

UNIVERSITY OF WISCONSIN
COLLEGE OF AGRICULTURE
MADISON, WISCONSIN

DEPARTMENT OF AGRICULTURAL ECONOMICS

1532 University Avenue
December 15, 1937

Dr. Paul L. Errington
Iowa State College
Ames, Iowa

Dear Paul:

My impression of your manuscript was that it is good, but not for the purpose of presenting your hypothesis.

If somebody else had presented the hypothesis and you were asked to write a commentary on it, your manuscript would be just right. It is not, however, a good presentation of the hypothesis.

How to make it a good presentation? One suggestion is to use an item-by-item comparison of the north and south. Thus you could weigh the cotton rat against the spermophile, and again you could weigh the scarce mouse population of the south against the abundant mouse population of the north.

As to details, I think you leave out a good many important points. I do not recall that you mention hibernation. You do not give any figures on comparative insect populations, whereas figures of some sort are available at least for the north.*

You speak of the cotton rat cycle, but as far as I am aware, no one has described it. Stoddard, of course, knows what it is, but your reader does not. Do you not have to describe it?

In many cases the verbiage seems unnecessarily technical. For example, "vertebrate predators" (are there any predators other than vertebrate?); "zones of influence" (you said that other ecologists had used the word "zones" in a parallel sense, but if they did I think they made a mistake. The word "zones" clearly implies geography, whereas you imply wholly qualitative differences. Wouldn't it be simpler to say "three kinds of situations"?).

You mention frigilline birds being more abundant in the north. My impression would be that in winter the exact opposite is the case.

At the end of Paragraph 1, I think the phrase "and naturally extended geographic range" is obscure. Why bring in this point at all? No one is sure you are dealing with extended range.


To make sure that my judgment was not too severe, I had both Frederick Hamerstrom and Albert Hochbaum give me an opinion on the manuscript without previous comment on my part. Both thought substantially as I did.

December 15, 1937

I am as keen as ever for you and Herbert to get this thing out and I am sorry I cannot be more optimistic as to its present status.

With best regards,

Yours sincerely,



Aldo Leopold
Professor of Game Management

P.S. Fred thinks that somewhere in your writings you have ventured the opinion that buffer levels have no effect on predation rate in the north. If you have, it is important that you state specifically in this paper that you have changed your mind.

Another point: Frederick and I have an unfavorable impression of the rather involved footnotes. We think there is no need to distinguish so sharply between material previously published and new material.

Another point: I think you should clearly call this whole thing a hypothesis rather than conclusions.

A.L.

*See "An Animal Census of Two Pastures and a Meadow in Northern New York," by George N. Wolcott. Ecological Monographs, Vol. 7, No. 1, January, 1937, pp. 2-90.