

Respected Vassilios Professor
Benj F. Ellington

Stenciled name

April 17, 1944

management) appropriate assignment to the present time I can believe
were shared in various places (including some sexerook on some
a number of beautifully labeled male bird and unprinted printed
from of. Indeed, when I did not own a microscope I was not
Professor Aldo Leopold here and I will surely be able to see
424 University Farm Place, 1st and 2nd, 1st and 2nd, 1st and 2nd
Madison 6, Wisconsin
Dear Aldo:

Among other things, I want to ask about the 1943 fall population
figure for P. du S. In your note of April 12, it is given as 217
birds, whereas in Kabat's report of February 24 it is 233. It is
quite important to decide which is valid, as with 233 the recovery
point falls only a little below the "ideal" curve (which, incident-
ally, conforms pretty closely to the famous Verhulst-Pearl curve
except that it flattens out much more at higher adult densities),
whereas 217 gives a point that lines up with the "off" points of
1930, 1940, and 1941 - the ones that I have suspected of defining a
curve of their own.

I am utterly uninformed as to the nature of Kabat's findings during
the past winter, but it certainly appears to me that the threshold
was again depressed to about the same extent as in 1940-41 and
1941-42, the winters following the last two depressions of summer
gains. Here, I may state, that, after going into the matter with
our Professor Brandner, the mathematician who has been fitting
curves for me, I am practically convinced that the depressed gains
and thresholds for the two year period, summer of 1940 through
winter of 1941-42, were very probably manifestations of the same
phase and also that the summer of 1942 and following winter -
severity of the late winter losses notwithstanding - did not repre-
sent any truly significant departure from the main pattern shown
by the 1931-36 data. Now if, in the summer of 1943, we get a
1940-41 type of depression of summer gain and this in turn is fol-
lowed by a depression of winter threshold of the 1940-42 type, it
would be at least very interesting, whatever may be reasons therefor.

In trying to plan my part of the Prairie du Sac contribution, I find
myself still in a mental deadlock on the question of not only how
much reference to 1943-44 would be correct to make but also how
much would be correct to leave out. I feel very strongly that, in
a paper that will probably take up 50 printed pages or more in a
scientific journal, it would be downright unethical to leave the
reader (or, let us say, the field) unnecessarily up in the air con-
cerning one of the major subjects (threshold phenomena) being

In the interests of the
war effort, we are using
both sides of the letterhead.

treated. This does not mean that Kabat should in any way whatever be deprived of any credit justly due him, either for his work in 1943-44 or any other time. But after taking the summary of findings through 1942-43, I can hardly go ahead and imply that nothing more is known if something more is known; and it would be nothing less than awkward to state in effect that more is known but that for reasons of divided jurisdiction or something like that I can't even mention what it is.

My suggestion as to a possible way out of this difficulty would be for Kabat to prepare for me a brief statement of his 1943-44 winter's findings on threshold phenomena, for quoting in my MS if and to what extent I felt it was needed to round out the treatment in a way that would be consistent with technical respectability. He could send this in the form of a dated letter from which direct quotation would be made, as is correctly done in scientific writings, irrespective of what ideas some people have about quoting only from "publications" (I am quoting from another letter of his concerning the winter of 1942-43); then the quoted statements could appropriately be followed by a passage suggesting that the reader, for a more extended account of the 1943-44 work, consult Kabat's forthcoming paper. If Kabat's statement contributes little that isn't already apparent from the 1929-43 data, or if the MS for other reasons really didn't need it, it could be left out. I am not at all desirous of introducing any more material into the MS if such can well be avoided but I want those subjects that fall into what I regard as the natural scope of the MS to be as solidly presented as possible.

And after all, you and Kabat will have your chance at the MS. In the event of continued and irreconcilable differences of opinion on factual and interpretive matters, we all will probably go our own sweet ways on our own responsibility, but I assure you that, in any respects concerning credit to Kabat and the extent to which his data will be used in my MS, I will let him (or you in his place) have the final word.

One other thing: In correspondence and conversation, you have repeatedly indicated that our use of the 1942-43 data ruin those data for Kabat. I can't see this at all. To my way of thinking, the limited reference made to that winter's studies, with due credit being given to Kabat for his special contributions to the discussion, leaves the data entirely usable for his thesis. With the detailed data still in his possession and for him to publish upon, I shouldn't think that a preliminary abstracting of a few high points would make any difference; at any rate, this is the sort of thing that is regularly done here and very nearly everywhere else that I know of. Indeed, when I did my own doctorate research at Wisconsin, a number of preliminary papers were put out and unpublished findings were quoted in various places (including your textbook on game management) without detriment to the thesis that I can perceive.

Sincerely yours,

Paul L. Errington,
Research Associate Professor.

PLE:FS