

AN UNTILLED FIELD FOR A REVISED KIND OF RESEARCH IN ZOOLOGY¹

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You notice my title is double-barrelled. The suggestion of a revised kind of research is one barrel, and of an untilled field of research is the other.

The revised kind of research has reference to research for answering the questions, In what way and how well do animals use their heads toward solving the problems by which they are always confronted under natural conditions? The intimation that these questions constitute an untilled field for research may seem a bit cryptic or even dubious. But I hope a brief presentation of a few facts and principles will convince you that such is not the case.

Wishing to be as matter-of-fact as possible, I am going to illustrate my points as far as I can by my studies on the California Woodpecker (*Balanosphyra formicivora bairdi*). What the king-pin in the life career of this bird is, no member of the Cooper Club needs to be told: *Acorn storing!* That is the way the phenomenon is pretty sure to drop into one's mind the moment he hears the bird's name. But do not fail to note that the storing business is only an aspect in the utilization by the birds of one of California's most widely distributed, abundant, and nutritious native food crops.

The idea of an animal's "using its head" is meant seriously in this discussion: None of the levity about it that goes with the parental reproof of the thoughtless child that if he would only "use his head" he would not make such blunders. The California Woodpecker's head is for us literally what you have if you collect this part of a specimen by decapitation, plus what the part was while it was still part of the living bird. That means, of course, a great many sub-parts—too many in fact to mention. But a few of these are of very special importance. For instance, the beak and mouth, the eyes, the ears, some of the neck muscles, and particularly the brain. Now, certain it is that a California Woodpecker uses its head quite differently toward solving its food problem from the way a Red-breasted Sapsucker, for example, or a Flicker, uses its head for the same purpose.

My leading question as applied to the California Woodpecker boils down, then, to this: What, if any, advantage, does this species get in the struggle with its food problem over woodpeckers which use their heads differently on the same problem? Do any of you doubt that this is a legitimate question to ask? But further, have you given it attention enough to be prepared with more than an exceedingly general, largely hypothetical answer? If you have, and have published your conclusions, you will do me a great favor by giving me the bibliographic references, for I have somehow missed them in my search of the literature on woodpeckers. Now I suspect you would be inclined to say that although the question may be legitimate just as a question, it is not fruitful as an actual research problem.

The main aim of this paper is exactly to contend that it is an exceedingly important problem, and is wide open to scientific investigation. Hence the suggestion of an "untilled field" of research. Although I have spoken and written on this subject rather frequently during the last decade or more, not until the last year have I gone into it systematically and persistently enough really to clarify my own thought—to say nothing of the thought of others—on the matter. While from

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personal observation and from the observations of many others, some attention has been given to a wide range of animal species, my one really concentrated effort has been on the California Woodpecker.

The answer (in part) to my question is: Several advantages accrue to the California Woodpecker as compared with its nearest of kin, from the unique way it uses its head in dealing with its food problem; (1) its survival ability is enhanced as registered in its population numbers; (2) its ecologic range is somewhat extended; and (3), and probably most significant of all, its domestic and social life is benefited.

Details of the evidence on which these conclusions rest cannot be even touched in this brief paper. As for (1) and (2), details enough, I hope, to turn the trick of proof are given in an extensive paper soon to be published in *The Quarterly Review of Biology*. So I must expect you merely to take or not take, as you choose, my word, pending a chance to read this paper. Although considerable evidence is in hand bearing on (3), that is, the domestic-social life of the species, this is dealt with only incidentally in the paper, and much more observational work is necessary on some of the points.

Now some persons may query in this wise: Granted that this particular bird, the California Woodpecker, uses its head in such a way as to make investigable problems of the kind you indicate, does it follow that other birds, and animals generally, do the same thing? Is it not true that the very exceptional character of this bird's way of living makes it present problems which have no counterpart with most species? Not so, I am convinced. All species—or at least all genera—of birds and mammals at any rate, present the same class of problems only in varying degrees of conspicuousness. Really it is the old familiar question of food habits, put, however, with more particularity than it usually is.

And this brings me to a few reflections on the legitimate query as to how it has happened—if it really has happened—that an important section of the phenomena of animal life should have remained even relatively uncultivated so far as research is concerned. At first thought it may seem surprising, if not incredible, that such a thing should have happened. But on second thought it will, I believe, be seen to be neither incredible nor surprising. The phenomena we are considering are a sort of no man's land between biology and psychology, as these sciences are academically conceived. The persistence with which these two provinces of knowledge of living nature have tried to stay apart, often rather contemptuous of each other, has been a serious spiritual disease of civilized mankind, and cries out to be cured.

Returning to woodpeckers, note that the leading question asked is one the answer to which, if it can be scientifically answered at all, must be answered through field research almost entirely. By its very nature the problem would no longer be that problem were the birds removed from their natural conditions and placed under experimental control.

It would seem then that animal ecology, a modern differentiation in the science of living nature, would be the subdivision of zoology into which our problem would fall. So let us look at ecology with reference to this matter. Were we to take at its face value almost any definition of ecology, we might suppose it would be the natural place for our problem. For instance "ecology is that branch of general physiology which deals with the organism as a whole, with its general life processes as distinguished from the more special physiology of organs" (Shelford, *Animal Communities in Temperate America*, 1913, p. 32). This is certainly broad

enough, and by implication specific enough to include such a question as ours. Yet I find scarcely a hint of any such question in this important book.

Nor does this type of question fare much, if any, better in the very latest ecological writings. "Ecology is a new name for a very old subject. It simply means scientific natural history" (Elton, *Animal Ecology*, 1927, p. 1). Devoted to the ecology of animals, as is the excellent book thus drawn upon, one might suppose questions like ours would be appropriate to it. Yet apparently the author of the book does not think so. At any rate nothing in its pages indicates that he does.

On the whole it appears that by a sort of tacit agreement, possibly on grounds of convenience, phenomena of the kind we are considering are excluded from ecology. But I suspect the exclusion implies something much more fundamental than mere convenience. It is deeply and firmly rooted, I believe, in biological theory. Look at it a moment from the standpoint of numbers of individuals—from the standpoint of the population problem.

Since Darwin's time especially, the idea of the geometrical rate of increase of organisms has held an enormously influential place in theoretical biology. That it is entitled to such a place is beyond question—the more so the more exact and broad becomes our knowledge. So obvious—almost terrifyingly obvious at times—is the tendency of species to over-run the earth, that it is not surprising, perhaps, that field botanists and zoologists in particular should use as one of the corner stones of their biological creed the formula "every species tends to increase up to the limits of its food supply."

Do you not see that what I am driving at is a summons to a more critical attitude in relation to this element in our creed? Do the particular food habits of a species cut no figure in the way the species meets its multiplicative exuberance?

Why do men and all other animals have brains at all, and of various grades of effectiveness, if this organ counts for nothing toward saving the creatures from their greatest dangers? How did they come by this part of their organization? Is it just a free gift of Providence under the name of Evolution? As the problems of animal ecology are now almost universally stated and studied no functional difference is recognized between the nervous equipment of even the major subdivisions of the animal kingdom. The brains, for instance, of a mammal, a bird, a fish, an insect, and a mollusc are treated as of equal rank.

Surely no real zoologist—zoologist, I mean, who is concerned with animals as nature presents them—can dodge these questions even if he would like to.

But almost certainly most zoologists will be ready to come back at me with: "But what you are talking about implies psychology and not zoology. Hence, important though the question may be, we steer clear of it.

"Very well," I say, "then let's consult the psychologists." Naturally we look first to the comparative psychologists—those, that is, who are professionally occupied with the minds of animals.

The reception we get here has more of the cold shoulder about it than has that received from the ecologists. We are told, often with an undisguised smack of contemptuousness, that with such questions psychologists have nothing to do. While it is admitted by some psychologists that although such questions seem to have some relation to their science, yet since the phenomena involved are not susceptible of laboratory control and measurement, they do not come within the scope of scientific psychology. Animal psychology is concerned with the minds of animals, not with their food problems.

It is important to realize the completeness with which all problems of animal life which cannot be definitely inventoried as mental, and subjected to experimental control, chiefly in the laboratory, are now debarred from psychology, especially in this country. Nowhere have I seen this more fully set forth and insisted on than in the writings of Professor C. J. Warden of the Animal Laboratory, Department of Psychology in Columbia University. (See, for example, *A Short Outline of Comparative Psychology*, 1927, and "The Development of Modern Comparative Psychology", *The Quarterly Review of Biology*, December, 1928.)

The truth seems to be that animal psychology as now professionally conceived is distinctly more unsympathetic with, and aloof from, the problems of animal activity as these are involved in animal life under the conditions of nature, than is ecology.

I must now touch upon my suggestion about a revised kind of research. "Revised kind", I say, rather than a brand-new kind because I do not by any means want to imply that the untilled field referred to is as wholly untilled as my treatment so far would indicate. The truth is, a great deal of excellent research is being done of the general sort I am longing to see more of. But here is a queer thing about it: What is being done is done largely by amateurs—by persons, that is, who have little or no standing among the scientifically elect. I will illustrate by referring to an instance right here in California.

The Michener family of Pasadena, mother, father and two growing sons, are making studies on their dooryard birds that are thrilling to all the family and could hardly fail to appeal strongly to all zoologists whose interests are in animal life in its full scope. In fact, I venture the opinion that results they are getting on the House Finch, especially, in bringing to light individual differences of their actions, are of first rank importance from their bearing on the general problem of personality. Not many aspects of human life are receiving more attention today, and are more urgently in need of deeper, more comprehensive study than that of the nature of personality. One of the gravest defects in understanding here is due, I am sure, to lack of anything approaching an adequate study of the problem from the comparative standpoint. I know of few if any instances of a more decisive move in the direction of such a study among animals in nature than in this Michener study of the House Finch.

It would be easy to mention many studies made by naturalists in various parts of the world that have much of the character of the research I am advocating. By way of illustration from the side of publication, I mention *British Birds*, edited by H. F. Witherby. The twenty and more volumes of this academically unsponsored journal are a real storehouse of observational material of the kind needed. And our own CONDOR and many of the University of California Publications from the Museum of Vertebrate Zoology are very much to the point.

What, then, do I mean by a revised kind of research? In what respects do studies which bring certain results of the right kind need revising? First and foremost in this respect: They need to be made on a much better understanding than now prevails of what the rather hackneyed words, the "animal mind", really mean. I have no longer the least doubt that the basic trouble here is the old, old bugbear, the mind-body problem. It is a hold-over of the doctrine that the Body is one thing and the Mind is an utterly different thing; that the body is something profane while the mind is something sacred.

Although great things have been accomplished during the last few decades toward ridding civilized mankind of this disease, there is much to do yet before a

complete cure is effected and the predisposing conditions are removed. And truly I do not believe any class of scientists is better situated and better minded just now for helping forward this aspect of man's salvation than are field naturalists, especially field zoologists. Surely no other class is so favorably situated for dealing with the comparative aspect of the problem. And it must be insisted that no problem of living nature, of human nature by no means excepted, has ever been solved even approximately without much application of the comparative method.

I earnestly suggest that all of you, as you go into the field this summer, whether birding, mammaling, reptiling, or what-not, set apart from your schedule of work, probably already made, a little time to ask seriously of some one or a few species, the question I have made the center of this paper: How does this species—how do the individuals of it—use their heads in solving the problems which from day to day they must somehow solve, or, as to some of them, suffer the death penalty for not solving it? Of course my suggestion does not imply that you should neglect legs, feet, wings, body and the rest. The whole thing must come into the reckoning, surely. But the role of the head, as I have defined it, is the major point of the discussion.

Put aside, for a while, all your long-cherished though perhaps rusty or otherwise decrepit notions about "instinct", "intelligence", "thought", "mind"; about "natural selection", "survival value", "margin of safety", "trial and error", "tropisms", "standard of efficiency" and so on, and just ask: "this particular individual or close-at-hand group of individuals, how does it—or they—use its or their eyes, ears, mouths, brains in this particular food or water problem, or this particular danger or reproductive situation?"

Probably you will not get far toward answering your questions in the time at your disposal. But I greatly hope that some of you may go far enough to convince yourselves that, given time enough, situations well enough selected, and skill and patience enough, it would be possible to go far toward some, at least, of your desired answers.

This brings me to my last two points: (1) The importance of school and college preliminary training in a field technique fitted to the enlarged, more philosophic-psychologic conception of zoological research.

Beyond a doubt, to my thinking, it is bound to be recognized sooner or later that training in zoology can no more be adequate without field work of the general kind I am talking about, than training in geology can be adequate without field work. Really, zoology with problems of the kind I am calling attention to left out, is no more full-rounded zoology than geology with stratigraphy together with erosion and sedimentation left out would be full-rounded geology. And think of the absurdity of supposing these earth phenomena may be studied as far as they need studying in the laboratory! Unquestionably, laboratory researches contribute much and in many ways to the solution of problems in both sciences. But to assume that field research in either may be dispensed with entirely or even largely would be unmitigated folly for either science, as much for the one as for the other.

Undoubtedly, what I am contending for implies several quite radical departures from prevailing curricular practices of both school and college education in zoology.

This leads to the other of my two final points: (2) Carefully selected field places for student instruction would be needed. Among the requisites of such localities would be the character of material available and accessibility. There is

no scarcity of good localities in such a Providence-favored country as California. Yet even here there is plenty of chance for the exercise of wisdom in choosing.

The only particularizing on this matter I have time for concerns the National and State parks. The admirable nation-wide move now under way to utilize the parks for educational purposes is directly in line with what I am contending for. But the move would need an enlargement of aims. I think I am right in understanding that at present the aim does not go much beyond that of enhancing the popularity of vacation outings; of making out-of-door recreation richer intellectually and esthetically; and of affording certain facilities to special students whose problems may at times call for work in such localities. This is all excellent. Not for a moment would I advocate a procedure that would minimize the value, or impair the effectiveness, of these aims. What I have in view is in addition to this.

The major impulse to what I conceive essential for getting the greatest possible good out of natural knowledge would have to be with the institutions of research and formal education. And central in the general purpose would be the laying of a foundation in the nature of things for the understanding of the nature of man himself. All effort in scientific research like all other human effort is motivated, finally, by human desire for human good. Sooner or later this vital truth must be seen with greater clearness and specificness than as yet it has been seen. Wherein lies the truth and wherein the falsity of the familiar sayings, "science for its own sake", "art for its own sake", and so on, must be discovered by deep and broad analysis of human nature and all the rest of nature.

In closing I cannot refrain from alluding to an incident on the campus of the University of California here in Berkeley that may contribute to what I am speaking in behalf of.

The enormous structure now taking shape just below California Hall is to be known as the Life Science Building. Thirteen (at last accounts) of the sciences of living nature are to be housed under this ample roof. That is a fine plan—fine in various ways. For one thing, it will bring together in one departmental library a great mass of literature which naturally has innumerable criss-crossings that must be artificially torn asunder if, as at present, the several departments are housed in different buildings on different parts of the campus.

Another fine thing about it is that it tends to fix attention on the question of what a life-science really is, and how many such sciences there are. And here comes my chance for turning the incident to my purpose. Since I have had no part in planning the building I am quite free to express any conjectures I may have about any aspect of the business.

Why, I wonder, "life science" rather than "biology" in choosing a name? The list of sciences to be housed here I notice contains psychology. This fact suggested to me instantly on learning it: Here is the explanation of "life science" instead of "biological science". For, I thought, how could psychology consent to be shut up in the same building with biology as latterly biology has defined itself?

According to that persistent fraud of a doctrine of the fundamental enmity between Body and Mind, biology has been the science of the Body, that is, a profane science; while psychology has been the science of the Mind, that is, a sacred science. Undoubtedly psychology is now moving rapidly away from its tradition in this matter. But surely no psychologist needs to be reminded of the tenacity with which people and things cling to their traditions.

Furthermore, we all know (or believe we do) much more about our minds than about our bodies, and prize (or suppose we do) our minds much more highly

than our bodies. Hence, psychology has seemed a much more important, a much nobler science than biology.

But, now, neither psychology nor biology could well feel any stigma from being identified, in a general way at least, with efforts at getting knowledge of natural objects that are alive. Even though some subdivisions of biology appear to be more interested in "living matter" than in living beings, yet the recognition by such biology of the fact that a certain kind of matter is alive gives that matter a peculiar interest.

Hence it is that psychology, of even the old-fashioned psychical kind, should be able to live and work quite comfortably in the same building with, let us say, biochemistry and biophysics, it being agreed that the building is called a life science building.

So I end with a hint as to what biochemistry and psychozoology may accomplish by strengthening each other's hands to the full extent of their ability. For this I return as usual to my woodpeckers. These birds eat acorns, you remember. Examination by experts of the Biological Survey, of the stomach contents of some specimens taken last fall by Mr. W. H. Burt and myself, proved that the birds were then living almost exclusively on this food. There was hardly a trace of any other food material in these stomachs.

Now how under the sun could I learn as much as I would like to about the composition and food value of acorns, and the digestive processes which the woodpeckers put them through in utilizing them as food, otherwise than by depending almost entirely on what biochemists can tell me? Their help is simply indispensable to my project as a whole.

Can I do anything for them in return? Well, to collect materials for their analyses is something, not much to be sure, because they could with a little trouble do that for themselves. But if they will go into the field with me and stay long enough and travel widely enough I will show them that the birds manage somehow to fabricate a lot of things, organs and tissues, which laboratory analyses do not throw much light on. But what is much more, the birds manage somehow to get a lot of energy from the nuts by means of which they accomplish a lot of things which chemical analysis throws hardly a ray of light on so far as details are concerned. In other words, I will undertake to convince the biochemist that by studying the activities of the California Woodpecker he can learn something about the chemical and physical properties of some of the substances of acorns that he cannot possibly learn from his laboratory researches.

Intensive cooperation between psychozoology and biochemistry can teach us a great deal about the causal efficiency of much of the natural order, both as to the animate and the inanimate parts of that order, that we can not learn in any other way.

Why not, then, make the beginning of this working together in the same building by all these sciences of living nature the beginning also of a more vital cooperation of these sciences than they have ever yet entered into for answering such queries as this of mine: In what ways and how successfully do men and all other animals use their heads to promote their own good?

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