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COLLEGE OF AGRICULTURE
MADISON, WISCONSIN

DEPARTMENT OF WILDLIFE MANAGEMENT

424 University Farm Place

February 26, 1943

March 12

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

Dear Paul:

I am at last sending you the revised draft for Prairie du Sac. Some of the drawings still need redrafting, but the rough prints here used will convey the content.

Changes Made

Length. Your 35 pages are now 45.

Content. I have added the pyramid of numbers made possible by Hanson's censuses of cottontail, small rodents, songbirds, and other former gaps. There is no other important change in content.

Presentation. I now realize that when we started to write this paper, I was entirely unfamiliar with your accumulated findings, and I had only the most superficial grasp of such basic concepts as threshold. These successive drafts have been a process of learning for me, and it has been a liberal education. I do not claim to have grasped everything yet, but I think I have made headway.

You, on the other hand, have known "too much". In other cases where I have pondered too long on a given study I have found myself lapsing into "commentary", as distinguished from "exposition". My only criticism of your draft is that many parts of it are commentary rather than exposition.

Commentary is the style which one adopts at a seminar where the audience already knows the facts, and the only question to be discussed is: what do they mean? In exposition one starts further back and first communicates the facts. I insert this merely to define my terms.

I think it has been my own initial unfamiliarity with the facts that has enabled me to make this diagnosis of your draft. The present draft is my attempt to shift to an expository presentation.

I have tried to preserve your wording, but in many places I couldn't, and the above is the reason.

The only other important change in presentation is the total elimination of literary passages. I now realize that these have no place in a joint paper because they are too personal, and hence unfair to the other party.

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Data. There are two important changes: I have played down the Prairie du Sac ruffed grouse data because I became convinced that Albert's spring figures, at least in recent years, are not censuses at all, but simply guesses which he supplied when asked for them. He regarded ruffed grouse as on a par with the small mammals on which he was expected to supply only a general appraisal of population level.

The second change is the dropping of data from small areas, both in Wisconsin and Iowa. I think we would weaken our case by using these small areas. When I presented this paper at seminar, the reaction of mature and able research men like Buss and Hickey was that even the larger areas were far less conclusive than Prairie du Sac.

Findings. When we talked over the phone, I may have given you the impression that important differences of opinion existed. They now seem rather minor. I favor a more tentative attitude on the mechanism of inverse gains, mainly for the reason that sex and age data which will accrue, beginning with this winter's bandings, will make it possible to make a much better guess at the mechanism a few years from now.

The only other change in findings is that I have tried to hook up inverse gains to the cycle.

Running Comment

pp. 1-3. Area. Few changes, except that I have abandoned the long footnote on financial support by years and simply listed the supporters without trying to say what years each of them contributed.

pp. 3-4. Weather. Shortened. I will add the weather data for the present winter to Figure 1 as soon as the data are available. These data will be of particular interest and value, because the winter differed so markedly from all the others.

What do you think of adding a qualifier paragraph pointing out that our studies fell in a warm, dry period, and the findings might be modified with the return of cold, wet weather?

pp. 4-5. Censuses. I used Table 1 to try to shorten this section.

pp. 5-11. Structure of the Community. This is all new and becomes possible by reason of the additional censuses made by Hanson. The pyramids are based on 1941-42 rather than averages, because 1941-42 is the only year available in many groups.

pp. 13-16. Winter Loss. This may need revision by you. You may want to add or subtract references.

pp. 16-19. Decline in Threshold. Please examine the bottom of p. 18 to see if the 1941-42 survivals are allocated to the right territorial groups (Bulletin 201, p. 395). The present distribution of coveys departs somewhat from the former distribution, and you can make the allocations better than I can.

Figure 7 is not a final drawing. I will defer a final drawing until you have passed on the text.

p. 20. Summer Gain at Prairie du Sac. This is an example of a spot where fuller expository presentation was needed.

p. 21. Movements. Both you and I have in the past considered the area as substantially self-contained. A year ago Irv Buss and I started a special drive on quail banding, he working mainly in Dunn County, and I in this region. The year-to-year banding returns point to a decisively lower year-to-year return than in pheasants (around 10 per cent for quail, as against 30 per cent in pheasants), whereas the bursal agings show only a slightly higher turnover rate (20 per cent of old birds in quail, as against 30 per cent in pheasants). This must mean more year-to-year dispersion in quail. The quail data come from areas of 1000 to 3000 acres; hence, it seems extremely doubtful whether even large areas like Prairie du Sac are anywhere near self-contained.

pp. 22-25. Summer Gains of Other Areas and Species. Here I have dropped the small areas, and the new slant on movements summarized above is one of the reasons why.

The most important change in this section is to regard interspecific depression as suggested rather than proved. I am, of course, open to persuasion, and I realize that you may see something that I do not.

pp. 25-26. Summer Gain in Snowshoe. I think you were groping toward the idea of applying our method of graphic analysis to Green's Lake Alexander data because one of your work sheets summarizes the data. I saw Green the other day and found him wholly unaware of the characters revealed in this discussion. By the way he is writing up the Lake Alexander study as a book.

pp. 26-28. Mechanism of Depression. Here, again, you may have in the back of your mind arguments that have escaped me. I don't think we can avoid presenting all of the possible explanations. I am open-minded on changes in attitude toward each one.

p. 29. Is Bob-white Cyclic? I totally muffed your view of this until recently, but am now convinced that the Prairie du Sac curve can be explained without any resort to cyclic assumptions.

p. 30. Summer Gains in Cyclic Theory. I would feel on shaky ground in adding this section but for Elton's book and his new paper in the November 1942 Journal of Animal Ecology. I will not attempt to discuss the bearing of his findings unless you disagree.

pp. 32-33. Summary. I am not sure that these summaries interjected at the end of captions are advisable, but they do make it possible to omit a lot of detail from the main summary at the end of the paper. What do you think?

p. 35. Raptors. Is this comparison of population mechanisms sound? You know so much more about raptors than I do that I await your judgment.

p. 36. Foxes. Figure 11 is clearly a fragment from which we can draw no deductions, but which might be useful for some other investigator. Is this sufficient reason for including it?

Should we add to this section a qualifier paragraph explaining that red foxes were scarce during this study, and that our findings on predation might change during a period in which they are abundant? Gastrow says the present red fox predation on quail is extreme. I think the question hinges on whether your Iowa studies cover enough areas where red foxes were abundant. You can pass on this better than I.

p. 38. Rabbits. This is changed and expanded to fit Hanson's census.

p. 40. Rats, Mice, and Shrews. Table 3 is a brief summary of much more voluminous data in Hanson's thesis. He is as yet undecided whether he will contribute Table 3 to this joint paper or whether he will withdraw it for use solely for a paper of his own. Naturally, this is for him to say.

p. 41. Squirrels. Hanson will supply the blank figures. He is working at the Badger Ordnance and is hard for me to get in touch with.

p. 43. Winter Birds. A detailed analysis of summer bird population is omitted for two reasons: 1. Neither Hanson nor I could devise a way to reduce his summer census to a unit area basis. 2. A single year's work is probably an insufficient base for so complex a subject.

pp. 43-44. Summary. This is still rough and has a weak ending.

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References. I have deferred a complete revision of the references until you have decided what text references you wish to add or subtract. The present draft merely lists the titles I have added. For other titles, please see the numbers in your list.

I suspect that the handling of references in the text is still uneven, i.e. we have too many in some places and perhaps not enough in others.

Remarks

It seems probable that your reactions may be too bulky to be fully discussed by letter. I am willing to come over to Ames or meet you at some intermediate point, should you think this advantageous. If we need a meeting, I would like to have it before spring field work, spring planting, and other added loads which come in April. However, I am certainly not prodding you after taking this long to perform my end of the job.

I hope the present draft will untangle the quotations you need for your other paper.

I have no further thoughts on place of publication. Bulletins seem out, especially since the present legislature has cut all research appropriations, including a considerable part of my Department's. I don't think I will be dropped, but college money is scarcer than ever. Wisconsin Transactions and Midland Naturalist each seem a sure bet if we have nothing better.

Tracy Storer would doubtless take this if we broke it up into several serial sections which could then be handled as one reprint, as was done with Green and Evans' Lake Alexander reprint.

Whenever you have even a preliminary comment that can be communicated by letter, you might shoot it on over so that I may proceed with revisions. This would also help me know what part of the uncompleted drafting work can safely go ahead.

In closing I want to pay my respects to Bulletin 201 and the horned owl bulletin and to several papers which I fear I have not previously absorbed.

Yours as ever,

Aldo

Aldo Leopold

P. S. Your draft and data sheets are returned herewith in accordance with instructions in one of your recent letters.

Professor Paul Errington

P.S.S. Miscellaneous Memos for P.L.E.

p. 1. Area. We always used 3200 acres, which presumably is the sum of the covey territories Bull. 201, p. 395. Hanson found the total block covered by the bird census, farm census, etc. about 4500 acres.

Map. Do we need a map of the area? We have one, but it is rough drafting (copy attached)

Areal Tables. We also have a classification of cover types and crop acreages. Should this be added?

p. 3. Weather. I have not tried to assess the present winter. It may qualify as one of the "killing" ones. The data will be complete By April 1 and will then be added.

Latin names of animals and plants. I have left these out, thinking they could most economically be given, if needed, as a special list at the end of the paper, thus avoiding ~~XXX~~ duplication and cluttering of text. I suppose this would depend somewhat on where published. What is your preference?