VALUE OF FIELD OBSERVATION IN ECONOMIC ORNITHOLOGY

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In the Wilson Bulletin for June, 1934 (Vol. 46, No. 2, pp. 73-90) is an article by E. R. Kalmbach entitled "Field Observation in Economic Ornithology."

Mr. Kalmbach is careful to say that he does not wish "to discredit in the least stomach examination as a fundamental procedure in the solution of problems in economic ornithology" (p. 74), but considering the superficiality of most reading and the proneness of humans to see in a thing what they wish to see, it is, after all, not surprising that some have jumped to the conclusion that the method of stomach analysis has been authoritatively pronounced obsolete.

It may be well, therefore, to cite from Kalmbach's paper phrases additional to that just quoted, that show he had no such intention. He says, "there must be no slackening in laboratory research" (p. 80). "Let it not be inferred, however, that stomach examination . . . does not play a most important, yes, indispensable rôle in our science . . . certain of the practical problems of economic ornithology lend themselves to direct solution solely or largely through this method of approach" (p. 80). "In this capacity, the laboratory channel of approach never will be excelled. Stomach analyses of extensive and representative material is the only means, furthermore, of creating a background for proper appraisal of the general economic status of birds, something that is needed as a check upon every local or specific [i. e. field] study" (p. 81). "It is a truism . . . that stomach examination, carefully conducted, gives the best possible index to the food items of a bird" (p. 88).

The writer hopes that this grouping of Kalmbach's references to the method of stomach analysis may be the proper corrective for those who thought they saw, in his essay, something to quite the contrary effect. Kalmbach certainly did not mean to condemn the method of stomach analysis for it is the principal reliance in researches he is now conducting and directing in one of which, for example, on the food habits of the armadillo, little progress could be made in any other way.

To bring out both sides of the argument and to combat the impression the paper has made that stomach analysis is obsolete, the following comment is offered: The method of stomach examination "has acquired, in both the scientific and lay mind, a status of finality shared by no other method of approach" (p. 74). Apparently it needs saying that there is no such thing as finality in any field of

knowledge. Least of all should the scientist admit that term to his vocabulary. In science all questions are forever open.

"Limitations in this well-established procedure" [stomach analysis] (p. 74). Every method of studying the food-habits of wild life has its limitations—they are not peculiar to laboratory work on the contents of the alimentary tract. We should avail ourselves of all methods, thus taking advantage of any special excellence each may have and profiting by any mutually corrective principles that may appear.

"Knowledge of field conditions and the circumstances surrounding the collecting of stomach material is the key to proper interpretation of food items" (p. 75). If that knowledge could be full and accurate this statement might be acceptable, but as a matter of actual experience we do not often get either full or accurate knowledge of the doings of any wild thing in the field. It is a commonplace of laboratory procedure to find that the collector's remarks on what a bird was apparently eating are not verified by analysis of the stomach contents. Not only is it difficult to make observations in the field that will be very helpful in connection with the study of individual stomach contents, but if we attempted to adopt a policy of having "the examiner of bird stomachs personally . . . collect every specimen which he later examines" (p. 75), meanwhile getting accurate "knowledge of field conditions and the circumstances surrounding the collecting" (p. 75), comprehensive investigations would be impossible. The proposal would be merely one more of those ideal conceptions that can not be accomplished in reality. What we have needed and still need in the case of certain groups of birds never reported upon is comprehensive information regardless of shortcomings as to detail that will give us a general idea as to food-habits over a wide range, perhaps the whole United States, at all seasons. We can get some sort of approximation to that knowledge by the analysis of a large number of stomach contents, well distributed both seasonally and geographically, but we can not get it by intensive field study simply because of the impracticability of allotting for the purpose, the time, the men, and the funds that would be necessary.

The field method of studying economic ornithology can not be our main reliance, not only by reason of impracticability, but also because it cannot be as comprehensive as the laboratory method, nor does it have as high a degree of accuracy. In this as in all sciences what we learn in the laboratory is the most exact and reliable part

of our knowledge. What we learn in the field is more tentative and usually in need of some process of checking or verification.

Even in studies where every effort is made to obtain in the field an accurate idea of what a bird is feeding upon, the results are far from satisfactory when checked with those derived from the analysis of stomach contents. For example, to facilitate laboratory study of a large series of stomachs of Gambel's Quail from Arizona, D. M. Gorsuch supplied notes on what he had come to regard as important foods of the species as a result of field observations. When work on the stomachs (178 in number) was completed, it was found that of foods considered important by the field observer and so recorded in his notes or in his recent report on the bird (Univ. Ariz. Bull., Vol. V, No. 4, May, 1934) six had that rank in the laboratory findings and five did not. The use of five unimportant items also was verified. Of foods not noted at all by the field observer seven were found of considerable, and eighteen of lesser, importance in the laboratory. In other words the field observer's results were verified in eleven cases (five of them unimportant) and not in five, while those results did not include at all seven items of primary, and eighteen of secondary, importance in the diet of the quail. This gives an efficiency rating of 27 per cent for the field observations, and that without taking into account some scores of minor items revealed by stomach analysis but which could not be learned at all by field study.

"Correct interpretation easily may be a matter of greater significance than the identification of the item itself" (p. 76). It should not be overlooked, however, that interpretation can not begin until the identification is made. Interpretation may be aided by field observation but it can safely be said that accurate identification of food items can be obtained in the vast majority of cases only by laboratory procedure. Without identification there is not even a beginning of knowledge.

"Correct interpretation" again, so far as applied to the intricate subject of wild life economics, denotes an ideal often unrealized. This science is peculiarly one of approximations and it can not be expected that interpretations will be of better quality than the data on which they are based. Thirdly, let it be remembered always that "interpretation" is a necessary adjunct to wild life food habit studies of whatever type. In other words, if we are wise we will bring to bear on each problem all of the information we can obtain from every source—with due credit to all and without undue disparagement of any.

English Sparrows consuming insects attracted to city arc lights "may suddenly assume the prosaic rôle of a scavenger of doubtful

utility" (p. 79). There is no doubt as to the utility, only as to its degree.

"Modern decisions" (p. 79). If the term "decision" is used in the ordinary sense of settling or terminating discussion of a given matter, it is another of the too positive expressions used in this discourse. See remarks under "finality", (p. 74). There are no such things as final decisions in wild life economics. Problems are often not only local but they may be very temporary. A "decision" may be no more than uttered before the situation changes. As applied to food habit studies, these things emphasize the necessity of generalizations from a satisfactory number of stomach analyses since the local and shifting field conditions in themselves can not be so summarized. Field problems are local and temporary and corresponding valuation is about all we can give to observations, interpretations, or decisions about them.

"General tendencies for good or harm can be shown" (p. 88). This is about all that can be expected from economic studies whenever they cease to be strictly local. It is not the function of general food habit studies to attempt to show in concrete terms how much good or harm is being done. The science, as remarked before, can only be one of approximations, hence its output should not be represented as more than statements of economic tendencies.

"The effect of feeding habits . . . in the final analysis, is the actual goal in many modern problems of economic ornithology" (p. 88). This has a matter-of-course sound but in reality the goal mentioned is attained only locally and then in case damage to a crop or other measurable thing is involved. The effect of feeding habits on unmeasurable things as natural populations of weeds, insects, etc., can hardly be referred to as a goal because in most cases it is unattainable.

"Determination of the economic status of a bird" (p. 88). There is no such thing as "determination" if that meant establishing an economic status that will have final and universal validity. It is possible to arrive at good summaries of average food habits which are the best guide we can have as to the general treatment of species. This is the basis ordinarily needed for legislation and regulation. It has been furnished by stomach analyses and is something we could never attain as a result of field studies on account of the very size and complexity of the problem.

"Economic status and food habits are, by reason of their fundamental aspects and definitions, antithetic" (p. 88). If the proposition were true, as stated, we would hardly base our studies of economic status chiefly on food habits. In the same paragraph Kalmbach admits

that the study of food habits reveals sustenance. Since choice of sustenance and its effects are what mold our opinions as to economic status, it would seem difficult to defend the antithesis.

"Primarily the objective in our problems is one of economics; yet the product of much research into the economy of birds is purely biological" (p. 88). Sufficient rebuttal to this pronouncement is that the science of economic biology fuses the two, in actuality, as well as in name.

Continuing directly from the preceding quotation, "This product, the result of painstaking stomach examination, often is looked upon as the end sought or, if not actually the goal itself, so close an approach to it that the intervening gap is but a step in a simple process of deduction. Therein lies a fallacy that has served as the theme of much of this paper" (pp. 88-89).

Has that fallacy in fact existed?

In 1880 S. A. Forbes the founder of scientific economic ornithology tells, in a paper on The Food of Birds[1] why he adopted the volumetric method of tabulating the contents of bird stomachs and says, "The comparison, however, is merely a quantitative one. . . . It is evident, therefore, that we cannot get at any close estimate of the economic values of this species in this indiscriminate way. . . . (and referring to insects in particular) the opinions of entomologists would probably be found to differ somewhat widely on the question of the relative values of these various elements, and each must form his own opinion from the data given" (pp. 102, 103). These remarks clearly show that Forbes recognized the necessity of interpretation of the results of stomach analysis—a recognition he put in deeds also by studies of the food of ground and lady-bird beetles so that their significance in the food of birds could be more accurately appraised. Forbes pursued his researches in the field as well as in the laboratory.

In 1897 F. E. L. Beal in commenting[2] on Gilmour's statement that even the consumption of waste grain by rooks must be counted against the birds, as it shows their taste for grain, says, "This is not fair. Grain so obtained has no value to the farmer and should not be reckoned as a loss."

In 1904 Professor Beal noted in a discussion[3] of the California linnet or house finch that "fruit forms only 9 per cent of the annual food; consequently if the birds . . . were not so superabundant, the

[1]Bull. Ill. State Lab. Nat. Hist. (I) 1 (3) Nov., 1880, pp. 80-148.
[2]The Auk, 14 (1), Jan., 1897, p. 11.
[3]Yearbook U. S. Dept. Agr., 1904, pp. 246-247.

harm done by them would scarcely attract attention. Their immense numbers cause the comparatively small percentage of fruit destroyed to swell into an enormous aggregate." The influence of local overabundance of birds on crop damage was a frequent topic of discussion by Professor Beal and together with the statements here quoted clearly shows recognition on his part of the necessity of interpreting the results of stomach analysis. It may be added that Professor Beal employed field work to a large extent in his studies.

In 1901 Sylvester D. Judd stated:[4] "It is not easy to determine the exact relation of birds to agriculture, even though all of the constituents of the food are known; for the actual ratio of benefit to injury in the food habits can only be roughly approximated." Judd describes various methods of investigation in economic ornithology and recommends a combination of field work with stomach examination (pp. 11-18).

In 1912 the reviewer explained[5] that "no one claims that percentages do express economic values. They are simply convenient handles to facts and they must be interpreted" (p. 452), and "it is very evident that interpretation of economic values is the most important point in presenting the results of stomach examination" (p. 453). 1912.

The reviewer has carried on field work in economic ornithology during a considerable part of his time for more than twenty-five years, and in connection with one project that he inaugurated—food resources of wild fowl—more field work has been done by him and his colleagues than on any other food habits undertaking of the Biological Survey.

Kalmbach charges that a fallacy has existed in that results of stomach analysis have been regarded as the end product. The quotations made here indicate that at least four of those cited by Kalmbach as leading students of economic ornithology have not entertained the "fallacy".

"To set forth the general course open to a fuller, a fairer, and withal, a scientific appraisal of the economics of bird life. Intensive field observations, which, in the attainment of their own peculiar objectives, may be conducted just as accurately and yield a product just as scientific as the painstaking work of the laboratory, come foremost" (p. 89).

The objectives certainly would have to be very closely limited to give validity to this dictum. As noted previously they can only

[4]Bull. 15, U. S. Biol. Survey, p. 17.
[5]Methods of Estimating the Contents of Bird Stomachs, Auk, 29 (4), Oct.,

concern readily measurable things, that is, almost exclusively crops. Field observations may yield valuable information supplementing stomach analysis in various directions, but from the very nature of things (except in the directions indicated) they can not be as accurate as laboratory work. To put the matter tersely, it is apparent that we can learn more about the harmful traits of birds by field observation and more about their beneficial tendencies by laboratory investigation. The two activities complement each other and to attempt to substitute either for the other in its proper sphere is a mistake.

Actual demand at the present time for work in economic ornithology certainly is just as strong for further laboratory investigation as it is for that in the field. In fact every field study brings its own problems that require laboratory analysis for their solution. Every one of the recent game bird investigations, and every one of the modern game management projects has produced its demands for analyses of stomach contents, feces, and pellets. Great need has arisen for information on the food of the birds concerned, their competitors, and predators, and appeals have been made in embarrassing volume to the food habits research laboratory of the Biological Survey, the only public agency from which the desired assistance can be obtained. More than 5,600 food analyses have been made of the game birds alone and hundreds of stomachs and crops are on hand waiting examination.

To sum up, it must be stated, contrary to the conclusions of some readers of Kalmbach's paper, that the method of laboratory analysis is still quite alive and in great demand as a food habits research procedure.

It can not be replaced by field work but can be valuably supplemented by that method. It should not be assumed that field work has been neglected in studies of economic ornithology in the United States. In fact, policies of the Biological Survey have always called for a combination of field and laboratory investigations and these policies have been carried out whenever practicable.

It must not be forgotten that field work has its limitations the same as laboratory work, and that it has the added disadvantage that it can never be carried on extensively enough to represent local conditions in all parts of the country. In other words, a general picture of the economic ornithology of the United States can never be obtained by field work alone, but only by intensive use of all available methods of food habits research.

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