

UNIVERSITY OF WISCONSIN
COLLEGE OF AGRICULTURE

MADISON, WISCONSIN

DEPARTMENT OF WILDLIFE MANAGEMENT

424 UNIVERSITY FARM PLACE

April 28, 1944

Professor Paul L. Errington

Iowa State College
Ames, Iowa

Dear Paul:

The 217 figure is the correct one. Kabat and Albert found they had counted one covey in two localities.

I think you are right in pointing out the need for a more logical division of function as between your paper and Kabat's.

We originally started with the idea that this paper would be the final interpretation of Prairie du Sac. This was my error as well as yours. Nothing can be final which is still being continued.

It seems to me a much more logical approach is this: You discovered the existence of threshold and of inversity, and are now reporting on their existence, together with such hypotheses as to their nature as you wish to set forth.

On the other hand I conceived, and have delegated to Kabat, the idea that banding analysis in conjunction with your techniques might, in time, shed additional light on the nature of the phenomena which you have discovered. Kabat will report on that later.

As long as we adhere to the premise that you are giving the final report on the nature of inversity and threshold, as well as on their existence, then of course you have need of all of Kabat's data, and are in an absurd position as long as you don't have it and use it. The answer seems to be to change the premise.

The details of 1943-44 history are the very essence of Kabat's enterprise, because they give him a banding analysis in conjunction with the previous methods of analysis.

I can't help but think there is a fundamental divergence of view as to what a grad student is working for. You speak of leaving the data "entirely usable for his thesis". We in Wisconsin never think of theses as anything but an academic routine. Certainly your proposed procedure would leave the data entirely usable for thesis, but my concern is not thesis at all, but Kabat's ultimate publication.

I don't want to assume, from a mere paper description, that your proposal would invade what to me is Kabat's field. On the other hand, I don't want you to assume that I am merely protecting a thesis. Neither can either of us assume that Kabat wants to commit himself on all the ramifications of 1943-44 at this time. Certainly the 1943-44 threshold was as low as the spring survival, but what this means Kabat might rather say about 1946.

(over)

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Your letter deals mainly with the needs of your paper. My mind deals mainly with the human needs involved. Supposing Stoddard had said to Wagner, in 1931, that he needed your first two years of unpublished data, because without them he could not give you a solid publication for future citation. What should Wagner have replied?

I repeat, Paul, what I said to you over the phone: I do not set myself up as the sole judge of proprieties. I agree with you that the MS itself will be the final criterion, but I want to make clear in advance from what viewpoint I will read the MS.

Dear Paul:

Just day before yesterday I saw one research administrator break the hearts and spirits of four students who had been lined up "end-to-end" on a long-time project like Prairie du Sac. His action was guided by a desire for the best possible immediate publication. I wouldn't be in that man's shoes for all the papers in Christendom. And I wouldn't give a nickel for the future prospects of his research station. He'll never get another good man to work for him.

Yours,

Aldo

P.S. I just wonder whether I have mistaken your meaning in some way. I remember with distinct approbation that you handled Hammy in

exactly the way that I hope to handle my students. I am not so confused in reconciling this perfectly evident history with the evidence no longer correspondence over the present question. I wish we could talk this out to be sure that we understand each other.

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