quiet copy

February 16, 1948

Mr. C. R. Gutermuth
Wildlife Management Institute
Investment Building
Washington, D. C.

Dear Pink:

Here is the paper "How and Why Research" that you asked me for.

I am sending it, plus an abstract, to Ed Graham, as you request, and also to Harrison Lewis who will be in the chair that day.

My operation is tomorrow. If I can't get to St. Louis I have requested Bob McCabe to read it for me.

I need hardl y explain that this paper aims to show the far-sightedness of a few outfits (like Delta) as compared with run-of-the-mill unit and P-R work. I realize that it is no small task to convince state administrators that P-R is unbalanced, but it has to start somehow, and you have given me the opportunity to start it.

With personal regards,

Yours sincerely,

AL:pm

Aldo Leopold

advance copy
confidential

North American Wildlife Conference
St. Louis, Missouri
March 8, 1948

WHY AND HOW RESEARCH?

Aldo Leopold

Much of the confusion about wildlife research arises, I think, from a false premise as to its purpose. It is often assumed that its sole purpose is to produce bigger crops. I challenge whether this should be the sole purpose, or even the main purpose. I suspect that too much emphasis on bigger crops is the least likely way to get bigger crops.

Understanding vs. Blood-and-Feathers

Daniel Boone had a stand of game to make any hunter's mouth water. Let us assume he had ten times as good a one as you or I have. Did he therefore have ten times as good a return from his day afield? I say no, because he had only a meagre understanding of wildlife. He had superlative skill in hunting, but the existence of wild things he accepted with as little thought as you or I accept the existence of a cloud, a breeze, or a sunset.

The wildlife crop, since Daniel Boone's day, has gone down, but the same scientific progress that brought about the decline has also added constantly to our understanding of wildlife, and hence to its value to us. Wildlife, to Boone, had only meat or trophy value.

Boone knew what a species was, but science itself did not then know where species came from. He had no inkling of distribution except within Virginia and Kentucky.

Boone knew migration, but he did not know that the April plover piping among his buffalo had just arrived from the pampas, and would next week pipe among the musk-oxen on the tundra.

Boone knew predation, but he never guessed that it benefited the prey as well as the predator; he had no concept of the intricate interdependence of wild things. To him, as to other pioneers, the only good varmint was a dead one.

Boone construed all animal behaviors as acts of volition; this is proven by his imputation of guilt or innocence to animals. Our present understanding of innate behavior patterns was then still a century in the offing.

Lastly, Boone had no inkling that the trillium he stepped upon had an evolutionary history as long as his own, and a <u>modus vivendi</u> as dramatic, and perhaps as important, as that of any buffalo, or Indian, or pioneer.

In short, Daniel Boone had little of that understanding of wild things which we possess, or are free to possess, today. It has grown on us so gradually that we are not conscious of it, or aware of how much it has added to the value of a day afield. Still less are we aware that its growth has only begun.

The primary purpose of wildlife research is, in my view, to develop and expand this understanding of the biotic drama. It must, of course, also contrive to keep wildlife on the map, in good quantity, and in as much diversity as possible.

I next show why it is more likely to yield something to shoot than the present insistence on immediate blood-and-feathers dividends.

The Futility of Short-cuts

It is an axiom in science that it is futile to attempt the practical in advance of the fundamental, because premature practicality is likely to end in a blind alley. Once in a blue moon research will, by accident, hit upon a discovery of practical value without any preliminary work on fundamentals, but when pursued as a policy, such accidental hits are a losing game.

This is why research on most American game species is in a blind alley today. The proof that we are in a blind alley is that we are unable to explain, much less to predict, current events. Show me the man who can explain the transcontinental pheasant depression of the last three years, or the recent peak in foxes, or the collapse of the jacksnipe in 1940. These events are enigmas. Add to them the grouse and rabbit cycle, which has always been an enigma, and the enigmas seem to be more prevalent than the understandings. In fact it could be said that deer and waterfowl are about the only major game groups in which current ups and downs can be explained, with confidence, in terms of visible causes.

I of course do not here refer to the slow declines arising from deterioration of environment, the causes of which are all too clear. I refer to quick changes. Environment seldom changes overnight.

In short, two decades of game research have exhausted the easy pickings. What do we do now?

Deep-digging vs. Practical Research

The thing for us to do now is what science always does in the same predicament: start over and dig deeper. Most game research is inhibited from digging deeper by a mistaken insistence on practicality by the Pittman-Robertson administration, and by the administration of the wildlife units.

Let me attempt a brief survey of what deep-digging research is going on today. At this stage of the game there are three earmarks; by one or more of which a deep-digging program may be identified.

The first is continuous yearly banding and census of sample populations. I could count on five fingers the research centres doing such work. It is notably scarce in P-R programs.

The second is continuous yearly sex and age classification of populations and bags. This has started in ducks, has long been active in big game, but is lamentably scarce in upland species. We even have formal "monographs" and bulletins without a defensible age-analysis.

The third is physiological exploration: the attempt to find inside the animal the reasons for his external behavior. There is hardly more of this now than ten years ago. Endocrinology, psychology, vitamin nutrition, and genetics are developing rapidly in zoology, ornithology, and animal husbandry, but not in game. Such physiological exploration as we have is almost confined to Patuxent and the "independent" universities and research stations. It is scarce in the units and in P-R programs.

It is hard to prove the reasons for this scarcity of deep-digging research. In my opinion it is because deep-digging is discouraged by those who hold the purse-strings (they having confused it with fumbling, which I will discuss later). In support of this opinion, let me cite a recent experience.

Wisconsin has a quail project, partially supported by P-R. It consists, in brief, of an attempt to find the reasons for the two big facts described by Errington, namely (1) threshold of security in winter loss, and (2) inversity in summer gain. Errington had to guess at the reasons because he did no continuous banding, and no continuous sex and age analysis. We now have six years of both, plus some physiological exploration.

Wisconsin was told, in 1947, that P-R support for this quail project could not be continued beyond the current year because it had delivered no practical results. In short, the very characters which, in my view, make this project valuable, disqualify it in the eyes of the P-R inspector.

Let me hasten to say that I put no blame on him. He was simply applying the approved policy of practicality.

Of course "practicality" and "deep-digging" are mere words; let me try to deal with the principle at issue. Any change in population level must arise from one of three cuases (1) something died, (2) something was never born, (3) environment changed. For reasons already cited, environment is ruled out as a cause for quick changes. What our Wisconsin quail project tries to do is to find out, quantitatively, what died, what was never born, and when. The only known way to find this out is continuous census, banding, sex and age analysis, and autopsy. The findings may extend far beyond quail. Wisconsin intends to go ahead with this project, but she may have to do so without P-R help.

Deep-digging vs. Fumbling

The gist of my argument is that the captains and the kings of research policy are confusing deep-digging with fumbling.

Let me concede without further delay that a costly lot of fumbling has hidden behind the bush of being deep. I have the impression that this fumbling has occurred among the "practical" boys, as well as among the deep-diggers. (The absurd statistics sometimes reported for state and national harvests of game are a good example.) I also concede that some excellent research work, including some of the deep-digging kind, has come to nothing because it was never published. I also concede that some excellent work has been published in such abstruse language as to be virtually unavailable. Of all these shortcomings, the first -- that is, straight fumbling -- is the most important.

Fumbling during the first decade of the wildlife research may be explained -- and dismissed -- as growing pains. It originated from many causes. One was the erroneous assumption that anybody with a sheepskin was competent to do research -- and that he could be paid a pittance. Another was the fact that trained men were very scarce, and often had to be chosen on the basis of what it was hoped they could do, rather than on the basis of what they had done. Another was the non-existence of standards whereby performances could be compared, and the virtual absence of inspection to do any comparing. Another was the instability and over-abundance of alphabetical conservation funds. Another was the notion that anyone who had "taken" freshman mathematics could analyze all wildlife data, and that statistical techniques were a peculiar fettish of our colleagues in the fisheries field. Most potent of all was the delusion that we knew more than we actually did. All of us fumbled, more or less, in the early days.

None of these alibis for fumbling exist today. To reduce fumbling is our most important job today. If we fail to reduce fumbling today, the well-springs of funds will dry up tomorrow. But to seek to reduce fumbling by putting a ban on deep-digging projects is scientific suicide. Such a policy is pulling our research gun through the fence, muzzle-first.

Balanced Research Programs

What I am asking for is a balanced program, which recognizes that some research jobs are short while others are long, and that the neglect of either is poor policy.

Some research leaders have seen this, and are doing an admirable job (sometimes surreptitiously) of combining the quick-easy with the long-slow approach. It is for such a balance, officially recognized and encouraged, that I am pleading.

To sum up: Good research gives us understanding of wildlife, and it may also add birds to the bag.

Research cannot be practical until it explains current events. In the species we cannot yet explain, deep-digging population analysis is the only known way to find out what dies, or what fails to be born.