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ORNITHOLOGY FOR A STUDENT OF EVOLUTIONARY PROBLEMS ¹

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NOT long ago I made a hurried visit to the graveyard wherein lie the remains of learned societies that I have seen come to life and go to death during my acquaintance with affairs of the intellect in California. Never mind how many tomb-stones I found there nor what inscriptions they bore. Our interest is in the living rather than in the dead. Reference is made to these graves merely for the sake of asking wherefore in the midst of such wide-spread death and decay, any of the creations referred to should possess real powers of endurance.

I am speaking of associations whose ends are mutual helpfulness among persons having some common intellectual interest, but which have to accomplish these ends without legal status and money endowment. What ones of all such have escaped the common lot? Everybody acquainted with the Cooper Ornithological Club knows one of them. There is one other, and only one as noteworthy as this. That is the Philosophical Union, the focus of which is here in Berkeley. It would be interesting to know why these two organizations so asunder in character and purpose should have struck such deep root into the intellectual soil of our community. One meaning of the fact is that in this, as in any community where many minds are working vigorously and without trammel, physical nature in her most objective, most sensuous aspects is bound to have the homage due her at one end of the intellectual gamut, while the most recondite problems of existence will enforce their claims to attention at the other. This is as it should be. It means intellectual health and symmetry. The whole universe belongs to the human mind, and the mind's determination to make good its exalted claims is irresistible. Proof of the validity of these claims is furnished by the circumstance that into whatsoever part of the universe the mind penetrates, it is there able to establish law and order; or if another form of expression be preferred, it finds there law and order of a sort fitted to its own powers and modes of working.

¹ Read at Northern Division Cooper Club, March 9, 1907.

If my purpose in starting off with this sweep is not obvious I will make it so. However little or much I may say to you tonight or ever, I would leave no doubt in your minds that I stand for the essential unity of all truth, for the worth-whileness and dignity of all real knowledge, for the fundamental interdependence and mutual concern of all sincere endeavor in whatever domain of learning. "Fine, even grand as sentiment," is likely to be exclaimed by almost any man of science however close-shopped a specialist he may be. I would convince you, if perchance some among you are unconvinced, that not only is this good as sentiment, but that it is good and, in the long run, inevitable in practice as well.

From now on we will stick to our text—the worth of ornithology to problems of Evolution. That American Ornithology has reached a higher development than any other department of systematic natural history appears to be pretty generally admitted among biologists, at least of our own country. I suppose that in some groups of plants and animals, the classification is as refined in certain particulars as is that of birds. But for balanced accuracy in the taxonomy and out-of-door knowledge of a whole class, few would question ornithology's claim to first rank. What does this mean for the standpoint of evolutionary research?

Experimentation in the laboratory sense is held by many to be the king-pin of today's biology. Fullness and accuracy in data gathering, criticalness in the use of terms, and rigor in the testing of guesses and theories is a truer characterization of the scientific spirit of the time. It is not so much the "statistical method" as the mathematical habit that has spread over our science. Not statistics but mathematics in whatever way it can get hold is to be the watchword from now on. This imperial science is bound to reign in biology as it does everywhere else.

"Only in experience is truth," said the greatest of modern philosophers. It is hard for biology, especially evolutionary biology, to take this dictum seriously to heart. But it must. Comprehension of problems and attitudes of mind rather than tools are what we prize. Thru these we are finding experimentation to be one wholly indispensable tool; but the very discovery of the power of the experimental method in biology is discovery of the limitation of that method.

So too with the statistical method. At the very moment when this proves its indispensability, it proves also its limitations; it proves its impotency except as it works hand in hand with other methods. So it is and always has been and always must be with all particular methods. Comte and his followers made out a hierarchy of the sciences and assigned to each its distinctive method. *Comparison*, you know, was held to be the characteristic method in biology.

The story of Louis Agassiz's criticism of an address at a scientific meeting attended by him soon after his arrival in America, that it was "descriptive but not comparative," is familiar. The incident marks the beginning of an era in American science, as earlier the comparative method in the hands of Lamarck, Cuvier, Goethe, the Milne-Edwards and others, had made an era in European science. The achievements reached thru comparison as the guiding light, stand forth too magnificently in the history of the last century's progress in biology to permit cavil as to its efficacy as an instrument. But powerful as it is, who today would think of attributing to it all power? It, too, proved its limitations in proving its indispensability.

But while instruments sooner or later reveal their limitations and hence their necessity of being coupled with other instruments, they also prove their dependence on skill and accuracy in handling. *Many methods well used* is the day's demand. Against this no caveat worth heeding can be filed. Observations have to be made; descriptions have to be written; nomenclatures have to be applied; measurements and enumerations have to be taken; experiments have to be performed; and all must

be done widely and accurately. This sounds like platitude to any of us so long as we listen wholly from within the enclosure of our own specialties. Only when we look at some other fellow sweating away in his field, do we falter about admitting the demand without qualification. If I chance to be a cytologist or a chemical biologist I am prone to estimate lightly the worth of questions of priority in naming new species, or of descriptions of cretaceous diatoms. If I am absorbed in the folklore of Polynesian races, or the trees of North America, I am likely to be dubious about my colleague who spends his substance on counting chromosomes in a cockroaches' egg. But despite the diversity and narrowness of specialization, I am sure we are, especially in these last few years, coming to see more and more clearly, not only that all these things must be done and well done, but that by and by they work into one another's hands; that they more and more support one another, and lean upon one another, and that all together will finally make up a magnificent whole.

My specific inquiry this evening is: Where is Ornithology to stand in the good time coming? What is it going to contribute to the on-coming of the better day? How are its incomparable riches of observation and description to be worked into the larger biology? By whom is the working to be done? The last question may be first answered for it is easiest. It will have to be done largely by ornithologists themselves, and by those of exactly the stamp that has always been the fiber of the Cooper Club. I mean ornithologists whose love for and knowledge of birds are in their very bones by reason of having entered there with their mother's milk almost; by reason of their having lived from nursling days in uninterrupted companionship with the birds.

One of the foremost merits of ornithology is that its interest reaches so large a part of all there is to a bird. It studies the living bird as well as its dead remains. It regards the nest as well as the builder of it. The eggs and changing young are noted as well as the adult. The home, the food, the songs, the movements; the specific, even the personal, eccentricities are not neglected. Just because birds, living, singing, nesting, appeal above all other objects in nature, not even excepting flowers, to the unsophisticated heart as well as mind of us humans, has this splendid store of knowledge been laid in. Formal, professional science, of necessity somewhat austere, is always inclined to look askance at sentiment and imagination, and hence to that in nature which specially allures these. The finger of caution is constantly raised against beauty as such, in color and form and gracefulness of movement, and against illusive suggestion and comparison. But despite this generally wholesome restraint, so compelling in these ways are some aspects of nature that they will not be altogether let alone. If official science will not heed them, amateur science will. Thus ornithology, over and above the large place necessarily assigned to it in general zoology by the constituted judiciary of the science, has ever been pre-eminently the amateur's field. And from the days of the Hon. Danais Barrington and Gilbert White, to say nothing of times antecedent to theirs, down to the present hour of the Cooper Club, knowledge of birds has come in large measure without professional sanction. And there is no doubt that much of this knowledge not only could not have been garnered by official science, but would not have been even if it could, since it would not have been regarded as quite worth the while. But now comes the highly significant thing. Official biology borne along by its own methods and results comes at length to see that it must have, with the rest, just the sort of data that amateur ornithology has been gleaning all these years.

The Darwinian tenet that "varieties are incipient species" made the trivial *kinds* of plants and animals glow with a significance they never before possessed,

and a concomitant impetus to their study resulted. In the absence of clearly defined conceptions of what either a variety or a species is, or of workable criteria for testing them, the multiplication of intergrading series ceased after a while, to be profitable to the student of evolution. For one occupied primarily with the making of a consistent, usable classification, such series are, of course, always important. But to him who seeks the meaning of these series, the mere exhibition of them does not yield much satisfaction; and the multiplication of instances after conviction is reached that the world is full of them, is not very enlightening. So it came about, not from the behests of science, but from that particular frailty of human nature which is impatient of efforts the value of which as measured by its own standards is not obvious, that "hair-splitting" in systematic botany and zoology fell into disrepute. Now, however, that the discoveries of Mendel and De Vries have put into our minds conceptions about *kinds* of organisms that we did not have before, and into our hands instruments for testing the character and validity of these, we see that it is exactly to the refined observations and descriptions of what exists in nature in the way of kinds, that those engaged on the problems of origin are compelled to turn for material to work with. Right, in science as in all else, may serenely await her day of vindication.

Species splitters, among whom American ornithologists have long sat on the front benches, have a right to be gratified that the very hands which a few brief years ago were pointed at them in disapprobation of their labors, are now stretched out to take from them the products of those same labors. You young bird men who a short while ago were likely to receive smiles of cynicism rather than of encouragement from biologists in high places for your enthusiasm in making out the subspecies of our song sparrows, our juncos, our kinglets, our horned larks and the rest, need no longer lament lest your work should have no reward but the pleasure in its performance. For a long time to come whatever of this sort you do will be rated higher on the scientific stock exchange than formerly it was.

But I am not going to let you off without an appendix to this reward of merit which I gladly give you. What further are ornithologists going to do in the premises? That they will keep on gathering information of the kind they have already garnered in such richness is to be hoped. Will they do more? Will they take a hand in searching after the significance of the facts, now that keener probing instruments have been devised? Two circumstances encourage the expectation that they will. In the first place the large amount of young blood there always is in ornithology, augurs well. Proverbially it is on the young men that new methods and new ideas have to rely mostly for getting themselves tried out.

In the second place it would seem that the insistence ornithology has long placed on precision should be a guarantee of its readiness to try other methods that are pre-eminently of this character. *Exactness* in observation, in description, in measurements, in terminology, has been its special glory. The critical habits engendered by these exactions should, it would seem, be rich and eager soil for still other exact methods to grow in. The ornithological positiveness as to what, on the morphological side, constitutes the species and subspecies, and the rigorous practices in testing these, leave little to be desired. This very positiveness and rigor, going thus far, ought to be intolerant of restraint on going farther. To the ornithologist who loves truth no less ardently than he does birds, the utter vagueness as to what his morphologically delimited groups would look like were they to be physiologically tested, cannot but forever fill the background of his scientific consciousness with foreboding. Cloddish and inadequate to the student of birds, above all naturalists, ought to be a classification that rests almost exclusively on

characterizations similar to those on which is based the classification of crystals and rocks. Form, color, proportions, texture, are these not, after all, the qualities upon which the ornithologist—in common with all other zoologists—relies for his classification to well nigh the same extent as does the crystallographer and the petrographer? And yet the bird is a *living* thing; exactly that about it which gives it its interest as compared with the crystal and the rock, quite ignored in its classification!

Can anything be more patent, when once you look the situation squarely in the face, than that our biological classifications *must* sooner or later be put on a broader foundation? Nothing that is *half* done is *well* done. Obviously our systems are not more than half done; for they practically ignore at least half of the nature of the objects classified.

Ill would it become me, a peculiarly unworthy member of the Cooper Club, to bolt into your midst with suggestions of new enterprises for the Club. I am not going to do this. Addressing you not as club members, but as a group of wide-awake ornithologists, I am merely going to point out wherein, as I see it, ornithology has a vantage ground quite its own in which to use such of the new instruments of research as have already proved their efficacy.

Would it not be practicable thru cooperation to test the nature of so-called ontogenic species among West-American birds? It seems to me that a few incubators, a few capacious but inexpensive out-of-door bird cages and a few competent ornithologists judiciously located in different parts of California would in a few years go a long way toward the final answer to this question. What considerable difficulty should there be in the way of taking the eggs of some of the bleached-out desert species like the Le Conte thrasher, the Abert towhee, the desert song sparrow, and the pallid wren-tit, to San Francisco, or Eureka, and rearing the broods to see what effect the new climate would have on the color?

Again who knows that the question of natural hybridization among birds might not be successfully attacked by breeding experiments? And what a capital problem this is, more than ever now that unit characters and Mendelian inheritance are among the realities of biology!

I can think of no set of facts an interpretation of which would be more illuminating than those presented by the supposed hybrids of the two flickers, the golden-shafted and the red-shafted. This problem appears to stand about where it was in 1892. Allen's studies on the distribution of the genus *Colaptes* and the color styles assumed by the "hybrids" between *auratus* and *cafer* were published in that year. Much as this good work advanced the subject, it left the most critical points as dark as ever. Do these two species actually mate together? If so are all of the offspring of the same pair marked in the same way? Are the hybrids fertile, and if so how are they marked? Do "hybrids" ever come from pure stock matings of either *auratus* or *cafer*? Perhaps these birds could not be induced to breed in captivity, but a whole string of such questions might be partly or wholly answered by studies in nature. An ornithologist well trained in general biology ought to be enabled to devote himself to this single question for an indefinite period. During the breeding season he should spend most of his time in the field; and when he could get away from the demands in this quarter, there would be plenty of laboratory and museum work on pigments, embryonic stages, moulting, anatomy etc. Furthermore the possibility of the birds breeding in captivity should be carefully tested. Pedigree culture, and crossing under control, would tell most could they be applied. No one but an ornithologist, however skilled in the methods of general biology, is equal to such a problem.

Another group of questions which ornithologists are in an especially favorable position to tackle is that of correlated characters and variations. Much could be done here without resort to breeding experiments. Observation coupled with anatomy and embryology could go a long way. Perhaps the most practical and interesting single question is that of whether or not the superficial characters ordinarily used for differentiating species are not associated, even if not actually correlated with other more deeply seated structural characters. I am not thinking about anatomical features that might serve as reliable tests of affinity, and hence as bases of more natural classifications. Of course I do not neglect the labors of such anatomists as Huxley, Fuerbringer, Shufeldt, Lucas and others in this field. Taxonomic trials with anatomical data have been carried far enough to justify, probably, the opinion of Newton and Gadow that "it is hopeless to attempt to arrive at a natural classification of Birds by a mechanical arrangement of even a great number of alleged leading characters."

What I have in mind is quite a different matter. It is this: Given two or more species of a genus well defined by characters generally used in ornithology, what other differentiating characters, if any, would a thoroughgoing examination of the whole animal discover?

I am quite sure that we must sooner or later, see that characterizing a species just far enough to place it in an artificial system of classification, is a very different matter from defining it thru and thru: that is, in such a way that nothing whatever truly distinctive about it shall have been left out. This is the sort of definition we shall demand when once we get red-hot after the problem of what a species really is. An individual bird consists of all there is of it from the time incubation begins until it dies. Isn't that so? If not, what segment of the life cycle does not belong to the individual? I am sure no bird man, thoroly imbued with what I take to be the distinctive spirit of ornithology, has the least desire to thus fragment a bird's life.

Well, if the real bird is the whole life of the bird, then for its whole life it is a member of its particular species; and if at *any* period of its life it has characters that are different from those possessed by any other species whatever, these must be specific characters and would surely be noted in a full description of the species. This, of course, means practically that the egg, not merely the *egg-shell*, the sperm, the embryo at all its stages, the fledgling, the adult bird in all its phases of moulting, with all its habits and songs, would have to be attended to in a thoroughgoing definition of the species.

Ornithologists have as a whole done better in this regard than other zoologists, and that is just the reason why they should do still more—vastly more—in the same direction. "To him that hath shall be given." The complement of the old truth is more to the point here: "Of him that hath accomplished shall more be expected."

One might easily designate other places wherein ornithology may be expected to shine in the new era of exacter, broader observation, and more critical testing of hypotheses and definitions into which biology is now fairly entered. The hard task, for instance, of establishing a more exact and trustworthy scale of values for characters, ornithology should contribute to largely. What department of biology except possibly entomology, is in better position to handle color from this point of view? It would be easy to designate other places in evolutionary theory at which ornithology might work with peculiar efficacy; but these are enough for a liberal program.

I close with a single reflection on the outlook of biological generalization of the day. At no time during the last twenty-five years have evolution hypotheses been so up in the air as just now. A few writers believe that the idea of evolution itself is going to smash. Sober, well-balanced naturalists are not skeptical to this extent. Many of them are, however, disposed to settle down to the view that search after a *method* by which species originate is time wasted simply because there is no such a thing. There are many factors, they say, in evolution, and biology has done all incumbent upon it when it has found out what they are. Certain it is now that there are various factors in species production, and it is a great achievement to have unearthed so many of them. Natural selection is a widely operative factor; so is sexual selection; so is orthogenesis; so is isolation; so, quite certainly, is mutation. The list, were it complete, of more or less distinct, more or less efficient, factors would be much longer. I ask are we to rest here? Having corralled these *factors*, are we going to write *finis* over the gate of the corral? Not if biological motive is true to itself. Does not your mind and mine, and every mind that is in the habit of thinking at all, start off immediately and unrestrainably, the last factor having been lodged in the corral, in quest of some one or at least a less number of factors or principles underlying those already captured? If species are fully produced by so many different causes, different combinations of these operating together in different groups of plants and animals, how do we know that species have anything in common? Is it a tenet of biology or any other physical or spiritual science that unlike causes produce like results? And if you are not certain that all species have something in common, what justification have you in attempting to treat them all alike in classification? What is the good of bothering about uniform rules of nomenclature if the rules are to apply to different things? But are we not warranted in believing, nay, are we not compelled by the totality of biological data to believe that there is more unity in evolution than all these factors indicate? Is there not fundamentality in the metabolic processes of organisms? Is not this true also of response to stimulus? Is it not true of reproduction? Has not the cellular theory of organization a unifying principle in it that is about the securest of all biological generalizations?

It is, I am confident, only stating what every thoughtful naturalist assents to without hesitation to say that the goal of biology—not a remote, but the immediate, animating goal—is greater unification of its knowledge. Minds can never rest from the search for deeper, more inclusive principles. This brings our evening's discussion to a close at the point from which it started.

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THE BIRD ISLANDS OF SOUTH AFRICA ¹

By W. L. SCLATER M. B. O. U.

ONE of the most remarkable forms of bird life at present existing is certainly the group of Penguins. These birds, which constitute the Order Impennes, stand wide apart from all the other living Orders of birds not only in their structure but also in their life history and distribution. They are the only birds in which the metatarsal bones of the adult show plainly their threefold origin, the bones in question being short and separated by deep grooves. The

¹ The spelling and capitalization in this article accord with the request of the author.