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COLLEGE OF AGRICULTURE
MADISON, WISCONSIN
6

DEPARTMENT OF WILDLIFE MANAGEMENT

424 UNIVERSITY FARM PLACE

September 4, 1943

Professor Paul L. Errington
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Iowa State College
Ames, Iowa

Dear Paul:

I've been down with a sciatic leg for nearly a month, hence the delay.

During that month Kabat has been:

(1) Checking his own diary against Albert's.
(2) Going over all the old diaries for a covey by covey survival history since 1929.
(3) Diagramming the 1942-43 covey history as Hanson did in 1941-42, and getting the weight history of each covey insofar as obtainable from banding records.
(4) Checking up on the exact location and history of pheasant populations, so we can be sure just what coveys could, or could not, have been subjected to competition. I think we are confused on this.

These data will reach you as soon as typed. Meanwhile it is clear to me that, except for census, Albert's journal can't be relied upon to give all the essential specifications all the time. This is no reflection on Albert. He simply lacks the technical man's practice in specifying conditions in words. Thus he repeatedly says "remains" (leaving the inference of a starved quail) when he means a pile of fresh feathers on top of new snows. He mentions fox tracks and fox flushes on only one third of the days these were actually seen by him and Kabat. (I suppose the daily repetition of fox visits to many coveys became monotonous, and seemed no longer worthy of mention.) He omitted general but important characteristics of the year, such as the nearly universal prevalence of corn shocks due to early snow and unfrozen soil.

These small points are enough to introduce frequent errors into "absentee" deductions, even by one as familiar with the area as you are.

If such errors crept into 1942-43 deductions, I think they may have crept into all the other years since you ceased checking up the area in person. This, as I recall, was 1935-36.

I fully realize that you sensed the baffling nature of the 1942-43 evidence. I by no means imply that either Kabat or I know the cause and nature of 1942-43 losses. I am saying that I have lost confidence in deductions (after 1936) based solely on Journal evidence. The third horizontal column in Table 3 "secure except for emergencies" is an example.

I am not saying there are no instances in which such deductions can be made. I am saying they can't be counted on often enough to fill out a table.

Since Bulletin 201 already details this method of analysis up to 1935-36, and if it can't be relied upon after, why should this paper use it at all? Why not limit ourselves to other kinds of evidence based on physical facts which can't be misconstrued, such as covey counts, weather records, etc. Covey history diagrams, such as the one Hanson made, certainly tell something. You discarded this method, as, of course, I am now discarding your method after 1936.

It seems to me, Paul, that we have two new and important facts to present: inversity, and whatever may come out of threshold. We are under no obligation to explain either one. The time to explain them is not now, but after 4-5 years of banding and age records. Here we are sweating blood about accounting for these facts, when our main business is to report these facts.

I suggest that we segregate facts from hypotheses in two different sections of this report. This is the only way to make clear what is fact, and what is hypothesis. I aim this sentence at my former manuscript, as well as at yours.

Let me give you an example of how hypothesis "sneaks in", and gets mixed with facts, when both are treated on the same page. Your manuscript proceeds on the hypothesis that substandard gains and extra large winter losses are linked. They could be linked only if both are internal, i.e. a property of the population, rather than external, i.e. a property of the environment. If they are internal, they could change from year to year; if external they could not change so rapidly. Your whole treatment of threshold assumes that rapid changes are possible, yet at its source, this is hypothetical. We don't even know that the breeding stock which registers a substandard gain was drawn from coveys suffering an extra large loss. Part of the breeders may conceivably have come from outside the area and suffered no loss. (Our banding continues to pile up evidence that the year-to-year movement of bobwhite is greater than that of pheasant.)

Another example of how hypothesis "sneaks in" is our assumption that because we can't see many changes in "food and cover" since 1929, that no great changes in environmental carrying capacity could have occurred. Meanwhile Albrecht's (Missouri) papers indicate that soil fertility changes may affect the welfare of animals profoundly, without any visible change in "food and cover". This, of course, would be a slow change, but it can't be left out of account in the hypothetical section of the paper. If you haven't seen the Albrecht papers, I will loan you my set. Our best soils men here take them seriously.

Another reason for revising at least parts of your MS is that Schorger's historical paper on Wisconsin quail will get into print before this does, and we can't afford to ignore it, especially in the hypothetical discussion of possible cycles. We can get access to it. The recent ups and downs ought, at least, to be compared with Schorger's history of ups and downs which goes back to the 1840's. Maybe some periodicity, or correlation with physical events, would show up.

I am not, in this letter, discussing our disagreement about the "clockwise behavior" of grouse and hares because I think it can be cleared up, provided we can see eye-to-eye on most of the changes that are mentioned here.

If the changes here mentioned sound as if they might be acceptable, I will attempt a new version incorporating them, but there will be parts I don't want (or am not able) to write. We'll have to throw the thing back and forth during actual construction.

If these changes are not acceptable, then I again urge you to assume sole authorship. Naturally all of Kabat's new analyses will be at your disposal in either event. I will return the MS if you decide to take over.

It was neglectful in me not to acknowledge safe arrival of the original notes and journals. Your 1943 notes are being added to the others.

Thanks for the Allee reprint. I read it very attentively. I have heard him speak on this subject, and heartily approve of his effort.

Yours as ever,

Aldo Leopold

Aldo Leopold

P.S. I've just started reading the mink-muskrat bulletin. I didn't know you were so far along with this; I'm glad of course that it's out.

PS This letter is an attempt to put on paper what seem to me to be the most important differences in viewpoint. I don't claim I am right in all of them, and I specifically admit I haven't studied the MS as carefully as I would have if I had not been sick, or as I intend to if we decide to make another joint attempt.