefully scrutinizing scientific research to ensure that it would not violate animals' inherent rights or spoil areas of natural beauty such as Jackson Hole. Even Fred Dean, whose repeated proposals to tag Denali's grizzlies had inspired Murie's most passionate statements of opposition to the technique, seemed to have come around. Although he continued to support tagging outside the parks, as Murie himself did, Dean encouraged the National Park Service to restrict scientific research in the proposed new national park on Alaska's Katmai Peninsula to studies that would directly contribute to the management of the park.¹²⁶

It soon became clear, however, that Park Service administrators would not forgo a technique that could dramatically improve their control over park wildlife, even if they were sympathetic to Murie's concerns. A few years after the Craigheads were evicted from Yellowstone, an Interagency Grizzly Bear Study Team began to coordinate grizzly monitoring for the northern Rockies. Its researchers depended heavily on radio-tagging, and Yellowstone's administrators, including Cole, welcomed them in.¹²⁷ After the Yellowstone grizzly population was listed as "threatened" in 1975, it became difficult to justify not using the most advanced techniques to ensure the population's survival. Moreover, trapping, tranquilizing, and tagging

126 Frederick C. Dean, "Brown Bear/Human Interrelationship Study: Katmai," National Park Service Natural Resources Final Report AR-68-04 (Anchorage, AK: National Park Service, 1968).

127 In December 1975, for example, when the study team's head Richard Knight asked Cole for permission to replace the collars on two bears that were denning inside Yellowstone, it was granted immediately. Robert Knight to Glen F. Cole, 23 December 1975; Robert C. Haraden to Robert Knight, 31 December 1975, Box N-39, Natural and Social Sciences Records, Yellowstone National Park, Heritage and Research Center, Gardiner, MT. See also Nathaniel P. Reed to Director, National Park Service, 10 November 1973-11-10, Box N-119, Natural and Social Sciences Records, Yellowstone National Park, Heritage and Research Center, Gardiner, MT.

wildlife remained central to the park's daily management practices. As John Craighead reminded the Yellowstone superintendent in 1970, “Hardly a day passes that members of your biology staff do not contact us requesting biological information on animals that we have marked in the past.”¹²⁸ Even as the rangers were removing the Craigheads' tags in preparation for the Yellowstone centennial, they were also tagging every “problem bear” that they transplanted away from dumps or campgrounds.¹²⁹ In the short term, the Craigheads lost their battle to phase out the dumps gradually and to continue their research. Over the long term, they fundamentally transformed park practices. When the Interagency Grizzly Bear Study Team took over grizzly research in the greater Yellowstone area in 1973—without the participation of the Craigheads but with representatives from state fish and game agencies and the U.S. Fish and Wildlife Service—Superintendent Anderson justified the removal of the Craigheads' tags to one member of the team by saying that tags had been damaged or of dubious value.¹³⁰ But the study team insisted that it have the right to tag bears in and around Yellowstone, and the Yellowstone administration agreed, desperate for the outside legitimacy that the team could lend to its embattled grizzly management program. In the end, the eviction of the Craigheads represented not so much a rejection of tagging as a consolidation of Park Service control over bear research and management, which continued to rely on the very methods that the Craigheads' research had helped embed into park practice.

128 John J. Craighead to Jack K. Anderson, 24 July 1970, Box N-92, Natural and Social Sciences Records, Yellowstone National Park, Heritage and Research Center, Gardiner, MT.

129 Edmund J. Bucknall to All Park Rangers through Chief Park Ranger, 6 March 1971, Box N-116, Natural and Social Sciences Records, Yellowstone National Park, Heritage and Research Center, Gardiner, MT.

130 Robert B. Finley, Jr., to Jack K. Anderson, 15 June 1973; Jack K. Anderson to Robert B. Finley, Jr., 27 June 1973, Box N-36, Natural and Social Sciences Records, Yellowstone National Park, Heritage and Research Center, Gardiner, MT.

Even in Mount McKinley, the National Park Service's most visited “wilderness” park, wildlife radio-tracking was soon accepted as a useful, even crucial research technique. In 1970, the park's long-range wildlife management plan stipulated that any scientific research in the park would be “in keeping with basic Park policy. The premise that the end justifies the means will always bear close scrutiny.”¹³¹ Only a few years later, the park administration decided that the means of radio-tracking was well-suited to the end of understanding why the park's once-vast caribou herd had begun to dwindle. Whether and how the park should intervene to boost caribou numbers depended on whether the decline had been caused by human activity. Park administrators approved a radio-tracking study of caribou migration, as did the Denali Citizens Council, which judged that the small number of animals involved in the study diminished “the likelihood of visitors being offended by seeing these marked animals.”¹³² If, as Adolph Murie had believed, the radio-tagging of even a few animals would destroy the park's “wilderness character,” then Denali stopped being a wilderness sometime around 1977.

Similarly, the Arctic National Wildlife Refuge in Alaska's northeast corner also became a site for wildlife tagging despite the opposition of wilderness purists. ANWR had been established in 1960 after a campaign that Adolph Murie, Olaus Murie, and Mardie Murie had helped to lead. Olaus Murie had publicly called for the refuge to be devoted to “basic scientific research, with the least possible equipment. It should be for the kind of scientific study based on thinking, based purely on close observation, trying to understand the relations among various animal forms and

131 Assistant Director [Edward Hummel] to Regional Director, Northwest Region, 14 January 1970, Box 7, Alaska Task Force, General files, 1972-1978, National Park Service, Record Group 79, National Archives and Records Administration, Anchorage, AK.

132 Tom Adams [Chairman, Denali Citizen's Council] to C.B. Harry, 28 March 1976; C.B. Harry, “Assessment of the Environmental Impact on Proposed Study of the Distribution and Movement of the McKinley Caribou,” 8 June 1976, Box 52, Alaska Cultural Resources, National Park Service, Record Group 79, National Archives and Records Administration, Pacific Region, Anchorage, AK.

the changing environment. We need to understand more, to interpret honestly, what we see in wild country.”¹³³ Privately, he told the executive director of the Wilderness Society that he was pleased that the Fish and Wildlife Service budget for research in ANWR had been cut because the lack of funds would limit the Service's activity there.¹³⁴ By 1974, however, several bears had been tagged despite the refuge managers concerns that handling, drugging, and tagging might cause “personality change.”¹³⁵ As in Yellowstone, tagging continued in ANWR despite such compunctions. In 1985, one historian bemoaned the state of Alaska's wilderness in the pages of the *Environmental Review*: “Not even the brown bear ... can count on roaming freely in his territory without being shot with a tranquilizer, tagged and equipped with a radio transmitter.”¹³⁶

In addition to helping to transform wildlife management and research in the national parks, the Craigheads trained a generation of wildlife biologists in radio-tracking and other hands-on techniques, especially as applied to large carnivores. For example, the first graduate student to work on the Yellowstone grizzly project was Maurice Hornocker, who went on to become the leading expert on mountain lions in the United States. In the 1970s, Hornocker and his students brought radio-tracking to the study of other species of big cats in Central America, East Africa, and South Asia, including seminal work on tigers in Nepal by John Seidensticker, a graduate student of Hornocker's who had also been one of the Craigheads' undergraduate research assistants in Yellowstone in the mid-1960s. Furthermore, even in the absence of a direct

133 Olaus J. Murie, “Wilderness Philosophy, Science, and the Arctic National Wildlife Range,” Paper Presented at the 12th Alaskan Science Conference, Fairbanks, AK, 28 August-1 September 1961, Box 1, Series 2, Wilderness Society Records, CONS 130, Denver Public Library, Denver, CO.

134 Olaus J. Murie to Howard Zahniser, 16 September 1961, Box 1, Series 2, Wilderness Society Records, CONS 130, Denver Public Library, Denver, CO.

135 Don Frickie to Jack K. Anderson, 26 September 1974. Anderson responded that just such concerns about behavioral change had motivated Yellowstone's decision to stop marking bears “unless absolutely necessary to determine if we had in fact handled a bear previously”; Jack K. Anderson to Don Frickie, 22 October 1974, Box N-39, Natural and Social Sciences Records, Yellowstone National Park, Heritage and Research Center, Gardiner, MT.

136 Morgan Sherwood, “The End of American Wilderness,” *Environmental Review* 9 (1985): 197-209, on 203.

lineage, the Craigheads' highly publicized work served as an influential model. In 1969, in the middle of another round of tortuous negotiations with the Yellowstone administration, John Craighead tried to convince Superintendent Anderson that the techniques he and his brother had developed represented the future of wildlife management. "At the risk of appearing immodest," he wrote, "I think I can say that the techniques of color marking, immobilizing, handling, radiotracking, and data gathering that we, our colleagues, and our students developed or perfected in the course of ten years of research effort in the Park are now being widely applied in other national parks throughout the world."¹³⁷ Anderson was not convinced, but Craighead was right. By the 1980s, radio-tracking would be everywhere.

¹³⁷ John J. Craighead to Jack K. Anderson, 14 April 1969, Box N-196, Natural and Social Sciences Records, Yellowstone National Park, Heritage and Research Center, Gardiner, MT.

4

A THING OF THE PAST **TRACKING TIGERS IN NEPAL, 1973-1981**

Even after experiences such as that of the Craigheads in Yellowstone had made it clear that wildlife radio-tagging was not a panacea for the woes of wild animals or the people who studied them, American wildlife biologists were eager to export their self-consciously modern techniques to the developing world. They were aided in this goal both by the U.S. government, which saw conservation aid as part of a broader strategy for gaining the favor of Third World leaders, by zoological parks, which had begun to expand their support for scientific research and conservation as a way of justifying their existence in the face of growing criticism from environmentalists and animal rights activists, and by environmental organizations such as the World Wildlife Fund. One of the most successful and influential of these efforts to export American-style wildlife biology in the 1970s was the Smithsonian-Nepal Tiger Ecology Project, which was conducted in Royal Chitwan National Park from 1973 to 1981. Funded by the U.S. branch of WWF, it was structured as a partnership between Smithsonian scientists and current and former Nepalese forest officers. Although it ended more than a quarter-century ago, the project is still celebrated in the informational plaques surrounding the tiger exhibit at the Smithsonian's National Zoological Park in Washington, DC. When I visited the exhibit in the fall of 2007, on my way to an interview with the project's founding American scientist, the loud and repeated roars of one of the tigers served as a reminder that, despite the artificiality of zoos and their constant efforts to transform the animals they contain into metonyms for the species, the animals who live within them are as real as their conspecifics in the wild. Rather than telling

visitors about the particular animals on display, however, the informational plaques focused on the endangered status of the species and described the American and Nepalese scientists who had used radio-tracking to study it in the wild. The message was that the exotic and charismatic wildlife of the developing world were in danger, and that the technologies of legibility developed by American wildlife biologists were key to their survival.

As I had learned in the Smithsonian's rich archival records about the project, and as tiger scientist and conservationist John Seidensticker confirmed in the interview following my visit to the exhibit, the rosy picture of technology transfer, cross-cultural collaboration, and scientific discovery painted by the exhibit was only part of the truth. The project had faced numerous challenges, many of them derived from Seidensticker's commitment to radio-tracking, which he had learned to use first as an undergraduate research assistant for the Craigheads in Yellowstone and then as a doctoral student studying mountain lions in Idaho with Maurice Hornocker, who had himself earned his doctorate with the Craigheads. After failing to convince Indian officials that it was a good idea to allow American researchers to use high-tech surveillance gear to study one of India's national treasures, the project found a more welcoming home in Nepal, where Kirti Man Tamang, a former forest officer studying for a doctorate in Michigan, helped smooth relationships with Nepal's monarchical government. Even with Tamang's help, however, the proposed radio-collaring of tigers in the newly-established Royal Chitwan National Park met fierce opposition from the Tiger Tops ecotourism lodge, from the international headquarters of the World Wildlife Fund in Switzerland, and from former East African colonial game officers affiliated with the U.N.'s Food and Agriculture Organization. For the Tiger Tops lodge, in particular, which was the only tourism concession operating in the park in the early 1970s, radio-

tagging appeared to threaten the bottom line. As its research director Charles McDougal argued in a letter to the Nepalese forestry department in the fall of 1973, a few months before the project officially began, “a tiger with a radio hanging around its neck is no longer an attraction.”¹ Thus, unlike the Craigheads, who were able to perfect their radio-tagging techniques in Yellowstone before they came under serious public scrutiny, and unlike Marshall, whose dramatic initial failures in tracking grouse took place in the relative privacy of a dedicated research site, Seidensticker's and Tamang's work began under fire, in a field site to which hostile parties had already laid claim. Furthermore, as Seidensticker soon discovered, radio-tagging tigers was not “a one man sneaking around in the grass with a dart gun operation.”² It was a massive operation that required the coordinated effort of experienced shikaris, trained elephants and mahouts, and numerous assistants. Within a few months, under the strain of difficult field conditions, constant criticism, and an unclear division of labor, the initially positive relationship between Tamang and Seidensticker had turned into a bitter struggle for control. If the Smithsonian-Nepal Tiger Ecology Project was an example of what Ramachandra Guha has called “authoritarian biology,” it was, at least at the beginning, an authoritarianism whose authority was fragile, contested, and internally divided.³

Ultimately, however, the project succeeded, which raises the question of how and why it was able to do so in the face of these serious challenges. One answer is that Tamang and Hemanta Mishra, a forest officer who took Tamang's place as the Nepalese co-principal

1 Charles McDougal to Secretary of Forests, His Majesty's Government of Nepal, 8 September 1973, Box 24, Smithsonian Institution, Assistant Secretary for Science Records, 1963-1978, Record Unit 254, Smithsonian Institution Archives, Washington, DC.

2 John C. Seidensticker to Michael R. Huxley, 25 December 1973, Box 24, Smithsonian Institution, Assistant Secretary for Science Records, 1963-1978, Record Unit 254, Smithsonian Institution Archives, Washington, DC.

3 Ramachandra Guha, “The Authoritarian Biologist and the Arrogance of Anti-Humanism: Wildlife Conservation in the Third World,” *Ecologist* 28 (1997): 14-20.

investigator of the project in 1976, saw the use of radio-tagging as a route to credibility within the international conservation community. Like Marshall and the Craigheads before them, they recognized that the technologically-mediated, intimate relationships with wild animals that the technique provided would give them a stronger voice in conservation debates. Furthermore, because of their expertise and the expertise of their field staff in techniques of tiger tracking and hunting that had been developed in the context of colonial and aristocratic hunting, they were able to reframe the technique as a hybrid of modern American technology and indigenous traditions. Initial attacks on the project by the Tiger Tops management and other critics focused on the aesthetics of radio-collars, which seemed to undermine the tiger's value to Western tourists as a symbol of exotic wildness. Over the course of the project, however, Tamang, Mishra, and their American collaborators were able to shift the focus to the pageantry of the hunt—to the skilled trackers who determined a tiger's location, the line of trained elephants that drove the animal out of hiding, the long sheets of white muslin that directed it toward a shooter hiding behind a tree blind. Visiting journalists, film crews, conservation leaders, and VIPs of all kinds were treated to the experience of accompanying researchers on elephant-back as they searched for and engaged with tigers, leopards, or rhinos. In 1981, for example, the president of WWF-US, Russell Train, visited the project, and he and his wife Aileen accompanied Mishra on several attempts to radio-tag a leopard. Describing in his journal the aristocratic hunts that had once taken place in Chitwan, he wrote, “While we can be glad that such massacres are a thing of the past, a sad aspect of the matter is that the training and keeping of elephants is a rapidly disappearing art. Our tiger project is one of the last—perhaps even the last—examples of their utilization in this fashion.”⁴ Through its hybridization with hunting traditions, radio-tracking had

4 Russell E. Train, Journal Entry, 6-9 February 1981, Box 36, Russell E. Train Papers, Library of Congress,

become both a route to professional authority for biologists from the developing world and a continuation of colonial-era practices under new terms.

Postcolonial Conservation

The postwar boom in international wildlife conservation began not in South Asia but in Africa, and in particular with concerns among American and European sportsmen and conservationists about the impact of decolonization on African wildlife. For Americans and Europeans concerned with the fate of wildlife in poor, newly independent nations, Africa served as an archetype and a laboratory.⁵ Despite widespread interest in international conservation immediately after World War II, including efforts by Progressive-era icon Gifford Pinchot to organize an international conservation conference, little concrete progress was made for next decade and half. The International Union for the Conservation of Nature, for example, remained small, underfunded, and largely ineffective during the decade after its founding in 1948.⁶ In the United States, despite internationalizing efforts by Aldo Leopold, William Vogt, Ira N. Gabrielson, and other leaders of the wildlife profession, as well as hopeful discussions within the pages of the *Journal of Wildlife Management* about the impact of wartime experience abroad on the current generation of wildlife students, attention remained focused on domestic issues.⁷

Washington, DC; Hemanta R Mishra to Kathryn S. Fuller, 18 July 1994, and Hemanta R. Mishra to Russell E. Train, 1 August 1994, Box 13, Russell E. Train Papers, Library of Congress, Washington, DC. On the genealogical links between colonial big-game hunting and postcolonial wildlife conservation, see John MacKenzie, *The Empire of Nature: Hunting, Conservation, and British Imperialism* (New York: Manchester University Press, 1988).

5 On history of the College of Wildlife Management at Mweka, see Elizabeth Garland, "State of Nature: Colonial Power, Neoliberal Capital, and Wildlife Management in Tanzania," Unpublished PhD Thesis (Chicago: University of Chicago, 2006).

6 The organization was originally called the International Organization for the Protection of Nature; it is now known as the World Conservation Union/IUCN.

7 Tracy I. Storer, Editorial, *Journal of Wildlife Management* 10 (1946): 75.

In the late 1950s, however, as European nations relinquished formal control of colonies in Africa and Asia, the prospect of native control over the colonial apparatus of wildlife management began to transform talk into action. In 1958, Harold J. Coolidge, a mammalogist at Harvard's Museum of Comparative Zoology who had been involved in international conservation since the 1930s, explained to one colleague the urgency of the situation: "We are faced with the establishment of new nations which have little or no experience in handling natural resources wisely, as well as continued and aggravated ruthless exploitation of resources in many areas."⁸ The first Fulbright fellowships for American wildlife biologists to work in East Africa were granted in the late 1950s, American and European philanthropies began pouring money for equipment and supplies into East Africa, and the IUCN organized the first major African wildlife conference, held in Tanganyika in 1960.

In addition to galvanizing existing international conservation organizations, the perceived risks to wildlife in and from independent African nations gave birth to new organizations dedicated to transferring the techniques and values of modern wildlife management to postcolonial states. The African Wildlife Leadership Foundation, for example, focused on providing wildlife management training to native Africans in advance of the expected expulsion of non-natives from leadership positions in the former British colonies of Kenya, Tanganyika, Zambia, and Uganda in 1966. The motive force behind AWLF was Russell E. Train, a judge from a prominent Washington, DC, family who had first traveled to Africa on safari in 1956. In 1962, Train explained the need for such training to C.R. "Pink" Gutermuth of the Wildlife Management Institute. "If it is not forthcoming there is going to be a wholesale replacement of

8 Harold J. Coolidge to Ernest Brooks, Jr., 5 December 1958, Carton 74, Sierra Club Records, Collection 71/103c, Bancroft Library, University of California, Berkeley, CA.

Europeans in the game and park departments by Africans *long before 1966* and these Africans are going to be utterly unqualified with absolutely disastrous results insofar as the game is concerned,” he wrote.⁹ Train's social and political connections in Washington and with prominent conservationists such as Coolidge helped AWLF quickly become a major force in East African wildlife conservation. The organization's first major project, which was initially suggested by East African colonial game warden Bruce Kinloch, was the establishment of a school where British expatriates from the colonial wildlife administration could train their native African replacements, with funding from the U.S. Agency for International Development, AWLF and its private donors, and the Frankfurt Zoological Society

For international conservation organizations as well as for many individual scientists and conservationists, wildlife conservation in decolonizing East Africa was a formative experience that shaped their subsequent work in other parts of the world. George Schaller, for example, who would go on to study tigers in India, snow leopards in the Himalayas, pandas in China, and many other East Asian species and habitats, carried out his first international work on mountain gorillas in East Africa the early 1960s. George Petrides, a professor at Michigan State University, was one of the first wildlife biologists sent to Africa on a Fulbright in the late 1950s; a decade and a half later, he served as dissertation advisor for Kirti Man Tamang, Seidensticker's co-principal investigator for the Smithsonian tiger project. Train used the AWLF as a springboard to a distinguished career in conservation, including work for the Conservation Foundation in the 1960s and the directorship of the Environmental Protection Agency in the 1970s. In the early 1990s, as president of WWF-U.S., Train was still emphasizing the themes of leadership training

⁹ African Wildlife Leadership Foundation Press Release, 29 July 1962, Box 44, Harold Jefferson Coolidge Papers, Administrative Papers of the IUCN etc., 1941-1969, HUGFP 78.14, Harvard University Archives, Cambridge, MA.

that he had first developed for AWLF some thirty years earlier, though now applied to conservation projects throughout the world. As organizations such as WWF-U.S., the New York Zoological Society, and the Smithsonian Institution built up their overseas presence in Asia, Latin America, and Oceania beginning in the late 1960s, they drew heavily on their experiences in decolonizing Africa earlier in the decade.

Because of its unparalleled opportunities for wildlife research, conservation, and fundraising, decolonizing Africa became the site of a scramble for territory, mostly figurative but sometimes literal, among non-governmental conservation organizations. In the United States, for example, the upstart AWLF came into direct conflict with the New York Zoological Society, which was aggressively expanding its international programs under the leadership of Fairfield Osborn, including significant work in Africa.¹⁰ The establishment of the World Wildlife Fund's U.S. Appeal in 1962 added another competitor to the field. Although WWF-U.S. was founded as part of an international family of national organizations dedicated to raising funds for IUCN's headquarters near Geneva, where decisions about how to spend the funds were to be made, the American branch quickly established its own research and conservation agenda. The U.N.'s Food and Agriculture Organization, staffed in part by ex-colonial officials and focused more on development than on conservation, represented another potential competitor, sometimes verging on an opponent. Although Americans working in East Africa and South Asia were highly dependent on British expatriates and former colonial game wardens and often had good working relationships with them, they also experienced clashes of culture and values. Helmut K. Buechner, for example, during his first year in East Africa in 1957, complained of having

¹⁰ Minutes, Board of Trustees 30 January 1964, Box 44, Harold Jefferson Coolidge Papers, Administrative Papers of the IUCN etc., 1941-1969, HUGFP 78.14, Harvard University Archives, Cambridge, MA.

difficulties adjusting to the “locals,” by which he meant the expatriate Brits, some of whom seemed to him intolerably racist in their treatment of native Africans.¹¹ On the level of diplomacy, Train was extremely wary of recruiting conservation-minded European royalty, such as Belgium's Prince Bernhard, to advocate for wildlife conservation in Africa because of the inevitable colonial overtones.¹²

By the late 1960s, when Ripley offered the Smithsonian's assistance in studying tigers to South Asian nations, even the kind of technical training that AWLF encouraged had become politically suspect in many developing nations. Scientific research also came under attack. In 1967, Coolidge advised a fellow WWF-U.S. director to tread softly during his visit to Central America, where many scientists and officials “have a feeling that we are constantly sending people down to tell them what to do, to push them around, and to criticize them.”¹³ Revelations that American scientists and development experts had provided cover for counterinsurgency efforts in Latin America and Southeast Asia put American scientists working in developing countries under added suspicion, and especially those with connections to governmental organizations such as the Smithsonian. The Smithsonian's reputation, in particular, had been compromised by its participation in military-sponsored projects such as the Pacific Ocean Bird Survey, which was accused of being a cover for Army biological weapons work. In the aftermath of the bird survey controversy, J.W. Fulbright warned Ripley that it would be “very wise” for the Smithsonian to avoid all further funding from the Department of Defense.¹⁴ The Smithsonian's

11 Helmut K. Buechner to Bruce G. Kinloch, 31 October 1957, Box 8, Helmut K. Buechner Papers, Record Unit 7279, Smithsonian Institution Archives, Washington, DC.

12 Russell E. Train to Philip K. Crowe, 13 June 1962; Russell E. Train to C. R. Gutermuth, 15 June 1962, Box 39, Russell E. Train Papers, Library of Congress, Washington, DC.

13 Harold J. Coolidge to Philip K. Crowe, 3 January 1967, Box 39, Harold Jefferson Coolidge Papers, Administrative Papers of the IUCN etc., 1941-1969, HUGFP 78.14, Harvard University Archives, Cambridge, MA.

14 S. Dillon Ripley to J.W. Fulbright, 6 June 1969; J.W. Fulbright to S. Dillon Ripley, 17 June 1969, Box 10,

reputation was not helped by the fact that its assistant secretary for science, Sidney R. Galler, had spent most of his career at the Office of Naval Research, or that Ripley had done intelligence work in Ceylon during World War II.¹⁵ When Ripley proposed that the Smithsonian conduct an ecological survey of the Mekong River region in 1972, as the war in Southeast Asia appeared to be winding down, one staff member warned him that any further revelations about Smithsonian links to USAID or the Department of Defense could undermine delicate negotiations then underway regarding the Smithsonian's research station in the Panama Canal Zone. "I do hope that all the existing SI arrangements with AID are 'clean'," the staff member wrote.¹⁶

In the context of general cutbacks in science funding in the late 1960s, the newly tainted nature of Department of Defense and USAID funding posed a serious challenge to one of Ripley's goals for the Smithsonian: the establishment of a global network of research sites on the model of the Smithsonian Tropical Research Institute in the Canal Zone. The Smithsonian's access to U.S. foreign currency reserves, the so-called P.L. 480 funds, was also largely cut off during this period. Without these government sources to rely on, the Smithsonian was forced to turn to non-governmental organizations such as WWF and to non-Smithsonian researchers such as Hornocker and Seidensticker. Although Ripley was closely connected to the leadership of WWF-U.S., relying on it for research support carried complications of its own. Tension between WWF-U.S., one of the largest and by far the most independent of the WWF National Appeals, and the WWF-International office in Switzerland had been growing since the early 1960s. By

Smithsonian Institution, Assistant Secretary for Science Records, 1963-1973, Record Unit 108, Smithsonian Institution Archives, Washington, DC.

15 David Challinor to Sidney R. Galler, 27 August 1968, Box 10, Smithsonian Institution, Assistant Secretary for Science Records, 1963-1973, Record Unit 108, Smithsonian Institution Archives, Washington, DC.

16 Martin Moynihan to S. Dillon Ripley, 29 December 1972, Box 18, Smithsonian Institution, Assistant Secretary for Science Records, 1963-1973, Record Unit 108, Smithsonian Institution Archives, Washington, DC; S. Dillon Ripley to Russell E. Train, 23 February 1973, Box 105, Smithsonian Institution, Assistant Secretary for Science Records, 1963-1978, Record Unit 254, Smithsonian Institution Archives, Washington, DC.

1969 the relationship had degenerated into what aviator Charles Lindbergh, who had begun campaigning for international wildlife conservation in the 1960s, described as a “melee.”¹⁷ Operation Tiger, a campaign to raise funds for South Asian tiger conservation, only exacerbated the tensions between WWF-U.S. and WWF-International. After a slow start in the early 1960s, when the WWF's British Appeal carried the fundraising burden almost single-handedly, WWF-U.S. had become the richest of the national appeals and the largest contributor to Operation Tiger. As their disaffection with WWF-International and IUCN grew, WWF-U.S. trustees became increasingly unwilling to give up control over how the funds they had raised were spent. In the early 1970s, the Smithsonian's proposed tiger project became a focal point for conflict between WWF-U.S. from WWF-International and nearly split the two organizations apart permanently.¹⁸

At the moment when American biologists became seriously interested in exporting wildlife telemetry to developing countries, the diplomatic and financial conditions for such exercises in technology transfer became much less welcoming than they had been a decade earlier. Efforts to export the technology had begun almost as soon as it was invented. In 1962, for instance, when the Craigheads were still perfecting their telemetry system for grizzlies in Yellowstone and before William Cochran had published his seminal circuit design in the *Journal of Wildlife Management*, Glen and Beverly Sanderson had already taken the technique to Malaya to study disease-carrying rats for the U.S. Army.¹⁹ The same year, Irven O. Buss, after returning

17 Charles A. Lindbergh to Harold J. Coolidge, 29 March 1969, Box 6, Harold Jefferson Coolidge Papers, Correspondence and Other Papers, 1904-1985, HUGFP 78.17, Harvard University Archives, Cambridge, MA.

18 Harold J. Coolidge to Frank Fraser Darling, 31 March 1969, Box 37, Harold Jefferson Coolidge Papers, Administrative Papers of the IUCN etc., 1941-1969, HUGFP 78.14, Harvard University Archives, Cambridge, MA.

19 Glen C. Sanderson and Beverley C. Sanderson, “Radio-Tracking Rats in Malaya: A Preliminary Study,” *Journal of Wildlife Management* 28 (1964): 752-768.

from a Fulbright scholarship to East Africa, had tried convince the Craigheds to share their equipment for a study on elephants. The real boom in attempts to export the technique began in the late 1960s and early 1970s, however, when radio-tracking studies were launched in Africa, Latin America, and South Asia. Another sign of the growing interest in international applications was a biotelemetry meeting held in Pretoria, South Africa, in December 1971, which brought together most of the leading figures in the field, including John Craighead and John Tester. In the late 1960s, USAID began funding biotelemetry work at the Denver Wildlife Research Center as part of its pest control program for developing countries. In all of these projects, wildlife biologists and their partners in developing countries were confronted by numerous diplomatic and politic challenges, including opposition from local residents, national governments, and international organizations. As in the domestic context, field practice was one of the sites where these subtleties were produced, negotiated, and, in some cases, resolved.²⁰

Diplomatic and Political Subtleties

As the international conservation movement gathered strength over the course of the 1960s, American wildlife biologists became increasingly interested in applying their new techniques abroad, particularly for research on high-profile endangered species such as the tiger. In the late 1960s, the Secretary of the Smithsonian Institution, S. Dillon Ripley, began warning

20 Irven O. Buss to P. L. Wright, 21 June 1962, Box 22, University of Montana Department of Zoology Records, RG 60, K. Ross Toole Archives, University of Montana, Missoula; Helmut K. Buechner to Hartmut Jungius, 7 March 1975, and Walter Leuthold, "WWF-Project No. 1155: Radio-tracking of elephants in Tsavo National Park, Kenya: Annual Report for 1974," 21 January 1975, Box 32, Helmut K. Buechner Papers, Record Unit 7279, Smithsonian Institution Archives, Washington, DC; John J. Craighead, "Trip Report of John J. Craighead, 1977," 21 December 1971, and Program for Biotelemetry Symposium, Pretoria, South Africa, 1971, 21 December 1971, Box 57, Helmut K. Buechner Papers, Record Unit 7279, Smithsonian Institution Archives, Washington, DC.; George B. Schaller to Helmut K. Buechner, 15 April 1968, Box 17, Helmut K. Buechner Papers, Record Unit 7279, Smithsonian Institution Archives, Washington, DC.

of the impending extinction of the tiger and offered the Smithsonian's assistance to South Asian nations in helping to prevent it. American scientists were not always welcomed with open arms in developing countries, however, either by residents or by representatives of the international conservation community. Ripley's proposal for tiger assistance came at a difficult time. By the late 1960s, the politics of exporting American technology had gotten much more complicated, and the American pose of innocence in contrast to its imperialist predecessors much harder to sustain. For the Smithsonian in particular, because of its close links to the U.S. government, but in general for U.S. researchers and sponsoring organizations, the 1970s were a chaotic time to do research abroad—a time for hashing out new arrangements, new protocols, for finding places where work could be done, and for forging hybrid practices. All of this was done in the context of a new sensitivity toward the reputation of the United States.²¹ In 1968, the Smithsonian's Assistant Secretary for Science—a booster of wildlife radio-tracking who had formerly headed the Office of Naval Research's Biology Branch—urged Ripley to brief Smithsonian-funded researchers on the new challenges of working abroad. “During these troublesome times when the activities of scientists no matter how pure in intent are not above suspicion, and when indeed the non-friends of the U.S. overseas appear to be multiplying, it is doubly important that scientists representing the Smithsonian Institution abroad have a full appreciation of the diplomatic and political subtleties that may affect their projects,” he wrote. Scientists were no longer free to ignore the political and diplomatic contexts of their work.²²

21 In 1967, Harold J. Coolidge cautioned Philip K. Crowe in 1967 before a trip to Latin America about “the tremendous sensitivity of the officials and scientists in the countries that you are going to be visiting. Many of them have a prejudice against the U.S. as you well know, and others have a feeling that we are constantly sending people down to tell them what to do, to push them around, and to criticize them.”; Harold J. Coolidge to Philip K. Crowe, 3 January 1967, Box 39, Harold Jefferson Coolidge Papers, Administrative Papers of the IUCN etc., 1941-1969, HUGFP 78.14, Harvard University Archives, Cambridge, MA.

22 Sidney R. Galler to S. Dillon Ripley, 18 December 1968, Box 10, Smithsonian Institution, Assistant Secretary for Science Records, 1963-1973, Record Unit 108, Smithsonian Institution Archives, Washington, DC. On the

The “subtleties” affecting the proposed tiger project proved to be numerous. From the Smithsonian's perspective, India was the logical place to conduct an intensive tiger study, but the combination of Indian nationalism, revelations that the Smithsonian had helped to provide cover for secret military research, and the generally tense relationship between the United States and non-aligned India in the early 1970s created a difficult environment for cooperation.²³ Handicapped by the lack of any large carnivore experts on its own staff, the Smithsonian turned for help to the best-known American expert on North America's largest cat, Maurice G. Hornocker, head of the Idaho Cooperative Wildlife Research Unit. In July 1971, the Smithsonian sent Hornocker on an exploratory tour of India, including a stop-over at IUCN headquarters in Switzerland. When he returned, Hornocker told the Smithsonian that he was “not enthusiastic” about conducting a country-wide survey, which he thought could best be carried out by Indian scientists with IUCN's support, but that an intensive ecological study of the tiger appeared promising.²⁴ The Smithsonian's reaction to Hornocker's suggestion was mixed; one staff member argued that allowing Hornocker to sidetrack the Smithsonian away from a country-wide census would undermine the institution's credibility and recommending holding the line for “*only* a survey but the *best* possible.”²⁵ Eventually, however, the Smithsonian agreed to go along with Hornocker's proposal, and Seidensticker made chemical immobilization and radio-tracking the

politics of the Smithsonian Tropical Research Institute in the Canal Zone during this period, see Catherine Ann Christen, “At Home in the Field: Smithsonian Tropical Science Field Stations in the U.S. Panama Canal Zone and the Republic of Panama,” *The Americas* 58 (2002): 537-575.

23 On the controversy over the Smithsonian's military-sponsored work, see Roy MacLeod, “Strictly for the Birds: Science, the Military and the Smithsonian's Pacific Ocean Biological Survey Program, 1963-1970,” *Journal of the History of Biology* 34 (2001): 315-352.

24 Director, Ecology Program, to Director, Office of Environmental Sciences, 15 September 1971, Box 15, Smithsonian Institution, Office of Environmental Sciences, Ecology Program Records, 1965-73, Record Unit 271, Smithsonian Institution Archives, Washington, DC.

25 I.E. Wallen to Dale W. Jenkins, 16 September 1971, Box 15, Smithsonian Institution, Office of Environmental Sciences, Ecology Program Records, 1965-73, Record Unit 271, Smithsonian Institution Archives, Washington, DC.

center of his research proposal. The techniques that had succeeded with grizzly bears in Yellowstone National Park and with mountain lions in Idaho, he argued, would work just as well on the other side of the world.²⁶ Convincing the scientists and government officials heading India's Project Tiger to endorse the study proved difficult, however. In March 1972, Seidensticker toured India with Smithsonian staff. While the Indian officials with whom they met were happy to talk about practical details of the proposed study, official approval remained elusive.²⁷ As summer and fall passed without any progress, it became clear that neither the Smithsonian nor radio-tagging would be welcome in India.²⁸ Even a visit by Ripley to New Delhi in to meet with Project Tiger leader Karan Singh and other Indian officials in September 1972 failed to produce results. The only projects likely to be approved were those that Indians had suggested themselves, as Ripley reported to his staff after returning to Washington.²⁹ As Michael Lewis has written, "The Indian state's power to control its territory trumped American economic power to bribe its way into India's national parks, at least for a few years."³⁰

26 Maurice G. Hornocker to Kennedy D. Schmertz, 23 July 1971; Director, Ecology Program, to Director, Office of Environmental Sciences, 15 September 1971, Box 15, Smithsonian Institution, Office of Environmental Sciences, Ecology Program Records, 1965-73, Record Unit 271, Smithsonian Institution Archives, Washington, DC.

27 John C. Seidensticker, Report on Tiger Research Development Trip, March 1972, Box 15, Smithsonian Institution, Office of Environmental Sciences, Ecology Program Records, 1965-73, Record Unit 271, Smithsonian Institution Archives, Washington, DC; John C. Seidensticker to Michael R. Huxley, 30 August 1972, Box 24, Smithsonian Institution, Assistant Secretary for Science Records, 1963-1978, Record Unit 254, Smithsonian Institution Archives, Washington, DC.

28 John C. Seidensticker to Dale W. Jenkins, 17 April 1972, Box 15, Smithsonian Institution, Office of Environmental Sciences, Ecology Program Records, 1965-73, Record Unit 271, Smithsonian Institution Archives, Washington, DC.

29 S. Dillon Ripley to Kennedy D. Schmertz, 12 October 1972, Box 15, Smithsonian Institution, Office of Environmental Sciences, Ecology Program Records, 1965-73, Record Unit 271, Smithsonian Institution Archives, Washington, DC. The 1970s were the nadir of the Smithsonian's post-World War II work in South Asia; see Michael Lewis, *Inventing Global Ecology: Tracking the Biodiversity Ideal in India, 1945-1997* (Hyderabad: Orient Longman, 2003).

30 Michael Lewis, *Inventing Global Ecology: Tracking the Biodiversity Ideal in India, 1945-1997* (Hyderabad: Orient Longman, 2003), 338.

Although India was home to the majority of the world's tiger population, a small population remained in the Nepalese *terai*, the grasslands and forests on the Nepal's southern border. The Smithsonian project's move to Nepal's Royal Chitwan National Park was made possible by a serendipitous encounter in 1972 at the Second World Conference on National Parks in Yellowstone—where the park administration had been zealously removing collars and tags from wildlife to ensure that the park would appear as “natural” as possible for the centennial—between Seidensticker and Kirti Man Tamang. Tamang was a former Nepalese forest officer who had recently served as the general manager of the Tiger Tops tourist lodge in Chitwan and was now studying for a PhD in wildlife management under George Petrides, a wildlife biologist at Michigan State University who had been working in international wildlife conservation since the late 1950s.³¹

Unlike the Indian officials with whom the Smithsonian had been negotiating unproductively for months, Tamang was enthusiastic about the project's potential. In December 1972, Tamang took Seidensticker on a whirlwind tour of government offices in Kathmandu, by the end of which they were in possession of a letter of approval from the Nepal's government.³² (Hornocker had withdrawn from the project in November 1972 to focus on big cats in Africa and to put some distance between himself and his former student Seidensticker, with whom his relationship had become increasingly tense.³³) It took another nine months of tortuous negotiations and additional visits to Nepal to revise the agreement so that it met Smithsonian

31 John C. Seidensticker IV to Michael R. Huxley, 30 August 1972, Box 24, Smithsonian Institution, Assistant Secretary for Science Records, 1963-1978, Record Unit 254, Smithsonian Institution Archives, Washington, DC.

32 Kirti Man Tamang to Michael R. Huxley, 18 October 1972, Kirti Man Tamang to Michael R. Huxley, 24 October 1972, and Jitendra R. Sharma to S. Dillon Ripley, 4 January 1973, Box 24, Smithsonian Institution, Assistant Secretary for Science Records, 1963-1978, Record Unit 254, Smithsonian Institution Archives, Washington, DC.

33 Maurice G. Hornocker to Michael R. Huxley, 8 November 1972, Box 24, Smithsonian Institution, Assistant Secretary for Science Records, 1963-1978, Record Unit 254, Smithsonian Institution Archives, Washington, DC.

requirements, but all parties were interested in seeing the project go forward. As Seidensticker noted, a “cooperative and hospitable atmosphere” was just as important as a large concentration of tigers to the success of the project, and the fact that Nepal had granted permission to use radiotracking equipment—“a sensitive issue everywhere on the Indian subcontinent”—was a particularly important sign of good faith.³⁴ The Nepalese Forest Ministry sent the Smithsonian a signed agreement that it could accept in September 1973, the radio-tracking gear cleared customs in October, and Seidensticker and Tamang started trying to catch their first tiger in December.³⁵

Funding for the project remained uncertain almost until the moment that field work began. With no foreign currency reserves or other sources of U.S. government funds to draw on, the Smithsonian was forced to appeal to the World Wildlife Fund for support, despite what Ripley considered the organization's “general amateurish quality” and an increasingly bitter feud between the U.S. branch of WWF and the organization's international headquarters in Morges, Switzerland.³⁶ Knowing that it would exacerbate tensions with the Morges office, the directors of WWF-U.S. approved an initial grant of \$30,000 to the Smithsonian in April 1973.³⁷ In November, as field work was about to begin, WWF-International sent an urgent cable to WWF-U.S. ordering it to suspend payment to the Smithsonian project, which it argued was draining money away from higher-priority conservation needs, and another cable to the Nepalese government implying that it could either support the Smithsonian project or accept a projected

34 John C. Seidensticker to Leonard Carmichael, 20 August 1973, Box 24, Smithsonian Institution, Assistant Secretary for Science Records, 1963-1978, Record Unit 254, Smithsonian Institution Archives, Washington, DC.

35 Emerald J.B. Rana to Michael R. Huxley, 19 September 1973, Box 24, Smithsonian Institution, Assistant Secretary for Science Records, 1963-1978, Record Unit 254, Smithsonian Institution Archives, Washington, DC.

36 S. Dillon Ripley to Michael R. Huxley, 13 September 1973, Box 120, Smithsonian Institution, Assistant Secretary for Science Records, 1963-1978, Record Unit 254, Smithsonian Institution Archives, Washington, DC.

37 WWF-U.S., “Projects for Recommendation to the April 16, 1973 Board Meeting for Approval and Fund Raising,” 16 April 1973, Box 1, Harold Jefferson Coolidge Papers, Administrative Papers of International Conservation Organizations, 1946-1977, HUGFP 78.19, Harvard University Archives, Cambridge, MA.

\$132,000 of WWF funds for tiger conservation, but not both.³⁸ Feigning a misunderstanding, Nepalese forestry officer Hemanta R. Mishra, who would later replace Tamang as co-principal investigator for the tiger project, cabled back to inform WWF-International that the Smithsonian project was approved and that Nepal would happily accept the proffered funds.³⁹ WWF-U.S., for its part, simply ignored WWF-International's request.

Another major source of opposition to the Smithsonian project was the Tiger Tops Jungle Lodge, a tourist hotel established in Chitwan in the late 1960s. The Tiger Tops management was concerned that, as its research director Charles McDougal put it, “a tiger with a radio hanging around its neck is no longer an attraction.” In September 1973, McDougal pleaded with Nepal's Secretary of Forests to site the tiger project somewhere other than Chitwan. If the study was to be conducted in the park, McDougal asked that it avoid collaring the lodge's “resident tigers,” a male and two females with cubs that regularly appeared at the Tiger Tops bait stations, where guests of the lodge could watch them feeding under spotlights from behind blinds.⁴⁰ McDougal's concerns and those of his boss, a former Pan Am executive named Jim Edwards, were seconded by United Nations Food and Agriculture Organization's wildlife advisor in Nepal. As early as January 1973, the FAO advisor had urged the Smithsonian to site the project somewhere other than Chitwan, where conflicts with tourists and villagers were likely.⁴¹ The relationship between Tiger Tops and the Smithsonian remained tense throughout the course of the project. In early

38 Telex, WWF-International to WWF-U.S., 25 November 1973, Box 24, Smithsonian Institution, Assistant Secretary for Science Records, 1963-1978, Record Unit 254, Smithsonian Institution Archives, Washington, DC.

39 John C. Seidensticker IV to Michael R. Huxley, 2 December 1973, Box 24, Smithsonian Institution, Assistant Secretary for Science Records, 1963-1978, Record Unit 254, Smithsonian Institution Archives, Washington, DC.

40 Charles McDougal to Secretary of Forests, His Majesty's Government of Nepal, 8 September 1973, Box 24, Smithsonian Institution, Assistant Secretary for Science Records, 1963-1978, Record Unit 254, Smithsonian Institution Archives, Washington, DC.

41 John Blower to Michael R. Huxley, 11 January 1973, Box 24; see also John H. Blower to Hemanta R. Mishra, 11 September 1973, Box 24, Smithsonian Institution, Assistant Secretary for Science Records, 1963-1978, Record Unit 254, Smithsonian Institution Archives, Washington, DC.

1976, for example, Smithsonian researchers collared a large male tiger just east of a guard post near the center of the park, which had been used as an informal marker of the territorial boundary between the Smithsonian project and the lodge. When the Tiger Tops staff realized that it was as one of the lodge's "residents," a large male whose territory extended to Chitwan's western border, it urged the Nepalese government to officially confine the Smithsonian research to the eastern half of the park.⁴²

As much as any theoretical innovations in biogeography or population dynamics, the experience of conducting research on endangered species in developing countries in the sensitive international context of the 1970s lay at the origin of the field that would emerge in the 1980s as "conservation biology." Eight months before tagging his first tiger in Chitwan, Seidensticker was already reconsidering the way he had initially framed the project. His experience with Indian and Nepalese government officials, the "endangered species 'heavies'" in Switzerland, and wildlife managers in the United States had convinced him that a new approach was necessary. "The moral here I guess is that if you are whacked about the ears often enough sooner or later you are bound to look for the reasons why," he told the Smithsonian administrator overseeing the project. Seidensticker had modeled his proposal for the tiger project on a radio-tracking study of grizzly bears in Yellowstone and on his work with mountain lions in Idaho, but "something is lacking," he wrote. "We are not turning people on." Instead of focusing exclusively on "natural populations"—whatever those are," scientists needed to start framing their proposals in terms of the biological, technical, and socio-economic constraints that kept populations of endangered species from flourishing. Doing so would highlight the urgency of field research, shift proposals

42 Hemanta R. Mishra to Charles McDougal, 5 March 1976, Box 53, Smithsonian Institution, Assistant Secretary for Science Records, Circa 1963-1986, Record Unit 329, Smithsonian Institution Archives, Washington, DC.

from passive to active, and carry “the ball one step more for the management people.” For tigers and other endangered species in South Asia, however, time was running out. “We have got to get with it,” Seidensticker concluded, “or there won't be anything to get with.”⁴³

The Vital Sense of Partnership and Full Cooperation

During their first month of field work, the project staff developed a methodology for capturing tigers that combined Seidensticker's experience with radio-tagging mountain lions in Idaho and Tamang's experience with finding tigers for tourists in Chitwan. The researchers sometimes spotted tigers alongside roads or in the brush, but most sightings took place when the tigers burst out of hiding to threaten one of the elephants they used to get safely around the park. A few days after the beginning of fieldwork, for example, a female tiger that Seidensticker was trying to shoot with a tranquilizing dart charged his elephant. The elephant had many years of experience in hunts by Nepal's ruling elite, but when the tiger charged she bolted for two hundred yards before coming to a stop. Despite such setbacks, the project quickly succeeded in attaching radio-collars to a leopard and a tiger, and Seidensticker judged the project's first twenty-five days a “great success.” The Switzerland-based “heavies” and their allies in the UN continued to intrigue against the project, but relations with Tiger Tops seemed to be improving. Jim Edwards, the lodge's touchy manager who had strenuously campaigned against the project with the Nepalese government, had even told Seidensticker that he Chitwan was “*the* place to do intensive work on tigers.” The biggest remaining challenge was ensuring a steady source of funding, without which, Seidensticker told the Smithsonian, the project had no chance of

43 John C. Seidensticker IV to Michael R. Huxley, 11 April 1973, Box 24, Smithsonian Institution, Assistant Secretary for Science Records, 1963-1978, Record Unit 254, Smithsonian Institution Archives, Washington, DC.

success. For the moment, for example, they could borrow well-trained government elephants, but they would soon have to pay for their own.⁴⁴

While the Smithsonian brought money, institutional support, and technological expertise that would have otherwise been unavailable in Chitwan, the success of the tiger project depended primarily on the knowledge and skills of Tamang and the Nepalese project staff, particularly the shikaris, or hunting guides. Tamang's experience as a forest officer and as general manager of Tiger Tops had made him “a tiger hunter of the first order,” and therefore just as important for finding and catching tigers in Chitwan as for handling political matters in Kathmandu.⁴⁵ Chief shikari Prem Bahadur Rai, with more than twenty years of tiger hunting experience, was also crucial for identifying tiger tracks and signs and directing a staff of about two dozen men in capture operations.⁴⁶ The project's method of catching tigers was modeled directly on techniques used for large aristocratic hunts. When a tiger had been located in the brush or drawn to a bait, the staff would unroll several hundred yards of white muslin cloth through the trees in the shape of a funnel. A line of elephants would then drive the tiger into the funnel, at the end of which one of the researchers would be waiting with a dart gun loaded with CI-744, an immobilizing drug.⁴⁷ When the darting was successful, the immobilized tiger was weighed, measured, and fitted with a radio collar and left to recover while researchers kept watch from a safe distance.

44 John C. Seidensticker to Michael R. Huxley, 25 December 1973; John C. Seidensticker to Michael R. Huxley, 2 December 1973, Box 24, Smithsonian Institution, Assistant Secretary for Science Records, 1963-1978, Record Unit 254, Smithsonian Institution Archives, Washington, DC.

45 Ibid.

46 Peter A. Jordan, Report on Visit to Smithsonian Tiger Project, Royal Chitwan National Park, November 1977, Box 54, Smithsonian Institution, Assistant Secretary for Science Records, Circa 1963-1986, Record Unit 329, Smithsonian Institution Archives, Washington, DC.

47 John C. Seidensticker to Michael R. Huxley, 2 December 1973, Box 24, Smithsonian Institution, Assistant Secretary for Science Records, 1963-1978, Record Unit 254, Smithsonian Institution Archives, Washington, DC.

As was perhaps inevitable for a project with two putatively equal leaders, each in possession of skills and resources that the other lacked, the American and Nepalese co-principal investigators frequently came into conflict over the eight years of the project. The most difficult collaboration was the first. Already in October 1973, before fieldwork had begun, Seidensticker was urging the Smithsonian to offer Tamang a monthly salary of only \$400, a sum that Seidensticker thought was more appropriate for a doctoral student than the \$1000 that Tamang had requested.⁴⁸ The relationship between principal investigators degenerated after fieldwork began and was exacerbated by poor relations between the Tamang's and Seidensticker's wives. Eventually Tamang returned to Kathmandu and refused to cooperate further, while Seidensticker struggled to keep working in Chitwan despite his inability to speak to the project's support staff, who spoke little English. After several months of uncertainty, the Smithsonian decided that its relationship with Tamang and—through him, the Nepalese government—was more important than its relationship with Seidensticker, and it gave Tamang full control of the project. The decision to remove Seidensticker from Nepal, he was told, was based mainly on his apparent reluctance “to genuinely resolve what has been a very serious problem of not developing the vital sense of partnership and full collaboration with the Nepalese involved in the project.”⁴⁹ At the same time, the Smithsonian's decision to back Tamang did not mean that its faith in him was unqualified. In 1976, as Tamang was wrapping up his research in Chitwan, one Smithsonian official warned another that he was “very Americanized in the thought and action” but maintained “a Nepalese attitude in terms of laxity and cunningness.”⁵⁰

48 John C. Seidensticker IV to Michael R. Huxley, 19 October 1973, Box 24, Smithsonian Institution, Assistant Secretary for Science Records, 1963-1978, Record Unit 254, Smithsonian Institution Archives, Washington, DC.

49 Michael R. Huxley to John C. Seidensticker IV, 9 July 1974, Box 53, Smithsonian Institution, Assistant Secretary for Science Records, Circa 1963-1986, Record Unit 329, Smithsonian Institution Archives, Washington, DC.

50 Ross Simons to Chris Wemmer, 31 August 1976, Box 27, Smithsonian Institution, Assistant Secretary for

The development of techniques for handling tigers inevitably led to a blurring of the line between research and management. Smithsonian project researchers were regularly asked to use their non-lethal capture methods to deal with rogue elephants and man-eating tigers. In 1974, after leaving Nepal, Seidensticker was invited to India to dart a man-eating tiger so that it could be transplanted to a new location; the capture and transportation were successful, but the young male was killed by another tiger almost as soon as it was released. As the growing tiger population inside Chitwan came into conflict with the growing human population immediately outside of it, chemical immobilization and radio-tracking helped reconcile wildlife control with conservation. In December 1978, for example, a schoolteacher on his way to morning ablutions at the border of the park was killed by a radio-collared tiger, leading to what Mishra later described as a “small riot” by villagers. Within twenty-four hours the tiger was tracked down using its radio-collar, immobilized using CI-744, and transported to the National Zoo in Kathmandu.⁵¹ Smithsonian administrators in Washington were wary about having a scientific research project transformed into a wildlife management service, but project staff found it difficult to refuse requests for help from the Nepalese government. In 1978, noting the project's increasing involvement in wildlife control, Theodore H. Reed, the director of the Smithsonian's National Zoological Park, advised the American graduate student then co-leading the project to avoid trying to make Smithsonian international relations policy in the field. Cross-border work with India, in particular, was dangerous territory. “These international relationships have to be handled by our superiors,” he told the graduate student. “The foot soldier may win the battle but

Science Records, 1963-1978, Record Unit 254, Smithsonian Institution Archives, Washington, DC.

51 Hemanta R. Mishra, “A Delicate Balance: Tigers, Rhinoceros, Tourists and Park Management vs. the Needs of the Local People in Royal Chitwan National Park,” n.d. (1981); Chris Wemmer, Ross Simons, and Hemanta Mishra, “Case History of a Cooperative International Conservation Program: The Smithsonian Nepal Tiger Ecology Project” (Draft, 1984), Box 59, Smithsonian Institution, Assistant Secretary for Science Records, Circa 1963-1986, Record Unit 329, Smithsonian Institution Archives, Washington, DC.

he does not plan the strategy.”⁵² Such advice was easier to give from Washington than it was to follow in Chitwan, where field conditions constantly required researchers to reconcile their research goals with the many other claims made upon them and upon the animals and habitats they studied.

The tiger project brought together some of the most influential developers of wildlife telemetry in the United States. In addition to the Smithsonian itself, which had gotten increasingly involved in telemetry since the mid-1960s, it drew on two main pools of expertise: the Craighead brothers and their students and colleagues in the Northern Rockies and the Bioelectronics Laboratory at the Cedar Creek Natural History Area in Minnesota. In the early and mid-1960s, Hornocker and Seidensticker had both worked as research assistants for the Craigheads' radio-tagging studies of elk and grizzlies in Yellowstone, the former as a graduate student and the latter as an undergraduate. After completing his master's degree, Seidensticker moved to Idaho to conduct a radio-tracking study of mountain lions under Hornocker, who had by then become the head of the Idaho Cooperative Wildlife Research Unit. Because the Craigheads were unwilling to share their equipment and because commercially available gear proved unsatisfactory in initial field tests, Seidensticker collaborated with an Idaho engineer named A.R. Johnson to develop radio-tracking receivers and transmitters from scratch. It was this equipment that Seidensticker and Tamang initially used to tag leopards and tigers in Chitwan. After Seidensticker left the project in 1974, the American co-principal investigators who followed him were all affiliated with the Cedar Creek group in Minnesota, and they replaced Seidensticker and Johnson's equipment with collars and receivers manufactured by

52 Theodore H. Reed to J.L. David Smith, 14 February 1978, Box 54, Smithsonian Institution, Assistant Secretary for Science Records, Circa 1963-1986, Record Unit 329, Smithsonian Institution Archives, Washington, DC.

Larry Kuechle and his colleagues. Melvin Sunquist, for example, who worked with Tamang in Chitwan from late 1974 to 1977, used his tiger research in Chitwan as the basis of a PhD under Donald B. Siniff, the University of Minnesota marine mammalogist and biostatistician who had helped develop Cedar Creek's automatic telemetry system in the mid-1960s. Sunquist's replacement, James L. David Smith, began working in Chitwan in 1977 and also used his work there as the basis of a PhD at the University of Minnesota. In addition to his primary advisor Peter Jordan, Smith's dissertation committee included Siniff and William H. Marshall, who had served as director of Cedar Creek, chair of the Wildlife Society's Telemetry Committee, and principal investigator of the pioneering NSF-sponsored "Grouse" project in the early 1960s.

Seidensticker's replacement, Sunquist, whom Simons described as "well qualified and even tempered," seems to have gotten along well with Tamang. Tensions re-emerged, however, after Sunquist was replaced by Smith, a former Peace Corps volunteer in Tanzania. A few months after Smith's arrival, Tamang was replaced by Hemanta R. Mishra, an officer in Nepal's National Parks and Wildlife Office who had supported the project since its beginning.⁵³ Smith initially won praise from everyone involved with the project, including Mishra, for his deft handling of both administrative matters in camp and political issues in Kathmandu. However, after Mishra returned to Nepal in 1978 from a year at the University of Edinburgh, the two began to spar over administrative issues. By May 1979, Mishra was demanding either that he and Smith be allowed to work completely independently, effectively splitting the Smithsonian project in half, or that he be allowed to take a "dictatorial stand" as the Principal Investigator approved by HMG and literally tell [Smith] what to do as he was told to do in the past before I joined the project.⁵⁴

53 Ross Simons to Kirti Man Tamang, 3 September 1974, Box 53, Smithsonian Institution, Assistant Secretary for Science Records, Circa 1963-1986, Record Unit 329, Smithsonian Institution Archives, Washington, DC.

54 Hemanta R. Mishra to Ross Simons, 25 June 1978; Douglas Heck to S. Dillon Ripley, 30 May 1978, Box 55, Smithsonian Institution, Assistant Secretary for Science Records, Circa 1963-1986, Record Unit 329,

Despite Seidensticker's optimism during the first month of fieldwork, Tiger Tops continued to oppose the project because of its aesthetic impact on Chitwan's tiger population and its potential impact on the lodge's business. To placate Edwards and McDougal, the Smithsonian researchers initially agreed to avoid tagging any tigers west of the Sukhibaar guard post located near the park's geographical center. The truce held until early 1976, when Sunquist and Tamang caught and collared a large male tiger just east of the guard post. When they showed McDougal a photograph of the tiger, he immediately recognized it as the lodge's "Dakre Tiger," whose territory extended to Chitwan's western border and who regularly visited the lodge's bait stations. This unintentional violation of the informal agreement provoked a new round of attacks by Tiger Tops on the Smithsonian project, including a strongly worded letter from McDougal to Mishra complaining that tagging was jeopardizing the viability of Tiger Tops as well as interfering with a documentary being filmed in Chitwan by Survival Anglia. Mishra, who would soon replace Tamang as the project's Nepalese principal investigator, supported the Smithsonian researcher's right to collar tigers throughout the park.⁵⁵ Donald B. Siniff, Sunquist's advisor, visited Chitwan in April 1976 and found Edwards and McDougal still very upset about tagging. He tried to convince them that a good interpretive program would make the collars "less distasteful" to their clients, but without much success.⁵⁶ These attempts at peace-making were interrupted in April 1976 by the accidental death of a tigress with yearling cubs from an overdose of CI-744, which

Smithsonian Institution Archives, Washington, DC; Peter A. Jordan, "Trip report: Visit to Smithsonian Tiger Project, Royal Chitwan National Park," November 1977, Box 54, Smithsonian Institution, Assistant Secretary for Science Records, Circa 1963-1986, Record Unit 329, Smithsonian Institution Archives, Washington, DC; Hemanta R. Mishra to Ross Simons, 3 May 1979, and attached "Gentlemen's Agreement," 6 November 1978, Box 55, Smithsonian Institution, Assistant Secretary for Science Records, Circa 1963-1986, Record Unit 329, Smithsonian Institution Archives, Washington, DC.

55 Hemanta R. Mishra to Charles McDougal, 5 March 1976, Box 53, Smithsonian Institution, Assistant Secretary for Science Records, Circa 1963-1986, Record Unit 329, Smithsonian Institution Archives, Washington, DC.

56 Donald B. Siniff to Ross Simons, 14 April 1976, Donald B. Siniff Personal Files.

led the Nepalese government to suspend all darting and tagging for an indefinite period. Although darting and collaring resumed a few months later, it remained a highly sensitive issue both for Tiger Tops and for the Nepalese government.⁵⁷ Representatives of the international conservation community also continued to criticize the project. After visiting Chitwan and hearing Smith describe Chitwan's tigers as “the most intensively studied population in the world” in 1977, for example, Francis L. Kellogg reported his unease with the project to WWF-International. “I could not help but wonder,” he wrote, “if the rare opportunity to observe and study *Panthera tigris* at Chitwan for Ph.D. theses had not overcome the necessity to allow this great animal such freedom from human pressure as is possible on today's overcrowded planet.”⁵⁸

The Meaning of Wildlife Conservation in Nepal

By 1976, Smithsonian administrators in Washington had begun to feel constrained by the project's narrow focus on the tiger and were attempting to broaden its focus. They began urging the researchers in Chitwan, both American and Nepalese, to focus on their broader goal of discovering “parameters for delineating natural reserve areas,” even though, for the moment, they might have to keep presenting their work to the Nepalese government and the World Wildlife Fund “under the tiger rubric,” as one Smithsonian administrator put it.⁵⁹ The weaknesses

57 Melvin Sunquist to Ross Simons, 29 April 1976, and Kirti Man Tamang to S. Dillon Ripley, 1 June 1976, Box 53, Smithsonian Institution, Assistant Secretary for Science Records, Circa 1963-1986, Record Unit 329, Smithsonian Institution Archives, Washington, DC.

58 Francis L. Kellogg to the WWF Director General, Report on a Visit to Nepal, November 1977; Douglas Heck to Francis L. Kellogg, 15 March 1978; David Challinor to Douglas Heck, 30 March 1978, Box 54, Smithsonian Institution, Assistant Secretary for Science Records, Circa 1963-1986, Record Unit 329, Smithsonian Institution Archives, Washington, DC.

59 Ross Simons to Kirti Man Tamang, J.L. David Smith, and Rebecca Troth, 10 November 1976, Box 53, Smithsonian Institution, Assistant Secretary for Science Records, Circa 1963-1986, Record Unit 329, Smithsonian Institution Archives, Washington, DC; Ross Simons to Chris Wemmer, 31 August 1976-08-31, Box 27, Smithsonian Institution, Assistant Secretary for Science Records, 1963-1978, Record Unit 254, Smithsonian Institution Archives, Washington, DC.

in this approach became apparent in 1976, when the botanical research of a graduate student funded by the Smithsonian was terminated by the Nepalese government, apparently because its connection to tiger conservation seemed too remote.⁶⁰ “[T]he meaning of wildlife conservation in Nepal hasn't advanced much beyond the concept of big game management as it was understood in the days of the British raj,” one Smithsonian administrator concluded.⁶¹ The incident helped convince Smithsonian administrators that it would have to de-emphasize tigers if it wanted to conduct broader ecological research.

By the late 1970s, the Nepalese government had its own reasons for seeking change. Among them was the high-risk nature of catching, tagging, and tracking tigers, both for researchers such as Tamang, who was seriously mauled in 1976, and for the tigers. The accidental death of the park's largest male tiger in 1979 triggered a major reevaluation of the project. Researchers had captured around twenty-six tigers since 1973, some of them repeatedly, and by 1979 nearly half of the park's tigers wore radio collars. The tiger in question was being caught in order to replace its collar; after it was hit with an immobilizing dart, it collapsed in a small pool and drowned before researchers could find it. Soon after the accident, the chief of the National Parks and Wildlife Conservation Office told the Smithsonian he was expecting final reports from all of the project's researchers.⁶² Fieldwork ended with the completion of Mishra's study of chital deer, one of the main prey species for Chitwan's tigers, in mid-1981. In an ironic

60 David Challinor and Ross Simons to S. Dillon Ripley, 3 April 1977, Box 53, Smithsonian Institution, Assistant Secretary for Science Records, Circa 1963-1986, Record Unit 329, Smithsonian Institution Archives, Washington, DC

61 Chris Wemmer to Ross Simons, 3 May 1979, Box 55, Smithsonian Institution, Assistant Secretary for Science Records, Circa 1963-1986, Record Unit 329, Smithsonian Institution Archives, Washington, DC

62 Biswa N. Upreti to Ross Simons, 28 November 1979, Box 55, Smithsonian Institution, Assistant Secretary for Science Records, Circa 1963-1986, Record Unit 329, Smithsonian Institution Archives, Washington, DC

twist, given his initial opposition to tagging in the park, McDougal—the research director of the Tiger Tops lodge—took charge of monitoring the remaining collared tigers.⁶³

One of the main legacies of the tiger project was the creation of the King Mahendra Trust for Nature Conservation, the idea for which emerged from discussions within the Smithsonian in the late 1970s. After visiting Nepal in 1977, the advisor of one of the Smithsonian project's American graduate students had praised the tiger project as “perhaps the most practical means of transferring technical knowledge and scientific approaches from our culture to another.”⁶⁴ But Smithsonian administrators believed that the continuation of such “scientific approaches,” including research by Smithsonian-affiliated scientists, would require a supportive institutional framework in Nepal that did not yet exist. In January 1978, Smithsonian administrators began discussing the possibility of establishing a “major ecological center on the subcontinent” at Chitwan.⁶⁵ By the early 1980s, the Smithsonian's thinking had evolved away from a zoological research station and towards a quasi-governmental trust that could funnel outside donations toward research and conservation in Nepal. Together with WWF-U.S., the Smithsonian succeeded in convincing the Nepalese royal family to charter the trust in 1983, with Mishra as its first director.⁶⁶ The Smithsonian administration had long seen Mishra's training as a conservation

63 Chris Wemmer, Ross Simons, and Hemanta Mishra, “Case History of a Cooperative International Conservation Program: The Smithsonian Nepal Tiger Ecology Project” (Draft, 1984), Box 59, Smithsonian Institution, Assistant Secretary for Science Records, Circa 1963-1986, Record Unit 329, Smithsonian Institution Archives, Washington, DC; David Challinor to Russell E. Train, 25 July 1979, Box 55, Smithsonian Institution, Assistant Secretary for Science Records, Circa 1963-1986, Record Unit 329, Smithsonian Institution Archives, Washington, DC

64 Peter A. Jordan, Report of Visit to Smithsonian Tiger Project, Royal Chitwan National Park, November 1977, Box 54, Smithsonian Institution, Assistant Secretary for Science Records, Circa 1963-1986, Record Unit 329, Smithsonian Institution Archives, Washington, DC

65 Ross Simons to S. Dillon Ripley through David Challinor, 13 January 1978; Theodore H. Reed to J.L. David Smith, 14 February 1978, Samuel R. Peale to the Ambassador, 16 March 1978; David Challinor to Douglas Heck, 30 March 1978, Box 54, Smithsonian Institution, Assistant Secretary for Science Records, Circa 1963-1986, Record Unit 329, Smithsonian Institution Archives, Washington, DC

66 Russell W. Peterson to Emerald J.P. Rana, 4 February 1983, Box 6, Russell W. Peterson Papers, Library of Congress, Washington, DC. On WWF's continued involvement in Nepal after the end of the tiger project from the perspective of one of its chief scientists, see Eric Dinerstein, *Tigerland and Other Unintended Destinations*

professional as one of the significant side benefits of the tiger project. In 1978, for example, the director of the National Zoological Park had emphasized to Mishra's advisor at the University of Edinburgh that his degree was important not only for his own career but also “for the benefit of the Nation,” which desperately lacked trained biologists.⁶⁷ As head of the trust, Mishra's role as a liaison between the Smithsonian and the Nepalese government would be institutionalized.

In addition to its scientific and institutional legacies, the tiger project also created opportunities to recreate the kind of elite hunting experience that colonial officials and Nepalese royalty had once enjoyed in the park, albeit transformed for a postcolonial, environmental age. In February 1981, as the tiger project was winding down, the president of the WWF-US, Russell Train, and his wife Aileen visited Chitwan, where they accompanied Hemanta Mishra and the project staff as they attempted to attach a radio-collar to a leopard cub recently sighted in the area. The demonstration was being conducted at the request of Smithsonian administrators, who hoped to show Train that WWF's money had been well spent. For Mishra, the demonstration was an opportunity to demonstrate the project's hybridization of “old Nepalese hunting techniques with modern scientific tools to catch large mammals.”⁶⁸ After the head shikari had located the cub's already-collared mother using radio-tracking gear from atop one of the elephants, the Trains accompanied beaters on elephants drove the animal toward Mishra, who was waiting in a

(Washington: Island Press, 2005).

67 Theodore H. Reed to Iain Taylor, 14 February 1978, Box 54, Smithsonian Institution, Assistant Secretary for Science Records, Circa 1963-1986, Record Unit 329, Smithsonian Institution Archives, Washington, DC

68 Hemanta R. Mishra to Kathryn S. Fuller, 18 July 1994, and Hemanta R. Mishra to Russell E. Train, 1 August 1994, Box 13, Russell E. Train Papers, Library of Congress, Washington, DC. Hemanta Mishra wrote that Thomas Lovejoy had asked him to demonstrate to Train “the field techniques developed in Nepal, particularly how we use old Nepalese hunting techniques with modern scientific tools to catch large mammals.” The morning of the tagging attempt, the crew, “a superstitious lot,” in Mishra's words, had sought the blessing of “Ban Devi, the Goddess of the Forests,” in a ritual led by Badai, the “elephant chief” and “camp witch doctor,” which involved sacrificing a goat, a chicken, and one or two pigeons at the base of a “Semal (silk cotton) tree,” sprinkling blood, chanting “holy mantras,” and then placing “Tikka (red powder mixed with rice)” on foreheads. (Mishra's account does not clearly distinguish between the Trains' two visits to Chitwan.)

tree with a dart gun. When it became evident that the cub was not accompanying its radio-tagged mother, the drive was called off. Early the next morning the Trains participated in another unsuccessful attempt to collar a leopard. In his journal, Russell Train noted that he and his wife agreed that they were glad the big cat—“a really magnificent animal”—had escaped untouched, though they were carefully not to mention their opinion to Mishra.⁶⁹ Despite the lack of success, the Trains had seen enough to understand how similar the “darting” operations were to the aristocratic hunts they had replaced. After leaving the project's headquarters on the eastern side of the park, they spent several nights at the Tiger Tops lodge. Their first night, sound asleep, they were awoken by the announcement that a tiger was at one of the baits. After a short walk in the dark, they watched from behind the blind as a female tiger fed under the spotlights—“enormous and perfectly beautiful,” and wearing a radio collar.⁷⁰

Through the agency of Tamang, Mishra, and other Nepalese researchers and conservationists, Cold War surveillance technologies, conservation biology, the aristocratic hunt, and ecotourism had been integrated into a single seamless experience. This outcome had not seemed inevitable when the project began in 1973, but by the mid-1980s it was an accomplished fact. In 1986, the Duke of Edinburgh, Prince Philip, who was president of WWF-UK from 1961 to 1982 and of WWF-International from 1981-1996, visited Chitwan, where he had an experience similar to the Trains. As John MacKenzie writes, the prince's previous visit to Chitwan in 1961 had included shooting one of the reserve's tigers. During this visit, however, the

69 Russell E. Train, Journal Entry, 6-9 February 1981, Box 36, Russell E. Train Papers, Library of Congress, Washington, DC.

70 Russell E. Train, Journal Entry, 6-9 February 1981, Box 36, Russell E. Train Papers, Library of Congress, Washington, DC; Hemanta R Mishra to Kathryn S. Fuller, 18 July 1994, and Hemanta R. Mishra to Russell E. Train, 1 August 1994, Box 13, Russell E. Train Papers, Library of Congress, Washington, DC. On the genealogical links between colonial big-game hunting and postcolonial wildlife conservation, see John MacKenzie, *The Empire of Nature: Hunting, Conservation, and British Imperialism* (New York: Manchester University Press, 1988).

nearest he came to “big-game shooting was to witness the tranquillising of a rhino called 'Philip' so that it could be fitted with a radio device. Here was the perfect symbol for the replacement of the hunting by the conservation ethos, imperial power by post-colonial environmental concerns. A rhino called 'Philip' had become a new form of trophy.”⁷¹ Again, although MacKenzie does not note the fact, Mishra led the tagging team, and it was he or one of his Nepalese staff members who did the darting, the collaring, and the naming. No longer tied to masculinity, courage, or empire, the rhino was less a trophy for Prince Philip than a gift or an honorary award from a Nepalese researcher whose work was funded by American donations.

American wildlife biologists who carried radio-tracking abroad in the 1970s believed that they would acquire unique insights into animal behavior and ecology, and they were often right. In Chitwan, for example, researchers gathered the first conclusive evidence of territoriality in tigers, information that helped convince the Nepalese government to significantly extend the park's borders. But to attribute these findings to the use of radio-tracking alone would be to seriously misconstrue the nature of the work that took place. New insights emerged not simply from the deployment of American technology, but from the hybrid practices that emerged when a modernized wildlife biology encountered other means of managing the relationship between humans and animals, such as hunting, ecotourism, or agriculture. In the Smithsonian-Nepal Tiger Ecology Project, the affinities between radio-tracking and the aristocratic hunting tradition created the logistical conditions for success, but they also produced political challenges in Kathmandu and Washington, where the differences between the Nepalese royalty's interest in tigers as a source of national prestige and the Smithsonian's interest in science, conservation, and

71 John M. MacKenzie, *The Empire of Nature: Hunting, Conservation, and British Imperialism* (New York: Manchester University Press, 1988), 310.

professional training emerged more clearly. Out of these complex mixtures of cooperation and conflict emerged the ideas of “sustainable development” and “biodiversity” that would guide the international conservation community in the 1980s, as well as a new cadre of international conservation leaders from the developing world. As Haraway writes, even though “Western forms of love and knowledge of nature have been profoundly colonial nature is no longer simply a western epistemological and social imposition. Like other languages of the colonizer that have been reinvented for other conversations, the languages of nature have become polyglot and international.”⁷² One of the ways in which such polyglot languages of conservation were developed was through the kinds of hybrid field practices exemplified by the Smithsonian-Nepal Tiger Ecology Project.

Research practice was one of the focal points for American scientists' grappling with international issues in the 1970s. The difficulty faced by the Smithsonian on following through with its offer for help in tiger conservation shows how, although scientists played a leading role in advocating for international conservation, the place and nature of science in international conservation was highly contested and uncertain. Development experts, conservationists, the tourism industry all disagreed. Even among the scientists there was conflict. What IUCN considered the necessary contribution of scientists to tiger conservation struck Hornocker as exactly the kind of “survey” work that American wildlife biologists, as opposed to “wildlife technicians,” had been struggling to move beyond for decades. Ultimately, the project succeeded in Nepal not because it was modern or scientific or backed by American money and power, although all of those things were important, but because of the “middle ground” built between

72 Donna J. Haraway, *Primate Visions: Gender, Race, and Nature in the World of Modern Science* (New York: Routledge, 1989), 274.

radio-tagging and the aristocratic hunt. American scientists abroad grappled with the changed position of the United States in the aftermath of the Vietnam War, when American prestige and economic power seemed to be at a low ebb, and when the United States' pretensions to being a non-imperial power came under heavy attack. For wildlife biologists, radio-tracking served as a symbol of America's awkward international position in the 1970s, as well as an occasion for the construction of hybrid practices between a modernizing American wildlife biology and indigenous traditions, which themselves were often hybrid products of colonialism.

5

A DIFFICULT TIME WITH THE PERMIT PROCESS **TRACKING BOWHEAD WHALES IN THE ARCTIC, 1977-1981**

Even in June, the wind in Barrow was biting cold when I stepped out of the tiny airport terminal to wait for the shuttle bus that would take me and a half-dozen other tourists on a day-long tour of the small town on Alaska's Arctic coast. The shore-fast ice that extended northward across the Chukchi Sea was beginning to break up, but it was still strong enough to carry the snowmobiles of researchers associated with the Barrow Arctic Science Consortium, a nonprofit collaboration between several North Slope native governmental organizations and the National Science Foundation's Office of Polar Programs. Barrow has been a center for Arctic science since the late 1940s and 1950s, when the U.S. Navy founded its Arctic Research Laboratory at the town's northeastern border in the context of early North Slope oil exploration and the establishment of the Distant Early Warning Line. Supported by the Office of Naval Research and host to a rotating cast of visiting scientists, NARL flourished from the late 1940s to the early 1970s. By the mid-1970s, however, shrinking federal research budgets and *détente* with the Soviet Union had led Navy leadership to decide to decommission the laboratory. Faced with the prospect of imminent closure, NARL's leadership decided to refocus its research efforts on a subject that had become increasingly controversial over the course of the decade and that, they hoped, would allow the laboratory to survive the Navy's withdrawal of support: the status of the bowhead whale. In the 1970s, as environmentalists began a campaign to "Save the Whales," Iñupiat whalers had begun to kill an increasing number of bowheads as they migrated to and from feeding grounds in the Beaufort Sea, in part because the development of the nearby

Prudhoe Bay oil fields had provided new sources of income that could be used to support expensive whaling crews. In 1977, these two developments collided when the International Whaling Commission, pressured by anti-whaling activists and concerned by scientists' low population estimates, declared a moratorium on further killing of bowheads, which the United States government declined to contest. Furious at what they saw as an abrogation of their traditional land rights and betrayal by the National Marine Fisheries Service, the whalers threatened to continue whaling despite the moratorium. At the same time, the federal Minerals Management Service, a division of the Bureau of Land Management, had begun a massive environmental impact study of oil and gas leasing on Alaska's outer continental shelf, including its potential impact on whales. In the midst of this tense stand-off, NARL applied to BLM to fund a study called "Project Whales," one of the key components of which was a proposal to radio-tag bowheads so that their migration paths could be accurately mapped.

For wildlife biologist Erich Follman and other NARL scientists, the proposed radio-tagging study seemed perfectly timed in both technical and political terms. After nearly a decade of failed experiments with the radio-tagging of marine mammals, scientists working with the Navy's marine mammal program and at Navy-affiliated research institutes such as the Woods Hole Institution of Oceanography had finally developed techniques of attaching radio-tags to cetaceans and tracking them under the harsh environmental conditions of the open ocean that seemed to hold promise. Politically, the conflict between the IWC and Iñupiat whalers over bowheads provided a pressing motive for better understanding whale movements, while the BLM's environmental impact study provided a means of funding. However, almost as soon as BLM had awarded the grant, the radio-tagging component was subjected to intense scrutiny from

two groups whose reasons for skepticism were very different but whose ultimate conclusions about the undesirability of the project were the same. One of the groups was, surprisingly, the tight-knit community of marine mammalogists who had recently made great strides in cetacean radio-tracking. Unlike the Craigheads, who had been blindsided by opposition to their radio-tracking studies of grizzlies and elk, marine mammalogists at the end of the 1970s were well aware of the potential for controversy in the use of hands-on field techniques. Fearful that scandals arising from careless research would harm the standing of the community as a whole, they began to police themselves. As Bruce Mate, a whale researcher at Oregon State University, told Follmann in the fall of 1978, “Most of the people I know who have heard about your BLM monies to continue radio tagging of whales (bowhead) feel that you are likely to have a difficult time with the permit process and some of the political constraints in working with the bowhead as an endangered species, especially if it involves research and development of untested techniques.”¹ Even the Office of Naval Research, the most enthusiastic federal sponsor of cetacean radio-tagging, urged NARL's director to shelve the tagging proposal. Despite this opposition, the project might still have gone forward if it had not been for the adamant opposition of leaders of the Iñupiat whaling community, particularly Eben Hopson, mayor of the North Slope Borough and one of the founders of the Inuit Circumpolar Conference. Hopson and the whalers were happy to collaborate with the other components of Project Whales, including high-tech studies of underwater bioacoustics and invasive tissue samples, but the radio-tagging project seemed to pose excessive risks to the whales, to the already difficult and risky practice of whaling, and to the whalers' own epistemological authority. NARL leaders were particularly

¹ Bruce R. Mate to Erich H. Follman, 24 October 1978, Box 63, Records of the Naval Arctic Research Laboratory, Ca. 1940's to 1980's, Accession 89-188, Elmer E. Rasmuson Library, University of Alaska, Fairbanks.

sensitive to Hopson's opinion because support from the local community was crucial to any hopes of continuing NARL's existence past the end of the decade, when the Navy was expected to end its support. Pressured by a scientific community newly fearful of scandal from one side and from a native community newly confident about defending its authority over natural resources from the other, the NARL bowhead tagging project collapsed before it began.

Follmann and Mate conducted a small pilot study of gray whales in Baja California, but it would be several decades before bowheads were tagged at Barrow.

In the late 1950s, radio-tagging had seemed like the royal road to professional authority; by the late 1970s, after the rise of environmental, animal rights, and indigenous rights movements that challenged wildlife biologists' exclusive right to speak for wild animals and the creation of a new set of oversight mechanisms by the environmental legislation of the early 1970s, it had become clear to at least some biologists that *not* tagging was sometimes the better option. Tagging continue to produce powerful, intimate relationships between biologists and wildlife animals, but it also created dangerous new vulnerabilities to outside critique—vulnerabilities that encouraged biologists to seek out non-invasive techniques, especially for animals as culturally significant and controversial as the bowhead. Radio-tagging might well have produced important insights into bowhead behavior, but because of these concerns it was abandoned in favor of bioacoustic surveys, which had proved by the mid-1980s that the population of bowheads was nearly four times as large as scientists had estimated in 1977. Over the course of the 1980s, the Iñupiat community progressively appropriated much of the facilities, ideas, and technologies that NARL scientists had developed in the course of Project Whales and earlier studies. Several former NARL scientists were hired directly by the North Slope Borough's

Department of Wildlife Management, while many others served as consultants to the department and to the Alaska Eskimo Whaling Commission. Erich Follmann, for example, moved to the University of Alaska-Fairbanks, where he led an AEW- funded study to adapt radio-tracking for use on harpoons so as to improve whalers' ability to locate animals that they had struck but not killed. Toward the end of our tour of Barrow we stopped by Ilisagvik College, a two-year community college “dedicated to perpetuating and strengthening Iñupiat culture, language, values, and traditions” that was established in 1995 on part of the former grounds of the Naval Arctic Research Laboratory.² Straying from the rest of the tour group to another wing of the same building that housed the college, I found a hallway lined with posters describing radio-tracking research by members of the North Slope Borough's Department of Wildlife Management. The department and the AEW- had recently agreed provisionally to the use of satellite tags to study bowhead whales, and the deputy director of the department, who I had spoken to by phone a few hours earlier, had assured me that the department fully supported the use of radio-tagging and other advanced research techniques. Where twenty-five years earlier it had been opposed as an unnecessary intrusion, radio-tagging was now integrated into the native corporation's natural resource management practices.

Field Research Under Scrutiny

In the early 1970s marine mammal conservation became one of the focal points for a renegotiation of the relationship between wildlife biologists and the public. In the postwar decades wildlife biologists had partly succeeded, with the help of techniques such as wildlife

² Ilisagvik College, “History,” http://webspaces.ilisagvik.cc/index.php?option=com_content&task=view&id=66&Itemid=63, retrieved on 20 April 2008.

telemetry, in achieving the professional status of which the founders of their field had dreamed in the 1930s. But as the popular environmental movement gained strength over the course of the 1960s, wildlife biologists' technocratic pretensions began to come under attack, as did their high-tech field practices. Indeed, hands-on research and management practices provided inspiration and opportunities for activist groups to become involved in wildlife conservation. By making wildlife manageable, they made wildlife managers responsible. Wildlife biologists differed in their responses to these challenges to their professional authority. Some argued that wildlife managers should stand up for their professional prerogatives, focus on their central constituency of sportsmen and conservationists (rather than the “new environmentalists”), and wait for the protectionist fad to pass.³ But most leaders of the field recognized that fundamental changes were taking place and that wildlife managers had to adapt. A. Starker Leopold, an aggressive professionalizer of wildlife management but also someone sensitive to constituencies other than hunters, had been struggling to reconcile the dependence of wildlife biology on funding from the sport-hunting industry with the increasing public interest in endangered species and non-consumptive uses since at least the early 1960s, when he had fought the against the Craigheads and others to keep public hunting out of the national parks. By the early 1970s, the situation had become critical. Writing to California Department of Fish and Game director G. Ray Arnett in May 1971, he warned that “the emerging wave of protective wildlife legislation” was “usurping the decision-making process in the field of wildlife conservation.” Wildlife managers could not afford to ignore this movement, Leopold argued; instead, they needed to take a proactive role in

3 C.R. Gutermuth to All WWF Directors, 10 April 1973, Box 1, Harold Jefferson Coolidge Papers, 1904-1985, Administrative Papers of International Conservation Organizations, 1946-1977, HUGFP 78.19, Harvard University Archives, Cambridge, MA. Gutermuth was forced to resign from WWF in 1973 after he was elected to the presidency of the National Rifle Association. Gutermuth had campaigned aggressively against gun control legislation after John F. Kennedy's assassination; C.R. Gutermuth to Warren Magnuson, 23 January 1964, Carton 92, Sierra Club Records, 1891-, MSS 71/103c, Bancroft Library, University of California, Berkeley.

endangered species protection. Otherwise they would simply be sidelined by other agencies and professions more willing to address the public's priorities.⁴ Wildlife biologists also began to realize that the desire to escape or transcend politics that had motivated much of their scientific work in the postwar years—including the development of wildlife telemetry—was no longer tenable. In 1972, Maurice Hornocker, who had helped the Craigheads launch their radio-tracking study of grizzlies in Yellowstone in the early 1960s and who had encouraged the Smithsonian to incorporate radio-tracking into its South Asian tiger project, explained to Idaho's fish and game managers that “politics” were not only necessary but good: “The argument that ... any plan will not work because of politics is wrong. Because of politics it *will* work. 'Politics' is used in a derogatory manner by most of us. This is wrong. We must become involved in the political process. This does not mean becoming a politician. It means interacting with people, pointing out options and working to convince [them] that this is the best way to go.”⁵

These political struggles over who could speak authoritatively about wildlife played out on a national scale in debates over marine mammal legislation that took place between 1970 and 1972. For wildlife managers the so-called Harris-Pryor Bill, which would have prohibited the taking of marine mammals without exception, seemed to represent everything that was naïve and misguided about the protectionist movement. The tight-knit marine mammal community quickly mobilized to oppose the bill. In 1971, for example, Carl L. Hubbs, an ichthyologist and marine

4 A. Starker Leopold to G. Ray Arnett, 7 May 1971-05-07, Carton 3, Margaret Wentworth Owings Papers, MSS 86/71c, Bancroft Library, University of California, Berkeley. In July 1971, Arnett told Ira N. Gabrielson that the marine mammal legislation being pushed by preservationist groups would “seriously jeopardize” California's management programs; G. Ray Arnett to Ira N. Gabrielson, 11 July 1971, Box 1, Ira N. Gabrielson Papers, CONS 37, Denver Public Library, Denver, CO.

5 Hornocker quoted his own 1972 comments in a speech given in 1985; see Maurice G. Hornocker, “Changing Attitudes and Wildlife Management Philosophy,” Paper Presented at the American Fisheries Society, International Association of Fish and Wildlife Agencies, Sun Valley, ID, 11 September 1985, Carton 3, Margaret Wentworth Owings Papers, MSS 86/71c, Bancroft Library, University of California, Berkeley.

mammalogist at the Scripps Institution of Oceanography in La Jolla, CA, told one of the California senators that the bill, if passed, “would put all oceanariums out of service, cripple all major zoos, force fur sealing from sane management to destructive pelagic operations, render modern tuna fishing illegal and make a farce of sound conservation.”⁶ Information and activism regarding the bill spread through informal networks of colleagues, teachers, and students. Hubbs, for instance, had heard about the bill from William Perrin of the National Marine Fisheries Service research center that was located immediately adjacent to Scripps in La Jolla. He immediately alerted his former student Kenneth S. Norris that, although the dangers of the bill were obvious to experts, “we see so much exuberance in conservation now days, much of it gone way out of control, that I'm sure we need to take action”⁷ A few months later, Norris joined G. Carleton Ray and William Schevill to testify against Harris-Pryor before a Congressional committee. “[N]o ecosystem on earth has escaped the hand of man,” they explained. “We have already intervened, and often deeply, in system involving marine mammals. Therefore, we must manage, if only to assure that this intervention is kept in control.”⁸ The bill also deeply concerned the Smithsonian Institution, where Sidney R. Galler warned S. Dillon Ripley that a “full scale political battle” was brewing in 1970.⁹ A prohibition on marine mammal taking would

6 Carl L. Hubbs to Alan Cranston, 12 May 1971, Box 17, Carl Leavitt Hubbs Papers, MC 5, Scripps Institution of Oceanography, La Jolla, CA. Scientists were also concerned about the Harris-Pryor bill for reasons of conservation; Karl Kenyon, for example, worried that prohibitions on killing fur seals on the Pribilof Islands would drive Japan and Russia into pelagic sealing, which would be far more dangerous to the long-term survival of the population; Karl W. Kenyon to Margaret W. Owings, 13 July 1971, Carton 1, Margaret Wentworth Owings Papers, MSS 86/71c, Bancroft Library, University of California, Berkeley.

7 Carl L. Hubbs to Kenneth S. Norris, 13 May 1971, Box 17, Carl Leavitt Hubbs Papers, MC 5, Scripps Institution of Oceanography, La Jolla, CA.

8 Statement of G. Carleton Ray, Program Director, William E. Schevill, and Dr. Kenneth S. Norris, Marine Mammal Council, before the House Committee of Merchant Marine and Fisheries, Subcommittee on Fisheries and Wildlife Conservation, Hearings on Ocean Mammal Protection, 23 September 1971, 92nd Congress, 2nd Session (Washington, DC: U.S. Government Printing Office): 399-406.

9 Assistant Secretary (Science) [Sidney R. Galler], Memo to the Files, 1 December 1970, Box 118, Smithsonian Institution, Assistant Secretary for Science Records, 1963-1978, Record Unit 254, Smithsonian Institution Archives, Washington, DC.

potentially hamper the Smithsonian's natural history collecting, its exhibits at the National Zoological Park, and its growing field research and conservation program. Opponents of the bill also pointed to its potentially devastating effects on native cultures. In a letter to several of the major American environmental groups, one Alaskan wildlife biologist pointed out that prohibiting hunting of polar bears, seals, bowhead whales, or other marine mammals would not necessarily lead to larger populations, despite what the public might think, and would irreparably harm native cultures that depended on hunting them. "It would be criminal," the biologist wrote, "to legislate an ancient and ecologically balanced native culture out of existence, forcing these people to leave their traditional homes or go on welfare, or both."¹⁰

The mobilization of scientists, natural resource managers, natural history museums, zoological parks and aquariums, and economic interests against the Harris-Pryor Bill helped bring about the passage of an alternative bill that was, in the eyes of the conservation community, "a scientific management authority rather than an emotional reaction piece."¹¹ Rather than prohibiting the taking and harassment of marine mammals entirely, the Marine Mammal Protection Act transferred authority over them from the states to the federal government and established a permitting authority called the Marine Mammal Commission. Despite this victory and the fact that the MMPA in some ways simplified the patchwork of state and federal regulations that had existed previously, many scientists still found its provisions burdensome. The act granted the three-member Marine Mammal Commission a great deal of discretion in the exercise of its permitting powers, which meant that its passage was only the beginning of a long

10 Robert B. Weeden to the Sierra Club, National Audubon Society, National Wildlife Federation, and Wilderness Society, 27 March 1972, Box 23, Alaska Conservation Society Papers, 1960-1993, USUAFV5-215, Elmer E. Rasmuson Library, University of Alaska, Fairbanks.

11 Daniel Poole to Ira N. Gabrielson, 21 August 1972, Box 1, Ira N. Gabrielson Papers, CONS 37, Denver Public Library, Denver, CO.

battle over implementation. In 1973, as Marine Mammal Commissioner Victor B. Scheffer was still setting up his offices in Washington, Hubbs wrote to whale activist Scott McVay, who had begun to campaign against the indigenous hunt of bowheads in Alaska and Canada, to suggest that marine mammals had an “almost fantastic ability to recover populations from the very brink of extinction.” Although the new legal protections for marine mammals were welcome, Hubbs added, he hoped that the new MMC would establish “reasonable regulations so that the occasional taking of moderate numbers for research, education, and exhibit can continue.”¹²

In practice, while the Marine Mammal Commission did sometimes reject applications for permits and often suggested alterations in research protocols, it mainly constrained fieldwork indirectly by requiring scientists to commit to a fixed research plan before entering the field and by requiring those plans to be published in the Federal Register, where they could be tracked by the press and by environmental and animal-rights activists. These restrictions were enough to inspire vigorous resistance by some marine mammalogists. In 1979, for example, Burney J. Le Boeuf, a marine mammalogist at the University of California at Santa Cruz, wrote to his colleagues to solicit what he called “horror stories, accounts of marine mammal researchers frustrated in attempting to do their work by the rigid, sometimes absurd, and often, abiological manners in which the Marine Mammal Protection Act of 1972 is regulated and enforced.” Despite its good intentions, he explained, the MMPA had stifled the advance of science. “We cannot exceed, deviate, modify, follow a lead, capitalize on serendipity, show any spark of imagination. We have been stripped of our most essential tool as scientists—our judgment.”¹³

Hubbs wrote to Le Boeuf to express his sympathy, although he noted that he himself had never

12 Carl L. Hubbs to Scott McVay, 2 February 1973, Box 54, Carl Leavitt Hubbs Papers, MC 5, Scripps Institution of Oceanography, La Jolla, CA

13 Burney J. Le Boeuf to Dear Colleague, 21 February 1979, Box 17, Carl Leavitt Hubbs Papers, MC 5, Scripps Institution of Oceanography, La Jolla, CA

experienced much in the way of constraints, perhaps because he had rarely depended on outside grants. “[B]eing a fellow rebel,” he replied, “I sympathize deeply with you over governmental (or other) restraints. ... There is too much 'Have Power, Use Power'.”¹⁴ Le Boeuf was an extremist, but even Roger S. Payne, a marine mammalogist with strong protectionist sympathies whom Hubbs and others credited with inspiring much of the “Save the Whales” movement, had started to publicly criticize the rigidity of the MMPA by the early 1980s.¹⁵

Happy to accept research funding but resentful of the oversight that came with it, marine mammalogists and other wildlife biologists in the 1970s shared with many other Americans an ambivalent relationship to the federal government and its growing regulatory authority. Even though the MMPA was significantly more responsive to scientists' demands than the Harris-Pryor Bill, it still seemed to place burdensome constraints on fieldwork. In his Christmas card of 1974, Karl Kenyon, an eminent specialist on sea otters and fur seals, told his friends that it was a good thing he had retired when he did, because the MMPA made it “difficult or virtually impossible to conduct much research. It is no easy matter for an FWS biologist to get a permit even to pick up dead animals on the beach, let alone kill needed specimens.”¹⁶ Like Kenyon and Le Boeuf, some scientists found themselves unable to use what they believed were the best available techniques because of the opposition of non-scientists and the constraints of new environmental laws. As Kenyon complained to Margaret Owings, a prominent California conservationist and founder of the Friends of the Sea Otter, the politics of conservation had changed in the 1970s. Now, he

14 Carl L. Hubbs to Burney J. Le Boeuf, 6 March 1979, Box 53, Carl Leavitt Hubbs Papers, MC 5, Scripps Institution of Oceanography, La Jolla, CA

15 Roger Payne, Oliver Brazier, Eleanor Dorsey, Judith Perkins, Victoria Rowntree, and Alan Titus, *External Features in Southern Right Whales (*Eubalaena australis*) and Their Use in Identifying Individuals* (Washington, DC: Marine Mammal Commission, 1981).

16 Karl W. Kenyon to Nathaniel and Margaret W. Owings, December 1974, Carton 1, Margaret Wentworth Owings Papers, MSS 86/71c, Bancroft Library, University of California, Berkeley.

wrote, “everyone from poets and philosophers to lawyers, preservationists and fishermen have [sic] a generous 'input'. Possible results are still obscure.”¹⁷

The Bowhead Controversy

For many centuries, the Iñupiat and Yup'ik Eskimos who live along the coasts of the Bering Sea, the Chukchi Sea, and the Beaufort Sea on Alaska's northwestern and northern borders (and into the Canadian Yukon) have depended on the bowhead whale for a major part of their subsistence needs. Pushing off from the ice in seal-skin boats called *umiaks* in the spring, when the pack ice has begun to break up and bowheads migrate toward the eastern portion of the Beaufort Sea, whaling crews look for tell-tale spouts indicating the presence of the animals, which can grow longer than sixty feet and weigh more than sixty tons. Harpooning a whale is only the beginning of the work. Floats attached to the harpoons help tire the whale, which typically dives after being struck, and make it possible for whalers to relocate it. Once the whale is dead, it must still be towed to the ice for butchering, a major community activity in which other whaling crews also participate.¹⁸

In the 1970s, this long-standing tradition suddenly became one of the world's most controversial conservation issues, attracting attention from whalers, environmentalists, indigenous rights activists, animal rights activists, diplomats, journalists, and scholars from around the globe. No single reason is adequate to explain why this happened; rather, the

17 Karl W. Kenyon to Margaret W. Owings, December 1981, Carton 1, Margaret Wentworth Owings Papers, MSS 86/71c, Bancroft Library, University of California, Berkeley.

18 A description of contemporary whaling practices produced by the Alaska Eskimo Whaling Commission with funding from the National Science Foundation can be found at the following site: “Tradition Whaling in the Western Arctic,” http://www.uark.edu/misc/jcdixon/Historic_Whaling/, retrieved on 3 December 2007; on the biology of the bowhead, see American Cetacean Society, “Fact Sheet: Bowhead Whale: *Balaena Mysticetus*,” <http://www.acsonline.org/factpack/bowhead.htm>, retrieved on 3 December 2007.

controversy was born from the confluence of several distinct but related developments on the North Slope, in Alaska, in the United States, and in the international community.

Iñupiat whaling traditions are ancient, but they have never been static. As environmental conditions and the social organization of Iñupiat communities has changed, so have the techniques used to hunt bowheads. Over the centuries before the arrival of Yankee whalers, whalers shifted the emphasis of their efforts between ice-based and shore-based whaling and spring and fall whaling, and made many other smaller adjustments in material culture and work practices. The arrival of Yankee whalers in the second half of the nineteenth century marked an important shift in this history in two ways. First, it introduced an array of new technologies, ranging from explosive-tipped harpoons to (by the second half of the twentieth century) two-way radios, outboard motors, and aluminum boats. Collectively, these new technologies made Iñupiat whaling safer for humans and more deadly for whales, though it remained far less efficient than pelagic Yankee whaling, let alone the industrial whaling pioneered by Norwegians and largely responsible for the decimation of whale stocks in the twentieth century. Second, it signaled the beginning of a century of scarcity of bowhead whales and scarcity-consciousness among those concerned with them and with whales in general. This cause of this scarcity had nothing to do with the indigenous subsistence hunt, and everything to do with industrial whaling linked to global markets for whalebone and oil.¹⁹

Scarcity-consciousness and a desire to regulate the hyper-competitive whaling industry in order to reduce waste and increase profits by dominant whaling nations led to the establishment of the International Whaling Commission in 1946, as one of many international natural resource

¹⁹ For a historical account the impact of Yankee whalers on Iñupiat whaling practices, see John R. Bockstoce, *Whales, Ice, and Men: The History of Whaling in the Western Arctic* (Seattle: University of Wisconsin Press, 1986).

organizations founded in the postwar years. Although initially closer to a trade association than a conservation organization, the IWC gradually shifted towards conservation under the influence of the United States, whose whaling industry had largely collapsed in the late nineteenth century. For its first few decades, the IWC failed either to conserve whale stocks or to effectively regulate competition, instead setting quotas that were so high that the whaling industry was unable to meet them. But by the 1970s, as the scientific community became increasingly vocal in its criticism of IWC policy, as the United States shifted the balance of power in the IWC by recruiting non-whaling nations, and as the popular environmental movement grew and appropriated the whale as one of its most powerful icons of an endangered natural world, the IWC began seriously to consider a complete (if temporary) moratorium on commercial whaling. Since its founding the IWC had included exemptions for what it identified as “aboriginal whaling,” which included the Iñupiat bowhead hunt. In the context of the United States' efforts to secure an international moratorium, however, the continuation of whaling in American waters—no matter how small-scale—became a serious political liability. If the United States believed that whaling was necessary to the preservation of Iñupiat culture, then, Japanese and Norwegian negotiators argued, it could hardly argue for the destruction of other nation's commercial whaling industries, upon which so many livelihoods depended.²⁰

20 On the history of the “Save the Whales” movement and the International Whaling Commission's turn to conservation, see Michael Heazle, *Scientific Uncertainty and the Politics of Whaling* (Seattle: University of Washington Press, 2006); David Day, *The Whale War* (San Francisco: Sierra Club Books, 1987); David D. Caron, “The International Whaling Commission and the North Atlantic Marine Mammal Commission: The Institutional Risks of Coercion in Consensual Structures,” *American Journal of International Law* 89 (1995): 154-174; Steinar Andresen and Tora Skodvin. “Non-State Influence in the International Whaling Commission, 1970 to 2006,” in *NGO Diplomacy: The Influence of Nongovernmental Organizations in International Environmental Negotiations*, edited by Michele Merrill Betsill and Elisabeth Corell (Cambridge, MA: The MIT Press, 2007): 119-148.

The U.S. defense of Iñupiat whaling became even more difficult to sustain when marine mammalogists estimated that perhaps fewer than two thousand bowheads remained, a number far too small to sustain the increases in Iñupiat whaling that took place in the 1970s. Iñupiat whalers operating out of villages on the Bering, Chukchi, and Beaufort Seas had begun to take an increasing number of whales and to aggressively defend their right to do so in local, national, and international forums. This was due in part to a cultural and political revitalization, common to many indigenous groups at the time, but it was also linked to the specific economic and political conditions of Alaska's North Slope. The discovery of oil in Prudhoe Bay, to the east of Point Barrow, in the late 1960s had ignited a major reorganization of land rights in Alaska, which started with the Alaska Native Claims Settlement Act of 1971 and culminated in the Alaska National Interest Lands Conservation Act of 1980.²¹

The discovery of oil on the North Slope had also led to a strengthening of native voices, first through the political process of allocating lands and establishing rights to taxation on oil profits, and then through the tax revenues themselves, which enabled a massive increase in the capacity of the native corporations that were established to govern towns such as Barrow and regions such as the North Slope Borough. As increasing amounts of cash flowed to native governments as well as to individual workers employed in the oil industry and its supporting industries, the number of capital-intensive whaling crews expanded. The number of whales landed grew, as did the number of whales “struck but lost”—that is, injured or killed but not brought to ice for butchering. Anti-whaling activists began to criticize Iñupiat whaling for being both “modern” (and therefore not deserving of the special protections afforded to traditional

21 Theodore Catton, *Inhabited Wilderness: Indians, Eskimos and National Parks in Alaska* (Albuquerque: University of New Mexico Press, 1997).

practices) and “wasteful” (and therefore not subject to the special legal exemptions for subsistence hunting). The author Barry Lopez was a supporter of indigenous hunting rights and thought that efforts to defend Iñupiat whaling were “courageous and humane,” but after a visit to Barrow in the winter of 1977, he was pessimistic about the survival of the bowhead under the intensified onslaught. “As far as I am concerned,” he wrote to an acquaintance at Friends of the Earth, “I saw the handwriting on the wall: wild creatures are not going to fare well at the hands of industrialized Eskimos.”²²

In this highly politicized context, the exact size of the bowhead population became a crucial issue. If the United States could demonstrate to the IWC that the Iñupiat subsistence hunt did not threaten the survival of the bowhead, its case for eliminating commercial whaling—which clearly did threaten the survival of the world's great whales—would be easier to press. On the other hand, if the bowhead whale was threatened with extinction, it would be easier for U.S. government regulators at the National Marine Fisheries Service to justify establishing a quota or even banning Iñupiat whaling altogether, despite provisions in the Marine Mammal Protection Act that exempted “non-wasteful” native subsistence hunting from regulation. In 1977, on the basis of preliminary census estimates, the IWC decided to establish a moratorium on bowhead whaling, a move that infuriated Iñupiat whalers and placed federal regulators in a dilemma. Either they could formally object to the ruling, thereby undermining the United States' long attempts to strengthen the IWC and secure a global moratorium, or they could accept it and thereby renege on promises to defend indigenous rights. They chose to accept it, launching an

²² Barry Lopez to Tom Turner, 29 October 1977, Carton 39, David R. Brower Papers, Series 5, Records of the Friends of the Earth, MSS 79/9c, Bancroft Library, University of California, Berkeley, CA.

intense period of conflict in the courts, among scientists, and in the press that lasted from 1977 to 1981, in which the demographics of the bowhead population was the key scientific issue at stake.

The category of 'traditional ecological knowledge' emerged in the 1970s in the context of native attempts to wrest control over natural resources from non-native governments. Its most obvious function was to legitimate native understanding of the environment vis-à-vis the scientific understandings that dominated policymaking and the courts, but it also distinguished native knowledge from other forms of local knowledge, such as that held by white hunters and trappers—or even by scientists whose long residence in Northern communities gave them some claim to 'locality.' At the beginning, however, wildlife managers saw little difference between such claims and those of other self-interested hunters who had resisted the professionalization of wildlife management since at least the 1930s. Iñupiat whalers' claims that the bowhead population far exceeded the estimates of marine mammalogists, and that, as a result, hunting regulations should be liberalized, echoed the claims of non-native hunters and trappers who had seen their own traditional relationships to wild animals transformed by wildlife managers with the support of federal and state governments.²³

The Promise of Tagging

In the spring of 1971, U.S. Interior Secretary Walter Hickel called for an international moratorium on the hunting of eight endangered species of whales. That June, American whale

23 On the expansion of a “national commons” in wildlife and , see Louis S. Warren, *The Hunter's Game: Poachers and Conservationists in Twentieth-Century America* (New Haven, CT: Yale University Press, 1997); Karl Jacoby, *Crimes Against Nature: Squatters, Poachers, Thieves, and the Hidden History of American Conservation* (Berkeley: University of California Press, 2001). On the parallel increase of the centrality of science to wildlife conservation in the United States over the course of the twentieth century, see Thomas R. Dunlap, *Saving America's Wildlife: Ecology and the American Mind, 1850-1990* (Princeton, NJ: Princeton University Press, 1991).

researchers gathered at Shenandoah National Park to discuss the state of the science. In their conference report, they concluded that radio-tracking was one of the most promising new techniques for understanding the migrations of whales and the relationships between various populations (known as “stocks” in the use-oriented parlance of the IWC).²⁴ From its tentative origins in the late 1950s at the intersection of national security concerns and wildlife management, the technology of radio-tagging had revolutionized the way biologists studied animals in the field. Because of the unique challenges of the marine environment, however, radio-tagging had only just begun to become a feasible technique for studying whales.

Since the 1960s, the Office of Naval Research had served as a steady source of support for a small network of marine mammalogists interested in using electronic surveillance techniques to study animals in the open ocean. ONR justified its marine mammal research to Congress and to the public in a variety of ways, but the Navy's primary aim in funding studies of whale communication was to aid anti-submarine warfare. In order to detect Soviet submarines and to hide its own ships, the Navy needed to map the underwater soundscape, including noises produced by so-called “false targets” such as whales and porpoises.²⁵ Secondly, the Navy was interested in using seals, dolphins, and other marine mammals for underwater operations. Wildlife telemetry thus leveraged the Navy's expertise in at-sea electronics to improve national security by helping scientists decode the noisy world of underwater biological communications. In the absence of a technique for locating individual animals in the open ocean, however, it was

24 Marine Mammal Council, U.S. International Biological Program, “A Digest of the Major Conclusions of the Conference,” 13 June 1971, Box 112, Smithsonian Institution, Assistant Secretary for Science Records, 1963-1978, Record Unit 254, Smithsonian Institution Archives, Washington, DC.

25 On the relevance of underwater cetacean vocalizations to anti-submarine warfare, see Gordon B. Tribble to Carl L. Hubbs, 13 December 1960, Box 54, Carl Leavitt Hubbs Papers, MC 5, Scripps Institution of Oceanography, La Jolla, CA; C.J. Whitbeck to Library of Underwater Sounds of Biological Origins, University of Rhode Island, 1 November 1968, and Howard E. Winn to Dean E. Holt, 28 January 1969, Box 3, Entry 36, Office of Naval Research Records, Record Group 298, National Archives and Records Administration, College Park, MD.

nearly impossible to conduct studies of the vocalization of a single animal over an extended period of time. From 1961 to 1965, with support from ONR, two marine bioacoustics researchers at the Woods Hole Oceanographic Institute, William E. Schevill and William A. Watkins, attempted to develop a radio-tracking system for whales. Woods Hole, like the Scripps Institution of Oceanography in Southern California—and, indeed, like American oceanography as a whole—was closely articulated with the Navy throughout the Cold War.²⁶ Watkins and Schevill were among the first scientists to record and study the underwater vocalizations of whales and porpoises, extending to the seas techniques that had been pioneered by Arthur A. Allen for birds and Clarence Ray Carpenter for primates.²⁷ Radio-tracking, they hoped, would allow them to understand how these vocalizations were related to cetacean behavior and social relations.

In August 1964, at a conference at Woods Hole on the use of high-flying aircraft and artificial satellites for oceanography, Schevill described their still-unsuccessful attempts to attach radio tags to North Atlantic right whales. Because hunting of right whales had been banned since the 1930s, standard techniques of whale identification, which depended on killing the whale to recover markers that had been embedded in its blubber, were useless. But radio-tracking presented formidable challenges of its own. As Schevill noted, “The transmitter has a number of conflicting requirements, such as the compromise between optimum antenna size and hydrodynamic requirements so as to be tolerable by the whale. There is also the sporting uncertainty of the actual tagging off the whale.”²⁸ By 1965, when the project ended, Schevill and

26 Jacob Darwin Hamblin, *Oceanographers and the Cold War: Disciples of Marine Science* (Seattle: University of Washington Press, 2005).

27 On C.R. Carpenter's field practices, see Georgina M. Montgomery, “Place, Practice and Primatology: Clarence Ray Carpenter, Primate Communication and the Development of Field Methodology, 1931–1945,” *Journal of the History of Biology* 38 (2000): 485-533.

28 William E. Schevill, “Application of Satellites or High-Flying Aircraft to Studies of Cetaceans and Other Large Marine Animals,” Paper Presented at the Conference to Study the Feasibility of Conducting Oceanographic Explorations from Aircraft, Manned Orbital and Lunar Laboratories, Woods Hole Oceanographic Institution, 18

Watkins had developed a small, waterproof cylindrical tag—about 1.5 cm wide by 15.5 cm long—containing a battery and transmitter, with an antenna at one end and a barbed metal point at the other. To embed it in the whale, they attached it to the end of a weighted pole and dropped it from a helicopter hovering over the surfacing animal. Although they were able to attach tags to free-swimming whales using this technique, they were unable to track them. Sometimes the tags were damaged in the attachment processes and sometimes the whales moved out of range, but the biggest problem was the brief amount of time the whales spent at the water's surface, exposing the tags' antennas to the air. Even when the researchers were able to detect a signal from a breaching whale, the length of the broadcast was insufficient to determine its exact location.²⁹ Because of these difficulties, Watkins and Schevill temporarily halted their work on radio-tracking after 1965, but they continued to consult with a geographically dispersed but closely linked network of marine mammalogists and engineers that ONR continued to fund to develop the technique.

Despite their best efforts, the development of cetacean tagging had stalled in the late 1960s. Numerous proposals for radio-tracking studies were turned down by review panels because of what appeared to be insurmountable technical challenges. In the mid-1960s, for example, Lowell Adams, an early booster of wildlife telemetry, became interested in radio-

August 1964, and "Recommendations of Panel on Marine Biology," 25 August 1964, Box 24, Carl Leavitt Hubbs Papers, MC 5, Scripps Institution of Oceanography, La Jolla, CA. Explaining the purpose of the conference to Carl Hubbs, Paul Fye of the Woods Hole Oceanographic Institution wrote: "For the first time the whole world ocean could be placed under continuous surveillance for purely scientific purposes. On the other hand, the remoteness of the vehicles from direct contact with the water presents problems that cannot be resolved by conventional thinking or standard oceanographic methods. Only an imaginative and daring new look at oceanography can hope to succeed, and we shall have to call on the full capabilities of scientific technology as it exists now and as it may develop in the future"; Paul M. Fye to Carl L. Hubbs, 1 July 1964, Box 24, Carl Leavitt Hubbs Papers, MC 5, Scripps Institution of Oceanography, La Jolla, CA.

29 William A. Watkins and William E. Schevill, "Development and Testing of a Radio Whale Tag," Woods Hole Oceanographic Institution Technical Report 77-58 (Woods Hole, MA: Woods Hole Oceanographic Institution, 1977).

tagging gray whales but because of his lack of expertise in marine mammalogy was unable to get funding from government sources. Unlike most researchers, however, he was able to bypass the peer-review process by convincing Palmer Beaudette, a wealthy socialite with an amateur interest in biology, to fund an attempt to radio-tag gray whales in Baja California. Soon after their first attempt had ended in failure, however, Beaudette's finances collapsed. The gray whale researchers already working in Baja had disapproved of Adams's study and, through the peer-review process, they continued to deny him government funding, bringing his whale-tagging efforts to a close.³⁰

Even well-respected biologists with field experience faced profound skepticism about radio-tagging. In the late 1960s, Roger Payne—later to become famous for his non-invasive research on cetaceans—repeatedly submitted proposals for tagging humpback whales and blue whales to NSF, but was denied for essentially the same reasons that Adams had been.³¹ Although NSF sponsored some radio-telemetry work on seals in Antarctica during the 1960s and 1970s, it left whale tracking to ONR. In the late 1960s, a nuclear engineer in San Diego named Jack Schultz proposed tagging blue whales to George Llano, the head of NSF's Antarctic program. Llano told Schultz that NSF could not afford to support the long-term development of an experimental technique with which ONR, the Smithsonian, and NASA were still struggling.

Twelve specialists had recently reviewed several radio-tracking proposals for NSF, he explained,

30 On Adams' gray whale radio-tagging plans and Carl Hubbs's skepticism towards them, see Lowell Adams to Director, Fish and Wildlife Service, 25 June 1965; Lowell Adams to Carl L. Hubbs, 27 July 1965; Carl L. Hubbs to Palmer T. Beaudette, 9 September 1965, Box 56, Carl Leavitt Hubbs Papers, MC 5, Scripps Institution of Oceanography, La Jolla, CA.

31 These repeated rejections, which were based primarily on technical rather than humanitarian considerations, may have helped push Payne toward the non-invasive research techniques of which he later became a well known advocate; see Roger S. Payne to George A. Llano, 8 March 1968; George A. Llano to Roger S. Payne, 6 May 1968, Box 7, Office of Antarctic Programs, Records of the Program Director for Biology and Medicine (G. [George] Llano), General Correspondence and Related Records, 1961-69, Entry 33, National Science Foundation, Record Group 370, National Archives and Records Administration, College Park, MD. Payne continued to support the use of radio-tags even after he turned to non-invasive techniques.

and while “they all endorsed the need for developing a system for whale tracking, to a man they stated that current results are based on unconfirmed techniques resulting in questionable data.”³²

The first major breakthrough in whale tagging came not because of the work of marine mammalogists but as a result of unrelated technological developments in the oceanographic instrument industry. Around 1970, Navy marine mammalogist William Evans recognized the potential of an automatic radio direction finder that had been developed by a San Diego company called Ocean Applied Research, which had been designed for locating oceanographic buoys. The device solved the major problem that had foiled Schevill and Watkins: it made it possible to obtain accurate directions of radio signals that were present only for a few seconds, the amount of time it might take for a whale to break the surface, breathe, and submerge again.³³

As in many other areas of Cold War science, the effort to study cetacean communications and behavior in the open ocean for the dual purposes of national security and the advance of knowledge forged new links across institutional and disciplinary boundaries. In San Diego's marine mammalogy community, in particular, the lines between the military, academia, private industry, and government became blurred. The career of William E. Evans is a good example. Evans began his career in the 1950s at Lockheed in San Diego, where he studied underwater acoustics and helped develop anti-submarine weapons systems. From there he moved to the Naval Missile Center at Point Mugu, north of Los Angeles, where he studied porpoises as part of the Navy's marine mammal training program. In 1968, he followed the marine mammal program back to San Diego, where it had been integrated into the Naval Undersea Research and

32 George A. Llano to Jack Schultz, 24 April 1970, Box 1, Office of Polar Programs, Records of the Polar Science Section, Records of the Program Manager for Biology and Medicine, Reading File, Entry 100, National Science Foundation, Record Group 370, National Archives and Records Administration, College Park, MD.

33 W.E. Evans, “Orientation Behavior of Delphinids: Radio Telemetric Studies,” *Annals of the New York Academy of Sciences* 188 (1971): 142-160.

Development Center. In the early 1970s, when bycatch of dolphin in Pacific tuna fisheries was becoming an important conservation issue, he spent several years as a visiting scientist at the National Marine Fisheries Service's research center in La Jolla, next door to the Scripps Institution of Oceanography. In 1977, he became the executive director of the new Hubbs-Sea World Research Institute, a research branch of Sea World that had been created, in part, to help justify keeping cetaceans in captivity in the face of growing public criticism. In the 1980s, Evans served as the leader of the U.S. delegation to the International Whaling Commission, head of the Marine Mammal Commission, Under Secretary of Commerce for Oceans and Atmosphere, and Assistant Administrator for Fisheries of the National Oceanic and Atmospheric Administration. The career trajectories of people like Evans could not force a convergence of the disparate interests of the military, the defense industry, the tourism industry, the academic community, and federal regulatory agencies, but they did help to smooth relations among them.

One of the first applications of Evans's radio-tagging system was in the “Deep Ops” project at the Naval Undersea Center's laboratory in Hawaii in 1969-1971. The purpose of Deep Ops was to determine whether pilot whales and killer whales could be trained to retrieve mines, torpedoes, missiles, and oceanographic devices from the ocean floor. Radio tags would allow the trainers to keep track of the whales even if they attempted to escape during operations in the open ocean. The whales were initially resistant to wearing the radio backpacks, but after extensive training and changes in the backpack design they learned to tolerate them. One of the two killer whales in the project, Ishmael, escaped during an open-ocean training session, in part because his radio tag malfunctioned, while the other, Ahab, was tracked down and recovered during a similar escape attempt with help of his radio signal.³⁴ The obvious usefulness of

³⁴ Clark A. Bowers and R. Scott Henderson, *Project Deep Ops: Deep Object Recovery With Killer and Pilot*

automatic direction finder quickly reinvigorated work on radio-tagging. Within a few years it was being used by Kenneth Norris in Hawaii, Carleton Ray and Doug Wartzok at Johns Hopkins, and Schevill and Watkins at Woods Hole in addition to Evans and other Navy marine mammal researchers in San Diego.³⁵

While the use of the automatic direction finder solved one of the major technical problems of cetacean radio-tracking, the other—a safe, reliable means of attaching tags to large whales that could not be captured or restrained—remained out of reach. Evans's tags worked only because they could be carefully attached to captive animals. In addition, new challenges to hands-on research were emerging that would prove much less amenable to technical solutions. Ironically, the Navy-sponsored research that had demonstrated the complex social relations and communications abilities of cetaceans had helped to convince a vocal segment of the public that further exploitation of the animals was unjustified, whether they were to be used as sources of raw materials, underwater guard-dogs for military operations, performers at amusement parks, or subjects of scientific research. For the Navy, national security justifications that had worked in the *Naval Research Review* and in the popular press in 1962 no longer seemed convincing a decade later. In the fall of 1972, for instance, the chief of naval research received an inquiry from a New Jersey senator on behalf of a constituent—Alice Herrington of the Friends of Animals—who was opposed to a radio-tagging study of walruses in the Bering Sea. He responded that the Navy's interest in marine mammals “should not be construed as an exploitation of these animals” but rather as an effort to better understand ambient ocean noise.³⁶ Such arguments did little to

Whales, (San Diego, CA: Naval Undersea Center, 1972); see also Blair Irvine, “Conditioning Marine Mammals to Work in the Sea,” *Marine Technology Society Journal* 4 (May-June 1970): 47-50.

35 Kenneth S. Norris to Carl L. Hubbs, 27 May 1971, Box 53, Carl Leavitt Hubbs Papers, MC 5, Scripps Institution of Oceanography, La Jolla, CA; G. Carleton Ray to Dean E. Holt, 5 November 1970, Box 2, Entry 36, Office of Naval Research, Record Group 298, National Archives and Records Administration, College Park, MD.

36 C.O. Holmquist to Harrison A. Williams, Jr., 3 November 1972, Box 2, Entry 36, Office of Naval Research,

convince animal rights activists and environmentalists that the Navy's interest were benign, especially since the marine mammal training program, for which the Navy requested a permit to capture three hundred animals in 1974, could not be construed as anything but exploitation.³⁷ Well-respected marine mammalogists such as Victor B. Scheffer, the first head of the Marine Mammal Commission, began to question publicly the Navy's marine mammal work. These issues began to fracture the consensus of marine mammalogists. In November 1973, after Scheffer criticized the Navy's marine mammal work in an article for *Smithsonian Magazine*, one prominent marine mammal researcher wrote him an irate letter defending the Navy, which had funded almost all of his research. "It is a plain fact," the researcher wrote, "that the Navy has done or has supported more basic ecological research on marine mammals and has developed more tools for the study of them than the NMFS and the National Science Foundation combined!!"³⁸

Rising public concern about marine mammals threatened to place new constraints on research, but it also drove a significant increase in funding and widened the range of federal agencies with an obligation to protect marine mammals. These new sources of support helped to open up the field of whale radio-tagging, which had been dominated since the early 1960s by ONR-affiliated researchers. As new people became involved, divisions over the proper use of radio-tagging started to emerge. In 1975, for example, a University of Minnesota marine mammalogist named Albert W. Erickson received a grant from the MMC to radio-tag killer

Record Group 298, National Archives and Records Administration, College Park, MD.

37 In 1974, for example, the *San Diego Union* published a critical article about the Navy's application to the Marine Mammal Commission for a permit to capture up to three hundred marine mammals that year.; "Navy Asks for 300 mammals," *San Diego Union*, 16 December 1973.

38 Victor B. Scheffer to Gordon Gunter, 21 September 1973, Box 17, Carl Leavitt Hubbs Papers, MC 5, Scripps Institution of Oceanography, La Jolla, CA; G. Carleton Ray to Victor B. Scheffer, 1 November 1973; G. Carleton Ray to C.D. Woodhouse, 2 November 1973, Box 2, Entry 36, Office of Naval Research Records, Record Group 298, National Archives and Records Center, College Park, MD.

whales in Puget Sound in collaboration with Sea World, which was planning to capture a pod of orcas for its theme park in San Diego. By the mid-1970s, the live-capture of killer whales for commercial display had become highly controversial, perhaps nowhere more so than in Puget Sound. While Erickson was enthusiastic about the technique's potential, William Evans, who had significantly more experience in tagging cetaceans, was much less so, and he attempted to convince Sea World that an aerial or ship-based census of Puget Sound's killer whales was far more urgent, in scientific terms, than a tagging study.³⁹ Regardless, Sea World went ahead with the capture of the orcas in March 1976, and Erickson managed to radio-tag and track several of those that were released.⁴⁰ Erickson subsequently tried to convince the MMC to fund further tagging studies, arguing that the technique was far more accurate and informative than the only alternative for identifying the animals—the use of photographic databases of natural markings. Public outrage over the Sea World operation had subsided by then, however, and the MMC refused to reignite the controversy by advocating for the use of such a hands-on technique on a population of charismatic mammals that was the subject of nearly constant public attention.⁴¹ Puget Sound's orca population subsequently became one of the great success stories for the technique of photo-identification, a hands-off method of identifying animals using databases of

39 William E. Evans to Frank Powell, Jr., 21 May 1973, Box 34, Carl Leavitt Hubbs Papers, MC 5, Scripps Institution of Oceanography, La Jolla, CA.

40 Albert W. Erickson, *Population Studies of Killer Whales (*Orcinus orca*) in the Pacific Northwest: A Radio-Marking and Tracking Study of Killer Whales* (Washington, DC: Marine Mammal Commission, 1978); see also “Minutes of the Public Portion of the Ninth Meeting of the Marine Mammal Commission and Fifth Meeting of the Committee of Scientific Advisors on Marine Mammals, Santa Barbara, California, 21 February 1975,” Box 10, Richard Allen Cooley Papers, Accession 95-285, Elmer E. Rasmuson Library, University of Alaska, Fairbanks.

41 Donald B. Siniff to Douglas G. Chapman, 12 May 1977, Box 11, Douglas G. Chapman Papers, Accession 4581-001, University of Washington Libraries, Seattle; Donald B. Siniff to Douglas G. Chapman, 9 September 1977; R. Eisenbud to Douglas G. Chapman, 14 August 1978, Box 9, Douglas G. Chapman Papers, Accession 4581-001, University of Washington Libraries, Seattle

distinctive natural markings—as well as unnatural markings such as the scars left by Erickson's radio tags.

In response to such criticisms, institutions that exhibited live marine mammals began to reshape their public images to include science and conservation as well as to entertainment and profit. Sea World was a prime target for criticism because it was a private theme park featuring performances by trained cetaceans that had been caught in the wild. In 1970, Sea World announced that it was planning to capture a gray whale calf for display at its park in San Diego. While many marine mammalogists were enthusiastic about the idea, the public response was highly negative.⁴² Nonetheless, under the supervision of Sea World veterinarian David Kenney, a calf was separated from her mother in Scammons Lagoon on Baja California and transported by air to San Diego in March 1971.⁴³ Sea World immediately began framing the capture as primarily a scientific rather than a commercial endeavor, even though its claim that the research conducted on Gigi would help save the gray whale from extinction elicited skepticism from others in the marine display business, including the director of the Scripps Institution's public aquarium.⁴⁴ Initially, Sea World had no intention of releasing the whale, which had been named Gigi. However, as Gigi's health declined, her size and appetite increased, and public criticism of the capture continued, the Sea World management changed its mind. In the spring of 1972, Gigi was released into the waters off San Diego with one of Evans's radio-backpacks attached.⁴⁵ The radio tag allowed Gigi to be tracked up the California coast to Monterey, where the signal was finally

42 Carl L. Hubbs to David W. Kenney, 7 January 1971, Box 56, Carl Leavitt Hubbs Papers, MC 5, Scripps Institution of Oceanography, La Jolla, CA.

43 Eleanor Coerr and William E. Evans, *Gigi: A Baby Whale Borrowed for Science and Returned to the Sea* (New York: G.P. Putnam's Sons, 1980).

44 Donald W. Wilkie to David W. Kenney, 26 March 1971, Box 56, Carl Leavitt Hubbs Papers, MC 5, Scripps Institution of Oceanography, La Jolla, CA.

45 Eleanor Coerr and William E. Evans, *Gigi: A Baby Whale Borrowed for Science and Returned to the Sea* (New York: G.P. Putnam's Sons, 1980).

lost, but it also helped to reinforce the point that the calf had been captured for primarily scientific reasons.⁴⁶ In Evans's proposal for releasing Gigi he wrote, "The only justifiable reason for procuring and studying an endangered species is to provide knowledge which will aid the scientific community in arriving at sound procedures for wise management, protection, and propagation of the populations of these species still surviving."⁴⁷ The participation of Evans and other scientists helped transform a failed commercial experiment into an uplifting story about "a baby whale borrowed for science and returned to the sea."⁴⁸ Over the course of the 1970s, the Sea World management continued to attempt to reposition the theme park as a conservation- and science-oriented institution, while also lobbying vigorously in Washington against legislation that would constrain its operations. Scientists were often willing to help legitimate these efforts. In 1974, for example, Erickson offered Sea World his assistance in assembling a new polar exhibit. "Considering the tenor of the day," Erickson wrote, "a prime concern of any polar exhibit utilizing polar mammals would be in defending any proposed action and in this function I trustfully might also be able to assist Sea World."⁴⁹ In 1976-1977, when Sea World spun off its research branch into the independent Hubbs-Sea World Research Institute, it mounted a carefully coordinated public relations campaign to ensure that the institute fulfilled its primary function, which was to counter claims that Sea World's sole reason for existence was to exploit marine mammals for profit.⁵⁰

46 Carl L. Hubbs to Alan Baldrige, 9 June 1972, Box 53, Carl Leavitt Hubbs Papers, MC 5, Scripps Institution of Oceanography, La Jolla, CA.

47 William E. Evans, Memo, 31 January 1972, Box 56, Carl Leavitt Hubbs Papers, MC 5, Scripps Institution of Oceanography, La Jolla, CA.

48 Eleanor Coerr and William E. Evans, *Gigi: A Baby Whale Borrowed for Science and Returned to the Sea* (New York: G.P. Putnam's Sons, 1980).

49 Albert W. Erickson to Frank Powell, Jr., 30 April 1974, Box 34, Carl Leavitt Hubbs Papers, MC 5, Scripps Institution of Oceanography, La Jolla, CA.

50 Richard B. Lippin to Frank Powell, Jr., 28 November 1975, Box 22, Carl Leavitt Hubbs Papers, MC 5, Scripps Institution of Oceanography, La Jolla, CA.

When the relevance to conservation was clearer and conflicts of interest were less apparent than in Erickson's orca study or Gigi's capture and release, scientists found it easier to conduct hands-on research, even with endangered species. At the National Marine Fisheries Service's Southwest Fisheries Center in La Jolla, where William Perrin had first sounded the alarm about the problem of dolphin bycatch in Pacific tuna fisheries, Jacqueline G. Jennings began testing radio-tags on dolphins; her work never attracted significant outside criticism. A method of reliably attaching tags to large whales in the open ocean was still lacking, however. Over the course of the 1970s a number of researchers—some new to radio-tagging and some affiliated with the ONR group that had been working on it since the early 1960s—began to focus on solving the attachment problem, using finback whales, humpback whales, and gray whales as experimental subjects. Gray whales were perhaps the best subjects, because they were generally docile, even friendly, and spent most of their time browsing in shallow waters near shore. But even grays were difficult to tag. In February 1973, for example, one researcher tagged a whale in Baja California but was able to track it for only five hours before the tag broke loose.⁵¹ Over the next few years, a loose consortium of ONR-affiliated researchers collaborated to develop a whale radio-tag that could be fired from a shotgun. In 1977, Schevill and Watkins took five different potential dart designs to an Icelandic whaling station, where they fired the darts into carcasses of whales that had been hauled into dock and used high-speed photography to record their behavior upon impact. By 1978, they were finally ready to test the tag in the field.⁵²

The person in the best position to take advantage of the new technique, however, turned out to be a researcher who had had no experience with cetacean radio-tagging or, indeed, with

51 Kenneth S. Norris to Carl L. Hubbs, 7 June 1972; Kenneth S. Norris to Carl L. Hubbs, 20 March 1973, Box 56, Carl Leavitt Hubbs Papers, MC 5, Scripps Institution of Oceanography, La Jolla, CA.

52 William A. Watkins and William E. Schevill, *The Development and Testing of a Radio Whale Tag, Technical Report 77-58* (Woods Hole, MA: Woods Hole Oceanographic Institution, 1977)

any form of marine mammalogy. In the fall of 1978, as Schevill, Watkins, and their colleagues began testing the new radio-tag on finback and humpback whales in Alaska and on California gray whales in Baja California, Erich H. Follman was searching for a way to become part of NARL's Project Whales, where he had been a postdoctoral fellow since 1977.⁵³ Follman had never studied marine mammals, let alone bowhead whales, but he had learned how to use radio-telemetry on red and gray foxes for his PhD at Southern Illinois University in the early 1970s and had continued to use the technique for physiological studies of captive arctic wolves and foxes at NARL. The idea of radio-tagging bowheads was not new—it had been proposed as early as 1973 by the Navy-funded marine mammalogist G. Carleton Ray—but for technical and political reasons it had never been implemented. By the late 1970s, however, the time seemed right. Schevill, Watkins, and their colleagues had apparently perfected the technique at just the moment when it could help resolve an international controversy over the status of the bowhead.

Resistance and Appropriation

Radio-tagging promised to help resolve a pressing environmental dilemma by revealing the migratory pathways of bowheads—a crucial piece of information that could be used to improve population estimates and assess the impact of whaling and offshore oil exploration. As NARL saw the end of Navy funding approach, it turned to the Bureau of Land Management, which was in the middle of a massive environmental assessment of the potential impact of opening the outer continental shelf of Alaska to oil and gas exploration. In 1978, BLM's Minerals Management Service gave NARL a large grant to support “Project Whales,” of which Follman's radio-tagging study was a significant component. Although it held promise, the technique also

⁵³ “Project Whales—New Research Program Proposed for NARL,” *NARL News* 2 (Sept 1978): 1-2.

involved handling an animal that had become overloaded with meanings by Iñupiat whalers, oil company representatives, federal bureaucrats, scientists, animal rights activists, and environmentalists—a cacophony of conflicting values and voices in which, it should be noted, the perspective of the whales themselves was often lost.⁵⁴ Ultimately, the intensity of the controversy on the North Slope hindered rather than helped Follman's proposal. Amidst the welter of competing claims to speak authoritatively on behalf of whales, Follman and his colleagues quickly realized that the radio-tagging proposal faced major challenges on two fronts: first, the scientific community, including ONR, NMFS, and the Marine Mammal Commission, which saw NARL as an inexperienced interloper, and second, the Iñupiat community as represented by the Alaska Eskimo Whaling Commission, which was suspicious of outside scientists and wary of hands-on research techniques.⁵⁵

In the face of this opposition, Follman had one trump card: the support of Oregon State University marine mammalogist Bruce R. Mate, who had been working independently of the ONR-funded group to develop a radio-tag. But even Mate, who was eager to see the technique used and to gain access to the funding that the Bureau of Land Management had offered to NARL, cautioned Follman about the challenges he would face. Opposition from scientists

54 The claim that the whales' perspective had largely been lost begs the question of what it might mean to include the perspective of the whales in such a debate. Animal rights activists made the strongest claims to speaking on behalf of the whales, but such claims were not uncontested. As Linda Nash has suggested, attributing “agency” to non-humans cannot simply mean exporting human forms of agency to animals (or anything else); it must involve a change in our understanding of agency even for humans; Linda Nash, “The Agency of Nature or the Nature of Agency?” *Environmental History* 10 (2005): 8 pars. 2 Dec. 2007 <<http://www.historycooperative.org/journals/eh/10.1/nash.html>>. Nash points to Latour's elaboration of actor-network theory as a promising path forward (e.g., Bruno Latour, *Pandora's Hope: Essays on the Reality of Science Studies* [Cambridge: Harvard University Press, 1999]). See also Timothy Mitchell's account of “hybrid agencies” in the chapter, “Can the Mosquito Speak?” in *Rule of Experts: Egypt, Techno-Politics, Modernity* (Berkeley: University of California Press, 2003): 19-53; Donna Haraway, *The Companion Species Manifesto: Dogs, People, and Significant Otherness* (Chicago: Prickly Paradigm Press, 2003).

55 These political challenges were discussed at a meeting of Project Whales staff on 27 December 1978, Box 63, Records of the Naval Arctic Research Laboratory, Ca. 1940's to 1980's, Accession 89-188, Elmer E. Rasmuson Library, University of Alaska, Fairbanks.

reflected the changing world of wildlife biology in the 1970s, when the cozy, cooperative atmosphere in which the field had been nurtured during the 1960s was replaced by a far more competitive environment. In this new environment, access to the most advanced techniques and the right to use them to address high-profile environmental issues were precious resources. Follman was only one of the new actors to join the radio-tagging game with the financial backing generated by the requirements of the Endangered Species Act, the Marine Mammal Protection Act, and other environmental laws. Like Follman, Jacqueline Jennings at NMFS's Southwest Fisheries Center in La Jolla recognized the potential utility of radio-tags for resolving the bowhead controversy, and she and her colleagues at NMFS fought to keep NARL from invading what they saw as their turf.⁵⁶ Even after Follman's project had been redirected toward gray whales in Baja California under the direction of Mate, and it had become clear that opposition from whalers would prevent the tagging bowheads off the coast of Alaska for the foreseeable future, NMFS remained extremely sensitive about potential overlap between its biotelemetry program and that of NARL.

Even more significant than the turf war between NMFS and NARL was the opposition from the Office of Naval Research, which had been the major supporter of cetacean radio-tagging since the early 1960s. Based on advice from the inner circle of ONR-sponsored radio-tag developers, ONR's program officers believed that the technique was still too experimental to be used in such a politically explosive situation. Even with an improved attachment, for example, it

⁵⁶ At the time, the National Oceanic and Atmospheric Administration was trying to consolidate its authority over marine environmental issues, in part by establishing dominance in remote sensing of the marine environment, which included radio- and satellite-tracking of marine mammals; see D.L. Alverson to H.L. Rietze, 19 July 1976; Ronald J. Morris to Fred Thorsteinson, 17 June 1976-06-17, Box 29, Bureau of Commercial Fisheries/National Marine Fisheries Service, Director's Correspondence, 1944-1979, Records of the National Oceanic and Atmospheric Administration, Record Group 370, National Archives and Records Administration, Pacific Region, Anchorage, AK.

was unclear how long the whale's body would tolerate the tag before rejecting it. In November and December 1978, ONR's Ronald Tipper tried to convince NARL Director John Kelley that the public's humanitarian concerns about hands-on research and the skepticism of the scientific community toward the technique required constraining the development of radio-tagging to a small, well-coordinated group—namely, the group that ONR had been cultivating since the early 1960s.⁵⁷

With the help of Mate, Follman might have been able to continue with his plans to radio-tag bowheads despite the opposition of the rest of the cetacean radio-tagging community, but he could not go ahead without the support of the Iñupiat community, which was vital to the day-to-day success of Project Whales and to the long-term future of NARL.⁵⁸ The whaling community had begun discussing tagging as early as 1977, the year that the Alaska Eskimo Whaling Commission was founded, perhaps in response to proposals from NMFS. Although it did not explicitly stake out a position against tagging, it was clear that the community was uncomfortable with the technique. Early in October 1978, it appeared that the whalers would accept Mate's tag, which was smaller than the OAR tag and meant to be attached with a harpoon rather than a shotgun.⁵⁹ Unfortunately for Project Whales, it quickly became clear that Mate's tag was not yet ready for operational use, which left the less-preferred OAR tag as the only viable option.⁶⁰ In late November 1978, the Project Whales staff held a meeting to discuss whale

57 ONR's Ronald Tipper wrote NARL director John Kelley to say that “we are especially concerned that a hurry-up tagging program may be premature and if the data return is poor may so arouse vocal members of the community as to impede continued long-term development”; Kelley reassured Tipper that he shared his concerns and invited him to visit NARL in person; Ronald C. Tipper to John J. Kelley, 15 December 1978; Ronald C. Tipper to John J. Kelley, 16 February 1979, Box 63, Records of the Naval Arctic Research Laboratory, Ca. 1940's to 1980's, Accession 89-188, Elmer E. Rasmuson Library, University of Alaska, Fairbanks.

58 Ray Dronenburg, “Project Whales,” *NARL News* 3 (March 1978): 8.

59 Gary A. Laursen, Notes on Telephone Conference With G. Hufford, 3 October 1978, Box 63, Records of the Naval Arctic Research Laboratory, Ca. 1940's to 1980's, Accession 89-188, Elmer E. Rasmuson Library, University of Alaska, Fairbanks.

60 Gary A. Laursen, Notes on Meeting With John J. Kelley, Commanding Officer, and G. Bienek, 13 November

tagging with leaders of the AEWCC, including Jacob Adams, Eugene Brower, Art Oomituk, and Eben Hopson, the mayor of the North Slope Bureau and the AEWCC's chairman. Although Hopson was the only one to speak out strongly against tagging, his opinion carried the day.⁶¹ As Kelley explained a few days later to the representative of BLM's Outer Continental Shelf program in Anchorage, "Resistance from the native community will definitely occur if we proceed with the bowhead study"—an unacceptable outcome given NARL's precarious position. Alienating the Iñupiat community would not only threaten the rest of Project Whales, whose day-to-day operation was heavily dependent on local cooperation, but would also complicate efforts to continue the laboratory in some form after the Navy withdrew its support.⁶²

As a result, bowhead tagging was pulled from Project Whales, though other aspects of the project—such as tissue sampling and bioacoustic surveys—went ahead as planned. Follman continued to hope that the tagging component it might be reinstated in later years when Mate's tag was operational; in the meantime, the BLM funds that had been intended for bowhead tagging went to support Mate's research on California gray whales in the lagoons of Baja California. In 1979, Mate tagged a gray whale and tracked it from Baja to Unimak Pass in the Aleutians, the longest continuous track that had yet been recorded.⁶³ Follman touted the success

1978, Box 63, Records of the Naval Arctic Research Laboratory, Ca. 1940's to 1980's, Accession 89-188, Elmer E. Rasmuson Library, University of Alaska, Fairbanks.

61 Gary A. Laursen, Notes on Project Whales Meeting, 20 November 1978, Box 63, Records of the Naval Arctic Research Laboratory, Ca. 1940's to 1980's, Accession 89-188, Elmer E. Rasmuson Library, University of Alaska, Fairbanks.

62 John J. Kelley to Gary A. Hufford, 22 November 1978, Box 63, Records of the Naval Arctic Research Laboratory, Ca. 1940's to 1980's, Accession 89-188, Elmer E. Rasmuson Library, University of Alaska, Fairbanks. Similarly, Project Whales director Gary A. Laursen told his staff in December 1978 that the "general consensus as regards the tagging of bowhead whales would appear to be that the probability of success, both in obtaining permission from the Eskimos and in receiving a tagging permit, is low"; Gary A. Laursen to Distribution, 8 December 1978, Box 63, Records of the Naval Arctic Research Laboratory, Ca. 1940's to 1980's, Accession 89-188, Elmer E. Rasmuson Library, University of Alaska, Fairbanks.

63 Erich H. Follman, "Gray Whales Radio-Tagged in Mexico," *NARL News* 3 (March 1979): 5-6; Erich H. Follman, "Tagged Gray Whale Located Off West Coast," *NARL News* 3 (May 1979): 1; Erich H. Follman, "Flash!! Gray Whale With Radio Tag Located at Unimak pass," *NARL News* 3 (June 1979): 4.

in the NARL Newsletter, but it failed to make much of a difference to Project Whales. BLM's support for biotelemetry wavered, and the project came near to being canceled in 1979.⁶⁴ BLM continued to fund Mate's work in Baja with an eye toward using the technique in the Arctic, but when NARL closed its doors and Project Whales came to an end in 1981, Follman had still not managed to get a federal permit or the whalers' permission to tag bowheads.

In the mid-1980s, another, far more successful component of Project Whales radically improved the census techniques used for estimating bowhead populations. The use of underwater hydrophones allowed scientists to detect passing whales even when weather conditions or ice cover made it impossible to spot them visually; in combination with improved aerial and shore-based counts, the bioacoustic surveys made it clear that there were far more whales than had been thought—just as Iñupiat whalers had claimed from the beginning of the controversy. Many of the whales migrated through areas of heavy ice cover far from shore. Rather than less than two thousand whales, there now appeared to be nearly eight thousand, a number easily large enough to absorb the impact of a well-regulated hunt. In the spring of 1981, the National Oceanic and Atmospheric Administration and the Alaska Eskimo Whaling Commission signed a cooperative agreement giving the AEWC regulatory authority over the bowhead whale. Radio-tagging, despite Follman's high hopes, had played no role.⁶⁵

Although Eben Hopson and other whalers had opposed tagging bowheads using the technology available to Follman and his colleagues in the late 1970s, they were in no way averse

64 Coordination with the Bureau of Land Management began to break down in spring 1979. BLM was unsure about which direction it wanted to go with the project, and especially with its biotelemetry component; Michael E. Brown to Technical Director [John J. Kelley], 17 March 1979, Box 66, Records of the Naval Arctic Research Laboratory, Ca. 1940's to 1980's, Accession 89-188, Elmer E. Rasmuson Library, University of Alaska, Fairbanks.

65 W.C. Cummings and D.V. Holliday, "Passive Acoustic Location of Bowhead Whales in a Population Census Off Point Barrow, Alaska," *Journal of the Acoustical Society of America* 78 (1985): 1163-1169.

to using scientific research to improve their understanding of bowheads and to raise their epistemological authority in policy debates. On the contrary, community leaders such as Eugene Brower argued that acquiring scientific expertise was fundamental to the survival of traditional Iñupiat cultural practices, such as the bowhead hunt.⁶⁶ When NARL closed its doors in 1981, many of its scientists stayed on in Barrow as employees of the North Slope Borough's Department of Wildlife Management or as consultants to the Alaska Eskimo Whaling Commission. In the mid-1980s, the native corporation governing Barrow took over the abandoned NARL facilities; its first tenant was the wildlife management department. In effect, the native community appropriated many of the scientists, facilities, techniques, and ideas of Project Whales—including the use of radio-tracking. While many Iñupiat whalers and hunters remained suspicious of scientists, including those working for native corporations, over the long term the integration of former NARL scientists and facilities into the native corporation helped smooth relations and render invasive techniques such as radio-tracking more palatable.

After the Iñupiat community took charge of establishing its own whaling quotas and conducting its own scientific research, Follman's expertise with radio-telemetry found a new use, one that matched the whalers' own agenda more closely than his Project Whales proposal had. Since at least the early 1970s, Iñupiat whaling had been criticized for the high proportion of whales injured but not recovered during the hunt, the so-called “struck-and-lost” rate, which was estimated at nearly fifty percent. Harpoons sometimes failed to stay embedded in the whale, ropes attaching the harpoon to the boat sometimes broke, and whales that had been fatally

66 In 1981, Eugene Brower wrote: “If our Culture and our subsistence use animals are to survive during these times of change we must learn much more about our animals, their management and their environment”, Eugene Brower to Friends, 14 December 1981, reprinted in Alaska Eskimo Whaling Commission, *Research Prospectus for the North Slope Borough and the Alaska Eskimo Whaling Commission for 1982-83 (Task Statements)* (Barrow, AK: Alaska Eskimo Whaling Commission, 1981): 11.

injured were sometimes lost under ice or intentionally cut loose when storms or rough seas made it too dangerous to tow them into shore. Criticism of the high struck-and-lost rate motivated the Iñupiat whaling community to improve its weapons and hunting practices—and, just as importantly, to make those efforts public. In 1981, the AEWK scientific advisory committee recommending developing a radio tag that could be attached to harpoons, thereby allowing whalers to locate struck whales even if they were out of visual range.⁶⁷ In 1983, Follman, who had moved to the University of Alaska at Fairbanks, began developing the radio tag for harpoon floats in collaboration with whalers and with funding from the AEWK.

Eventually whales too would be tagged, though progress was slow and most initial work took place mostly in non-Alaskan waters—either in the Canadian portion of the Beaufort Sea, far to the east of Point Barrow, or in the Arctic waters of eastern Canada and Greenland, where an entirely separate population of bowheads (also known as Greenland right whales) migrates. Throughout the 1980s, experiments were conducted with conventional radio-tracking and satellite-tracking of whales by Mate at Oregon State University, by members of the ONR-supported group, by scientists at the NMFS, and, increasingly, by researchers at non-U.S. institutions, such as the Sea Mammal Research Unit in Scotland. In 1988, Douglas C. Wartzok, a member of the ONR-supported group who had begun working on cetacean tracking at Johns Hopkins University in the 1960s, tracked bowheads in the Canadian and American regions of the Beaufort Sea using conventional radio tags, and in 1992, Bruce Mate—still funded by the Bureau of Land Management—tracked several bowheads using a satellite tag. But by this time, when bowhead population was clearly growing despite the annual hunt and offshore oil development

⁶⁷ Thomas F. Albert to Murray Johnson, 8 May 1981, and attached John J. Kelley, “Prospectus for the Science Advisory Committee of the Alaska Eskimo Whaling Commission,” January 1981, Box 2, Douglas G. Chapman Papers, Accession 4581-001, University of Washington Libraries, Seattle.

was, for the moment, on hold, the urgency that had motivated the original bowhead tagging proposals from Follman and the NMFS was gone.

Although tagging of bowheads in Canada and Greenland began as early as the late 1980s, it took nearly three decades for tags to be deployed in Alaskan waters. On June 24, 2004, scientists with the University of Fairbanks and the Alaska Department of Fish and Game held the first of several meetings with representatives of the Alaska Eskimo Whaling Commission to discuss plans for a satellite-tagging study of bowheads. As in the late 1970s, whalers raised questions about the impact of tagging on both whaling and whales; there were also concerns about the potential for the results of a tagging study to negatively impact whaling quotas. At a meeting on February 3, 2006, at which researcher Lori Quakenbush presented the study plan, the AEWC passed a resolution to withdraw support for the study “if evidence of harm to the whales was discovered or if any interference with subsistence whaling activities occurred.”⁶⁸ With this conditional support from the Iñupiat whaling community, the first tagging of bowheads off the North Slope took place in 2006, when two whales were tagged near Barrow, one each during the spring and fall migrations. In 2007, eight tags were deployed, five in the eastern Beaufort Sea with help from whalers at the villages of Kaktovik (located within the Arctic National Wildlife Refuge) and Aklavik (in the Yukon), and three near Barrow. The operations near Barrow were conducted by Lewis Brower, an Iñupiat whaler, and the scientist John Craighead George, a nephew of the Craighead twins. George was a former NARL staff member who had arrived in

68 Previous satellite tagging studies of bowheads cited in the planning document included Bruce R. Mate, G. K. Krutzikowsky, and M. H. Winsor, “Satellite-Monitored Movements of Radio-Tagged Bowhead Whales in the Beaufort and Chukchi Seas During the Late-Summer Feeding Season and Fall Migration,” *Canadian Journal of Zoology* 78 (2000): 1168-1181; M. P. Heide-Jørgensen, K. L. Laidre, Ø. Wiig, M. V. Jensen, L. Dueck, L.D. Maiers, H. C. Schmidt, and R. C. Hobbs, “From Greenland to Canada in Ten Days: Tracks of Bowhead Whales, *Balaena mysticetus*, Across Baffin Bay,” *Arctic* 56 (2003): 21-31.

Barrow in 1977, just as Project Whales was beginning, and like many NARL scientists had remained involved with conservation and science on the North Slope.⁶⁹

This tagging study was, in a sense, the culmination of a process of mutual appropriation that had begun awkwardly in the crisis atmosphere of the 1970s. At a speech given to the Alaska Forum on the Environment in February of 2004, North Slope Borough Mayor George Ahmagoak stressed the importance of the “original Iñupiat environmentalism” and the practical knowledge of hunters to the preservation of the North Slope environment. Today, however, he went on, “stewardship is more complicated. It's about western science and management and politics. ... That's why we have funded quite a bit of our scientific research. Our bowhead whale studies have been so comprehensive and long-term that the bowhead is now among the best understood whales on earth.”⁷⁰ The story of the failure of attempts to tag bowheads in Barrow in the late 1970s and the eventual appropriation of the technique by the Iñupiat whaling community is part of a larger narrative about shifting attitudes towards intensive scientific management of wildlife and wilderness. By the late 1970s, the scientific community was well aware of the stakes involved in the use of invasive techniques to study charismatic megafauna in the field using highly visible, hands-on techniques. Follman's plan to radio-tag bowheads failed not only because of opposition from the Iñupiat community but also because of opposition from other scientists, who feared a public backlash should anything go wrong. The conduct of fieldwork in

69 Craig George's full name is John Craighead George; he is the son of Jean Craighead George, sister to John and Frank Craighead. The bowhead study was coordinated by Lori Quakenbush of the Alaska Department of Fish and Game; for sample tracks of the movements of tagged whales, see <http://wildlife.alaska.gov/index.cfm?adfg=marinemammals.bowhead>, retrieved on 3 December 2007. The results of the study are also featured on the website of the North Slope Borough's Department of Wildlife Management, which is closely allied to the Alaska Eskimo Whaling Commission: <http://www.co.north-slope.ak.us/nsb/myweb/bowheadprojects.htm>, retrieved on 3 December 2007.

70 George N. Ahmagoak, Sr., “Keynote Presentation to the Alaska Forum on the Environment,” 11 February 2004, Anchorage, AK, <http://www.nativescience.org/assets/Documents/PDF/%20Documents/MayorAFESpeech2004.pdf>, retrieved on 24 April 2008.

wildlife biology had become a subject for public debate. Through the peer-reviewed grantmaking and permitting processes, as well as through less formal channels, the community of wildlife biologists began to police its own field practices with an eye to preserving its public reputation. Over the next two decades, the public debate over the practices of wildlife biologists produced an evolution in the relationship between scientists and special-interest advocacy groups, ranging from animal rights organizations to native corporations. As Peter Alagona has shown, a similar but more public debate over the radio-tagging and captive breeding of California condors in the 1980s resulted in a transformation of attitudes toward “management” among environmentalists. By the late 1980s, instead of accusing condor researchers of “mutilative biology,” as Friends of the Earth founder David Brower and others had in the early 1980s, environmentalists embraced such techniques as a useful “last resort” in the context of a biodiversity crisis.⁷¹ Moreover, though they had been skeptical of the use of invasive techniques when they were in the hands of government scientists, they began to realize that such techniques could serve as powerful tools for advancing their own agendas.

71 Peter Alagona “Biography of a ‘Feathered Pig’: The California Condor Conservation Controversy,” *Journal of the History of Biology* 37 (2004): 557-583. On the use of the logic of “last resort” in psychiatry, see Jack D. Pressman in *Last Resort: Psychosurgery and the Limits of Medicine* (New York: Cambridge University Press, 1998).

6

THE EYE OF THE ALBATROSS TRACKING ALBATROSSES ON THE HIGH SEAS, 1989-2004

The headquarters of Collecte Localisation Satellites, a French company that operates a global surveillance system called Argos, is located in a light industrial park on the outskirts of Toulouse, one of the capitals of the European aerospace industry. Except for the picturesque Canal du Midi that runs past it, nothing about CLS distinguishes it from innumerable office buildings around the world; it gains all of its distinctiveness from its role as a node in the “space of flows” of modern capitalism that Manuel Castells has described.¹ More explicitly than most such nodes, CLS is about bringing disparate geographical locations—indeed, the whole world—together into a single “here.” When I visited in the summer of 2007 to interview some of its scientists and managers about the use of Argos to track wild animals, I was taken to the data processing and control room, where a supervisor gave me an overview of the system while standing in front of an LCD screen displaying a two-dimensional projection of the globe. Plotted on the screen were the locations of more than four thousand individual “platform terminal transmitters,” or PTTs, including PTTs that had been attached to albatrosses, whales, walruses, caribou, and numerous other wild animals. For certain kinds of animals, such as the seventeen extant species of albatross, Argos has transformed scientists' understanding of their behavior in the wild as well as the everyday rhythms of fieldwork. Once scientists have fitted an albatross with a PTT harness at a breeding colony in a remote island near the Antarctic, for example, they can receive daily updates on the animal's location via email or the Internet from the comfort of

¹ Manuel Castells, *The Rise of the Network Society* (Malden, MA: Blackwell Publishers, 1996).

their offices in places like Bordeaux or Cambridge. Such information would not merely have been difficult to obtain before the invention of satellite tags; it would have been impossible. Sailing before the wind on their enormous wings, rarely landing and leaving no traces when they do, albatrosses can travel many thousands of miles across open ocean during a single foraging trip, a journey that no seagoing vessel or aircraft could hope to accompany. Although leg bands have been in regular use for nearly a century, they have provided only the spottiest understanding of albatross movements. The Argos system still depends on intimate contact with the animal in order to attach the PTT, but in many ways it fulfills the “transcendent vision” that, although it was quickly abandoned in the face of field conditions, initially inspired biologists such as Dwain Warner to develop what has since come to be known as “conventional” radio-tracking in the late 1950s and early 1960s.

In fact, Warner, with the assistance of engineer William Cochran, was the first person to publicly propose tracking animals by artificial satellite in the early 1960s. Albatrosses were near the top of his list of candidates, but it would take more than a quarter-century for his dream to become a reality, and when it did it would be under conditions very different from those he had imagined. Among other things, it was French scientists, not Americans, who first succeeded in tracking an albatross by satellite. In 1989, Henri Weimerskirch, an evolutionary biologist who had studied albatrosses and other seabirds on the remote Crozet Islands of the southern Indian Ocean for his doctoral thesis, adapted an Argos PTT built by a Japanese company for use on wandering albatrosses, the largest and among the most endangered of the albatross species. In the following decade, as other groups joined in the effort to satellite-tag albatrosses, Argos enabled biologists to create a global map of how albatrosses used the oceans. The motivation for this

effort was partly scientific, but it was also driven by increasing concern about human threats to albatrosses, especially the growth in longline fisheries that began in the late 1980s and exploded in the 1990s. This, too, reflected a major change from the 1960s, when the major threat to albatross survival had been human activity at the breeding colonies, particularly the transformation of Pacific atolls into Cold War air bases. Satellite tracking held out the promise of finding win-win solutions for fishermen and albatrosses, neither of whom gained when hungry albatrosses, diving for bait as a longline was unspooled from the back of a fishing boat, were caught and drowned on the hooks. If satellite-tracking could reveal where and when albatrosses tended to feed, it might be possible, Weimerskirch and other albatross biologists hoped, to establish regulations to protect them.

Albatross conservation faced challenges other than the limited amount of information that scientists had about their behavior on the high seas, however—challenges that were both political and cultural in nature, and whose solution would demand novel ways of using radio-tracking that had little to do with transcendence. Albatrosses have a long and illustrious history of serving as literary symbols, so much so that the vast majority of references to albatrosses in the popular press have nothing to do with the bird, but unlike more charismatic animals—such as grizzlies, tigers, or whales—they did not have a large and vocal constituency of environmentalist supporters at the beginning of the 1990s. To make matters worse for conservationists, the threats to albatrosses were focused in international waters, where national laws had only limited purchase and where enforcement was, in any case, nearly impossible. To address the threat of longline fisheries would require both inspiring commitment among a largely apathetic public and cooperation among a number of fishing nations, not all of which were sympathetic to the

albatross's plight. For researchers and conservationists concerned about the larger ecosystems of which albatrosses were a part, the challenge was even greater; fish have never elicited significant public concern or sympathy. At a scientific meeting on albatrosses held in Hobart, Tasmania, in 1997, British ornithologist John Croxall expressed his hope that albatrosses could serve as “the symbol and the spirit of the status of the global marine system.”² One of the strategies adopted to meet this challenge was the use of the emergent World Wide Web to distribute real-time information about individual satellite-tagged albatrosses to schoolchildren and the public. The Albatross Project that had been launched by David Anderson, a biologist at Wake Forest University, the year before was one of the first and most successful efforts along these lines. Over the course of the project, tens of thousands of schoolchildren were given an opportunity to plot the movements of individual albatrosses using the same raw data distributed to scientists by CLS. Instead of using radio-tagging to establish an exclusive, privileged relationship that would consolidate wildlife biologists' epistemological authority, it was now being used to inspire curiosity and concern among the public. The results of this engagement, Anderson and others hoped, would be to spur interest in science as well as concern for the environment—concern that would, sooner or later, translate into political action to protect endangered species.

By turning the logic of radio-tracking as a pathway to professional authority on its head, efforts such as Anderson's Albatross Project transformed the scientist from an expert whose technologically-mediated intimacy with wild animals gave him or her unique authority to speak on their behalf to a specialist who enabled a kind of virtual intimacy between individual animals and individual members of the public. In his interviews with the press, Anderson stressed the fact

2 J. P. Croxall, “Research and Conservation: A Future for Albatrosses?” in *Albatross Biology and Conservation*, edited by Graham Robertson and Rosemary Gales (Chipping Norton, Australia: Surrey Beaty & Sons, 1997): 276-286, on 286.

that, as he put it, “There's no filter.”³ In addition to transforming the role of the scientist, the Albatross Project and the many other similar projects that were launched beginning in the mid-1990s with funding from the National Science Foundation, the National Aeronautics and Space Administration, and other organizations also transformed the way the agency of radio-tagged animals was depicted. With the scientist no longer actively pursuing the radio-tagged animal in the field, interpreting the data, and presenting finished results to policymakers and the public, the satellite-tagged animal's agency in producing information about its own movements came to the fore. Researchers using radio-tagging to study marine mammals had described them as potential “oceanographic platforms” since at least the 1970s, but in the 1990s and early 2000s they began to speak of them as “oceanographers” as well. For albatrosses, the transition from passive targets of tracking to active participants in research was epitomized by the “Big Bird Race” sponsored by Ladbrokes, a British betting company, in 2004, in which gamblers were invited to place bets on which of a dozen satellite-tagged albatrosses would complete its migration from Tasmania to South Africa first. The scientists in this scenario were “trainers” who faded into the background once the tag had been attached, while the albatross “horses” and their Argos PTT “jockeys” played starring roles. Radio-tracking had been made into a channel for connection instead of, or in addition to, a means of control.

Natural History in the Space Age

One of the unlikely stars in the scientific and media discourse around wildlife satellite tracking has been the albatross—more accurately, the seventeen or so currently recognized

³ Scott Lafee, “Public Has Opportunity to Help Science Take Flight,” *San Diego Union-Tribune*, 18 February 1998.

species of albatross, most of which have been designated “endangered” or “threatened” by the World Conservation Union/IUCN. Of these, the wandering albatross is the largest, best-known, and among the most at risk of extinction. Because of their size and the large distances they travel in search of food, albatrosses featured prominently in the first proposal to track animals by satellite, which was developed by Dwain Warner and William Cochran in 1962. The preliminary proposal Warner submitted to NASA in April 1962, which mentioned Canada geese, albatrosses, and several other species as possible candidates for tracking, was not funded, so a year later Warner submitted a more detailed and more narrowly focused proposal involving the Laysan albatrosses that bred on the Midway Islands in the northwestern reaches of the Hawaiian chain.⁴ Warner's own research had focused primarily on neotropical migrants, but he had likely encountered albatrosses while conducting ornithological research for the Army in the South Pacific during World War II.⁵ More importantly, the Laysan albatross had been the subject of intensive research by American biologists since the mid-1950s, when the Navy decided that the birds were hindering operations at its air base at Midway. Midway's albatrosses had received attention before—in March 1945, for example, the *New York Times* published an article by a Marine describing “the famous Midway gooney” as the “best loved of all birds” on the island—but it was only after the immediate military exigencies of the Pacific War gave way to the tense standoff of the Cold War that the birds became more than a source of entertainment for bored servicemen.⁶ In the 1950s, when Midway was incorporated into the Distant Early Warning system that stretched from the North Pacific to the North Atlantic, the Navy became concerned about the threat posed by albatrosses to aircraft that landed on or took off from the island,

4 Dwain W. Warner, “A Study of Albatross Flight and Energetics by Satellite Radio Relay and Other Instrument Systems,” Grant Proposal to NASA, 15 April 1963, Dwain Warner Personal Papers.

5 Interview with Dwain Warner, Stanchfield, MN, August 2004.

6 Milburn McCarty, Jr., “Midway's Birds,” *New York Times*, 4 March 1945.

particularly jets. U.S. Fish and Wildlife Service biologists were called in to find ways to keep albatrosses away from the runway, though the birds proved insensible to anything but the most aggressive methods. (Two of the biologists were quoted in the *Washington Post* as saying that the birds “paid scarcely any attention to mortar fire.”⁷) In 1957, after one aircraft sucked an albatross into its engine, jet operations from the island were temporarily halted.⁸ Though careful to avoid offending the sailors and airmen who served on Midway, conservationists were quick to protest efforts to eliminate the notoriously trusting birds. When the Navy revealed in October 1959 that it had killed some 30,000 of Midway's albatrosses and was proposing to eliminate hundreds of thousands more, the president of the Audubon Society of the District of Columbia described the proposal as “an affront to the American people and destructive of American prestige throughout the world.”⁹ Robert Cushman Murphy, president of the National Audubon Society, urged the Navy to seek alternatives “before it horrifies decent public opinion the world over.”¹⁰ The subsequent controversy helped prevent the complete extermination of Midway's albatrosses, but by 1964 national security had won out over intangibles such as prestige or decent public opinion and the Navy moved forward to eliminate significant fraction of the island's albatross population.¹¹ At the same time, in response to this conflict and others, such as the nuclear weapons tests at Amchitka Island that took place between 1965 and 1970, conservationists began to strengthen their critique of national security as a justification for environmental damage.¹²

7 Nate Haseltine, “Gooney May Be Looney But Still Be Fun,” *Washington Post, Times Herald*, 7 March 1955.

8 William M. Blair, “U.S. Plans to Curb Periling of Planes by Midway Bird,” *New York Times*, 23 April 1958. In 1957-1958, the Navy estimated that 17 percent of all takeoffs and landings involved a bird strike, although none of the strikes resulted in a crash or injury. At the time of peak bird populations in November, the number reached as high as 40 percent; Irston R. Barnes, “DEW Line Dooms Albatross,” *Washington Post, Times Herald*, 18 October 1959.

9 Irston R. Barnes, “DEW Line Dooms Albatross,” *Washington Post, Times Herald*, 18 October 1959.

10 John C. Devlin, “Albatross War Brings Protest,” *New York Times*, 21 October 1959.

11 Malcolm Barr, “Gooney Birds Lose Midway Fight to Navy,” *Washington Post, Times Herald*, 11 January 1964.

12 Concerns about the impact of military activities on wildlife refuges and other natural areas were scattered and infrequent during and immediately after the war, but not nonexistent; see, for example, the concerns about

As Warner remembered it when I spoke to him in August 2004, he and Cochran came up with the idea of animal tracking by satellite over a few beers during the biotelemetry conference held at the American Museum of Natural History in the spring of 1962. In “Space Tracks,” an article published the next year in the museum's magazine, *Natural History*, they proposed that the use of the Doppler location method—in which the frequency shift produced by the movement of a satellite relative to a transmitter could be used to reconstruct the transmitter's location on the earth's surface—would allow the development of satellite tags small enough that they could be carried by large birds such as the Canada goose or the wandering albatross.¹³ As Warner described it, the goal of such a system was comprehensive, continuous, objective observation of “motile” responses of animals to their environment; the satellite was a means of providing the

military activity on Amchitka, an island in the Aleutian chain, that were expressed by the director of the U.S. Fish and Wildlife Service to the acting director of the Bureau of Land Management in 1946; Albert M. Day to Fred W. Johnson, 17 October 1946, Series 1, Box 1, Ira N. Gabrielson Papers, Denver Public Library, Denver, CO. Between the late 1950s and late 1960s such concerns became the subject of widespread discussion among wildlife biologists. A. Starker Leopold, for instance, helped mobilize wildlife managers to oppose the use of national security justifications for damage to wildlife refuges. In 1967, in the context of nuclear weapons tests on Amchitka, he wrote to mammalogist Stanley Cain: “I am sure the Army, the Navy, the Air Force, the Marines and the Coast Guard all have needs for island bases of one sort or another all over the Pacific; there is no real purpose in meeting with these agencies to listen to their needs. What I want to know is what we can do to assure that certain critical specific islands can be protected from all of these needs and retained as National Wildlife Refuges”; A. Starker Leopold to Stanley Cain, 8 November 1967, A. Starker Leopold Papers, Bancroft Library, Berkeley, CA. Leopold also corresponded on the subject with Thomas L. Kimball of the National Wildlife Federation, who wrote: “All too often, there looms up examples of where the Military has blundered in and arbitrarily embarked up on a program for an activity which would seriously impair the wildlife and refuge values”; Thomas L. Kimball to A. Starker Leopold, 17 November 1967, Series 1, Box 1, Ira N. Gabrielson Papers, Denver Public Library, Denver, CO.

- 13 The Doppler location method was the basis of the U.S. Navy's Transit system, which allowed ships to locate their own positions and was crucial for accurate missile targeting. Transit emerged from efforts to track Sputnik and other satellites; the first Transit satellite was launched in 1959 and the system became operational in the early 1960s. It was developed largely by the Johns Hopkins University Advanced Physics Laboratory; see Donald A. Mackenzie, *Inventing Accuracy: A Historical Sociology of Nuclear Missile Guidance* (Cambridge, MA: MIT Press, 1990), 144-145. The Doppler location method depends on the shift in frequency that takes place when a receiver and a transmitter are in motion relative to each other. As the satellite approaches the transmitter, the frequency increases; as it grows more distant, the frequency decreases. Sampling this shift at multiple points produces estimates of the distance of the receiver from the transmitter; these estimates can be visualized as a series of spheres centered on the satellite. The intersection of these spheres and the earth's surface can be used to determine the location of the transmitter. For a transmitter in motion, multiple distance estimates must be acquired within a short period of time to produce an accurate location. The transmitting frequency must be extremely stable because any drift would be mistakenly interpreted as a Doppler effect.

scientist with the all-seeing eye that would make that transcendent vision possible on a continental or even global scale. “Space Tracks” was essentially a popularization of a preliminary proposal that Warner, with Cochran's technical assistance, submitted to NASA on April 16, 1962, a month after the AMNH meeting.¹⁴ It outlined the reasons for developing the system and a set of technical specifications worked out by Cochran. As elsewhere, Warner stressed the need for biologists to embrace new technologies in order to transcend the limits of the human senses and of existing techniques such as bird-banding. “How can we find out where the penguins of the Antarctic go after mating season; the routes of the wandering albatross or the Caribbean turtle; the forces governing caribou movements; the track of the Canada goose?” The answer: “artificial satellites.”¹⁵ Cochran's technical proposal was essentially an inversion of the Doppler-based Transit system that had recently been developed by the Navy to provide locations to ships at sea. Cochran's experience with tracking satellites at the University of Illinois under the supervision of ionospheric researcher and radio astronomer George Swenson made the idea of tracking animals by satellite an easy leap. Instead of tracking a satellite in an unknown orbit from a known ground position, one would be tracking an unknown ground position from a satellite in a fixed orbit. Warner's and Cochran's proposal anticipated the Argos system developed in the 1970s in almost all respects, including the need for computerized analysis centers to process the massive amounts of data such a system would produce.¹⁶

14 Dwain W. Warner, “Preliminary Request for Research Grant and for Instrument Space on a Satellite in Polar Orbit,” Grant Proposal to NASA, 16 April 1962, Dwain Warner Personal Papers.

15 Dwain W. Warner, “Space Tracks: Bioelectronics Extends Its Frontiers,” *Natural History* 62 (1963): 8-15, on 12.

16 On August 2-4, 1962, Warner presented the idea of satellite tracking of migratory birds to a meeting of researchers working at the Delta Waterfowl Research Station in Manitoba, Canada. After the meeting, John B. Moyle shared Warner's idea with the Minnesota Conservation Department's fish and game research staff, writing: “[Warner's] most recent idea is that birds in migration can be tracking by use of an earth satellite somewhat like Telestar. It is evident that this is a rapidly changing field of investigation that shows considerable promise”; John B. Moyle to Staff of Section of Research and Planning, 9 August 1962, Box 3, Minnesota Conservation Department Game & Fish Division, Research and Planning Section, Administrative Files, 1957-1966, Minnesota Historical Society, St. Paul, MN.

For Warner and Cochran, the need for such a system was made evident by the difficulty of studying bird migration using conventional wildlife radio-tagging, then still only a few years old. In October 1962, as Kennedy and Krushchev were playing chicken over missiles in Cuba, they were waiting in a station wagon at a wildlife refuge in South Dakota for a dozen blue snow geese that they had radio-tagged to begin their southward migration. With them was a graduate student, Dennis Raveling, whose recollection of the study on the occasion of Warner's retirement some twenty-five years later revealed the pervasiveness of Cold War tension at the time: "How well I remember assisting you and Bill Cochran while listening to radio Moscow and radio Havana in between monitoring radio-instrumented geese in the first successful field test of telemetry of wild, long-distance migrants at Sand Lake, South Dakota during the Cuban missile crisis in October 1962!"¹⁷ The study was, in fact, less of a success than Raveling remembered. Despite the impending winter, the geese refused to migrate, and the researchers returned to Minnesota after several weeks with little to show for it. Tracking the birds by satellite would have allowed them to return to Minneapolis as soon as the tag was attached and still acquire migration data without the trouble of a high-speed chase to the Mexican border. Tests on high atmosphere balloons suggested that a transmitters and power sources small enough to be carried by birds would be powerful enough to be detected from space.¹⁸ Such experiences spurred Warner and Cochran to continue developing a means of studying migration by satellite. In March

17 Dennis G. Raveling to Dwain W. Warner, 1 October 1986, Dwain Warner Personal Papers. Raveling's letter was a contribution to a scrapbook of memorabilia collected for presentation at a retirement dinner held in Warner's honor at the University of Minnesota on 31 October 1986; see Warner Retirement Committee to Colleagues, 17 September 1986, Dwain Warner Personal Papers. In another letter written for the same event, L. David Mech recalled Warner telling him about wildlife satellite tracking the first time he entered his office in 1963; L. David Mech to Dwain W. Warner, 14 October 1986, Warner Personal Papers. When I interviewed Warner in August 2004, he told me that William Cochran had been "petrified by the idea the Russians were going to annihilate us through Cuba" and returned home to his wife in St. Paul in advance of the rest of the researchers.

18 Warner, "Space Tracks," 13.

1963, when *Science News* published a brief article on wildlife tracking by satellite, Warner was hopeful that a receiver for animal tracking could “piggy-back” on a “research polar-orbiting rocket” for a thirty-day pilot project within the next year. Like “Space Tracks,” the *Science News* article advanced a model of wildlife research in which the ultimate desideratum was total surveillance: “a round-the-clock vigil on animals, no matter where they wander on land, sea or air.”¹⁹

The ongoing controversy over Midway's albatrosses provided the context for Warner's proposal to NASA to track Laysan albatrosses by satellite, which would be able to take advantage of the ongoing research program at Midway. At a time when most research was focused on the behavior of the birds around their breeding colonies, the proposal was distinctive for its focus on the behavior and energetics of individual albatrosses during their extraordinary long-distance foraging flights. “Ultimately,” Warner wrote, “the responses of population of organisms are a reflection of the sum total of *individual* reactions and responses to the conditions of their environment.” Satellite tracking would mean that even the most far-ranging individuals were no longer “immune to typical observational studies.”²⁰ Despite its timeliness, the proposal went nowhere. When I interviewed William Cochran and George Swenson in August 2006, Cochran recalled Swenson telling him that it had served as comic relief at the end of a long day of reviewing proposals in Washington; Swenson remembered only that the earth and space scientists who reviewed such proposals were extremely jealous of satellite space. The navigational abilities of the Laysan albatross did not rank high on their list of scientific priorities,

19 “Satellites Track Animals,” *Science News Letter* 83 (2 March 1963): 130.

20 Dwain W. Warner, “A Study of Albatross Flight and Energetics by Satellite Radio Relay and Other Instrument Systems,” Grant Proposal to NASA, 15 April 1963, Dwain Warner Personal Papers. William Marshall had used very similar language in justifying the Grouse Project of the early 1960s; in the era of ecosystem ecology's ascent, it was in part a defense of the “life history” approach upon which wildlife management as a profession had been founded in the 1930s.

no matter how concerned the Navy and the Audubon societies might have been with bird-strikes on Midway.²¹

The Advantages of Remoteness

Although Warner and Cochran's proposals were not funded by NASA or other federal agencies, the "Space Tracks" article helped launch discussions about the possibility of using satellite technology for studies of ecology, animal behavior, and wildlife management. Scientists who studied far-ranging animals in hard-to-reach places, such as albatrosses, sea turtles, whales, and polar bears, were particularly enthusiastic. As the initial developmental phase of conventional wildlife telemetry began to wind down, excitement about satellite tracking increased. The first conference was held in late May 1966 at the Smithsonian Institution's Museum of History and Technology in Washington, DC. As with the 1962 biotelemetry conference at the American Museum of Natural History, the key instigator of the conference was Sidney Galler. In the fall of 1965 Galler had left the Office of Naval Research's Biology Branch to become the Smithsonian's Assistant Secretary for Science.²² The Smithsonian had had little if any involvement with radio-tracking before Galler's arrival, but during his five years there the institution became an important interface between scientists interested in radio-tracking and the Washington science-funding bureaucracy. The 1966 conference was cosponsored by NASA and by the American Institute of Biological Sciences, which had recently launched its BioInstrumentation Advisory Council to promote the incorporation of electronic devices into biology.²³ As its main technical representative, NASA sent Marjorie Townsend, a project

21 Interview with William W. Cochran and George Swenson, August 2006.

22 James Bradley, Assistant Secretary of the Smithsonian, announcement regarding appointment of Sidney R. Galler, 9 August 1965, Box 20, Record Unit 271, Smithsonian Institution Archives, Washington, DC.

23 In 1965 Marjorie R. Townsend became the first woman to manage a NASA space launch. She worked at

manager at the Goddard Space Flight Center who had helped design the Interrogation, Recording and Location (IRLS) system for the Nimbus B satellite, which was planned for launch in 1967-1968. John Tester was there to represent the Cedar Creek group, but neither Cochran, who had by then left Minnesota to return to the Illinois Natural History Survey, nor Warner, whose career had been sidelined by health problems, were in attendance.²⁴

Although the IRLS system would make it possible to collect data from ground platforms by satellite for the first time, it was far from ideal for animal tracking.²⁵ One engineer estimated the cost of an IRLS transponder that would meet NASA's most stringent technical specifications at \$65,000. The cost-per-transponder could be significantly reduced by relaxing some of the specifications, but the transponders would still be heavy and bulky enough to burden even the largest land animals, let alone birds. The IRLS system located platforms using the same Doppler technique that Warner and Cochran had proposed for animal tracking, but unlike that system, which required the tag to transmit only a single, stable frequency, IRLS depended on two-way

Goddard Space Flight Center from 1959 to 1980 on science and applications satellites, including the Nimbus series and its Interrogation, Recording and Location System. From 1975 to 1980, she was in charge of all advanced mission planning for science and applications satellites, including NOAA's meteorological satellites. See biographical information associated with her collected papers, which are held at Virginia Tech Libraries in Blacksburg: <http://ead.lib.virginia.edu/vivaead/published/vt/vibl/v00183.xml.frame>; Constance Holden, "NASA Satellite Project: The Boss Is a Woman," *Science* 179 (1973): 48-49. Townsend collaborated with Vagn Flyger on a proposed polar bear study; see Vagn Flyger and Marjorie R. Townsend, "The Migration of Polar Bears," *Scientific American* 218 (1968): 108-116.

- 24 The thirty-eight attendees of the conference included representatives of the Smithsonian, NASA, the Department of Interior, the National Research Council, the National Science Foundation, the American Institute of Biological Sciences, the Arctic Institute of North America, the Commission on Undergraduate Education in the Biological Sciences, and the Department of Defense, as well researchers from universities and research institutes; "IRLS Conference Participants," in BioInstrumentation Advisory Council, *Some Prospects for Using Communications Satellites in Wild Animal Research* (Washington, DC: American Institute of Biological Sciences, 1966): 19-21, in Box 20, Office of Environmental Sciences, Ecology Program Records, 1965-73, Record Unit 271, Smithsonian Institution Archives, Washington, DC.
- 25 The report was developed over the summer of 1966; it is dated 1 July 1966, but Lloyd E. Slater's included foreword is dated 1 August 1966; BioInstrumentation Advisory Council, *Some Prospects for Using Communications Satellites in Wild Animal Research* (Washington, DC: American Institute of Biological Sciences, 1966), in Box 20, Office of Environmental Sciences, Ecology Program Records, 1965-73, Record Unit 271, Smithsonian Institution Archives, Washington, DC.

digital communications between the tag and the satellite.²⁶ Participants in the 1966 conference estimated that IRLS transponders would weigh at least 20 pounds. Because of these issues of cost and weight, much of the conference discussion focused on outlining the requisites for a more suitable system to succeed IRLS. Nonetheless, a number of the researchers attending the conference proposed demonstration studies using IRLS in the hope that even modest success with the largest of animals would generate support for a future system dedicated to animal tracking. Candidates for tracking discussed during the conference included whales and other cetaceans, tuna and sharks, marine turtles, large land animals such as grizzlies, elk, caribou, and elephants, penguins and large sea birds (such as albatrosses), and polar bears.²⁷ Most of the researchers proposing these studies had previous experience using conventional radio-tracking, and their rationales for turning to satellite tracking addressed the kinds of frustrations and difficulties that Warner and Cochran had experienced in their failed attempt to track geese during the Cuban missile crisis.

From Experiment to Operation

Although studies of bird migration remained the holy grail of wildlife satellite tracking, it became apparent as early as 1966 that it would be technically impossible with the IRLS system.

If the IRLS package weight of 25 pounds was just barely acceptable for animals such as elk,

26 Howard Baldwin, "Some Background on the IRLS Opportunity," in *Some Prospects for Using Communications Satellites in Wild Animal Research* (Washington, DC: American Institute of Biological Sciences, 1966): 2-3, on 2, in Box 20, Office of Environmental Sciences, Ecology Program Records, 1965-73, Record Unit 271, Smithsonian Institution Archives, Washington, DC.

27 Discussions among participants in the conference to develop a "roster of potential experiments" continued over the summer; see Lloyd E. Slater to IRLS Animal Tracking Group I Enthusiasts (Howard Baldwin, Helmut K. Buechner, Frank C. Craighead, Archie Carr, William Schevill, and Vagn Flyger, copied to Sidney Galler, Dale Jenkins, John Olive, and Marjorie Townsend), 18 July 1966, Box 59, Helmut K. Buechner Papers, Record Unit 7279, Smithsonian Institution Archives, Washington, DC.

polar bears, or green turtles, which weighed anywhere from 300 to 800 pounds and were not especially aerodynamically sensitive, it was obviously far too much for even the largest albatrosses, which weighed at most 25 pounds themselves. Enthusiasm for the technique, however, outran the technical possibilities, as evidenced by the unsuccessful efforts of the Australian ornithologist David G. Nicholls to garner support from NASA and NSF for tracking pelicans and other birds in and around Australia. Nicholls began by contacting George Llano, director of NSF's Antarctic biology program, who referred him to Galler, who in turn put Nicholls in touch with Buechner.²⁸ Buechner encouraged Nicholls, shared the technical reports and proposals that the Smithsonian had developed with him, and forwarded his proposal on to his collaborators.²⁹ By August 1970, as a result of these communications, Nicholls had realized that the IRLS system would never work for birds and wrote to George Jacobs at NASA to withdraw his proposal.³⁰ Buechner and his colleagues still hoped that NASA would launch a new, simpler system similar to the one Warner and Cochran had proposed, one that would allow significantly lighter tags—but until such a system existed, bird tracking would have to wait. In Nicholls' case, the wait would amount to more than two decades. It was only in the early 1990s, after Weimerskirch and his colleagues had successfully satellite-tracked several wandering albatrosses in the Southern Indian Ocean, that he would attach his first satellite tag to an albatross. One reason for the long delay was that the one experiment that NASA did support between 1969 and 1974 demonstrated that the engagement with individual animals and environments upon which satellite tracking depended carried serious risks—risks that NASA was not, in the transitional

28 George A. Llano to David G. Nicholls, 25 April 1970; Sidney R. Galler to David G. Nicholls, 4 May 1970; Box 57, Helmut K. Buechner Papers, Record Unit 7279, Smithsonian Institution Archives, Washington, DC.

29 David G. Nicholls to Helmut K. Buechner, 1 July 1970, Box 57, Helmut K. Buechner Papers, Record Unit 7279, Smithsonian Institution Archives, Washington, DC.

30 David G. Nicholls to George Jacobs, 21 August 1970; Helmut K. Buechner to David G. Nicholls, 4 August 1970; Box 62, Helmut K. Buechner Papers, Record Unit 7279, Smithsonian Institution Archives, Washington, DC.

phase after the end of the Apollo program and in the belt-tightening mood of the early 1970s, willing to tolerate.

Although Buechner believed that NASA had decided to withdraw entirely from animal tracking in the wake of the publicity disaster of the tagging of “Monique,” the truth was more complicated. NASA decided not to support Buechner's applications to use the new Doppler location system aboard Nimbus 6, but it did not totally give up on animal tracking by satellite. At a 1973 meeting on wildlife satellite tracking in Santa Cruz, discussion focused mainly on the Random Access Measurement System (RAMS) expected to be carried aboard Nimbus 6 as the most technically promising option, especially since it was seen as a test run for a successor operational system to be carried aboard NOAA's Tiros weather satellites.³¹ Unlike the IRLS system aboard Nimbus 3 and 4, which had been used for the Monique study, RAMS on Nimbus 6 did not require sophisticated communications between the satellite and the transmitter platform. The tag broadcast a simple message at a predetermined frequency and the satellite listened for it—thus “random access” instead of “interrogation.” Without the need for a receiver, the tags could be significantly smaller, lighter, and cheaper, and therefore easier to adapt to animal tracking, just as Warner and Cochran had argued more than a decade earlier.³²

After the failed initial effort with IRLS, the RAMS system offered a second chance for wildlife biologists to convince NASA that wildlife tracking was worth the risk and to obtain a voice in the development of an operational system. The feasibility of using RAMS to study wildlife was demonstrated with a study on polar bears conducted by one of the groups of wildlife biologists with whom Buechner had been collaborating. Ironically, by the time satellite tracking

31 The 1973 Santa Cruz report was reprinted as Appendix A of Sebesta and Arno, *Wildlife Monitoring Program Plan*, 57-188.

32 See “Nimbus Program History,” http://atmospheres.gsfc.nasa.gov/nimbus/pdf/Nimbus_History.pdf, retrieved on 26 October 2004.

had become feasible for polar bears, the question that had initially motivated its use in the mid-1960s had been answered using old-fashioned (non-radio) tags and increased coordination among the polar bear biologists of the five circumpolar nations (USA, Canada, USSR, Denmark, and Norway). The polar bear was not, these studies had demonstrated, “a 'citizen' of the whole vast frozen Arctic” but a resident of particular areas that for the most part fell under the jurisdiction of single nations—a scientific result that dramatically simplified negotiations toward a polar bear conservation treaty in the mid-1970s.³³ But new questions and new sources of funding had been raised by the discovery of oil on Alaska's North Slope in 1969, which led to a boom in onshore development in the area just west of the Arctic National Wildlife Refuge as well as proposals to develop oil and gas deposits in the Chukchi and Beaufort Seas—prime polar bear habitat. Because of the passage of the National Environmental Protection Act, the Marine Mammal Protection Act, and the Endangered Species Act, the discovery of fossil fuels on the North Slope was a windfall for wildlife biologists as well as oil companies. In March and June 1977, under leadership of Jack Lentfer of the Alaska Department of Fish and Game, and in cooperation with the Naval Arctic Research Lab, with A. Lawrence Kolz of the Denver Wildlife Research Laboratory as the main engineer, three mature female polar bears were fitted with RAMS collars and released near Barrow.³⁴ In contrast to the Craighead's 1970 elk study, there was no high-profile protest against the satellite-tracking study. Newspapers were far more interested in the geopolitical resonances of a satellite-tracked polar bear from Alaska wandering

33 Lawrence E. Davis, “Polar Bears Found to Stay at Home—Sort Of,” *New York Times*, 16 September 1969. The making of the polar bear treaty is described in Anne Fikkan, Gail Osherenko, and Alexander Arikainen, “Polar Bears: The Importance of Simplicity,” in *Polar Politics: Creating International Environmental Regimes*, edited by Oran R. Young, and Gail Osherenko (Ithaca, NY: Cornell University Press, 1993): 96-151.

34 A. Lawrence Kolz, Jack W. Lentfer, and Henry G. Fallek, “Polar Bear Tracking Via Satellite,” Presented at the 15th Annual Rocky Mountain Bioengineering Symposium, 17-18 April 1978, Ames, IA.

into Soviet territory than they were with humanitarian questions.³⁵ One reason for the lack of humanitarian protest was that the transmitter and attachment were far better designed; the polar bear collars weighed less than half as much as the elk collars, about 5kg as opposed to 11kg. (As the largest land-based carnivore in existence, polar bears may also have been harder to depict as victims than a female elk.) Moreover, the remote location and harsh conditions in which polar bears lived—the very factors that made research difficult and satellite tags appealing—had the added advantage of keeping away journalists and hiding any untoward consequences. The collars may well have contributed to the bears' deaths, but since the bears were never recaptured, it was impossible to know. But perhaps the most important difference between the Monique study and the polar bear study was that the latter was not conceived as a publicity stunt. In 1979, the experiment was repeated on a broader scale: eleven bears were tagged in Alaska, Canada, and Greenland as part of a cooperative project among polar bear researchers from the United States, Canada, Denmark (which administers Greenland), and Norway.³⁶

The Development of Argos

By the time of this second use of RAMS to track polar bears, a new system called Argos had been put into operation by the National Oceanic and Atmospheric Administration (NOAA) in cooperation with NASA and its French equivalent, the Centre National d'Etudes Spatiales (CNES). Otherwise very similar to the RAMS system, Argos differed mainly in that it was intended to be a reliable, long-term observation system based on known technologies. The

35 “Russ to Help Find Pregnant Polar Bear,” *Los Angeles Times*, 30 December 1977; “First Catch Your Bear...,” *Nature* 271 (1978): 106.

36 Jack W. Lentfer and Douglas De Master, “Satellite Radio Tracking of Polar Bears,” in Charles Cote, Ralph Taylor, and Eugene Gilbert, *Nimbus 6 Random Access Measurement System Applications Experiments*, NASA SP-457 (Washington, DC: NASA, 1982): 52-53.

origins of Argos lay in 1971, when NASA and CNES had successfully collaborated in the development of an experimental system to track meteorological balloons called EOLE.³⁷ Since the late 1960s, the French government had prioritized the development of an independent national space capacity while continuing to cooperate with the U.S. and, to a lesser extent, the Soviet Union. Among the eventual results of this effort toward independence were the development of the Ariane launcher and the SPOT earth imaging system, both of which outcompeted the equivalent American efforts.³⁸ Commercial pressures and incentives were far less in the case of Argos, however, which was dedicated to environmental and scientific uses and was thus seen as an appropriate area for continued cooperation. In May 1978, as final preparations were being made for the launch of the first Argos instrument, the president of CNES described Argos and a closely-related search-and-rescue system to the House Subcommittee on Space Science and Applications as “the highlight of present Franco-American cooperation.”³⁹ The first Argos instrument, which allowed initial testing of the system to begin, was launched on the Tiros-N weather satellite in October 1978; the second instrument, which made the system operational, was launched on NOAA-6 in June 1979.⁴⁰

Although animal tracking was discussed as a possible application for Argos from the beginning, the system's development was driven by well-organized meteorological and oceanographic user communities and by the desire of the system's French engineers and

37 J. C. Morakis and C. E. Cote, “The Evolution of Location and Data Collection Systems in the United States,” Presented at the AIAA, ASME, and SAE, Joint Space Mission Planning and Execution Meeting, Denver, CO, July 10-12, 1973.

38 See National Academy of Sciences, *U.S.-Europe Collaboration in Space Science* (Washington, DC: National Academy Press, 1998): 14-41.

39 Statement of Dr. Hubert Curien, President of CNES, on May 16, 1978, International Space Activities, Hearings Before the Subcommittee on Space Science and Applications of the Committee on Science and Technology, U.S. House of Representatives, 95th Congress, 2nd Session, May 16-18, 1978, (1978): 2-15, on 8

40 Jean-Luc Bessis, “Le System Argos apres deux ans de fonctionnement,” in *Data Collection and Location by Satellite: User's Conference, October 1-2, 1980, Quebec, Canada* (Toulouse: Service Argos, 1980): unpaginated.

administrators to maintain high standards of precision and reliability. Both of these factors initially made it difficult for wildlife biologists to gain access to the system. That animal tracking was something of an afterthought is reflected in the fact that it was only in late 1979, after Argos was operational, that the Argos Operations Committee received approval from the World Administrative Radio Conference to use the Argos frequency for animal tracking.⁴¹ In January 1980, Argos announced that NOAA's experimental marine mammal research program had been allocated platforms for the coming year, becoming the first wildlife project to be accepted.⁴² But the desire to ensure high precision and reliability for the system as a whole, even when strict standards might be inappropriate for a particular application, placed serious constraints on animal tracking. For example, Service Argos, following NASA standard practices, required all Argos tags to be certified in Toulouse before they were deployed. Any PTTs that emitted less than one watt were rejected, even though wildlife biologists were willing to accept a drop-off in performance in exchange for a lighter package and a longer battery life. Similarly, in order to maintain high system-wide levels of precision, Argos refused to distribute location estimates from satellite passes in which fewer than four messages had been received, even though it was technically possible to calculate a rough location estimate from just two messages. For wildlife biologists studying animals such as whales that surfaced only intermittently (thus decreasing the number of messages likely to be received during a single satellite pass) and traveled thousands of miles (thus decreasing the need for accuracy), such constraints were deeply frustrating.⁴³

41 "Use of the Argos System for Animal Tracking Programs," *Argos Newsletter* 9 (1980): 7.

42 "Admissions," *Argos Newsletter* 7 (January 1980): 3.

43 On the social construction of accuracy in the context of missile guidance, see Donald A. Mackenzie, *Inventing Accuracy: A Historical Sociology of Nuclear Missile Guidance* (Cambridge, MA: MIT Press, 1990).

Nonetheless, wildlife biologists who had begun experimenting with IRLS or RAMS in the late 1970s quickly shifted their focus to Argos.⁴⁴ Although the number of PTTs expected to be deployed for animal tracking were insignificant in compared to those used for meteorology and oceanography, Service Argos was eager to increase the total number of users in order to justify extending the life of the system. Wildlife biologists were encouraged to attend the Argos user conferences, and biological applications were prominently featured in the Argos newsletter. Still, the number of wildlife biologists using the system was tiny. At a user conference in Quebec in 1980, one Service Argos representative reported an estimated demand of seven PTTs for animals in 1980, 19 in 1981, and 36 in 1982; candidates under discussion for tracking included dolphins, basking sharks, tortoises, wild boars, kangaroos, migrating birds, and bears.⁴⁵ In 1981, the Argos newsletter focused on animal tracking, with a picture of Jacqueline Jennings of the National Marine Fisheries Services and a tagged dolphin on the cover—although, tellingly, the tag in question had been designed to work with the Nimbus RAMS system, since permission to use Argos had only been granted in 1980.⁴⁶ The same issue of the newsletter included information about efforts to track leatherback sea turtles by Michele Duron of the La Rochelle Museum of Natural History, basking sharks by I.G. Priede of the University of Aberdeen, and loggerhead turtles by Robert Timko of NMFS, who was using the same basic electronics package that had been used on polar bears by Lentfer and his colleagues.⁴⁷ At this point the use of Argos for

44 Jacqueline G. Jennings, "The Use of Nimbus-6 and Argos Satellites to Track Dolphins," in *Data Collection and Location by Satellite: User Conference, Quebec, Canada, October 1980* (Toulouse: Service Argos, 1981), unpaginated (9pp). Jennings attended a user conference in Toulouse in March 1979 and another in Washington in September 1979; "Argos Data Processing Conference," *Argos Newsletter* 5 (May 1979): 4-5.

45 Jean-Luc Bessis, "Le System Argos apres deux ans de fonctionnement," in *Data Collection and Location by Satellite: User's Conference, October 1-2, 1980, Quebec, Canada* (Toulouse: Service Argos, 1980): unpaginated.

46 Jacqueline J. Jennings, "Suivi d'animaux/Animal tracking," *Argos Newsletter* 11 (April 1981): 1-2.

47 Michèle Duron, "Tortues luths/Leathery Turtles," *Argos Newsletter* 11 (April 1981): 2-4; I.G. Priede, "Suivi des requins pèlerins/Tracking of Basking Sharks," *Argos Newsletter* 11 (April 1981): 4-5; Robert Timko, "Suivi d'une tortue marine/Sea Turtle Tracking," *Argos Newsletter* 11 (April 1981): 5-6.

animal tracking remained aspirational. When the Argos newsletter again focused on animal tracking in 1984, there was no more discussion of RAMS, but progress remained slow. Bruce Mate of Oregon State University—who advised Erich Follman on his ill-fated proposal to radio-tag bowhead whales in the late 1970s—reported that a collaboration with Telonics and with the National Center for Atmospheric Research had produced an Argos tag suitable for use on whales, which had successfully been deployed for six days in July 1983 on a humpback whale off Newfoundland and would soon be tested on gray whales Baja California.⁴⁸ Priede reported that a basking shark had successfully been tracked using an Argos tag in June 1982, and Duron and her colleagues reported testing attachment techniques for sea turtles, though no data had yet been collected. This issue also included one of the first published reports of efforts to develop an Argos PTT light enough for birds by John French, a engineer working with Priede's group at Aberdeen.⁴⁹

In the mid-1980s, Service Argos underwent a major reorganization that indirectly helped open up the system further to animal tracking. A renegotiation of the memorandum of understanding between NASA, CNES, and NOAA took place beginning in 1985 and culminated in an agreement to extend the service into the 1990s. At the same time, Service Argos was transformed from a division of CNES into a state-owned *société anonyme* and renamed Collecte Localisation Satellites. While technically still under the control of CNES, its management, under the leadership of president Michel Cazenave, had a great deal more autonomy than before.⁵⁰ It also established a new U.S. subsidiary near Washington under Jean-Luc Bessis,⁵¹ who declared

48 Bruce Mate, “Suivi de baleines/Tracking of Whales,” *Argos Newsletter* 19 (April 1984): 1-3.

49 John French, “Suivi d'oiseaux/Tracking Birds,” *Argos Newsletter* 19 (April 1984): 11-12.

50 “C.L.S. and Its President,” *Argos Newsletter* 26 (June 1986): 13; “Service Argos, Inc.,” *Argos Newsletter* 31 (September 1987): 1-2.

51 “Back Home ...,” *Argos Newsletter* 26 (June 1986): 13.

that the opening of the office “is confirming its role as a worldwide, operational system,”⁵² and established of a second processing center in Landover, MD, to complement the one in Toulouse.⁵³ In addition to the new office and processing facility in Maryland, CLS also opened regional offices in Seattle and Melbourne, beginning its expansion from a solely French company to a truly global entity.⁵⁴ CLS also made moves to satisfy the demands of specialized user groups, including wildlife biologists. As an article in the December 1987 issue of the Argos newsletter noted, the use of the system for animal tracking had undergone “extremely rapid growth” in the previous three years. To facilitate this interest, CLS established a new “Wildlife Service” beginning on November 1987 that provided location data with as little as two messages during a single satellite pass. Noting that European and African users were interested in purchasing “turnkey” packages that included both service and hardware, CLS also entered into a short-lived partnership with an American PTT manufacturer.⁵⁵ The result of these changes in system policies, together with the development of wildlife-appropriate Argos tags, was an explosion in the use of Argos for animal tracking in the late 1980s.

The Bird-Borne Transmitter

In some ways, the design of satellite tags for birds merely re-posed the same technical challenges faced by developers of conventional radio-tracking such as William Marshall or William Cochran in the early 1960s; it was as if the advances in miniaturization that had taken place in the previous two or three decades had simply been undone. But the difficulties were also

52 Jean-Luc Bessis, “The Turning Point,” *Argos Newsletter* 27 (September 1986): 1.

53 “Service Argos Inc.,” *Argos Newsletter* 31 (September 1987): 1-3.

54 Michel Taillade, “Argos in 1988: Decentralization and Closer Links With Users,” *Argos Newsletter* 32 (December 1987): 1-2.

55 Philippe Courrouyan, “Wildlife,” *Argos Newsletter* 32 (December 1987): 13-15.

qualitative. Conventional radio-tracking was technologically simple, an artifact of the radio age rather than of the information age. Even though its success had depended on such novel technologies as the transistor, on Cochran's ingeniously simple circuit design, and on an extended process of trial and error around attachment methods, the basic technology was something that a biologist could feasibly reproduce in his basement workshop. And it required little or no standardization. Satellite tracking, in contrast, was a complex system that depended on conformation with rigid standards that had not been established with wildlife tracking in mind.

There was also a subtler challenge. The trouble-shooting process for conventional radio-telemetry had depended on the development of an intimate sense of the landscape, both for understanding the behavior of radio signals and for understanding the kinds of animal behaviors and environmental challenges that might lead to the failure of a tag in the field.⁵⁶ For the most part, satellite tracking removed the need for an intimate understanding of the radio landscape on the part of the scientist, but it also made it extremely difficult to understand why a tag had failed. It expanded the spatial scale of tracking from the several hundred acres of the foot-based radio-tracker to the several-hundred-square miles of the car- or airplane-equipped tracker to the scale of continents, ocean basins, and even the globe. If the signal from a swan that had been tagged in Alaska was lost somewhere over the Northern Rockies on its way to the Chesapeake Bay, what had gone wrong? Had the battery or the electronics failed? Had the swan died? If the latter, was the tag somehow to blame?

Because of these challenges, only a few of the pioneers of conventional radio-tracking successfully made the transition to manufacturing Argos PTTs. William Cochran focused on building extremely miniaturized conventional radio-tags to track the migration of smaller and

⁵⁶ See Chapter 1.

smaller birds, while Advanced Telemetry Systems, which emerged from the Cedar Creek Bioelectronics Laboratory in the mid-1980s, limited itself to repackaging Argos PTTs that had been manufactured elsewhere. One notable exception to this pattern was the Arizona-based company Telonics, one of the only manufacturers of conventional radio-tags to successfully make the transition to manufacturing the highly complex Argos PTTs. Rather than simply repackaging transmitters in forms appropriate for wildlife research, as ATS and other companies did, Telonics developed its Argos PTTs from the ground up. Although the Telonics principals—Dave Beatty and engineer Stanley Tomkiewicz—had not been involved in the formative phase of invention that produced wildlife radio-tagging between the late 1950s and mid-1960s, they had become a major provider of biotelemetry gear in the 1970s. In 1982, they began to focus on developing satellite tags, collaborating with Bruce Mate to develop Argos PTTs for whales and with the USFWS and the Alaska Department of Fish and Game for large land animals. Through its collaborations in Alaska with USFWS biologists such as Steven Fancy and Larry Pank, Telonics quickly became the dominant provider for satellite-tagging of large land animals. Telonics tags were first tested on caribou and muskoxen in 1984; by April 1987, the tag weight had been reduced to 1.2kg. This was still far too large to be used on birds or small mammals, but it was more than adequate for large mammals.⁵⁷ By 1990, the list of animals that had been tracked in Alaska and the Northern Rockies by USFWS using Telonics PTTs had expanded to include polar bears, caribou, muskoxen, brown bears, gray wolves, Pacific walrus, Dall sheep, elk and mule deer. In some cases, the studies were on a scale that dwarfed the kinds of demonstration experiments that the Craigheads, Lentfer, and others had conducted with IRLS

⁵⁷ Steve G. Fancy, Larry F. Pank, David C. Douglas, Catherine H. Curby, Gerald W. Garner, Steven C. Amstrup, and Wayne L. Regelin, *Satellite Telemetry: A New Tool for Wildlife Research and Management*, U.S. Fish and Wildlife Service Resource Publication 172 (Washington, DC: Department of Interior, 1988).

and RAMS in the 1970s and early 1980s. Between 1985 and 1988, for example, federal and Alaska state biologists deployed 109 Argos tags on polar bears.⁵⁸ Very conscious of the public relations aspects of instrumenting wildlife, however, Tomkiewicz warned biologists and engineers to move slowly. “At present,” he noted in the Argos newsletter in 1988, “satellite telemetry is riding a 'high' in terms of the public opinion we should make every attempt to assure that it retains this standing.”⁵⁹

While Telonics focused on large animals and urged caution in pushing the envelope, other groups tried to develop a tag that would realize the now several-decades-old dream of tracking birds by satellite. The early lead in bird-borne transmitters was taken by a partnership between USFWS biologists in the Chesapeake Bay area and engineers at the Johns Hopkins University's Applied Physics Laboratory, or APL.⁶⁰ Though APL worked almost entirely on military technologies, it had gotten briefly involved in wildlife telemetry in 1969 when Johns Hopkins ornithologist William J.L. Sladen hired two of its engineers to add an altimeter to one of Cochran's tags for a study on the migration of whistling swans, which the Air Force had sponsored in the hope of reducing bird strikes.⁶¹ In mid-1982, APL was approached about

58 All authors of the report worked at the Alaska Fish and Wildlife Research Center in Fairbanks except for Richard Harris, who worked at the Montana Cooperative Wildlife Research Unit; see Richard B. Harris, Steven G. Fancy, David C. Douglas, Gerald W. Garner, Steven C. Amstrup, Thomas R. McCabe, and Larry F. Pank, *Tracking Wildlife by Satellite: Current Systems and Performance*, Fish and Wildlife Technical Report 30 (Washington, DC: U.S. Fish and Wildlife Service, 1990), p2.

59 Stanley M. Tomkiewicz, Jr., “La Telemesure par satellite dans le suivi des animaux sauvages/Wildlife Satellite Telemetry,” *Argos Newsletter* 34 (July 1988): 6-11, on 11.

60 On APL's historical involvement with Doppler location, see Harold D. Black, “Satellites for Earth Surveying and Ocean Navigating,” *Johns Hopkins APL Technical Digest* 2 (1981): 3-13; Robert J. Danchik, “The Navy Navigation Satellite System (Transit),” *Johns Hopkins APL Technical Digest* 5 (1984): 323-329.

61 The APL engineers found the swan altimetry project more difficult than they anticipated. As they wrote, “The transmission of measurements from one point to another is the highly developed science of telemetry intended for use with aircraft and missiles. However, these telemetry systems were not developed with the swan project in mind. Here it was necessary to return to fundamentals and design for very low weight, long life, and simplicity”; W.A. Good and J.W. Hamblen, “How High Does the Whistling Swan Fly?” *APL Technical Digest* 10 (November-December 1970): 2-10, on 2. The engineers also reported that at some point during the study, “a frantic bird watcher called Dr. Sladen's headquarters and reported that one of the swans she was observing had an arrow sticking out of its back. Questioning revealed that it was one of the early style transmitters with one of the

wildlife telemetry again by two USFWS biologist: one of Sladen's former doctoral students, William Seegar, and former member of the University of Minnesota's Cedar Creek group, Mark Fuller. Seegar and Fuller hoped that APL could help them design a light-weight satellite tag for tracking swans, eagles, and other large birds. The APL engineers, led by Thomas Strikwerda, only learned about Argos after they began the project. In 1983, Paul Howey, an engineer who had previously developed a wildlife telemetry system at the University of Bath, was hired to work full-time on the bird-borne transmitter project. As the APL team developed the transmitter package, they tested it on an Andean condor and golden eagle held at the nearby Patuxent Wildlife Research Center, the USFWS's headquarters for endangered species research.⁶² The transmitter was certified by Service ARGOS in Toulouse in October 1983; after the addition of a solar panel and attachment materials, it weighed less than 170 grams.⁶³

The first test on a non-captive wild animal took place in May 1984 using a mute swan in a wildlife refuge on the Eastern Shore of the Chesapeake, but the first real attempt to track migration took place in 1985, when the tags were attached to trumpeter and tundra swans in Alaska and a bald eagle on the Chesapeake Bay.⁶⁴ The lack of success with these first tests led the team to seek out better candidates, which they defined as “easy to catch,” “easy to catch a second time,” and large enough to carry the transmitter easily. These criteria turned their attention to the large, colony-breeding sea-birds. In early January 1985, with the support of

whip antennas. She was advised not to pull the 'arrow' out”; Good and Hamblin, “How High Does the Whistling Swan Fly?” 5. William Cochran repeated this story to me in an interview.

62 Andean condors were being held at Patuxent in order to test techniques that would be used on the similar but far more endangered California condor; see Peter S. Alagona, “Biography of A 'Feathered Pig': The California Condor Conservation Controversy,” *Journal of the History of Biology* 37 (2004): 557-583; Noel F. R. Snyder, and Helen Snyder, *The California Condor: A Saga of Natural History and Conservation* (San Diego: Academic Press, 2000).

63 Thomas E. Strikwerda, Harold D. Black, Nadav Levanon, and Paul W. Howey, “The Bird-Borne Transmitter,” *Johns Hopkins APL Technical Digest* 6 (1985): 60-67.

64 Sue Anne Pressley, “Inventions Helping to Save Eagles,” *Washington Post*, 11 April 1985.

Cedar Creek ecologist David Parmelee, they attached the tags to several Southern giant petrels near Palmer Station in Antarctica. A report in the April-June 1986 issue of the APL newsletter, the cover of which featured a photo of the captive golden eagle on which they had initially tested the tag, included the triumphant claim that the team had “pioneered bird tracking by satellite.” But the test was at best a qualified success. Though some data was collected by the satellite, none of the five petrels returned to the site where they had been tagged, suggesting that the impact of the tags on the birds' survival was, in the understated terms of the report, “appreciable.”⁶⁵ For the moment, bird tracking by satellite remained out of reach.⁶⁶

While Americans had dominated the first two decades of development of conventional radio-tracking, satellite animal tracking was from the beginning a far more international affair. Since the late 1970s, Priede's group at the University of Aberdeen had been developing at first Nimbus RAMS and then Argos tags for use on marine animals, specifically the basking shark, an animal weighing as much as 7 tons which feeds on plankton near the ocean surface and is thus one of the few pelagic fish for which a radio tag attached to a tethered float is suitable. In the early 1980s, John French began collaborating with Priede's group to develop a satellite transmitter for birds. In 1988, Priede and French announced that their 145g tag for birds, the Mariner Type 15S, with a predicted battery life of 35 days, was now commercially available after having been testing on whooper swans and mute swans in Scotland.⁶⁷ Around the same time a

65 The report further noted that “[s]atellite transmitters are far more complex and therefore less reliable than conventional tracking transmitters; considerable effort must go into weight reduction and reliability engineering. In addition, thorough testing on various bird species is required to finalize package and harness design in order to minimize effects on bird behavior and prolong survival”; Thomas E. Strikwerda, Mark R. Fuller, William S. Seegar, Paul W. Howey, and Harold D. Black, “Bird-Borne Satellite Transmitter and Location Program,” *Johns Hopkins APL Technical Digest* 7 (1986): 203-208, on 207.

66 Fuller, Seegar, and the APL team persisted, however, and eventually developed a successful bird tag, on the basis of which Paul Howey founded a company called Microwave Telemetry in 1991. Microwave Telemetry's first product was a 95g Argos PTT for bird-tracking; see <http://www.microwavetelemetry.com/>

67 I.G. Priede, “Suivi d'oiseaux/Tracking Birds,” *Argos Newsletter* 34 (July 1988): 1-3; John French, “Améliorations dans l'autonomie des balises miniatures/Power Budget Improvements for Miniature PTT's,” *Argos*

Japanese company called Toyo Communications Equipment Company, or Toyocom, joined the competition over bird PTTs with a commercially available tag weighing as little as 150g, about the same size as those that Howey and French were building.⁶⁸ Toyocom had been involved in animal tracking for the purposes of tracking dolphins and other marine animals in collaboration with Tokai University and the Japanese Ministry of Agriculture, Forestry, and Fisheries.⁶⁹ In 1983 and 1985, Japanese researchers had unsuccessfully attempted to track penguins in the Antarctic. By 1989, they were also involved in tracking fur seals and loggerhead turtles in the North Pacific.⁷⁰ The new miniature Argos PTT was the first, however, that researchers outside of Japan began to use.

Optimal Foragers

As the APL team and their USFWS partners recognized in choosing the Southern giant petrel as a subject of study, and as Warner and Cochran had recognized two decades earlier, the large colony-breeding sea birds of the southern hemisphere provided attractive targets for satellite tracking. Because they returned regularly to their breeding colonies, they were easy to catch and easy to catch a second time. They were also large, strong flyers capable of carrying heavy tags. And the high expense of a satellite-tracking study could be scientifically justified since little was known about their behavior away from the breeding colonies. The problem with choosing giant petrels was that they were not quite “giant” enough. In 1989, two French

Newsletter 34 (July 1988): 12-13.

68 The tag was certified by CLS in October 1987; “Certification—News in Brief,” *Argos Newsletter* 32 (December 1987): 11-12.

69 Makasi Soma and Masayuki Tsustumi, “Suivi d’animaux par satellite/Biological Telemetry Utilizing the Argos System,” *Argos Newsletter* 34 (July 1988): 14-17.

70 Yasuhiko Shimadzu, “Suivi d’animaux au Japon/Animal Tracking in Japan,” *Argos Newsletter* 38 (November 1989): 1-3; Yasuhito Naito, “Suivi de tortues de mer au Japon/Sea Turtle Tracking in Japan,” *Argos Newsletter* 38 (November 1989): 2-5.

researchers working on the Crozet Islands in the Southern Indian Ocean achieved the first major success in satellite tracking of birds with a study of wandering albatrosses, which weigh up to 11kg and have a wingspan of up to 3.5m.⁷¹ Henri Weimerskirch and his advisor Pierre Jouventin, evolutionary biologists associated with the Centre d'Etudes Biologiques des Animaux Sauvages (CEBAS) at the Chizé Forest near Beauvoir, France, also had the advantage of long experience working with the birds.⁷² As they noted in a publication based on Weimerskirch's doctoral thesis in 1987, albatrosses “are amongst the easiest birds to census, catch, mark and recapture; they are large, breed in accessible groups often numbering some thousands of individuals, and are quite fearless of humans,” characteristics that made them “ideal subjects for studies in demography”—as well as, it would turn out, for satellite tracking. Weimerskirch's dissertation on the relationship between the distribution of marine resources and the life-history characteristics (such as lifespan and birth rate) of albatrosses called for better information about the birds' foraging trips.⁷³ Weimerskirch heard about Toyocom's miniaturized PTT in the context of dolphin tracking, most likely in the July 1988 issue of the Argos newsletter, which featured an article on dolphin tracking in Japan.⁷⁴ Between January and March 1989, Weimerskirch and his team tagged

71 The Crozet Islands are part of the Terres Australes et Antarctiques Françaises (TAAF), a French administrative unit comprising a scattering of remote islands in the southern Indian Ocean and a triangular slice of the Antarctic continent that is also claimed by Australia (see <http://www.taaf.fr/>).

72 Henri Weimerskirch, “Ecologie Comparée des Albatros des Terres Australes Françaises,” PhD Thesis, Academie de Montpellier, Université des Science et Techniques du Languedoc, 10 March 1986.

73 H. Weimerskirch, J. Cloubert, and P. Jouventin, “Survival in Five Southern Albatrosses and its Relationship with Their Life History,” *Journal of Animal Ecology* 56 (1987): 1043-1055.

74 Weimerskirch and Jouventin's accomplishment attracted the attention of the press; in the United States, many local newspapers reprinted either the Reuters wire report or Walter Sullivan's subsequent *New York Times* piece. Both articles emphasized the potential usefulness of the study for conservation and the fact that the researchers were French. The Reuters wire described Weimerskirch and Jouventin as “conservationists” rather than as scientists. In contrast, Sullivan's article emphasized the technological and scientific aspects of the study and made no mention of its implications for conservation; “Satellite Study Sheds New Light on the Powers of the Albatross,” *Reuters News*, 22 February 1990; Walter Sullivan, “Albatross Wanders Far Afield,” *New York Times*, 20 March 1990. Neither article provided historical context, although a popular article published in the *Independent* late in 1991 by a member of the Science and Technology Studies Group at Imperial College, London, placed the satellite study in the context of the grand evolution of animal identification technologies from the seventeenth century to the present. The key advantage of radio-tracking over older techniques, Russell

several albatrosses on Possession Island, one of the Crozets, and tracked the birds through their foraging trips and safely back to their nests.

In the decade after the paper resulting from this study was published in *Nature* in 1990, there was an explosion of satellite-tracking studies of albatrosses, both by Weimerskirch and his colleagues and by a number of other research groups, some collaborating closely with the French group and some working independently. The major result reported in the *Nature* article, which was coauthored by Jouventin and Weimerskirch, was simply the fact of having successfully gathered data on foraging journeys by satellite, although the authors also tried to relate the speed and direction of albatross flight to environmental factors such as daylight and wind conditions. The authors noted that “one of the most stimulating prospects that satellite tracking holds is to determine how foraging strategy can be optimized when weather conditions have such a large influence on the movements of great albatrosses.”⁷⁵ The key terms here were “foraging strategy” and “optimized,” which placed the research program squarely in the tradition of optimal foraging theory. Argos made it possible to extend the language of rational choice to albatross foraging. Satellite-tagging studies of albatrosses on foraging journeys emphasized the individual decision-maker, optimizing his or her energy efficiency and evolutionary fitness in the context of an unpredictable environment in which resources were sparsely distributed and often coincided with threats such as longline fisheries.

argued, was that it allowed “continuous monitoring”; Nicholas Russell, “Satellites Track Animal Migrations,” *Independent* (London), 21 October 1991.

75 Pierre Jouventin and Henri Weimerskirch, “Satellite Tracking of Wandering Albatrosses,” *Nature* 343 (1990): 746-748, on 747.

The Limits of Enclosure

Starting with Jouventin and Weimerskirch's initial *Nature* article in 1990, satellite tracking of albatrosses was closely linked to conservation. Although all major albatross breeding colonies were protected, populations of certain species continued to decline, and it was hoped that satellite tracking might provide an explanation. As Jouventin and Weimerskirch wrote, “Detailed knowledge of their movements at sea may prove critical to the conservation of the Wandering albatross and particularly of the closely related Amsterdam albatross.”⁷⁶ The nature of threats to albatrosses was clarified in 1991—without help from satellite tags—when the Tasmanian ornithologist Nigel Brothers published an influential article showing that many albatrosses were dying as bycatch in longline fisheries.⁷⁷ The use of longlines had grown dramatically after driftnets were discouraged (and eventually banned) in order to protect dolphins, sea turtles, albatrosses, and other pelagic species. But these many-mile-long fishing lines with baited hooks also posed a threat to albatrosses, which became caught on the hooks when they tried to steal the bait before the lines sank. By the early 1990s, almost all reports of satellite tagging of albatrosses mentioned the hope that insights into albatross foraging behavior might help reduce the bycatch problem.⁷⁸

76 Pierre Jouventin and Henri Weimerskirch, “Satellite Tracking of Wandering Albatrosses,” *Nature* 343 (1990): 746-748, on 746. In a description of the study published in the *Argos Newsletter*, Weimerskirch wrote that previous attempts to track birds by satellite “had failed because the platforms were too heavy, the birds too small or wild, or because the techniques used to fit the PTT to the animal were unsatisfactory. All who had attempted tagging birds w/ relatively heavy PTTs had failed, and our albatross project was cause for much initial skepticism”; Henri Weimerskirch, “Suivi d'albatros dans l'océan austral/Albatross Tracking in the Southern Ocean,” *Argos Newsletter* 40 (August 1990): 1-5, on 1. He also linked the use of satellite tags to the policing of “the remotest and most hostile areas of the planet, which had long been a refuge and sanctuary for certain species” but were now under attack. Satellite tagging offered one answer to the question that Weimerskirch then posed with respect to regulations meant to protect these areas: “But how could such regulations be policed in the open sea, outside economic zones?”; Weimerskirch, “Albatross Tracking in the Southern Ocean,” 5.

77 Nigel Brothers, “Albatross Mortality and Associated Bait Loss in the Japanese Longline Fishery in the Southern Ocean,” *Biological Conservation* 55 (1991): 255-268.

78 One of the first articles to link explicitly the use satellite tags to study albatrosses and the problem of fisheries bycatch emerged from researchers affiliated with the British Antarctic Survey; P.A. Prince, A.G. Wood, T. Barton, and J.P. Croxall, “Satellite Tracking of Wandering Albatrosses (*Diomedea Exulans*) in the South

In particular, researchers argued that information about overlap of albatross feeding and fisheries might allow regulators to prohibit longline fishing in certain areas of the ocean at certain times of the year, thus allowing both birds and fishing industries to thrive.⁷⁹ By the early 2000s, however, the limits of this proposed strategy of temporal and spatial protection were becoming clear. First, satellite tracking had demonstrated that albatrosses ranged so widely that any closure that would significantly protect them would also shut down the fisheries completely. In 2003, when a Global Procelliform Tracking Workshop was convened in Gordon's Bay, South Africa, to aggregate all of the satellite tracking data that had been collected for albatrosses and petrels since Jouventin and Weimerskirch's 1989 study, the resulting global map showed few areas in the Southern Hemisphere where albatrosses did not wander.⁸⁰ Second, enforcing such closures would be nearly impossible. Indeed, existing regulations and fishing quotas that had the full weight of economic interest behind them could not be enforced. Even as legitimate fishermen began to deploy mitigation devices to reduce albatross bycatch—streamers that frightened away the birds or weighted lines that sank so quickly that the birds were unable to reach the bait—so-called “pirate fisheries” were decimating both albatrosses and stocks of valuable fish such as the Patagonian toothfish. States such as Australia could propose monitoring foreign fishing vessels with satellite tags and remote sensing systems, but even if a pirate fishing boat was detected, the enormous extent and harsh environmental conditions of the Southern

Atlantic,” *Antarctic Science* 4 (1992): 31-36. This link quickly became a feature of coverage in the popular press as well; for example, in the following article on the work of David Nicholls's tagging efforts: Andrew Darby, “Mrs Gibson Goes Too Far for Albatross Protectors,” *Sydney Morning Herald*, 30 August 1995.

79 For examples of this argument, see Henri Weimerskirch and Rory P. Wilson, “Oceanic Respite for Wandering Albatrosses,” *Nature* 406 (2000), 955-956; Henry Fountain, “Discovering Early Clues to Where and Albatross Wanders on Its Year Off,” *New York Times*, 5 September 2000.

80 BirdLife International, *Tracking Ocean Wanderers: The Global Distribution of Albatrosses and Petrels: Results From the Global Procellariiform Tracking Workshop, 1-5 September 2003, Gordon's Bay, South Africa* (Cambridge, UK: BirdLife International, 2004).

Ocean made it difficult to intervene.⁸¹ Tension in the fisheries around Antarctica had grown so intense by 1997—particularly between Australia and the Korean and Taiwanese fishing industries, which were perceived to be the most egregious violators of international fishing agreements—that the language of “war” became common in press coverage.⁸²

In the context of this broader controversy, albatrosses were the subject smaller agreements forged at a series of meetings over the course of the late 1990s. Some of them were devotedly specifically to albatrosses, while others—such as meetings of the Commission for the Conservation of Antarctic Marine Living Resources—focused on a much broader range of issues.⁸³ As it had in the science of albatross tracking, Australia took the lead in pushing for conservation measures with the support of environment minister Robert Hill.⁸⁴ In 1999, the U.N. Food and Agriculture Organization adopted an “International Plan of Action for Reducing Incidental Catch of Seabirds in Longline Fisheries.”⁸⁵ Starting in 1999, the Agreement on the Conservation of Albatrosses and Petrels, or ACAP, was developed within the framework of the Convention on the Conservation of Migratory Species of Wild Animals. ACAP was opened for signatures in July 2001 and went into force in February 2004 after being ratified by a sufficient number of signatories. ACAP did not set aside any sanctuaries in time or space; instead, it called for management and protection throughout the albatross's extensive range. It also stipulated that parties to the agreement “shall seek to make local communities and the public in general more aware of the status of albatrosses and petrels and the threats facing them.”⁸⁶ This was something

81 James Woodford, “Satellite Imaging to Counter Illegal Fishing,” *Sydney Morning Herald*, 4 March 1997.

82 Andrew Darby, “War in a Cold Climate,” *Sydney Morning Herald*, 10 June 1997.

83 Don Woolford, “Antarctic Nations Meet to Protect Region,” *Australian Associated Press*, 24 October 1998; Tom Allard, “Icy Reception a Pleasure for Hill at Antarctic Talks,” *Sydney Morning Herald*, 26 January 1999.

84 Alan Thornhill, “Australia to Press for Tighter Controls on Toothfish Plunder,” *Associated Press Newswires*, 19 January 1999.

85 “Global Plan to Help Save Albatrosses,” *Australian Associated Press*, 15 July 2000.

86 The website for the Agreement on the Conservation of Albatrosses and Petrels is: <http://www.acap.aq>; the full

that several albatross researchers had already begun to do in innovative ways that took advantage of the growth of the World Wide Web in the late 1990s.

Animal Protagonists in Global Narratives

As biologists and conservationists met in technical meetings to share data and hash out political agreements, international conservation organizations began to single out the albatross for special attention, not only for its own sake but also as a symbol of the threats to the oceans. While older symbols such as the whale and the dolphin continued to motivate much conservation activism, the albatross became a new mascot for global ocean conservation.⁸⁷ Prompted by resolutions by the World Conservation Union/IUCN, the World Wildlife Fund, and other major environmental organizations, newspapers such as the *New York Times* devoted significant coverage to the threat of long-line fishing to albatrosses and depleted fish stocks.⁸⁸ In 1997, BirdLife International launched its Global Seabird Conservation Programme, and Greenpeace began a campaign to close the southern bluefin tuna fishery. Documentary films also helped to position the albatross as a new symbol of ocean conservation; in 1993, for example, a documentary on the albatross research of Weimerskirch and his team on Crozet Island was shown on Australian television.⁸⁹

The explosive growth of the World Wide Web over the course of the 1990s provided an alternative medium through which to portray individual animals as protagonists of environment

text of ACAP as amended in 2006 is available at:

http://www.acap.aq/en/images/Core_Documents/Final_agreement_Amended%20MoP2_2006_English.pdf

87 Dick Russell, "Vacuuming the Seas," *E Magazine* (July/August 1996): 28-34.

88 William K. Stevens, "Long Line Fishing Seen as Damaging to Some Fish and to the Albatross," *New York Times*, 5 November 1996.

89 David Parer and Elizabeth Parer-Cook's "Mysteries of the Ocean Wanderers" was broadcast as part of the Australian Broadcasting Corporation's "Wildscreen" series in November 1993; Hugo Kelly, "Ingenuity Helps Save Albatross," *The Age*, 3 November 1993; Barbara Hooks, "Wild Thing," *The Age*, 30 September 1993.

dramas. In the fall of 1996, David Anderson of Wake Forest University in Winston-Salem, North Carolina, received a grant of \$200,000 from the National Science Foundation's Division of Environmental Biology—the same division that had funded William Marshall's Grouse study in the early 1960s—to conduct a satellite-tracking study of Laysan and black-footed albatrosses in the North Pacific. The study's key innovation was that, as the award abstract put it, the study data would be “distributed in near-real time to over 39,000 fifth and sixth grad students via email and educational satellite networks. These students will participate in the data analysis, to increase their scientific literacy and to attract them to scientific careers.”⁹⁰ (By June 1998, when the Albatross Project was the subject of an article in *USA Today*, Anderson estimated that 10,000 students had participated.⁹¹ The total number of students that participated over the 3-year life of the project through email updates or the project's web site was perhaps 20,000.) Anderson's proposal fit into broader efforts at the National Science Foundation to integrate scientific research and education, but it also directly addressed a pressing conservation issue. Anderson's team tagged its first birds in late January 1998 on Tern Island, an atoll located in the northwestern end of the Hawaiian chain.⁹² The Albatross Project was targeted at schoolchildren, but it was also represented in the media as a fun, educational activity in which any member of the public could participate. An article in the San Diego Union-Tribune in February 1998 invited “readers and nonscientists” to “join the action” by visiting the website or subscribing to email updates.⁹³

90 National Science Foundation, “Award Abstract #9629539: CAREER: Evolutionary Ecology of Avian Reproduction,” <http://www.nsf.gov/awardsearch/showAward.do?AwardNumber=9629539>, retrieved on 22 January 2008.

91 Mark Glassman, “Soar Point: Decline of the Albatross,” *USA Today*, 3 June 1998.

92 Kelly McCollum, “Web Site Lets Thousands of Schoolchildren Follow Research on Albatrosses,” *Chronicle of Higher Education*, 13 February 1998.

93 Scott Lafee, “Public Has Opportunity to Help Science Take Flight,” *San Diego Union-Tribune*, 18 February 1998.

Anderson's website was only one of many to make satellite-tagging data available to the public in the late 1990s. Another NSF-funded program called WhaleNet was launched in 1994, several years before the Albatross Project began, with a focus on radio-tagged whales, seals, porpoises, and sea turtles under the supervision of J. Michael Williamson at Wheelock College.⁹⁴ In September 1998, *Audubon* magazine published a list of web sites where its readers could “get involved in tracking animal movements,” including the Albatross Project as well as others focused on sea turtles, marine mammals, mallards, elephants, cranes, eagles, manatees, and geese.⁹⁵ Also in 1998, CLS published a special issue of the *Argos* newsletter focused on the World Wide Web as a mechanism for distributing data to scientists and sharing results with the public, which included a feature article on a the website “Satellite Tracking of Threatened Species,” a collaboration between Goddard Space Flight Center and the Patuxent Wildlife Research Center that had been online for two years.⁹⁶ In 2000, NASA launched a website and associated educational program called “Signals of Spring,” which, like the Albatross Project, focused on engaging schoolchildren with the results of wildlife satellite tracking.⁹⁷ Having hesitated to support the technology in the early 1970s, when the risks of handling large, symbolic animals were made apparent by the Monique fiasco, NASA now embraced it as a means of demonstrating its continuing relevance and ability to engage the imagination of children.

Satellite tagging made it possible to bring the day-to-day of individual animals, even those that

94 National Science Foundation, “Real Science for Younger Scientists,” *Discoveries*, 8 November 2004, http://www.nsf.gov/discoveries/disc_summ.jsp?cntn_id=100732, retrieved on 22 January 2008. On WhaleNet, see Andrew Blake, “Saving the Whales in Cyberspace,” *Boston Globe*, 16 October 1994; Marie C. Franklin, “Environment Network Brings Life to Science,” *Boston Globe*, 13 November 1994.

95 Wendee Holtcamp, “Tracking by Internet,” *Audubon* 100 (1 September 1998): 57.

96 Margaret Williams, Allen Lunsford, David Ellis, Jon Robinson, Patrick Coronado, and William Campbell, “Satellite Tracking of Threatened Species,” *Argos Newsletter* 53 (August 1998), http://www.cls.fr/html/argos/documents/newsletter/nslan53/gsf_en.html, retrieved on 8 March 2008.

97 Signals of Spring is available at the following address: <http://www.signalsofspring.net/>; see also Meghan Marrero and Glen Schuster, “Signals of Spring Brings ARGOS Data to Kids,” *Argos Forum* 61 (October 2004): 14-17.

spent years soaring over or swimming under the ocean, into classrooms, and thereby to promote identification and sympathy.

This strategy became perhaps most explicit in conservationist Carl Safina's 2002 book, *Eye of the Albatross: Visions of Hope and Survival*, which intertwines a fictionalized account of the life of a real satellite-tagged Laysan albatross named Amelia with historical and contemporary narratives about marine science and conservation.⁹⁸ One of Safina's key scientific interlocutors for the book was Dave Anderson, whose Albatross Project had brought the movements of individual albatrosses to tens of thousands of schoolchildren.⁹⁹ As its title suggests, *Eye of the Albatross* rested heavily on the conceit that satellite tags, “the biggest advance in seabird tracking in over 150 years,” allowed humans to see the world through nonhuman eyes.¹⁰⁰ After studying albatrosses, Safina wrote, “I began not just seeing the world albatrosses encounter, but—in a subtle shift of perception—seeing the world as an albatross encounters it.”¹⁰¹ In Safina's account, the scientific practice of capturing, tagging, and tracking a wild animal was transformed so that the satellite-tagged albatross was the agent and the scientist its partner or even passive subject: “I let Amelia draw me a map of her world,” he wrote.¹⁰² Technology, too, gained a new kind of agency; after the albatrosses were tagged, Safina wrote, “most of the data accrual will happen almost passively as technology takes over.”¹⁰³ The role of the scientist in Safina's account was not to control or to dominate, nor even, really, to defend

98 Carl Safina, *Eye of the Albatross: Visions of Hope and Survival* (New York: Henry Holt, 2002). In a follow-up volume, Safina took a similar approach to sea turtles, another group of species whose global journeys have been elucidated by the use of satellite tags; Carl Safina, *Voyage of the Turtle: In Pursuit of the Earth's Last Dinosaur* (New York: Holt, 2006). See also Carl Safina, *Song for the Blue Ocean: Encounters Along the World's Coasts and beneath the Seas* (New York: Henry Holt, 1998).

99 Safina, *Eye of the Albatross*, xv.

100 *Ibid.*, 38.

101 *Ibid.*, xiii.

102 *Ibid.*, xiii.

103 Safina, *Eye of the Albatross*, 52.

nature; it was “to draw out what the animals cannot tell us. To give words to the wordless, and voice to the voiceless, so that we can try to reach the ones among *us* who have so far been beyond words.”¹⁰⁴

Perhaps the most creative combination of wildlife satellite tracking and the Internet was launched in the spring of 2004 at the instigation of Tim Nevard, an Australian conservationist looking for business-friendly ways to raise money and awareness for environmental issues. Nevard brokered an agreement between Ladbrokes, a United Kingdom-based bookmaker with one million registered online customers in 160 countries as of January 2004, and the Tasmanian conservation agency to conduct a fund-raising stunt called the “Big Bird Race” (or “Ultimate Flutter”). The idea was that “punters” would be able to place bets on satellite-tagged Tasmanian shy albatrosses migrating from Tasmania to South Africa as if they were horses in a race. Winners would take home their winnings, but any profits would go toward albatross research. In its publicity for the project Ladbrokes pushed the analogy with a horse-race. The albatrosses were “horses,” their celebrity sponsors were “owners,” the scientists were “trainers,” the breeding colonies were “stables,” the satellite tags were “jockeys,” the web-based map on which punters could follow the albatrosses' progress was the “grandstand,” and the journey from Tasmania to South Africa was “one of the world's longest steeplechases.” The Big Bird Race was designed to serve multiple purposes: it would provide funding for Tasmanian researcher Rosemary Gales and her colleagues to study albatrosses; it would raise public consciousness about the threat of longline fisheries to albatrosses and the need for ratification of ACAP; it would provide entertainment for the players and cash for a few lucky winners, as well as,

104 Safina, *Eye of the Albatross*, 97-98..

perhaps, a means of making virtue out of vice; and, more certainly, it would help Ladbrokes build an image as a company with an environmental conscience.¹⁰⁵

One of differences between animal tracking by satellite and conventional radio-tracking was the ability of the former to efface the mediation of scientists—to make it seem as if each satellite-tagged animal were inscribing its own track onto a map of the world. Radio-tagged animals had been described as partners or allies in research since the beginning of conventional radio-tracking, but the ability of satellites to automate the data-collection process gave the metaphor renewed force. As early as the 1970s, scientists had suggested that wild animals could be used as “mobile sensor platforms” to gather environmental data that would otherwise be prohibitively expensive or impossible to gather.¹⁰⁶ In the late 1990s, this kind of language proliferated. Barbara Block of Stanford University's Hopkins Research Station became one of the most prominent practitioners of this rhetorical move. Block's research focused on tuna, an economically valuable animal whose movements were difficult to track because of the inability of radio signals to penetrate salt water and the species' ocean-spanning journeys. To solve this problem, Block partnered with engineers to develop tags that would record light levels, salinity, pressure, and other environmental parameters for months or years before detaching from the fish and floating to the surface, where they would transmit the recorded data to a satellite. These so-

105 Ladbrokes, Press Release, “Big Bird Race to Highlight Plight of the Albatross,” 22 January 2004.

106 Writing in 1972 in the context of their work with the Navy Marine Mammal Program, William Evans and J.S. Leatherwood may have been the first to suggest using marine mammals as an platforms for data collection; W.E.Evans and J.S. Leatherwood, *The Use of an Instrumented Marine Mammal as an Oceanographic Survey Platform* (San Diego, CA: Naval Undersea Center, 1972). A decade later, Jacqueline G. Jennings of the National Marine Fisheries Service suggested that as “small 'mobile buoys', marine mammals in particular could provide a wealth of information on the global ecology of much of the world's oceans”; Jacqueline G. Jennings, “Tracking Marine Mammals by Satellite—Status and Technical Needs,” *Oceans* 14 (1982): 751-754, on 753. In 1999, the French researcher Yvon le Maho predicted that “animals will become a precious ally for investigating inhospitable regions of the globe”; Yvon le Maho, “Argos: An Ideal Tool for Tomorrow's Scientific Challenges,” *Argos News* 54 (April 1999): 8-11. In 2003, Mike Fedak, one of the pioneers of marine mammal satellite tagging at the United Kingdom's Sea Mammal Research Unit, described radio-tagged marine mammals as “oceanographic samplers”; Mike Fedak “Animals as Oceanographic Samplers,” *Argos Flash* (July 2003): 13-17.

called “archival pop-up” tags provided new insights into tuna behavior. Among other things, they revealed that tuna in the Atlantic constitute a single intermixed population with breeding areas in the Caribbean and Mediterranean, rather than separate populations. In 2000, one newspaper article quoted Block as saying that “it may be that electronic tags are the only thing which will prevent the virtual demise of some of these animals.”¹⁰⁷ The power of the scientist in this scenario did not derive primarily from her role as an expert providing authoritative interpretations of data, but rather from her role as a facilitator of direct, seemingly unmediated connections between the natural world—in this case, migratory animals and their environments—and human society.¹⁰⁸ The scientist was not the one who would read the book of the world and explain its contents to the rest of society, but the one who would, through the creation of self-inscribing hybrids, made the book of the world legible to everyone.

107 Andrew Morgan, “Scientists Take 10 Years to Map World of Water,” *Sunday Telegraph*, 31 December 2000.

108 According to E. Don Stephens, a Canadian fisheries expert, Block's research would allow fisheries managers to transcend the ignorance and politics that have hampered the development of sustainable fishing policies until now: “Without her, we'd be in exactly the same place we were 15 years ago: a bunch of theoreticians waving their hands and a bunch of European fisheries politicians arguing the case based on no data. ... If the managers do not accept [Block's] evidence ... then it seems to me that they will never accept any evidence and that their argument is not based on logic but rather is based on shortsighted political grounds”; Andrew C. Revkin, “Tracking the Imperiled Bluefin From Ocean to Sushi Platter,” *New York Times*, 3 May 2005.

