

September 29, 1933

Mr. Herbert L. Stoddard,
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Thomasville, Georgia

Dear Herb:

Your letter and my m.s. arrived today and I certainly appreciate your prompt attention. My paper was an appalling mass to inflict upon anyone with any expectation of immediate reply.

In looking over your comments I find myself sunk by a consciousness of the utter futility of trying to discuss complex things by correspondence. I would give a considerable number of the few shekels I possess just to spend a couple days with you alone. It isn't that I want to force my stuff down your throat, but you are practically the only man in my field except Leopold with whom I can discuss many of these things down to minute details.

And I'll confess that at this stage I feel so far beyond my depth philosophically, that I'm sticking to the data as close as I can get. The data seem to be hinting things very largely at variance with what is commonly thought even by biologists. They seem to be hinting so insistently, however, that I don't see how they can be disregarded unless the data themselves are all wet - which I surely don't think to be the case, as they were gathered and published year by year without the remotest idea on my part as to what they would ultimately signify when considered collectively. And I'll confess further, that a number of my own views suffered reversals when I got the data spread out for analysis.

I would say that the data do not support the old conception that populations breed up to their available food supply. Indeed, I don't see how the adequacy of the food supply affects intrinsic carrying capacity at all except as it first influences the bobwhite's choice as to what will be a covey territory. Plainly the territory must have enough food so the covey will station itself there in the first place, but it does not follow that the food has to be enough to take the birds through.

Perhaps my definition of intrinsic carrying capacity is what is causing much of the confusion. I refer not the number of birds that an environment will succeed in wintering, but to the maximum number that it can possibly take through under optimum climatic conditions. This appears to be a rather constant and definite

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thing for my important observational areas on which I have done more than one year's work, with the sole exception of "A". Populations above the intrinsic carrying capacity seem winter-vulnerable to predation, whether Cooper's hawks and horned owls are present or not; if the population is vulnerable something seems bound to happen to it to the extent of its vulnerability. This, I think, is demonstrated by rough graph which I am inclosing. You may keep it if you wish.

I'm trying not to attempt to stretch my northern data to your southern quail country, but I have a dire and heretical suspicion that when the drouth you mention renders quail populations vulnerable to Cooper's hawks it also renders them vulnerable to enough other predators so that it makes little difference what gets them. Granted that the Coopers hawks might do the job in a week whereas the other predators would take materially longer, I suspect that the end result would be about the same so far as birds getting through the winter were concerned. Maybe not. I'm not pressing the point, for what I know about southern quail isn't too much.

I'm inclosing for your permanent record, if you want them, copies of my tabulated area data. It is upon the data in these tables, plus the food-cover area data already published, that I base my statements. I'll be eternally condemned if I can see any correlation between the predation rate data and the kinds and numbers either of predators or of buffers. The only thing that seems measurably to count is the density of the quail population in relation to the intrinsic carrying capacity of the land.

I'd like to have you look over the tabular data and see if they justify the conclusions that I express. Leopold thinks they do and believes that I have something entirely new and very significant. McAtee agrees with the conclusions but says he isn't surprised. Myself, I am still a bit unsettled, for this has meant the puncturing of former ideas which I had considered sound. But, in terms of my quail discourse, if the ideas were vulnerable to puncturing they might as well be punctured sooner as well as later.

I'm not permitting myself the luxury of any opinions on carrying capacity other than for the winter. I simply haven't any summer or fall population dope except inadequate nesting data and a very few reliable figures on reproductive resiliences. There is plenty here that could be looked into more deeply than I think has yet been done.

As to your suggestion about shortening the paper, I'm doing what I can about it. I believe I can cut out some of that deadly reading which is made up of presentation of area data in the text, since this material is given in the tables anyway. I still wish to present

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enough so that the reader will not have the feeling that I'm expecting him to take anything for granted.

I don't know what to do about those decimals you advised me to drop. They do give an impression of extreme accuracy which we both know to be impossible in some respects, but on the other hand I'm afraid that the paper would lose if I got rid of them all. While a decimal in a bird per 64.1 acres may be too finely drawn, a density of a bird per 3 acres is not the same as one per 2.6, etc.

I wish you would write me again and let me know if this letter has cleared anything up or has merely muddled matters more. And I don't think that I need tell you that I appreciate and respect your criticism, whether I agree with it or not.

Sincerely yours,

PLE*B

Paul L. Errington,
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