century, with far less confidence in his ability to speedily solve it and with far less exaggerated notions of his own importance in the grand aggregate of Nature, than man entertained at the beginning of our era. But no devotee to science finds humiliation in this departure from the primitive concepts of humanity. On the contrary, he has learned that this apparent humiliation is the real source of enlightenment and encouragement; for notwithstanding the relative minuteness of the speck of cosmic dust on which we reside, and notwithstanding the relative incompetency of the mind to discover our exact relations to the rest of the universe, it has yet been possible to measure that minuteness and to determine that incompetency. These, in brief, are the elements of positive knowledge at which we have arrived through the long course of unconscious, or only half-conscious, experience of mankind. All lines of investigation converge towards or diverge from these elements. It is along such lines that progress has been attained in the past, and it is along the same lines that we may expect progress to proceed in the future.

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## ZOOLOGY OF THE TWENTIETH CENTURY.*

LOOKING over the vice-presidential addresses given before the American and British Associations during the past year or two in an eager search for suggestions, I found a prevailing tone of retrospect. The advance of science in the nineteenth century was a favorite theme, and little wonder in view of the century's marvelous events. Since by the arrangement of the council I lost my opportunity to be an end-of-the-century historian last year, I shall essay the rôle of a prophet this. On the historical side I could have given you something very interesting, I assure you. Not I, but the council that delayed my address to the following century, must be held responsible for the poor substitute I am able to present.

We have stood in retrospect at the close of the nineteenth century and marveled at what it brought forth. Here at the threshold of the twentieth century it is natural that we should wonder what it will unfold. Will the changes be as great, and in what direction will advance chiefly be made? I am the more content to consider such questions for three reasons: First, because we can use history to formulate predictions; second, because the attempt may possibly influence to some slight degree the future development of zoology; and third, because the attempt is tolerably safe, since we shall none of us know all that the century will bring forth.

Comparing the beginning of the twentieth century with that of the nineteenth, we find the most striking advances to have taken place in our morphological knowledge. The nineteenth may indeed be designated the morphological century. The demands of systematic zoology first made anatomical studies necessary. Later, comparison came to be accepted as the fundamental zoological method, and comparative anatomy, emancipated from its servitude to systematic zoology, became an independent science. Still later embryology arose, at first as a descriptive science and then as a comparative one. Out of embryology arose modern cytology, which in turn is creating a comparative histology. Partly as a result of studying embryology as a process has arisen the modern tendency toward comparative physiology. As a result of the general acceptance of the evolution doctrine, the study of the geographical distribution of organisms and of adaptations has gained a new meaning. From the great matrix of 'gen-

* Address of the Vice-president of Section F, Zoology, American Association for the Advancement of Science, Denver Meeting, August, 1901.

eral biology' there has begun to crystallize out a number of well-defined subsciences.

Looking broadly at the progress made during the past century, we see that zoology has become immensely more complex, due to its developing in many lines, and that the new lines are largely interpolated between the old and serve to connect them. The descriptive method has developed into a higher type—the comparative; and of late years still a new method has been introduced for the study of processes—the experimental. The search for mechanisms and causes has been added to the search for the more evident phenomena. The zoologist is no longer content to collect data; he must interpret them.

In view of the past history of our science, what can we say of its probable future? We may be sure that zoology will develop in all these three directions: (1) The continued study of old subjects by old methods; (2) the introduction of new methods of studying old subjects; and (3) the development of new subjects.

I am not of those who would belittle the old subjects, even when pursued in the old way. There is only one class of zoologist that I would wish to blot out, and that is the class whose reckless naming of new 'species' and 'varieties' serves only to extend the work and the tables of the conscientious synonomy hunter. Other than this all classes will contribute to the advancement of the science. No doubt there are unlabeled species, and no doubt they must, as things are, be named. And no doubt genera and families must be 'revised' and some groups split up and others lumped. So welcome to the old-fashioned systematist, though his day be short, and may he treat established genera gently. No doubt there are types of animals of whose structure we are woefully ignorant; no doubt we need to know their internal anatomy in great detail. So welcome to the zootomist in this new century, and may he invent fewer long names for new organs. No doubt there are groups of whose relationships we know little and which have been buffeted about from one class to another in a bewildering way. We need to have them stay fixed. So welcome to the comparative anatomist and the embryologist, and may their judgment as to the relative value of the criteria of homology grow clearer. No doubt our knowledge of inheritance and development will be immensely advanced by the further study of centrosomes, asters and chromosomes. Welcome, therefore, to the cytologist, and may he learn to distinguish coagulation products and plasmolytic changes from natural structures. All these subjects have victories in store for them in the new century. To neglect them is to neglect the foundations of zoology.

But the coming century will, I predict, see a change in the methods of studying many of these subjects. In systematic zoology fine distinctions will no longer be expressed by the rough language of adjectives, but quantitatively, as a result of measurement. There is every reason to expect, indeed, that the future systematic work will look less like a dictionary and more like a table of logarithms. Our system of nomenclature, meanwhile, will probably break down from its own weight. Now that the binomial system of nomenclature has been replaced by a trinomial, there is no reason why we should not have a quadrinomial nomenclature or even worse. It seems as if the Linnæan system of nomenclature is doomed. What will take its place can hardly be predicted. The new system should recognize the facts of place-modes and color varieties. We might establish certain categories of variation such as those of geographical regions, of habitat, of color. A decimal system of numbers might be applied to the parts of the country or the kinds of habitat, and the proper number might take the place of the varietal or subvarietal name. Thus the northeastern skunk might be designated *Mephitis mephitica* 74 and the southeastern skunk *Mephitis mephitica* 75 (adopting the Dewey system of numerals). The Maine skunk would then be 741 ; that of New York 747, and so on. This much for a suggestion.

So likewise for the morphologist the coming century will bring new aims and new methods. No longer will the construction of phylogenetic trees be the chief end of his studies, but a broad understanding of the form producing and the form maintaining processes. The morphologist will more and more consider experiment a legitimate method for him. The experimental method will, I take it, be extended especially to the details of cytology, and here cytology will make some of its greatest advances.

Not only will the old subjects be studied by new methods, but we have every reason to believe that new sub-sciences will arise during the twentieth century as they did during the nineteenth. Of course we cannot forecast all these unborn sciences, as cytology and neurology could hardly have been forecast at the beginning of the nineteenth century. But we can see the beginnings of what are doubtless to be distinct sciences. Thus comparative physiology is still in its infancy and is as yet hardly worthy of the name of a science; there is no question that this will develop in the coming decades. Animal behavior has long been treated in a desultory way, and many treatises on the subject are rather contributions to folk-lore than to science. But we are beginning to see a new era—an era of precise, critical and objective observation and record of the instincts and reactions of animals. One day we shall reach the stage of comparative studies, and shall have a science of the ontogeny of animal instincts. This will have the same importance for an interpretation of human behavior that comparative anatomy and embryology have for human structure.

Prominent among the advances of the century will be the ability to control biological processes. We shall know the factors that determine the rate of growth and the size of an animal, the direction and sequence of cell-divisions, the color, sex and details of form of a species. The direction of ontogeny and of phylogeny will be to a greater or less extent under our control.

The study of animals in relation to their environment, long the pastime of country gentlemen of leisure, will become a science. Some day we shall be able to say just what conditions an animal's presence at any place; and, more than that, we shall be able to account for the fauna—the sum total of animal life of any locality—and to trace the history of that fauna. This is at least one of the aims of animal ecology. It is a reproach to zoology that the subject of animal ecology should lag so far behind that of plant ecology. When zoologists fully awaken to a realization of what a fallow field lies here this reproach will quickly be wiped out. As it is, we have a notion that the factors determining the occurrence of an animal or of a fauna are too complicated to be unraveled. As a matter of fact, the factors are often quite simple. Let me illustrate this by some studies I have made this summer on the Cold Spring Beach. This beach is a spit of sand, 2,000 feet long and 50 to 75 feet broad, running from the western mainland into the harbor and ending in a point that is being made several feet a year through the cooperation of wave, tide and a silt-transporting creek of fresh water. On the outer harbor side is a broad, gradually sloping, sandy and gravelly beach, covered by high tide and devoid of living vegetation. Above that is a narrow zone—the middle beach—covered with débris of storms, supporting a few annual plants, and bounded above by a storm-cut bluff. Above is the upper beach, covered with a perennial, sand-loving vegetation. On the lower beach the zonal distribution of animals is striking. Just above the water are found the scavenger mud snails and, further up, a crowd of *Thysanura*—small insects that rise to the surface of the water when the tide comes in. These find a living on the finer débris or silt that settles on the pebbles during the high tides. In this zone also *Limulus* lays its eggs in the sand, and its nests are crowded with nematodes that feed on the eggs. During the breeding season scores of the female *Limulus* die here, and their carcasses determine a complex fauna. First, carrion beetles (*Necrophorus*) and the flesh fly live on the dead bodies; then the robber flies and tiger-beetles are here to feed on this fauna, and finally numerous swallows course back and forward gleaning from this rich field. At the upper edge of the lower beach is a band of débris dropped at slack water and consisting especially of shreds of *Ulva* and many drowned insects, chiefly beetles. At this zone, or just above under the drier but more abundant wreckage of the last storm, occur numerous Amphipoda of the genera *Orchestia* and *Talorchestia.* Associated with these marine creatures are numerous red ants, sand-colored spiders and rove-beetles. The amphipods feed on the decaying sea-weed. The ants are here looking chiefly for the drowned insects. Their nests are further up on the middle beach, but the workers travel to the edge of the high tide to bring away their booty. The rove-beetles are general scavengers. The spiders, which are mostly of the jumping sort (of the family *Attidæ*), feed on the active insects and amphipods. At a higher zone, and above all but the storm-driven tides, one finds the nests of the ants, especially under logs, certain predaceous beetles and the xerophilous grasshoppers and crickets. Finally, on the plant-covered upper beach one finds characteristic leaf-eating beetles, grasshoppers and carnivorous insects. Now all this seems commonplace enough and not especially instructive, and yet if you go to the shore of Lake Michigan you will find on a similar beach closely similar, if not identical, forms (excepting the beach fleas and the horseshoe crabs) you will find similar ants, spiders, rove-beetles, tiger beetles and sand-grasshoppers. This fact alone shows the greater importance of habitat over geographical region in determining the assemblage of animals that occurs in any one place. It may be predicted that studies on the relation of animals to their habitat will multiply, that they will become comparative and that the science of animal ecology will become recognized as no less worthy and no less scientific than the science of morphology.

Studies on the origin of species were far from being unknown in the nineteenth century, but they were for the most part fragmentary, or speculative, or narrow in view. The opinion that there was one method of evolution seemed to hold sway. It seems to me that the signs of the times indicate that we are about to enter upon a thorough, many-sided, inductive study of this great problem, and that there is a willingness to admit that evolution has advanced in many ways. The attempt, therefore, to explain all specific peculiarities on the ground of natural selection, or on the ground of self-adjustment, or on the ground of sport preservation through isolation, we may expect equally to prove futile. All these causes are no doubt real in some cases, but to exclude any one or to deny that new causes may be found in the future is equally dangerous and unscientific.

It is often said that the factors of evolution are inheritance and variation. In the new century careful and quantitative studies will be made on these factors. We shall get at quantitative expressions of the more complicated forms of heritage in the same way as Galton has given us an expression of a simple form of inheritance. We shall hope to understand why some qualities blend and others refuse to do so. We shall learn the laws of mingling of qualities in hybrids and get an explanation of the monstrosities and the sterility which accompany hybridization. What we call reversion and prepotency will acquire a cytological explanation, and it may be that the theory of fertilization will be seriously modified thereby. When we can predict the outcome of any new combination of germ plasms then, indeed, we shall have got at the laws of inheritance.

As for the other factor, that of variation, I anticipate interesting developments in our knowledge of its laws and of its causes. The methods by which this knowledge is to be acquired are doubtless comparative observation, experimentation and a quantitative study of results. Within the last decade a profound student of variation (Bateson) has declined to discuss its causes, holding that we had no certain knowledge of them. Even the categories of variation are still unenumerated. The science of variation is therefore one of those that we may hope to see established in this century. I feel convinced that statistical studies are first of all necessary to lay the foundations of the science.

As an illustration of an application of statistics to evolution studies I will give some account of my work during the past two years on the scallop of our east coast, *Pecten irradians*.

*Pecten irradians* is a bivalve mollusc of flattened, lenticular form, that inhabits our coast from Cape Cod southward. The Cape Cod limit is a rather sharp one, but southward our scallop passes gradually into the closely related forms of the South American coast. This fact would seem to indicate its southerly origin. To get light on the evolution of the group, I have studied and measured over 3,000 shells, chiefly from four localities: (1) Cold Spring Harbor, Long Island; (2) Morehead, North Carolina; (3) Tampa, Florida, and (4) the late Miocene or early Pliocene fossils of the Nansemond River. The fossil shells, to which I shall frequently refer, were found imbedded in the sand at Jack's Bank, one mile below Suffolk, Virginia. The bank rises to a height of 25 to 30 feet. Shells were obtained from three layers, respectively, one foot, six feet and 15 feet above the base of the bluff. Of course, the upper shells lived later than the lower ones and may fairly enough be assumed to be their direct descendants. The time interval between the upper and lower levels cannot be stated. As I have measured sufficient shells from the bottom and top layers only I shall consider them chiefly. I wished to get recent *Pectens* from this locality, but the nearest place where they occur in quantity is Morehead, North Carolina. These *Pectens* may therefore stand as the nearest recent descendants of the *Pectens* of the Nansemond River.

The *Pecten* shells have a characteristic appearance in each of the localities studied. After you have handled them for some time you can state in 95 per cent. of the cases the locality from which any random shell has come. First of all, the shells differ in color, especially of the lower valve. In the specimens from Cold Spring Harbor this is a dirty yellow; from Morehead, yellow to salmon; from Tampa white through clear yellow to bright salmon. Second, the antero-posterior diameter of the shell becomes relatively greater than the vertical diameter as you go north. Thus, the antero-posterior diameter exceeds, on the average, the dorsoventral diameter: at Tampa, by about 1.5 mm.; at Morehead, 2.5 mm.; and at Cold Spring Harbor, 6 mm. The fossil *Pectens* have an excess of about 4 mm.

Comparing the fossils with the *Pectens* of Morehead we find, as shown above, that the fossils are more elongated. Comparing the depth of the right valves having a height of 59 mm., we get:

| | |
|---|---|
| From the lowest level, Jack's Bank | 8.8 mm. |
| " " highest " " " | 9.1 mm. |
| " Morehead | 19.7 mm. |

Hence the recent shells are much more nearly spherical than the fossils; there is a phylogenetic tendency toward increased globosity.

The average number of rays in the different localities is as follows:

| | |
|---|---|
| Lower level, Jack's Bank | 22.6 |
| Middle " " " | 22.1 |
| Upper " " " | 21.7 |
| Morehead and Cold Spring Harbor | 10.3 |
| [Tampa | 20.5] |

Here it appears that there is a phylogenetic tendency toward a decrease in the number of rays of *Pecten irradians*. To summarize: The scallop is becoming, on the average, more globose, and the number of its rays is decreasing and its valves are probably becoming more exactly circular in outline. The foregoing examples illustrate the way in which quantitative studies of the individuals of a species can show the change in its average condition both at successive times and in different places.

But the quantitative method yields more than this. It is well known that if the condition of an organ is expressed quantitatively in a large number of individuals of a species the measurements or counts made will vary, *i. e.*, they will fall into a number of *classes*. The proportion of individuals falling into a class gives what is known as the 'frequency' of the class. Now it appears that in many cases the middle class has the greatest frequency (and is consequently called the mode) and as we depart from it the frequency gradually diminishes, and diminishes equally at equal distances above and below the mode. One can plot the distribution of frequencies by laying off the successive classes at equal intervals along a base line and drawing perpendiculars at these points proportional in length to the frequency. If the tops of these perpendiculars be connected by a line there is produced a 'frequency polygon.' The shape of the frequency polygon gives much biological information. When the polygon is symmetrical about the model ordinate we may conclude that no evolution is going on; that the species is at rest. But very often the polygon is more or less unsymmetrical or 'skew.' A skew polygon is characterized by this: that the polygon runs from the mode further on one side than on the other. This result may clearly be brought about by the addition of individuals to one side or their subtraction from the other side of the normal frequency polygon. The direction of skewness is toward the excess side. The skew frequency polygon indicates that the species is undergoing an evolutionary change. Moreover, the direction and degree of skewness may tell us something of the direction and rate of that change. There is one difficulty in interpretation, however, for a polygon that is skew may be so either from innate or from external causes. In the case of skewness by addition we may think that there is an innate tendency to produce variants of a particular sort, representing, let us say, the *atavistic* individuals. In this case skewness points to the past. The species is evolving *from* the direction of skewness. In the case of skewness by subtraction there are external causes annihilating some of the individuals lying at one side of the mode. Evolution is clearly occurring away from that side and *in* the direction of skewness.

Now so far as we know at the present time there is no way of distinguishing skew polygons due to atavism from such as are due to selective annihilation. But in many cases at least the skewness, especially when slight, can be shown to be due to atavism; and this is apparently the commoner cause. This conclusion is based first upon a study of races produced experimentally and whose ancestry is known, and secondly upon certain cases of compound curves. Take the case of the ray flowers of the common white daisy. A collection of such daisies gathered in the fields by DeVries gave a mode of 13 ray flowers with a positive skewness of 1.2. The 12- or 13-rayed wild plants were selected to breed from, and their descendants, while maintaining a mode at 13, had the increased positive skewness of 1.9. The descendants of the 12-rayed parents had a stronger leaning towards the high ancestral number of ray flowers than the original plants had. The 21-rayed plants were also used to breed from. Their descendants were above the ancestral condition as the descendants of the 12-rayed plants were below. The skewness — 0.13 is comparatively slight. In this case we have experimental evidence that polygons may be skew *toward* the original ancestral condition.

Of the compound polygons it is especially the bimodal polygon that frequently gives hint of two races arising out of one ancestral, intermediate condition. Consequently we should expect the two constituent polygons to be skew in opposite directions; and so we usually find them to be. For example, Bateson has measured the horns on the heads of 343 rhinoceros beetles and has got a bimodal polygon. The polygon with the lower mode has a skewness of + 0.48; that with the higher mode a skewness of — 0.03. One might infer that the right-hand form, the long-horned beetles, had diverged less than the short-horned from the ancestral condition. Again, as is well known, the chinch bug occurs in two forms—the long-winged and the short-winged. Now, in a forthcoming paper my pupil, Mr. Garber, will show that the frequency polygon of the short-winged form has a skewness of + 0.44, while that of the long-winged form has a skewness of — 0.43. On our fundamental hypothesis the ancestral condition must have been midway between the modes.

Still a third class of cases that gives evidence as to the significance of skewness is that where two place modes have moved in the same direction but in different degrees. Thus the index (breadth ÷ length) of the shell of *Littorina littorea*, the shore snail, as measured by Bumpus, has at Newport a mode of 90, at Casco Bay of 93. The skewness is positive in both places and greater (+ .24) at the more southern point than at Casco Bay (+ .13). This indicates that the ancestral races had a higher index even than those of Casco Bay, probably not far from 96, and also that the *Littorina littorea* of our coast came from the northward, since the northern shells are the rounder. We have historical evidence that they did come from the northward. Likewise the *Littorinas* from South Kincardineshire, Scotland, have a modal index of 88 and a skewness of + 0.065, while those of the Humber, with a mode of 91 have a skewness of + 0.048. These figures suggest that if the mode were 97 the skewness would be 0, and this would give practically the same value to the ancestral index as arrived at for the *Littorinas* of our coast. It will be seen from these illustrations that the form of the frequency polygon may be of use in determining phylogeny.

While skewness is thus often reminiscent, we must not forget the possibility that it may be, in certain cases, prophetic. This has come out rather strongly in a piece of work I have been engaged on during the past year. I have been counting the number of rays in recent *Pecten irradians* from various localities and have obtained in some cases evident skewness in the frequency polygons. To see what phylogenetic meaning, if any, this skewness has I sought to get a series of late fossils. After careful consideration I was led to go to the Nansemond River for the late Tertiary fossils found there and already referred to. These served my purpose admirably. We may now compare the average number of rays from the two extreme layers at Jack's Bank and at Morehead with the indices of skewness of the frequency polygons from the same localities.

| Place. | Avg. No. of Rays. | Index of Skewness. | $\sigma$ |
|---|---|---|---|
| Morehead, N. C. | 17.3 | —0.09 | 0.81 |
| Upper Layer, Jack's Bank. | 21.7 | —0.16 | 1.0 |
| Lower Layer, Jack's Bank. | 22.6 | —0.22 | 1.24 |

This series is instructive in that it tells us that the gradual reduction in the number of rays has been accompanied at each preceding stage by a negative skewness. This skewness was thus *prophetic* of what was to be. The skew condition of the frequency polygon we may attribute to a selection taking place at every stage, and the interesting result appears that the selection diminishes in intensity from the earliest stage onward. It is as though perfect adjustment were being acquired. If adjustment were being perfected we might expect a decrease in the *variability* in the rays at successive periods. And we do find such a decrease. This is indicated in the last column where $\sigma$ stands for the index of variability. From this column it appears that the variation in the number of rays has diminished from 1.24 rays in the Miocene to 0.81 rays in recent times. This fact again points to an approach to perfection and stability on the part of the rays. Just why or wherein the reduced number of rays is advantageous I shall not pretend to say. It is quite possible that it is not more advantageous, but that there is in the phylogeny of *Pecten irradians* an inherent tendency towards a reduction in the number of multiple parts. As a matter of fact there are other *Pectens* in which the number of rays is less even than in *irradians*.

The reduction in the variability of the rays with successive geological periods has another interest in view of the theory of Williams and of Rosa, according to which evolution and differentiation have of necessity been accompanied by a reduction in variability. Evolution consists, indeed, of a splitting off of the extremes of the range of variation, so that in place of species with a wide range of variability we have two or three species each with a slight range of variability. In the particular case in hand, however, it is not certain that the lower Jack's Bank form-unit (named *Pecten eboreus* by some one) has given rise to any other form than something of which *Pecten 'irradians'* of Morehead is a near representative. The evidence indicates that the reduced variability is solely the effect of the skewing factors.

The upshot of this whole investigation into the biological significance of skew variation is then this: Skewness is sometimes reminiscent and sometimes prophetic. In our present state of knowledge it is not possible by inspecting a single skew curve to say which of the two interpretations is correct in the given case. But by a comparison of the frequency curves of allied form-units the state of affairs can usually, as in the examples given, be inferred. A method of interpreting the single skew curve is a discovery for the future.

I realize that I have been bold, not to say rash, in this attempt to forecast the zoology of the twentieth century. I suppose, after all, I have merely expressed my personal ideals. Let those comfort themselves, therefore, who like my picture not and let them draw one more to their taste. These matters of detail are after all less important; but the general trend of the science I believe to be determined by the great general laws that will hold, whatever the detailed lines of development. First, students of the science will cling closer to inductive methods without abandoning deduction. Speculative web-spinning will be less common, will be less attractive, and will be more avoided by naturalists of repute. Great generalizations will be made, of course, but made with caution and founded at every step on facts. Second, the science will deal more with processes and less with static phenomena, more with causes and less with the accumulation of data. The time is coming when the naturalist who merely describes what he sees in his sections will have neither more nor less claim for consideration than he who describes a new variety of animal. It is relations, not facts, that count. Third, the science will become experimental, at least in so far as it deals with processes. Nothing will be taken for granted that can be experimentally tested. Better experimental laboratories will be founded and larger experimental stations, such as Bacon foresaw in the new world, will be established. Fourth, the science will become more quantitative. This is the inexorable law of scientific progress, at least where processes are concerned. I repeat that there is no reason to expect or desire the abandoning of the lines of work already recognized and followed for a half century or more. Rather holding fast to and extending the old lines of investigation, zoology will be enriched by new fields of study lying between and uniting the old. As chemistry and physics are uniting and occupying the intervening field, as geology and botany are coming close together in plant ecology, so will zoology and mathematics, zoology and geology, zoology and botany find untouched fields between them and common to them. Working in these new fields and by the aid of new methods, the naturalist of the future will penetrate further into the nature of processes and unravel their causes.

The zoology of the twentieth century will be what the zoologist of the twentieth century makes it. One hundred years ago the prerequisites of the naturalist were few and the opportunities of getting them were small. He must have studied with some master or have worked as an assistant under a naturalist in some museum. The places were few, the masters often difficult of approach. Now while, on the one hand, the training required in vastly more exacting, on the other hand, the opportunities are generous. Just because of the fact that zoology is spreading to and overlapping the adjacent sciences, the zoologist must have his training broadened and lengthened. A zoologist may well be expected to know the chief modern languages (let us hope this requirement may not be further extended), mathematics through analytics, laboratory methods in organic as well as inorganic chemistry, the use of the ordinary physical instruments, advanced geology and physiography, botany, especially in its ecological, physiological and cytological aspects, and animal paleontology. The list of prerequisites is appallingly long; zoologists of the future will be forced to an earlier and narrower specialization, while at the same time they must lay a broader foundation for it.

But if the prerequisites of the zoologists are to be numerous their acquisition will be easy. Even now scores of universities put the services of the best naturalists at the disposal of students and offer free tuition and living to come and study with them. Librarians, great museums, great teachers are made available to him who would work and has the requisite capacity.

All these advantages will, however, count for nothing if zoological research does not attract the best men, and if the best men be not accorded time and means for research. Our best students slip from our grasp to go into other professions or into commerce because we can offer them no outlook but teaching, administration, and a salary regulated by the law of supply and demand. We must urge without ceasing upon college trustees and corporations the necessity of freedom for research and liberal salaries if America is to contribute her share to the advance of zoology in the twentieth century.

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## SCIENTIFIC BOOKS.

*Leçons de physiologie expérimentale.* By M. RAPHAEL DUBOIS, professor in the University of Lyons, with the collaboration of M. EDMOND COUVREUR. Paris, Georges Carré et C. Naud. Pp. vi + 380.

These lessons in experimental physiology constitute a course of demonstrations, or lectures illustrated by demonstrations, given successfully by Professor Dubois and his pupil and collaborator, M. Couvreur, to the students in physiology of the faculty of sciences of the University of Lyons. As the authors state in the preface, they are now published with the view "of relieving the students attending the demonstrations from the necessity of taking notes, so that they may be able to devote to what they see the greatest amount of attention possible." In addition to viewing the demonstrations, the students are expected to repeat for themselves, under the direction of a master, all the classical experiments. For those who do not possess the advantage of expert supervision it is intended that the exercises described shall serve as a guide by the aid of which they may acquire a practical knowledge of physiology.

While it is encouraging to learn that something is being done to improve the teaching of practical physiology in the countries of continental Europe, where hitherto it has in general scarcely entered as a factor of any importance into the education of the student of science and particularly of medicine, we doubt whether there is a single teacher of experience in America or England who would bestow an unqualified approval upon the method adopted in this book. At the same time we can most heartily congratulate the young gentlemen (and ladies, if such there be,) of Lyons whom it releases from the bondage of the note-book and the pencil, and whose eyes and fingers (*facile princeps* in the armamentarium of physiology) it sets free for the practical study of this fascinating science.

Two well-established methods of imparting a practical knowledge of the subject are in vogue among us in schools of good standing; demonstrations by a teacher to small classes of students and practical exercises performed by the students themselves. Each of these methods has its uses, although for most purposes, and wherever the number of students is not unmanageably large, the second is by far the most satisfactory. The French lesson in experimental physiology, as typified in the Lyons course, is neither a demonstration pure and simple nor an exercise calculated to guide the student in individual practical work. It is rather a lecture on some portion of physiology, with a certain amount of actual demonstration or of talk about instruments and methods 'shoved into the belly of it.' Not full enough for systematic lectures, not precise enough in the practical directions, nor arranged with sufficient simplicity and order to be of much use as a laboratory guide, such hybrid disquisitions are neither likely, we fear, to thoroughly instruct the learner in the facts of the science nor to introduce him to a real knowledge of the methods by which the facts have been ascertained.

But when this has been said, criticism has exhausted its quiver. Faulty as is the plan of these lessons for the purposes of the elementary student, they are capable of being used with much advantage by teachers of practical physiology whom they will supply, in a somewhat