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DEPARTMENT OF WILDLIFE MANAGEMENT

424 UNIVERSITY FARM PLACE

September 5, 1944

Professor Paul L. Errington
Iowa State College
Ames, Iowa

Dear Paul:

Cyril has spent 4 days on the MS. and I have spent 2. We here attempt to give you what we can. No order of importance is implied in the arrangement of our comments.

General

The paper is superior in "tone" to any so far. It is nowhere dogmatic.

It is superior as a piece of writing. It is unlabored, simpler than previous ones, and flows smoothly (with local exceptions).

It is entirely satisfactory in respect of the use of Kabat's and Hanson's data.

It seems to us, for reasons given later, to impute to the data a precision they do not (in our opinion) possess, but this is partly offset by the admittedly speculative nature of your deductions.

The last half contains much new material. We do not purport to pass on all of this without further study.

The summary is inadequate as a picture of the work. The title is awkward and takes in too much territory.

Precision of the Basic Data.

Kabat has just finished a complete overhaul of Gastrow's Journal, including your annual comments and notes, for the post-1935 period. This overhaul raises in our minds the following questions, which pertain to the precision which may (in our opinion) be imputed to the data.

1. Dates of Fall Census. Weather, plus the vagaries incident to financing, and the laxity of supervision on my part, caused a large year-to-year variation in the date on which the fall census was begun, how long it was under way, and when it was "added up". Quail were of course dying and perhaps moving the while. You will doubtless agree that any attempt to correct for this variable would be unwise, but it introduces an error which might be larger than some of the distinctions subsequently drawn on rates of mortality and gain.

Thus in 1942-43 and 1943-44 the census began in October and was completed earlier than in most years of the 1936-1941 period. If it had begun in November, as in earlier years, the census might have been 5-10 % lower. Conversely, the 1929-30, 1932-33, and 1936-37 censuses began much too late to show any possible early season losses.

2. Adding Up Covey Counts. Unless you had correspondence with Gastrow which is not in our records, the Journal, during certain years, does not sustain some of the counts which we have both been using. In short, there is a percentage of surmise in any "footing up" of coveys, including our own, and this is probably larger during the years 1936-1941 when neither of us was on the ground. This debatable "margin" might be larger than some of the distinctions subsequently drawn.

Kabat's sheet, attached, indicates that this debatable margin would affect summer gain per cent during 4 years, and in one year (1937) it would double the gain.

3. Segregating Emergency Losses. There are at least three debatable points which enter different years in different combinations:

(a) In some years, as 1937-38, there was no projection of pre-emergency loss rates through residual post-emergency periods. We can't make out what your rule was on this.

(b) Some emergency weather clearly described in the journal seems to have been overlooked (as in 1940-41 and 1941-42).

(c) There remain the differences of interpretation already discussed.

The net effect yields the following comparison for years in which we differ:

Year	PLE	Kabat
1936-37	31%	10-17% ~~6-13%~~
1937-38	8%	17%
1938-39	16%	30%
1940-41	48%	34% or less
1941-42	54%	34%

Neither Kabat nor I imply that we have the same skill as you do in applying your criteria, but I think it is fair to say that you lack Kabat's familiarity with recent conditions on the area.

Kabat's supporting argument, insofar as it can be briefly stated, is attached.

This MS goes much further than any previous one in admitting that classification of losses is for most years not an objective process, but an exercise of judgment on which good men will disagree. I think your paper would gain by asserting this explicitly, and by allowing lesser weight to doubtful years. Some years are, of course, so clear that no difference of judgment is possible.

4. Pheasant Plantings. During the period 1939-1943 Art Boehmer released on his farm, in August of each year, 30-80 pheasants raised by the local sportsman's club. This might invalidate all of the summer gain data on pheasants. In addition, as you know, we do not regard Albert's pre-1942 pheasant counts as valid, especially those on spring survival.

Discussion (by AL)

It is, of course, impossible for me to estimate, without repeating all your analyses, the degree to which these questions affect the structure of your argument. My guess is that they cast doubt on the validity of your finer distinctions, such as three or four "phases" of depression, and I can't see how mathematical formulae can be spun from such inherently debatable data.

I suggest a section on possible errors just ahead of your summary. This would enable you to comment on how errors might carry through the chain of logic. You already cover in part where they might arise in the first instance.

I am much impressed by the possible linkage between depressed gains and depressed thresholds. You had mentioned this point before, but never developed it in a way I could grasp. I suspect there are still simpler clearer ways to develop it, but regardless of treatment, I regard the point as well worth making, even though you have to emphasize still further the debatability of the data.

Your treatment of King's data seems to me an extension of the "clockwise" progression in gains which I once proposed and you rejected. It seems to me to omit one point I made at that time, which seems to me still valid: that the reason for the progression of depressed gains in grouse, as distinguished from their random occurrence in quail, is that the latter are subject to weather mortality, and the former are not.

I think that you are not very magnanimous toward those who have dealt with cycles in terms of gross population. You certainly have added a new way to look at cycles, and I predict it will have great value, but the fact that something went haywire with many species about 1936 and about 1927 has been common knowledge for years. There is an inference, perhaps unintentional, that you discovered the fact. What you discovered is a new way to interpret the fact. You have also added some new species to the list.

I doubt the validity of all prairie chicken data for all small areas like Faville Grove and Hunt City. The combined population graphs are valid because chickens might depress other species, but the separate gain and loss graphs for chickens (as in Fig. 7) should either be omitted or more positively dismissed as probably meaningless. (See p. 44).

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Shouldering aside all detail, it is my opinion that this paper can be the best thing you have done if you can persuade yourself to go just a little further in seeing its weaknesses, in discarding the shakiest parts, and in pointing out the doubts elsewhere, and in avoiding over-fine distinctions. If I were you, I'd spend another month pruning it down.

Best regards,

Aldo (ak)

Aldo Leopold

P.S. Some running notes, mostly editorial, are attached. MS returned.

(Typed from A.L.'s handwritten letter and mailed during his absence to avoid delay.)

3 enclosures, plus MS.

encl. 1

Kabat's and Errington's figures on non-emergency losses, and Kabat's reasons for his interpretations for the following winters.

Winter	Non-emergency losses Errington	Kabat	Reasons
1936-37	31%	10-17%	A. Gastrow reported bad weather on Jan. 7. Drastic losses occurred in practically every covey shortly after this date. Only two coveys suffered any appreciable loss prior to the advent of bad weather. And one of these might have been shot at, according to Gastrow. The latter figure in column three represents pre-emergency losses which include the covey that was reported as being shot at.
1937-38	8%	30%	Unfavorable weather did not occur until the latter half of January. Losses up to mid-winter tallied up to 53 quail or about 30%. In order to get a figure similar to Errington's it would be necessary to designate the time prior to Dec. 11 as the pre-emergency period. If this were the case, in Errington's figures, there would then be a complete lack of projection of non-emergency losses for the period that constitutes the majority of the winter.
1938-39	16%	30%	A. Gastrow's Journal together with the Madison weather report indicate that losses due to adverse climatic conditions must have been at a minimum. Gastrow's only mention of bad weather was a blizzard on Mar. 18. However, the census before and after this date showed that no quail disappeared at this time. Therefore, all of the winter loss would be classed as non-emergency.
1940-41	48%	34% or less	I regard my quail figure of 34% as very conservative because 38 quail, or 14% of the total in the area, disappeared immediately after the Dec. sleet storm. Gastrow also states that the fencerows were snowed in all winter. Knowing the area as it is at present makes me realize that several coveys (S. of 60, Pulvemacher, and Schoolhouse among others) were destitute of any form of cover. Sudden decreases in the size of other coveys which occurred several times also indicated emergency losses. The 14% difference in Column two and three included only the losses sustained by the S. of 60, Pulvemacher, and Thompson coveys.
1941-42	54%	34% or less	Because 52 quail representing 20% of the winter loss occurred shortly after a severe blizzard on Jan. 2, I would place the emergency losses at no less than 20%. Gastrow's Journal and the Madison Weather Bureau show that more snow fell in 1941-42 than in the average winter.

The interpretation of the causes of winter losses in the winters of 1942-43 and 1943-44 have been discussed previously.

enc 2

Rates of Summer Gain as Calculated by Kabat, and Reasons for Departure from the Originals

Fall Density	Year	Summer Gain in Per Cent	Spring Density	Reasons for Disagreement with the Originals
140 145 PLE	1936	100 % 107 PLE	140 70. [illegible]	The only way in which it would be possible to get the original total on fall density of 145 would be to include a census count of 5 birds made on Dec. 9. Albert Gastrow called this small unit the Golf Ground covey. It was never flushed again. Two other coveys ranging in this vicinity were not found on this date. It is highly probable that the 5 birds belonged to one of these coveys.
158 163 PLE	1937	530 262 PLE	25 45 PLE	The fall density for this year was 158. The only possible way to get an approximation of the original figure of 163 is to include a census of what A. Gastrow thought was the Bongard (2) covey of 8 birds. This group was never flushed, but its tracks were seen just once on Dec. 27. It is very probable that these were made by another covey located in this area. The original spring count for this year was 45, my figure is 25. To get 45 one must include the Grieber and E. Boehmer coveys. The former was flushed for the last time on Jan. 21, and the latter on Feb. 8. Neither covey could be found on Feb. 25.
148 138 PLE	1938	280 254 PLE	39 39 PLE	The difference in fall density between my figure and the original can be attributed only to an error in either mine or the original calculations. Careful tabulations of A. Gastrow's census counts by me resulted in the figure of 148.
288 273	1940	116 105 PLE	133 133 PLE	My figure for the summer gain is based on a fall total of 288 rather than 273 as in the original. Notes, on which calculations were made in the original, failed to include the Pulvemacher covey of 15 quail.

enc 3

Errington MS

Title: I don't like the new one. One has to read twice to get meaning. Why not say "Internal Population Controls in Northern Bobwhite."? Pls note the word "controls." Find We cannot assume that what we study brackets the whole field of "population phenomena." It is presumptuous in us to do so. A pathologist likewise preempting the whole field would be resented by us, still there must be pathological population phenomena as well as psychological ones.

p13 (1) Suggest wording so as not to imply that legal protection equals actual protection. "...suffered little shooting on the area. They were legally protected throughout the study."

p13 (2) Suggest adding if you want to: "A local farmer has raised and released 30-80 pheasants on the area each year since 1939. The releases were in August."

p15 (3) As I have said before, I think that it should be said, preferably here, that while most winter movements in and out of the area were traced, that you know nothing of spring & fall movements in and out; that even if such movements are large, they probably ~~counterbalance~~ proceed both inwards and outward, and that the area is a valid sample of population levels in the region even if not self-contained. (Note: I think you disagree with this, but it is my opinion, and also Kabat's.)

This is done on p24.

p16. (4) I don't think we can say that pathology made no "extreme reductions", in summer, nor can we be sure "atypical immaturity" did not. Squealers may have died, on a large scale, at least during the 1935-41 period, before any fall census was made. Such losses would, to be sure, not affect the fall census figure, but they would affect the real population behavior.

I find the bottom of p15 and top of p16 rather obscure in their bearing on the main chain of argument. Only hazy spot so far.

Statement in MS refers to wintering bobwhites

p.18 (5) Not clear that "earlier" refers to earlier during the same winter.

p18 (6) "Responsiveness" not clear unless the reader actually reads the reference. "For data on how the horned owls responded to the presence of vulnerable quail during this period see 24 - - " etc. (am not sure myself without looking up the reference in full)

p19 (7) It is confusing for the reader to figure out when you begin with a new winter. Sidecaptions listing the winters in chronological order would help.

p19 (8) Same as (7).

p21 my paper not cited for 1935-36, though it remains in the bibliography (52).

p22 (9) Since no one was on the area except Albert, are you not obliged to support the food shortage of 1937-38 by some sort of evidence. Hauling in of cornshocks? Fall plowing? or was it buried by snow? Why did the food situation differ markedly from other winters?

p23 (10) "Even so" misleads the reader. What you mean is that the losses were even heavier than the circumstances would lead you to expect. At first reading it gives the contrary impression.

p29-30 (11) Too defensive. Nobody who knows anything is going to expect details of summer ecology.

p42 (12) I think "pattern" is hardly qualified for non-emerg. loss.
p44 (12a) Captions separating competitive species would help.
p45 (13) I am lost for three paragraphs here, perhaps because I am unfamiliar with winter loss graphs. I fear your readers will be. Need a separate graphs for the two "groups" of years?

p47 (14) Since the "summer gain" of pheasants since 1939 may have come out of chicken-wire, the inclusion of pheasants in summer gain computations may be vulnerable. Personally I doubt whether the plantings had any effect by November, but we can't prove they didn't.

p52 (15) Behind computations like Fig 10 is the assumption that summer gains are determined by mortality in young. While this is a probable assumption,

p.46 misspelling

it is by no means ~~a necessary~~ the only possible one. In pheasant we now have a wholly new and unsuspected behavior: laying of dozens of eggs without nesting. We simply know nothing about the summer period. I do not object to the speculation in Fig 10, but if inversity is a fact then Fig 10 is bound to line up as it does, under the assumptions you make.

You have me lost in saying that Fig 10 "fails to show straight proportion to density." To me it shows just that.

p53 (16) You spoil a good paper by two fine-spun deductions. Why not let it go as differing degrees of depression? Much simpler for reader.

In view of the possible errors, the spotting of particular years as representing particular degrees leaves you vulnerable. Even with the errors, your data probably show that different degrees of depression exist. That's the main point.