

May 9, 1933

Dr. W. T. Hornaday,  
Director of Campaign Activities,  
Permanent Wild Life Protection Fund,  
1 Bank St.,  
Stamford, Conn.

Dear Dr. Hornaday:

I was pleased to receive your letter of April 18 and would appreciate it if you would send me a copy of your book "Thirty Year's War for Wild Life". I have read it but would like a personal copy.

I have thought over carefully your suggestion to incorporate more opinion into my publications. From the standpoint of reaching the average reader you may be right; such a policy on my part would doubtless save the reader errors in interpretation but I'm much afraid that it wouldn't save me errors in presentation. So much of my work deals with ecological problems of increasingly intricate nature concerning which I feel we have no business to indulge in opinions, in view of the nebulous state of our knowledge; under circumstances of this sort about the most that a scientist is justified in doing is to write up the evidence as well as he can for the use of other workers in the field. After we have data, not only from one but many sources, and not only from one but many investigators, perhaps we will be in a better position to understand what the evidence really means.

So many of the old beliefs regarding factors determining plant and animal populations have been so drastically shaken during the past few years that I am fast coming to the conclusion that nobody knows a great deal about anything. For example, after four years intensive study of the ecology of the bobwhite, I have had, during the past few months, a growing consciousness of what looks like the emergence of a basic biological principle from the welter of data we have been able to glean and piece more or less together. If critical examination of the data at hand and those which follow bears out the present concept, it may be necessary to revise radically many of the ideas we have been in the habit of accepting as evident truths; if the data do not continue pointing in the direction that they seem, it is entirely likely that the concept will never get any farther than those of my associates with whom I have discussed it. If so, it will be just one more lead that didn't get anywhere, and I will not be forever kicking myself for going off half-cocked.



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In short, I am coming to regard opinion as a luxury which I, in a scientific capacity, cannot afford, though I confess that I am not yet entirely divorced from it. Practically all of the detected errors in my old publications -- and which would now cause me plenty of irritation if I didn't regard them as evidences of mental growth -- may be ascribed to attempts to stretch limited amounts of data into broad generalizations. Then, too, I often gain impressions which when checked over prove to be at variance with facts. Again and again I have caught myself doing this, and I see the same thing reflected in the attitudes of veteran out-doormen, whose ideas on some phases of wild life ecology are not infrequently a little ridiculous in the light of research findings.

It is not, then, a matter of modesty which causes me to soft-pedal opinion in my writings; it is simply that my opinion is too apt to be wrong, irrespective of what may be my rating and background as an investigator. Neither am I a timid creature nor one who cares over-much whether any given faction approves or doesn't approve of my findings. I may not always have such an independent viewpoint, but at any rate I have it now! What I will be and what I will believe in the future remains for the future to see.

Relative to the "shoot-'em-up" theory of many quail hunters, I imagine that my views are not very different from yours. I had occasion some months ago to answer an inquiry on this topic and am enclosing a carbon copy of my letter to save a detailed discussion here. Please return the carbon copy for it is the only one I have for my files. I hope that you will be able to make out the blurred print.

Specifically, in reply to your question, I might remark that shooting probably is conducive to slightly more dispersal than occurs naturally, but what of it? The quail do not need this "help", but, even if they did, a functional Cooper's hawk could do a far better job of dispersing and at far less cost to the covey. That scattered survivors from shot-out coveys start branch coveys is hardly substantiated by the exhaustive work of Stoddard, who found that the remnants of depleted coveys joined up!

So far as the theory of shooting causing quail to "breed better" is concerned, there is an element of truth in this but not what the hunters commonly think. Mere shooting will not raise quail populations above the level that they attain when unshot. The theory may reasonably arise from loose observation that quail populations after ascending up to the carrying capacity of the environment may remain roughly stationary though given complete protection from shooting, whereas moderately shot populations may maintain approximately the same level anyway. The theory may be made to look still more plausible by the biological phenomenon of rapid recovery (after



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excessive shooting or natural cataclysms) of populations greatly depleted but still well within the normal carrying capacity of the land; the reproductive resilience of comparatively light populations in environment inherently favorable is high, and decreases progressively as the environment is filled up and as increasing abundance brings on constantly increasing environmental pressure. This is just another phase of the so-called law of diminishing returns as it pertains to animal populations.

I wish to thank you, Dr. Hornaday, for the interest expressed in your letter, and trust that you will not be hesitant about writing me at any time. I really value honest criticism and weigh it conscientiously, even though my ultimate courses of action may often indicate the contrary.

I'll send you other reprints, etc., as they come out.

Sincerely yours,

PLE\*B

Paul L. Errington,  
Asst. Prof. Wild  
Life Research