A POPULATION SURVEY OF THE RING-NECKED PHEASANT IN MICHIGAN

Thesis for the Degree of Ph. D.

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RALPH AUSTIN MacMULLAN

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George J. Wallace
Major professor

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A POPULATION SURVEY OF THE RING-NECKED PHEASANT IN MICHIGAN

By

Ralph Austin MacMullan

AN ABSTRACT

Submitted to the School for Advanced Graduate Studies of
Michigan State University of Agriculture and
Applied Science in partial fulfillment of
the requirements for the degree of

DOCTOR OF PHILOSOPHY

Department of Zoology

1960

Approved George J. Wallace

ABSTRACT

This study of Michigan's pheasants from the standpoint of population dynamics had three objectives--(1) to reconstruct a history of past populations, (2) to acquire information on current population levels, and (3) to devise better sampling methods, when needed, for obtaining that information. The study was made chiefly during the years 1947-1950, but data were collected each year thereafter through 1956. The work was sponsored by Federal Aid in Wildlife Restoration Project, Michigan 38-R.

Although a number of private releases were made beginning in 1895, pheasants were not established until after 1918 when the State began a release program. Pheasants were well established by the early 1920's and the first pheasant open season for hunting was held in 1925. Reports from hunters were the best source of information prior to this study. The State's computed kill based on compulsory hunter reports was determined to be a good index to fall populations.

Pheasant distribution is outlined, and a correlation with land and soil formations described. Five study areas were selected. Each had a distinctive pheasant population and land formation, and in total comprised about three-quarters of the primary pheasant range.

Data were collected from extensive surveys made by sportsmen, farmers, biologists, rural mail carriers, and conservation officers.

Roadside surveys by the mail carriers and conservation officers were most useful, and complemented each other. The approximately 500 carriers who regularly cooperated provided good volume of data, but could be asked only infrequently to make surveys. The officers (about 55 in pheasant range) did not provide as large a volume of data, but could be asked to record observations over long periods to determine the effect of phenology on surveys.

Surveys were made during the four seasons. Data were recorded by county units, and examined by tabulations for study areas.

Spring--Crowing-cock counts, self-adjusting for phenology, were considered the most reliable for spring cock population estimates. Carriers' spring surveys of both pheasant density and sex ratio were sensitive to phenological differences. As the days progressed in mid-April, observed density increased. The carriers' counts were correlated to crowing-cock counts, suggesting a method for adjusting counts for phenology. Sex ratios obtained from observations of harems may be more nearly true than those obtained from all observations.

Summer--Brood density indices increased as the summer progressed from early June to mid-August, at a predictable rate, permitting adjustment for timing of brood counts. Summer brood counts by carriers showed an excellent correlation with fall kill. From the former, kill could have been predicted with an average error of 4 per cent (greatest error 15 per cent) in an 11-year period. Brood sizes reported by carriers did not change significantly from year to year.

Fall--September extensive roadside surveys during mid-day were valueless. Because of differential vulnerability of adults and young cocks to hunting, I was unable to determine true cock age ratios. Sex ratios reported by hunters were not valid. Hunter success data, discreetly used may yield valid indices to fall populations, but computed kill remains the best index.

Winter--Roadside observations of pheasants were correlated with snow depth. Regressions of cocks, hens, total birds and sex ratio noted by officers each day on daily average snow depths were plotted for two entire winters. The regression was apparently not linear; in addition the regression differed for cocks and hens, and hence sex ratios changed as snow depth increased. In no regressions did the Y-intercepts and the slopes show the same relationship. Interpretation of the data was hampered by a lack of a knowledge of the true population dealt with.

Populations trends by study area from 1937 to 1956 were reconstructed and discussed. Areas of lake-bed soil origin showed similar patterns, although widely separated geographically.

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PREFACE

In 1946, faced by a pheasant depression, the Game Division of the Michigan Department of Conservation set up a research project to survey the ring-necked pheasant (*Phasianus colchicus*) population in Michigan. Its purpose was to determine what things affect pheasant numbers--both in time and space. To do this it was also necessary to devise new census methods. This paper is a report on methods for estimating pheasant numbers and a reconstruction of past populations.

The study is not from the traditional life-history standpoint, but rather from the population dynamics standpoint.

Collection of Data

The bulk of the material for this report was collected while I was in charge of pheasant investigations for the Game Division from 1947 to 1950. This work was financed by Federal Aid in Wildlife Restoration Project W-38-R. J. P. Linduska was the first leader of this project and conducted surveys in 1946 and early 1947. After 1950 the Game Division continued many of the surveys that I had set up and by prior agreement I have used the data from them when necessary.

Status of Our Knowledge of Pheasant Biology in 1946

Pheasants had been managed on the preserves in Europe for many hundreds of years. Details concerning their propagation had been well worked out and we knew many basic life history details. Leopold *et al.*

(1943) had determined an average life span of wild pheasants from a population turnover study. Since pheasants are polygamous, a surplus of cocks could be hunted each fall with little prejudice to the next spring's breeding effort. Shick (1947) had found that in pens, one rooster could assure as good fertility in fifty hens as in three. Studies of the effects of hunting on pheasant populations indicated that hunting seasons restricted to cocks only could safely allow extremely heavy hunting pressure. Shick (1952) on the Prairie Farm had demonstrated that 200 gun hours per acre during a 22-day season could remove as many as 90 per cent of the cocks, leaving a sex ratio of 10 hens per cock with no apparent damage to the next year's production. Certainly it was well proven that Michigan's hunting seasons on cocks only were not seriously affecting pheasant populations.

Research workers had begun to work on census techniques. The habit of cocks of crowing repeatedly in early morning hours during the breeding season was being exploited to develop an index based on counting the number of crowings heard per unit of time. (McClure, 1945; Kimball, 1949) Roadside censuses were being tried with mixed success--mostly poor. Methods for aging pheasants as young of the year or older had been worked out (Linduska, 1943; 1945; Kimball, 1944).

The Extensive Survey

In 1946, when this project started, the pheasant had been established for only about 30 years in most of its American range. Although some research had been started in the 1930's (and interrupted by the war in the 1940's), to my knowledge no extensive study of state-wide populations had been undertaken.

From the outset, it was decided that this study should be extensive, dealing with state- or area-wide pheasant populations rather than an intensive survey of populations on small areas.

Most of the research on pheasants in North America had been intensive. Such studies had offered some clues to the population dynamics of pheasants. But when interpreted in terms of a state-wide pheasant population, intensive studies had been found wanting in two particulars:

1) Truly representative study areas are difficult to find. Data from studies in small areas are often inadequate or even misleading when interpreted in terms of the entire pheasant population of the region they are meant to represent.
2) Even in areas that may be representative, the small number of birds involved in intensive studies allows large magnification of error when conclusions are applied to a widespread population.

The intensive worker is also uncertain about the effect his activities may have on the observed animals and the possibility that factors peculiar to his study area are not common to the rest of the pheasant range.

An example of the inadequacies of intensive studies as indicators of state-wide populations is shown in Chapter 2; if one had used population figures from Rose Lake and the Prairie Farm (two quite thorough intensive studies in Michigan) as an index to population trends of pheasants in all of Michigan, he would have been misled.

The most important shortcoming of intensive studies as indicators of large populations is sample size; even though an intensive study provided accurate information on a pheasant population, we would probably need many dozens of such studies to represent the state adequately.

The extensive study is not meant to replace the intensive study, but rather to complement it and to answer more accurately questions too often improperly asked of the latter. Extensive studies may sometimes require intensive studies for interpretation and may point out the need for further investigation of certain life history phenomena.

Extensive surveys also have inherent shortcomings. They are dependent upon data which are often not gathered critically. In some cases observers are untrained and in virtually all cases data are gathered casually, incidental to other duties and subject to the voluntary cooperation of the observers. Despite the fact they cover wide areas, care must be taken to assure adequate sample size in extensive surveys, too.

Heretofore, these shortcomings had discouraged the use of data gathered extensively. The "roadside survey" developed in Iowa (Bennett and Hendrickson, 1938) and Pennsylvania (Randall and Bennett, 1939), in which trained observers drove selected routes on carefully selected dates and time of day, had been severely criticized and in some forms has been shown to be statistically inadequate (Fisher, Hiatt and Bergsen, 1947).

Notwithstanding these objections, the extensive survey was tried in Michigan, and has become a useful tool. Today, extensive surveys have become the basis for setting pheasant hunting regulations each year, and are the Game Division's best source of information on pheasant populations from season to season.

Acknowledgments

I am indebted to the Game Division and particularly to Mr. H. D. Ruhl, Chief, for the opportunity to conduct this study, to

J. P. Linduska who designed and supervised the first extensive pheasant surveys on this project and to F. H. Dale and D. W. Douglass who served as my supervisors. R. I. Blouch and V. S. Janson who followed me as leaders of this project continued a number of the surveys I had conducted, providing me with data over a longer span of years. I express my appreciation to L. L. Eberhardt for his help, advice and instruction in the statistical parts.

To professors G. J. Wallace, G. A. Petrides, and W. B. Drew, the members of my graduate committee, I offer my sincere thanks for their patience and encouragement during the long drawn-out period this study has taken, and for their help in the preparation of this report.

Many employees in the Conservation Department--biologists, officers, aides, and others--have contributed in advice and untold thousands of hours of work collecting data.

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Chapter 1

THE ESTABLISHMENT OF PHEASANTS IN MICHIGAN

1.1 First Releases (1895-1917)

On March 27, 1895, Mr. Arthur G. Baumgartel liberated several pairs of ring-necked pheasants on the Henry Harrington Farm at Harlem, in Ottawa County. This was the first recorded release in Michigan.[1] A brood was observed in the vicinity on August 24, 1895.

This release was not hastily conceived. For some years Baumgartel had considered the need for a new game bird. He and his friends had weighed the relative merits of pheasants and Hungarian partridge as suitable birds for Michigan. In 1893 he consulted with Emerson Hough, western representative of "Forest and Stream" and they decided that the Mongolian Pheasant was the best bet. He had tried to obtain wild birds from Oregon where they had already become established, but that State had already forbidden exportation of pheasants. So he purchased two pairs of pheasants from a private game farm in New Jersey in August, 1893. That year the Michigan Legislature passed a law protecting pheas-

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[1]This event is rather uniquely commemorated by a granite memorial on the Harrington Farm to the side of US-31, six miles north of Holland. On it is etched the outline of a cock pheasant, and this inscription: "THIS BOULDER COMMEMORATES THE FIRST / PLANTING OF PHEASANTS IN MICHIGAN. / THEY WERE RELEASED NEAR THIS SPOT / BY A. G. BAUMGARTEL / OF HOLLAND, MICHIGAN, MARCH 27, 1895. / ERECTED BY / HOLLAND FISH AND GAME CLUB / HOLLAND POINTER AND SETTER CLUB / NOVEMBER, 1940."

ants for 5 years. In 1894 Baumgartel organized the Holland Rod and Gun Club and the club posted a $5.00 reward for "information leading to the arrest and conviction of anyone shooting the pheasants."

In the fall of 1895 more birds were liberated. No known broods resulted from these releases, and it can be assumed that the birds disappeared shortly thereafter.

Wilson (1948) has described this first attempt to establish pheasants in Michigan and reports a number of other newspaper accounts of releases by private persons in the early 1900's. He concluded that stocking attempts were numerous with " . . . little more than a fraction of them succeeding." By "succeeding" Wilson probably meant breeding in the wild, for there is no evidence that any of these releases resulted in a permanent colony of pheasants, although quite probably some released birds did breed.

A search of the records of State organizations responsible for game matters shows no reference to pheasants prior to 1913. In 1914 the Game, Fish, and Forest Fire Department of the Public Domain Commission issued its first biennial report, which listed permits to keep game animals. In that 1913-14 biennial report, 2 of 158 permits were for pheasants. In 1915-16, 6 of 349 permits were for pheasants, most for one or two pairs.

There was no official magazine reporting on Conservation affairs in Michigan before 1922, but from 1913 to 1922 the "Michigan Sportsman," a commercial magazine, did a serviceable job of reporting on game matters. There are no references to pheasants in the incomplete volumes of this magazine available in Michigan libraries for the years 1913 through 1915. In the 1916 volume, however, the sponsors of the magazine were carrying on a vigorous campaign urging the State to establish a game farm for pheasants. Their campaign was thorough, their arguments many and optimistic. One of their most persuasive points was that New York State had only recently acquired a game farm and had succeeded in establishing thriving colonies of pheasants. It is significant that in their arguments as to why the pheasant should be able to adapt itself here, not one reference is made to a colony already established in Michigan. If there had been successful colonies, however small, it seems likely they would have been mentioned.

Perhaps the most revealing official statement on the status of pheasants before the State's release program in 1918 is one by D. R. Jones, Chief Deputy, Department of Conservation, in a letter to Baumgartel dated December 17, 1926:

> "Personally, we have no data other than that submitted by you as to the early introduction of these fine birds in the State. However, we do know that W. B. Mershon and some of his associates secured and liberated somewhere between twenty-five and fifty birds in the Saginaw valley country during the 90's, and apparently the planting was not successful, as no hunter or observer, so far as we know, ever reported seeing birds in the locality up until some time after distributions were being made from the State Game Farm . . .
>
> "There was also a small liberation of ring-necks made by public spirited sportsmen of Clarkston, Oakland County, about 1905 or 6 and it is reported that a few of these birds survived and hung on up to the time distribution was started from the State Game Farm . . .
>
> "Along about 1911 or 12 some hunter from Gladwin County, I think, sent in feathers found in the woods that were classified as English or Chinese ring-neck . . .

He concludes by saying:

> "It is safe to say that not one hunter in one thousand ever saw a ring-necked pheasant until after the distribution of the birds from the State Game Farm was started in 1917."

Thus, if there were any colonies of pheasants established in Michigan prior to 1918, they must have been very small and restricted.

1.2 The State's Release Program

The State purchased its present game farm at Mason in the fall of 1916. About 200 birds were purchased in the spring of 1917 and a stock of breeders was raised, but no birds were released. After a successful breeding season on the farm in 1918, 2,396 birds were released in the fall. Several thousand birds were released each year thereafter.

Part of the release program begun in 1918 was distributing eggs (and some day-old chicks) to cooperating farmers and sportsmen to hatch, raise, and release. While only a small percentage of these eggs resulted in birds released, such releases undoubtedly contributed to the stock, too. Pheasant releases were greatly reduced after 1951 and terminated in 1958.

1.3 Growth of the Pheasant Population

From 1918 through 1953, an average of about 24,000 eggs and chicks were distributed to cooperators and in addition an average of 6,700 grown pheasants were released by the State each fall (McCabe, *et al*., 1956).

Unfortunately, I can find no record of pheasant releases by county prior to about 1930. We know that most of the birds were released in the southern third of the state, but some were also released in the northern two-thirds.

The pheasants released in 1918 and 1919 must have done remarkably well. These birds, plus the second-generation progeny of the 1918 releases, produced a population in the fall of 1920 which prompted this rather remarkable observation in the preface of the 1919-20 biennial report (op. cit.) quoting opinions expressed by " . . . sportsmen who have gone afield during the open season, 1920."

> " . . . the introduction of the ring-necked pheasant to Michigan covert has proven successful to a degree exceeding expectations; . . . the experimental stage has been passed and the species established as a permanent game bird in the State."

In 1923 the Department of Conservation recommended a statewide season on pheasants (November 1-2 and 14-15) with bag limits of 2 per day, 4 in possession and 8 for the season. The legislature did not see fit to implement this recommendation. But the implication is obvious--after five years, in which perhaps 45,000 birds were released, pheasants were well enough established that game administrators recommended an open season.

In 1925 the season was opened in the entire state for 7 days October 25 to October 31, inclusive. There has been an open season each year since then.[1]

The biennial reports from 1916 to 1930 contain much discussion and speculation concerning the establishment of pheasants in Michigan. In view of what we now know about the distribution of pheasants, these early opinions are interesting and pertinent.

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[1]Leopold (1931) reports there was a 45-day open season on pheasants in 1910. I can find no basis for the statement in Michigan records, and conclude it is in error--possibly due to an erroneous report which confused the meaning of the words "partridge" and "pheasant." The season on ruffed grouse ("partridge") was open for 45 days in 1910.

Undoubtedly they represent many different viewpoints; they are subjective, and they may even occasionally reflect the unfounded optimism of politicians. But despite the absence of statistical support, they are often astute and even prophetic. Following is a chronological paraphrasing of statements in these biennials.

<u>1917-18</u>

John Baird, State Game, Fish, and Forest Fire Commissioner, stated that already the release of pheasants "appears to have solved the small game proposition for the lower counties of Michigan." He felt it was doubtful that pheasants would do as well "north of the Saginaw Valley" but intended, nevertheless, "to make every effort to distribute the pheasant throughout the entire area of the state."

<u>1919-20</u>

" . . . we have passed the experimental stage in the matter of establishment of ringnecked pheasants in Michigan,. . ." a remarkable statement to make after three years of releases.

> "The prime purpose of the Department in bringing the bird to Michigan was to provide a substitute for the native ruffed grouse, now almost exterminated in the depleted cover of the southern counties with their improved and sparsely wooded farming areas."

Farmers reported that pheasants were breeding prolifically in all cleared land of southern Michigan.

For the second time it was mentioned that pheasants were not expected to thrive in northern coverts, although administrators thought at the time that they were doing surprisingly well in the north. Expressions of doubt as to the suitability of pheasants for the north were based on fear of excessive predation as well as unsuitability of the range.

1921-22[1]

Although there still seemed to be some argument as to whether or not the pheasant was a "budder," observations indicated it was not, and hence the bird would be restricted to the southern third of the state where snow depths were not excessive.

1927-28

By 1929 it was concluded that pheasants were firmly established, that about 30 of the southernmost counties could be considered pheasant range, and that the game farm had fulfilled its main purpose of supplying

> " . . . breeding stocks which, when released, proceed to 'go wild,' increase of their own accord, and so continue to make and maintain satisfactory hunting."

Since repeated plantings in the north had failed, it was thought that further plantings would be useless unless the local people fed the birds in the winter. There was some doubt that even such care would maintain birds, but a large scale experiment in Manistee County was under way to see if such a system would work.

Department officials speculated that there might be about 100,000 pheasants in Michigan in 1928. This estimate was based on a pheasant range of 30 counties, or about 20,000 square miles, and the assumption that there must be at least 5 birds per square mile in this range.

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[1]Biennial report of the Department of Conservation, which was created in 1921.

By 1930 pheasants were apparently still expanding. In the fall of 1930, the State Conservation Department made the first official state-wide survey of pheasants. Conservation Officers throughout the state were asked to appraise their districts as "pheasant territory" using the terms "excellent, good, fair, poor, and hopeless." The map shown in Figure 1.1 shows the areas that administrators considered "good hunting." This conclusion is made in the 1929-30 biennial report:

> "A rather rolling country with tangled swales, unpastured wood lots, and brushy fence rows alternating with grain, clover and uncultivated fields, and where some ground is bare during most of the winter seems most favorable.
>
> "In most level areas, or where most of the land is cleared or farmed intensively, or in areas containing high percentages of wild woods or in deep snow districts, the pheasant seems to succeed poorly, if at all."

Despite the fact that this conclusion was based on uncritical opinions of a large number of non-professional observers spread over the entire state, usually with only one observer per area, the conclusion they reached was probably sound. It is quite likely more than coincidence that observers from the flat, heavy clay soils of lake-bed origin bordering the Great Lakes in southeastern Michigan should rather consistently report that country as poor pheasant range. That same lake-bed soil in southeastern Michigan later became the best pheasant range in Michigan and among the best in North America.

In 1934 officers were again asked to appraise their districts, this time with terms "abundant, scarce, or suitable for pheasants but none reported." Figure 1.2 shows the areas in which the officers said pheasants were abundant. These areas are roughly twice

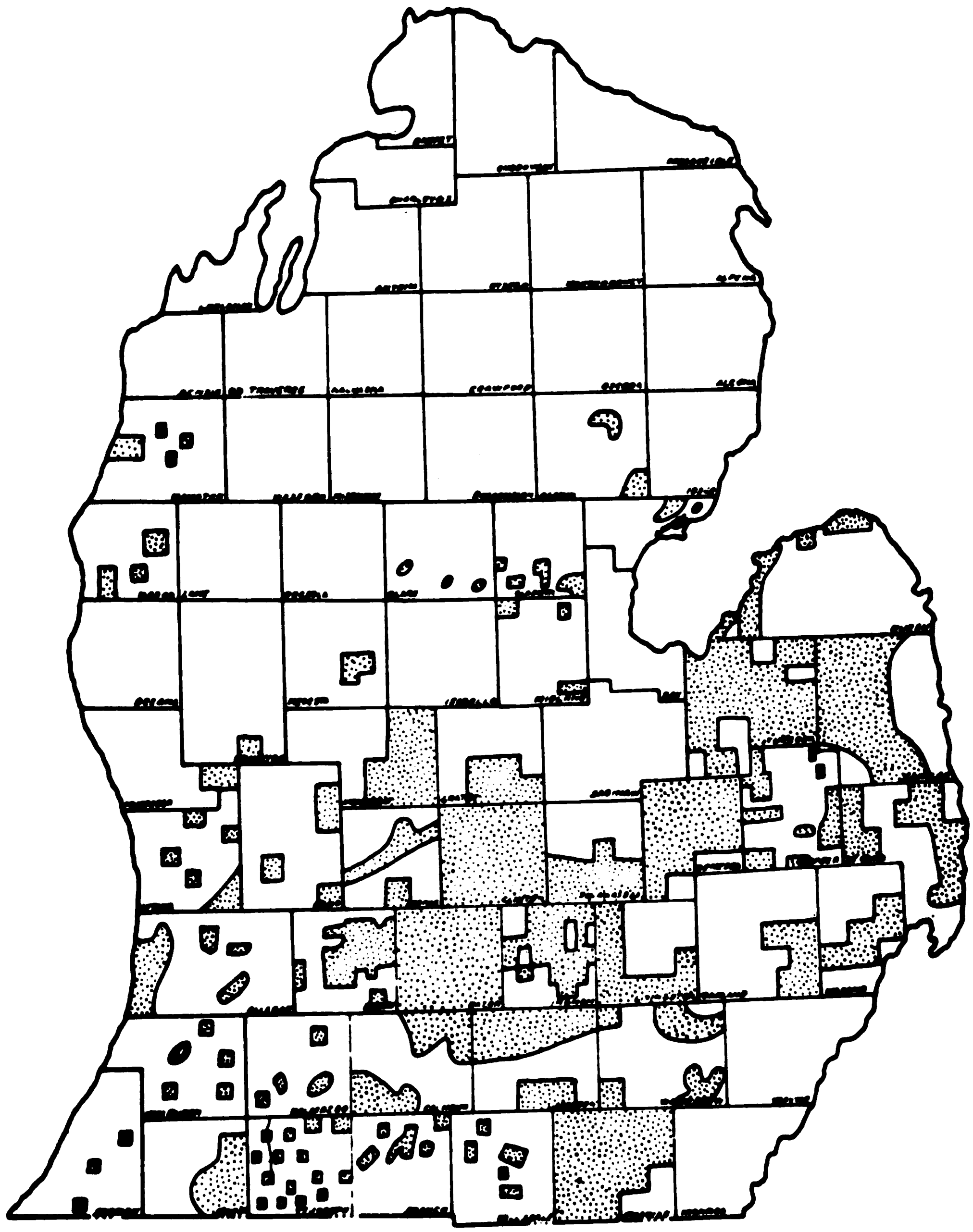

Fig. 1.1--Areas considered good pheasant hunting in 1930.

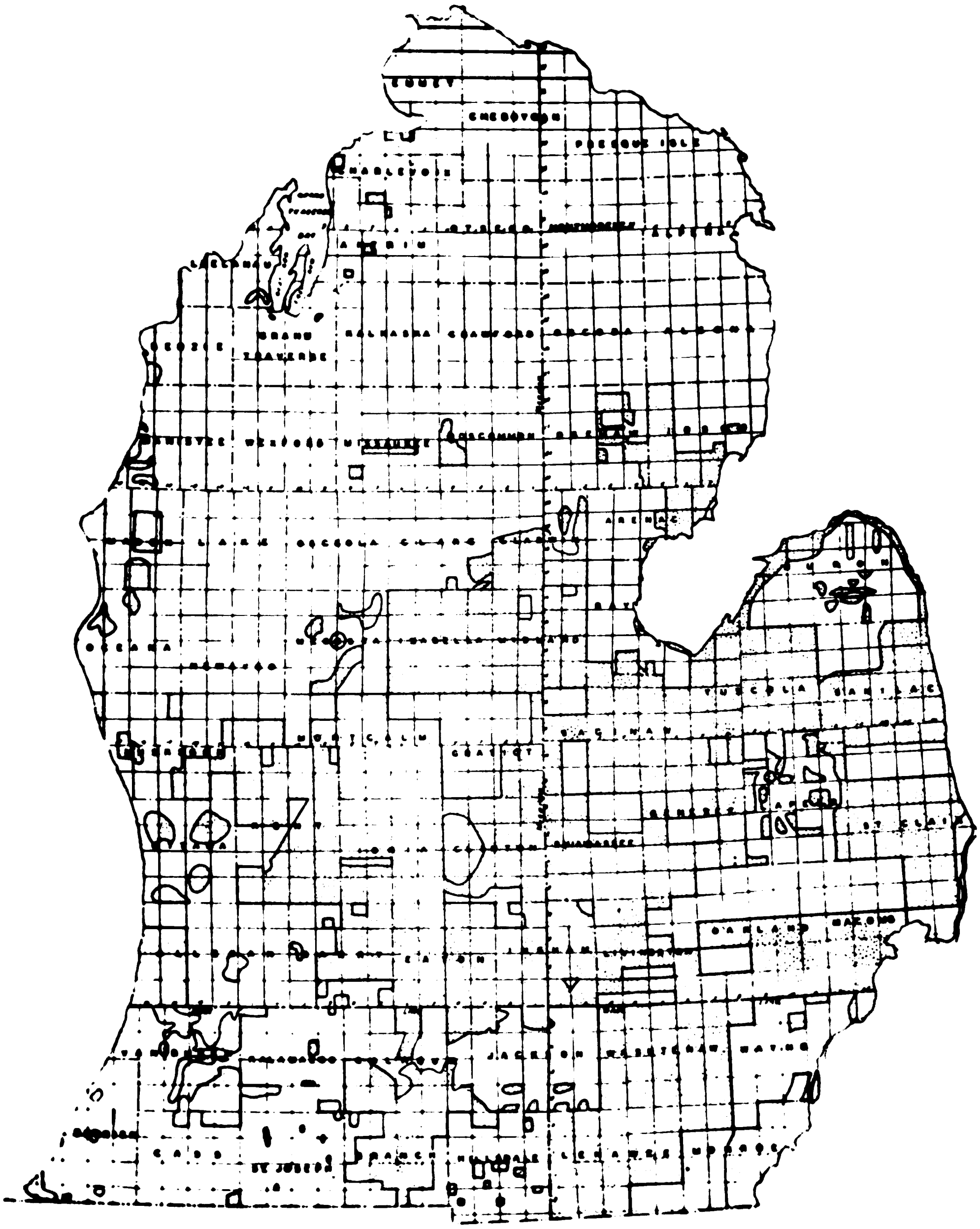

Fig. 1.2--Where pheasants were considered abundant in 1934.

the acreage classed as good hunting in 1930, although the 1934 classification is certainly more restrictive. This would indicate that pheasants had increased substantially from 1930 to 1934, and the lake-bed clay areas in southeastern Michigan apparently were beginning to produce many more pheasants.

The original records from which these maps were prepared have been destroyed, but the 1930 map is referred to in the 1929-30 biennial, in which it is stated rather conclusively that

> "It has become evident . . . that north of town line 20 little, if any, of the territory has proven capable of maintaining even fair hunting except locally and as a result of repeated and heavy plantings."

In spite of the fact that pheasant populations were still building up in the early 1930's, these two maps of pheasant distributions show quite clearly that pheasants had about spread to the limits of their range by that time. There are no large areas inhabited by pheasants today that were not colonized in 1930.

1.4 The Genetic Origin of Michigan's Pheasants

Michigan's pheasants are a mixture of a number of subspecies of the genus *Phasianus*. Examination of the plumage of cock pheasants picked at random from Michigan's pheasant range would show this mixed ancestry; rarely would one find a pheasant typical of any one subspecies.

Taxonomists and historians are not completely agreed on the derivation and taxonomic status of the species and subspecies of *Phasianus*. Introductions of several subspecies, freely interbreeding where their ranges overlap, from several widespread areas in Asia, and long confinement and artificial mixing and selection in game farms have thoroughly confused the genetic composition of the game farm birds, from which our wild birds come.

Delacour (1951) describes the pheasants brought into Europe and America as follows:

Phasianus colchicus torquatus is one of a group of 17 subspecies in eastern Asia. This subspecies (and presumably others closely allied) was the stock from which perhaps most of the introductions to England in the 18th century and America in the 19th century were made.

The so-called Mongolian Pheasant he describes as P. c. mongolicus, typical of the Kirghiz pheasants, a group of three subspecies in western Asia, far removed from Mongolia. They are not linked to the eastern Asia groups. He reports that Hazenbeck introduced some of these birds to England in 1900, and that from these the game farm stocks (in America as well as England) of Mongolian Pheasants were developed.

The game breeders' English black-neck pheasants are presumably the result of introductions of P. c. colchicus, (or one of the other two subspecies in this group of Caucasian pheasants) from the western edge of Asia.

Taxonomists agree that P. versicolor, the green pheasant, restricted to the islands of Japan, stands the test of a separate species.

The melanistic mutant (P. c. mut. tenebrosus) was developed in English game farms about 1880. It breeds true, with no intermediate forms, and appears to be exceptionally hardy.

Michigan pheasants most nearly resemble torquatus; commonly, cocks show coloration indicating mongolicus ancestry. Rarely one sees a bird that shows characteristics of versicolor.

Occasionally a cock is lacking the white neck ring. This may or may not reflect colchicus influence, since the white neck ring is a variable characteristic, and certain individuals of the 17 subspecies in the torquatus group of eastern Asia may lack it.

The Game Division receives perhaps a half a dozen reports each year of observations of white, or partially white pheasants. Some of these may be the result of albinism or natural mutation, but I feel that some may result from private releases or escapes from private holdings of the game breeders' fancy white pheasant.

There are some melanistic mutants in a small colony of pheasants around Rose City, in Ogemaw County. They are descendants of a private release in the middle 1930's. While these mutants are not common, the strain maintained itself at least 15 years, until the early 1950's, when the Game Division released a few dozen more game farm melanistic mutants in that area.

Hunt (1956) recalls that at the Mason State Game Farm in the spring of 1918 " . . . breeding stock of about 500 females [presumably Chinese ring-neck stock] . . . were mated to some very fine purebred Mongolian males." The releases starting in the fall of 1918 were undoubtedly successful, so we can assume Michigan's pheasants were strongly influenced by Mongolian stock from the first.

1.5 Establishment of Pheasants in Neighboring States

Pheasants apparently became well established in all of the Lake States (Wisconsin to Ohio) at about the same time. As in Michigan, private releases were made around the turn of the century, with but few succeeding. Indiana, Ohio, and Michigan were operating State release programs in the second decade of the century. Illinois and Wisconsin began serious release programs in 1928 and 1929, respectively, but other private releases had already established the birds in some areas. Leopold (1931) prepared a map showing the distributions of pheasants in the Lake States in 1928-29. From the map we can say that in general pheasants had:

1) become established and completed their major expansion in Ohio, Indiana, and Illinois,
2) reached the limits of their range but were still increasing in Michigan,
3) still some potential range in Wisconsin in which to spread.

We can also conclude that the establishment of pheasants in each of the Lake States was essentially independent of pheasant populations in adjoining states. Probably by 1940 pheasants had become established in all the Lake States range suitable to them. It is unlikely that any sizable new area will be colonized by the brand of pheasants we now have.

Chapter 2

VALIDITY OF ANNUAL PHEASANT KILL COMPUTATIONS

2.1 Introduction

When this study was begun in 1946, kill figures computed from small game hunters' compulsory reports were the only statistics that showed any promise as a reliable source of information on state-wide populations for the preceding years. This system had been in effect since 1937, and provided what appeared to be reasonable estimates of each fall's kill. There were, however, no other data on state-wide populations with which to evaluate this computed kill. There were a number of other surveys of doubtful value. One of the first objectives of the project, then, was to try to evaluate this computed kill as a measure of fall populations, and to investigate the possible usefulness of other kill surveys that had been made.

In the following sections, the computed kill figures and various other hunting season surveys are discussed, more or less in chronological order.

2.2 Bird Hunters' Tally Cards

In 1929, the Department started its first system for obtaining specific data on pheasant hunting in Michigan. Department workers distributed "Bird Hunters' Tally Cards" to hunters, who recorded such information as the hours and days they hunted, and the number of birds they saw and the number they shot. These cards were returned to the Department and tabulated. Tabulations from these tallies for the years 1929 through 1935 are summarized in Table 2.1.

At best, only a few hundred tally cards were returned to the Department, so the sample is extremely small. Nevertheless, the reporters may have been a rather consistent group whose reports could reasonably be compared from year to year.

Probably the best measure of pheasant populations from these tally cards is in terms of success indices (birds flushed or shot per gun hour). Any extension of kill per gun hour to total kill through calculations involving numbers of hunters seems impractical. In recent years (1937-1959) a 10 per cent change in numbers of licenses sold from one year to another has been unusual. But license sales during 1929-1935 fluctuated radically--a 55 per cent increase in 1933, and a 32 per cent decrease in 1931! There may well have been complicating factors not known to us today which were responsible for these fluctuations. For example, in 1931 the Department changed vendors from county clerks to Department-selected private dealers. Whatever the reasons for these changes in license sales, suspicion of their validity as a source of data on numbers of hunters precludes their use as a factor in estimation of total kill.

If this appraisal is of any value, we might interpret the period of 1930-1935 as one in which pheasants were increasing, with the suggestion of a slight slump in 1935. In 1935, the last year of this survey, publication of the tally card in newspapers did not result in a much larger return, so the system was abandoned.

TABLE 2.1

PHEASANT HUNTING DATA FROM BIRD HUNTERS' TALLY CARDS[1]
1929-1935

Year	Days in Season	Season Bag Limit	No. of Reports	Hunter-days	Gun-hours	Cocks Bagged	Birds Flushed	Kill per Gun-hour	Birds Flushed/ Gun-hour	License Sales
1929	7	4						.19		340,801
1930	7	4	462	609	2,997	420	2,971	.14	.99	332,726
1931	12	4						.15	1.13	244,860
1932	12	4						.15	1.05	214,318
1933	12	4	299	1,843	8,782	923	9,353	.11	1.06	332,149
1934	12	4	611	2,456	12,839	1,637	16,986	.13	1.32	346,609
1935	13	6	291	2,507	11,593	1,400	10,557	.12	.91	385,554

[1] Data for some years were calculated incorrectly and were incorrect in the Biennial, but have been corrected in this table.

2.3 Kill Reported on License Stubs

During this same period another kill-reporting system was in operation, apparently more or less experimentally. When a hunter bought his small game license, he was asked to record on a stub the number of pieces of game he had killed the previous year. I can find no published reports on these results, but there are a few unsigned memoranda in the Game Division files listing data collected by this sample. What could be located are shown in Table 2.2.

These data are difficult to interpret. The question of actual hunter numbers in 1932, mentioned in the last section, applies here too. On the other hand, the principle involved in this system--determining total kill by applying (1) average pheasant kill reported by (2) a large "random" sample (those reporting) of (3) all hunters to (4) total license sales--was the forerunner of the hunter report card system adopted by the Conservation Department in 1937.

Using this system of calculation, I have prepared total kill estimates for the three years of data I could locate. They differ considerably from the estimates made at the time. The latter, however, were obviously not meant for publication, and were admittedly subjective. At least in 1935 the estimate was based on incomplete returns. Further, administrators at the time may have known of good reasons, not clear to us, to suspect the accuracy of the license sales figures.

About all we can say of these calculations is that quite probably the legal cock pheasant kill for 1932 to 1935 was somewhere between 500,000 and 800,000 per year.

TABLE 2.2

PHEASANT KILL COMPUTED FROM LICENSE STUBS
1932-1935

Year of Kill (1)	No. of Hunters Reports (2)	No. of Pheasants Killed (3)	Kill per Hunter Reporting (4)	License Sales (5)	Per Cent Sample (6)	Computed Pheasant Kill[1] (7)	Total Kill Estimate Made at the Time (8)
1932	69,845	178,035	2.55	214,318	33%	546,510	846,600[2]
1934	112,949	252,832	2.24	346,609	33%	776,404	Unknown
1935[3]	1,260	2,100	1.67	385,554[4]	.3%	643,875[5]	670,000[6]

[1]Column 4 x Column 5. (My computation)

[2]Based on "estimated" license sales of 320,000.

[3]Six counties only.

[4]316,100 (82%) of these were estimated to be hunters in Region III. (southern Michigan)

[5]Kill by Region III (southern Michigan) hunters only. Very likely the 70,000 license holders in northern Michigan shot some birds.

[6]Apparently based on subjective estimate of 275,000 pheasant hunters and a preliminary success figure of 2.4 pheasants shot.

2.4 The Compulsory Hunters' Report Card System

Starting in 1937, each hunter was required by law to report the game he took each year to the Department of Conservation, on an addressed card furnished with the license. These report cards remained essentially unchanged from 1937 until the system was discontinued in 1956.

The annual pheasant kill for the state, as computed from these small game report cards, is compared with license sales for the years 1937 through 1956 in Figure 2.1. These kill figures will be referred to as the "computed kill" in the rest of this report. The method of computing this figure is shown in the appendix.

2.5 Van Coevering's Free Press Pheasant Tally

Jack Van Coevering, Outdoor Editor of the Detroit Free Press, has conducted a "pheasant tally" each year since 1931. Shortly after the pheasant season each year he requests hunters to fill out a form which he publishes in the Free Press. He usually receives about 2,000 replies, and it is reasonable to assume they represent a fair sample of pheasant hunter performance, although the sample is undoubtedly biased according to distribution of the paper's circulation in the state, and by the type of hunter interested enough to submit a tally. Results of his tally are shown in Table 2.3

I compared Van Coevering's surveys with the State's computed kill for the 13-year period from 1937 to 1949 inclusive. This comparison is shown graphically in Figure 2.2. In this figure, the two graphs are equated arbitrarily at 1937, since units for the two lines are not directly comparable. The correlation-

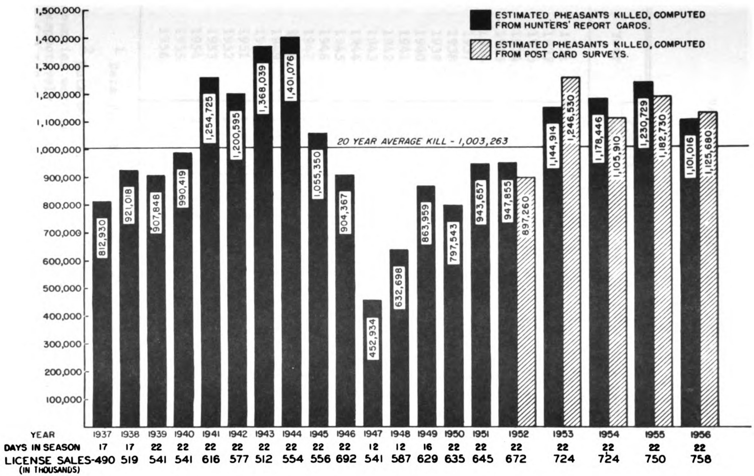

Fig. 2.1--Pheasant kill computed from small game hunters' report cards, 1937-1956.

TABLE 2.3

VAN COEVERING'S FREE PRESS PHEASANT TALLIES[1]
1931-1956

Year	Pheasants Seen per Hour			Sex Ratio (Hens/Cock)	Number of Counties Involved[2]
	Hens	Cocks	Total		
1931	-	-	2.4		12
1932	1.5	0.8	2.3	2.0	27
1933	1.7	0.9	2.7	2.0	23
1934	2.4	0.7	2.1	3.4	31
1935	1.3	0.8	2.1	1.6	30
1936	1.5	0.8	2.3	2.0	27
1937	1.5	0.9	2.4	1.7	34
1938	1.8	1.1	2.9	1.6	34
1939	1.7	0.9	2.6	1.9	44
1940	1.9	0.9	2.8	2.0	31
1941	3.1	1.6	4.7	1.9	27
1942	2.8	1.4	4.2	2.0	26
1943	4.3	2.0	6.3	2.2	19
1944	3.6	1.3	4.9	2.8	29
1945	1.9	0.8	2.7	2.4	25
1946	1.4	0.6	2.0	2.3	35
1947	1.1	0.6	1.7	1.8	34
1948	1.0	0.6	1.6	1.8	37
1949	1.5	0.7	2.2	2.1	32
1950	1.3	0.7	2.0	1.8	-
1951	1.8	1.0	2.8	1.8	-
1952	2.1	1.0	3.1	2.1	-
1953	2.1	1.2	3.3	1.8	-
1954	2.7	1.0	3.7	2.7	-
1955	2.6	1.2	3.8	2.2	-
1956	1.6	0.9	2.6	1.8	-

[1] Data from Van Coevering (1949; 1950;1957).

[2] Data not available 1950-1956. However, 34, 36, and 30 counties were involved in tallies made in 1957, 1958, and 1959 respectively. Number of hunters' reports has been between 1,900 and 2,500 most years.

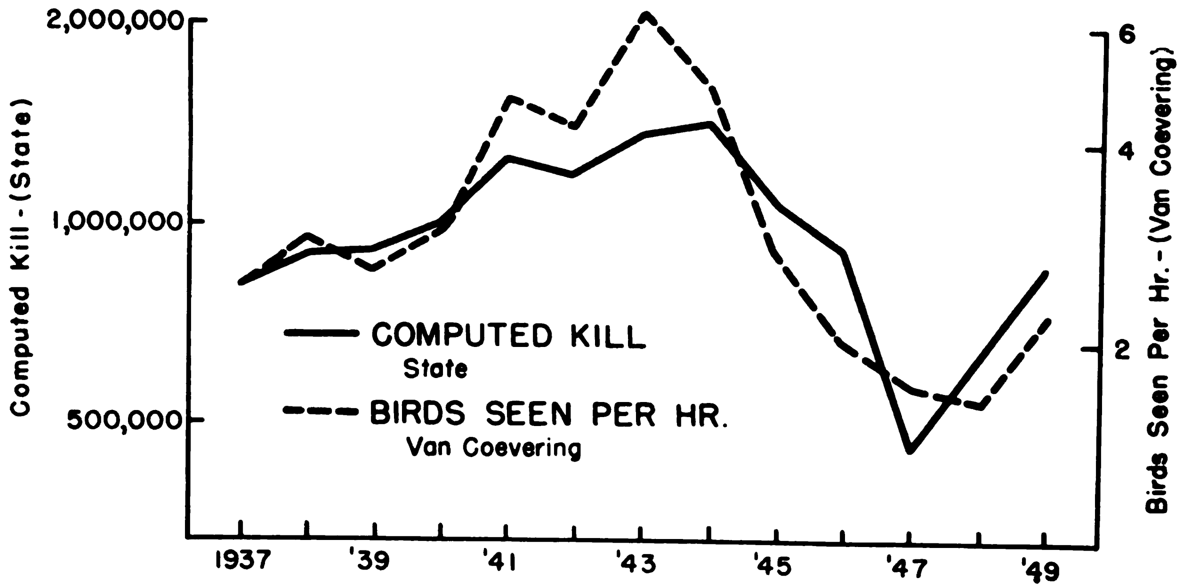

Fig. 2.2--Computed kill compared to Van Coevering's tally.

coefficient of birds seen per hour by hunters reporting to Van Coevering ~~on~~ the computed kill for the 20-year period 1937 to 1956, inclusive, as shown in Figure 2.3, was calculated at r = .876.

This is a good correlation. Whatever the differences between the two, it seems significant that the trends shown are similar, and that after 13 years the lines did not diverge appreciably. The greatest concern for the reliability of the computed kill had been that as the report card returns dropped, the dwindling sample might represent an increasing proportion of more successful hunters. Such a bias would progressively inflate the computed kill. Since Van Coevering's number of reporters has stayed remarkably constant, we can say, with reasonable confidence, that this inflation has not resulted, unless some unknown bias has affected the two relatively independent surveys in the same way.

2.6 Wayne County Sportsman's Club Pheasant Tally

In 1947, Victor Beresford, Secretary-Editor of the Wayne County Sportsman's Club (the "WCSC") in Detroit, sent a form to some 7,000 members with the request that each member answer questions concerning his pheasant hunting during 1947, and the 1946 season as well. Returns were sent to the Game Division and tabulated on IBM machines. The results were compared with the State's pheasant kill computation. The 1946 data were disregarded because of the long interim between hunting and questioning. Data obtained from these tallies appeared to have merit, and so I designed a form asking for considerably more detail for the WCSC for 1948 and 1949. This form was designed particularly to collect

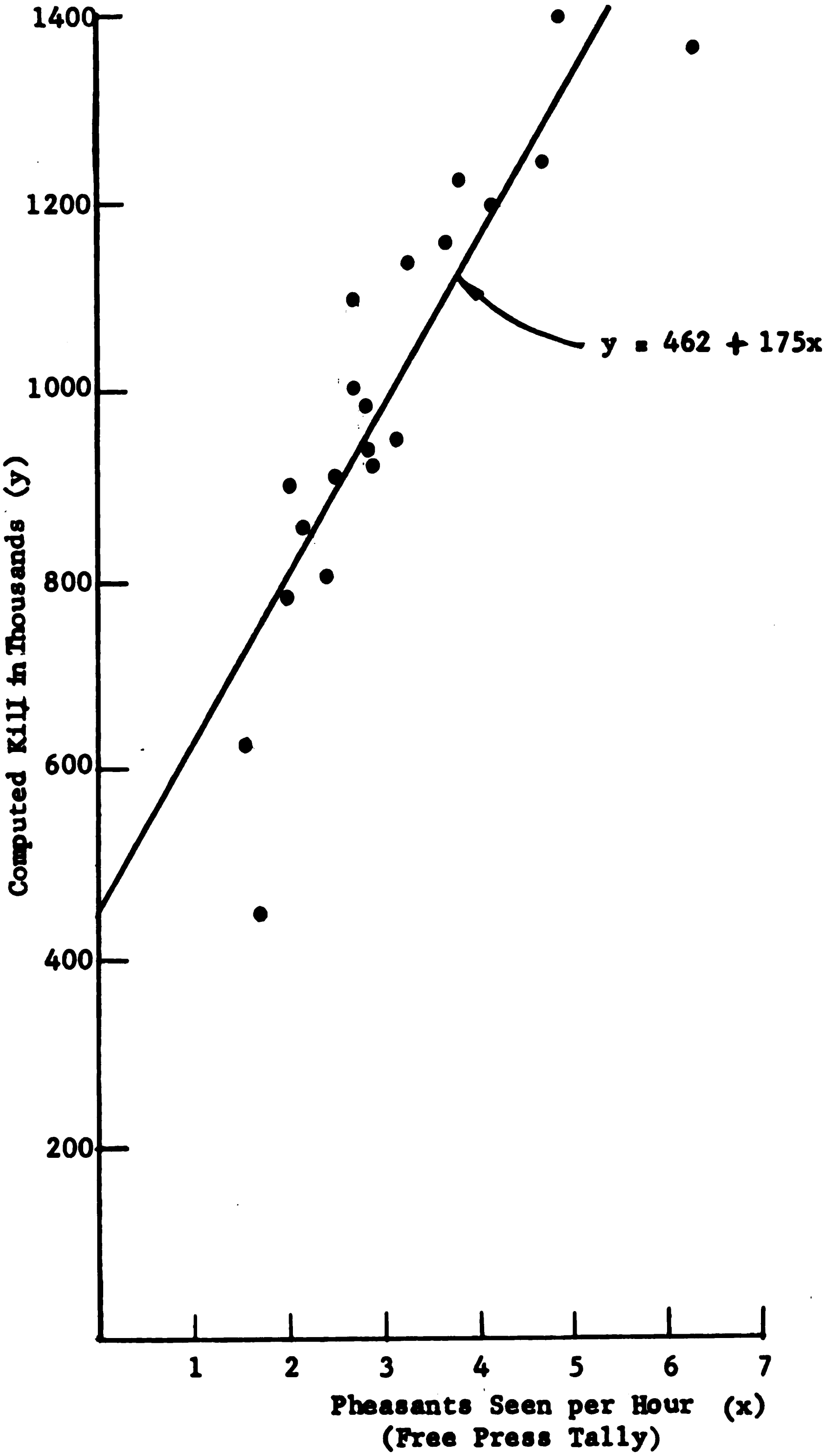

Fig. 2.3--Regression of computed kill on pheasants seen per hour in Van Coevering's tally.

data which could be compared to the computed kill. In addition, I asked for hours hunted and pheasants killed each day of the season, which the Game Division didn't get on the hunter report cards. Table 2.4 summarizes the data obtained from these WCSC pheasant tallies for 1947-48-49.

It would be difficult to calculate any index to total kill for the State from these tallies. A success index from WCSC members, however, can be compared to a success index obtained from the Game Division's report cards. This is done in Table 2.5. Note that this comparison is made only with hunters who reported shooting <u>one or more</u> birds, since unsuccessful hunters are not directly comparable. (The State's report card does not specifically ask the hunter whether he hunted pheasants or not, and the WCSC form does.)

Another comparison of success indices is shown in Table 2.6. Using the formula shown in the table, apparently the State success ratio could be predicted accurately on the basis of the WCSC sample.

2.7 Comparison of Computed Kill with Kill on Sample Areas

Both Van Coevering's tallies and the WCSC tallies show good correlation with portions of the State data. Since both samples are independent of the State sample, they serve as a measure of proof of the value of the State figures, even though the two former are undoubtedly subject to bias because both groups are heavily represented by the metropolitan Detroit area (e.g., more than half the WCSC sample hunted in the counties surrounding Detroit in 1949).

However good these correlations may be, they do not offer any information as to how real the figures are. All three tallies were

TABLE 2.4

WAYNE COUNTY SPORTSMAN'S CLUB PHEASANT TALLIES
1947-1949

	1947	1948	1949
No. tallies returned	434	738	545
No. who bought small game licenses[1]		586	
No. who reported hunting pheasants[2]	321	502	469
Cocks shot per pheasant hunter	1.69	1.65	2.09
Hours hunted for the entire season	17.6	15.3	15.6
Sex ratio of birds flushed	1:2.5	1:2.3	1:2.7
Birds flushed per hour	1.2	1.1	1.4
Hours to kill each Cock	10.4	9.2	7.4

[1]Not asked in 1947

[2]This question was specifically asked in 1948 and 1949, and was not asked specifically in 1947

TABLE 2.5

FREQUENCY DISTRIBUTION OF HUNTERS ACCORDING TO NUMBER OF COCKS SHOT, AS REPORTED TO WCSC AND STATE, 1949

Number of Cocks Shot Per Hunter	Per cent Hunters Shooting 1, 2, 3, etc. Cocks	
	State	WCSC
1	34	35
2	26	23
3	13	10
4	12	13
5	5	7
6	6	7
7	1	2
8[1]	3	3
Total	100	100

[1] Season limit

TABLE 2.6

COMPARISON OF COCKS SHOT PER HUNTER, WCSC AND STATE
1947-1949

Year	Cocks Shot per Hunter [1]		Ratio of State to WCSC
	WCSC	State	
1947	1.69	1.76	1.04
1948	1.65	1.79	1.09
1949	2.09	2.24	1.07
Average	1.81	1.93	1.07

[1] State and WCSC indices are not directly comparable. (See text, Section 2.6)

obtained on a voluntary basis.[1] It is possible, for example, that in a voluntary system there is a tendency for the more successful hunters to report their good luck, which would cause an inflation of the computed kill. On the other hand, there might be a tendency for more unsuccessful (and therefore disgruntled) hunters to report their poor luck; this would deflate the computed kill. Hunters who belong to sportsman's clubs are not likely to be an unbiased sample of Michigan's over one-half million small game hunters. Possibly club members are more likely to send in their report cards than the average small game hunter. So none of these samples is random.

To shed some light on how close these tallies are to actual kill, we need to compare them with an actual measurement of pheasants killed on sample areas. Although such areas are rare, the Department had two study areas where actual kill was measured--the Rose Lake Wildlife Experiment Station near Lansing, and the Prairie Farm in Saginaw County.

Rose Lake[2] has varied in size from one to two thousand acres, so it is relatively small. It is difficult to say how it compares with the rest of the state's pheasant range. The station lies astraddle the Clinton-Shiawassee County line; Table 2.7 shows how the average number of birds killed per 100 acres for a 10-year period

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[1]Despite the fact it is a misdemeanor to fail to submit the Department's report card, the law was not enforced. Hence, in fact, the system was voluntary.

[2]The Rose Lake Wildlife Experiment Station is described by Allen (1941). Researchers obtain careful measurements of pheasant kill each season, as well as other pheasant population estimates during the year.

TABLE 2.7

COMPARISON OF ACTUAL 1937-1949 KILL ON TWO SAMPLE AREAS WITH COMPUTED KILL IN CONTAINING COUNTIES

	Cocks Killed per 100 Acres During Hunting Season				
Year	Clinton Co.	Rose Lake Wildl. Exp. Station	Shiawassee Co.	Prairie Farm	Saginaw Co.
1937				7.3	7.5
1938				14.8	8.9
1939				15.6	9.0
1940	7.1	10.3	4.9	12.6	9.5
1941	9.1	17.4	7.0	20.6	11.8
1942	8.8	11.3	6.8	16.9	13.2
1943	8.4	6.9	8.6		
1944	8.7	5.8	6.9		
1945	7.6	2.8	6.9		
1946	7.5	2.8	5.0		
1947	3.6	2.5	3.0		
1948	4.1	2.4	4.3		
1949	5.5	2.5	5.9		
Total	70.4	64.7	59.3	87.8	59.9
Average	7.0	6.4	5.9	14.6	10.0

on Rose Lake compares with the computed kill for these two counties.

It is to some degree coincidence that the average kill per 100 acres at Rose Lake should fit in so neatly between the averages for the two counties. Notice that the Rose Lake kill was considerably higher than either county during the first three years of this period, and lower during the succeeding seven years. If any other period had been used, the averages would, of course, have been different. In a way, this local change in Rose Lake's pheasant population supports the notion that computed kill figures are reasonably close to the actual kill. Rose Lake has been both above and below the two counties in pheasant kill.

A similar comparison can be made between actual and computed kill in an area of high pheasant populations. The Prairie Farm, an 8,500-acre diked area 13 miles south of Saginaw, represents some of Michigan's best pheasant range. Shick (1952) conducted a pheasant research project there during the early 1940's. Hunting on the Prairie Farm reached an almost unheard of pressure, and an unusually high harvest of pheasants was made several years in a row. Table 2.7 compares the actual kill on the Prairie Farm to the computed kill for Saginaw County.

There is no way of knowing how representative Rose Lake or the Prairie Farm is of the counties with which they are compared. Nor do I pretend any great accuracy for computed kill figures broken down to county. These comparisons, however, are some evidence that the computed kill figures are probably not too far from actual kill.

2.8 Studies of Bias of Hunters' Report Cards

The percentage return of small game hunters' report cards steadily declined from about 40 per cent in 1937 to about 10 per cent in 1950. If these returns were random, sample size would be far more than adequate. Since they were not random, such things as sample size, date of return, per cent return, etc., could all introduce sizable bias.

Studies of these possibilities for bias did not warrant expensive statistical investigation, since the chief use of the computed kill was to determine trends. It would necessitate great expense to check them with a personal interview system, with no assurance of eliminating the bias. But since these computed kill figures were to be the reference point from which to begin my analysis of pheasant populations, I investigated biases as far as practical. These investigations were admittedly rather superficial, but nevertheless somewhat revealing.

Sample Size. Geboo (1941a) reported on a study of sample size, apparently made in 1941, in a Game Division memo. The memo is somewhat ambiguous, but from her data it appears that even 20 per cent of the returns for one year (apparently a net of 7.6 per cent of the small game hunters) could be used to compute a total kill no more than 3 per cent different from a computation using all the returns for that year.[1]

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[1] Geboo made her study on computed kill of cottontail rabbits and deer. Pheasants and rabbits are reported on the same hunter report card--deer are reported on a separate card.

Relative success of reporting vs. non-reporting hunters. Geboo (1941b) also supervised a study in which 6,000 non-reporting hunters were visited by Department workers and asked to give the information asked for on the report card. I can find no record of how many were interviewed, and there appears to be some doubt about the randomness of the sample. But those who said they hunted pheasants reported an average success of 2.96 cocks, compared to 2.88 cocks per hunter who reported voluntarily.

Success of hunters according to date of return. Are either the more successful hunters (proud of their kill) or the more unsuccessful hunters (disgruntled and wishing to complain) prone to send their reports in earlier than the other group? If either is more prone to report, then the shift in percentages of hunters reporting might represent a shift in bias over the years which would lessen the reliability of the computed kill for determining long-term trends.

In 1948 and 1949, I compared the returns received by mid-January (around 20,000) with final returns (around 50,000). The comparison is shown in Table 2.8. If the kill had been computed on these first 20,000 or so reports (representing about 3 per cent of the small game hunters), the final computed kill would have been 2.2 per cent high in 1948 and 8.5 per cent high in 1949. This does not seem to be excessive bias.[1]

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[1] An examination of a similar computation in 1947 (not made by me) indicated the kill would have been estimated about 20 per cent too high. I have no way of rechecking the original data; calculations may have been in error. But it is possible that the two years I calculated may not be representative.

TABLE 2.8

COMPARISON OF COMPUTED KILL AS DETERMINED FROM FIRST 20,000 RETURNS WITH FINAL RETURNS

	1948		1949	
	Sample	Final	Sample	Final
Estimated License Sales	565,068	583,369	645,206	626,941
Per Cent Difference	-3%		-3%	
No. Report Cards Received	20,404	52,026	21,238	51,381
Date by Which Received	Jan.19		Jan. 16	
Net Per Cent Return	3.6	8.9	3.2	8.1
No. Who Hunted Pheasants	13,238	31,522	13,199	31,207
Per Cent Who Reported Hunting Pheasants	65%	61%	62%	61%
Total Estimated Pheasant Hunters	367,294	352,627	400,027	391,000
Success of Those Who Reported Hunting Pheasants	1.76	1.79	2.36	2.24
Per Cent Difference	-1.6%		+5.1%	
Estimated Total Pheasant Kill for State	646,347	632,698	944,064	863,959
Per Cent Difference	+2.2%		+8.5%	

2.9 Computing Kill by Random Sample Mail Survey

In 1954 the Game Division started a new system of computing small game kill. Following the season, about 4,500 small game hunters, picked at random, are mailed a questionnaire concerning their kill. With several reminder letters, response of hunters has been about 95 per cent--about as good as can easily be obtained in such surveys. From this sample the kill is computed. The randomness of the sample avoids the criticism of the hunter report card system that it might be biased by an atypical success of the sample reporting.

The results of these mail surveys for the years 1952-1955, inclusive, are compared to computed kill in Figure 2.1. Blouch (1956) has explained how such surveys are conducted and how the computations are made.

2.10 Computing Kill by County

The computed kill is calculated by county. While sample size for the state-wide figure has been shown to be adequate, it does not necessarily follow that sample size would be adequate to compute accurate kill by county--especially in those counties with low pheasant populations and low hunting pressure.

As the first step for examining county figures, the county kill by year was graphed on a large map, each county's graph being super-imposed on the proper county on the map. These graphs are shown in Figure 2.4. A close inspection of county graphs in this figure suggests one flaw in them. Genesee County quite regularly reported the largest kill for any county. Genesee County has some excellent pheasant range, but it is unlikely it can compare with

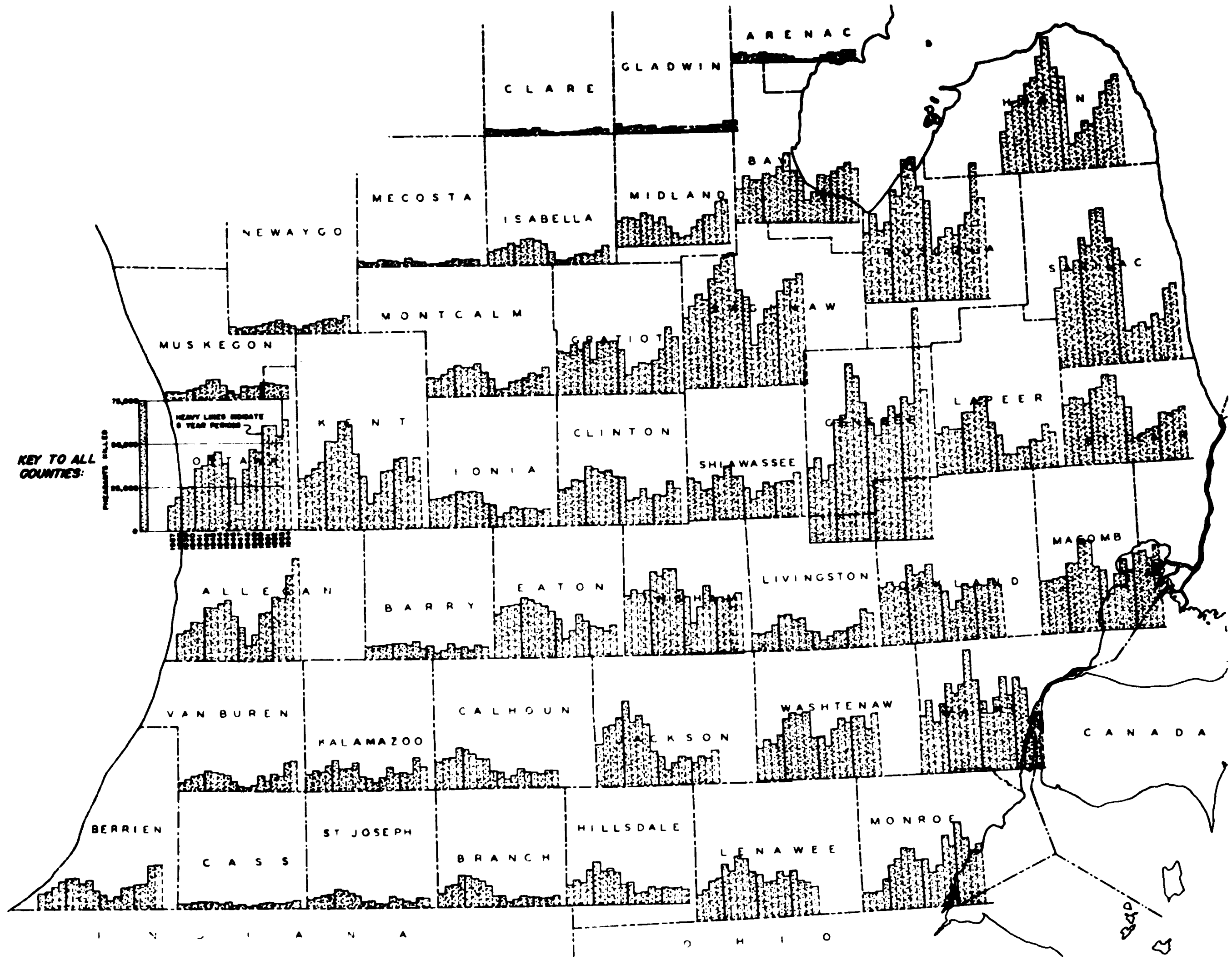

Fig. 2.4--Graphs of computed kill by county.

pheasant production in Huron or Tuscola counties. Wayne County has excellent pheasant range, but a large portion of it is metropolitan, and the Wayne County kill appears unreasonably high. This suggested there was an inflation in the computed kill for some metropolitan counties.

This inflation is probably a result of the kill computation method. Hunters are asked to report the county in which they shot their pheasants, but no attempt is made to distinguish if birds are shot in more than one county. Hence, there is a tendency for hunters, more numerous in metropolitan counties, to overflow into adjacent counties to hunt, yet innocently report their kill as taken in their county of residence--or if they list more than one county for pheasant kill, to put their county of residence first. While this does not affect the total state kill computation, it does affect the computed kill by county. This inflation of kill is most obvious in the metropolitan counties surrounded by good pheasant range. Thus, the cities of Detroit, Flint, Saginaw and Grand Rapids, all near good pheasant range, appear to influence their counties considerably. Kalamazoo, Jackson, Battle Creek, surrounded by poorer pheasant range, do not appear to influence their counties as much.

This is a subjective observation, and there is no practical way to appraise the inflation, if it truly exists. The possibility that it exists must be considered, however, when making interpretations from county kill figures.

The computed kill by county has been useful in determining population changes in various portions of pheasant range. It is an advantage the mail survey does not offer with its present sample size.

2.11 Conclusions

Figure 2.5 summarizes graphically all the estimates we have of pheasant kill.

It appears that we had no good measure of the pheasant kill until 1937 when the small game hunters' compulsory report card system was started, from which we obtained an annual computed kill. Computed kill figures appear to be reasonably representative of actual kill in the state, and to be useful to reconstruct trends.

(1) There was no consistent divergence of population trends determined from Van Coevering's Tally and the computed kill from 1937 to 1949--a period when Van Coevering's reports remained relatively stable, while the percentage of report cards returned were decreasing.

(2) Success indices of hunters reporting on small game report cards were very close to those of an independent sample of hunters (WCSC) from the Detroit area for two years.

(3) Comparison of computed kills for three counties was reasonably close to known kill on two sample areas in these counties.

(4) Studies of the bias of computed kill figures, although inconclusive, indicated that (a) sample size was probably adequate, (b) success of non-reporting hunters was not appreciably different from that of reporting hunters,

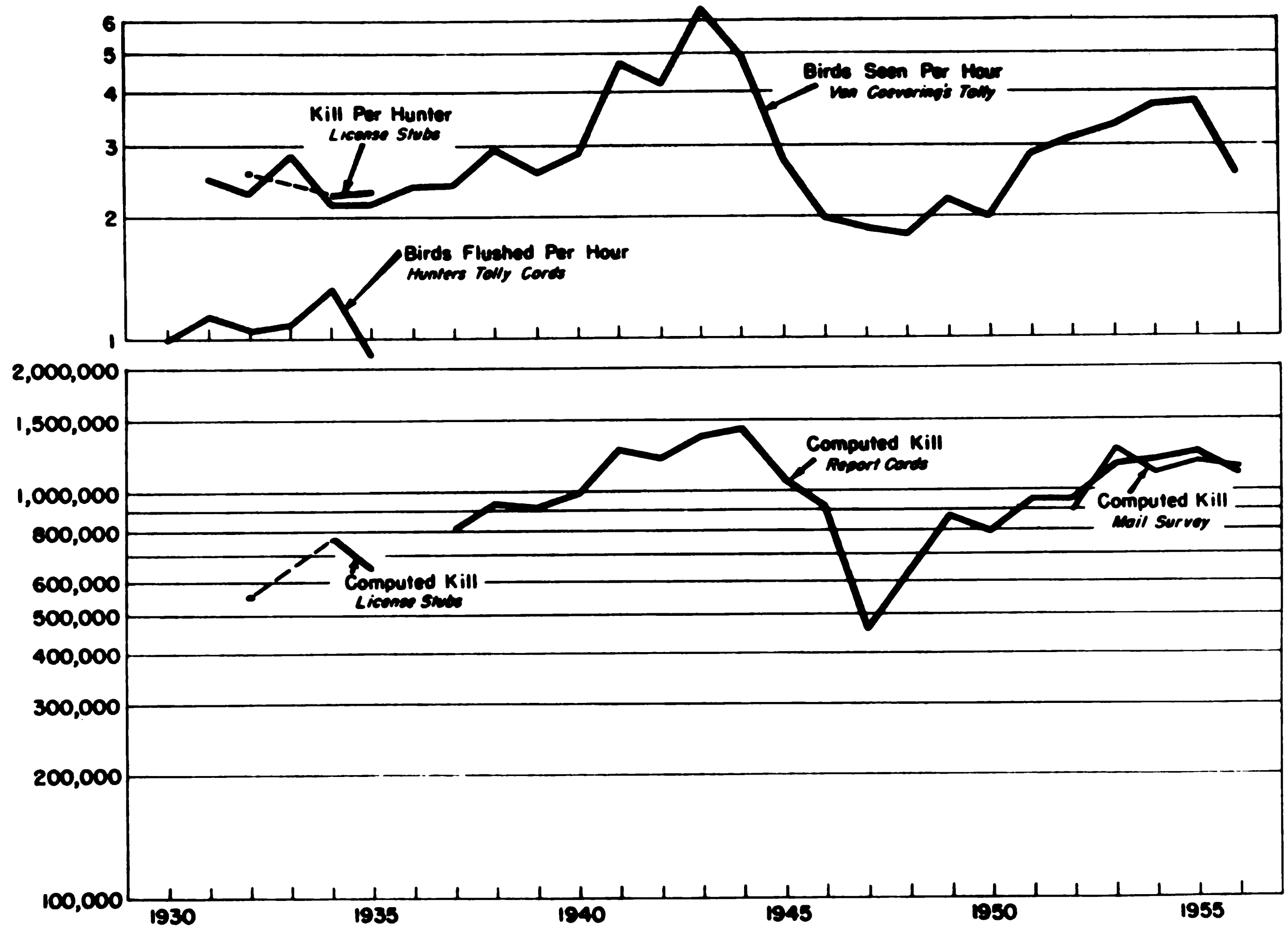

Fig. 2.5--Indices to fall populations, 1932-1956

(c) there was some question as to whether success of hunters differed according to the date at which they sent in their returns, although in two years studied early reports differed from the final computed kill by only 2.2 and 8.5, respectively.

(5) Computed kill figures did not differ greatly from those computed from other independent and random mail surveys.

(6) Pheasant kill computed by county and by groups of counties offered a useful basis for comparing areas of pheasant range, although there appeared to be an inflation of kill figures from some metropolitan counties.

With the exception of Van Coevering's Tally the figures prior to 1937 are speculative. The curve is plotted to the ratio scale. Assuming that pheasants were following Pearson's growth curve, and were in the period of rapid expansion at least from 1925 to 1930, the log of the population curve should follow roughly a straight line from its origin, shortly after 1918. If these assumptions are correct, then we can guess that the kill in the first open season (1925) might have been around a quarter of a million cocks, and that from 1930 to 1935 the kill may have been somewhere around a half to three quarters of a million cocks.

The graph in Figure 2.5 summarizes the best information on pheasant populations that we have for the time prior to 1946, when other state-wide surveys were begun.

Chapter 3

DISTRIBUTION OF PHEASANTS

3.1 Introduction

The term "distribution" is defined for the purposes of this report as relative geographical density of pheasants. It should be understood that relative densities may change from year to year as well as from area to area.

I have already shown that pheasants had apparently spread to the geographical limits of their range by as early as 1930 (Figure 1.1). Inspection of the county kill figures (Figure 2.4) shows no major changes in distribution, geographically, although relative densities from area to area have changed over the years. It was probably not until 1944 that pheasant numbers had built up to the point that all of Michigan's pheasant range was fully occupied.

1) Pheasants increased rather steadily and rapidly up to 1944.
2) In the early 1930's the flat, heavy clay land of southeastern Michigan was considered second-rate habitat, and it was not until the early 1940's that this area came to be considered the best pheasant range.

3.2 General Distribution of Pheasants

A variation of some of the data in Figure 2.4 is presented in Figure 3.1. The kills in each county for a 10-year period, 1937-1946, and for one more-or-less average year, 1949, can be compared geographically. The years 1937-1946 saw pheasants build up to a peak and start down toward a low. In 1949 about 864,000 cocks were shot, which is approximately midway between the high and low extremes in kill in 1945 and 1947, respectively.

The two maps in Figure 3.1 suggest a rather sharp demarcation between high and low pheasant densities on a line between Arenac and Muskegon counties. Field inspection of many of the counties on the northern edge of this line (e.g., Gladwin, Clare, Mecosta, and Newaygo) shows further that most of their pheasant populations are in the southern part.

It may be significant that the border between two great soil groups, the podzols and the gray-brown podzolic soils as published by the United States Department of Agriculture (1938), seems to fit almost exactly as a border between Michigan's best pheasant range and the northern marginal range.[1] In many places, pheasant populations dwindle to an occasional colony of very limited extent within a mere 10 miles north of this line. About 98 per cent of the pheasant kill during the years 1937-1946 was made south of this line.

3.3 Correlation of Pheasant Distribution with Soils

Inspection of the maps in Figures 2.4 and 3.1 suggested that soil might be an important determining factor, directly or indi-

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[1]Schneider and Whiteside (1954), however, place the border between these great soil groups much farther south--on an approximately east-west line between St. Clair and Kent counties.

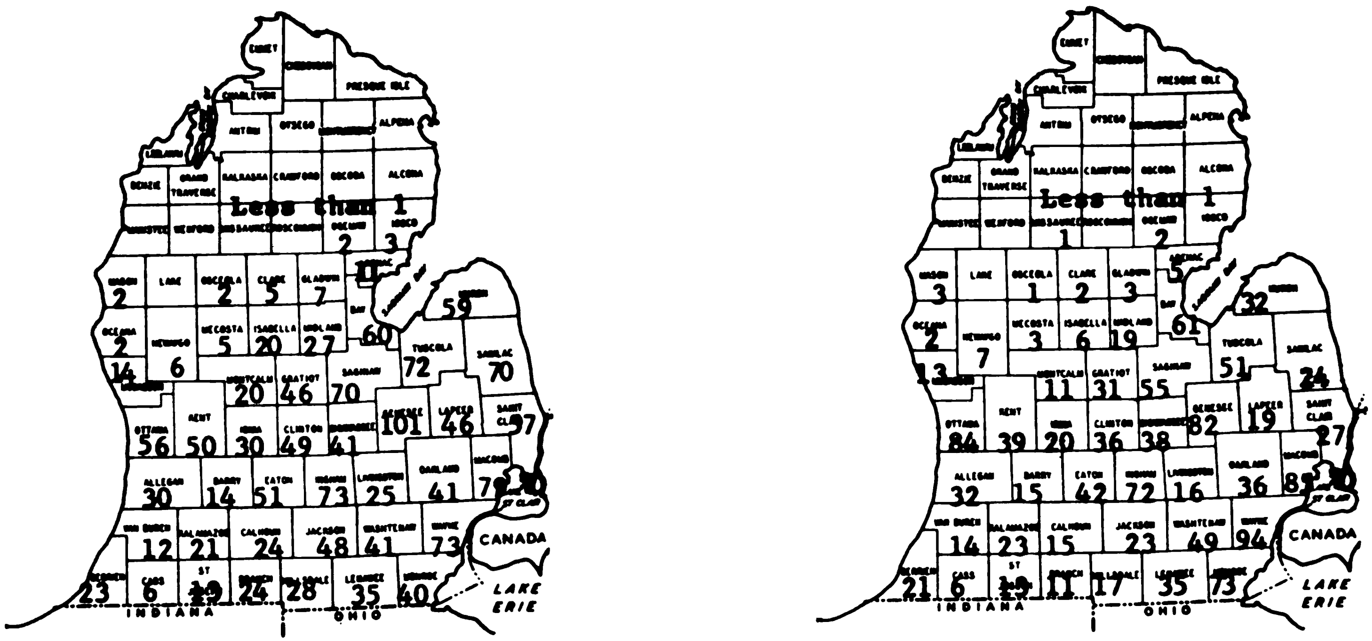


Fig. 3.1--Computed kill per square mile by county.

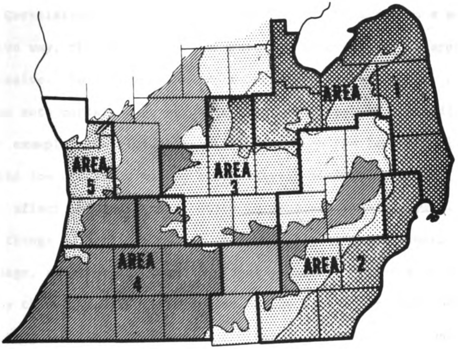
rectly, in the distribution of pheasants. So I compared distribution as demonstrated by the above maps with four soils maps:

1) The United States Department of Agriculture's (1938) map of the major soil associations of the United States.
2) Leaveret's (1924) map of the surface formations of Michigan, which shows land surface formations based on their geological occurrence.
3) Millar's (1948) map showing the land formations produced through the action of glaciers. In effect, this is a simplified version of Leaveret's map, and designates areas of till plain, outwash plain, moraine, and lake bed.
4) Veatch's (1930) generalized soil and land map, which groups soils according to such characteristics as fertility, topography, texture, and drainage.

This comparison of pheasant distribution with these soils maps showed:

1) highest pheasant densities on soils of lake-bed origin, particularly lake-bed <u>clay</u> soils, although some <u>sandy</u> soils appeared to support high densities <u>when of lake-bed origin</u>.
2) average to high densities on soils of till-plain origin.
3) low pheasant densities on areas of outwash plain origin and on pronounced moraines.
4) marginal densities on dry, outwash plain <u>sands</u>.

This general relationship of soils to pheasant distribution is demonstrated in the map in Figure 3.2. The soils of southern



Land Type	Pheasant Population
Level clay soils of lake-bed origin.	High
Sandy, but of lake-bed origin, level	High
Level or gently rolling loamy till plain.	Moderate
Sandy loam or sand outwash plains.	Low

Fig. 3.2--Groups of soil associations in southern Michigan and study areas.

Michigan are separated into four groupings. The boundaries of these soil groupings are for the most part based on actual boundaries of the major soil associations as determined by Schneider and Whiteside (1954).[1]

Correlating distribution with soil is difficult. In a subjective way, this correlation of pheasant density to soil appears impressive. To state how the relationship works is another thing. We can note certain connections without much fear of contradiction --for example, the high pheasant densities on the lake-bed clays, and the low densities on the sandy outwash plains. The land could affect pheasant distribution in many and involved ways. Such things as soil fertility, texture, origin, trace elements, drainage, and topography could affect pheasants directly or indirectly through their determination of the biota. Or they could affect agricultural practices and other cultural features which in turn could affect pheasant populations indirectly.

Albrecht (1944) has stated the case for the influence that the more nutritive soils may have on wildlife populations. But despite discussion with soils scientists, I have been unable, from this gross comparison, to tie down pheasant density to any single factor such as soil productivity, fertility, topography, or type of farming. Quite probably there are a number of interrelated factors which combine to determine pheasant abundance. I believe, however, that the correlation demonstrated here is sufficiently valid to justify more intensive study.

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[1]Cash H. Wonser, soils scientist for the Game Division, assisted me in the preparation of the map shown in Figure 3.2.

3.4 Definition of Pheasant Range

Figure 3.3 delineates the areas I have defined as primary and marginal pheasant range. I placed the boundaries on county borders, since I used counties as the unit for collection of data.

3.5 Selection of Study Areas

I designated five representative areas of primary pheasant range as study areas (shown in Figure 3.2), on the basis of five criteria:

1) Homogeneous populations, based on data in Figures 2.4 and 3.1.
2) Homogeneous land characteristics discussed in Section 3.3.
3) At least 3,000 square miles in area (with the exception of Area 5, which was unavoidably smaller).
4) Short north-south dimension to minimize variation due to climatic or phenological differences.
5) Free of the metropolitan bias, described in Section 2.8, when practical.

Selection of these areas was to some degree arbitrary. This is inevitable since I used counties as the basic unit. Thus, statistics compiled for each of these units must not be considered in an absolute sense, but as indices. If this rule is followed, such arbitrariness presents no difficulty, since this selection of study areas is a form of statistical stratification.

Table 3.1 summarizes some statistics for the study areas. The areas comprise approximately 72 per cent of primary pheasant

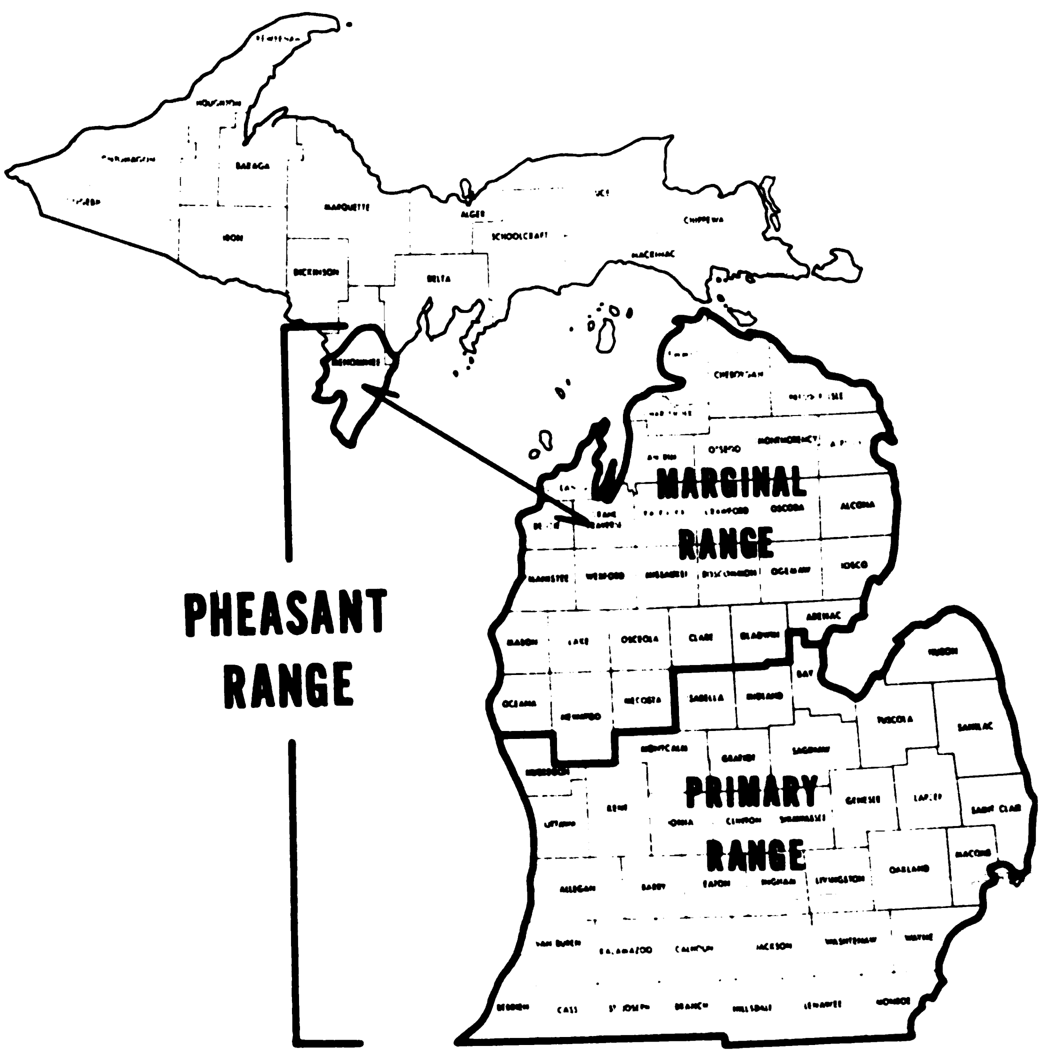


Fig. 3.3--Administrative delineation of pheasant range.

TABLE 3.1

STATISTICS CONCERNING STUDY AREAS

Area		Square Miles of Area	Number of Counties	Approximate Dimension in Miles		1949 Pheasant Kill Per Sq. Mi.	Relative Pheasant Population
				N-S	E-W		
1		4,597	6	100	100	39.7	High
2		3,120	5	80	85	62.4	High
3		3,378	6	75	75	39.6	Medium
4		4,514	8	75	110	15.0	Low
5		1,897	3	75	45	42.0	High
Total	5 Areas	17,506	28	165	250		
	Primary Range	24,316	38	165	250		

range and provided 74 per cent of the pheasant kill in primary pheasant range during the 10-year period 1937-1946.

Table 3.2 shows the percentage of first- and second-class land in each of the study areas. Comparison of these land classes would be more significant if it could be made on the basis of soil types rather than on county units. It is perhaps noteworthy that Area 4, with the lowest pheasant density, has a low percentage of first-class land. Area 5 has a high pheasant density, but has the lowest percentage of first-class land of any of the areas. This may be explained by the fact that Area 5 has high densities, indeed, but they are restricted to portions of the area, and the remainder of the area resembles Area 4.

3.6 Conclusions

Relative pheasant densities appear to be correlated with soil groupings. The factors responsible for this correlation are unknown. They may be direct influences of soil on pheasants or indirect influences of soil on other phenomena such as cover or cultural features which, in turn, influence pheasants. Despite the obscurity of these factors, the correlation seems definite enough that the subject should be pursued. I recommend further study of pheasant populations in comparison with the characteristics of each of the soils groupings in Figure 3.2.

TABLE 3.2

PER CENT FIRST- AND SECOND-CLASS LAND IN STUDY AREAS[1]

	Area					Total 5 Areas
	1	2	3	4	5	
Per Cent First-Class Land	56.9	52.4	60.5	33.2	23.7	47.1
Per Cent Second-Class Land	32.0	29.1	32.3	41.9	29.6	33.8
Total	88.9	81.5	92.8	75.1	53.3	80.9

[1]Taken from Veatch(1941).

Chapter 4

DEVELOPMENT OF EXTENSIVE SURVEYS

4.1 Introduction

We now have a hypothetical curve showing annual fall populations (Chapter 2) and a gross analysis of pheasant distribution (Chapter 3). I have selected study areas on the basis of this information. These data are all from one time of the year, and all from one family of sources--hunting season data. They might have some common biases.

The next step is to obtain population data for each study area, for each season of the year, and for each year of the study. If we can do that, we can compare data on a single population from independent sources. If this leads to valid measurements of populations, we will have the basis for determining what factors affect pheasants--in two dimensions, time and space.

The bulk of the data in the rest of this report was obtained from extensive surveys. This chapter is a discussion of how these extensive surveys were conducted. For the benefit of those who might wish to use such surveys, some space is devoted to their organization, and to surveys which were tried, but abandoned as impractical.

4.2 Definition and Description of the "Extensive Survey"

For our purposes an extensive survey can be defined as the collection of pheasant observations through a system of sampling, usually interpreted in terms of indices to certain populations. It is to be distinguished from the intensive survey which implies a study of smaller areas involving total pheasant populations rather than indices based on samples. Following is a list of characteristics of the extensive surveys I have used:

1) They cover large areas -- as much as one half the state.
2) They are reported in indices rather than absolute figures. While indices can in some cases be converted to absolute figures, extensive surveys customarily describe pheasants in terms of birds observed per unit of time or distance rather than birds per unit of area.
3) They are based to a large degree on sampling techniques. Sample size, distribution, and consistency of the observing group (i.e., turnover and uniformity of habits of observers) are important. To some degree the use of the five study areas is a form of stratification of the sample.
4) Extensive surveys <u>may</u> be made by groups of untrained observers, so long as the groups are consistent.
5) They usually require large numbers of observers.

There were five sources for manpower to run extensive surveys:

1) <u>Sportsmen</u>, who have a vested interest in pheasants, might cooperate at other times of the year as well as during hunting seasons. There are more than a third of a million pheasant hunters in the state.

2) Rural mail carriers, who had been used as observers in other states.

3) Department of Conservation personnel other than game biologists might be asked to help survey pheasants.

4) Farmers might be canvassed for their observations of pheasants.

5) Game biologists, although few in number, might obtain extensive data of some sorts (e.g., hunter-performance data from personal interview with hunters, crowing-cock counts, etc.).

Data were needed for four seasons of the year.

1) Information on the number of breeders present each spring.

2) Data on production of broods in the summer.

3) Data from hunting that was not available from kill reports (e.g., age ratios of cocks, hatching dates, hunter success).

4) In winter, population data, measures of winter loss, post-season sex ratios, etc.

In the following paragraphs, the various types of extensive surveys we developed are described. In later sections the data obtained from them are discussed and analyzed.

The forms used for polling cooperators are important. It is essential that they be simple, easily understood, and of convenient size. Provisions should be made for mailing reports back. Simplification of the forms is worth considerable thought. A seemingly trivial mistake, such as a form that is too large for the return envelope, might well result in fewer returns. As is true of all such surveys, instructions must be explicit. Any ambiguity in the questions asked or forms supplied might result in questionable data.

4.3 Surveys by Sportsmen

In the fall of 1946, when this project was started, Linduska asked sportsmen to report pheasant observations made in the field during the winter of 1946-47. They were furnished with comprehensive forms asking for a good many types of information, but so designed that almost any observation which might be of value could be reported. Over 15,000 of these forms were distributed to members of Michigan United Conservation Clubs. Only a few dozen were returned and no usable data accumulated from them, so this type of survey was abandoned.

In the fall hunting season of 1946, Linduska also distributed about 15,000 pheasant-aging forms, illustrated in Figure 4.1, to sportsmen's clubs, through license dealers, hardware stores, biologists and officers. Only about 500 were returned. During the seasons of 1947, '48, and '49, smaller numbers were distributed. About 300 or more were returned each season, recording ages on four to six hundred birds. These data are discussed in Chapter 8, with other age ratio data, but their value is questionable. Despite the instructions, sportsmen were obviously not aging birds correctly.

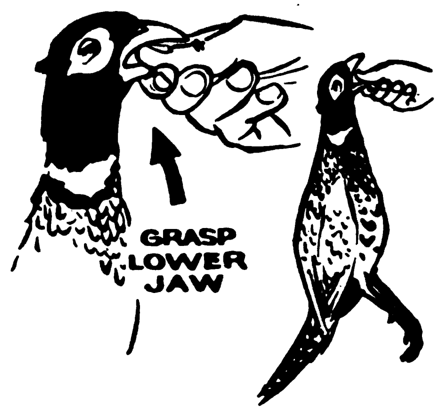
4.4 Surveys by Rural Mail Carriers

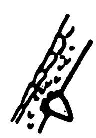
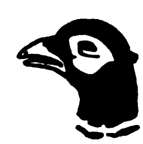
Bennett and Hendrickson (1938) reported on the "roadside census" technique of estimating pheasant populations in Iowa.

PHEASANTS are an annual crop, and the number available to hunters in the fall is largely dependent upon the number produced in the spring and surviving until Autumn. A comparison of the number of young and old birds shot by hunters is a valuable means of measuring the survival rate of young from year to year and in one locality as compared with others. Simple methods for distinguishing young and old birds are shown below and hunters are urged to report the age of their kill on the attached postage paid card.

INSTRUCTIONS

Pick up your bird by the lower jaw, inserting your thumb inside the mouth as shown in the drawing. If the lower jaw breaks or bends when the bird is lifted, the cock is this year's young and the leg spurs will be found to be short and blunt. Show the number of this type killed in column (1) on the attached card. If the bird can be lifted by the lower jaw and shaken without the jaw breaking or bending, the bird is an old one and generally has long sharp spurs. Tally any birds killed having these characteristics in column (2).



		Number of Cocks Killed	
This form may be used for a party record. Have your partners record their kill.		(1) Young Cocks	(2) Old Cocks
		Lower jaw broke when bird lifted by it.   *Spurs short and blunt*	*Lower jaw supported weight of bird.*   *Spurs long and sharp*
Date Birds were Killed	County Where Killed		

Your name and address is solicited, but not required ➡

Name:...

Street or R.F.D.......................................

City:...

RECORD YOUR KILL AND MAIL THIS CARD TODAY

Fig. 4.1--Pheasant-aging cards sent to sportsmen.

Later Randall and Bennett (1939) reported on the use of the same technique in Pennsylvania. In the 1940's several other states experimented with roadside censuses by rural mail carriers (hereafter referred to as carriers). In the spring of 1946, 712 carriers in the Lower Peninsula of Michigan were asked if they would be willing to make a summer count. Nearly two-thirds (466) expressed their willingness to do so.

During the summer and fall of 1946, the carriers were asked to conduct four surveys. Between 250 and 361 of the 466 cooperated. Again in 1947, summer and fall surveys yielded good returns.

Inspection of the two years' returns from carriers showed two encouraging things:

1) The relative number of broods they saw from area to area and year to year was fairly well correlated with the relative number of pheasants reported shot by hunters on their hunter report cards. Here, perhaps, was a potential method of predicting fall kill!

2) The percentage who responded was remarkably high for a supposedly disinterested group. Moreover, for the most part, the same carriers were responding for each survey. The carriers are somewhat more professional than might be supposed--their turnover is low, they are a conscientious group, and in the words of one of them, "being government employees, we are used to making surveys. We make a lot of surveys even sillier than your pheasant survey. That's part of our job."

So once again we invited the more than 900 carriers in pheasant range to help us. The Rural Letter Carriers' Association, to which most of them belonged, encouraged their members to assist, which helped immeasurably.

Beginning in 1948, the carriers were asked to make three surveys annually--a spring survey in April, a brood survey in July, and a postseason survey in early December or late November. Close to 500 carriers have helped regularly in these surveys.

Figure 4.2 shows the distribution of carriers in pheasant range. Since pheasants are restricted principally to farm land, and since virtually every farm is visited each weekday by a carrier, a large percentage of Michigan pheasant range is covered.

Carriers' observations are measured in this paper in terms of "10 carrier-day" periods. Use of the carrier-day rather than miles of travel as the unit saves a prohibitive amount of clerical work. The "10 carrier-day" unit can be converted and interchanged with a "per mile" unit with no appreciable error. A comparison between the two units "birds seen per 10 carrier-days" and "birds seen per mile of travel" is shown in Table 4.1. As a rule of thumb, the 10 carrier-day period can be equated to 500 miles of travel. The rural mail carrier surveys carried on in many other states are conducted in many different ways, and so comparison in any but a general way must be done carefully.

The mileage of a carrier's route does not change from day to day. Carriers' routes average very nearly 50 miles each in all counties. So, for the size of the sample (around 500 carriers), the average mileage or time spent on his route would change but

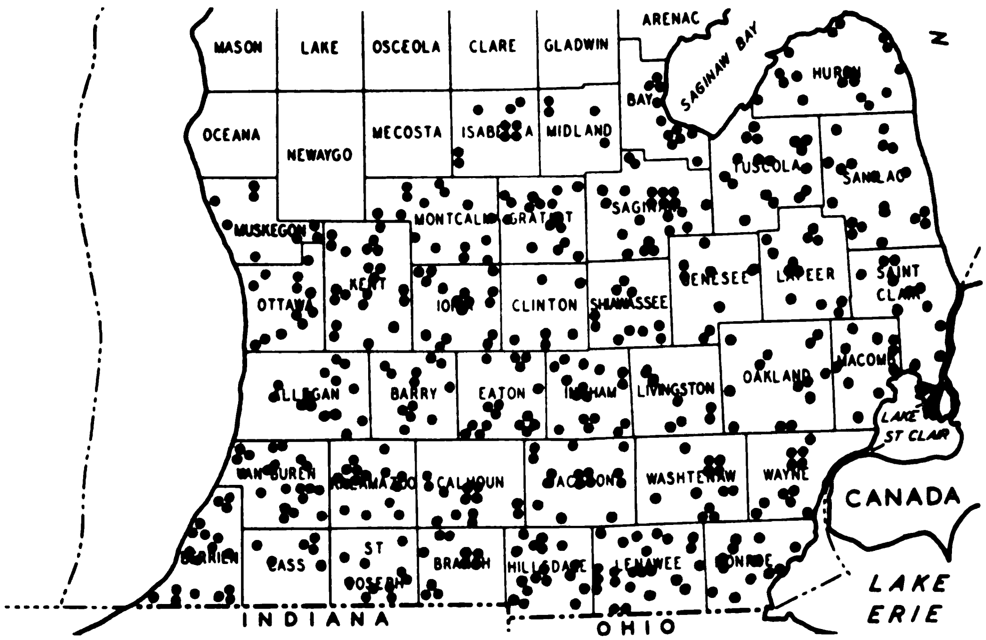


Fig. 4.2--Location of carriers' routes.

TABLE 4.1

AVERAGE LENGTHS OF CARRIERS' ROUTES[1]

Area		No. of Carriers	No. of Miles in Routes	Average Miles in Each Route
1		79	4,038	51.1
2		43	2,133	49.6
3		54	2,757	51.1
4		77	3,932	51.1
5		26	1,239	47.7
Total	5 Areas	279	14,099	50.5
	Primary Range	383	19,165	50.0

[1]Based on July, 1946 returns (383 carriers).

little from survey to survey. Further, results of surveys are always compared to other surveys by carriers; so the unit, whether it be time or linear distance, is immaterial. Finally, even though routes might vary 2 miles (4 per cent), we have no reason to believe that time (hours on the route) is any less valid as a unit than miles traveled. For nearly 30 years the unit of measurement of deer counts made by Department personnel has been "time spent in deer territory" rather than miles traveled.

4.5 Surveys by Conservation Officers

Michigan conservation officers (hereafter referred to as officers) are carefully selected and well trained. They are interested in their work and are cooperative, which makes them valuable observers. The nature of their duties and their daily routine provide a consistency of observations. The fact that they are Department employees made it easy to arrange for them to record pheasant observations in any fashion and period required. In some years, officers have recorded daily pheasant observations for nine months of the year.

In many instances, officer counts complement the carrier surveys--each supplying what the other lacked. With only one or two officers per county, data for small units were often lacking in sample size--which the carriers' surveys made up for. On the other hand, carriers could not be asked to make counts for long periods of time, or during the Christmas rush, for example; the officers could be.

The number of officers in pheasant range has varied from year to year. In 1946 their numbers were low due to post-war adjustments. In 1950, however, there were about 55 officers in the 34 counties of the Department of Conservation's Region III. Region III is composed of three districts. Since reports of officers were handled through administrative channels, their data were tabulated by these districts. Figure 4.3 compares Region III, including these three districts, with the 38 counties that have been designated as primary pheasant range. Four counties on the northern edge of pheasant range in another administrative region were covered by carriers and not covered by officers.

As in the case of the carriers' tabulations, daily pheasant observations have been tallied in terms of units of time, rather than linear mileage. This was a necessity, since officers record mileage semi-monthly, usually by reading their car speedometers. Asking (as we have tried) for daily mileage resulted in unsatisfactory tabulation. Many neglected to mention it, and many others had to estimate it. Since it was not part of their official routine, it was neglected. In interpreting observations by two-week periods, however, mileage may be used, since that figure can be assumed to be reasonably accurate. Even so, there is considerable evidence that officers' observations of birds per unit of mileage is not so useful as observations per unit of time (i.e., observer-day). Officers' duties are seasonal, and at certain times they may be checking for fishermen on lakes or walking traplines in completely different habitat--so car mileage would be meaningless.

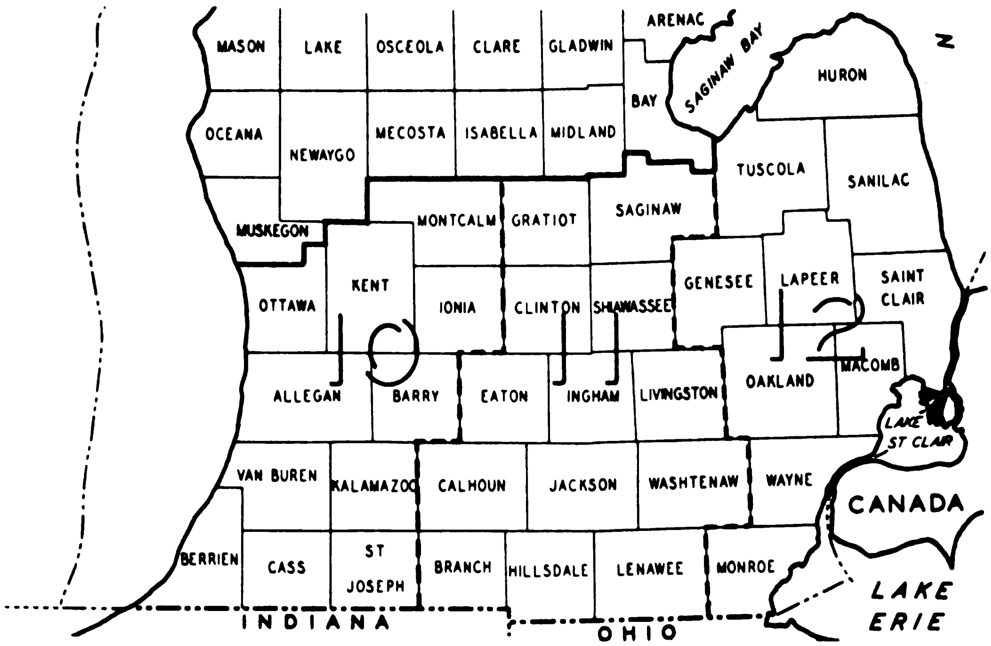


Fig. 4.3--Officers' administrative districts.

4.6 Surveys by Farmer Cooperators

In the spring of 1948, the names of over 600 farmers who were interested in conservation were obtained from county agents, officers, or district game managers who knew them personally. In the spring of 1948, 1949, and 1950 the farmers were asked for information on nests and broods discovered during spring work, particularly while plowing sod or mowing hay. They have been a good source of information which in the past was difficult to obtain. For example, studies of clutch size, to be statistically valid, must be determined from a large number of observations. In Michigan, Rose Lake and the Prairie Farm combined had recorded clutch sizes on less than 100 nests during the 13 years the station had been in existence, and the 4 years the Prairie Farm had been a study area. Yet in each of the 3 years the farmers cooperated, they reported more than 100 clutches.

The number of nests in hay and sod are thought by some workers to be indicative of net pheasant production for any year. Finding pheasant nests is extremely difficult. Farmer cooperators have given us good information on numbers of nests they found per acre--data that would otherwise have been difficult to get on an extensive basis.

The farmer cooperators appear to be an unusually interested and responsive group. So far they have been canvassed only for performance data (e.g., actual number of nests found per definite number of acres plowed or mowed), which can be used objectively. The group may be large enough, however, that they could be asked for more subjective information, such as number of broods they see on their farms each winter. I attempted this, but was not satisfied that I could convert their returns into usable information. Nevertheless, I feel this possibility should be explored further.

4.7 Crowing-cock Surveys

Kimball (1949) reported on a technique for extensive analysis of the cock pheasant population in the spring by use of a crowing-cock survey. Basically, this consists of running a 20-mile route starting one-half hour before sunrise. Each mile a stop is made and all pheasant crows heard during a two-minute period are recorded. At the conclusion of each route the total or average number of calls is calculated, and constitutes an index to the abundance of cock pheasants along that route.

In 1948 I experimented with the technique of the crowing-cock survey in sample areas, and in 1949 set up a system of routes covering the 38 counties in primary pheasant range.

These routes were not picked at random. Rather, they were set up geographically to cover pheasant range. An attempt was made to put one route in most counties in pheasant range. Where one county obviously represented two types of range (Lenawee County, for example), we tried to set up two routes, one in each type of range. This was not always possible, due to manpower limitations.

We tried to run each route at least three times during the spring. If weather conditions prevented accurate counts they were repeated up to four or five times. This usually resulted in at least one good count on each route. We selected the maximum figure for each separate survey as the value for that route.

Routes were run by biologists and wildlife student aides. Considerable care was taken to train and select these men. Insofar as it was practical, a different person was used on each of the three runs on each route each spring to minimize individual differences in hearing. Individuals whose hearing was not well tuned to the frequency of a pheasant cock's crow were not used.

Since geographical coverage was good, year to year comparisons of crowing-cock averages should be reasonably valid as an index to the state-wide cock population. Since routes were not randomly selected, however, comparisons from area to area must be made with caution.

4.8 Surveys by Game Biologists

Game biologists and part-time student assistants on the project helped in many ways, although there were so few of them that their surveys could rarely be called extensive. In some cases, however, (e.g., crowing-cock counts) we relied heavily on them. At other times biologists could collect data at the same time that other extensive surveys were being run, to add to, or appraise, the extensive-survey data.

Biologists and students provided all the manpower for contacts of sportsmen for hunter-performance data during the hunting seasons.

There were many other instances where biologists were responsible for collection of data extensively. Aging of pheasants and counting of crowing-cocks, for example, had to be done by trained people, and only through the use of all available biologists could we get enough data over large enough areas to be meaningful.

Finally, we depended heavily on biologists for advice on the type of information we should be gathering, the timing of surveys based on the phenology of their districts, and on unusual circumstances that might be a clue to what was happening to pheasant populations as a whole.

4.9 Summary and Conclusions

Linduska (1947) described the objectives of the extensive survey, and the first carrier and officer surveys run in 1947.

Surveys by sportsmen proved valueless and were abandoned. Per cent returns were low and they could not, as a whole, be relied on to age pheasants accurately. The carriers proved to be a willing group, and were used for three surveys a year--spring, summer and late fall. Officers could be used for longer periods (all summer or winter) and their data were numerous enough to be valid for several indices.

The carrier and officer surveys nicely complement one another. Officers cooperated over long periods of time, but their operations lacked bulk. Carriers furnished large samples of data that the officers could not match, but their surveys were infrequent and for short periods.

Farmers were very cooperative and provided extensive data on clutch size, brood size, hatching dates, timing of certain agricultural practices, and nest and brood density data.

Biologists were used for crowing cock counts, hunting season contacts of hunters and for examinations of birds for biological data. They also made some counts simultaneously with other extensive surveys.

During the five years (1946-1950), about 60 extensive surveys exclusive of hunter kill reports were made. They involved close to 10,000 individual reports, involving many million miles of driving and observations of close to a million pheasants.

Such extensive surveys are inexpensive to run, and involve very little cash outlay. When the limitations of extensive surveys are recognized, they are an excellent source of data at a minimum of expense.

Chapter 5

SPRING POPULATIONS

5.1 Introduction

The crowing-cock survey offers biologists a good method for censusing cock pheasants. But estimates of spring breeding populations from the crowing cock count are dependent upon the sex ratio of the spring population. Moreover, crowing-cock surveys are limited by the availability of biologists. So there was a need to attempt to develop other spring surveys--to try to determine breeding season sex ratios, and to try to find an easier method for censusing cocks and hens alike.

In the spring the habits of cocks and hens differ. Cocks display, and hens become progressively more secretive as they begin to nest and become broody. So the relative observability of the two sexes differs, and probably shifts as the spring progresses. Determining true sex ratios, then, promised to be a difficult problem. But a relative sex ratio from spring to spring might have some use, if we could find a way to time the counts so they could be made at the same time (phenologically) each spring.

To get this spring population data, I set up an extensive crowing-cock survey system and a system of extensive roadside censuses to be run by the carriers.

5.2 Crowing-cock Surveys and Carriers' Surveys

The crowing-cock survey as it is used in Michigan was described in Section 4.7. The location of crowing-cock routes is shown in Figure 5.1, and results of the counts are shown in Table 5.1. The latter are tabulated by study area. Relative abundance of pheasants from area to area roughly follows our other data on pheasant numbers (e.g. Area 1 high, Area 4 low populations). While sample size of data by area is rather small, this appraisal of the spring cock population might offer a partial basis for determining unusual winter mortality in an area, and for gaining insight into relative rearing success for the areas.

In April, 1948 I asked the carriers to make an experimental one-week survey. They recorded the number of pheasants seen while driving their routes for a six-day period.

I attempted to time the survey so it would come at the height of breeding activity. At the time it appeared to be a good choice; crowing activity was good and breeding seemed to be in full swing. But there was no method for timing the count since no state-wide crowing-cock survey was run that year. The carriers reported about 1,700 pheasants. Analyzing the data by study area showed fair correlation with what we knew of pheasant populations, so the survey showed some promise.

Officers were not asked to make an April survey. Judging from their winter counts, the officers were too few in number to gather an adequate sample. The April count had to be relatively short, since pheasant observability was shifting rapidly. Moreover, the officers were making daily observations for the entire

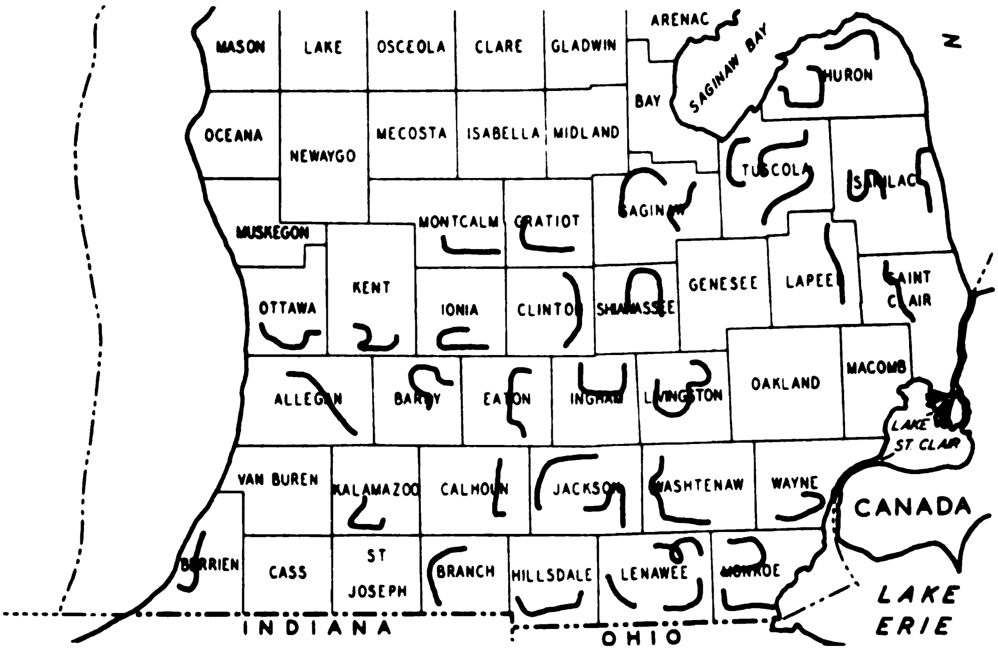


Fig. 5.1--Location of crowing-cock survey routes.

TABLE 5.1

SUMMARY OF CROWING-COCK SURVEYS

Area		Average Number of Calls Heard Per Two Minutes									
		1949		1950		1951		1952		1953	
		No. of Routes	Av. No. of Crows	No. of Routes	Av. No. of Crows	No. of Routes	Av. No. of Crows	No. of Routes	Av. No. of Crows	No. of Routes	Av. No. of Crows
1		10	5.8	10	10.7	10	12.0	10	12.9	7	12.4
2		7	12.7	7	12.2	7	16.4	7	14.6	3	10.3
3		6	8.1	6	11.0	6	12.2	6	14.5	6	14.9
4		5	4.0	5	5.7	5	6.1	5	5.8	5	4.8
5		2	3.8	2	6.5	2	12.7	2	12.8	2	12.8
Total	5 Areas	30	6.8	30	9.2	30	11.8	30	12.1	23	11.0
	Primary Range		7.0		9.7		11.8		12.7		11.2

winter, and I did not want to wear out my welcome with them.

For the seven-year period 1949-1955, we have spring data from crowing-cock surveys, and from two-week April surveys by the carriers.

5.3 Sex Ratios

The carriers recorded observations of cocks and hens separately. For three years they also recorded observations of harems separately from the other observations.[1]

Table 5.2 summarizes the carriers' observations, and Table 5.3 summarizes the sex ratio observations, by area, distinguishing the harem observations from all other observations for the three years. Of course, these sex ratios are not necessarily true sex ratios--quite likely they are not. However, they may represent trends. These observed sex ratios vary between what seems to be narrow limits--1.3 and 1.8. Shick (1952) had observed "true" sex ratios on the Prairie Farm as high as ten hens per cock during years of high pheasant populations.

Observations on harems varied even less--between 2.1 and 2.4 for the three years! Also the frequency distribution of the harem sizes was very similar for the three years (Table 5.4 and Figure 5.2).

If our data on harem sizes is valid, there are two logical interpretations we might make:

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[1]A harem was defined as any hens seen in the neighborhood of a cock. If no cock was seen in the vicinity, hens were not considered to be a harem.

TABLE 5.2

SUMMARY OF CARRIERS' SPRING SURVEYS

Dates in April (Incl.) (Sundays excepted)		1948	1949	1950	1951	1952	1953	1954	1955
		5-10	4-16	3-15	16-28	14-26	6-18	5-17	18-30
No. of Days of Count		6	12	12	12	12	12	12	12
No. of 10 Carrier-day Periods		167.2	582.9	568.9	679.2	658.0	645.4	769.1	758.0
Pheasants Observed	Cocks	622	2,159	1,689	4,794	4,825	2,964	5,643	7,541
	Hens	1,074	2,803	2,790	6,614	7,370	4,892	10,318	12,198
	Total	1,696	4,962	4,479	11,408	12,195	7,856	15,961	19,739
	Birds Seen Per 10 Carrier-days	10.14	8.51	7.87	16.80	18.53	12.17	20.75	26.04
	Sex Ratio	1.7	1.3	1.7	1.4	1.5	1.7	1.8	1.6

TABLE 5.3

SEX RATIOS OBSERVED BY CARRIERS IN SPRING SURVEYS

Areas	1948						1949						1950					
	Not in Harems			Harems			Not in Harems			Harems			Not in Harems			Harems		
	Cocks	Hens	Sex Ratio	Cocks	Hens	Sex Ratio	Cocks	Hens	Sex Ratio	Cocks	Hens	Sex Ratio	Cocks	Hens	Sex Ratio	Cocks	Hens	Sex Ratio
Area 1	171	427	2.5	47	104	2.2	426	648	1.6	77	216	2.8	346	713	2.1	61	208	3.4
Area 2	73	73	1.0	20	35	1.8	492	464	.9	102	186	1.8	315	470	1.5	47	106	2.3
Area 3	78	79	1.0	37	107	2.9	298	275	.9	72	130	1.8	245	360	1.5	59	103	1.7
Area 4	48	47	1.0	8	19	2.4	181	93	.5	26	51	1.96	144	118	.8	27	48	1.8
Area 5	31	44	1.4	25	54	2.1	53	127	2.4	33	84	2.5	94	131	1.4	27	72	2.7
Total 5 Areas	401	670	1.7	137	319	2.3	1450	1643	1.1	310	667	2.2	1144	1792	1.6	221	537	2.4
Primary Range	469	727	1.6	153	347	2.3	1765	1975	1.1	394	828	2.1	1423	2139	1.5	266	651	2.4

TABLE 5.4

FREQUENCY DISTRIBUTIONS OF HAREM SIZES AS REPORTED BY CARRIERS

Class Size	1948		1949		1950	
	No. of Harems	Per Cent Dist.	No. of Harems	Per Cent Dist.	No. of Harems	Per Cent Dist.
1	67	43.8	176	44.7	99	37.2
2	35	22.9	112	28.4	71	26.7
3	24	15.7	54	13.7	40	15.0
4	13	8.5	30	7.6	29	10.9
5	8	5.2	6	1.5	10	3.8
6	2	1.3	9	2.3	9	3.4
7 or more	4	2.6	7	1.8	8	3.0
Total	153	100%	394	100%	266	100%
Av. Harem Size	2.3		2.1		2.5	
Sex Ratio (all observations)	1.7		1.3		1.7	

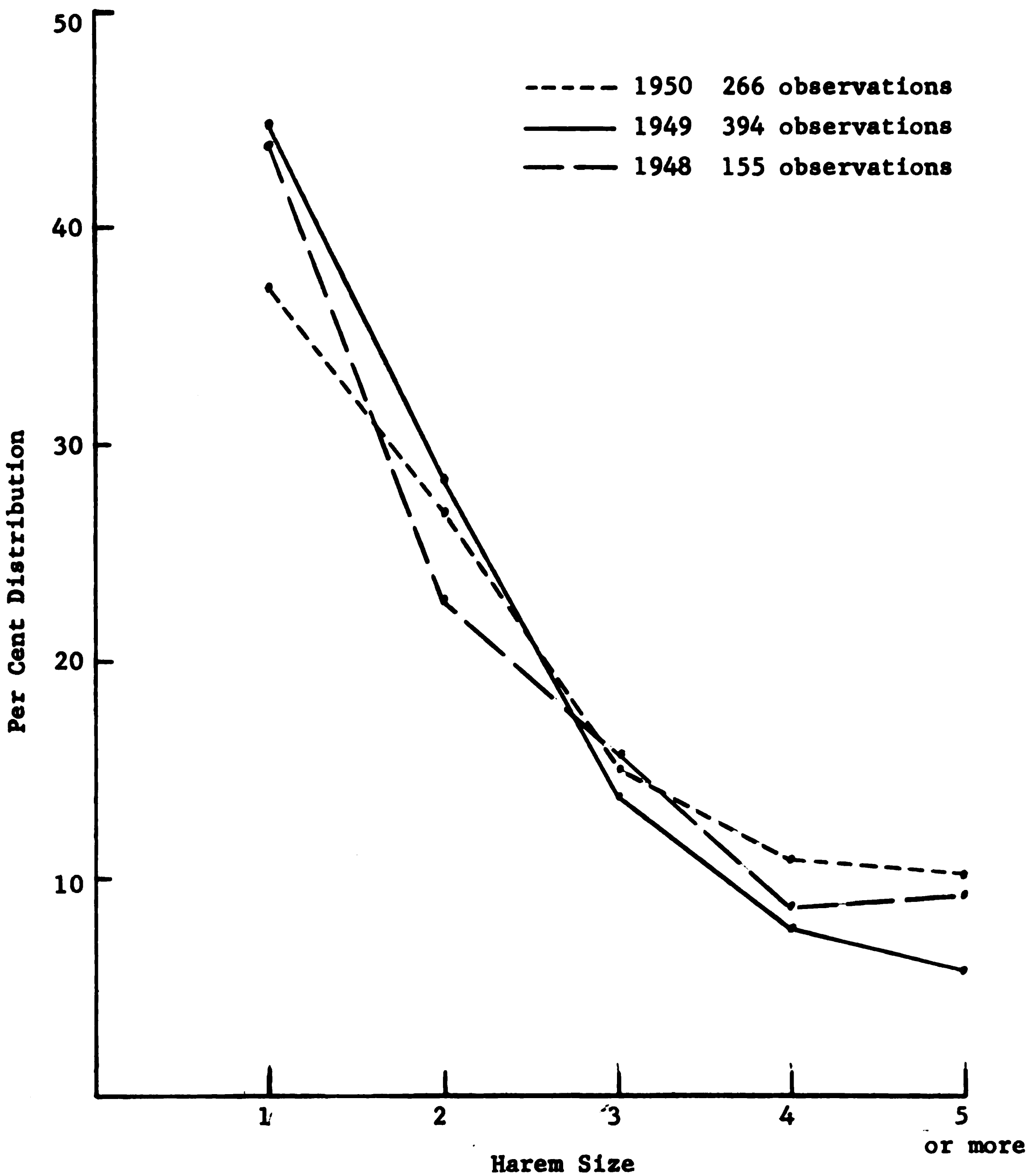

Fig. 5.2--Frequency distributions of harem sizes as reported by carriers.

1) Size of harems may reflect true sex ratio while random observations do not. If so, then it would mean the sex ratio did not change significantly during the three-year period.

2) It may be that the true sex ratio has little influence on harem size, but that harem size is an "internal life history constant" determined by the biology of the bird itself. In other words, cocks may select a certain number of hens for their harems, little influenced by the number of hens. Some pheasant workers have suggested that there may be a segment of bachelor cocks (without hens) in a population.

The available evidence suggests that harem size may be a more reasonable measure of the change in sex ratio from year to year than the total sex ratio observations. However, sex ratios based on harem observations must still be regarded only as observed sex ratios rather than true sex ratios. I have no means to convert the observed to true sex ratios.

5.4 Density

The crowing-cock counts should offer a good index to cock pheasant numbers. They are more or less self-adjusting for phenological timing. Routes are run several times during the height of crowing activity--which can be easily determined. On the other hand, the carriers' count is set up well ahead of time and is run at a predetermined time--which may not be phenologically identical to other years. So the crowing-cock survey must be considered the more likely index to true spring numbers.

Table 5.5 compares the two indices for the seven-year period during which they ran concurrently. They do not appear well correlated. The inconsistencies between the two are very likely due, then, to differences in timing of the carrier counts from year to year. We can postulate that if a count is run a week later (phenologically) one year than another, one would expect, at this time of the year, that the later count would report relatively more birds.

Let us assume for the moment that the crowing cock index is a valid measure of the cock pheasant population, since the counts are self-adjusting for phenology. We can assume that the two surveys are both estimating the density of essentially the same population of birds. Let us also assume for the moment that sex ratios are somewhat the same for the years involved, and thus the crowing cock index is measuring not only cocks but, to a large degree, total birds.

We can plot the regression of the carriers' index on the crowing-cock index (Figure 5.3). Inspection of the points shows that in four years (1951, 1952, 1954, 1955), the carriers' index appears high. Three of these four counts were run at least two weeks later than the others! If there was an expected acceleration in birds seen as the season progressed at this time of the year, we would expect these counts to be inflated.

If this expected acceleration could be measured, then adjustments could be made for the different dates the surveys were run. In 1950 I had asked the carriers to record their observations by day. These are summarized in Table 5.6. The mean of the second

TABLE 5.5

ADJUSTMENT OF CARRIERS' DENSITY INDICES
TO COMMON STARTING DATE

Year	Day Count Started	Deviation From April 5 (days) (d)	Crowing Cock Index	Carriers' Index (c)	Adjusted Carriers' Index[1] (x)
1949	4	- 1	7.0	8.51	8.82
1950	3	- 2	9.7	7.87	8.55
1951	16	+11	11.8	16.81	11.67
1952	14	+ 9	12.7	18.53	13.63
1953	6	+ 1	11.2	12.91	12.41
1954	5	0	11.4	20.75	20.75
1955	18	+13	12.5	26.04	17.13

[1]Adjusted by the formula $x = \frac{c}{1 + .04d}$, on the basis that values of c are inflated by 4 per cent per day after April 5 (see text).

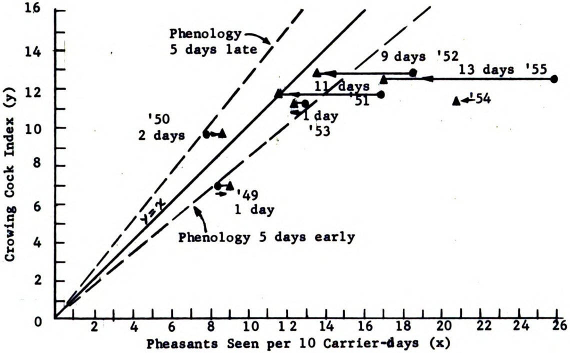

Fig. 5.3--Relationship of crowing-cock index to carriers' spring density index, adjusted to common starting date (April 5).

TABLE 5.6

DAILY PHEASANT OBSERVATIONS BY CARRIERS
SPRING, 1950

April	Cocks Observed	Hens Observed	Daily Sex Ratio		Total Birds Observed	
3	136	227	1.67		363	
4	99	176	1.78		275	
5	110	206	1.87	Av.	316	Av.
6	118	186	1.58	1.74	304	327.0
7	134	243	1.81		377	
8	120	207	1.73		327	
Total	717	1,245				
10	155	301	1.94		456	
11	122	235	1.93		357	Av.
12	149	213	1.43	Av.	362	419.5
13	144	240	1.67	1.59	384	
14	173	253	1.46		426	
15	229	303	1.32		532	
Total	972	1,545				
Total	1,689	2,790	1.68		4,479	
Average					373.25	

week's observation of birds was 28 per cent higher than the first week's--an average increase of 4 per cent per day.

I then arbitrarily assumed a ratio of 1.0 of crowing-cock index to carriers' index. Using this arbitrary regression line of $y = x$ and the crowing cock index intersect for the mean, I adjusted this mean by 4 per cent for each day the counts were started after (or before) April 5. The points were all adjusted then, as if the counts had all started on the fifth of April--assuming, of course, this 4 per cent daily expected increase was legitimate. This brought the points for 1951 and 1952 well within the grouping of the others, but left the 1955 point still considerably out, as well as the 1954 point, which received no adjustment.

If this relationship of the two indices was real, we could expect then, that phenological differences might be the most likely reason for the points for 1954 and 1955 to be non-conforming. For these points to be high on the carriers' index side would mean that phenology was early those years, the breeding cycle was more advanced, and hence the carriers saw proportionally more of the population.

Inspection of the weather records showed that, indeed, these springs <u>were</u> early.

In 1954 the Michigan average temperature was 2.6° F. above normal in April, 1.1° F. below normal in March. But February was 9.9° above normal--<u>the warmest February on record</u>! April, 1955, was 7.6° above normal, <u>the warmest April on record</u>! Thus, phenology may be largely responsible for this inflated carrier index in 1954 and 1955. And it may well be that a good correlation exists between the carriers' and the crowing-cock indices.

This comparison is necessarily crude and cannot be the basis for prediction. One might determine the relationship by multiple regression of (1) carriers' index on (2) day (chronology) of the count (or deviation from a mean day) on (3) crowing-cock index. But another unmeasurable unknown remains--the effect of phenology.

Limits allowing for 5 day's variation in phenological timing of counts (shown as dotted lines on each side of the regression line) include all the points except 1954 and 1955. Counts those years would be about two weeks early phenologically if the 4 per cent daily increase were valid at all population levels and at any period of April, which is extremely unlikely.

While the case for this relationship borders on the emperical, it seems to be rational. No further study of it seems worthwhile until we are able to obtain more data by which we can determine more certainly how this daily expected increase in observations may work. In addition, the assumption that sizable sex ratio changes are not occurring must be supported.

5.5 Conclusions

The crowing-cock index is considered the best measure of spring cock populations, since crowing-cock counts are self-adjusting for phenology.

Observability of pheasants increases (apparently at a rate of about 4 per cent per day) during early April. Carriers' spring pheasant density indices correlated well with crowing-cock indices when adjusted phenologically.

Harem size as observed by carriers is more stable than sex ratios based on all observations. Frequency distributions of harem sizes were very similar for the three years they were determined. Harem size may be a better measure of sex ratios than all observations.

Chapter 6

BROOD PRODUCTION

6.1 Introduction

Perhaps the most important and most profitable aim of the game manager is to encourage brood production. Pheasants are essentially an annual crop, and the success of a fall hunting season depends largely on broods produced in the summer.

Decreased brood production appears to be the principal reason for the pheasant depression in the mid-forties. Starting in 1945, in three years Michigan's pheasant population dropped to a third of a previous level it had taken 8 to 10 years to reach. No unusual mortality to adults was noted. Even the abrupt removal of perhaps a third of the population during a three-week hunting season has no appreciable effect on pheasant population trends (Allen, 1947).

Since the ultimate determination of a fall population is essentially by broods produced the summer before, an accurate measurement of brood production is perhaps the most important statistic to the game manager who must set fall hunting regulations in the summer. Information on the number of broods, the number of chicks in the broods, and the survival of these chicks to the fall hunting season is needed. Ultimately we are looking for a method for predicting from summer populations the number of pheasants available for the fall harvest.

6.2 Brood Production

In the summer of 1946, Linduska made the first exploratory extensive brood counts. Carriers and officers in pheasant range made semi-monthly counts in June, July and August. The counts were alternated so that the officers counted the first half and the carriers the second half of each month. The brood density indices for these surveys are shown in Figure 6.1. Although we have no way of knowing the relation of these density indices to each other, there is an obvious increase in number of broods seen as the summer progresses. It can be assumed from this graph that broods will probably be seen in greatest numbers from the middle of July to the middle of August. In June, many broods are still hatching and not so readily seen at that stage. On the other hand, broods begin to break up in late August.

In 1947 both officers and carriers made only one count--in the last half of July. After a thorough study of the 1946 and 1947 data, I decided to ask the carriers for only one count, in late July, and to have the officers count by semi-monthly periods during the months of June, July and August. The count made by the carriers would provide a large sample. (In late July, 1949, for example, carriers saw nearly 2,000 broods during the same period the officers reported 183.) The officers' counts, covering the entire summer, might provide a timetable for brood observa-

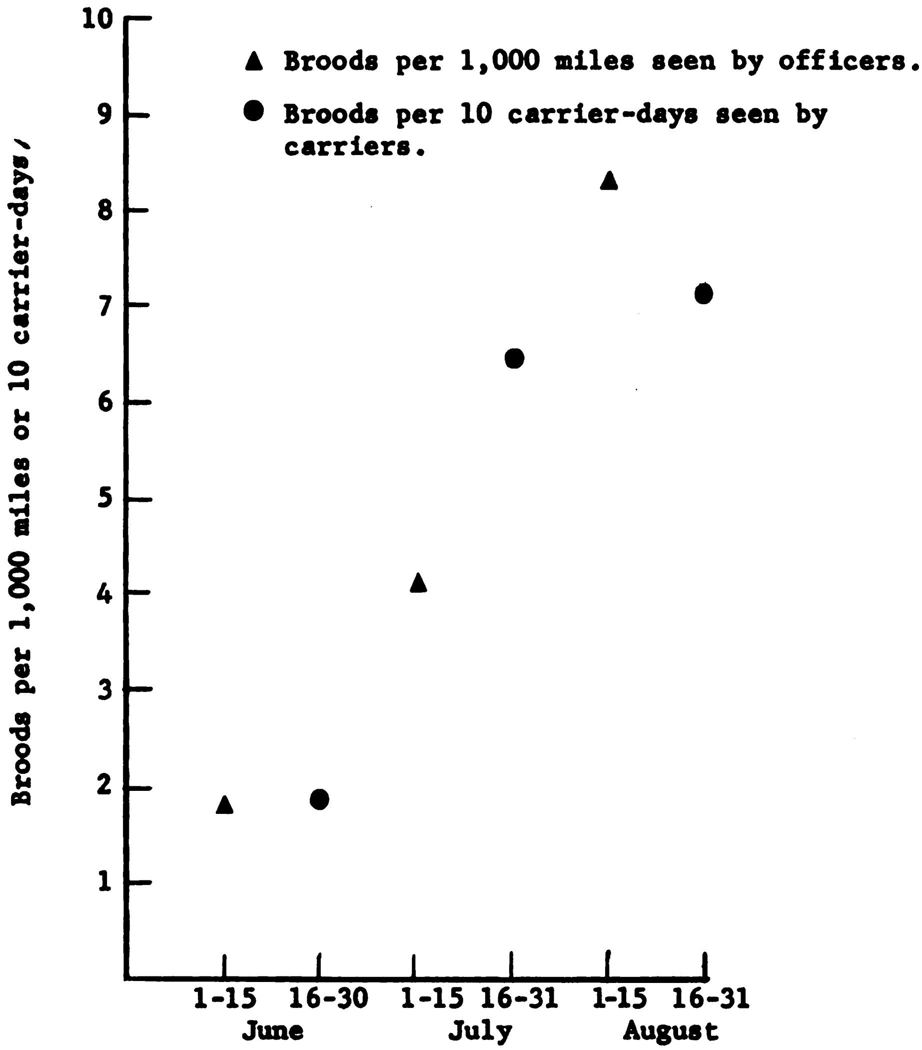

Fig. 6.1--Brood density indices as reported by carriers and officers, 1946.

bility. If so, then carriers' counts might be more accurately compared from year to year.

6.3 Timing of Brood Surveys

Figure 6.2 shows the brood density indices determined by officers for four years. In 1948, the number of broods observed in the last half of August increased. In 1949 and 1950, the number of broods observed in the last half of August dropped, which may be a reflection of the scattering of broods as the chicks mature.

Aside from the drop in late August, brood observations seem to follow a very consistent pattern from year to year. The regression of these brood density indices on periods of the summer up to August 15th is also shown in Figure 6.2. The slope of the regression line represents the rate of increase in number of broods seen as the season progresses. From this slope (or perhaps a similar slope for a shorter period of time) one could predict the expected daily increase in brood observability. A one-week difference in timing of brood counts might mean a difference of perhaps 10 per cent in the counts.

In 1949 and 1950, the carriers' tally forms were designed so that each day's observations were recorded during their count. The counts followed a pattern of daily increase in broods similar to the regression line of the officers' counts in Figure 6.2. The mean daily broods observed each week increased from the first week to the second week by 1.83 per cent and 1.48 per cent per day, in 1949 and 1950 respectively, with coefficients of varia-

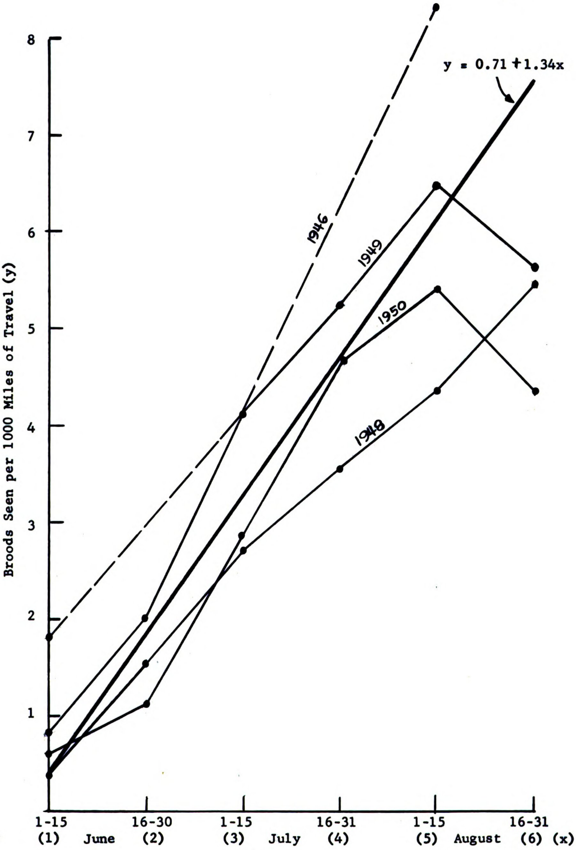

Fig. 6.2--Officers' brood density indices.

tion of 10.1 per cent and 12.5 per cent respectively. This rate is about what one would expect from the regression line in Figure 6.2. Examination of data in 1958 and 1959, collected in the same manner, showed no such increase, however. Average broods observed each day during the first week were almost the same as averages for the second week, in both years. Apparently a two-week count is too short to show this increase in all years.

Klonglan (1955) and others (Bennett and Hendrickson, 1938; Randall and Bennet, 1939; Koziky et al., 1952) have reported that precipitation may have a strong influence on early morning roadside counts. Klonglan states that even rainfall the night before a count caused wide variation. I made a gross comparison of daily rainfall during the carriers' late July brood counts and could see no consistent relationship between rainfall and number of broods observed.

Daily brood observations by carriers, for two representative years, are graphed in Figure 6.3. The population was about average in 1949, high in 1958. A synopsis of precipitation each day across southern Michigan is included.

6.4 Brood Size

It has been a common suspicion that one reason for differences in pheasant production from year to year might be a difference in the average number of chicks in each brood brought to maturity.

As a result of this study, based on reports of over 15,000 broods by carriers and officers, I conclude that this suspicion

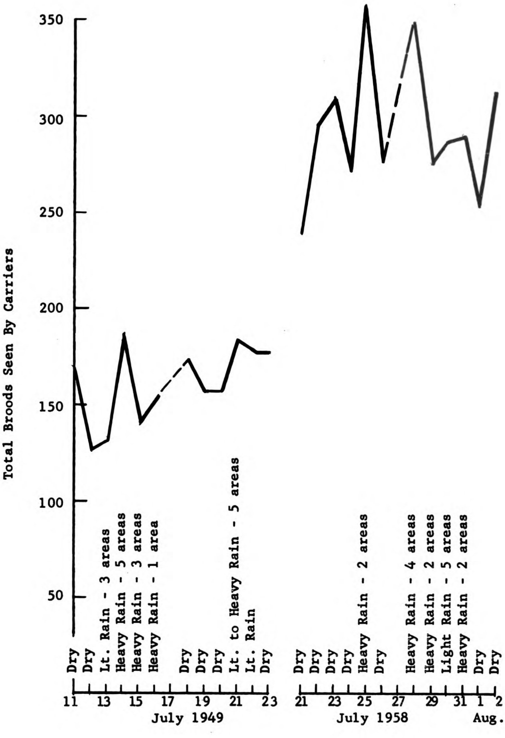

Fig. 6.3--Carriers' daily brood counts compared with precipitation.

is not well founded. I could find no evidence from these extensive counts that annual changes in brood size had any appreciable effect on total pheasant production.

Since brood size would be expected to vary with age of the brood, and since broods are produced later (or earlier) some years than others, carriers and officers were asked to estimate the approximate age of the broods they observed.

This posed a technical problem in instructing large numbers of untrained cooperators by correspondence. This was done by specific instructions and comparisons with other birds, such as robins, quail and crows, and by the use of visual aids. I do not know the actual average age of 1/4-grown chicks, for example, as they are recorded by carriers and officers. But one can expect that with the large bulk of observations and about the same group of cooperators each year this actual age, whatever it is, should be the same from year to year.

In addition to separating broods by age, I felt it was also necessary to compare brood sizes which were determined during a rather short period of time. Even though the chicks are aged correctly, brood sizes obtained by lumping data from the three months of June, July, and August, must be used discriminately. For example, in 1946 carriers observed 357 1/4-grown broods in the first half of June, for an average reported brood size of 8.4. In the first half of July that year, they observed 525 1/4-grown broods for an average reported size of 6.7, a difference of 1.7 chicks! This difference may be real, or it may be in part due to differences in cover conditions and hence visibility of the broods.

Finally, I felt that any study of brood sizes should involve large numbers of observations. With an average which could be expected to be somewhere around 6 to 8 chicks, with sizes varying from 1 to 20, the variation might be expected to be large.

The sizes of 8,787 broods observed by carriers in late July are shown in Table 6.1. Brood size does not appear to shift appreciably from year to year. The differences the table does show could be largely a reflection of sampling variation. The larger spread shown by 3/4-grown broods is suspect because phenology from year to year might cause a differential due to chicks maturing and leaving the brood. Ignoring 1946, the spread is only .41 chick, comparable to the 1/2-chick variation in quarter and half-grown broods.

There is one obvious reservation to the conclusion that brood size does not change significantly--there is no way of knowing how accurate the carriers' counts are. The final proof of their accuracy must lie in performance. This is discussed in the next chapter. Some insight into the reliability of the carriers' brood sizes might be provided, however, by inspection of the frequency distribution of their observations.

In Figure 6.4, the frequency distribution of observations of 1/2-grown broods for four years is graphed. With no attempt at this point to evaluate this distribution statistically, there is evidence that the carriers are not just guessing at observations. The curves are roughly similar from year to year. There is not much evidence that they are prone to lump observations in the

TABLE 6.1

SIZE OF BROODS REPORTED BY CARRIERS

Year	1/4 Grown	1/2 Grown	3/4 Grown	All Broods
1946	6.23 (497)[1]	6.21 (899)	5.97 (654)	6.14 (2050)
1947	6.65 (286)	6.48 (335)	5.38 (128)	6.36 (749)
1948	6.45 (565)	6.38 (804)	5.08 (379)	6.12 (1748)
1949	6.22 (568)	6.10 (957)	5.08 (509)	5.88 (2034)
1950	6.67 (774)	6.63 (1031)	4.97 (401)	6.34 (2206)
5 Year Av.	6.44	6.35	5.36	6.14
Tot. Spread	.45	.53	1.00	.48

[1]Figures in parenthesis are number of broods on which each brood size is based.

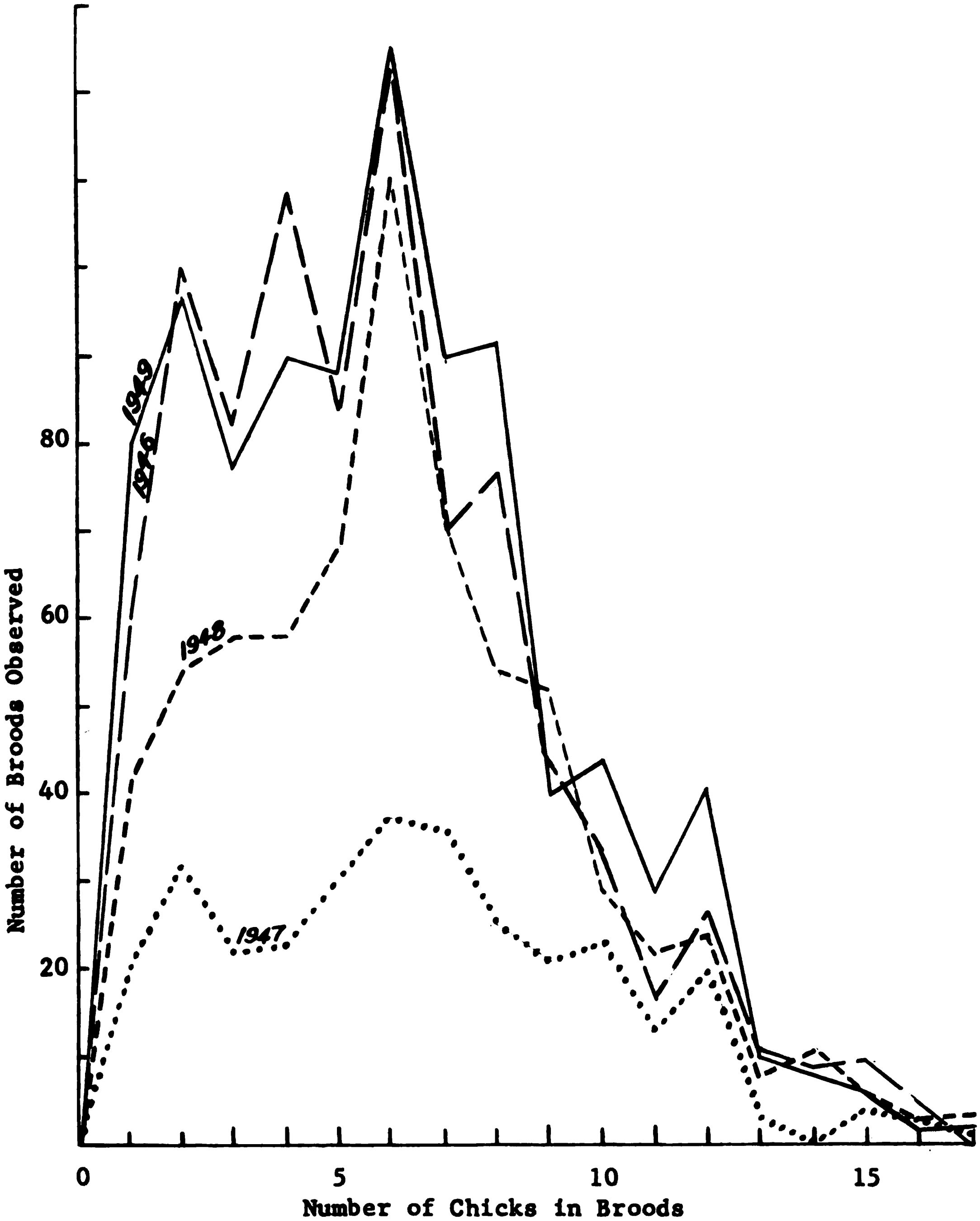

Fig. 6.3--Frequency distributions of sizes of half-grown broods observed by carriers.

"5" or "10" categories, as so often happens (although there is a slight tendency for even numbers to be higher than odd).

Further analysis of brood size could be pursued; logically, brood size should be compared by study area. That is not in the province of this study, however, since we are interested in brood size principally for its effect on estimates of total populations. L. L. Eberhardt analyzed brood sizes based on the data in Table 6.1, as well as similar data from later years, and concluded that brood size appeared to have no significant difference from year to year, but did have significant variation from area to area.[1] Even so, there is still no way of knowing whether these differences are real life history phenomenon, or merely differences in simple observability of pheasants. There are obviously real differences in cover conditions between the flat open land types of the lake-bed clay country and the rolling, brushy types in southwestern Michigan.

6.5 Percentage of Hens Without Broods

There has been a suspicion in Michigan that loss of production has been fewer broods produced for some reason or another, rather than differences in the degree of attrition of chicks from broods. The studies of brood size in the previous section support this suspicion.

During these July brood counts carriers were asked to record the hens they saw which apparently had no broods. Those hens

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[1]Unpublished data in Game Division files.

which have not hatched a brood by late July are extremely unlikely to contribute many young birds to the shootable fall population. A cock hatched on the first of August would hardly be colored enough by mid-October to be easily recognizable in the field as a cock.

Data were tabulated as percentage of hens seen that had no visible broods. For the five years of this study, this percentage was as follows:

1946	41%
1947	47%
1948	37%
1949	35%
1950	37%

For the two years with poorest pheasant production, 1946 and 1947, these percentages were highest, so this percentage may reflect production in a general inverse way. Its specific use as an indicator of production, however, is doubtful at this time. Observability of hens with or without broods may vary, so this percentage may be an index rather than absolute. The best evaluation of this percentage must wait until we have a reliable index to true productivity based on true fall age (and sex) ratios.

6.6 Conclusions

Brood counts increase at a predictable rate during the summer period. Counts are apparently not appreciably affected by rainfall. On the basis of nearly 9,000 observations of broods by carriers, no significant difference in brood size from year to year could be detected in a five-year period. The percentage of hens without broods observed may reflect good or poor production years, but is probably not usable to calculate specific productivity.

Chapter 7

PREDICTION OF FALL KILL FROM SUMMER BROOD COUNTS

7.1 Introduction

The principal reason for the search for an accurate measure of pheasant populations in mid-summer was to find a method for predicting fall populations, as a basis for setting hunting season regulations. During the first few years of the summer brood counts, there appeared to be general correlation between increase and decrease of the brood counts, and increase and decrease of the kill during the corresponding fall. This general correlation seemed to exist for the study areas as well as for the entire state.

In 1949, with four years of data to work with, a series of 16 different correlation coefficients were calculated, analyzing the data on the basis of county, area, and year, and weighting the areas on the basis of size. Values of "r" for these correlations are shown in Table 7.1. These values are not particularly pertinent nor useful, except to show that excellent correlations existed, even on the individual county level; none was below .65 and 9 of the 16 were above .90.

7.2 Correlation of Carriers' Brood Counts with Computed Kill

The regression of computed kill on broods seen by carriers by study areas for the four years 1946-49 is shown in Figure 7.1.

TABLE 7.1

VALUES OF CORRELATION COEFFICIENTS (r) FOR 16 COMPARISONS OF COMPUTED KILL WITH CARRIER BROOD DENSITY INDICES

Data Level	Correlated Unit	No. of Points	r Values
County	1946	27	.99
	1947	28	.95
	1948	38	.65
	1949	37	.85
	1946-49	130	.79
Year	Area 1	4	.98
	Area 2	4	.65
	Area 3	4	.97
	Area 4	4	.99
	Area 5	4	.74
Area	1946	5	.97
	1947	5	.88
	1948	5	.93
	1949	5	.90
	1946-49	20	.91
Area (weighed)	1946-49	20	.93

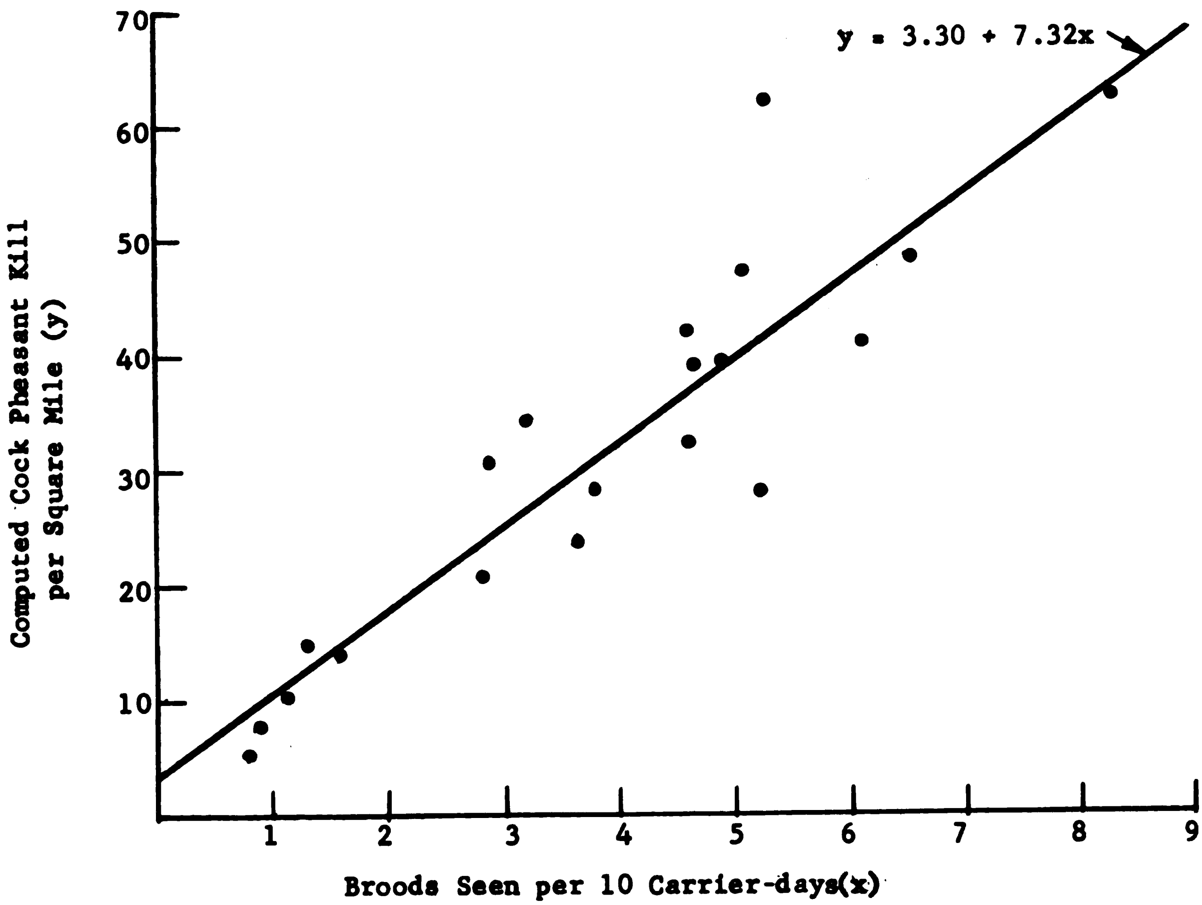

Fig. 7.1--Regression of computed kill on carriers' brood density indices for study areas, 1946-49.

This regression line could obviously be used to predict fall kill from the carriers' brood counts.

The four years used in these correlations represented relatively low pheasant populations, however, and assumed a straight-line relationship. The regression formula shown in Figure 7.1 might not necessarily be valid for predicting kill on the basis of carriers' brood counts during years of higher pheasant populations. Following 1949, carriers' counts in July were continued each year in the same fashion, and at the same time the pheasant kill was computed from hunter report cards through 1956, when the system was discontinued and replaced by a sampling system.

On the basis of 11 years of data, the relationship between carriers' brood observations and computed kill the following fall can best be shown by the regression illustrated in Figure 7.2, where computed kill is plotted against the logarithm of broods seen per 10-carrier days. The correlation coefficient is .978--an extremely close correlation. Since there is an unknown sampling error in each of the measures involved in this correlation, this "r" value may be less than the true correlation. Nevertheless, its usefulness in predicting fall kill from summer brood counts is obvious. Table 7.2 shows the detail in support of Figure 7.2.

Figure 7.3 compares the computed kill with brood density by area for the 11 years. There is, of course, more spread since the areas involved are smaller. Because of the likelihood of differences between areas (such as brood size mentioned in Chapter 6 and discussed in the next section), it is impractical to calculate

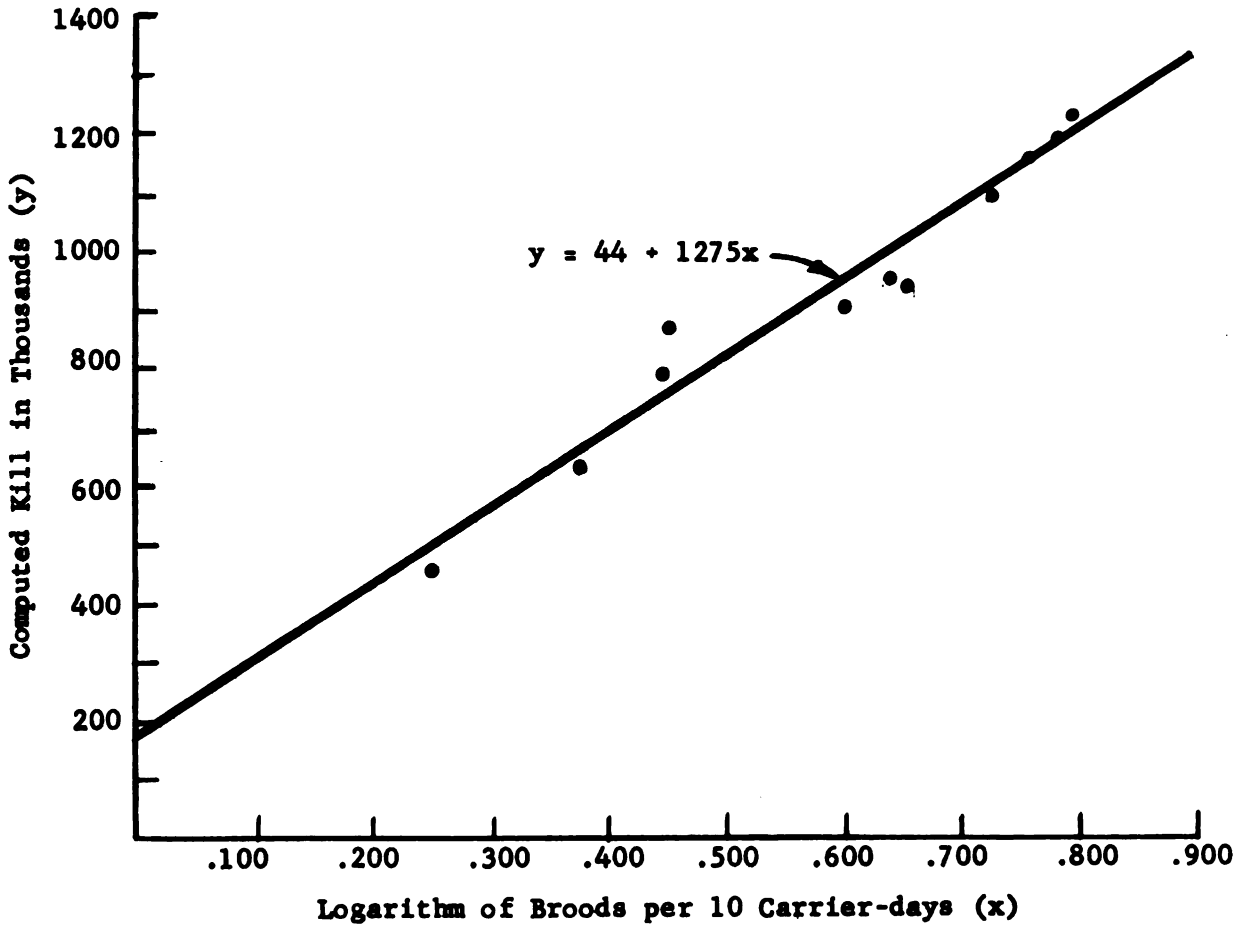

Fig. 7.2--Regression of computed kill on logarithm of carriers' brood density index for primary pheasant range, 1946-1956

TABLE 7.2

REGRESSION OF COMPUTED KILL ON LOGARITHM OF CARRIERS' BROOD DENSITY INDICES

Year	Broods per 10 C. D.	Log Broods per 10 C. D. (X)	Kill In Thousands (Y)
1946	5.04	.7024	904.4
1947	2.25	.3522	452.9
1948	2.97	.4728	632.7
1949	3.61	.5575	864.0
1950	3.60	.5563	797.5
1951	5.74	.7589	943.7
1952	5.49	.7396	947.9
1953	7.27	.8615	1,144.9
1954	7.68	.8854	1,178.4
1955	7.86	.8954	1,230.7
1956	6.74	.8287	1,101.0
Sums		7.6107	10,198.1
Means		.69188	927.100

$Y = 927.100 - 882.348 + 1275.29X \qquad r = .978$

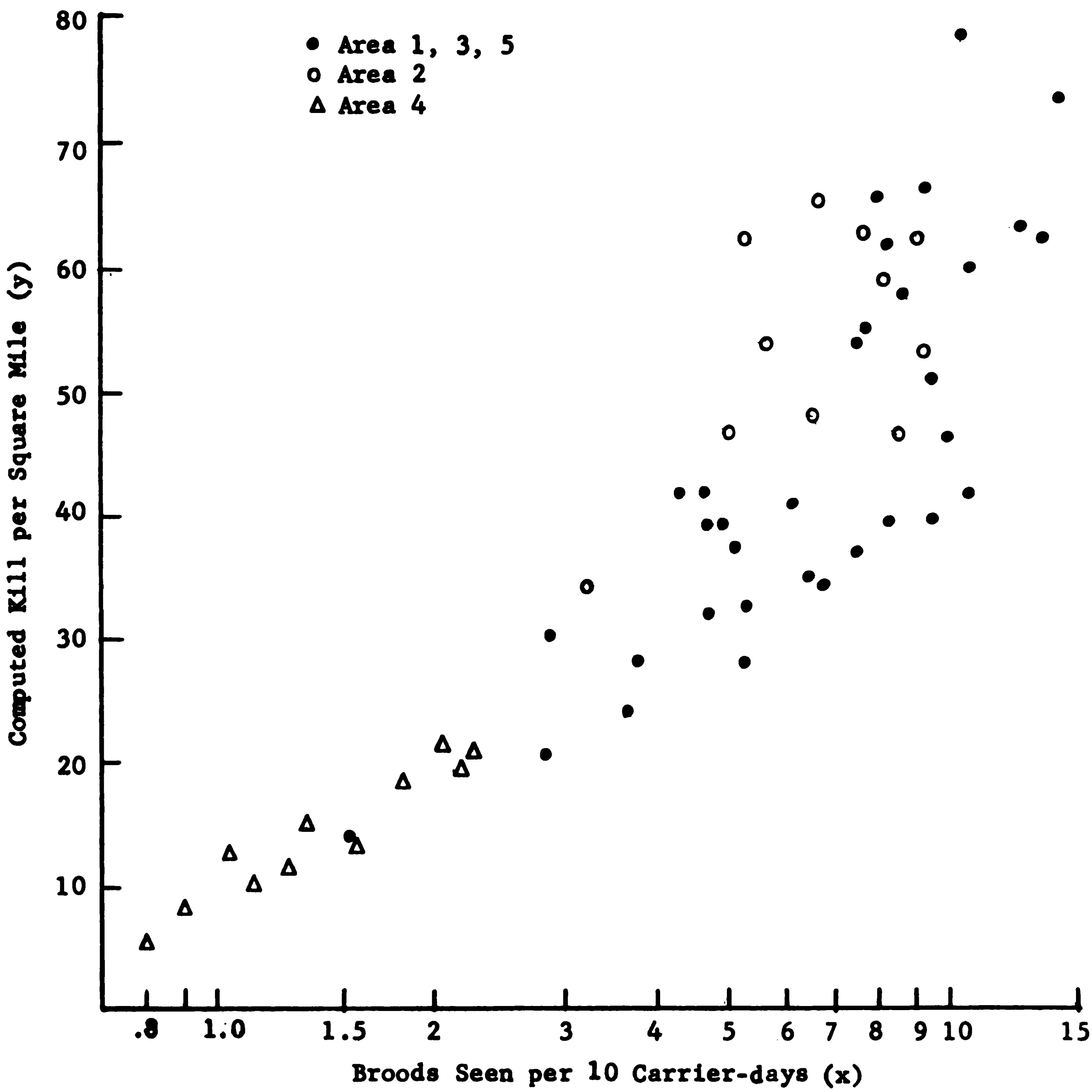

Fig. 7.3--Comparison of computed kill with logarithm of carriers' brood density index for study areas, 1946-1956.

the regression on the basis of these points; rather, regressions should be calculated separately for separate areas, or groups of similar areas, when enough data have been accumulated.

To illustrate this likelihood of differences in regression between areas, the points for the two most diverse areas--2 and 4--are identified in Figure 7.3. Regression formulae calculated for each would be quite different.

On the basis of the regression shown in Figure 7.2, and with the benefit of hindsight, one can calculate how accurately the kill might have been predicted from carriers' brood counts for the 11 years involved in the regression. Table 7.3 shows the deviation from the regression for the individual years.

7.3 Source of Error in Correlation of Brood Density with Computed Kill

This method of predicting fall kill may appear disarmingly simple. It by-passes a number of factors or considerations, or makes a number of assumptions which are not particularly proven. The proof of the system is in its performance.

Six of these factors are discussed briefly below. It must be remembered that the simple fact that the system works is not proof of any of these assumptions. I do not know to what extent errors introduced by one factor might be compensated for by another. Nevertheless, the fact that none of these considerations has seriously skewed the regression curve in the 11 years involved lends support to tentative conclusions regarding them.

In essence, one must assume a consistent relationship between the broods present in late July and the October population from year to year. If the following factors are not compensating, then

TABLE 7.3

ERROR IN PREDICTION OF COMPUTED KILL FROM CARRIERS' BROOD DENSITY INDICES

Year	Kill in Thousands		Approx. Error in Per Cent of Computed Kill
	Predicted	Computed	
1946	935	904.4	3%
1947	495	452.9	8%
1948	645	632.7	2%
1949	750	864.0	15%
1950	750	797.5	6%
1951	1,010	943.7	6%
1952	985	947.9	4%
1953	1,140	1,144.9	0%
1954	1,175	1,178.4	0%
1955	1,180	1,230.7	4%
1956	1,095	1,101.0	1%
			Av. 4%

each, in turn, must be quite consistent from year to year.

1) Late summer mortality. Following grain harvest in mid-July, when most chicks are at least half-grown, mortality to young caused by weather or food shortage should be at a minimum. Barring an epizootic or similar catastrophe, mortality would logically be expected to be consistent from year to year. This is in sharp contrast to the preceding period, from the breeding season through hatching, when weather and even food conditions could conceivably alter mortality radically from year to year.

The fact that the size of 1/2-grown broods observed by carriers does not change appreciably from year to year would suggest that mortality to chicks in the early summer, when chicks are small, must operate principally on whole broods rather than individual chicks. Thus, our conclusion that loss of pheasant production seems to operate on the entire brood or the hen before she brings off a brood rather than attrition to individual chicks.

2) Per cent of the fall pheasant population harvested. Small game hunting license sales and resulting hunting pressure in Michigan are high enough that pheasants are probably hunted to the point where a 10 per cent change in hunting pressure would not greatly change total kill. License sales changed less than 5 per cent each year in 6 of the 11 years, and in 2 other years the change was less than 8 per cent (see Figure 2.1). The year 1946 was a striking exception; sales were 25 per cent higher than in 1945 and 22 per cent higher than in 1947. This increase is generally credited to the interest of returning servicemen in 1946, many of whom may have had a particular interest in hunting that

year which dissipated in following years. In many ways, the 1946 data were more at variance with the "average" than other years. Season length and bag limit were reduced in 1947, 1948 and 1949 (see Figure 2.1).

About 70 per cent of the present 22-day season's total kill usually occurs during the first two days and the first weekend. So even the 12-day season in 1947 and 1948 was probably enough to harvest the pheasant crop, and the total kill was probably but little less than it would have been had the season been 22 days long those years. This is especially true since during these three years of restricted seasons pheasant populations were low.

Regulations have specified various opening hours, particularly on the first day or two of hunting. This has apparently affected total kill but little. Analysis of the Rose Lake data, where data from previous years were available for comparison, did not show any change in total season kill traceable to a change in opening hours (Black, 1950).

Hunting conditions are popularly supposed to have considerable influence on the harvest of cocks. The conditions most regularly mentioned are weather (principally rain, temperature, moisture conditions on the ground, snow cover and wind) and cover (amount of standing or unharvested corn, lushness of the season's growth, and whether or not there have been frosts heavy enough to knock down some herbaceous growth). Although hunters may fancy that these things affect their individual success, I was not able to detect any measurable effect these conditions have on total state kill for any season. Quite probably these conditions are less influential than the average hunter supposes, and quite probably the conditions vary much less than he supposes.

3) Age ratios of cock kill. Determinations of ages in the fall kill show a shift as the season progresses (see Chapter 8). True age ratios of the fall cock population are unknown. Yet a year of good brood production will ordinarily have a higher percentage of young birds in the fall than will a year of poor production. Since the fall kill is so dependent upon the young cocks of the year, one might suppose that changes from year to year in the percentage of young cocks in the male segment of the population will upset this correlation.

The effect of a change in age ratio need not be as influential as one might suppose, however. It is common to have very disparate observed age ratios in the kill--perhaps as high as 1 adult to 15 or even 20 young. In these instances, adults would compose from 5 to 7 per cent of the kill. Thus, though a changing number of adults caused a shift of age ratio from 1:15 to 1:20--a 33 per cent change in numbers of adults--the net change in the kill would be only 2 per cent. If, of course, the number of adult cocks stayed the same and the shift was caused by a different number of young, this would be a net change of about 33 per cent--but that would be measured in the carriers' brood density index. It may well be that the net percentage gain or loss of the adult proportion of the kill is not large in any one year, and hence does not greatly influence the correlation.

4) Size of broods. Brood size apparently does not change appreciably from year to year in Michigan (Section 6.4).

As a further check against the influence brood size might have, correlations of brood size with carriers' brood density indices and with computed kill for eleven years were calculated (Table 7.4). No significant correlation was found.

5) Precision and accuracy of brood surveys and computed kill. There is no clear-cut measure of the precision of each of these population measurements now available. Each is a sampling system--each has a sampling error. Again, we must fall back on the performance of the system to determine its usefulness. The likelihood that the computed kill is a useful index to actual kill was discussed in Chapter 2. The accuracy of the brood density index is, in turn, determined by how accurately we can predict the kill from it.

6) Timing of the brood survey. The importance of timing brood surveys was discussed in Section 6.3. Determining the phenological status of pheasant brood production is extremely difficult. The dates in July that I picked for the carriers' surveys each year were carefully weighed in the light of that spring's phenology. We do not know precisely how phenology affects pheasant breeding. But if the peak of cock crowing, for example, appears to be a week later one year than another, it would be reasonable to try to run the mail carrier counts one week later.

The opening of the pheasant season was changed from October 15 to October 20 in 1952. The population is undoubtedly a trifle lower on October 20 than on October 15.

Assuming the 1.65 per cent daily expected increase in broods seen by mail carriers (Section 6.3) is valid, the 15 per cent

TABLE 7.4

CORRELATIONS OF BROOD SIZE AND TWO POPULATION INDICES

Year	Kill in Thousands (X_1)	Broods Seen per 10-Carrier Days (X_2)	Average Brood Size (X_3)
1946	904	5.04	6.4
1947	453	2.17	6.3
1948	633	2.85	6.2
1949	864	3.63	5.9
1950	798	3.60	6.3
1951	944	5.74	6.2
1952	948	5.49	5.9
1953	1,145	7.27	6.1
1954	1,178	7.68	6.1
1955	1,231	7.86	5.9
1956	1,101	6.74	5.8

$r_{x1x3} = -.546 \qquad P_{05} = .602$

$r_{x2x3} = -.474 \qquad P_{05} = .602$

maximum error in our predictions (Table 7.3) could have been accounted for by a 10-day timing error in mail carrier counts.

All of these seven variables are capable of introducing error. Some, such as hunting regulations, may vary with population levels and be automatically compensated for; others may compensate each other. The sum total of these errors, however, must be contained within the error shown by the regression in Figure 7.2. Correction of the possible errors mentioned above can only refine this regression which, as it now is, appears to offer good information.

In the final analysis, one strong proof of both the kill figures and the carriers' brood surveys is their consistently small deviation from the regression. It would quite likely be rewarding to try to compensate for some of these errors--particularly timing of the counts. But to be able to predict fall kill from a summer count with an error averaging 4 per cent is a rare and satisfying experience for the game manager.

Chapter 8

FALL POPULATIONS

8.1 Introduction

In Chapter 2 the computed kill figures were appraised, and in Chapter 7 it was shown that there was an extremely close relationship between the number of broods present in the summer and the fall kill. This demonstrated that the fall kill is almost directly dependent upon the brood production the previous summer. Adult birds contributed either so small or else so consistent a proportion of the kill that they did not seriously affect this relationship.

It follows then, that total kill, accurately determined, is a good index to the fall population. If we knew the fall sex ratio, and if we knew what percentage of the fall cock population was harvested, it would be possible to calculate the fall population.

This chapter deals with other indices to the fall kill, attempts to determine pheasant numbers by other than total fall kill computations, and attempts to determine fall sex and age ratios.

8.2 Preseason Sex Ratio Surveys

In the fall, just prior to the hunting season, the sex ratio of a pheasant population is usually close to 1:1. Despite the fact that pheasants are polygamous, and that in a hunted population a sizable percentage of the cocks are removed each fall, young of the year about equally divided as to sex are the preponderant part of the fall population. Moreover, the disparate sex ratio of adults in the winter is probably brought closer to 1:1 during the summer by a higher mortality of hens, due to the increased hazards of nesting (mowing machines, increased vulnerability to predators, etc.).

Sex ratio observations on opening day of hunting season. The most common attempt to get a preseason sex ratio has been from sight observations of pheasants by hunters on the opening day or days of hunting seasons. Allen (1942) devised a system for calculating preseason populations, in which opening day sex ratio observations played a part. While such observations have some merit, they have two drawbacks. (1) Observations collected from a large number of hunters are not particularly accurate, especially when large numbers of pheasants are observed in a day. (2) The removal of cocks by legal hunting during the observation period, even though it be for a period as short as one day, prejudices the ratio in favor of hens. This is a serious handicap when opening day pressure is heavy, as it is each year in almost all of Michigan's primary pheasant range. It has been a common occurrence for one-third of the total pheasant kill to be taken on an opening day. On the Prairie Farm, for example, the percentage of the total kill that was taken opening day for a number of years was as follows:

(Shick, 1952)

1937	1938	1939	1940	1941	1942
19%	38%	50%	33%	35%	30%

Similarly, during the eight-year period 1951-1958 at the Rose Lake Wildlife Experiment Station, an average of 34 per cent of the total season pheasant kill was taken on the opening day.

In Table 8.1 observed sex ratios on opening days at the Prairie Farm and Rose Lake are compared to the calculated preseason population for a number of years--some of high pheasant populations, some of low. There does not appear to be a correlation between relative abundance of birds and the disparity of the sex ratios.

It is likely that there is a considerable error in these opening day observations.

Preseason roadside counts. In 1948 officers were asked to keep track of the pheasants they saw from September 15 to October 15 when the pheasant season opened.

Since there were certain to be many birds in broods too young to be sexed, observations of these young were recorded separately. Half of these young birds could be assumed to be cocks, half hens. The total of such observations was halved, and half added to the cock tally, half to the hen. Results are shown in Table 8.2.

These results are implausible. The total number of birds observed per mile of travel dropped far below the July brood counts, indicating that the birds were relatively more difficult

TABLE 8.1

OBSERVED SEX RATIOS ON OPENING DAYS

Year	Prairie Farm[1]		Rose Lake[2]	
	Opening Day Sex Ratio (Hens/Cock)	Calculated Preseason Cock Population per 100 A.	Opening Day Sex Ratio (Hens/Cock)	Calculated Preseason Cock Population per 100 A.
1937	1.2	13.7		
1938	1.4	21.4		
1939	2.0	20.4		
1940	1.6	17.5	.9	18.0
1941	1.0	22.9	1.3	31.7
1942	1.4	19.9	1.3	16.7
1943			1.3	11.6
1944			1.0	15.6
1945			.7	7.3
1946			1.3	5.2
1947			1.2	4.4
1948			1.4	6.2
1949			.9	6.5
1950			.8	5.2

[1]Shick (1952).
[2]Rose Lake files.

TABLE 8.2

FALL PHEASANT OBSERVATIONS BY OFFICERS

Date (1948)	Miles	Pheasants Observed					Sex Ratio Hens per Cock
		Cocks	Hens	Chicks	Total Cocks	Total Hens	
Sept. 15-30	46,418	1,088	972	322	1,249	1,133	.91
Oct. 1-15	54,444	3,484	1,856	181	3,574	1,946	.54

to see at this time of the year. Even at this date, pheasants appear to be segregating by sex, to some degree. In many broods so young that the cocks were only partly colored, it would obviously be very easy to mistake a young cock for a hen.

Shick (1952) was unable to obtain a satisfactory preseason sex ratio from field observations on the Prairie Farm. Stokes (1954) attempted to obtain a preseason sex ratio on Pelee Island in the late 40's. Three different types of sampling he tried resulted in three widely differing sex ratios, none of which seemed plausible. He laid part of the difficulty to the fact that cocks were engaging in fall sexual display at this time of the year, and hence were disproportionately obvious to the roadside counter.

This type of survey was abandoned. It does not seem to warrant further study.

Sex ratios based on brood counts. It has been suggested that a sex ratio might be obtained from the July brood counts by assuming equal division of the chicks as to sex, and adding those data to the counts of hens and the estimated cock population. We have no way of knowing whether survival of broods is similar to survival of adults from late July to the hunting season, nor do we have valid data on the adult sex ratio at that time of the year, nor can we be confident that the observability of hens with broods is at all similar to that of hens without broods. Hence, I dismissed the possibility of determining a sex ratio at this time of year from brood counts.

8.3 Age Ratios in the Fall Kill

During the 1948, 1949, and 1950 hunting season, biologists and student aides interviewed hunters for bag checks, and aged several hundred cock pheasants in the bag each year. Bursa measurements, supported by the mandible test, were used (Linduska, 1945; Figure 4.1). Although ages were recorded for several hundred birds each year, the ratios of these cocks examined is of questionable worth.

Kimball (1948) found that in South Dakota in 1947, examination of nearly 12,000 birds (a more than adequate sample!) resulted in a progressive shift of the young:adult ratio from 5.0 to 1.2 over a six-week period. Allen (1941), Stokes (1954), and others have noted similar decreases in proportion of young in the cock kill as the season progresses.

In the 1949 season, biologists and aides measured the bursae of about 775 cock pheasants. There was considerable overlap in the measurements, with no clear-cut distinction between adults and juveniles.

Petrides (1949) has succinctly pointed out the difficulty of obtaining rearing success from age ratios determined by examination of cocks:

1) "small" errors in aging may cause large errors in interpretation, even though misidentification of adults and young are somewhat compensating
2) extent of adult mortality must be known
3) the adult sex ratio must be known
4) samples must be carefully taken

Despite the care taken to obtain good age ratios of cocks shot, I do not consider that the ratios were reliable indicators of pheasant rearing success; spring sex ratios for the period were not reliable, the samples (areas covered) were not similar from year to year, and there is some question about the accuracy of the examinations.[1] This does not preclude the possibility that a careful sample taken each year might yield an approximate idea of relative rearing success in a poor production year compared to a good production year.

As a result of my somewhat negative conclusions as to the value of our observed age ratios, Eberhardt and Blouch (1955) made a more thorough study of this shift in sex ratios by day of the season for the years 1950-1953. They also concluded that age ratio comparison might be made from examination of first day age ratios where hunting effort in the areas to be compared is comparable. This will still not provide a method for converting age ratios to true rearing success.

8.4 Hunter Success as a Population Measurement

Hunter success is a much misunderstood and often misused statistic. The layman is prone to use it as an index to total pheasant populations. Hunter success can be a good index to

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[1]For administrative reasons, emphasis on bag checks varied from year to year and from area to area each year. Thus, combination of these unweighted samples was not practical for direct comparisons from year to year.

total populations, so long as it is used as a comparative figure under similar conditions, most important of which is hunting pressure. The harvestable surplus of pheasants for any given area may be taken by varying numbers of hunters (Allen, 1947), with inversely varying success. Thus, total kill figures for any area are likely to be more nearly an index to actual populations than the success of the hunters who harvested that total kill.

During the years of this study, hunter success samples obtained from hunter bag checks were not strictly comparable as to effort expended and the location in which the samples were taken, so they are not easily comparable. A hunter success factor enters into the tabulation of hunters' report cards to determine total kill, however, (Section 2.4). In this instance, the success factor (birds shot per hunter reporting) is used legitimately, since the system assumes a comparable sample from year to year, and hunter success is not used as a statistic on its own, but rather as a step in a computation which considers total hunters as well as their success.

8.5 Hunter Opinion Polls

As they were checking hunters' bags, biologists asked hunters whether they had seen more, the same, or fewer pheasants than the previous year. The several thousand hunters interviewed each year responded as follows:

	1946	1947	1948	1949
More	56%	5%	50%	70%
Same	25%	80%	30%	16%
Fewer	19%	15%	20%	14%

This summary of hunter opinion does not offer any apparent index to pheasant populations, although the percentage reporting more birds may indicate, very grossly, sizable increases or decreases in pheasants from year to year. Unaccountably, about the same percentage of hunters reported fewer pheasants--in good years and bad--in spite of extremes from 5 per cent to 70 per cent reporting more birds.

Surprisingly, the percentages of hunters reporting the three classifications of abundance remained relatively constant throughout the season. In 1949, for example, the percentage of hunters who reported seeing more pheasants than in the 1948 hunting season was as follows:

Days of Season	Percentage Reporting More Pheasants	Number of Hunters Interviewed
October 15	65%	1,053
October 16	77%	790
October 17-23	77%	556
October 24-30	75%	380

8.6 Conclusions

Attempts to determine density and sex ratio indices from roadside surveys in the fall were unsuccessful.

Sex ratios observed by hunters on the opening day of pheasant seasons were unreliable as preseason sex ratios, due to unreliability of hunters' observations and the removal of as much as one third of the cocks shot in the season during the one day of observation.

Age ratios determined by biologists from bursa and mandible examinations were not clear-cut. Moreover, age ratios shifted during the season because of differences in vulnerability of adults and juveniles to hunting.

Because of this uncertainty of sex ratios and age ratios, I could not determine any indices to true productivity (rearing success).

Hunter success should be used as an index to populations only when compared to hunter success indices obtained under similar conditions, especially with regard to hunting pressure.

I conclude that the computed kill remains the best index to fall populations.

Chapter 9

WINTER POPULATIONS

9.1 Introduction

Changes in the habits and observability of pheasants in winter and spring cause extreme variation in observed pheasant sex ratios and density indices. For example, Linduska had the officers record the pheasants they saw while driving during the first months of 1946 (Table 9.1). Obviously, observed sex ratio and density shifted radically. The variability continued into April (Chapter 5). It has been commonly suspected that snow cover might have an influence on observability of pheasants; and indeed, the data in Table 9.1 would support that suspicion to some degree--but in no particularly obvious pattern.

9.2 Carriers' Postseason Surveys

Results of carriers' surveys made following the hunting seasons in 1946-1949 are shown in Table 9.2. On some days counts were more than double counts on other days. It was apparent that short one or two week surveys with such large daily variation could not be expected to yield accurate indices to densities or sex ratios. Examination of the data further supported the earlier suspicion that amount of snow cover might have considerable influence on the number of pheasants observed each day. It was not practical to ask the carriers to count for protracted periods of time.

TABLE 9.1

OFFICERS' LATE WINTER SURVEY - 1946

	February		March		April	Total
	1-15	16-28	1-15	16-31	1-15	
Miles Driven	7,679	20,210	28,115	30,987	20,294	107,285
Total Birds Seen	1,286	3,633	2,553	1,035	682	9,189
Birds/100 Miles Driven	16.7	18.1	9.1	3.3	3.3	8.6
Sex Ratio (hens per cock)	6.1	7.3	5.5	3.8	2.1	5.4
Average Snow Depth (inches)	5.47	17.94	1.47			

TABLE 9.2

SUMMARY OF CARRIERS' POSTSEASON SURVEYS

Year	No. of Carriers	Item	Pheasants Observed on Days in December						Total
1946	329	Day	2	3	4	5	6	7	
		Total	512	384	624	512	457	554	3,034
		F/M	4.0	3.8	4.5	3.4	3.0	3.9	3.7
1947	321	Day	8	9	10	11	12	13	
		Total	376	347	356	393	423	502	2,397
		F/M	2.0	2.5	2.1	2.2	2.2	2.4	2.2
1948	388	Day	6	7	8	9	10	11	
		Total	354	346	274	288	343	356	
		F/M	1.6	1.8	1.8	2.2	2.2	2.2	
		Day	13	14	15	16	17	18	
		Total	314	356	288	316	240	367	3,842
		F/M	1.7	1.9	2.3	2.3	1.6	2.3	2.0
1949	523	Day	(28)[1]	(29)	(30)	1	2	3	
		Total	1,370	1,225	1,008	1,004	809	669	
		F/M	3.4	2.9	3.9	3.2	3.2	2.6	
		Day	5	6	7	8	9	10	
		Total	715	718	741	1,081	1,543	1,603	12,546
		F/M	3.1	3.0	3.4	3.1	4.4	3.5	3.3

[1]Days in parentheses are November dates.

9.3 Officers' Winter Surveys

During the winters of 1948-49 and 1949-50, I had the officers record daily observations of pheasants for the entire winter period, and I compared their observations to depth of snow on the ground.

The "officer-day" was used as the unit of observation; it was impractical to obtain the daily mileage of officers. But as in the case of the carriers, the routineness of their duties permits an interchange of units--miles or days--with no appreciable error. (Interchangeability of units is discussed in Section 4.4).

Weather records were obtained from the U. S. Department of Commerce, East Lansing Weather Bureau Office, or from the Monthly Climatological Summaries published by that office. With the help of A. H. Eichmeyer, in charge of the East Lansing office, I selected one reliable[1] weather station as near the center as possible of each of the five study areas.

To obtain a daily average snow depth measurement that was representative of the 38 counties in the southern third of Michigan, I averaged the snow depth recorded at each of the five stations for each day.

Beginning in the winter of 1949-50, snow depth was recorded at Michigan stations as a "trace" (.4 of an inch or less) or to the nearest whole inch. For statistical purposes, a trace was

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[1]Care must be used in selecting weather stations because the reliability of the weather bureau cooperators, the adequacy of their equipment, and the location of each station in relation to factors that might affect snow readings, varies from station to station.

valued arbitrarily as .2 of an inch, an average of from .0 to .4 of an inch of snow. Since .2 is close to zero, no appreciable error from this arbitrary selection was anticipated.

The officers' daily observations for the two winters, 1948-49 and 1949-50 are graphed and compared with daily average snow depth in Figure 9.1. Observations were smoothed. After trying one month's data on a 3-day, 5-day, and 7-day moving average, I decided to use the 3-day moving average, since it seemed to smooth without obliterating peaks, and yet made comparison much easier. The snow depth curves were not smoothed since there was little reason to suppose that observability of pheasants was affected by a given snow depth for more than a day before or after the day of observation. Thus in effect, I assumed that the effects of snow cover on observability of pheasants was essentially mechanical; that snow cover makes the birds easier to see, rather than seriously affecting the habits of birds.

This is probably not strictly correct. Atmospheric conditions may affect the habits of pheasants, and storms certainly do. Of course, excessive snow depth may affect habits of birds. Nevertheless, for the purpose of this study, the reasons for changes in observed density, whether due to behavior of the birds or simple observability or both, are of incidental importance--major concern here is in the observability of pheasants, *per se*.

From Figure 9.1 it can be seen that pheasant observations, smoothed by a 3-day moving average, followed snow depth remarkably well. It appeared from these graphs that snow depth had an important effect on the observability of pheasants. It should be

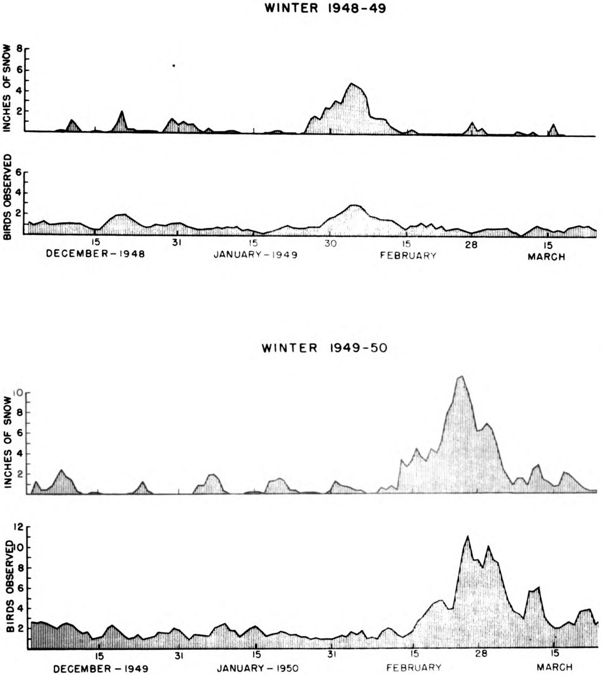

Fig. 9.1--Comparison of daily average snow depth with daily pheasant observations by officers. ("Birds observed" smoothed by 3-day moving averages)

pointed out that the graphs in Figure 9.1 are for the purposes of inspection only, since observations are smoothed. For the statistical studies, the original data were used.

No allowance was made for mortality over the winter. Any mortality that occurred would probably weaken these correlations; hence, if mortality could be considered, the correlations would probably be even better.

The regression of birds seen on snow depth was calculated for the two winters. The formulae and graphs of this regression are shown in Figure 9.2.

If the effect of snow depth on observability of pheasants is a constant factor from year to year, then the difference between these two lines should be due to differences in population size. In other words, if the difference between the years lies only in pheasant population densities (and not in observability), then the ratio of the numbers of pheasants seen in the two years at the same snow depths should be constant, and that constant ratio would be the ratio of the two population densities. Whether the two lines do hold the same ratio may be appraised by comparing the constant terms, the "y-intercept" (a) and the "slope" (b). The ratio of the y-intercepts is 2.22 and that of the slopes is 1.63, so it seems that the lines do not hold a constant relationship, suggesting that observability changed as snow depth increased.

If snow depth has such an effect on observability of birds, quite possibly observations made only on days when there was no snow on the ground might offer a better comparison of density from one year to another. Comparing the officers' observations

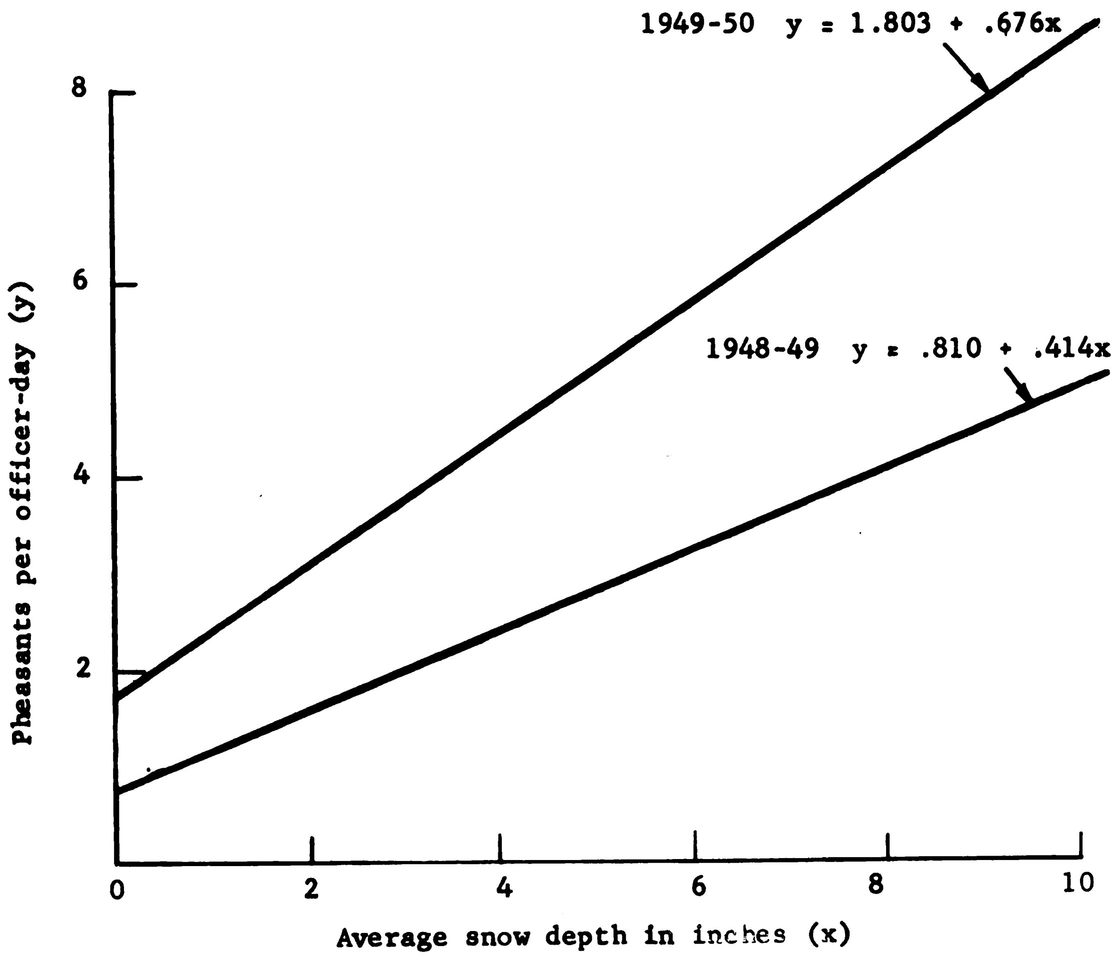

Fig. 9.2--Regression of pheasants observed per officer-day on daily average snow depth.

for only days with no snow cover or a trace of snow present (Table 9.3), we find that they saw 60 per cent more birds in 1949-50 than they did in 1948-49, on days with no snow. This is not consistent with the 122 per cent increase suggested by y-intercepts in the regression lines in Figure 9.2.

Differences in the total population would, of course, reflect differences in the cock and hen component of each year's population. Regression lines for cocks and hens separately are shown in Figures 9.3 and 9.4.

To analyze this further, let us assume the model shown in Figure 9.5. If P is the observability of pheasants (or the proportion of a population seen) then we may see a certain number when there is no snow on the ground (a) and increasingly more at the rate (b) as snow depth increases. Of course, this may not be a straight line regression. It may be a curve or there may be a decided jump as soon as some snow falls, etc.

This change in visibility as snow depth increases is complicated by the probability that observability of cocks and hens behaves something like the model in Figure 9.6; cocks are probably always more visible than hens in winter, but the visibility of hens, with some snow cover, may increase faster than visibility of cocks.

Referring back to Figure 9.5, we can say that $y = pN$; birds seen (y) = visibility (p) times the true population (N). Using the regression formula from Figure 9.5, we can state the same thing by the formula:

$$y = (a + bx)N$$

TABLE 9.3

OFFICERS' PHEASANT COUNTS ON DAYS WITH A TRACE OR NO SNOW

	No. of Days	Officer Days	Pheasants Observed			Birds per Officer Day	Sex Ratio Hens/ Cock	Per Cent Increase Over 1948-1949			
			Cocks	Hens	Total			Cocks	Hens	Total	Sex Ratio
1948 - 1949											
No Snow	33	1,414	468	863	1,331	.94	1.8				
Trace	56	2,389	642	1,519	2,161	.90	2.4				
1949 - 1950											
No Snow	31	1,191	508	1,284	1,792	1.50	2.5	27%	77%	60%	39%
Trace	30	1,207	596	1,633	2,229	1.84	2.7	88%	111%	104%	13%

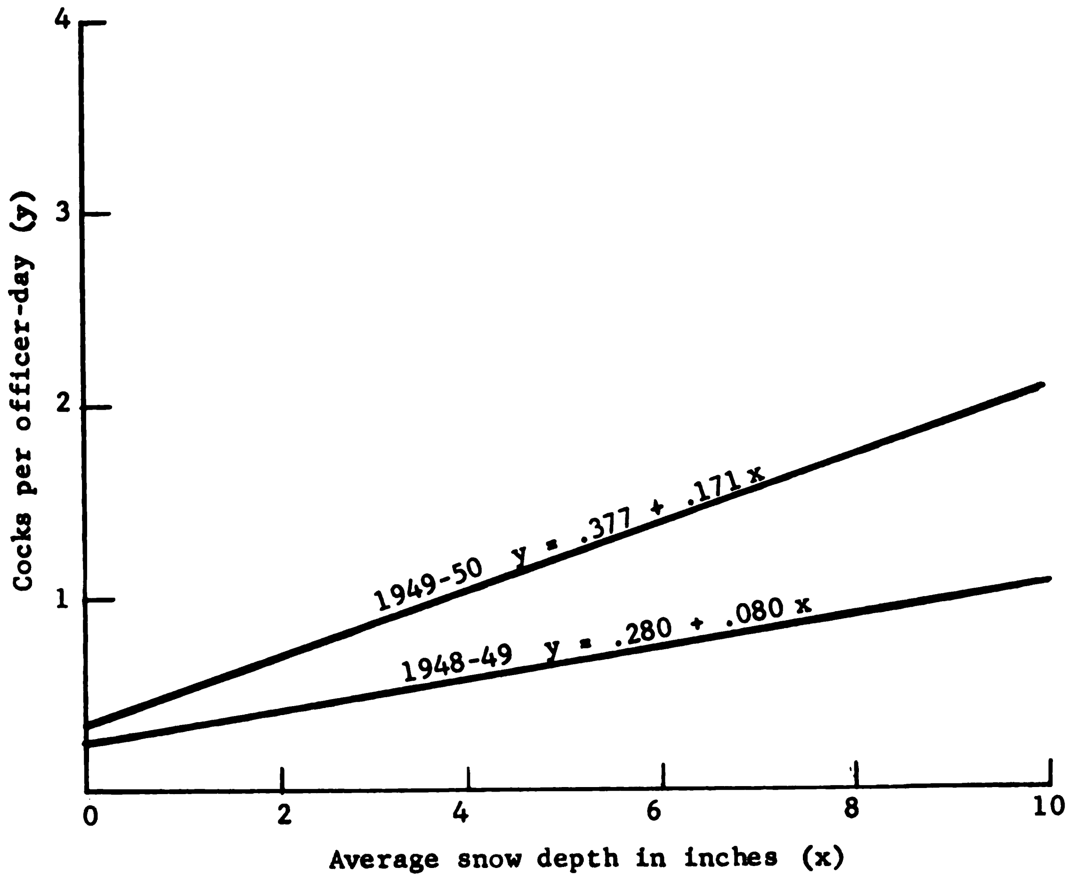

Fig. 9.3--Regression of cocks observed per officer-day on daily average snow depth.

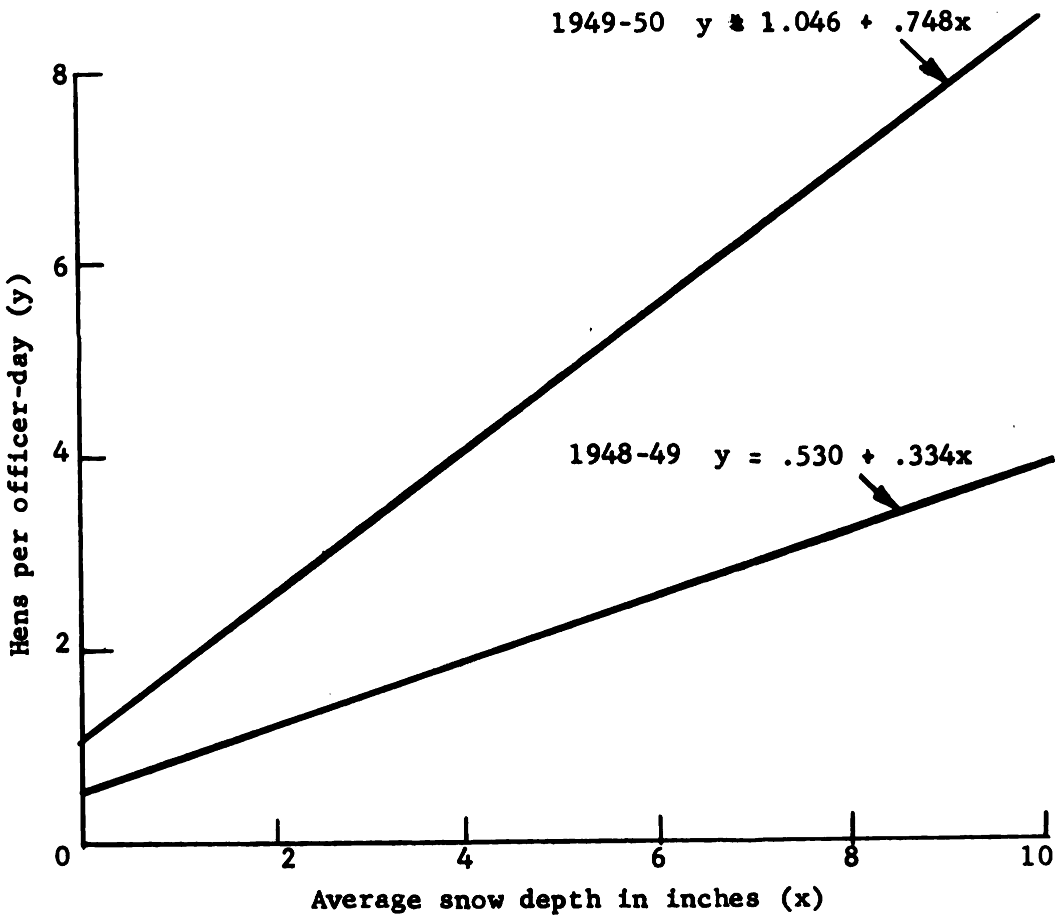

Fig. 9.4--Regression of hens observed per officer-day on daily average snow depth.

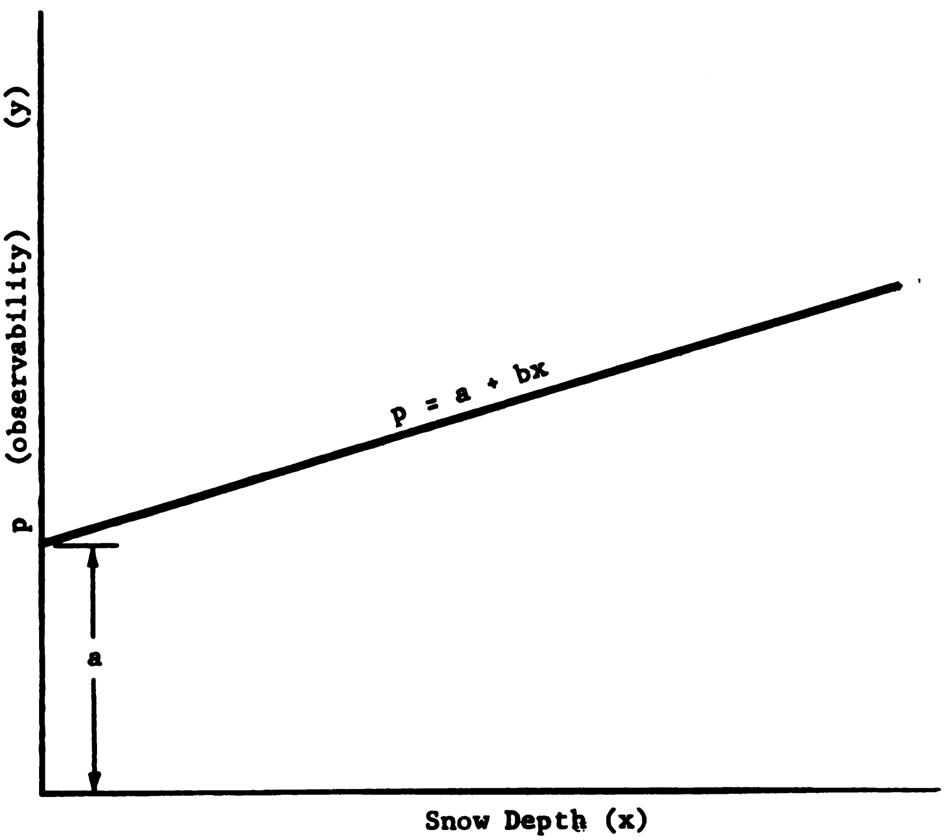

Fig. 9.5--Model suggesting a relationship of observability of a pheasant population to snow depth.

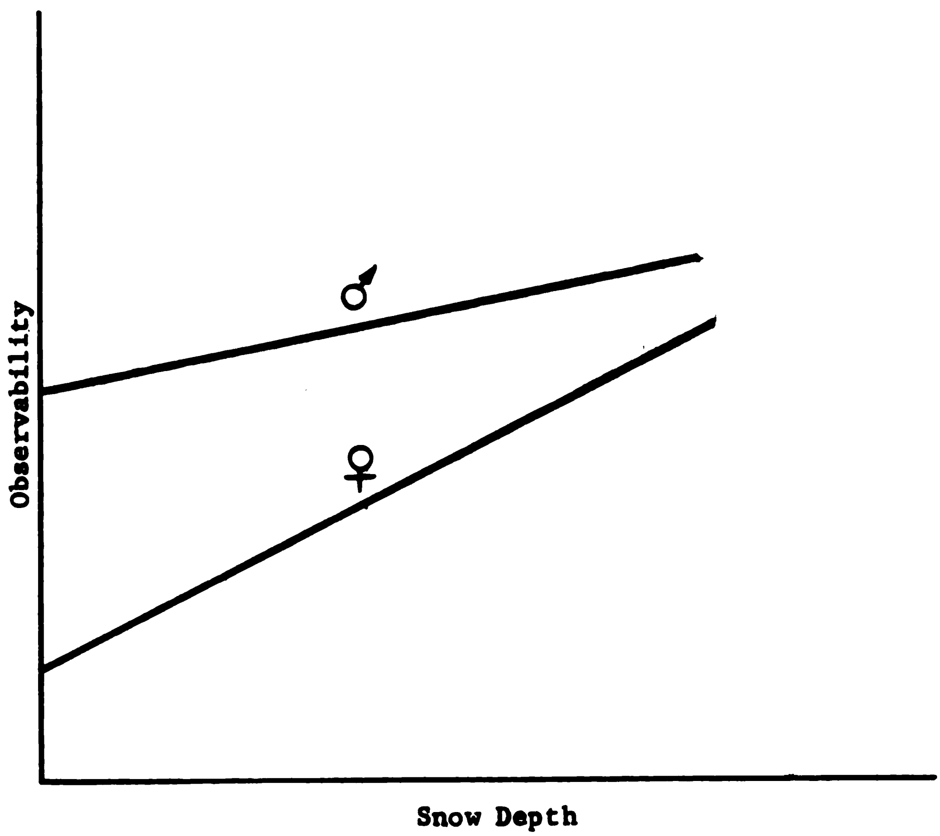

Fig. 9.6--Model suggesting shift in relative observability of cocks and hens as snow depth increases.

The a and b coefficients of this formula have been calculated for cocks and for hens for each of two winters. Comparing these coefficients from year to year yields these ratios:

	y-intercept (a)	Slope (b)
Cocks: (a_2, b_2)	$\frac{a_2N_{49-50}}{a_2N_{48-49}} = \frac{.378}{.280} = 1.35$	$\frac{b_2N_{49-50}}{b_2N_{48-49}} = \frac{.171}{.080} = 2.14$
Hens: (a_1, b_1)	$\frac{a_1N_{49-50}}{a_1N_{48-49}} = \frac{1.046}{.530} = 1.97$	$\frac{b_1N_{49-50}}{b_1N_{48-49}} = \frac{.748}{.334} = 2.24$

With the exception of the y-intercept for cocks, all ratios are fairly close together. One might thus speculate that the 1949-50 population was about twice as large as that of 1948-49.

Using the same ratios between "a" values and between "b" values for the two years for sex ratios, gives values as follows:

	y-intercept (a)	Slope (b)
1949-50	$\frac{\text{hens}}{\text{cocks}} \; \frac{1.046}{.378} = 2.77$	$\frac{.748}{.171} = 4.37$
1948-49	$\frac{\text{hens}}{\text{cocks}} \; \frac{.530}{.280} = 1.89$	$\frac{.334}{.080} = 4.18$

This is but one more demonstration that observed sex ratios also increase as snow depth increases. The increase (slope) in this instance, however, was not consistent with the y-intercept.

In Figure 9.7 the curves for this increase in sex ratio for the two years are plotted.

There is no easy way of determining sampling error in these studies. Despite a bulk of data we have no way of referring it to true density or sex ratios. Very large sampling errors are

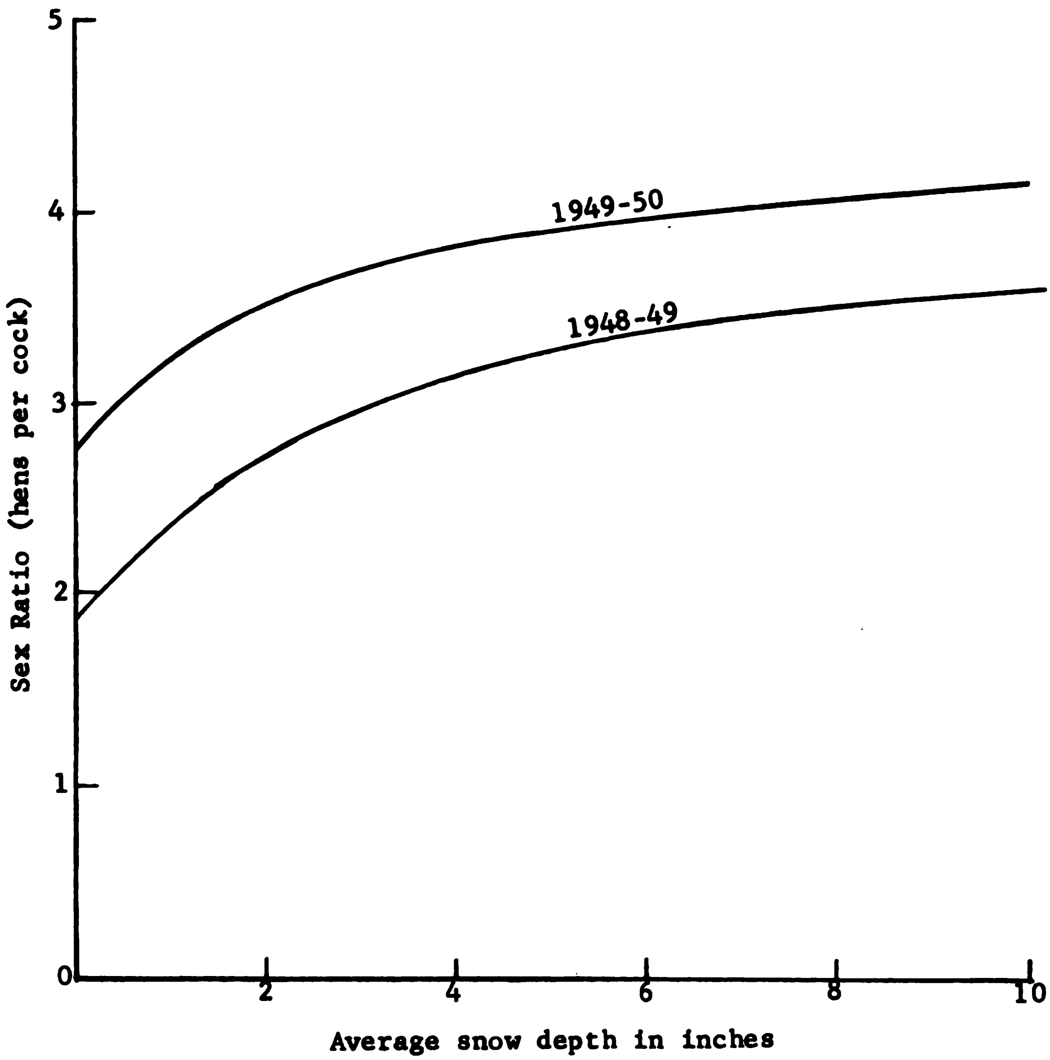

Fig. 9.7--Increase in sex ratio as snow depth increases.

probably present. One of the 55 officers reporting may, in one day, report more than half the total observations. The duties of one officer may carry him past a large concentration of birds one day, not the next. Quite possibly, however, we could devise ways to improve the sampling.

In addition, I am basing this speculation on two years' data. It may happen that these two years are not representative of other years. For example, data on the two winters presented here almost certainly indicate more birds and more hens per cock in 1949-50 than in 1948-49. But elsewhere in this paper we have shown evidence that sex ratios may not ordinarily change appreciably from year to year (Chapters 5 and 7). In Table 7.3, however, we can note that the carriers' prediction of fall kill in 1949 showed the greatest error recorded--15 per cent low! This could be explained by saying that for some as yet undisclosed reason, a much greater than normal percentage of the cock population was harvested in the 1949 hunting season. This, in turn, would have to mean a greater number than normal of hens per cock in the winter of 1949-50!

9.4 Conclusions

Observability of cocks and hens differs due to physical appearance as well as differences in habits. Observability changes, at different rates for the two sexes, as snow depth increases, so observed sex ratios change. Hence, any indices to winter populations are likely to be only relative, and far removed from direct indices of true populations.

On the other hand, there is a thread of consistency running through the pattern of observations which may have a potential for determining changes in density and sex ratio which will be of value.

I recommend further investigation of the relationships of observability of pheasants to snow cover after the collection of more data over a greater span of years, along with attempts to improve the collection of the samples.

Chapter 10

POPULATION TRENDS--ANNUAL AND LONG TERM

10.1 Introduction

Most of our seasonal measures of populations are expressed as indices whose relation to true populations is unknown. These indices, then, must be used only for comparison with similar indices obtained in other areas or other years. For example, sex ratios obtained from hunter observations on the one hand and carriers' spring counts on the other hand might be compared with their counterparts in other areas or other years, but could not necessarily be compared with each other to determine shifts in sex ratio from one season to another.

Figure 10.1 is a schematic diagram of the type of population data we would ideally like to have. If we had accurate information on all the items listed in the diagram, we would be a long step toward knowing quite precisely what is happening to pheasants throughout the year. In the foregoing chapters we have discussed the collection of data for a large share of these items. In the following sections I will summarize what comparisons can be made between these seasonal indices, and conclude with a review of long-term trends of pheasant populations on the study areas.

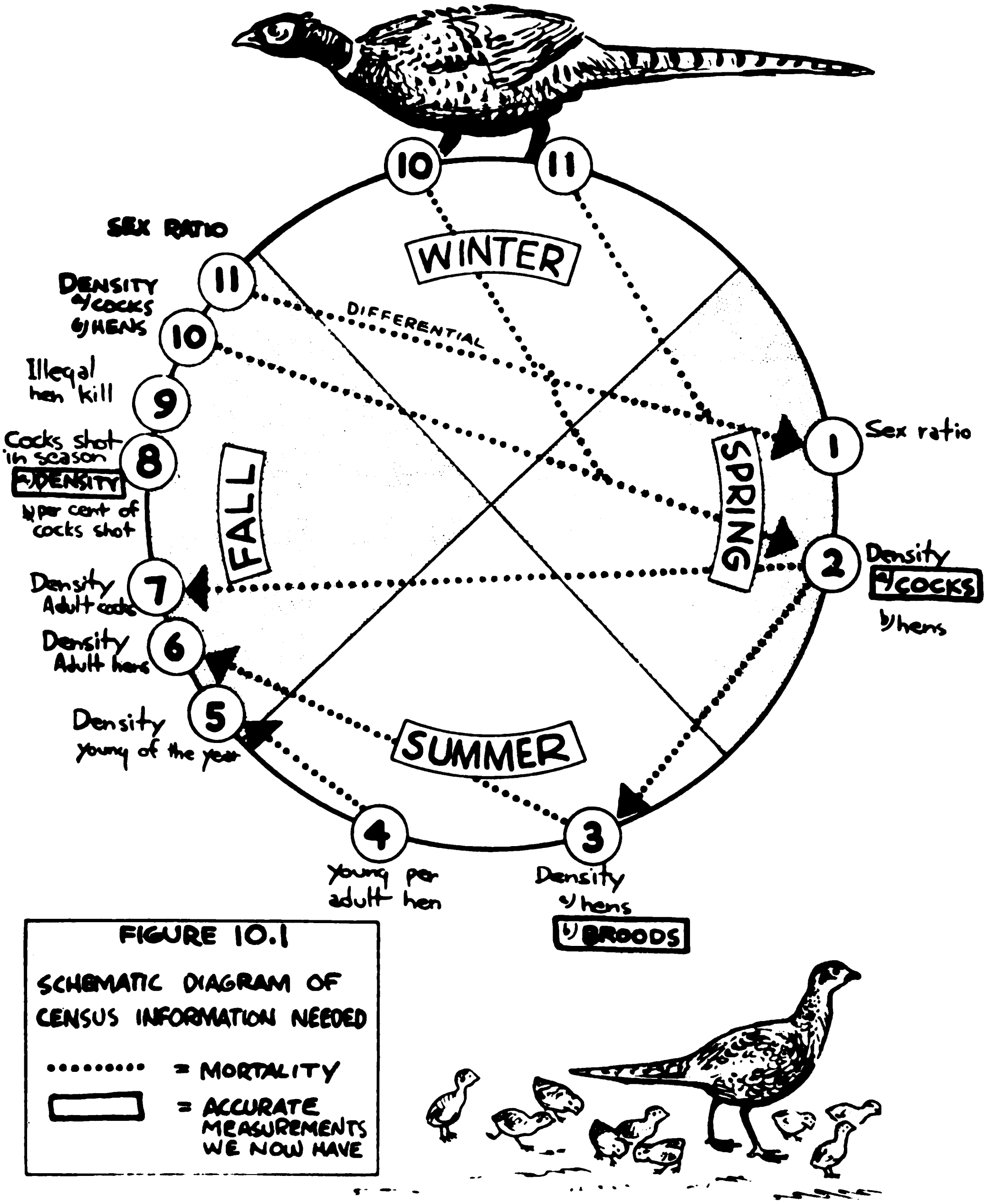

FIGURE 10.1

SCHEMATIC DIAGRAM OF CENSUS INFORMATION NEEDED

10.2 Reliable Indices to True Populations

In Chapter 7, I demonstrated a close correlation between brood density indices in July and computed hunting season kill in October-November. The close correlation over a long span of years (Table 7.3) permits us to state that each of these two indices is a reasonably accurate reflection of true populations at the time they are taken, despite a long list of assumed and perhaps self-compensating factors which are not supported (or negated) by other independent data (Section 7.3).

There is one other good index to true populations. The crowing-cock count can be considered a reasonably accurate measure of the cock population.

Thus, we have three seasonal references to true populations to which we can relate our other indices. In only one--the computed kill--do we have data in terms of birds per unit area.

10.3 Sex Ratios

Since sex ratios are commonly widely disparate in a pheasant population where a sizable segment of the cock component is abruptly removed each fall, indices to total populations must be modified by the sex ratio, when one wishes to deal with such things as productivity. I have demonstrated in other sections that observability of pheasants differs by sex. Petrides (1949) discusses this involvement, and more recently Dale (1952) has published a comprehensive review of the importance of sex ratios. Allen (1942) reported upon an inventory method which utilized sex ratios in determining such items as percentage of cocks removed by hunting. These and other authors have done much to take the confusion of sex ratio differences out of calculations. There have been few contributions, however, toward taking the confusion out of determining true sex ratios from observed. The complications one gets into by ignoring even relatively small changes in sex ratios in calculations point out the necessity, in turn, of using true sex ratios rather than ratios which may be biased by the observations that obtained them.

Workers on intensive study areas such as Rose Lake (Section 2.7) and the Prairie Farm (Shick, 1952) have obtained reasonably valid sex ratios by actually censusing pheasants on relatively small areas. But I feel that despite the care and precision with which we may obtain indices to sex ratios from extensive surveys, we are still far short of tying these indices to true sex ratios.

In Chapter 9, I suggested a system which might allow removal of one bias--snow cover--from sex ratio observations. In Chapter 5, I suggested that by careful attention to phenology one might obtain a "better" sex ratio. But in both these instances, the information is improved only to the point that the comparison of indices is improved. We are closer to, but still far from knowing true sex ratios. It is the nature of pheasants to show behavior patterns that differ by sex for almost all seasons of the year. This trait distinguishes pheasants from less polygamously inclined birds such as ruffed grouse and quail.

I would caution then that, as Dale (1952) admonishes, we carefully determine sex ratios, that we apply standard tests to determine such things as sampling error probabilities, and that we not neglect sex ratios when they should be considered in our calculations. But I would further caution that we should beware of a false sense of security by this regard for sex ratios. No amount of care and precision can make up for the fundamental bias resulting from the relative observability of hens and cocks, which I feel is inherant in virtually all our extensive surveys.

We must make one important speculation on the broad subject of pheasant sex ratios. The extremely good correlation between the carriers' summer brood counts and the hunting season computed kill implies a more or less consistent percentage removal of the cock pheasant component of each fall's population! This, in turn, suggests that postseason sex ratios may not vary greatly from year to year!

There is one very obvious exception to this statement. Although the discussion in Chapter 9 of the officers' daily counts during the two winters of 1948-49 and 1949-50 still does not permit us to determine accurately the difference in sex ratios between the two winters, it is almost indisputable that there were considerably more hens per cock in 1949-50 than there were in 1948-49. In other words, almost certainly sex ratio did change appreciably. But the hunting season kill in 1949 was 15 per cent greater than predicted from carriers' surveys--the largest error by far in the 11-year correlation! If the percentage of cocks killed in 1949 was unusually high, a more disparate sex ratio in the winter of 1949-50 would inevitably follow. This exception in 1949 may be one which helps prove the rule.

This hypothesis that sex ratios ordinarily should not change greatly from year to year may or may not be supported by spring sex ratio surveys. The carriers reported rather uniform harem sizes for the three springs 1948, 1949, 1950 (2.3, 2.1, 2.5, respectively, Table 5.4).

This is speculation. There may be compensating factors that we cannot appraise. But we cannot overlook the fact that this system of predicting fall kill with an 11-year average error of prediction of only 4 per cent demands a fairly constant percentage removal of cocks--or some unknown compensating factors.

10.4 Meshing Seasonal Population Indices

We have excellent information on pheasant density at least three times a year--spring, summer, and fall. We have some information on sex ratios. How can we tie these various pieces of information together? Since we are dealing with different indices, a system of conversion factors will probably have to be devised. We would be premature to try to devise such a system now, but in time I think it can be done. Following is a synopsis of the possibilities for bringing seasonal population data together:

Summer to fall. In Chapter 7, I have discussed the relationship between summer and fall population density measurements. This is a precise alignment of summer to fall density. Sex ratio of chicks in July can be assumed to be very near 1:1. We have no measure of adult sex ratios in mid-summer, nor in the fall just before the hunting season. But since ordinarily young of the year, evenly divided as to sex, are the preponderant part of the fall population, we can assume the preseason sex ratio of all pheasants (adult and young together) is fairly close to 1:1--precisely how close, we cannot say.

Fall to winter. We have a good measure of the fall kill. If we knew the postseason sex ratio, these two sources of data would provide us with the means for estimating remaining cocks and the hen population. A valid age ratio of cocks shot would allow us to go one step further to determine rearing success. Our determinations of sex and age ratios are imperfect, but there is promise they can be improved. In the meantime age ratios, carefully obtained, may be compared for relative (not true) rearing success, if or when it is true that sex ratios do not change appreciably. We may find, with practice, that sex ratios (relative, but probably not true) may be obtainable in winter by the use of a factor to minimize or eliminate the effect of snow cover on observability of the birds.

Winter to spring. Changes in sex ratio from early winter to spring would give us differential winter mortality. Changes in density for the same period would give us total mortality. I do not feel that either the density or sex ratio measurements we now have are directly comparable. But if the total fall cock population can be determined, an index to cock mortality from fall to spring will be available, since the crowing-cock count is a good index to true populations, and may soon be convertable to cocks per unit area. I doubt, however, if we will be able to convert observed sex ratios in winter or spring to true sex ratios.

Spring to summer. If sex ratios could be assumed to be constant from spring to spring, then the crowing-cock counts compared to summer brood counts would give us a usable measure of relative productivity (not true productivity).

10.5 Population Trends by Study Areas

Figure 10.2 shows the trend of populations by study area for the 20-year period 1937 to 1956, inclusive. The computed kill per square mile (Chapter 2) in each of the study areas is used as the unit for comparison. We have already concluded that computed kill can be considered an index to the fall population, on the strength of its excellent correlation with summer brood production.

Since study areas are based on political (county) rather than biological boundaries, the relation of the areas to one another is only a general one.

The graph in Figure 10.2 is on a semi-logarithm scale. We are dealing with areas with widely differing population densities. Comparison of the logarithm of the kill per square mile permits us to compare population trends in the areas by comparing slopes of the curves, regardless of the population level.

The chief value of this comparison of the trends of pheasant populations in the study areas will be comparison of these trends, in turn, with the many factors suspected of affecting pheasant populations as those factors have changed over the years in the study areas. Study of these factors is outside the scope of this paper. But this analysis should provide the basis for such study.

I have casually compared the trends of Michigan's pheasant

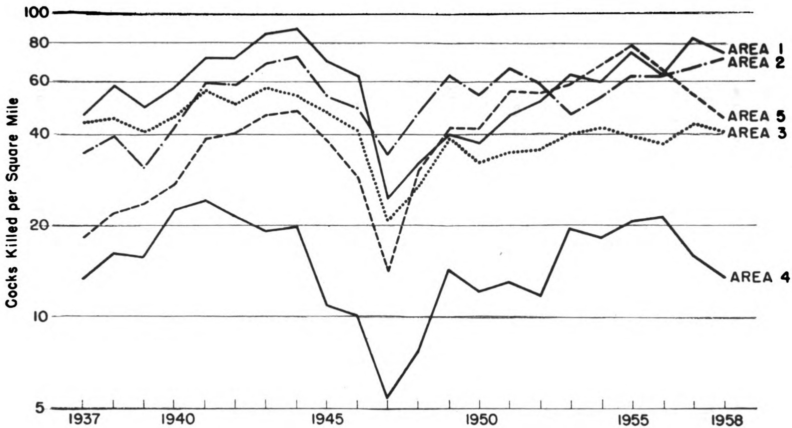

Fig. 10.2--Computed kill in study areas, 1937-1958

populations with trends in other states. They have much in common. Figure 10.2 represents a build-up to a peak in the middle 40's and a serious depression in 1947. In general, pheasants followed this pattern across the continent. The exact date of the peak varies, generally between 1942 and 1945. The depression was generally considered to be at its lowest in 1947, although in some states the low point was considered to be in 1946 or even 1948.

More specific comparison is difficult. Kill figures are regarded with more skepticism in most other states and provinces than in Michigan. I would suggest that pheasant kill figures in other states might be re-examined in the light of our evaluation given in Chapter 2, and that other states' population data be compared with Michigan's. (For example, our Study Area 2 is contiguous with Ohio's good pheasant range, Study Area 4 is contiguous with Indiana's pheasant range, etc.)

The rank of the study areas as producers of pheasants has changed over the years:

<u>Area 4</u> (southwest; type locale--Kalamazoo County) has consistently been the poorest. It represents the poorest part of primary pheasant range.

<u>Area 3</u> (central; type locale--Ingham County) can be considered moderately good, but not quite top-notch pheasant range. It can be considered the most stable of the better areas.

<u>Area 5</u> (west central; type locale--Ottawa County) was in fourth place before the depression. It competed for first place by the middle 50's.

<u>Area 2</u> (southeast; type locale--Lenawee County) was usually

second best before the depression. From 1947 through 1952, in and following the depression, it was the best area. Since 1952 it has dropped back to second or third place.

<u>Area 1</u> (the "Thumb"; type locale--Tuscola County) is famous as Michigan's best pheasant range. With the exception of 1947-1952, it has been at or very near the top.

It is noteworthy that the peak varied among study areas within the state of Michigan. Area 4 quite certainly (and possibly Area 3) reached a peak in 1941. Areas 1, 2, and 5 quite definitely reached a peak in 1944. Thus, the poorer areas peaked in 1941, the better three years later in 1944 (considering Area 5 as one of the better areas).

Referring back to Figure 3.5, we can see that Areas 1 and 2 differ geographically only in latitude. The highest pheasant populations of each are on lake-bed clay soils. Their population curves follow each other most nearly of any of the areas. They reached a peak together. Now compare their curves to Area 5's. If Area 5's curve prior to the 1947 depression were about doubled, it would very closely resemble the curves for Areas 1 and 2. But Area 5 is more distant, geographically, from Areas 1 and 2 than any other area.

The only fundamental characteristic I can determine that Areas 1, 2, and 5 share in common to the exclusion of Areas 3 and 4, is a large amount of lake-bed plains. Thus, Areas 1, 2, and 5 are distinguished as Michigan's best pheasant range--a distinction they share with similar areas of lake-bed plains which support the highest populations in neighboring states--Ohio (Leedy and

Hendershot, 1947), New York (Brown and Robeson, 1959) and Ontario, especially as typified by Pelee Island (Stokes, 1954).

SUMMARY

This study of Michigan's pheasants from the standpoint of population dynamics, had three objectives--(1) to reconstruct a history of past populations, (2) to acquire information on current population levels, and (3) to devise necessary new sampling methods for obtaining that information. The study was made during the years 1947-1950 when data were obtained from a large number of extensive surveys, and from certain of these surveys repeated annually from 1951 through 1956. Other data in the Game Division files covering the period 1895-1946 were analyzed.

Pheasants were first introduced into Michigan by a release of several birds in Ottawa County in 1895. At least one brood was produced. Despite a number of other releases by private citizens in the ensuing years, pheasants were not established anywhere in Michigan prior to 1918 when the State began a release program. The State released grown birds in the fall and gave eggs (and in some cases day-old chicks) to private citizens who requested them. From 5,000 to 10,000 birds were released each year. By 1921 pheasants were established in much of southern Michigan. In 1923, five years after the beginning of the release program, authorities recommended a pheasant season; in 1925 the Legislature opened the first season on pheasants. By the late 1920's, pheasants had probably spread to the limits of their range, although the relative distribution was considerably different than it is today.

Records of hunter-performance during the fall hunting seasons were the best source of information on pheasant numbers prior to this study. From 1929 through 1935, a few hundred hunters reported on their pheasant hunting each year on "Bird Hunters' Tally Cards" furnished by the Game Division. From 1933 through 1936, license vendors quizzed small game license purchasers about their previous year's pheasant hunting and recorded the data on a special stub of the license. Computations from these tally cards and license stubs provided only fragmentary and dubious information on pheasant kill.

Beginning in 1937, hunters were compelled by law to return to the Game Division "Small Game Hunter Report Cards" issued to them with their license. The pheasant kill was computed by county each year from these report cards. I concluded this kill computation to be relatively accurate. I based this judgment on a comparison of the computed kill to hunting data concerning two large groups of Detroit sportsmen, measured kill on two study areas and examination of three possibilities for bias or error in these computations: (1) inadequate sample size, (2) dates of the returns (early vs. late), and (3) differential success of those hunters who reported compared to those who were delinquent. None of these biases appeared to be excessive.

Michigan's primary pheasant range was designated as the 38 counties south of a line running roughly from Saginaw Bay to Muskegon. The balance of the Lower Peninsula and the south half of Menominee County in the Upper Peninsula were designated as marginal pheasant range. The remainder of the Upper Peninsula has only an occasional pheasant.

Pheasant distribution appears well correlated to soil and land types. Five study areas, totaling about three-quarters of the primary pheasant range, were picked on the basis of soil and pheasant distribution.

The extensive survey is defined as a collection of observations by sampling, yielding indices to populations. Useful extensive roadside surveys were made principally by rural mail carriers and conservation officers. The carriers provided a large volume of data on each survey, but could be asked to make only short counts a few times a year. With only one or two officers per county, their extensive surveys did not provide as much data but they could be asked to make surveys over protracted periods. Surveys of and by sportsmen did not provide usable data. Farmer cooperators provided some data on nesting. Biologists made crowing-cock counts and interviewed hunters.

Measurements of pheasant populations for each of the four seasons are discussed.

Spring populations. I set up crowing-cock routes in most of the 38 counties of primary pheasant range. Crowing-cock counts offered the best index to spring cock populations, since they were self-adjusting for phenology. Habits of pheasants differ radically between sexes as the days progress in spring, making roadside surveys extremely sensitive to errors in timing. Seven years of carriers' April roadside surveys, adjusted to a common starting date, showed good correlation to crowing-cock counts, with the exception of two years when the springs were early and the carriers' counts were correspondingly inflated.

Observations on pheasant harems may provide a better index to sex ratios than observations of all birds. I was unable to convert spring sex ratio indices into true sex ratios.

Summer populations. Officers made roadside counts of broods during June, July and August. Carriers made similar counts in late July. For four years the number of broods seen by officers in each consecutive semi-monthly period increased from June to mid-August at a similar rate. The regression of broods seen by officers on period of the summer was plotted, offering an opportunity to adjust brood counts for phenological differences. Rainfall did not appear to have a direct bearing on observability of broods by carriers. Brood sizes reported by carriers did not change significantly from year to year--half-grown broods varied only from 6.10 to 6.53 chicks during a five-year period when the population went down and began to recover. Frequency distribution of brood sizes was also relatively consistent during these years. The percentage of hens without broods seen by carriers was greatest during years of poor pheasant brood production, but this index appears inadequate by itself as a true measure of productivity.

Carriers' July brood counts provided an accurate method for predicting fall kill. The regression of the computed cock kill on the logarithm of the brood density observed by carriers in primary pheasant range yielded an "r" value of .978 for the eleven years 1946-1956. Using this regression as a method for predicting fall kill, the error of prediction for the eleven years averaged 4 per cent--with a maximum of 15 per cent error. This good correlation is somewhat unexpected, since it assumes a consistency from year to year of seven contributing factors--some of which heretofor often have been considered variable. These seven factors are discussed: (1) late summer mortality, (2) percentage of the fall population harvested, (3) age ratios of cock kill,

(4) brood size, (5) accuracy and precision of kill computations and brood surveys, and (6) timing of brood surveys. Some variation in these factors from year to year undoubtedly occurs, but it may be in part compensatory.

Fall populations. The computed kill is judged to be the best information on fall populations. Preseason roadside surveys by officers did not yield plausible sex ratios or usable density indices. I do not consider that sex ratio observations by hunters on opening day of pheasant season are reliable. Because of a difference in vulnerability between young and adult cocks, age ratios of cocks killed shifted as the hunting season progressed. Hunter success must be used with discretion as an index to populations. Field interviews with hunters yielded little usable biological data, and polls of hunters' opinions on abundance of pheasants were not useful.

Winter populations. The number of pheasants observed by carriers and officers on winter roadside surveys correlates well with depth of snow on the ground. I plotted the regression of the daily observations by officers of cocks, hens, total pheasants, and sex ratio on the daily average snow depth in the five study areas, for two entire winters. Observability changes at different rates for the two sexes as snow depth changes. Hence, winter observations yield only relative density and sex ratio indices, with an unknown relation to true populations. Differences in observability of the two sexes are probably due to differences in visibility of the colored cocks and drab hens, as well as differences in habits of the two sexes. Sample size of the observations is probably less than adequate to determine accurately the relationship between the sexes, but the indisputable correlation between snow depth and total birds observed suggests that this matter is worth further study. Possibly observations of pheasants on days with no snow may be most nearly indicative of true populations.

Population trends--both annual and long-term--are discussed. Three annual surveys provide an index to true populations--the crowing-cock index, the carriers' summer brood survey, and the computed kill. The difficulties of determining true sex ratios from observations are discussed. I conclude we have no good measure of true sex ratios at any season of the year. The possibilities for juxtaposing indices to populations for adjacent seasons are discussed. The only indices to populations for adjacent seasons that can be directly linked are summer brood density index and computed kill.

The trend of populations for the five study areas, as determined by computed kill, is graphed for the 22-year period 1937-1958. All areas were at their lowest point in 1947, but the three best pheasant areas peaked in 1944, the two poorer areas in 1941. The three best areas exhibit similar population trends as distinguished from the two poorer areas, although the former are widely separated geographically. The only obvious factor common to the three best areas and lacking in the poorer areas is large acreages of soil of lake-bed origin.

A P P E N D I X

METHOD FOR COMPUTING KILL FROM SMALL GAME HUNTER REPORT CARDS

The method for computing pheasant kill from small game hunters' report cards can be illustrated by the computations for a typical year. A portion of the work sheet used to compute the 1948 pheasant kill is shown in Appendix Table 1. The capital letters in parenthesis below refer to their counterparts in the table.

1) Data from the 52,026 report cards received (K) were punched on IBM punch cards, sorted, and recorded by county (A) on the work sheet.
2) The 31,522 hunters (J) who reported that they hunted pheasants and the number of pheasants they reported shooting were distributed by counties as shown in columns (B) and (C). Only one county of hunt was recorded for each hunter, although he may have hunted in more than one county.
3) The 2,417 hunters listed as "Incompletes" (I) shot 5,560 pheasants but neglected to state in what county they hunted. These hunters and the pheasants they shot were spread among all the counties in proportion to the hunters and kill recorded for each county by using the formulae (L) and (M). Thus, the number of hunters (B) in each county was multiplied by 1.083044150 and the product entered in column (D). The number of pheasants reported shot in each county was multiplied by 1.109023883 (M) and the product entered in

column E.

4) At the time the computations were made, 1948 small game license sales were estimated to be 582,000. The 52,026 report cards received were $\frac{1}{11.18671433}$ of that total. The number of hunters, and the number of pheasants they reported shooting in columns (D) and (E), respectively, were thus multiplied by the denominator of this fraction (N) which is called the "Constant for Computing." The estimates for each county were entered in columns (F) and (G), respectively. Column (G) then, shows the "Computed Kill" for each county.

5) Column (H) lists the average number of pheasants reported shot per hunter reporting. This is not a true hunter success figure, since some hunters who hunted pheasants but shot none may have neglected to indicate that they hunted pheasants.

APPENDIX TABLE 1

REPRODUCTION OF A PORTION OF 1948 COMPUTED PHEASANT KILL WORK SHEET

(A) County	(B) Hunters	(C) Pheasants	(D) Hunter Spread	(E) Pheasant Spread	(F) Computed Hunters	(G) Computed Pheasants	(H) Average Ph/Hunter
Allegan	660	1,207	716	1,339	8,010	14,979	1.87
Barry	302	392	327	435	3,658	4,866	1.33
Bay	636	1,433	689	1,589	7,708	17,776	2.31
. . .	. . .	. . .	. . .	. . .	. . .	. . .	. . .
Wayne	1,901	3,425	2,060	3,798	23,045	42,487	1.84
Lower Peninsula S. of T.L. 16	28,714	50,527	31,098	56,035	347,883	626,848	1.80
Lower Peninsula N. of T.L. 16	391	471	424	523	4,744	5,850	1.23
Total Lower Peninsula	29,105	50,998	31,522	56,558	352,627	632,698	1.79
Total Upper Peninsula	-	-	-	-	-	-	-
Total State	29,105	50,998	31,522	56,558	352,627	632,698	1.79
(I) Incompletes	2,417	5,560					
Did not hunt pheasants					229,373		
Total license sales					582,000		
(J) Total returns reporting pheasant hunting	31,522						
(K) Total returns	52,026						

(L) Hunter Spread: $\frac{2,417}{29,105} + 1 = 1.083044150$

(M) Pheasant Spread: $\frac{5,560}{50,998} + 1 = 1.109023883$

(N) Constant for computing = $\frac{582,000}{52,026} = 11.18671433$

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