THE UNIVERSITY OF WISCONSIN
COLLEGE OF AGRICULTURE

Madison 6

DEPARTMENT OF WILDLIFE MANAGEMENT

424 UNIVERSITY FARM PLACE

December 17, 1947

Dr. Paul Errington
Department of Zoology
Iowa State College
Ames, Iowa

Dear Paul:

Jim Beer tells me at this late date that he prefers not to go to Chicago. Since the symposium is focused on age composition and its possibilities of throwing light on population mechanisms, I have to ask you to include his data in your speech. I have asked him to send you his data as soon as possible.

I am guessing as to whether or no this will upset your plans. I of course hope it won't. Jim should have made his decision earlier, but that can't be helped now.

I enjoyed our brief visit at Lafayette, and hope to be able to talk some more with you at Chicago. Thanks again for sharing your room with me.

Give Caroline my love and best Christmas wishes to both of you.

Yours as ever,

Aldo

AL:pm

Aldo Leopold

WEDNESDAY MORNING CONCURRENT SESSIONS A AND B,
DECEMBER 31.

9:30 A.M. ; Session A, Parlors A, B and C, Congress Hotel; Round-table Discussion on Bird and Mammal Population Mechanisms; Joint session with the Society for the Study of Evolution and the American Society of Naturalists.

ALDO LEOPOLD, Presiding

1. Introduction by Chairman. (15 minutes)

2. Buss, Irven O. , R.K.Meyer and Robert A. McCabe, Pheasant populations. (25 minutes)

   Discussion from the floor. (10 minutes)

3. Kabet, Cyril and Irven O. Buss. Bobwhite populations. (10 minutes)

4. Errington, Paul L. and James R. Beer. Muskrat Populations. (10 min.)

5. Hawkins, Arthur S. and Frank C. Bellrose, Jr. Duck Populations. (15 minutes)

6. Elder, William H. and Robert Smith. Canada Goose Population. (10 minutes)

7. Hale, James B. Cottontail populations. (10 minutes)

   Discussion of cottontial populations and general discussion from the floor. (15 minutes)

Ecological Society
Chicago 12/31/47

ROUND-TABLE DISCUSSION
Game and Fur Population Mechanisms

Introduction by Aldo Leopold

This discussion is intended to illustrate three points:

1. The causes of fluctuation in population levels are not yet understood.

2. The possible causes are so numerous that they must be narrowed down before the final search can begin.

3. Age and sex composition promises to accomplish the needed narrowing.

Fluctuations Are Not Understood.

A decade ago we wildlife managers thought we understood all major fluctuations except cycles; that is to say we were sure that food, cover, predation, weather and other "visible factors would ultimately explain all major changes except that rhythmic fluctuation characteristic of northern grouse, hares, and rabbits.

Today unexplained fluctuations are occurring in groups we once thought of as well understood. Here are some of the recent events which seem difficult to explain in the terms with which we are familiar.

The Pheasant Low. During the last three years the bottom has fallen out of this species. The decline was simultaneous in timing, and nearly transcontinental in extent. Even the fabulous Dakotas felt the pinch.

It seems unlikely that either predation, or weather in the ordinary sense, or agricultural changes would operate so uniformly in either space or time. Has the pheasant become cyclic? It is too early, of course, to answer this question. We know only that the decline coincided in time with the cyclic decline in grouse, and lacked the geographic spottiness characteristic of ordinary local ups and downs.

The Fox High. During the last three years there has been an upsurge of foxes, nearly transcontinental in scope. It still prevails in many states. Fox highs have occurred before, but transcontinental synchronism is either new south of Canada, or was not previously detected. A 10-year fox cycle has been supposed to prevail in Canada, at least since 1900. Is the Canadian fox cycle spreading southward?

Recessions. A peculiar population behavior, not yet named but here called recession, seems to take place in gallinaceous birds. It is confined to new transplantations or newly invaded ranges. Thus pheasants in parts of western New York collapsed in 1936 and have never regained previous levels. It is alleged that a similar recession followed their introduction to Oregon in 1881: they are said to have flared to great high and then receded to a lower level. Hungarian partridge in the Lake States, at first successful on certain soils, receded to a low level after 1936, (but there are now signs of recovery, so this may not be a recession). Pinnated grouse have invaded the Upper Peninsula of Michigan during the last two decades. They flared up and later receded, sometimes to the point of extinction, in a kind of west-to-east wave. A wave of sharptails followed, but this was clouded by artificial transplantations, and has not yet receded.

These cases suggest that recession is a type of population behavior which sometimes occurs in gallinaceous invaders. Recessions look like cyclic lows that stay low, and most of the dates coincide with cyclic declines. Could the present low in pheasants be a recession? That would be a calamity for those pheasant states in which intensive agriculture precludes falling back on native species.

Jacksnipe. In 1940 there was a collapse, evidently sudden, in the continental population of the Wilson's snipe or jacksnipe. A federal closed season for the ensuing 7 years has failed to bring more than a small degree of recovery (until perhaps in 1947). This bird was less heavily shot than the ducks, makes less use of the drouthy prairies for nesting, and as far as I know, is exempt from botulism.

Here, then, are four major changes that are not convincingly explained in terms of visible causes. Add the cyclic fluctuations which have always been an enigma, and there is little left that we seem to understand. In fact it might be said of the game groups that only deer and waterfowl remain explainable in terms of ordinary environmental factors.

Narrowing Down the Search for Causes.

Any major change in population level must ultimately boil down to one or more of three causes: (1) something died or was killed; (2) something was never born; (3) the habitat changed.

Habitat does not change overnight, nor does it change uniformly across large regions. Habitat therefore seems to be ruled out as the cause of such recent events as I have cited. The present search, therefore, is for what died, what was never born, or both.

I can think of only one possible way to trace what died in a wild population: census it for a period of years, find its age and sex composition for a period of years, and also band it if possible. The changes in composition should ultimately shed light on the changes in level, or vice versa. The banding should detect any movement affecting either composition or level.

How to trace what was never born is a more difficult matter, but some progress has been made. Ovarian evidence of egg-laying have been developed, but it was also discovered that egg-laying is no proof of reproduction, at least in pheasant, for many eggs are spilled at random. A parallel doubt applies to the use of placental scars as evidence of reproduction in mammals.

The speakers who follow me will tell what has been done so far to test the combination of census, composition, banding, and reading ovaries. Let it be clear that we make no pretense of reporting a finished job; in some species we have barely passed the initial stage of developing criteria of age. In order to reduce time, each speaker presents not only his own findings, but those of others, including some who have generously consented to our use of unpublished data. I hope the audience will listen and discuss these brief reports with two critical questions in mind:

(1) Will such work narrow down the field of conjecture as to what dies or was never born?

(2) If so, will this yield clues to population mechanisms?

PROGRAM

1. Introduction (10 minutes)

2. Pheasant Populations (25 minutes) Robert A. McCabe, University of Wisconsin.
   Speaking for: Irven O. Buss, Wisconsin Conservation Department,
   R. K. Meyer, Zoology Deparment, University of Wisconsin.
   James Kimball, South Dakota Conservation Department.
   Levi L. Mohler, Nebraska Conservation Department.
   Discussion from floor: 10 minutes.

3. Bobwhite Populations (10 minutes) Cyril Kabat, Wisconsin Conservation Depart.
   Speaking for: Irven O. Buss, Wisconsin Conservation Department.
   Donald R. Thompson, Wisconsin Conservation Department.
   Discussion from floor: 10 minutes.

4. Prairie Grouse Populations (10 minutes) G.A. Ammann, Michigan Conservation Department.
   Discussion from floor: 5 minutes.

5. Muskrat Populations (10 minutes) Paul L. Errington, Iowa State College.
   Speaking for: James R. Beer, Wildlife Department, University of Wisconsin.
   Discussion from floor: 10 minutes.

6. Duck Populations (15 minutes) Arthur S. Hawkins, U.S. Fish and Wildlife Service.
   Speaking for: Frank C. Bellrose, Illinois Natural History Survey.
   Robert Smith, U.S. Fish and Wildlife Service.

7. Canada Goose Populations (10 minutes) William H. Elder, University of Missouri.
   Speaking for: Robert Smith, U.S. Fish and Wildlife Service.
   Discussion of duck and goose populations from floor (10 minutes).

8. Cottontail Populations (10 minutes) James B. Hale, Wisconsin Conservation Department.
   Discussion of cottontail population and general discussion from the floor (10 minutes).

9. Summary by chairman (5 minutes).

Summary

My colleagues have now presented some samples of the new approach to wildlife populations. Its basic point of departure is the concept that a population is not merely a number, but an aggregate of age and sex classes in which the age and sex ratios portray, with mathematical accuracy, the current equilibrium of reproduction vs. mortality.

Like most innovations, this new approach is not really new. It doubtless had diverse points of origin, but I happen to be familiar with one of them. Western cowmen used this same approach to deduce population statistics for their "invisible" herds of feral cattle. They computed, from a known tally of calves branded and steers shipped, not only a census but a mortality rate, and they knew, without finding all the carcasses, at what ages and in which sex the mortality occurred, and this in turn enabled causes of mortality to be deduced, and one habitat or range to be compared with another for productivity. Such computations had sufficient precision to enable bankers to loan cold cash for the purchase of herds which had never been seen in their entirety, and never would be. Such computations even at times, resulted in the conviction of "rustlers" who could not produce evidence of breeding stock commensurate with their steer shipments.

The Forest Service later used this identical technique to compute the size of "invisible" herds of cattle, the owners of which were reluctant to pay full grazing fees. Many a court sustained or rejected suits which rested entirely on ratios between steer shipments (available in the freight office) and the herds which produced them.

Still later, when problems of excess deer and elk arose on the National Forests, the range managers quite naturally employed their cow-techniques to the computation of game censuses, the determination of age and sex compositions, and the computation of removals necessary to balance the herd with its available forage.

I suppose these ideas migrated from big game to the small game and fur field here discussed, but I offer this as a conjecture, not as an assertion, for we can't band an idea like a pheasant, nor does it offer a bursa or molt pattern for reading its age or previous condition of servitude. The taxonomy and genetics of ideas is still obscure: the best we can say is that like Topsy, they just grow up. Here is this one - What preliminary deductions can be drawn?

To me the clearest is this: that thin populations produce a higher percent of young than dense populations, and shot areas a higher percent than refuges. This inverse relation was originally reported by Errington in quail, but it is now visible in pheasants also.

An inconclusive but important deduction is the apparent extension of cycles to new species and new latitudes, and the hint that in muskrats litter size and intraspecific tolerance may vary with cyclic highs and lows.

Perhaps the most important and simplest item of progress is the standardization of age-classes. Incredible as it may seem we now know, for the first time, some sample age compositions, and some sample turnover periods (time requisite to to extinguish a generation).

Equally important but much more elusive is the composition of social units, such as quail coveys. When we know that a covey is two or more old birds plus the remnants of two or three broods, for each of which we know the birthday. We have a jumping-off point of great promise for future research.

I end this discussion with a plea for continuity. Let us be satisfied with nothing less than ten years of repetition of these population measurements. I hope that the captains and the kings who hold the purse-strings can see the tragedy of too many starts and too few finishings. We are dealing with a problem much more complex and difficult than the layman ever guessed, and persistence is the only hope for its ultimate solution.