Invited Paper:

WILDLIFE BIOLOGY AND NATURAL HISTORY: TIME FOR A REUNION

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Abstract: I find considerable evidence that wildlife management has broken partially free of its roots and is showing signs of malnourishment. It also is beset with various ailments, including addiction to technology, lust for statistics, professional hubris, and the delusion that research and management are synonymous. The wildlife management discipline started as applied natural history, and most of its star practitioners were broad-based naturalists, intimate with the landscapes and organisms in their charge. There are reasons to believe that the wildlife profession would do well to regraft itself to those natural history roots, especially in view of the changing roles that will be manifest as this century comes of age.

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Aldo Leopold is the patron saint of wildlife management. He was a naturalist and a natural historian, which means that he both studied and wrote about natural history. He followed in the large footsteps of Charles Darwin and many other intellectual giants. E. O. Wilson is today perhaps our best known naturalist. Darwin (1859) revolutionized biology and the world. He laid the groundwork for all disciplines in related fields. Leopold strongly influenced the nature and development of our profession. Darwin was the most accomplished naturalist the world has known, and he was *known* as a naturalist. Leopold, on the other hand, did not share that label, because the term naturalist had fallen into disuse by the early part of the 20th century.

THE HISTORY OF NATURAL HISTORY

Natural history as a term has a complex, somewhat unusual history—etymologically and in terms of its popularity as a descriptor. It is still widely misunderstood and misapplied, and not everyone thinks of it as a badge of distinction. In our age of specialization, terms describing generalization tend to fade. In the 18th and 19th centuries, men like Charles Darwin, Alfred Russell Wallace, Joseph Banks, Alexander von Humboldt, and many others were easily called naturalists, and presumably thought little about the tag. Centuries later, the analogous generalist term is probably "ecologist," and certainly there are myriad subtitles to accompany that term. But no one then was known as a "plant naturalist," or an "insect naturalist," or an "animal naturalist." Rather, the term "naturalist" had both breadth and power.

It is not only the tendency toward specialization that has driven the term naturalist to near extinction; it has also fallen victim to popularization. As David Elliston Allen has described in his brilliant book, *The Naturalist in Britain,* natural history was seized and ingested by a cadre of shakers and boosters during the late 19th and early 20th centuries. They sought to make it march in lockstep with good hygiene and fundamental Christianity; even the nascent Boy Scout movement took up the banner (Allen 1994).

And then there is the problem with the parent term itself: Natural History. Where does the "history" enter into the identification of birds or the preparation of a small mammal skin? The term "life history" is a little easier to understand, but it is still somewhat awkward.

It turns out that the history in natural history has little or nothing to do with history as we commonly conceive and use the term, i.e., something to do with the past. It turns out that "history" in this application has an archaic definition (Oxford English Dictionary). When the term was coined, "history" meant "description" (i.e., "systematic account"). Viewed in this context, everything fits; natural history is a description of nature.

Certainly it is true that the use of the terms "natural history" and "naturalist" fell into disuse and even disrespect. But now these terms are reemerging. This is good news, and I think much

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of the recrudescence is due to the work, the reputation, and the respectability of one man—E. O. Wilson of Harvard University. Here is the arch-scientist who can write, the viewer of the big picture who also is the world's foremost ant expert. His popularity is well deserved and enduring; the perspective of this naturalist (Wilson 1994) is priceless.

But we are still a race of definers, describers, and we are almost always made to rest easier if we have a definition to comfort and guide us. We lust after and love definitions, and we all have our favorites. Two of mine are, "To do science is to search for patterns in nature" (Robert MacArthur [MacArthur 1972:1]) and "Puritanism: The haunting fear that someone, somewhere, may be happy" (H. L. Mencken [Mencken 1949:624]).

I am just finishing a 3-decade career of teaching natural history at a small, public liberal arts college that is somewhat famous for its innovative approaches to education. Many of my now 2,200+ students have been from outside the mainstream college student population (although most of them have been entirely conventional). We have never had a wildlife curriculum per se, but over the years, I taught ornithology, mammalogy, evolutionary ecology, biostatistics, plant ecology, and many other classes with traditional titles that would be appropriate in a wildlife curriculum.

As an undergraduate at the University of California, Berkeley, I took a number of "wildlife courses" (more on this later) and had begun my teaching career at Humboldt State College (now University) in its wildlife department. As an undergraduate at Berkeley, 1 of the courses that helped to define my life was Zoology 113, "Natural History of the Vertebrates." In that course and in several other wildlife courses, I was introduced to field trips. Typically these occupied at most an afternoon. The few 2- or 3-day field trips we took were given far more to drinking beer than to serious academic study. I was as enthusiastic about drinking beer as the next guy (and most wildlife students, in those days, were guys), and we saw some nice places and some interesting organisms, but where was the rigor? Weren't we wasting our time, to 1 extent or another?

I vowed that if I were ever to be in a position that would allow me to take undergraduates in the field, I would travel long distances, stay out for good periods of time, and make academic rigor integral. The college at which I landed (The Evergreen State College, in Olympia, Washington) was brand new, and the faculty ran the place for many years before administrators were able to get the upper hand. It was heady stuff, writing our own tickets, setting our own schedules, teaching more or less what we wanted to teach—what we knew best and most enjoyed.

It worked well for me and, I think, for the many students who went into the field with me. In the natural history program (which I taught with my friend, a plant ecologist colleague, Al Wiedemann) we often spent 25 days of spring quarter in the field, travelling in college vans, camping out, running down the coast from Washington to northwestern California, over to Klamath Falls, Hart Mountain, and Malheur in eastern Oregon, then back to Olympia by way of the John Day fossil beds. A couple of times we took the natural history students to performances of Shakespeare's plays in Ashland, Oregon (this might have been the liberal arts part of their education).

The rigor originated in the "Natural History of the Vertebrates" course, where we were obligated to maintain a field journal, species accounts, and a specimen catalog according to a system established by Joseph Grinnell, the founder of the Museum of Vertebrate Zoology at the University of California, Berkeley. The system is demanding almost beyond imagination, in some respects, but I won't describe it here. Suffice it to say that if you're going to write it down, you've got to know what it is, where it is, and what it's doing. Denial denied.

In any case (to get back to natural history, the definition), I eventually wrote a little book (Herman 1986) describing this system, and my students were obligated to it in many of my classes. In the course of writing this book, I delved into sundry definitions of natural history, and I found plenty of them. From these sources and from my own experience, I developed the following definition:

> *Natural history is the scientific study of plants and animals in their natural environments. It is concerned with levels of organization from the individual organism to the ecosystem, and stresses identification, life history, distribution, abundance, and inter-relationships. It often and appropriately includes an esthetic component.*

Many dictionaries include in their definitions the comment that the term is "often used in relation to amateur work." And because "professional" means you get paid for it, an argument can be made for the case that Darwin's natural history was "amateur" work, because old Chuck Darwin never held a meaningfully paying job! Of course, we wildlife professionals are professionals in every sense of that word. Of course.

Hunting and Natural History

One of the components of the wildlife profession that remains more or less mandatory is hunting, a badge of the profession. Most wildlifers hunt, and one senses that those who do are at least more accepted than those who do not. It may also be true that those who do not hunt do not advance professionally at a pace as rapid as those who do. I am a hunter, and I have tried at various times to explain hunting to students who did not have the advantage of learning hunting from their fathers.

There is nothing that I would rather do than shoot decoyed ducks, providing that there is a dog in the blind that will retrieve ducks with skill and dispatch. I have flown peregrine falcons (*Falco peregrinus*) and other raptors since the middle of the last century, obligating myself to make the huge sacrifices that are the burden of all falconers for the very occasional successful flight at wild game. A peregrine stooping successfully from several hundred feet at a duck or pheasant or gray partridge (*Perdix perdix*) is absolutely exhilarating; occasionally it is a perfect poem. A northern goshawk (*Accipiter gentilis*) that has finally learned to take jackrabbits (*Lepus* sp., by grabbing the head and the hindquarters simultaneously) is a wonder to witness in action; the fact that she flies directly from the fist compounds the thrill.

But I would still rather shoot decoyed ducks than anything else I have tried—standing up or lying down.

In 31 years of teaching undergraduates, and spending long enough times in the field with some wonderful kids I often got to know quite well, I have never been able to explain my passion for hunting. Sometimes they can understand at least the framework of falconry, but I have never been able to explain hunting to the extent of convincing a nonhunting student that it is a legitimate pursuit. Even Jose Ortega y Gasset's wonderful aphorism, "We kill to hunt, we don't hunt to kill" (Ortega y Gasset 1972:110–111) hasn't helped. More often than not I risk rejection or sacrifice respect when I tackle the subject around a campfire. As a result, I gave up trying to make the case some 10 or 15 years ago. I just can't do it. I do not think it is possible.

On the other hand, I encounter no opposition, no philosophical or principled challenges, to fishing. For many years now I have taken great satisfaction in teaching a few students the basics of fly-fishing. After 2 weeks of netting and processing a thousand passerines in a bird-banding class in the mountains of Oregon, my students and I shift location and set up camp on a natural lake in another mountain range. The fish are mostly introduced rainbow trout (*Oncorhynchus mykiss*), but with an occasional stunning brook trout (*Salvelinus fontinalis*) that has been there over a winter or even longer. By picking my times, I can more or less guarantee that each of a dozen of these students will hook or catch at least 1 decent trout over a period of 3 or 4 days.

No one ever questions this activity (which is usually much crueler than hunting). Even the most resolute, proselytizing vegan can't wait to learn a simple roll cast, can't wait to learn to set that hook properly in a the lip of a fish, strip line and then retrieve it, land the fish, and let it suffocate on the worn grass at the edge of the lake.

THE MODERN WILDLIFE PROFESSION: OUR NATURAL HISTORY ROOTS

The modern wildlife profession reached adolescence just after the close of the Second World War, when thousands of veterans, flush with G.I. Bill money, went to college. These survivors (virtually all male) included a fairly high percentage of hunters and fishermen, and it wasn't long before academia had places for them to study. Such a deal! A profession doing something that you had learned to do from your dad, an avocational vocation.

Aldo Leopold—again, the patron saint of the profession—had written the first real text on the subject a generation earlier, a 2-pound book called *Game Management* (Leopold 1933). His classic *Sand County Almanac*—and his untimely death in a grass fire—were only a few years away (Leopold 1949). Leopold, as a faculty member at the University of Wisconsin, was from early on in his career a leader in the field. The growth of the wildlife profession would spread rapidly, especially to other Land Grant institutions, which later on would host the graduate programs and cooperative units that process the elite in the profession to this day.

My guess is that Aldo Leopold regularly rolls over in his modest grave at some of the circumstances and practices of modern wildlifery.

How would he feel about the easy resort to the predator control that he tried to teach us to use so very sparingly? What would he say about those National Wildlife Refuges that still host privately owned livestock to the clear detriment of wildlife? And how would he feel about the sniveling acquiescence to "social factors" and the denial that supports it?

And of course he would cheer many components of the modern wildlife profession: new methods of taxonomy, radiotelemetry, some statistics (perhaps), the pay scale, the expansion of the National Refuge System, the role of nongovernmental organizations such as Ducks Unlimited, the inclusion of women in the wildlife work force, and the Endangered Species Act. Perhaps he would particularly be pleased with the new status of programs recognized for nongame, or wildlife diversity. Both lists could be expanded.

But I think the thing he would most lament is the decline of the role of natural history in the study and practice of wildlife biology.

My undergraduate time at Berkeley came a little less than 10 years after Aldo Leopold's death. My major was wildlife conservation and my major advisor was a professor by the name of Aldo Starker Leopold, Aldo's first-born son, known as A. Starker Leopold.

Starker was a striking figure and a splendid teacher. He stood a bit over 6 feet, and he was stocky. His voice was sonorous, his elocution professional. And he was a bit of a dandy. On campus, he usually wore 2-toned perforated oxford shoes, and a suit with just a bit of a sporting air. His dark, swarthy skin owed much to his Hispanic mother, and his thinning but shiny, jet-black hair was parted right down the middle. Once, while working at something in the Museum of Vertebrate Zoology, where Starker held forth, I witnessed his flair when a young coed approached him, fairly gushing. "How was your trip to Africa, Dr. Leopold?" Starker smiled as he looked down at her, and his reply was well timed, perfectly delayed. "Magnificent," he said, slowly.

And Starker was a hunter. He hunted ducks at the base of the Marysville Buttes, on a private club. His hunting partner was Woody Middlekauf, an entomology professor. He hunted deer in California's Coast Range and elsewhere. One Friday in the early fall, all of us in his wildlife course were assembled in our classroom. I think there was a good chance that many of our classmates were looking forward to the lecture, as I was. In those days I said of Starker that notes were unnecessary; his words issued and simply plated out on my cerebral cortex.

Starker was a few minutes late, an unusual circumstance. The door opened, but the man who entered was wearing Levis' jeans and a bright plaid shirt. I don't remember the footwear, but it's safe to guess that the 2-toned oxfords had given way to boots. As he made his way to the lectern, jaws fell, and we looked at each other. This was an event, and not 1 for which we were prepared.

He smiled and said, "I'm dressed this way because I'm leaving to go either-sex deer hunting immediately after my lecture." The mid- and late 1950s were the beginning of the controversy over what most homemade experts called "doe hunting." Starker had done his homework. He knew the reality of the need to remove does from the deer population. He was pitted against a state legislator named Pauline Davis, and the fight was often down and dirty. But the wildlife science, and Starker, eventually prevailed.

Starker hunted quail with the McMillan brothers, 2 ranchers from the arid midsection of California who had worked with Starker and his students and colleagues to enhance habitat and water availability ("gallinaceous guzzlers" invented by another pioneer naturalist-wildlifer, Ben Glading [Glading 1943]) until there were huge populations of California quail (*Callipepla californica*) where before there had been only scattered small coveys. Eben and Ian McMillan ran fancy English pointers and sometimes hunted from horseback. Eben was the best naturalist I ever knew, a wheat farmer and cattleman who set standards so high that no one I've known since has even come close. It was he who introduced me to California condors (*Gymnogyps californianus*) and another of my mentors, Carl B. Koford, the dean of the old school of condor study. One of Starker's last books was *The California Quail* (Leopold 1977); Ian was to be the coauthor, but he pulled out at the last minute, fuming with Starker because he could not endorse the section Starker had written about fire as a management tool to enhance quail populations.

Starker had come to Berkeley to study under Joseph Grinnell, the legendary pioneer naturalist and evolutionist who had founded the Museum of Vertebrate Zoology shortly after the turn of the century. Starker arrived in Berkeley in late 1937; Grinnell died on 25 May 1939. In 1981, near the end of his life, Starker told me a number of fine anecdotes from his few meetings with Grinnell.

In my days as Starker's advisee, there was a tradition of work parties, and on 1 of these field trips, a group of us travelled to McMillan Country to plant *Atriplex* as quail cover and food. Starker's skills as a naturalist were evident constantly. He identified and otherwise commented on myriad plants and animals. At that time I didn't know *Sand County Almanac* well, and I had not yet read *Round River* (Leopold 1953). Later, I was able to understand Starker's expertise in terms of his father's teaching and the time he spent afield with his dad and his siblings.

Starker was renowned for many innovations; 1 of them was his introduction of liquor in general and the thermos Martini in particular to field trips. In Mexico, during the very extensive fieldwork he and his students did in preparation for his now classic *Wildlife of Mexico* (Leopold 1959), he baked fresh bread in camp every day and often took responsibility for much of the other cooking, at the same time he was holding up his end of the collecting and maintaining his journal in a beautiful hand that is indistinguishable from his father's in the original manuscript of *Sand County Almanac.*

Starker was an intellectual giant and effective politician far beyond normal expectations. He was the first and 1 of the few members of the wildlife profession to be elected to the National Academy of Sciences. My point is that this son of the founder of modern wildlife science was—like his father—a generalist of deep expertise who appreciated the beauty of everything he hunted and otherwise observed. He was indeed a naturalist.

National Wildlife Refuges: Unnatural History?

It can be argued that our National Wildlife Refuge System is the heart of wildlife biology in the United States. The now more than 500 units in this system showcase the manifestations of the development of our discipline. We expect that these refuges would depend on the science of wildlife biology to strongly influence, if not form, their policies and practices. We hope the best science would dictate the framework of conservation on these sites.

For several decades, I have had the privilege of teaching on several of these large refuges in the arid American West. I have known and worked with many professional personnel on these refuges, and I have battled with some managers over policy and management techniques. But mostly I have loved these places, treasured them for their wildness, landscapes, and beauty. I have done my level best to do my part to ensure that these gems will persist as the living museums they are, well into the future. In this process, I have seen a broad spectrum of behavior, bureaucratic and otherwise.

Malheur National Wildlife Refuge in eastern Oregon is widely considered 1 of the crown jewels of the Refuge system. Originally established in 1906 by Theodore Roosevelt, it is a diverse oasis in a sea of agriculture and abused rangelands. When I first visited Malheur, in 1966, the manager was John Scharff, who by that time had managed Malheur for 30 years. He had some unique alliances with local ranchers and maintained the headquarters compound like a private estate. He often hosted celebrities of various sorts. Supreme Court Justice William O. Douglas was a regular visitor at Scharff's grand home.

But when a friend and I appeared at the office (we were looking for prairie falcon [*Falco mexicanus*] nests, but of course didn't make that the centerpiece of any conversation), Scharff was eager to talk with us. He knew all the wildlife, not only the game species. It was a rich, rewarding meeting. I was reeling with new information as my friend and I headed a few miles east of headquarters to camp (at Scharff's suggestion) on the shores of Harney Lake, part of the refuge. We woke in the morning to the sight of 2 active golden eagle (*Aquila chrysaetos*) nests from where we had rolled out our sleeping bags.

Less than a decade later, Scharff was replaced by a much younger man who had risen in the bureaucracy and had been selected for the prize post because of his managerial skills. A nice enough guy, he had virtually no interest in wildlife, and he did not hunt. He lived in Burns, the nearest town, 35 miles away, and drove both ways daily through some to the most diverse and richest wildlife habitat in North America, along a route that I have traversed almost every Memorial Day weekend for the last 30 years. Hundreds of my students have learned more along those miles than on virtually any other similar stretch.

One morning when I was in the headquarters office, the new manager arrived in a state of great excitement and agitation. His voice was raised, and he acted as if he had had a vision, or at least a major epiphany. He had. He had seen a yellow-headed blackbird (*Xanthocephalus xanthocephalus*)—in fact several—and he said he had stopped and rolled down his pickup window to "get a better view." "And there was this cacophony of sound," he added, flailing his arms and rocking his head. He described the male blackbird (fairly straightforward!) and asked what it was. We wondered: What was this guy missing? Was that window routinely shut, along with his eyes and ears?

That epiphany about ended it. We looked forward to additional episodes of observation and excitement from this manager; if they came, they were few in number, lower key, and happened beyond my perception.

But there were many times when this manager's managerial mentality reared up and flew in the face of reason, conservation, and natural history. My conservation colleagues and I had been locked in a fight with Malheur National Wildlife Refuge about grazing. We lost the battles *and* the war (grazing persists and has in fact increased on Malheur), but we had some good shouting matches and were able regularly to demonstrate that denial and just plain lying often trump wildlife science when dealing with management policies. In other words, the science can say something quite unequivocally, but counts for nothing in the face of longstanding myths (e.g., "grazing benefits wildlife") and political pressure (the euphemism is "social factors").

This battle had been going on for about 8 years when I undertook a study of the snowy plover (*Charadrius alexandrinus*) in eastern Oregon and western Nevada (Herman et al. 1988). Harney Lake was 1 of the most important sites for nesting plovers, and so received much attention. Sometime in early April, we found our first plover nest, in the sandy gravel at the margin of this starkly beautiful alkaline sink lake. The first nest of a season is always a high for ornithologists, and this was no exception; John Bulger and I were thrilled.

I had already discussed at length the matter of grazing on this site, which was a Research Natural Area and a candidate for Wilderness status. "No problem," my manager friend had said "Grazing has been eliminated on that site; the cows are off."

The morning after our precious find, I went back to check for a complete clutch, and saw several cows nearby as I climbed the fence. Locating the cryptic nest site by means of almost equally cryptic locators, I was much deflated to find that the eggs were crushed, the nest substrate still damp with the contents. A cow's hoof had made a direct hit, and the trail was obvious. I shooed the cows away, wishing that the stones were larger.

Refuge Headquarters was only 40 minutes away, but I was there in 30 minutes. I ran into the manager's office, livid, on the threshold of hyperventilation. In no uncertain terms I told him the story at what I considered an appropriate, effective decibel level. Heaving and fuming, I waited for his answer, which I had fantasized would have to be an elaborate apology. Not at all. "Calm down, Steve, calm down. There's no reason for you to get upset. Those are only trespass cows." End of story.

His response was certainly bureaucratic and managerial. Had he also been a naturalist, or had training as a naturalist, or simply been imbued with the value of being a naturalist, his response probably would have been different. His toolbox for evaluating this situation was woefully depauperate. Missing from it was the esthetic aspect of natural history—appreciation of the beauty (or even the image) of snowy plovers, the setting of the surreal lake and its Pleistocene shoreline. My memory of the 2 golden eagle nests I had seen there 15 years earlier (1 was active that year), and the other wildlife, rounded out the picture. Missing too was his recognition that "his" refuge is a w*ildlife refuge*, not rangeland. Had he been a surgeon, he might as well have been doing surgery with a chain saw for a scalpel and without benefit of an anesthetic. And I'm sure he might have argued that *I* needed an anesthetic as our encounter progressed.

Is this appropriate behavior for an employee of the refuge system? If natural history-based wildlife biology cannot be practiced on our national wildlife refuges, where can it (or otherwise defined, science-based wildlife biology) be practiced?

I am tempted to tell the story of another refuge manager, also in Oregon, who presided over a quarter million acres (125,000 ha) that should have been a centerpiece of greater sage-grouse (*Centrocercus urophasianus*) protection and management. Instead he had, during the final several years of his tenure, arranged for an old ranch house on the refuge to serve as a base property for a buddy hobby rancher, thus opening the gate for him to qualify for a grazing permit on the refuge. It wasn't long before an already dreadful situation was significantly worse—grass understory gone, headcuts rampant, springs drying up, aspens (*Populus* spp.) prevented from reproducing. When I confronted this refuge manager with what I perceived as the facts, he told me, among other things, that he was "managing for horned larks" (*Eremophila alpestris*). No, I won't tell that story.

And in justice to the first manager I profiled, I must admit that he later saw the error of his ways, or at least had some second thoughts. Years after our encounters, after he had risen to a much loftier position within the U.S. Fish and Wildlife Service, he invited me to address all of the refuge managers in the Southwest Region. I was much humbled and deeply touched, thoroughly thankful for the opportunity, plagued with second thoughts myself, about my earlier behavior. And so I addressed these key players on the matter of grazing on public lands and National Wildlife Refuges. That was my mandate, and my talk was formed primarily from the scientific literature, although I did read a short poem at the end. The first manager to approach me after I had answered questions and left the podium identified himself as the manager of a prairie-chicken refuge, and he was excited: "Ya' know, Dr. Herman, when that little prairie-chicken hen hatches her brood, the first thing she looks for is a cattle trail, and if she couldn't find one she'd just lose all those chicks!"

After the meeting, my friend I'd known first in Oregon hosted a barbecue dinner party for a group of us at his lovely home overlooking the city. It was a very convivial affair, and once again, I was pleased to have been invited. When the time came for us to leave, my friend and his wife positioned themselves by the front door—receiving line style—and, shaking hands, chatted with each guest as he or she left. When I got to the appointed spot, I thanked my hosts sincerely, and my friend and I fell into reminiscing about the Old Days. The matter of the trespass cows came up, that led to mention of Harney Lake, and then I inappropriately lamented the fact that Harney Lake had not made the cut as a Wilderness Area. My friend's face fell, and tears rolled down from his disappointed eyes. The guests behind me in line began looking at their feet and elsewhere, and I rushed to reassure my unintended victim. Is anyone ever too old to learn to identify birds, or prepare a small mammal study skin?

The Role of Natural History in Modern Wildlife Curricula

When I bring up natural history, a common response is "Yeah, that oughta be taught in every grammar school." I always wonder if they think trigonometry and organic chemistry should also be dispensed with at that level.

Modern wildlife curricula are, in my view, top-heavy with statistics, chemistry, mathematics, and other complexities of little value to the practicing wildlife biologist. While it is often perceived that these courses are essential, their real role often is twofold: (1) to pack lecture halls for professors who couldn't attract and wouldn't have students if their courses were not required, and (2) to weed out students who declared wildlife majors in large part because they liked wildlife. Let's clear the decks for the real achievers! So that they can end up writing grazing permits and stocking pen-raised pheasants, fending off Endangered Species Act petitions, writing Environmental Impact Statements with the status quo as the most obviously attractive alternative, massaging sage-grouse counts, or concluding that pygmy rabbits (*Brachylagus idahoensis*) and cattle are "compatible." Forgive my cynicism; I focus on what are probably exceptions to the rule of honest, earnest, productive wildlife professionals, but we all know the other part of the spectrum exists, too.

It is my view that our wildlife curricula would benefit from significant revision. In addition to the inclusion of courses in natural history, which is fundamental education for students who must be competent at identification of vertebrates and higher plants in the field, they need to be educated in how to accomplish these, and related tasks, in a quantitative manner. Some of the chemistry and statistics and other hurdle courses might be replaced with courses in literature, even music appreciation. Good things could be done with seminars featuring much about Aldo Leopold, for example, and supplemented with material from naturalists such as Loren Eisley, Peter Matthiessen, E. O. Wilson, and similar authors who celebrate natural history in their writings.

Natural history is a component of literacy, and there is every reason why wildlife biologists should be literate in every sense of the word.

THE DISCONNECT BETWEEN MANAGEMENT AND NATURAL HISTORY

Managers have to manage. I have always been very selective about asking local resource managers to talk to my classes on field trips. At best this can be a useful way to introduce students to the wildlife field and to familiarize them with local problems and solutions. At worse, one risks letting some proselytizer natter on forever about his or her favorite weed problem and how he or she is going to solve it with pesticides, or how the unit is starved for personnel and funding, etc. In the absence of careful preparation, students mistake uniforms and assertiveness for authority and science.

Another reason I resist these encounters is that it is a lazy way to teach. Wildlife programs commonly use this technique. The professor gets a free pass and the refuge manager (for example) gets a free audience of young impressionable students who want eventually to work in a similar role; the status quo is served and strengthened.

And wildlife programs are places where training often substitutes for education.

A few years ago, I got stuck with just such an encounter, largely because it was set up by a colleague. The manager in this case was in charge of a large state forest and wildlife area. He stood on the road above the students, sun reflecting off parts of his uniform. He did a nice sketch of the history and mission of his unit, and then launched into a fiercely delivered xenophobic diatribe against an exotic weed that he perceived as a major threat to his charge. The volume of his delivery increased and his enunciation began to fail as foam accumulated in the corners of his mouth. When he finished, I thanked him and asked him a question: "How would you respond to the statement, 'Sometimes the best management is no management at all'?"

His jaw dropped and he seemed to have been struck dumb. He said nothing for a full minute or more, and then I could hear poorly defined noises emerging from his throat. He said he simply didn't have any idea how to answer my question. He was flabbergasted, and never did try to put together a reply.

All too often the passion to manage focuses on what is hoped to be a magic bullet, and all too often this is a pesticide. It may be something called Oust® for the alleged control of cheatgrass (*Bromus tectorum*), it might be the hugely toxic Endrin for grasshopper "control," or it might be 1 of many herbicides for the "control" of brush.

Silent Spring Redux

I was a *New Yorker* magazine subscriber when Rachel Carson's *Silent Spring* was first published there, in installments, in 1962. It was a bombshell. Corporate America was not slow to set demons upon it. Miss Carson, who had a Master's degree in biology, was called all sorts of names, but 1 of the most absurd charges was that she wasn't a scientist. The book was reviewed negatively in *Time* magazine and elsewhere; it came up in 1 of President Kennedy's news conferences. This is no place to retell the whole story, but suffice it to say that the science was solid, the descriptions of havoc were accurate, and the predictions were remarkably prescient. In short, the point was made: If these hugely toxic poisons were to be used at all, they should be used as stilettos, not as broadswords, and they should never be used except as a last resort.

At the time that book emerged, I was working as a technician for the Department of Biological Control at the University of California, Berkeley, so I was pre-adapted to accept and laud the message. Not so for many of my colleagues in entomology!

Years earlier, in 1957, I had had my own firsthand experiences with pesticides, when I was hired by a huge farming corporation in California's San Joaquin Valley as an economic entomologist. All summer long, I traipsed through cotton and alfalfa fields in that terrible heat looking for economically significant populations of insect and mite pests. It was my job to call the planes—the spray planes. I used quantitative, scientifically based techniques in my search, and, from this experience and others, I learned many things. The 2 most important were that (1) most pesticide applications are frivolous, unnecessary; and (2) there were plenty of negative effects on wildlife, including vertebrates (Herman and Bulger 1979).

Planes were spraying all around my fields, often laying down multiple applications of really toxic stuff on neighboring ranches. I couldn't find anything to spray, so I didn't call the planes. I wondered if I was doing something wrong, so I called on the chairman of the Department of Entomology and other academics at Berkeley to check my work. They came to the field. They checked my work. It was fine. But finally the pressure was too great. The spray plane company came to me and asked me if I was a fly fisherman. Yes. Well, they said, they would be pleased to fly me into an old strip on what was now a Wilderness Area, a place called Monache Meadow, high in the Sierra Nevada. I could stay for a weekend or longer, and they would pick me up, bring me back. No strings attached, of course, or even alluded to. Alas, I was too young (21) and far too idealistic. I declined, but, given such a proposition later in life, I probably could have done both, so to speak. The pesticide pushers were also upset. What if the word got out that Miller & Lux was cutting alfalfa without the expense of spraying? What if they were able to harvest cotton without attacking cotton bollworms (*Helicoverpa zea*) with their beloved DDT?

When dove season opened that year, I got involved with some hunters who were very good and very enthusiastic (I hunt doves only in as much privacy as I can find, and even then I have to apologize to my dog for the lack of work I produce for her). An organophosphate insecticide called Azodrin® had been introduced that year. It was used on cotton, and cotton fields were most of the mourning dove (*Zenaida macroura*) habitat in the valley. Rumors of dead doves and other birds in and around some fields were the buzz of the hunter aggregations that night, and they were followed by accounts of dead dogs—dogs that had entered some fields on retrieves. They dropped dead with doves in their (presumably) soft mouths. Eventually these stories were verified and expanded upon. The publications were written by California Department of Fish and Game personnel. The residue analyses depended on chemistry, but these analyses were done by chemists, not wildlifers.

Rachel Carson was dead by the time that the nefarious effects of DDE-induced eggshell thinning were first revealed and finally demonstrated beyond any doubt. Joseph Hickey, who replaced Aldo Leopold at the University of Wisconsin, was a prime mover in doing the research and making the case. He called the DDE-eggshell thinning story "Perhaps the best proven ecological fact of the 20th century." Without elaborating, I will just point out that a good portion of the wildlife community was slow to accept the reality of the relationship, not far behind the nozzleheads.

And so the lesson of *Silent Spring* was a harbinger of 1 of the most dismal wildlife tragedies in history—the population declines of many species of bird-eating and fish-eating birds. But out of that tragedy grew 1 of the greatest triumphs of our profession—the recovery of many of those populations, most especially the recovery of the peregrine falcon and its subsequent delisting as an endangered species. And this is another success story that owes its debt to natural history, to Derek Ratcliffe, the discoverer of eggshell thinning. Had this British naturalist not been broadly trained, the phenomenon might have gone unrevealed for another decade or so (Ratcliffe's germinal paper was published in the journal *Nature* [Ratcliffe 1967]). Naturalists are equipped to see the whole as well as the parts.

The data, the story, the framework are there. Are they not taught to wildlife students? Why would so many professionals so blithely accept a pesticide solution in the face of these and many, many other facts? Never underestimate either the power of denial or the comfort of acquiescence.

NATURAL HISTORY, OBSERVATION, AND MODERN WILDLIFE TECHNOLOGY

A natural history approach to wildlife studies requires the development of acute powers of observation, 1 component of which is patience. These skills are anchored in the business of learning to locate and identify terrestrial vertebrates, to find and key out plants, in short to scan a landscape and sort out its components. Students with identification skills are positioned to begin qualitative and quantitative observations of behavior, using nothing more than binoculars and field notebooks. Their familiarity with the landscapes they study becomes increasingly fine-grained as their catalogs of organisms and sightings swell. Behavior is movement, for the most part, and even the novice can describe it to his or her benefit and the benefit of others; even the beginner can sort out patterns of behavior in nature.

During the last couple of decades, I have regularly encountered students who want to initiate their research with radiotelemetry. They often believe that this spectacular technology is indispensable, even at the beginning of a study. My rule of thumb is that radiotelemetry should be employed only after as much of a question as possible has been answered with simple observation and other nonintrusive means.

Not many years ago, again on a major national wildlife refuge, a military-scale predator control program was mounted in an attempt to reduce predation on nesting sandhill cranes (*Grus canadensis*). Common ravens (*Corvus corax*), black-billed magpies (*Pica hudsonia*), coyotes (*Canis latrans*), badgers (*Taxidea taxus*), raccoons (*Procyon lotor*), and other potential and demonstrated violators were poisoned, dynamited (coyote dens), shot from helicopters, otherwise snuffed.

Ravens were considered the primary offenders. Their involvement was (supposedly) easily demonstrated by characteristic holes in crane eggs that had been eaten or preyed upon (please, not "predated" or, even worse, "depredated!"). But apparently no one had actually observed the predation in action.

I asked about this repeatedly because I thought that some valuable information might be gained by observing the actual interaction. How did the ravens pull this off? How did the cranes let this happen? Obviously, cranes and ravens had been neighbors in the same landscapes for at least millennia; obviously, both had somehow reproduced and endured.

The potential for success observing the dynamics between ravens and cranes was huge. The crane territories were mapped, and for the most part were easily observed from a good distance, by observers sitting on rimrocks with spotting scopes. I was ready to participate myself, and to engage eager students in the enterprise. The refuge was heavily grazed in the winter, and 1 hypothesis was that the cranes were obligated to nest in sparse cover, leaving their eggs uniquely vulnerable.

I wasn't suggesting that the refuge staff leave their vehicles long enough to do all of the observation themselves (although I certainly thought they should be involved). I had fleets of students anxious to do this and similar work, and offered them and myself up as volunteers.

But we were never taken up on the offer, and, as far as I know, no one there has ever observed the interaction of ravens poking holes in sandhill crane eggs. The expensive predator control program came and went. Large numbers of predators were taken out, money was made, and the predation, cranes, predators, and grazing persist to 1 extent or another. My observations suggest that the corvids were seriously impacted; I have seen many fewer ravens and magpies in the area during the many years since the predator pogrom was completed.

How much technology is enough or too much? During 1981, I was in Davis and Berkeley, California, collecting material for a biography of Joseph Grinnell. Starker Leopold had planned to study with Grinnell, and had arrived at Berkeley in 1937, as I have pointed out earlier. He met with Grinnell several times before Grinnell's untimely death in May 1939. Starker's doctoral project was a rigorous examination of factors that determined wildness in wild turkeys (*Meleagris gallopavo*; Leopold 1944). His breakthrough work showed that pen-raised turkeys developed a syndrome of physiological and behavioral traits that doomed them when released in the wild. The wildlife policy that resulted was a complete abandonment of reliance on pen-raised turkeys for wild turkey population restoration. One of the most stunning modern wildlife restoration accomplishments—the recovery of wild turkey populations throughout the continental United States—is anchored in the natural history of these birds as observed and interpreted by Leopold (1944).

I hadn't seen Starker for many years, and was much saddened by his condition when we crossed paths in 1981. I saw him first walking on campus. He was overweight, limping, obviously in pain. An Emeritus Professor by then, he was confined in a workspace so tiny that he and his office mate (the retired dean of the College of Forestry) often backed their chairs into each other when they pushed away from their antique desks. His body was fading, but the mind, the voice, the attitude remained. Our conversation was interrupted by several phone calls. Starker was putting together a Ducks Unlimited banquet, lining up wealthy donors. He would excuse himself, massage and oil the caller with his old smoothness, hang up, and return to me with anecdotes about Grinnell.

One of the things he told me was that Grinnell was suspicious of students using field glasses (binoculars were not widely available then). I can't remember the extent of this de facto prohibition, or if it extended to Grinnell himself (who, like most naturalists in his day, probably observed most of his birds down the barrel of a 12-gauge shotgun).

While Starker was talking with Grinnell in the living room of Grinnell's Berkeley home, a flock of goldfinches (*Carduelis* spp.) landed on a lawn across the street—really some distance away. Starker saw what we would now call American goldfinches (*C. tristis*), and so did Grinnell. But Grinnell leaned forward in his chair, to see a little better, and then pointed out to Starker a single (now) lesser goldfinch (*C. psaltria*) in the sizable flock.

No one I know would today question the use of optics to aid identification or observation, although the use of tape recorders to attract birds for closer examination remains controversial in both amateur and professional circles.

The potential negative effects of invasive, intrusive, often mutilative marking techniques in wildlife biology often are ignored or denied, and researchers seem to assume that a marked animal is the equivalent of its untrammeled cohort. Horror stories abound, as we all know, but somehow most of them evade the literature. But if God had meant a hatchling grouse to be encumbered with subcutaneous transmitters and a trailing antenna, He would have made them that way. No wonder survival studies amplify mortality and tend to implicate predation. The same thing is true of banded birds, but of course to a much lesser extent. Those little aluminum bracelets must mean that their bearers are at least slightly less fit than they were prior to the application of the band.

However, it must be said that those of us with Luddite tendencies often overstate our case. In 1985, while checking some bird boxes on a contract with the Forest Service, I was asked to help put a backpack transmitter on a great gray owl (*Strix nebulosa*). I had never before seen a great gray owl. I met a young technician at an appointed spot. She showed me my first great gray, then proceeded almost immediately to catch it with a long-handled net. I was assigned to hold it down while she applied the transmitter. Having heard for years how this largest of North American owls was really nothing but a bundle of feathers (much smaller underneath than a great horned owl [*Bubo virginianus*]), I was amazed at how much strength it took to hold it down.

The technician with the transmitter had never put 1 on an animal before. She worked from a printed set of instructions while I restrained her patient. My skepticism mounted as she checked the position of the straps, first on the instruction sheet, then on the bird. It was a complicated process, but finally the poor creature was outfitted with the transmitter and released.

It flew to a nearby tree and sat in plain view. I set up my camouflage camp stool (my daughters used to tell my students, "Watch out, Dad disappears when he sits down on his camouflage camp stool!") and my scope, notebook and pen in hand. I was going to document, minute by minute, the demise of this tortured bird.

For several minutes the bird was in mild disarray. The transmitter seemed a bit crooked, the antenna clearly awkward. The bird began preening, rearranging everything methodically. Twenty minutes later I couldn't see even a hint of the transmitter, and the antenna trailed out perfectly. Another 10 minutes and the bird flew away smoothly, mothlike as is typical for the species. Valuable data were gathered from that owl for weeks and months.

Confusing Research with Management

The confusion of research and management often takes the form of what amounts to experiments that fail the definition of that term because there are no controls. Radiotelemetry often falls into that category, often but not always because individuals without radios cannot be monitored (obviously). A particularly savage and dangerous example of such behavior comes from the history of the California Condor Recovery Program.

Soon after the initiation of this program, in 1980, the modern hands-on researchers sought permission from the state of California and the U.S. Fish and Wildlife Service to capture, apply patagial wing tags and radios to individuals of the precariously small (fewer than 30) remnant population of condors, as well as to take eggs from the wild and cage some individuals for captive breeding attempts.

Those of us who were suspicious of the proposed pace and potential hazards of these proposals (I chaired the California Condor Advisory Committee, which advised the California Department of Fish and Game and the California Fish and Game Commission) successfully restrained these intrusions for a number of years, favoring a slower pace that would leave some of the wild population in place while the captive breeding experiment proceeded, but opposing the handling and mutilation of these last wild condors.

Permission to trap and apply radios and patagial markers was eventually granted, and it was I who suggested an expert raptor trapper who, it seemed to me, was the person who could do the job with greatest safety to the birds. The program proceeded with what I perceived as ferocity, what the cagers saw as a glacial pace.

With a minimum of research on surrogate species, birds were trapped and draped with patagial wing tags (involving the perforation of the patagium) and radios on both wings. Tail mounts were considered but rejected. The argument that seemed to win the day was that the radios would allow any individual condors that "got in trouble" to be picked up immediately and ministered to. This was done in spite of the fact that the rocky canyons and generally rough terrain of condor country were perhaps the worse possible site for an experiment with radios on these birds during the 1980s. And it wasn't just the plan that was experimental: the radios were primitive, each radio an experiment in itself. I recall with hindsight clarity a report of 1 condor that lost 1 of its radios, and a few days later died in a collision with a tree. Then 6 condors were lost in a relatively short span of time, triggering the decision to bring all of the remaining condors into captivity.

At least 1 of those 6 lost condors wore a radio, but none of them were ever heard from again, nor have their remains been found.

These birds (and all of the captive-bred condors that have been released in recent years) were and are being "managed" in part by radiotelemetry. The extent to which this experiment impacted the project—and the species, negatively—will never be known with precision. The benefits of the radios are obvious, but what was the cost/benefit ratio, especially with those last few wild condors?

When political times get tough, patched-together research often substitutes for courageous, science-based management. "Stakeholder" committees often confect what may look to some like research, but what are in fact another means of reinventing the status quo. In a case I observed, 1 of these ponderous groups (that included otherwise sane resource agency workers) cooked up a scheme to "evaluate the compatibility of pygmy rabbits and domestic cattle (*Bos taurus*)." Observation had failed to tell them that a 1,600-pound (800-kg) animal that ate the same food as a burrowing animal of less than a pound (0.5 kg) might win the fight. The result is another captive breeding program, this time for pygmy rabbits. And simple observation finally won out over the "research and statistics." Rabbit burrows were collapsed and the habitat trashed; the cows came off, albeit perhaps too late.

Monitoring often is the bone tossed to environmentalists, the assurance that the project is real science. And environmentalists often are eager to accept it as a condition for the project to go forward. But the monitoring is seldom done, and those who flaunt it usually fail to set some threshold—potentially revealed by the monitoring—that will kill or alter the program.

Years ago, on yet another desert refuge in the West, the shrub–steppe vegetation had deteriorated so badly from grazing that the water tables in some of the meadows had dropped significantly. A project meant to restore the meadows was hatched and check dams were installed to boost and spread the water. No matter that these barriers would prevent fish passage.

The project was approved on the condition that there would be "annual monitoring" to assess the results. Six years after completion of the project, a bunch of us were brought to the remote site as part of a demonstration tour. The range conservationist in charge of the project spoke for some time. When he had finished, I asked him about the findings of the annual monitoring. "Well," he said, "We haven't been able to do any of that yet." I asked why. "Because we haven't had a single year since we did it that had rainfall identical to that of the year when we finished it, and the temperatures have been pretty different, too."

NARROWING THE GAP BETWEEN FIELD AND LABORATORY, OBSERVATION AND ANALYSIS

One of the problems plaguing our profession is the distance between various subdisciplines or tools that serve field work in the form of research, but which require special skills not normally found among or taught to wildlifers.

One such critical component is biostatistics. My experience is that a wildlife biologist and a biostatistician seldom occupy the same body and mind. I have suggested that statistics should not be such a huge requirement in wildlife curricula that it excludes students who have other skills or interests of equal or greater importance. This is not to advocate the elimination of statistics in courses of wildlife study, especially, of course, at the graduate level. Ideally, every wildlifer should know at least enough statistics to prevent himself or herself from being fooled by statistics that deny reality, as some applications of statistics often do.

We often farm statistics work out to desk-bound experts before (good) or after (often bad) research is done. Chances are pretty good that this person has the best of intentions but little familiarity with the organisms or landscapes involved, let alone the difficulties of obtaining a decent sample size or comparable plots for experiment and control.

Well, this person needs to *go to the field*. He or she needs to accompany the researchers in the field, be plugged into binoculars or receivers or other field necessities, and should be in on the choice of study sites, the protocol of data gathering, the rain, the snow, the impossible roads, the flat tires. And, of course, the beer. Only in this way can the normal fantasies of a biostatistician be dealt with, and his or her analysis be truly useful and honest. Something can be said, too, for the biologist exposing himself or herself to the skills of the statistician.

A similar schism has plagued the pesticide–wildlife field from the beginning, and mandates a modification of my attitude toward chemistry in wildlife curricula.

The problem is that field biologists and chemists usually live in different worlds. Wildlife parts collected in the field typically are frozen or otherwise preserved there and subsequently sent to chemists in a laboratory, where they are analyzed for pesticides by those other folks in white coats. Never the twain shall meet, and again, the problem of sample size, condition of the sample, even the biology of the organism being analyzed may be a problem for the chemist. And again, vice versa. The biologist is not likely to understand or be able to conduct the analytical gymnastics necessary to the answer he or she needs; more often than not the analytical laboratory does not beckon such folks.

In the early days of the DDT controversy, this problem was everywhere. Chemists weren't biologists, and biologists certainly were not chemists. Without perceiving the thin shells, I was (as a falconer) looking at the remnants of thin-shelled peregrine eggs in failed peregrine nests long before Derek Ratcliffe discovered and described eggshell thinning (Ratcliffe 1967). This near miracle led eventually to the near banning of DDT in this country (Ruckelshaus 1972), which in turn was the primary agent that had caused eggshell thinning in the first place.

I hated chemistry. I took organic chemistry so many times (3) that people accused me of having a minor in it. I spent the day of the final exam the first time I took it at a golden eagle nest. I saw no potential application for chemistry in my future. Then came my entomology experience, and, almost simultaneously, the peregrine falcon population crash and the eventual connection with pesticides, especially DDE, the environmental form of DDT.

I was studying pesticides and western grebes (now Clark's grebes [*Aechmophorus clarkii*]) at Clear Lake, California, as part of my Ph.D. program in the mid-1960s, when it became clear to those of us in the pesticide–wildlife field that we needed to get some North American peregrine tissues, and related materials, for analysis. It was at about this time that I met Robert W. Risebrough. Bob is the only person I've ever known who could span the gap between chemistry and field biology. During 1968, we flew twice to Baja California in a chartered light plane, ostensibly to find and collect grebes (finding none the first trip, we went back, ostensibly still hoping to find some). We neither found nor collected grebes, but we did collect all sorts of fish and potential prey items, osprey (*Pandion halieatus*) eggs, and least storm-petrels (*Oceanodroma microsoma*). Most significantly, we collected (with the help of a Mexican game warden) the first brown pelican (*Pelecanus occidentalis*) eggs that were analyzed for chlorinated hydrocarbons.

Risebrough is a brilliant analytical chemist and a consummate naturalist. He had a laboratory in Berkeley where I was soon to reengage with chemistry. A peregrine egg that had been collected in Baja shortly before our visit was analyzed in Bob's lab, along with a number of other important specimens. The resulting paper, which also first documented PCBs in North American wildlife, was the product (Risebrough et al. 1968). I spent the better part of the summer of 1969 learning gas chromatography in Bob's lab.

The following year I studied pesticide–wildlife problems in Guatemala and returned with pounds of sodium sulfate loaded with lipids from sundry samples, including a series of cattle egret (*Bubulcus ibis*) eggs from a heronry that had failed as a result of massive eggshell thinning. The next year, I started my teaching career at Evergreen. One of the first things I did was initiate the establishment of my own analytical chemistry lab.

We were ready when resource agencies got permission from Congress to exhume DDT from its coffin in 1974, to treat an infestation of a native insect—the Douglas-fir tussock moth (*Orgyia pseudotsugata*)—that had popped up, as it regularly did, in eastern Oregon, Washington, and parts of Idaho. No matter, of course, that the pest population was doomed by the time it was detected; a naturally occurring polyhedrosis virus was ravaging it and was, in the end, the primary factor leading to its demise.

An Environmental Protection Agency contract allowed me to field a dozen or so Evergreen undergraduates. Our job was to study the effects of the DDT application on nontarget organisms. The details of our findings are available in *Wildlife Monographs* 69 (Herman and Bulger 1979), but the relevant facts here are these: The work was basically an exercise in field natural history. Relying primarily on observation but buttressing that with quantitative techniques like spot-mapping for birds, dropcloths for insects, Surber samplers in streams, and so on, we dodged the agency and industry folks who were constantly after us and got some serious, rigorous science done.

Of course we collected all sorts of samples for residue analysis. And perhaps most remarkable is that the students who did the fieldwork were for the most part the ones who did the chemical analyses later that year. A chemist colleague trained virtually all of them to analyze in the laboratory what we had collected in the field, and it worked.

Our circumstances were unique, largely because we were unhampered by administrators in those early days of Evergreen. A similar feat would be impossible today, and I do not suggest that this sort of thing is possible elsewhere, at least to the extent that it was during my environmental and academic Camelot.

My point is that the integration and cross-pollination of normally disparate disciplines like field biology and statistics, or field biology and analytical chemistry, should always be goals in our profession. Our work inevitably is strengthened by the synergy that is the product of these extensions. Today the linkage between rapidly developing fields such as molecular genetics, classic wildlife biology, and management looms large, and natural history is at the root of all of them.

Finally, it seems to me that we would benefit not only from reuniting wildlife biology and natural history, but that we might want to resurrect some of the older terms, concepts, and goals. "Management" has become a catch-all, a cover for some things that may not be at all respectable. The U.S. Fish and Wildlife Service has an "Office of Information Management." "Wildlife Management" is a perfectly valid term, but doesn't it capture only part of our job in this new century? Shouldn't we bring back the term "Wildlife Conservation" and admit that preservation and sustainability are legitimate objectives in the wildlife field? We need to gear up for the end of hunting as we know it. We need to begin shifting gears to accommodate those who increasingly will be viewing wildlife through optics without crosshairs.

IN CONCLUSION

No, it's not only in grammar school where we should be teaching natural history. It's fine to start those kids out with an appreciation of the natural world at that young age, but the nuts and bolts of this very sophisticated discipline deserve to be taken much more seriously. This is not the simple stuff of memorizing different configurations of benzene rings—any compulsive personality with some time can do that. These subjects need to be part of our wildlife curricula, and those who teach them need to be *educated* as well as *trained* in the fundamental importance of natural history to wildlife management and research. And they need to be proud that they are teaching elements of natural history that link their students so clearly, and so appropriately, to giants like Charles Darwin, Aldo and Starker Leopold, and E. O. Wilson.

We would do well to remember that management is always a delicate thing, demanding skill and sensitivity rather than a formula. I would like to think management can include preservation; after all, passive management is a legitimate management strategy, perhaps especially on the huge and relatively wild refugia in the American West. Each of these precious places—whether they be National Wildlife Refuges, National Parks, National Forests, or Bureau of Land Management lands—is unique, and defies management by a generic formula. Eastern Oregon is not South Dakota, Texas is not even Arizona.

Our job is to protect beauty, whether or not we admit it. One of the primary indices by which our jobs and our landscapes must be judged is the extent to which we are able to protect or restore ecological circumstances that compare favorably to those conditions that prevailed before Europeans invaded this continent, before the invention of agriculture doomed wildness on much of this planet.

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