They are arranged in the order of the mean distances from the sun.

Name.	Period in years.	Date of last Perihelion passage.	Approximate date of next return.
Encke	3·303	1895, Feb. 4	1898, May 26
Tempel	5·211	1894, April 23	1899, July 10
Tempel-Swift ...	5·534	1891, Nov. 14	1897, May 28
Winnecke	5·818	1892, June 30	1898, April 25
Finlay	6·627	1893, July 12	1900, Feb. 26
D'Arrest	6·691	1890, Sept. 17	1897, May 27
Wolf	6·821	1891, Sept. 3	1898, June 30
Faye	7·566	1896, March 19	Now visible.

The mean distances of the comets from the sun range from 2·218 to 3·854, but the aphelion distances do not vary so greatly in proportion—a fact which suggests the controlling influence of Jupiter. It is remarkable that such a small number of regularly returning comets seem to be permanently attached to our system.

Effect of Spots on Sun's Diameter.—Observations of the sun's diameter, made in the latter half of last year by J. Sykora, of the Charkow Observatory, have led to a result which may be of considerable importance if established by further investigations (*Ast. Nach.*, No. 3330). The observations were made with a 6-inch refractor by projecting the image of the sun together with that of the micrometer wires. The diameter measured in the direction of the points of appearance or disappearance of spot groups was found in the great majority of cases to be greater than the diameters in neighbouring parts of the sun as measured on the same days. Some of the results are as follows, the first column giving the diameter in the direction of spot groups, and the other two showing adjacent diameters:

	m. s.	m. s.	m. s.
June 22 ...	2 8·62 ...	2 8·38 ...	2 7·97
July 5 ...	8·37 ...	8·04 ...	8·21
,, 12 ...	8·30 ...	8·27 ...	8·27
Sept. 5 ...	8·52 ...	8·25 ...	8·44
,, 9 ...	8·41 ...	8·29 ...	8·36

It is concluded that although the spots themselves may be depressions, they produce an elevation of the surface of the sun in the regions where they are formed.

THE SPECULATIVE METHOD IN ENTOMOLOGY.

The annual general meeting of the Entomological Society of London was held on January 15, the President, Prof. R. Meldola, F.R.S., being in the chair. After referring to the affairs of the Society and to the great literary activity of English entomologists during the past year, the President called attention to Mr. Oswald Latter's discovery of the secretion of potassium hydroxide by *Dicranura vinula*, &c., and to Mr. F. Gowland Hopkins's researches on the pigments of Pierine butterflies. The address then proceeded as follows:—

The association of chemistry and biology in researches such as those to which I have drawn attention, has suggested a comparison between the methods of research in vogue in the two great departments of science of which these two subjects are respectively typical. All science necessarily begins with observation or experiment, *i.e.* with ascertained facts, and it is perhaps unnecessary to assert that no mere collection of facts can constitute a science. We begin to be scientific when we compare and coördinate our facts with a view to arriving at generalisations on which to base hypotheses or to make guesses at the principles underlying the facts. Having formed the hypothesis we then proceed to test its accuracy by seeing how far it enables us to explain or to discover new facts, and if it fails to do this to our satisfaction we conclude that our guess has been a bad one and requires modification or replacing by a better one, *i.e.* by one more in harmony with the facts. I take it that the course of progress is the same in so far as these fundamental methods are concerned in both departments of science, the physical and the biological. It is possibly a matter of individual opinion as to how large a body of facts should be accumulated before we attempt to draw any general conclusions. There can be no doubt that the requirements of one branch of science cannot be measured by those of another branch to which it has no near relationship. But however large the number of facts, and however cautious or conservative the worker may be, it is an established doctrine taught by the whole history of science, that real progress only begins when we go to seek for facts armed with at least the suggestion of a principle if not with a complete theory based on facts already accumulated by observation or experiment. This is the whole difference between scientific observation or experiment and mere random or haphazard observation. A naturalist of the old school, William Swainson, writing in 1834,[1] speaks of the "observance of nature, without making any attempt to generalise the facts so acquired," as "a mere amusement, fascinating indeed, and even useful, but totally disconnected with the objects of philosophic science." Now I venture to think that entomology in this country has been retarded in its development for want of a little more of this "philosophic science"; by an unwillingness on the part of our most active workers to give rein to the imagination—by an overcautiousness which is damping to the speculative faculty. There are no doubt many present who will not agree with this view, but I claim indulgence while I state my case in its support. It will, I think, be conceded that we have passed beyond the mere fact-collecting stage. It appears to me that in entomology we have arrived at a state where we are suffering from a plethora of facts; if we are not in a position to explain everything connected with the development, life-histories, instincts, classification and distribution of insects as a class of animals, we are at any rate in a position, speaking paradoxically, to know what we want to know, and I do not see how we are going to advance unless a more generous use is made of hypothesis as a scientific guide. It is this point which I desire to urge and to show that there is no real danger in boldly facing what the late Dr. Romanes aptly calls the bugbear speculation.

In the first place, with respect to the physical sciences, there is abundant justification for the view which I am advocating. We have there long ceased to collect random facts; observations and experiments are suggested by hypothesis. That prince among experimental philosophers, Michael Faraday, was wont to say: "Let us encourage ourselves by a little more imagination prior to experiment." The state of affairs is well summed up in one of the latest works on chemistry in which the author, in introducing the fundamental principles of modern investigation says:

"The history of the exact sciences teaches us that we may discover new laws of nature in two essentially different ways, one of which may be designated as the empirical, the other as the theoretical. Thus in one way by suitable observations, one collects abundant material . . . and then by a repeated and purely empirical grouping of the data so obtained, he seeks to approach the desired goal. . . . The second way, on the other hand, leads from suggested conceptions regarding the nature of certain phenomena, through pure speculation to new information, the correctness of which must be determined by a subsequent research."[2] One other recent utterance by my colleague, Dr. W. M. Hicks, the President of Section A at the last Ipswich meeting of the British Association, will serve to give us a glimpse into the spirit of progress in pure physics: "By our imagination, experience, intuition, we form theories; we deduce the consequences of these theories on phenomena which come within the range of our senses, and reject or modify and try again. It is a slow and laborious process. The wreckage of rejected theories is appalling; but a knowledge of what actually goes on behind what we can see or feel is surely, if slowly, being attained. It is the rejected theories which have been the necessary steps towards formulating others nearer the truth."[3]

And now let us consider how far these methods, recognised as valid in the physical sciences, are applicable to the biological sciences, of which entomology constitutes a branch. Of course, I am not claiming for our subject the position of an exact science, and to suppose that it could be advanced by purely deductive methods would be absurd. But I am endeavouring to hold the balance between a more liberal use of the speculative method, on the one hand, and the deadening influence of refusing to speculate at all, on the other hand. I am putting forward a plea for an increased use of the imagination, because I hold that

1 'Preliminary Discourse on the Study of Natural Science," p. 51.
2 "Theoretical Chemistry," by Walter Nernst, translation by Prof. Palmer, 1895, p. 2.
3 Address to the Mathematical and Physical Section of the British Association, Ipswich 1895.

the time has arrived when this may—nay, must be allowed, if our science, with its immense wealth of raw material, is to take that rank to which it is entitled among the departments of modern biology. If, as is undoubtedly the case, the speculative method has been found fruitful in other fields of natural history, it behoves us as co-workers in the great battle for truth to re-examine our weapons—to ask ourselves seriously whether the time and energy of our most active workers is being utilised in the best way for the advancement of knowledge.

To many it may appear that the use of hypothesis as a guide to investigation is so obvious, that no special advocacy is required. All I have to say, in this case, is to express the earnest wish that the Fellows of this Society who hold such a view may be numerous—the more numerous the better. I will venture to remind you, however, that my predecessor in this chair has stated, with respect to this method of handling entomological problems:

"I feel, however, for myself, and I think that others must also feel, that however great and important is the knowledge which we may ultimately attain, by endeavouring to discover the laws which govern the development, variation, and distribution of insects, the knowledge we have of the actual facts is in many cases quite insufficient to bring such speculations to a definite end. I also feel that the number of persons whose talents are sufficiently great to enable them to steer a straight course through the numerous difficulties, contradictions, and doubts which constantly surround such inquiries is very limited" (*Proc. Ent. Soc.* 1893, p. xlvi.).

I am sure Mr. Elwes will not ascribe any personal motive to me in making use of this passage, as representing the views of what may be called the conservative school of entomologists. I feel only too acutely the truth of his remark that many agree with him in this opinion; at the same time I am sanguine enough to believe that there are many who do not, and on behalf of this constituency I have felt it a duty to urge a claim for the speculative method, not as displacing the older method of collecting and recording facts altogether, but as a stimulus to more systematic investigation, rendered imperative by the general advance of biological science. For my own part, I believe that the time has gone by when every attempt at discovering natural law in the organic world by the aid of entomological observations, is to be met by this prevalent cry of *non possumus*.

If we turn to results as a measure of the value of methods, it will, I imagine, be conceded that we can show good cause in favour of theorising. I may be permitted to draw some illustrations from the Lepidoptera, the only order to which I can lay claim to some slight special knowlege, and in which our former President is a recognised authority. In the following remarks I desire most emphatically to dissociate myself from controversial matters, because my sole aim in this address is to clear the atmosphere for the more healthy use of the speculative faculty by our younger and rising workers. I wish it to be understood that in speaking of any particular hypothesis, I am not now raising the question of its soundness or unsoundness—that is, logically, a distinct issue—but I am simply adducing the hypothesis in order to illustrate the results of its introduction into modern scientific thought. I begin with the phenomena of mimicry and protective resemblance among butterflies and moths as first explained by our late distinguished Fellow and past President, Henry Walter Bates, in his memorable paper of 1861, which was followed by the well-known memoirs of Wallace and Trimen on the same subject. It will be remembered by all who are familiar with the history of the subject, that this was the first application of the theory of natural selection of Darwin and Wallace to explain a new set of phenomena. It was a speculation evolved by Bates, not when collecting in the Amazon Valley, as is generally supposed, but while looking over his specimens when he had reached London, and was pondering, at his own fireside, over the meaning of the remarkable superficial resemblances among the butterflies of different groups which he had brought home.[1]

The Batesian theory was fruitful; it carried with it the explanation of the resemblance between insects of distinct orders and of the assimilation of insects and other animals in colour and form to the objects among which they lived; it prompted further observation and experiment because more evidence was required as to the protected character of the insects which were copied; it raised the whole question of the existence of such protected species in nature, and the question has been answered so far in the affirmative, although there is still a large field for further experimental observation waiting to be explored. The facts have increased enormously since 1861, the search for new instances having been stimulated by the explanation suggested by Bates, and the systematist is now no longer in danger of being deceived by superficial resemblances.

[1] I owe this statement to Mr. Bates himself, who has often made it to me.

The theory of Bates left unexplained the resemblance between species belonging to protected groups to which he had himself called attention in his original paper; an extension was required and was made by our Hon. Fellow, Fritz Müller in 1879, and as a result, whether this extension be considered valid or not—a point which I am not now raising—the systematist is now more fully alive to the superposition of external similarity upon structural resemblance due to true blood-relationship, as can be seen from the writings of Moore on the genus *Euplœa*, and of Wood-Mason and others on certain Papilionidæ. As another result of Fritz Müller's hypothesis, the question of inherited knowledge of edible and inedible species on the part of insect-eating creatures has likewise been raised, and has already led in the hands of Prof. Lloyd Morgan to some interesting experimental conclusions.

As the product of a theory we thus have a large body of real and tangible knowledge gleaned from nature! Mere casual observation would never have revealed the widespread existence of the phenomenon if the stimulus to look out for it had not come from the theoretical side.

It is not the bare record of the comparatively few cases of mimicry that constitutes the highest value of these classical memoirs—it is the speculation, the hypothesis, the suggested *cause* of the phenomenon that has given vitality to what would otherwise have been a disconnected and meaningless set of facts. But the consequences of the introduction of the theory of natural selection into the subject of insect colouration have not yet been exhausted. From the observation that the species which are mimicked are generally gaudily coloured and take no special means to hide themselves, it is but a step to the well-known theory of warning colours propounded by Wallace in 1867. That theory, in itself the outcome of a question raised by Darwin in connection with his theory of sexual selection, stimulated the experiments of the late Jenner Weir and of A. G. Butler, the striking observations of Thomas Belt in Nicaragua, the detailed researches of Weismann into the origin and meaning of the colours of caterpillars, and the later systematic series of experiments conducted by Poulton. Yet another example I will permit myself to make use of because it is one in which I have some personal interest. In considering the subject of adaptive colouration as explained by Bates and Wallace, a difficulty occurred in the case of species which are of variable colouring: I ventured to suggest, as far back as 1873, that this kind of colouring would be explicable by natural selection, if we supposed that this agency could confer a power of adaptability on the individual. At that time no mechanism could be conceived of by which such individual adaptability could be acquired, excepting the direct assimilation of the colouring-matter of food-plants in the case of caterpillars or other vegetable feeders. This, of course, carried with it the implication that natural selection could work on physiological processes if they were of use, just as well as upon any external morphological character. Stimulated by this hypothesis, other cases of variable colouring were sought for and found. The subject was later taken up by Prof. Poulton, who, for many years, conducted experiments and obtained results which are now familiar to all naturalists. The original speculation, that variable colouring was the result of an individual adaptability due to natural selection, implies that this faculty is of bionomic value. I am not now concerned with the validity or otherwise of this assumption; that is an issue on which opinion appears to be divided; although, I have no doubt in my own mind on the point, it is not necessary to state the case with any bias on the present occasion. Now the experiments of Poulton have shown that this colour variability is of very much more frequent occurrence than was ever dreamt of in 1873, and his facts have, in the main, been substantiated by the independent observations of many other experimenters. And it turns out also that the mechanism of the process is not even the simple assimilation of colouring-matter from the food-plant, excepting in the case of green caterpillars, in which it has been shown that chlorophyll in a modified form passes into the blood. The colour variability of caterpillars and pupæ in response to the external stimulus exerted by coloured surfaces, as established by these experiments, has brought us face to face with a fundamental problem in insect physiology, the solution of which we are anxiously awaiting. The mere possibility of being able to state the problem in its present form—apart from any question of the adaptive value of the colouration—is a step forwards; is an incentive to further experiment, and this is the legitimate end and aim of all scientific speculation.

Were I to attempt, however, to pass from what has already been accomplished to that which is yet awaiting investigation—to the questions which rise on all sides as pressing for solution, there would be no limit to this address. In view of the splendid opportunities afforded by insects for treatment as living organisms capable of revealing natural laws by skilled experimental research, is it not pardonable if we sometimes give way to the unphilosophic thought that the possession of chitinous exoskeletons by these creatures, whereby they lend themselves so admirably for preservation as cabinet specimens, is an arrangement expressly designed for the retardation of entomological science? The scientific workers at living insects in this country are deplorably few as compared with those who devote themselves to cabinet entomology. The one great desideratum of modern biology is an experiment station where protracted observations can be carried on year after year on living animals, each set of experiments prompted by hypothesis and with the definite object of answering some particular question in relation to variability and inheritance, the nature of the action of the environment, the effect of selection, &c. This was a dream of the late Dr. Romanes; he has not lived to see it fulfilled, but if it should be realised in our time our entomologists will, I venture to hope, not be behind with suggested lines of work.

If by way of comparison we now turn to that branch of the subject in which the empirical method has hitherto almost exclusively been employed, viz. the taxonomy of this same order Lepidoptera, the results are most instructive. In view of the immense body of facts, the number of named species and the mass of published descriptive matter, I do not think I shall be wrong if I say that the best energies of the acutest workers have been concentrated on this subject from the middle of the last century down to the present time. A record of nearly a century and a half against the thirty odd years that have elapsed since the introduction of the theoretical method into the biological sciences. Is there any indication that all this work has brought us nearer the "definite end" to which it was and is directed—the natural classification of the Lepidoptera—to an extent commensurate with the number of workers and the time bestowed upon it? It is only quite recently that any decided advance has been made, and that through the work of Hampson, Comstock, Chapman, Meyrick, and others. It cannot be said that we have been waiting all these years for materials—for a few thousand new species is one of the best "collected" groups in the whole world of insects—in order that this sudden rush might be made. I take the view that we have been waiting rather for method than for additions to the lists of species; that we have hitherto too much disregarded the spirit of the speculative method in our taxonomic work, and that we have now happily found a band of workers who refuse to submit to the plea of inability because all the existing species of Lepidoptera have not been collected and named.

After advancing these arguments in favour of a more liberal use of the "scientific imagination" in connection with entomological subjects, I feel it incumbent upon me to define the position a little more fully in order to prevent misunderstanding. The conditions of speculation in the two great departments of natural science which have been under consideration are not exactly the same, and the differences in the method of treatment must not be lost sight of. If in the physical sciences there is, to use the expression of the late Prof. Stanley Jevons, "unbounded license of theorising," it is because we can appeal to nature so readily by the experimental method, and get our answer one way or the other, by imposing rigid conditions which are under our control. In the biological sciences this is not the case; all who are acquainted with experimental work in biology know how difficult it is, generally, to get definite answers to our questions—the conditions are vastly more complex when we come to deal with living organisms. I remember once remarking to the late Mr. Darwin how difficult it was to get nature to give a definite answer to a simple question, and he replied, with a flash of humour: "She will tell you a direct lie if she can." The practical result of this difference is that the speculation of an hour may take a lifetime for its verification. But I see no reason why, on these grounds, we should repress the spirit of speculation. If, as our former President says, it is given to few to be able to speculate with advantage—and in this I thoroughly agree with him—it is our paramount duty for the present and future welfare of our science, to give every man's honest thought our most serious attention, and to encourage the faculty whenever and wherever we find it, as the most precious means of advancing scientific knowledge. The "bugbear" is a very harmless animal if you look him boldly in the face, and if you treat him gently and put him into harness he will drive the chariot of science for you at a speed that will leave the empirical method far behind in the race for the knowledge of nature's ways.

The great service which the founders of the modern doctrine of evolution have rendered to science has, in my belief, been not only the particular theory of species transformation with which their names will ever be associated, but the importation into biology of the methods of the physical sciences. Writing to Wallace, in 1857, Darwin said: "I am a firm believer that without speculation there is no good and original observation" ("Life and Letters," vol. ii. p. 108). In the same letter he remarks: "You say that you have been somewhat surprised at no notice having been taken of your paper in the *Annals*. ["On the Law that has regulated the Introduction of New Species," *Ann. and Mag. Nat. Hist.*, 1855.] I cannot say that I am, for so very few naturalists care for anything beyond the mere descriptions of species." This statement of 1857 does not hold good in 1896; other methods of biological research have been introduced—the road to biological fame is no longer through the sole channel of technical systematic work, and we owe it to the writer of that letter more than to any other worker and thinker of our time, that the horizon has been extended on all sides.

The misapprehension to which my remarks may possibly give rise, and which I am most anxious to prevent, is that in urging the claim of the theoretical method I am introducing the danger of rash and promiscuous speculating by all kinds of dabblers in the subject. There is much justification for this attitude, but an analysis of the supposed danger will, I think, serve to show that it is not a very formidable one after all. It appears to me, moreover, that the advantage of giving an impetus to observation along preconceived lines far outweighs any passing danger arising from hasty speculation. It is notorious in the history of modern science that no single branch has escaped the efforts of well-intentioned, but quite irresponsible outsiders, to set our various houses in order for us. On critical examination it will be found, however, that none of these attempts, even when they have been lucky enough to forestall the conclusions arrived at by legitimate methods, have led to any practical issue in the way of observation or research. I am addressing my remarks on the present occasion to a Society composed more or less of experts; I am not inviting "the man in the street" to favour us with his views on this, that, or the other question, but I am asking the working entomologists among us to bear in mind that their studies may be directed so as to throw light on some of the broad biological problems of the day, if they will, as Faraday said, encourage themselves by a little more speculation. Judging from the part played by this method in the development of modern science, it is perhaps not going too far to say that it is better to have speculated erroneously than never to have speculated at all. Illustrations might be adduced showing that erroneous theories have often done good service to science, and that for this reason they have been temporarily retained, even when recognised as inadequate to meet the growing body of new evidence. This was the case, for example, with the old "fluid" theory of electricity. So also the "corpuscular" theory of light enabled Newton to develop optical science to a remarkable extent, although this theory is now among what Dr. Hicks calls the "wreckage."

Another source of danger in biological speculation to which I am also alive, is that we have the public eye upon us to an extent that is not experienced in other departments of science. I am bound to confess that I never could quite make out why this should be the case. It is possible to speculate about the constitution of matter, the degradation of energy, the age of the solar system, and other great problems of the universe, with any degree of dogmatism without exciting public discussion. But as soon as ever an effort is made to explain something in the living world, no matter how modestly, the speculator is forthwith treated as though he had thrown down a public challenge. Perhaps it is for this reason that biology is more subject to unauthorised and unscientific intrusion; because it gives opportunity for the pure *littérateur* to pose as a theorist. The speculations of the physicist or chemist are, moreover, generally expressed in a symbolical language which is not understood by the public at large, and their ideas, however revolutionary, thus escape newspaper and magazine notoriety. As far as my reading extends, I am inclined to believe that even in the case of the purely literary treatment of biological problems by writers who are not experts, the danger of overweighting the science with hypothesis is much exaggerated. Writers of this class are often capable of taking a wider and more philosophic grasp of a problem than a pure specialist, and ideas of lasting value have sometimes emanated from such sources. I imagine that nobody will dispute that Mr. Herbert Spencer's writings have largely influenced the public mind—whether we agree with the details of his doctrines or not—in accepting the broad principle of evolution, although this profound thinker lays no claim to an expert knowledge of any branch of natural history. But every working naturalist can ascertain for himself the credentials of any particular writer: my remarks are simply offered with the object of claiming more consideration for such writers, as a class, on the part of practical workers. The philosophic faculty is quite as powerful an agent in the advancement of science as the gift of acquiring new knowledge by observation and experiment. It is not often that the faculties are combined in one individual.

The general conclusion to which these considerations point is that the biological theorist, by virtue of the complexity of the factors, the difficulty of experimental verification, and the tendency on the part of the public to mistake tentative hypotheses for established theories, should put forward his views with more explicit caution than is necessary in the case of the physical sciences, where experimental evidence is more easily obtainable, and where the self-constituted philosopher but rarely gets a hearing. All this amounts, however, to nothing more than a plea for caution, and not for total abstinence. To disallow speculation because a complete theory cannot be formed out of the existing materials, is simply to put a check upon legitimate advancement. I freely admit that it is possible to carry speculation to an unscientific extreme—to fritter away a plausible hypothesis by mere metaphysical discussion, or to bury a real and important issue under an incubus of verbiage. But this is not the legitimate use of the speculative method; it is an accident, which the scientific worker will know how to avoid, and which is contingent upon the present condition of biological investigation. We cannot test our speculations off-hand by a few crucial experiments, as in physical science, and in the meantime the logic-chopper may get hold of our idea and whittle it away. On these grounds, however, I again fail to see any reason for repressing speculation. It might as well be argued that because the action of fire, carried to an extreme, carbonises organic matter, we should therefore eat our food raw. The irresponsible manipulation of biological hypotheses by pure speculators does no real or permanent injury to the cause of science, and may indirectly do good by directing public attention to the work which is being carried on. I rather think the absence of public sympathy, in connection with theoretical research in chemistry and physics, exerts a depressing influence; the inventor of a new hypothesis in these subjects moves entirely in an atmosphere of his own creation, which even his colleagues seldom venture to penetrate. That biological speculations are more prone to such unauthorised treatment is no more a reason for refusing to speculate than the circumstance that generations of fact-collectors have wasted their time in amassing large stores of disconnected observations, which for want of system are practically of no avail to the scientific worker, is an argument in favour of repressing observation. It is possible to be quite as unscientific in the accumulation of facts as it is to become metaphysical by over-speculation; there is as much danger in one direction as there is in the other. Yet the most ardent advocate of the theoretical method has not taken it upon himself to declare that observation must cease until he has explained all the facts at present available. This, however, is practically the position taken up by those who refuse to recognise that existing knowledge is sufficient to enable considerable advance to be made by the legitimate use of the theoretical method.

One other point demands consideration, in conclusion. If latitude for the exercise of speculation is to be allowed, where, it may be asked, is the line to be drawn? How are we to distinguish between the cautious theoriser and the writer who permits himself "unbounded license?" These are questions to answer which requires nothing but an exercise of individual judgment. A sound speculation may emanate from the happy possessor of a philosophic mind although he may never have done any technical biological work. But this kind of speculator naturally fails to secure that hearing to which the practical worker is entitled. Although valuable generalisations may occasionally be given out by great thinkers, the expert biologist shows wisdom in giving his most serious attention only to those who are familiar with their data at first hand—who have themselves gleaned their information directly from nature. By such workers only can the true value of the evidence be fairly weighed and estimated. I should be very sorry if the remarks which I have ventured to offer in the course of this address were to be interpreted into a general public invitation to speculate on biological problems. But I do raise the question here as to the kind of biological work which is to be recognised as a fitting preparation for the exercise of the speculative faculty. It used formerly to be asserted that he only is worthy of attention who has done systematic, *i.e.* taxonomic, work. I do not know whether this view is still entertained by entomologists; if so, I feel bound to express my dissent. It has been pointed out that the great theorisers have all done such work—that Darwin monographed the Cirripedia, and Huxley the oceanic Hydrozoa, and it has been said that Wallace's and Bates's contributions in this field have been their biological salvation. I yield to nobody in my recognition of the value and importance of taxonomic work, but the possibilities of biological investigation have developed to such an extent since Darwin's time that I do not think this position can any longer be seriously maintained. It must be borne in mind that the illustrious author of the "Origin of Species" had none of the opportunities for systematic training in biology which any student can now avail himself of. To him the monographing of the Cirripedia was, as Huxley states in a communication to Francis Darwin, "a piece of critical self-discipline,"[1] and there can be no reasonable doubt that this value of systematic work will be generally conceded. That this kind of work gives the sole right to speculate at the present time is, however, quite another point. It might be argued with some show of reason that exclusive devotion to systematic work cripples the imaginative faculty.[2] The methods of attacking the problems connected with living organisms have been increased and improved from every side, and the anatomist and physiologist, the morphologist, the embryologist, the student of bionomics, have all an equal claim to contribute to biological theory. The particular problems relating to the transformation of species are no doubt best dealt with by those who, by systematic work, have acquired a true notion of what is meant by the term "species." But so far as entomology is concerned, it must be confessed that the greater part of our systematic work has emanated from cabinet entomologists, who know nothing of the species they describe as living organisms by direct observation, and to me it appears doubtful whether this kind of work does confer any special faculty of speculating with advantage on the species question. It seems rather that the "field-naturalist" in the old sense of the term has the advantage, and I may remind you in this connection that during the voyage of the *Beagle*, when Darwin began to make those observations on island life which afterwards led him to take up the question of species transformation, he was essentially a "field-naturalist," his systematic work on the Cirripedia not having been commenced till after his return. So also Wallace, at the time when he independently elaborated the theory of natural selection, was certainly not a systematist in this narrow sense. He has been good enough to favour me with his views on this point, in a letter dated December 31, 1895, in which he says: "I do not think species-describing is of any special use to the philosophical generaliser, but I do think the collecting, naming, and classifying some extensive group of organisms is of great use, is, in fact, almost essential to any thorough grasp of the whole subject of the evolution of species through variation and natural selection. I had described nothing when I wrote my papers on variation, &c. (except a few fishes and palms from the Amazon), but I had collected and made out species very largely, and had seen to

[1] "Life and Letters," vol. i. p. 348. Even in the days of my studentship, Huxley lectured on Natural History at the Royal School of Mines with the aid of diagrams and specimens only: practical work in the laboratory was unknown.

[2] See a letter from Darwin to Bates in 1861, "Life and Letters," vol. ii. p. 379.

some extent how curiously useful and protective their forms and colours often were, and all this was of great use to me."

I had hoped to be able to discuss some of the current problems which are before biologists, and towards the solution of which entomology might contribute largely. Such, for example, are Galton's and Weismann's views on the non-transmissibility of acquired characters, the *rôle* of what Mr. Bateson calls "discontinuous variation" in the origin of species, the recent efforts to throw light on the all-important subject of variability by the statistical methods introduced by Galton and now being worked at from the experimental side by Weldon, and from the mathematical side by Karl Pearson. I feel, however, that I have trespassed already too long upon your forbearance, and while again thanking you for the honourable position in which you have placed me, I can only express the hope that my special plea for a more liberal use of the speculative method among our working entomologists will not be regarded by those who hold different views as a breach of the privilege of that office to which by your courtesy I have been elected. Should there be any who entertain this opinion, I beg them to make a liberal discount for personality, and they will find that the ultimate motive has been to promote the best interests of our science.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

OXFORD.—The Waynflete Professor of Mineralogy (Mr. H. A. Miers), who has been absent during the early part of the term through illness, announces a course of lectures on Elementary Crystallography.

In a Congregation held last week, the proposal that a sum not exceeding £150 per annum for three years from October 1, 1894, should be applied out of the Common University Fund in maintaining a scholarship to be held by a student at the Marine Biological Station at Naples, was agreed to, *nemine contradicente*.

In a Congregation to be held on February 18, a form of statute amending the provisions of a statute made for the administration of the Lichfield Trust for Clinical instruction, will be proposed. The object of the statute is to provide for the conduct of the Pathological Department at the Radcliffe Infirmary by the Regius Professor of Medicine, or a person appointed by him, and for giving instruction in Pathology in accordance with the Regulations of the Board of the Faculty of Medicine.

The interest of the University is at present absorbed in the resolutions respecting the admission of women to the degree of B.A., which are to be submitted to Congregation on March 3. The first resolution, which proposes that women shall, under certain conditions, be admitted to the degree of B.A., will if carried tend to promote the study of every subject by women in Oxford, and therefore has an ultimate bearing on scientific studies. At the same time it will compel women to go through Responsions and the other examinations from which they are now exempt. There are some who think that this will be injurious to their interests. The most that can be said in the case of those who wish to read Natural Science is, that it will compel them to learn Latin and Greek either before they come to Oxford, or after they have come up. If the latter, they will find themselves obliged to keep four years residence, which most do not as things now are. If they know enough Latin and Greek to pass Responsions before coming into residence, their case will not be altered, for a woman competing for honours in one of the final subjects in the Honour School of Natural Science always passes the preliminary examinations required by the statutes in the case of men. It is not proposed that the strict B.A. course should be obligatory on all women students. Those who do not wish totake up Latin and Greek, but wish to read Natural Science or another subject such as History, will be allowed to do so under existing regulations, and so may escape Responsions; but they will also have to forego the distinction of the degree.

CAMBRIDGE.—St. John's College has made arrangements for the admission of post-graduate students desiring to pursue a course of advanced study or research under the new regulations of the University. Until the statutes now before the Privy Council are approved, candidates for admission are required to present a letter of recommendation from the Professor or other teacher under whom they propose to work in Cambridge. Further particulars may be learned on application to one of the tutors of the College.

Mr. J. N. Langley, F.R.S., Lecturer in Physiology, has been approved for the degree of Doctor of Science.

T.R.H. the Grand Duke of Hesse and Prince Henry of Prussia have presented to the Museum of Zoology the skeleton of a wild boar.

The following have been appointed Electors to the undermentioned professorships: Chemistry, Prof. Thomson; Plumian (Astronomy), Sir G. G. Stokes; Anatomy, Downing (Medicine), Surgery, and Pathology, Prof. Foster; Botany and Physiology, Prof. Allbutt; Geology, Dr. Phear; Mineralogy, Prof. Liveing; Zoology, Mr. J. W. Clark; Experimental Physics, Prof. Clifton; Mechanism, Mr. Horace Darwin.

A MEETING was held at Cardiff last week to start a public subscription in aid of the erection of new buildings for the University College of South Wales. A sum of £20,000 is required to meet the conditional grants made by the Treasury and the Drapers' Company. Contributions amounting to £13,000 were promised at the meeting. Lord Windsor, who presided, will contribute £2500, and a substantial sum has also been promised by Lord Tredegar. It is expected that £30,000 will be raised. Mr. Alfred Thomas, M.P., contributes £1000, and Mr. John Cory a like amount.

Science announces the following gifts to education in America: The University of Pennsylvania has received a gift of 5000 dols. from Mr. Charles M. Swain, and of 5000 dols. anonymously, the money to be used without restrictions. The will of the late Martin Brimmer, of Boston, to take effect on the death of his wife, bequeaths 50,000 dols. to Harvard University. Ground has been broken for the first of the four buildings of the new biological school of the University of Chicago, which is to be erected with part of the 1,000,000 dols. recently given by Miss Culver. It is proposed to erect special buildings for zoology, botany, anatomy, and physiology, instead of one biological building, as planned before the receipt of Miss Culver's gift.

PRINCIPALS of Technical Schools and others who assist in deciding the character of instruction in chemistry, would do well to take to mind the lesson contained in the following extract, referring to the work of the Chemical Department, from the programme just received from the Central Technical College: "The object aimed at in this part [first year] of the course will be to encourage habits of accuracy and thoughtfulness, and to teach the art of experimenting with a logical purpose rather than to impress mere facts. . . . As soon as students have acquired the necessary proficiency as analysts and sufficient skill in preparing pure substances, they will be encouraged to undertake an original investigation, in order that they may learn to apply their knowledge, as well as develop their powers of observation and reasoning: and thus become fitted to solve problems which are continually presenting themselves in practice, and to improve and advance the industry with which they may be connected. The importance to students of thus devoting themselves, sooner or later, to the higher branches of chemistry cannot be too strongly insisted on; in no other way is it possible for them to acquire the breadth of view and the power of grappling with new problems, as they arise in practice, which are required of the technical chemist."

DR. H. E. ARMSTRONG has been for some time trying to instil a little scientific spirit into the School Board for London. In an address recently delivered at the Borough Polytechnic Institute, and printed in full in the *Technical World*, he described the excellent results attained by the introduction of the scheme of instruction in scientific method, drawn up by a Committee of the British Association. The Board has every reason to be proud of what its science demonstrators have done to promote the reformed methods of science instruction, of which Dr. Armstrong is the most active exponent. The methods have been proved to be practicable, and the results obtained by following them are most satisfactory. It remains for the School Board to recognise this by extending to all its schools in the metropolis (girls' as well as boys' schools) the teaching which has been so successfully carried on in one of its districts. If that were done, a great advance in education would be assured. Those who are engaged in the work of technical education are agreed, as Dr. Armstrong pointed out, that it is all but impossible at the present time to give true technical education in this