Values for Varmints: Predator Control and Environmental Ideas, 1920–1939

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IN THE last twenty years public attitudes and policy toward predatory mammals have changed drastically. The wildlife biologist's notebook and binoculars have replaced the hunter's traps, guns, and poison; federal grants for wildlife preservation have taken the place of bounty payments; and species which were hated, feared, and hunted are now cherished, studied, and encouraged. This abrupt reversal of public sentiment and policy (marked most conveniently by President Richard Nixon's ban in 1972 on poisoning predators) is the visible evidence of a long and largely hidden process of social and intellectual change.[1] As Americans

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[1] T. S. Palmer, "Extermination of Noxious Animals by Bounties," in U.S. Dept. of Agriculture, *Yearbook of Agriculture, 1896* (Washington, D.C., 1897), 55–68; Stanley P. Young and Edward A. Goldman, *The Wolves of North America* (1944; reprinted, New York, 1964), 196–

adapted to life in a country without a frontier, to living in an industrial, urban society in which nature was not a threat but a weekend vacation, they came to have a different appreciation of nature and of the relationship between people and the biological world in which they lived. They came, too, to have different ideas about animals.

Since the late nineteenth century, when the fate of the buffalo suggested the need for some wildlife policy, Americans have sought to preserve at least parts of the native wildlife and have justified this activity, and the use of public funds for it, on a variety of grounds. Animals were part of our heritage, they provided sport, they were aesthetically pleasing, they were necessary to a full experience of nature (which had spiritual benefits), or they showed forth the beauties of evolution. Recent public sentiment and legislation, though, go beyond this. The modern defense of animals is explicitly grounded in scientific studies and theory. Ecology shows, the argument runs, that all species are part of a whole, and that to remove one is to disturb all. Each species, then, has a place and a value as part of the ecosystem.

Our concern here is with the formation of the scientific ideas and studies which justify the new wildlife policies. When did ecology (the scientific discipline) begin to provide evidence to buttress the vague and sentimental appeals about the "balance of nature" and "nature's economy" which since antiquity have marked the larger body of "ecological thought" (to use these words in a broad sense)? Our test case is a controversy which occurred between 1924 and 1931, a lively and occasionally bitter debate between a group of mammalogists, who opposed the government's poisoning of predators in the West, and the U.S. Department of Agriculture's Bureau of Biological Survey, which was responsible for doing the poisoning. The mammalogists claimed that the Survey was destroying many nontarget animals, possibly entire subspecies. In addition, the program, they contended, led to the

320, 340–364; Richard Nixon, "Special Message to the Congress Outlining the 1972 Environmental Program," in *Public Papers of the Presidents of the United States: Richard Nixon, 1972* (Washington, D.C., 1974), 173–189; Executive Order 11643, Feb. 8, 1972, *Federal Register*, XXXVII, 2875. Roderick Nash, *Wilderness and the American Mind* (3rd ed., New Haven, 1982) is the best single guide to changing attitudes of Americans, but, like most surveys of the subject, contains little on animals or science.

increase of rodents and rabbits by killing their natural enemies. The Bureau of Biological Survey responded that poisoning was necessary for the stock industry and desirable for all because it destroyed "pest" animals. Man, explained Survey scientists, had destroyed the "balance of nature" and now had to manage the land and the animals.

Some scholars have argued that the protest of the mammalogists initiated the modern environmental movement. The evidence adduced here challenges that view. A close examination of the controversy of the 1920s reveals that the mammalogists defended predators on the grounds that they were necessary for a "balance of nature," but this defense was not based on scientific evidence that demonstrated how predators and prey fit into a connected system. An adequate basis for a defense of predators as part of an "ecosystem" did not emerge until the 1930s and later when scientists had gained a clearer understanding of environmental relationships.[2]

The discussion here is in three sections. The first deals with the federal program, which killed thousands of coyotes each year and prompted the controversy of the 1920s. The second focuses on the mammalogists' challenge to the federal program. The last part compares the scientific evidence of the 1920s with the research of the 1930s and discusses the genesis of the modern "environmental" outlook.

THE WOLF IN THE GARDEN

Federal involvement in predator control began in 1885, when the Department of Agriculture's then new Office of Economic Ornithology and Mammalogy (in 1896 the name was changed to the Division of Biological Survey which became a bureau in 1905) began to study ways of poisoning rodents, pest birds, and predators. Direct action started in 1905 when the Forest Service hired trappers to kill wolves on national forest grazing lands. In re-

[2]Donald Worster, in *Nature's Economy* (San Francisco, 1977), discusses ecological thought in a broad sense and in chapter 13, "The Value of a Varmint," offers an interpretation of the mammalogists' protest that links the incident more closely to modern environmental values.

sponse to western demands for federal help, Congress in 1914 provided money for experiments on predator control and the next year launched what became a continuing program of eradication. It directed the Bureau of Biological Survey to undertake the job, including the Forest Service's program.[3] By this time predator populations were declining. The larger ones had never been very plentiful, and relentless hunting had almost wiped them out. Scattered populations of wolves, mountain lions, and bears survived, but the only animal numerous enough to justify the appropriations was the coyote, and it was on the "little wolf" that government trappers concentrated.[4]

The program roused little comment and no opposition, for virtually no one was concerned about the preservation of predators, particularly not as part of the general fauna of the West. The practice of killing predators on sight had a long and respectable history. Europeans had done it for centuries, and Massachusetts Bay and Virginia had offered bounties on wolves as early as the 1630s. The bounty system spread with settlement, remaining an almost ubiquitous feature in state statutes into the twentieth century.[5] The warfare against predators accelerated in the last third of the nineteenth century as settlers moved into the trans-Mississippi West. Professional "wolfers" used strychnine in large quantities in the 1870s, and ranchers added rifles and steel traps (just coming into wide use). Poison was cheap and popular, and cowboys seemed to have regarded it almost as a social duty to "lace" any carcass they encountered on the range. A few states passed out free supplies of strychnine, and, as late as the 1920s, some western states allowed unregulated sale of poisons in drugstores.[6]

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[3]The activities of the Bureau may be best traced through the annual reports of the Bureau of Biological Survey. The successor agency is the Fish and Wildlife Service of the U.S. Department of the Interior, and the program is in the Animal Damage Control Division. Jenks Cameron, *The Bureau of Biological Survey* (Baltimore, 1929), 45–46.

[4]The Bureau's annual reports provide statistics on the number of animals reported killed.

[5]T. S. Palmer, "Extermination," 57–59.

[6]*Ibid.*, 67; David E. Lantz, "Use of Poisons for Destroying Noxious Animals," in U.S. Dept. of Agriculture, *Yearbook of Agriculture, 1908* (Washington, D.C., 1908), 421–432; see also "Predatory — Sale of Poisons" file in General Correspondence, 1890–1944, Bureau of Biological Survey, Fish and Wildlife Service, Record Group 22, Records of the United States Fish and Wildlife Service, National Archives (hereafter cited as RG 22), which contains correspondence on the states' poison policies. Ernest Thompson Seton recounted a case of accidental poisoning in which a cowboy mistook the strychnine bottle for the quinine and died in agony. *Trail of an Artist-Naturalist* (New York, 1940), 310.

The slaughter of native wildlife in the last part of the nineteenth century caused concern about animals, but efforts to preserve them concentrated on a few useful or beautiful species. Animals which ate other animals, particularly animals which preyed on those man wanted or was raising, were still beyond the pale. Even such staunch defenders of wildlife as Ernest Thompson Seton—a naturalist, artist, and writer of animal stories—limited their sympathies.[7] "Lobo, King of the Currumpaw," hero of one of his most famous stories, was still an "outlaw king." Seton's change of heart when he finally traps the old wolf, his recognition of the spirit of the wilderness and of freedom which the animal embodies, is touching, but he does not release the wolf (which dies from a broken heart). Lobo's death means the passing of something wonderful, but it is the price of civilization. In "Bingo, the Story of a Dog," Seton, caught in a trap, is about to be "devoured by the foe I most despised" and is saved only by his wolf-killing dog.[8] Consider, too, William Hornaday, one of the most active advocates for wildlife in this period. Hornaday fought hard to eliminate market hunting, cut bag limits, and stop the hunting of songbirds, but he was quite forthright in his condemnation of "bad" species. The peregrine falcon, he thought, looked best "in collections," Cooper's and Sharp-shinned hawks should be "shot on sight," the Great Horned Owl was an "aerial robber and murderer," and the wolf a "master of cunning and the acme of cruelty." Even the Audubon Society, dedicated to protecting birds, killed predators on its preserves—to save the "good" species.[9]

Scientists were no different than other people and, if less emotional about the "beautiful" species, still were hardly interested in saving the predators. There were few in the American Society of Mammalogists in the 1920s who protested characterizations (in the columns of the *Journal of Mammalogy*) of predators as robbers, murderers, and thieves. At the beginning of the decade Aldo

[7]For a discussion of Seton's role, see John Henry Wadland, *Ernest Thompson Seton: Man and Nature in the Progressive Era, 1880–1915* (New York, 1978).

[8]Ernest Thompson Seton, *Wild Animals I Have Known* (New York, 1898). "Lobo" was praised when it appeared in *Scribner's Magazine*, XVI (Nov. 7, 1894), 618–628, under the title, "The King of Currumpaw: A Wolf Story" (Wadland, *Ernest Thompson Seton*, 210) and it was reprinted as late as 1942 (*Reader's Digest*, XLI [Nov. 1942], 103–106). *Wild Animals I Have Known* is still in print. The quote from "Bingo" is in *Wild Animals I Have Known*, 180.

[9]William T. Hornaday, *Wildlife Conservation in Theory and Practice* (New York, 1914), 141–151; Hornaday, *Our Vanishing Wildlife* (New York, 1913), 80, 140.

Leopold led a campaign to rid New Mexico of its last predators, a goal he thought desirable and attainable. In 1929, for example, he had no objection to his friend Herbert Stoddard's remarks about exterminating hawks, remarks which might have come from any landowner anxious about his quail or chickens. Similarly, Joseph Grinnell, one of the leaders in the fight against predator poisoning in the 1920s, could, as late as 1915, casually defend scientific collectors on the grounds that the "average collector can and does on all occasions destroy Cooper and Sharp-shinned hawks, and in this way certainly makes up for the small birds he shoots."[10] Even during the debate over coyote poisoning, the scientists attacking the Survey's program were careful to endorse in principle predator control.

That predators were "bad" was one idea of a set held, at least implicitly, by most of the population. People accepted the primacy of human claims to the land; wildlife's claims came later, if at all. The public also viewed nature as disconnected areas and species. The land could be managed by manipulating the parts, retaining the desirable ones and eliminating the others. Unwanted species might survive in the National Parks—but even there their tenure was insecure, for the Park Service used predator control into the 1920s. The concept of nature as a functioning system in which each species played a part was evident in appeals to a "balance of nature," but that was a vague concept, which had no experimental grounding and was not fully accepted even within the scientific community. The Survey's scientists could, and did, argue that man had destroyed the natural balance and was now responsible for managing nature.[11] It is understandable,

[10] Julius M. Johnson protested against this attitude in a letter to the *Journal of Mammalogy*, VIII (May 1927), 173, asking why a mountain lion's kill should be thought a crime. The offending article was M. E. Musgrave, "Some Habits of Mountain Lions in Arizona," *Journal of Mammalogy*, VII (Nov. 1926), 282–285; Musgrave was with the Biological Survey. Grinnell's comment is in "Conserve the Collector," *Science*, XLI (new series) (Feb. 1915), 229–232, and was reprinted in a posthumous collection of essays, *Joseph Grinnell's Philosophy of Nature* (New York, 1943), 65–72. On Leopold, see Susan Flader, *Thinking Like a Mountain* (Columbia, Mo., 1974). Stoddard's comments are in *The Bobwhite Quail* (New York, 1931), 212. Leopold's correspondence with Stoddard indicates that both, by this time, were moving away from earlier positions. Leopold Papers, Department of Wildlife Ecology, University of Wisconsin Archives.

[11] Frank N. Egerton, "Changing Concepts of the Balance of Nature," *Quarterly Review of Biology*, XLVIII (June 1973), 322–350. See also "Balance of Nature" file, General Files, Division of Wildlife Services, RG 22.

given this situation, that congressional appropriations for poisoning pest animals excited little interest and less alarm. Poisoning was just a way to get rid of an unwanted part of nature. What requires an explanation is why there was a fuss at all about killing "varmints."

THE POISON CONTROVERSY, 1924–1931

In 1924 a few zoologists, mainly westerners, began to argue against the predator control program. It was, they said, killing many "innocent," nontarget animals, causing a serious drain on the wildlife of the West. This point of view found strongest expression at the Museum of Vertebrate Zoology at the University of California, Berkeley, where Joseph Grinnell, the director since 1908, took a leading, but covert, role (to shield his institution from angry state legislators). Grinnell's assistants, Joseph Dixon and E. Raymond Hall, carried much of the public battle into the late 1920s, when one of his friends, A. Brazier Howell, took the lead. These men had lived and worked in the West for years. Many of them had seen it when it bore few visible traces of human influence, and they were appalled by the rapid destruction of primitive conditions.[12] The expansion of the Survey's operations in the early 1920s especially alarmed them and provided a target for their protests. Discontent became open opposition at the 1924 meeting of the American Society of Mammalogists, where several people debated the Survey's policies with two of its biologists, E. A. Goldman and W. B. Bell.[13] The Survey was not controlling

[12] H. E. Anthony to Joseph Grinnell, June 3, 1925, Anthony file, Correspondence of the Museum of Vertebrate Zoology, Berkeley, Calif. (Cited hereafter as MVZ). Evidence of the mammalogists' feelings comes principally from the correspondence files of Joseph Grinnell, E. Raymond Hall, Joseph Dixon, C. C. Adams, Lee R. Dice, H. E. Anthony, and A. Brazier Howell, MVZ. See also "Predatory—Sale of Poisons," file in General Correspondence, 1890–1944, and "Predatory—States," file in General Files, Division of Wildlife Services, RG 22.

[13] The session included H. E. Anthony, "General Status of Predatory Mammal Problems"; Lee R. Dice, "Scientific Value of Predatory Mammals"; W. B. Bell, "Predatory Mammals, a Practical Problem in Economics"; E. A. Goldman, "The Predatory Mammal Problem and the Balance of Nature"; Joseph Dixon, "Food Predilections of Predatory Mammals"; and C. C. Adams, "The Conservation of Predatory Mammals." The papers of Dice, Goldman, Dixon, and Adams were printed in the *Journal of Mammalogy*, VI (Feb. 1925). Bell's is in Research Reports, General Files, Division of Wildlife Research, RG 22.

predators, its opponents claimed, but exterminating them; local populations were being wiped out and some subspecies might already be extinct. Valuable study material was being lost to science, for once the original populations were gone it would be impossible to reconstruct their role in the area or their relation to other populations of the same species. There were economic considerations as well. Dead predators furnished fur, which was an important part of the economy in some areas, but live predators controlled or curbed rodents which competed with cattle and sheep for grass. Finally, the animals might have other, as yet undiscovered, values. C. C. Adams cited the recent discovery of insulin in the shark's liver as an example of the good that might come from "vermin."[14]

Goldman, who bore the burden of defending the Survey, said that scientists need not be alarmed. Predator control might cause "local extermination," but the Survey did not intend to kill off entire species and was not, in fact, doing so. Even if large predators were eliminated from the United States, he pointed out, they would still survive in Canada and Mexico. As for the smaller predators, particularly the coyote, they were in no danger at all. His main argument, though, was that poisoning was necessary. "Large predatory mammals, destructive to livestock and game, no longer have a place in our advancing civilization." They were an unacceptable drain on resources. In any event, he concluded, the Survey was not responsible for the policy; it had simply taken over work that the ranchers had been doing and was only hastening a process which had been underway for decades.[15]

The debate began with this relatively low-key confrontation — there was no attempt to appeal to the public or conservation groups — because the mammalogists were confident that they could, with little difficulty, persuade the Survey to change its policy. The agency had begun as a purely scientific enterprise (despite its position in the very practical Department of Agriculture). Specimens and scientists went back and forth between

[14] Adams, "Conservation," 85–87, provides the best argument; see also Dice, "Scientific Value," 25–27.

[15] Goldman, "The Predatory Mammal Problem"; see also W. B. Bell, "Hunting Down Stock Killers," in U.S. Dept. of Agriculture, *Yearbook of the Department of Agriculture, 1920* (Washington, D.C., 1921), 289–300.

government and academia, and close personal relationships reinforced professional ties. Neither side could afford, or wished for, a break in the relationship. The addition of the predator and rodent control program, though, had changed the survey's interests and leadership. It was becoming a service agency, and its newest clients, the woolgrowers, had a more effective source of influence than professional ties or even sympathetic congressmen on appropriations subcommittees. They paid the bills. In 1918 the Survey had begun cooperative projects with states, counties, and local livestock associations. It furnished the trapper with his equipment, and the "cooperator" provided most of the money, usually though a tax on livestock in the affected area. By the mid-1920s these funds were a quarter of the Survey's budget.[16] The scientists inevitably became less important.

Goldman's explanations did not satisfy the mammalogists, and the Society appointed an investigating committee whose membership reflected the division of opinion. There were two government biologists, Vernon Bailey and E. A. Goldman, and three academics, Joseph Dixon, C. C. Adams of the New York State College of Forestry and the Roosevelt Wildlife Station, and Edmund Heller (chairman), director of the Milwaukee Museum. The committee's report was delayed until 1928 largely because the members, once they got beyond the accusations and countercharges, found remarkably little to go on.[17] Evidence of population dynamics and the relationships among various species was needed to settle the question, but there was hardly any data available and no theory which would help the committee to make sense of the data it had. The first systematic works on animal ecology had appeared only a few years before the controversy broke out, and those studies—Victor Shelford's *Animal Communities of Temperate North America* (1909) and C. C. Adams's *Guide to the Study of Animal Ecology* (1913)—were pioneer efforts written to organize a scattered literature.[18] Charles Elton, whose *Animal Ecology* (1926) became a classic

[16]Cameron, *Biological Survey*, 45–46.

[17]For the discussions on the technical delays and political considerations, see Joseph Dixon and E. W. Nelson files, MVZ. The mammalogists delayed the presentation of the report when Nelson retired, wishing to give his successor, Paul Redington, a chance to get settled in his office.

[18]Victor E. Shelford, *Animal Communities of Temperate North America* (Chicago, 1913); C. C. Adams, *Guide to the Study of Animal Ecology* (New York, 1913).

in the field, later commented that when he wrote that book the subject was just beginning to move from "easy generalizations about adaptations and the balance of life" to the new world of "limiting factors," "food chains," and "quantitative methods."[19]

Scientists were halfway between the old natural history and the new mathematical ecology. Most had been trained and had formed their professional careers before ecology had become a recognized discipline. Grinnell, for instance, had been largely self-educated in zoology. He was interested in subspecies and the distribution of characteristics and their correlation with the environment, but as a means of studying "evolution now in progress," not ecology.[20] He trained his students in taxonomy and resisted not only ecological studies at the museum, but also ecological theories which his students developed after leaving.[21] Some of the younger scientists had been trained as ecologists, but they were the exception. Professional organization was rudimentary; the Ecological Society of America had been formed less than a decade before the controversy broke out and, significantly, it was the American Society of Mammalogists, not the Ecological Society of America, which took the lead in the controversy over poisoning.[22]

Though convinced that predators were not harmful but, on the whole, beneficial (because they controlled rodent populations), the scientists could not prove it. Their arguments about the "balance of nature" reflect this failure. Scientists and laymen alike invoked the concept to explain why animal populations were stable despite fantastic reproductive potentials. Some mechanism in nature trimmed the excess, keeping each species within limits, and predators were often thought to be responsible. The idea, though, was as

[19]Charles Elton, *Animal Ecology* (London, 1927), vii; W. C. Allee, Orlando Park, Alfred E. Emerson, Thomas Park, and Karl P. Schmidt, *Principles of Animal Ecology* (Philadelphia, 1949), 55–59.

[20]Joseph Grinnell, "Significance of Faunal Analysis for General Biology," *University of California Publications in Zoology*, XXXII (Nov. 1928), 13–18.

[21]Tracy Storer, "From Observation to Experiment" (Oral interview, Archives University of California, Davis), 42, said that he had, in 1918, proposed work in Yosemite National Park which would have been an early ecological study, but Grinnell vetoed the plan. See also Grinnell to Barrington Moore, undated, in which Grinnell reviews Svihla's "Ecological Distribution of the Mammals on the North Slope of the Unita Mountains," Moore file, MVZ.

[22]Robert L. Burgess, "History of the Ecological Society of America, 1977," in Frank Egerton, ed., *History of American Ecology* (New York, 1977).

vague as it was appealing. How did it work, how did it guarantee just enough predators to check the prey and not too many? More troubling, how could it account for cyclic phenomena or the obvious and spectacular periodic irruptions, such as the famous hordes of lemmings? There seemed no reason why predators should stabilize a prey population at a particular level or act on cyclic variations — no reason, in short, why it should work.[23]

The difficulties of invoking the concept of "balance of nature" became evident in the disagreement over the cause for an irruption of mice in Kern County, California, in 1927. The Survey had conducted a predator control campaign in the county in 1924–1925, and in January 1927, a horde of mice, which had been breeding in the fallow fields of a dry lake bed, spread out across the country. They overran stores and homes; a slick coating of crushed rodents made roads impassable. Traps and poison yielded incredible numbers — two tons at one warehouse. Stories circulated about housewives who had spent a week on the furniture, never touching the floor.[24] E. Raymond Hall, a mammalogist at the Museum of Vertebrate Zoology and one of Joseph Grinnell's students, arrived on the scene shortly after the peak of the epidemic and spent some time reconstructing its course. The abundance of food and shelter in the lake bed, he said, had been a major factor, but the lack of natural enemies was the key. The Survey and local farmers, by killing the animals which ate mice, had caused the problem. Stanley Piper, an employee of the Survey, arrived a week later and drew different conclusions. He believed that the long grass had hidden the mice and provided food for the increasing hordes. It was unrealistic, he thought, to blame men or to rely on natural predation; there had been fluctuations in animal populations long before people had come to disturb the "balance of nature." But whatever the situation before, man was now the dominant force and must manage nature — and Piper pointed to the Survey's role in poisoning the hordes.[25]

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[23] Egerton, in his "Balance of Nature," discusses the history of the concept. See also the "Balance of Nature" file in General Files, Division of Wildlife Services, RG 22.

[24] E. Raymond Hall, "An Outbreak of House Mice in Kern County, California," *University of California Publications in Zoology*, XXX (Feb. 1927), 189–203; Hall field notes from MVZ.

[25] Hall, "Field Notes"; Stanley E. Piper, "The Mouse Infestation of Buena Vista Lake

Given the rudimentary state of ecology, the committee was hardly likely to settle the issue, even if its members had agreed. They did not. Their main report was noncommittal, representing as it did common ground. Predators, it said, had values—educational, scientific, and economic—and should be preserved, either in national parks or in isolated areas of the public domain in the West or Alaska. The supplementary report, signed by Dixon, Adams, and Heller, condemned the Survey's policy in the strongest terms. It charged that the livestock industry in the West, with the cooperation of the state governments and the Survey, was conducting an extermination campaign based on false data and bad scientific theory. The estimates of stock lost to predators (the basic information used to justify the program) came from ranchers and state officials who had every reason to exaggerate the extent of the danger. Other evidence (largely analysis of the stomach contents of dead predators) was inaccurate and biased. Predators could not be shown to be a serious drain on stock, and current policy should give way to a "system of intelligent controls" adapted to the particular needs of differing western regions.[26]

Adams used this report to challenge the conduct of the program, and Paul Redington, head of the Survey, replied in a public letter. He emphasized the committee's "general agreement" with the Survey that predators should not be allowed outside special sanctuaries and that "civilization will require all space except those areas that are in advance specifically reserved for wildlife conservation." The supplementary report, he went on, criticized chiefly the lack of research done before control operations were started and the possibility that the Survey was eliminating study material for the scientists. Research on control, he insisted, was not needed; predators clearly had to be curbed to protect stock, and that had to be done now. As for scientific data, the Survey was doing research on the habits and distribution of predators while it carried on control operations.[27]

Basin," Calif. Dept. of Agriculture *Monthly Bulletin*, XVII (Oct. 1928), 538–560.

[26] "Report of the Committee on Wild Life Sanctuaries, Including Provision for Predatory Mammals," *Journal of Mammalogy*, IX (Nov. 1928), 354–358.

[27] Paul G. Redington, "Policy of the U.S. Biological Survey in Regard to Predatory Mammal Control," *ibid.* X (Aug. 1929), 276–279. Adams's letter precedes this article.

The letter did not answer the mammalogists' charges: the lack of scientific study to justify mass poisoning; the absence of data to show that poisoning was economical; and possible effects on non-target species, and indirectly, on rodent populations. Indeed, the letter studiously ignored these charges that had been raised in the meeting in 1924 and in the report. Redington did take pains, though, to refute the charge that the Survey was carrying on an extermination campaign. By this time even "bad" animals had some claim to consideration. "Extermination drives" resulted in bad public relations even when practiced on species that the public did not perceive as part of the country's normal wildlife. The point was already a sensitive one; Redington had warned field workers to do what they could to dispel the impression that predator control was doing away with wildlife, and he called for public education and elimination of the word "extermination" from the Survey's vocabulary.[28] Neither then nor later, though, could the agency correct that unfortunate impression. In article after article there were references to "wiping out" predators and "getting rid of" the "stock killers" while lauding the government hunters doing the job. Redington himself talked of "extermination" when testifying before Congress in 1927, and in 1931 the head of the House subcommittee dealing with agricultural appropriations was clearly under the impression that predator control was designed to "get rid of" varmints.[29] The most embarrassing incident came in 1929, when Jenks Cameron's *Bureau of Biological Survey*, one of a series of books on government departments, described the predator control campaign under the name "extermination." In a discussion with Adams, E. A. Goldman, a Survey biologist, denied the charge. The author, he said, had no position with the Survey and was probably a "hack writer" (a species which even the

[28]Paul G. Redington, speech opening conference of field representatives in Ogden, Utah, April 23–28, 1928. In that speech he spoke of "the opposition . . . of those who want to see the mountain lion, the wolf, the coyote, and the bobcat perpetuated as part of the wild life of the country." Copy in Report of Conferences, 1928–1941 file, General Files, Division of Wildlife Services, RG 22.

[29]The files of the Bureau of Biological Survey, RG 22, National Archives, are replete with such references, and include articles on predator control—pro and con—dating from the early 1920s. See also House Committee on Appropriations, *Hearings on Agricultural Department Appropriations Bill For 1933*, 72 Cong., 2 sess. (1932).

editor of the *Journal of Mammalogy* had condemned as a predator that might well be wiped out without loss to the community).[30]

Redington did not calm the mammalogists' fears and a second, more public and vehement, round of protest began. The leader was A. Brazier Howell, an anatomist, mammal enthusiast, and friend of Joseph Grinnell. Howell was impatient at the delays and "kid glove" treatment that the mammalogists were giving the Survey. He circulated a petition and in early 1930 presented it, with the signatures of 148 scientists and a covering letter, to Redington.

The current program, the petition said, was an imminent danger to the "very existence of all carnivorous mammals, including those valuable species which constitute the chief check upon injurious rodents and are a vital element of our fauna." These strong words, however, did not mean that the scientists endorsing them were against predator control or that they wanted to preserve the larger predators throughout the country. We do not, Howell went on, "deny that control of predatory mammals is advisable in certain instances and in certain places; only that it is greatly and dangerously overdone. Also we make no mention of wolves and mountain lions which, whatever their values from an aesthetic viewpoint, are truly killers and are destructive. Our claims are based on the economic viewpoint alone."[31]

Coyotes, Howell said, helped keep down rodents which would otherwise compete for the grass with cattle and sheep. The Survey, before it had begun predator control work, had made that point, but it had neglected to do so in recent years. As for the effect on stock, the data did not justify widespread poisoning. There had been, he pointed out, no scientific analysis of the coyote's diet; the Survey was relying on the observations of its untrained trappers, which the head of its own food-habits laboratory, W. C. McAtee, refused to accept.[32] Here the weakness of the evidence is clear. Howell was correct in saying that the Survey had

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[30]Cameron, *Biological Survey*, 44–46; C. C. Adams, "Rational Predatory Animal Control," *Journal of Mammalogy*, XI (Aug. 1930), 353–358. The editor's intemperate outburst against the distortions of reporters is in *Journal of Mammalogy*, XIX (Nov. 1929), 373.

[31]Copies of the petition and correspondence are in Howell file, MVZ.

[32]On the matter of food habits, see the testimony of Tracy I. Storer, who visited Washington, D.C., in the spring of 1930 while Congress was holding hearings on the predator control program. His comments on the Survey are the more valuable in that he was a neutral

no data worth considering and that it had in earlier years defended predators on the grounds that they were part of the "balance of nature." His own evidence, unfortunately, was just as weak; he could not show that coyotes actually played a role in checking the population growth of the mice. It seemed plausible, but plausibility is not scientific proof.

There was also, Howell said, the program's toll on nontarget species. The Survey claimed that these losses were negligible, but, Howell said, its evidence was poor. The trappers who reported kills had no incentive to hunt for the bodies of nontarget animals and every reason to overlook them. The testimony of outside observers suggested that they did. Howell cited the reports of Dixon, whom he characterized as the best observer the agency had in California (Dixon, like many of the mammalogists, including Howell, had worked for both sides), to show that field observations did not bear out the claims of low losses among fur-bearers. Whatever the good intentions of officials in Washington, he charged, the men in the field were exterminating native wildlife.[33]

By the time Howell delivered his blast, the Survey had yielded to heavier pressures — from the woolgrowers. It asked Congress for a million dollars a year for ten years to reduce predator populations to a very low level, after which predator control expenses, it said, could be permanently and drastically lowered. Late in April 1930, the House and Senate Agriculture committees heard testimony on a bill to give the Secretary of Agriculture authority to pursue predators and other injurious animals on public and

observer (so far as that could be said of anyone who was involved, even peripherally). He confided his views to his private field notes (which he kept from 1912 to 1963), which were not intended for publication. On May 2, 1930, he had lunch with W. C. McAtee and visited the food habits laboratory. "When I enquired about mammal stomachs I was told that very few had been examined. I was shown data sheets or rather reports received from the 'leaders' in predator animal control in several western states. The food examinations are made chiefly by trappers. From 25 to 33% is 'unidentified.' There were very few records made of species other than coyotes." Storer then gave figures indicating almost no collections. "Obviously skunks are not being reported since it is practically impossible in the Western states to get coyotes without numerous skunks, by either trapping or poisoning. Dr. A. K. Fisher came in while this discussion was in progress and endeavored to defend present practices but he could not cite any work done on food habits of mammals in the predatory class." Unpublished field notes of Tracy I. Storer, 1912–1963, p. 1232, in possession of Dr. Ruth Risdon Storer, 619 Oak Avenue, Davis, Calif.; W. C. Henderson, "The Control of the Coyote," *Journal of Mammalogy*, XI (Aug. 1930), 336–350. This article includes the comments by biologists on Henderson's paper.

[33] Howell to Redington, April 14, 1930, Howell file, MVZ.

private lands and to conduct work for the "eradication, suppression, or bringing under control" of these creatures. Western congressmen and senators opened the hearings with vivid descriptions of the hardships that their constituents were suffering. Ranchers and representatives of the National Wool Growers Association added their support, and the Survey produced an impressive array of facts and figures to justify its request.

Howell and Hall testified against the program and primed congressmen friendly to their position with questions for the Survey's witnesses. They made little headway. Most congressmen were not interested in preserving "useless" animals, and it was hard to make a case against the program with the available evidence. The scientists could point to deviations from approved field practices, the death of some nontarget animals, and local opposition to the program, but they could not show that it was economically unsound or biologically dangerous. They also presented no alternative. They were not calling for an end to predator control but for more caution in the field and for modifications in the program, and it was easy for the Survey to promise that valid objections to the program could be met under the current set-up.[34]

The Survey's attempts to reach an understanding with its opponents failed. Goldman and W.C. Henderson appeared at the mammalogists' meeting in May to explain and defend the program. Goldman, in a speech entitled "The Coyote — Archpredator," painted a bleak picture of western ranchers beset by a "bold and ruthless marauder," while Henderson used the controversial stomach content studies to show that coyotes were fond of beef and mutton.[35] They met a barrage of criticism. Hall claimed that errors permeated Henderson's work, including his food studies, the information gathered by trappers, and his reports on the effects of large doses of poison on small animals. Both Dixon and

[34]"Control of Predatory Animals," *H. Doc. 496*, 70 Cong., 2 sess. (1929); House Committee on Agriculture, *Hearings on Control of Predatory Animals*, 71 Cong., 2 sess. (1930); "Predatory and Other Wild Animal Control," *H. Rep. 2396*, 71 Cong., 3 sess. (1931); Senate Committee on Agriculture and Forestry, *Hearings on Control of Predatory Animals*, 71 Cong., 2 and 3 sess. (1931).

[35]E. A. Goldman, "The Coyote — Archpredator," *Journal of Mammalogy*, XI (Aug. 1930), 325–334; Henderson, "Control," 336–350.

Hall condemned the stomach content studies for systematic bias (much of the evidence came from animals killed by eating poisoned bait that consisted of flesh of domestic stock) and the reports of the government hunters for inaccuracies. Hall and Adams said that the government had refused to cooperate with outside scientists and was not "coming clean" on its "extermination" policy. It claimed to be restrained, but was actually continuing to poison wherever it could. Howell repeated his charges that the Survey was in the woolgrowers' pocket. The committee appointed a year before to review the earlier report generally agreed that the current program could not be scientifically justified and called again for change.[36]

The mamalogists and the Survey did agree on a joint inspection team to look into allegations that field men were routinely violating guidelines, but mutual suspicion doomed that enterprise. Goldman told Howell that he knew what he would find in the field, and Howell suspected a cover-up. In the Survey, on the other hand, rumors circulated among the hunters about investigations and possible firings (a serious concern during the Depression). Some supervisors considered the whole operation suspect. The Survey also failed to face fully and fairly the mammalogists' charges, justifying its work rather than investigating problems. The trip raised more questions (and tempers) than it settled, and in the end the mammalogists repeated their earlier condemnations of the Survey for conducting an indiscriminate campaign against native wildlife and, despite claims to the contrary, of "drumming up" business.[37]

The tide, though, was running the other way. The next year Congress passed the Animal Damage Control Act, approving a ten-year plan for predator and rodent control and providing statutory authority which remained the legal basis for the program into the 1970s. Opposition died down, for the discouraged scientists became convinced that they could not change public policy

[36]E. Raymond Hall, "Predatory Mammal Destruction," *Journal of Mammalogy*, XI (Aug. 1930), 373–377; C.C. Adams, "Rational Control," 353–358; A. Brazier Howell, "At the Cross-Roads," *Journal of Mammalogy*, XI (Aug. 1930), 377–389. The resolution was printed in *Journal of Mammalogy*, XI (Aug. 1930), 431.

[37]Howell to Hall, Sept. 5, 1930, Howell file, MVZ, Berkeley; see also Dixon, Anthony, Adams, and Dice files, MVZ, and correspondence of O.J. Murie (who headed a regional

or public opinion during the economic emergency. There were scattered protests during the succeeding decade, but they were largely against the inhumanity of poisoning or trapping. The complaints were not based on scientific evidence and did not cause the Survey any serious problems.[38]

VALUES FOR VARMINTS

The scientists concentrated on saving wildlife in areas which were, by law, already committed to that goal. That, for most large species, meant the National Parks. The Park Service alone had large untouched areas, a mission to protect them, a constituency, and public support. By this time, too, it had moved beyond a concern with monumental scenery to other goals. Protecting wildlife, which, in theory, had been a part of the Service's mission since the establishment of Yellowstone in 1872, was becoming an actual policy. In the mid-1920s the park superintendents had expressed their opposition to poisoning and trapping, and by the early 1930s the Park Service had ended predator control on its land. In 1930 Harold Bryant, one of Grinnell's students, became chief naturalist in charge of organizing not only campfire talks but also a full program of wildlife protection. With some opposition, the Park Service began scientific study of wild animals, including predators, and in 1933, it started a new publication series—"Fauna of the National Parks."[39]

The defense of wildlife, however, amounted to a policy of encouraging outdoor zoos, islands of natural conditions in which

office for the PARC in Jackson, Wyoming), in "Report on Poisoning" file, Predatory Animals, Murie Papers, Conservation Center, Denver Public Library. See also "Predatory—Sale of Poisons" file in General Correspondence, RG 22.

[38] For protests by the mammalogists and other groups, see "Predatory—Sale of Poisons" file in General Correspondence, RG 22. The problem of the predator control division "drumming up" business recurred; see Clarence Cottam to J. T. McBroom, Jan. 16, 1967, in "Policy—Correspondence re Control Policy" file in General Records, Division of Wildlife Services, RG 22.

[39] George M. Wright, Joseph S. Dixon, Ben H. Thompson, "Fauna of the National Parks of the United States," in U.S. Dept. of the Interior *Fauna Series No. 1* (Washington, D.C., 1933). That Murie's work was not completely accepted even within the Park Service is apparent in a letter from Olaus Murie to H. E. Anthony, Dec. 5, 1945, in Murie file, Department of Mammalogy, American Museum of Natural History, New York. Here Olaus commented on the problems his brother Adolph had encountered in carrying out his work in Yellowstone and Mt. McKinley National parks.

remnants of the native fauna would be preserved for scientific, educational, and aesthetic interest. Outside these areas human economic needs would be the overriding guide to management, and the native wildlife would exist only if it did not seriously affect man's interests. There was in this an implicit acceptance of the desirability and practicality of human management of the land and of nature as a set of interesting, even beautiful, parts. There was no concern with preserving wildlife as part of the whole landscape, or for its needs to be weighed in the same scale as human economic drives. The lack of ecological theory and field work based on such theory was responsible for the scientists' position. This becomes clearer when the views of the 1920s are compared to the findings of ecological research in the next decade, findings which dramatically reshaped the public's vision of nature and changed the attitudes of conservation organizations toward predators. The research of the 1930s laid the basis for the modern defense of predators as integral parts of the ecosystem and represents the maturation of ecological theory.

The change in scientific understanding is most apparent in the work of Paul Errington, a former graduate student under Aldo Leopold at the University of Wisconsin and a person who played a key role in shaping ideas about predation. Errington began work on the population dynamics of northern bobwhite quail in 1929, and three years later had accumulated data on winter mortality that led him to some startling conclusions. Contrary to all expectations, the kind, the number, and the density of native predators did not seem to have much affect on the survival of prey populations, and more data only strengthened his conviction. The quail population seemed much more sensitive to conditions of food and shelter, what he initially called "carrying capacity" and later "thresholds of security." Good areas seemed to carry a high number of quail through the winter, bad ones a low number, regardless of normal weather variations and fall population. Other research on small-game species confirmed the broad outlines of Errington's ideas, particularly that predation was not normally a major check on prey populations. Animals did not seem, as the common view had it, to live in terror, nor did they breed in a frantic race to keep ahead of relentless thinning by the flesh-eaters. Errington had not, as he pointed out, found a key to ex-

plain predation. He had, though, shown that old ideas needed to be tested and should not be accepted casually.[40]

Adolph Murie's research on large predators between 1937 and 1941 reinforced the position that Errington had developed. Murie's two pioneering studies, one on the coyote in Yellowstone National Park and the other on wolves and Dall sheep in Mt. McKinley National Park, went beyond the usual analysis of the composition of the predator's diet to look at the age, sex, and state of health of the prey. He provided the first serious quantitative evidence of the effect of a large predator on a prey population, and the first scientific justification for allowing predators to multiply unchecked in the National Parks. Under normal conditions, he found, wolves and coyotes were not a serious hazard to their prey. Old, young, or ill individuals were susceptible, but the population as a whole was not. The evidence suggested, in fact, that predators played a positive role, culling the herds of the weak, sick, and injured.[41]

The new ecological information soon began to spread beyond the readers of the biological monographs. Where the Audubon Society, for example, had formerly defended hawks for destroying rodents, it now spoke for all species, regardless of their economic value, as parts of the ecosystem, and *Bird-Lore*, the society's journal, cited Errington's work as early as 1935 as the basis for its new stand. The new information also left its imprint on nature writing. By the late 1930s Aldo Leopold was publishing those essays which were later to appear as *A Sand Country Almanac* (1948), the bible of the environmentalist movement. Rachel Carson's *Un-*

[40]Paul Errington, "Bobwhite Winter Survival in an Area Heavily Populated with Grey Foxes," *Iowa State College Journal of Science*, VIII (1933–1934), 130; Errington, "Vulnerability of Bobwhite Populations to Predation," *Ecology*, XV (April 1934), 110–127; Errington, "What is the Meaning of Predation?" in *Annual Report of the Smithsonian Institution for 1936* (Washington, D.C., 1937), 243–252; Errington and H. L. Stoddard, "Modifications in Predation Theory Suggested by Ecological Studies of the Bobwhite Quail," *Transactions of the Third North American Wildlife Conference* (Washington, D.C., 1938), 736–740.

[41]For a survey of the literature, see John P. Russo, "The Kaibab North Deer Herd: Its History, Problems, and Management," Arizona Game and Fish Department, *Wildlife Bulletin No. 7* (Phoenix, 1964). Graeme Caughley disputed conventional wisdom in "Eruption of Ungulate Populations, with Emphasis on Himalyan Thar in New Zealand," *Ecology*, LI (Winter 1970), 53–72. See also Adolph Murie, "Ecology of the Coyote in the Yellowstone," U.S. Dept. of the Interior *Fauna Series No. 4* (Washington, D.C., 1941); Murie, "The Wolves of Mt. McKinley," U.S. Dept of the Interior *Fauna Series No. 5* (Washington, D.C., 1944).

der the Sea Wind (1941) and Sally Carrighar's *One Day on Beetle Rock* (1944) emphasized not individual animals but the connections among them. Even National Park Policy began to take account of this science. As early as 1940 the National Park Service was seeking to preserve all species in a natural balance, and in the post-World War II period this idea would come more and more to guide the parks' wildlife management work.[42]

Scientific proof of a natural world of connected parts would justify, in the 1960s, rejection of the ideal of total human control of the land and of the pastoral vision of landscape, making management not a matter of controlling nature but of adapting human enterprises to it. This scientific base and the concern with wildlife as part of the whole country, not just part of the biological islands of the parks, were lacking in the 1920s and distinguishes the controversies of that decade, despite their modern tone, from the later and successful efforts to change the predator control program. Twenty years later Olaus Murie, president of the Wilderness Society (but in 1930 a regional supervisor for the Survey), said that "the scientists who became so concerned at that time did not, I believe, understand their own motivation.... The big issue put forth was that 'innocent animals' were being killed incidental to poisoning operations. Deep in their hearts, if they had thought it out fully in those formative years of the opposition, was concern for the coyote itself...."[43] Murie was right; the scientists were concerned "for the coyote itself," and they had not "thought it out fully." They had not, because the science of ecology had not yet provided a sufficiently clear understanding of the workings of the ecosystem to enable them to buttress their love of animals with a scientific defense of them as useful or important. Paradoxically, only when the animals were seen as part of a system, when the value of a varmint would lie in its role in the complex workings of nature, would there be a full and open commitment to the despised coyote as something worth preserving for its own sake.

[42] *Bird-Lore*, XXXVII (March–April 1935), 122. See later volumes for the Society's shifting defense of predatory animals. See also Rachel Carson, *Under the Sea Wind* (New York, 1941); Sally Carrighar, *One Day at Beetle Rock* (New York, 1944); Aldo Leopold, *A Sand County Almanac* (New York, 1948); Victor H. Cahalane, "The Evolution of Predator Control Policy in the National Parks," *Journal of Wildlife Management*, IV (July 1939), 229–237.

[43] O. J. Murie to C. C. Presnall, Dec. 7, 1952, in Miscellaneous P file, Murie Papers.