

other sides of the same subject formally presented by some one who may not fully agree with the preceding author, but who has had time to prepare a written paper defending his position and advocating his opinions. While persons may differ in their opinions as to which is the better plan, the consensus of opinion of the members present was that the program presented gave them much information and food for thought. Many branches of mechanical science and engineering were touched upon, and while special emphasis was put upon those sessions devoted to aeronautics and hydrology, it was thought that the place and its surroundings warranted it.

The attendance at the meetings of the section has been excelled in recent years only by the 1902-3 Washington meeting. It is thought that this is encouraging for the future of the section. It is to be hoped that the members of the association connected with the section will show their continued interest in it by their attendance and by presenting papers at future meetings.

WM. T. MAGRUDER,  
*Secretary.*

A REPLY TO RECENT STRICTURES ON  
AMERICAN BIOLOGISTS.\*

A NOT uncommon, though possibly more or less indefinitely formulated, opinion has recently found an expression in print† to the effect that American systematic zoology has degenerated into a mere recording of minute facts, instead of being a study of problems; in other words, that it has been reduced to a somewhat low-level, though possibly sometimes useful, craft, and has lost caste among the sciences.

It must be admitted at the outset that

\* Read at the Twenty-first Congress of the American Ornithologists' Union, at its meeting in Philadelphia, November 18, 1903.

† Talcott Williams, 'On the Skirmish Line of Science,' *Booklover's Magazine*, II., November, 1903, p. 458.

this criticism is deserved to a limited extent. If we take ornithology as an example, what are the results of our labors in this country? Look over the long files of the *Auk* and see what they contain: An astounding and in many ways admirable record of facts relating to the distribution of our birds, their habits, their specific and subspecific characters! The refinement and acumen of discrimination with regard to the latter have reached a high degree of development, and it is doubtless true that the birds of North America are better known than those of any other part of the globe of even approximately similar extent. Our collections of native species are vastly larger and more complete than those of any other country and our methods and technique, both of collecting and of recording, greatly superior to those of the rest of the world. And the work goes on unceasingly, and the details are being more skilfully and accurately and voluminously elaborated every day. In fact, we are working so fast and so well that we have left the rest of the ornithological world far behind. Some of the younger European ornithologists are trying to catch up, but they will never be able to do so because the North American material can only be had here and because we have gained such a lead in the race.

But for what purpose are we accumulating all this minute detail, this enormous material? What are we straining our best faculties, our acknowledged ingeniousness, for? Thus far we have but little to show that would give a satisfying answer to these questions. On the surface, at least, it looks as if we were following these pursuits chiefly for their own sake, for the satisfaction of mere accumulating, for the exercise of these mental faculties. To the outsider it must certainly appear as if we regard the work we are doing as an end, not as a means towards an end. The ques-

tion then arises: Have we really, in our eager pursuit of the details, lost sight of the final object of our studies, which alone can justify the expenditure of brain work and money; and have we thus degraded our science to a mere sport or a brilliant juggling with facts and words? Have we forgotten that the problems are the essential part of science and that the records are only the tools with which to work out the problems?

Thus far there are but slight indications on the surface that the higher problems have attracted much attention, and I am afraid that many of us must plead guilty to having groped in the lower regions so long that we almost forgot that there is something higher. Nevertheless, any one who has the opportunity to look below the surface must be aware that a notable amount of thinking and theorizing is going on without causing much outward commotion. While this holds true to a slight extent for the whole range of problems, it is particularly so with regard to a certain limited class, referring, as I do, to the problems more or less intimately connected with the question of life zones or the zonal distribution of life. Here, thanks to the brilliant work of some of the most prominent scientists of the American Ornithologists' Union, considerable progress has been made in the right direction, and more may be confidently expected in the near future.

It is not difficult to demonstrate just why this class of problems should first receive attention. The explanation is that not only have the requisite facts been recorded on an unprecedented scale and with a clear understanding of the requirements of the case, but nearly all the material necessary for at least a partial solution of the problems are available within the boundaries of this country. The question, up to a certain point at least, is a local one, viz., the interrelation between the North American

biota and the various zonal areas which its component animals and plants occupy. Up to this point our scientists will be able to solve the problems. It must be conceded, furthermore, that the truly monumental way in which the material is being gathered, recorded and elaborated makes it possible for them to construct upon it a philosophic building which shall be more enduring than the ephemeral structures of past times. We may confidently look forward to the establishing of proof where formerly we had only uncertain theories and hypotheses.

Just here we have reached the point where we become aware of our limitations and their cause. We have the means of approaching and solving the local problems and questions in so far as they can be elucidated by local work, but we are utterly shut out from attacking the larger, more universal problems, without which we shall have to submit to the stigma of being mere *sciolists*, a name applied the other day to American biologists generally by a reviewer of the achievements of American science.

Whether we accept a holarctic region, or recognize the nearctic and the palaearctic as two separate regions, no one will now deny that a great portion of the biota of the northern parts of the new and the old world is intimately related. But when it comes to the questions as to the extent and the degree of this intimacy; whether their faunas and floras have a common origin; or whether they are a blending of two or more biotas, and in such a case, where each component part originated and how the blending took place, by what routes and at what time—when it comes to these and similar questions, we find that opinions and theories are digressing in all directions. If we ask ourselves, in what genetic relation do the animals and plants now inhabiting the northern world stand to those living before them in the same territory; whether

the forms which to-day occupy a certain region are descended directly from those whose bones we find in the strata underneath them, or have originated in some far distant continent from ancestors indigenous there, we meet again a distressing amount of uncertainty and diversity of opinion. And if we inquire into the reason for all this controversy, this lack of agreement among biologists, one cause stands pre-eminently forth as responsible, viz., *insufficient and defective records!*

Thirty or forty years ago the biologists, with an almost childish faith, believed that they had gathered all the material that was to be had, and that they would exhaust the supply of facts in a very few years. Europe, according to them, was thoroughly explored, the records were complete; northern Asia had just been covered by the magnificent expeditions of Middendorff, Schrenck, Radde and others; the biotic secrets of North America were divulged in the Pacific Railroad reports, the Mexican Boundary reports, the reports of the expedition west of the 100th meridian, and of the survey of the territories. They conceded that a few more species might be expected to turn up in the interior of Africa, but these, it was thought, would not be able to alter conclusions materially. And so they proceeded to speculate and generalize, to pull biology out of the mire of mere recording and gathering of facts into the regions of real science. But unfortunately, although theories and hypotheses multiplied, they nearly all led in different directions, and each philosopher came to results at variance with those of the others, according to the kind of material or the portion of the record he happened to get hold of.

Far be it from me to speak lightly either of the records and material gathered by the men of that generation, or of their generalizations. It was not their fault if

the ultimate results have been disappointing. Many of their records are of permanent value; a great deal of their material still serves as foundation for our present work; some of their conclusions and theories have proven to be correct. Without them we were not standing where we are now. The fault lies with their successors who considered the preliminary work finished and who failed to continue it systematically and symmetrically.

Shortly after the period alluded to it became painfully clear to biologists that the amount of facts, material and records which had been gathered was not only a mere handful as to numbers, but even more hopelessly defective as to accuracy and minuteness of the data. With the opening up of the world by means of improved facilities of communication, the enormous mass of new material representing unexpected forms in endless number fairly swamped the systematic biologist during his work of recording and describing. This flood of new species and genera naturally affected the scientists of Europe most, as it accumulated in the museums of countries which not only previously had colonial possessions all over the world, but now by the division of Africa despoiled a whole big continent of its most striking novelties. The American biologists, on the other hand, whose field was in nearly all cases limited by the political boundaries of the United States, were shut out from the rest of the world and reduced to a more intensive cultivation of their own area. The result has been curious in more than one way. On the one hand, our development became defective, because we lost touch with the outer world and so in a measure were left behind; while, on the other hand, we extended the accurate, *i. e.*, scientific, knowledge of our own field far beyond that of the rest of the world.

This, then, is the standpoint we occupy

to-day: We are still confronted by the same problems which our predecessors failed to solve because of their lack of definite and detailed knowledge of the facts. With a view to their solution we have gathered a material which, for technical perfection and minute accuracy, is unrivaled. It has but one fault—it is terribly lopsided. We have or are in a fair way of obtaining shortly most of the data relating to the nearctic region, but we have not a scrap of the right kind of material relating to the other half of the northern world. Nor is the right kind of material in existence anywhere at present; for while there is preserved in the numerous museums of Europe a large number of specimens, and while the literature contains a vast accumulation of records, neither the data accompanying the former nor the observations contained in the latter are, as a rule, so precise or so detailed as now required. This widely scattered material, in addition to its insufficiency due to superficial and haphazard collecting, is distressingly uneven in quality. Moreover, it has not been worked up according to uniform methods, nor by workers occupying the same viewpoint. Its component parts are not only uncorrelated, but they are at present utterly impossible of correlation.

Thanks to the example set here, Europe is just beginning to realize the fact that she has neglected her own fauna. Some of her more wide-awake biologists have recently attempted to grapple with the problems I have alluded to above, but they have not advanced much farther than to formulate them. They have found their *records far too insufficient and defective.*

Lest I be accused of exaggerating let me quote what I wrote more than two years ago in a review of the attempt, by a prominent European biologist, to generalize from the incomplete data at hand.

After having said that one of the distinct merits of his work was that it revealed the defects in our knowledge, I continued:

"It is a kind of stock-taking by which we find out just how our business stands. It must then be admitted with regret that the status is not as satisfactory as one might have reason to expect. There is yet a great uncertainty as to the exact and detailed distribution of many of the larger and more important animals in the Arctic regions and in Europe. The grosser facts are known of course in a general way, but they are not sufficient for the purpose. The finer details are still unknown, or if known in some isolated cases are unavailing because they are as yet only isolated."\*

This statement has remained unchallenged ever since and but little has been done to remedy the defects in a comprehensive way. What is true of Europe is no less true of Asia. Let me recall to you that a distinguished member of this union, in a paper published during the present year and dealing with a single class of vertebrates only, found himself obliged to bewail his impotency to settle important questions by such statements as these:

"Material from northern Europe available for comparison with the Siberian series is too scanty \* \* \* to be of any importance. \* \* \* "

"Also material is lacking in sufficient quantity to give much new information in respect to the supposed difference. \* \* \* "

"But lack of material prevents a critical consideration of the subject. \* \* \* "

"Without other material it is impossible to compare the present series. \* \* \* "

"In the absence of specimens \* \* \* it is provisionally referred. \* \* \* "

And so forth no less than eight times in the same paper under eight different

\* Scharff's 'History of the European Fauna,' *Amer. Naturalist*, XXXV., 1901, p. 113.

species.\* I may add that he had the contents of all the leading museums of our country at his disposal.

Another member of the union, who also brought together all the available material from the American museums, published a monograph of a holarctic genus of birds last year.† He recognized 35 different forms by name, 22 from the nearctic region and 13 from the palearctic region. As a basis for this work he had no less than 2,150 specimens, a material which, if it had represented evenly the range of the genus, might have been sufficient to give an exhaustive account of the various forms and might have led to valuable generalizations with regard to their origin and their distributional migrations, but the ridiculous inadequacy of the palearctic material for the purpose may be plainly seen when I state that while the American specimens at his command numbered 2,108 specimens, or an average of over 95 specimens per recognized form, the European and Asiatic material consisted of 42 specimens, or 3¼ specimens per recognized form. Of three of the latter there is not a single specimen in any American museum.

One more striking example, this time derived from the class of mammals. The only museum in North America which has made strenuous efforts to obtain palearctic mammals, and which by all odds contains the largest material from the holarctic region, possesses about 94,000 specimens from the nearctic, as against about 3,300 from the entire palearctic. It is safe to say that this enormous discrepancy is even excelled in the other museums. A similar census of the birds in the same museum was not practicable, but it is perfectly safe to say that the proportions are nearly the

same, or thirty nearctic specimens to each palearctic. The discrepancy is the more marked when we consider that the area comprised in the palearctic region is nearly twice as large as that of the nearctic, so that area for area the palearctic material in our museums is scarcely one and two thirds per cent. of the total holarctic material.

It is unfair, therefore, to blame the modern American biologist for his failure to enter a higher philosophic sphere. He has the ambition to do so, he has also the ability; moreover, he has done part of the preliminary work and done it exceedingly well. But as yet he is without the means.

And now, how can this unfortunate condition be remedied?

There is only one way, viz., *the acquisition of more and better palearctic material and records*, collected by professionally trained observers; worked up together with and conformably with the nearctic material and records already gathered and elaborated with such signal success. Because of the possession of the latter it follows of necessity that the American biologist should also gather and elaborate the former. The work already done pre-eminently qualifies him to carry the whole to a satisfactory conclusion. He has done the first part well; he will do the remaining work equally well, if only given the chance.

It may seem strange to call for more material and more records in reply to the accusation that we are losing ourselves in that very kind of work. It must be borne in mind, however, that what is here called for is not the insatiate, indiscriminate accumulation with no clear purpose in view, but a well-digested, premeditated search for the material which bears directly on the problems already outlined and which experience has shown to be indispensable for their solution.

The fact is that we are not losing our-

\* J. A. Allen, *Bull. Amer. Mus. Nat. Hist.*, XIX., 1903, pp. 126, 129, etc.

† H. C. Oberholser, *Proc. U. S. Nat. Mus.*, XXIV., No 1271, 1902, pp. 801-884.

selves as alleged; it only appears so to the outsider. Yet, it is necessary that our higher duties should be held up to view 'lest we forget.' Moreover, the time has come for the gathering of the new material unless we are to sink back into a shallow rumination of the old. The American biologist stands ready to expand his dominion into the old world, if he be given the means, and when he shall be through with his work, the facts and records will be in such a shape that the philosopher can rear a structure upon them that will stand.

The means by which he may be put in this enviable position have been set forth in another connection\* and need not occupy us here.

LEONHARD STEJNEGER.

WASHINGTON, D. C.,

November 14, 1903.

#### SCIENTIFIC BOOKS.

*The Positive Philosophy of Auguste Comte.*

By L. LÉVY-BRUHL. Authorized translation, to which is prefixed an introduction by FREDERIC HARRISON. New York, G. P. Putnam's Sons. 1903. Pp. xiv + 363. 8°.

Anything that will help to make the philosophy of Auguste Comte known to the readers of English can not fail to be useful. The English translation, therefore, of a work on that subject by such a man as M. Lévy-Bruhl, the well-known author of the 'History of Modern Philosophy in France,' and who 'writes as a student and not an adherent of Comte,' is especially welcome.

It will probably be one day regarded as the most remarkable anomaly in the history of science that the work which formed the turning point from metaphysical to scientific philosophy—the 'Positive Philosophy' of Auguste Comte—remained three quarters of a century without being translated into the English language. This singular circumstance has led to some very peculiar results, and accounts for the totally false idea that the English-

\* *Carnegie Inst. Yearbook*, No. 1, pp. 241-266, 'Plan for a Biological Survey of the Palearctic Region,' by Leonhard Stejneger and Gerrit S. Miller, Jr.

speaking world entertains with regard to Comte and his doctrines. Many suppose that he was a very bad, irreligious man. An eminent divine recently stated from the pulpit that 'Comte, the great French philosopher, taught that religion was only a phase of superstition that belonged to the childhood of the race and would be outgrown.' Interrogated as to where Comte taught this doctrine, he was unable to cite any work or passage. The fact is that Comte had a strong religious nature, and one of his aphorisms was that 'man is becoming more and more religious.'

Others, like Huxley (who does not seem to have read the 'Positive Philosophy'), see nothing of value in Comte's system. A common opinion is that it is a sort of utopia, and Comte's name is frequently associated with that of Fourier. Scarcely any one has the idea that he was a scientific man in the accepted sense of the expression, although he was by profession a mathematician.

The fact that Comte wrote another and later work, his 'Politique Positive,' in which he drew up a program of social regeneration and founded a cult, created the general impression that he was only a dreamer. His zealous followers from the standpoint of the cult saw to it that this work should be translated into English. There is no doubt that this did incalculable harm to Comte's entire system. For, in the first place, as M. Lévy-Bruhl clearly shows, it is impossible to understand the 'Politique Positive' without an acquaintance with the 'Philosophie Positive.' If Lévy-Bruhl had done nothing else than to dispel the illusion that the 'Politique Positive' was an after-thought, the product of a diseased mind, and a mere dream of a fanatic, it would have fully justified his writing this book. The few who have read the 'Philosophie Positive,' and especially those who have also read the five early papers written from 1819 to 1825, know already that the 'Politique Positive' was contemplated by Comte from the beginning, and was steadily kept in mind during all the patient years that it required to write the 'Philosophie Positive.' That work was to be simply the necessary preparation and scientific foundation for his final great con-