wholly solved, but which must and will be solved before the American university will become what it can become. Of course, this absence of inner freedom of action is often due to the primitive condition of many of our universities or to the fact that many of them are in the transition stage from college to university, and will disappear as these institutions more closely approach the university ideal. But whatever may be the causes and excuses for these conditions, the truth is there is more 'paternalism' in the universities of this 'free' country than in those of military Germany. There are dangers connected with freedom, very true, but these dangers cannot be avoided and are the price we must pay for the blessings of liberty.

Another element of strength of the German university, one that could not develop without the factor just mentioned, and without which the university could never have reached its present status, is the spirit of investigation among its members. The German professor is, above everything else, a scientific investigator. This phase of development also has its shadow sides and dangers, as Professor Paulsen shows. But it is true, nevertheless, as he says, that the position which the German people at present holds in the scientific world, it owes in the main to its universities, and these owe what they are and what they accomplish to the principle on which they are based: they are scientific institutions and their teachers are scientific investigators. And that is just exactly the goal at which our own best universities are aiming—in spite of the protests of small colleges that do not see that the function of the university is not identical with that of the college—and why they are beginning to inspire respect in foreign lands.

It would, of course, be impossible to touch upon all the interesting topics taken up by Professor Paulsen, within the narrow compass of this review. The most vital questions of university education are discussed by the author in his usual sensible, quiet and sane manner. He tries to see the things as they are, their good and bad sides, and he speaks as one who knows. His remarks on the lecture system, which, when supplemented by seminars and practical exercises, he regards as the best, on the whole, and his views on the elective system (*Lernfreiheit*), will prove helpful to many of us, at the present stage of our development. His defense of the German system of appointing professors, which is frequently attacked in Germany, is also interesting. The German plan is not perfect, of course; no system can be perfect that is applied by imperfect human beings, and illegitimate influences will always play their part in the selection of professors as long as human nature remains what it is. At the same time, it seems to me, the Germans are much more careful and impartial in their choice and maintain a higher standard than we do. Local, personal, political and sectarian influences are stronger with us than with them. It is true also that we are making great improvement along this line, and that the results are seen in the greater efficiency of our faculties, but appointments are frequently made in the United States, even in good universities, which 'outsiders' do not understand and the initiated understand only too well. We shall outgrow all that too, but we have not outgrown it yet.

This book of Professor Paulsen's is, in my opinion, the most satisfactory exposition of university problems and the most helpful practical guide in solving them that has been published in recent years, and cannot fail to find an appreciative circle of readers. It will bear good fruit in our country and increase the debt of gratitude which we owe to the German universities for what they have done for our higher education.

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DISCUSSION AND CORRESPONDENCE.

'SO-CALLED SPECIES AND SUBSPECIES.'

THE article in the issue of SCIENCE for August 8 (N. S., Vol. XVI., pp. 229–231), under the above caption, is opportune, even if the author falls somewhat short of hitting the mark. He appropriately takes as his text Mr. Oberholser's recent 'Review of the Larks of the Genus *Otocoris*,' and presents the layman's view of the deplorable addition of a number

of new trinomial names 'to our already overburdened nomenclature.' Mr. Clark has done good work in certain ornithological lines, for which he deserves due credit, but his labors, so far as his published writings show, have been quite foreign to the subject upon which he here descants with the confidence becoming only to an expert. His statements and point of view, however, show lack of experience and familiarity with such lines of research as are involved in the consideration of trinomials and 'so-called species and subspecies.' Evidently he has never attempted to analyze and classify 2,150 specimens of larks, or of any similar varied and widely distributed group.

I am not writing to defend the work of Mr. Oberholser or of Dr. Mearns, which Mr. Clark has chosen as a subject for comment; nor to approve of the tendency of fine splitting now so much in vogue in certain quarters; but to correct certain unwarranted impressions that the lay reader may derive from Mr. Clark's statements and criticisms. Mr. Clark says: "The important question which this (Mr. Oberholser's) monograph raises is how far is it desirable to recognize these varieties (of larks) by name? Or better, are the diversities of size and color in a specified geographical area, sufficiently constant to warrant recognition as subspecies?" These are old questions, already many times discussed. The first question of 'how far,' etc., will ever be a matter of personal equation and temperament; in reply no hard-and-fast line can be laid down; so long as there are ultraists and conservatives, so long will there be 'splitters' and 'lumpers.' To the second question only an emphatic *yes* is admissible; and in Mr. Clark's contention to the contrary he affords conclusive evidence that he is writing without possessing that familiarity with the facts of the case which can only be attained by long experience in a field which is yet obviously little known to him. This is evidenced by the following, among other statements he makes: "To many persons it would seem to be almost an axiom that a character which can not be stated in language or in figures of any sort is not sufficiently conspicuous to bear the weight of a name." "Another rule which to the layman would seem to be axiomatic is that characters which can not be recognized regardless of the locality where the specimens are collected are worthless." This is naturally the layman's view of the case, but what are the facts, as known to the expert?

In ornithology, and especially in mammalogy, perfectly 'good species' are often so similar in size and color that even the expert cannot satisfactorily identify them from descriptions, and hence, almost from time immemorial, direct comparison with authentic material has been necessary in order to settle such difficult cases. As all experts in this line of study well know, forms that may be indistinguishable by descriptions are, when brought together, and especially when series are compared, so noticeably different that there is no trouble in distinguishing them at a glance. They present to the eye differences that are sufficiently impressive but which, owing to the imperfection of descriptive terms, cannot be adequately expressed in keys or in diagnoses. Hence when new material comes to hand from localities the fauna of which is as yet imperfectly known, the expert feels compelled, in a greater or less number of instances, to appeal to his confrères for the loan of authentic representative specimens of the described forms to which his own doubtful specimens seem most closely allied. Nor is it any disgrace to the expert, nor any reflection on present-day methods that constant resort has to be made to such aids.

As Mr. Clark very truly says: "The chief value of systematic zoology lies in its service as a basis for progress in knowledge of the laws of distribution, variation and evolution. Recognition of well-defined subspecies is essential to accurate knowledge, but bestowing names upon all sorts of individual diversities and inconstant trivialities is the very worst extreme." And, after quoting some very 'sensible words' on this point from Mr. Oberholser's paper, he goes on to ask "* * * but can degrees of variation be properly set forth if they cannot be 'intelligibly expressed on paper'?" We submit that the 'layman,' who is naturally so troubled and confused by the modern ways of finding out how and to what

extent animals are modified by their environment, is not the proper arbiter to determine the value and bearing of expert knowledge. If in other fields of scientific research it is not demanded that the investigator stop his work at the point where his results are within the comprehension of the lay mind, why should the student of birds and mammals be expected to refrain from extending his researches beyond the point of convenience for the layman?

Mammals, being sedentary, are very susceptive to climate or other physical influences; birds being to a greater or less extent migratory, are perhaps, generally speaking, less so although when non-migratory they respond, often with great readiness, to environmental influences; but in the case of non-sedentary species, the fact of migration, combined with the ever-varying seasonal conditions of plumage, increase the difficulty of discriminating and geographically limiting localized forms. The factor of intergradation between neighboring forms over areas connecting the main differentiation regions also complicates the problem of identification and leaves a considerable proportion of connectant specimens that cannot be satisfactorily referred to one rather than to another of two or more geographically adjacent forms. But this is as it should be, if environment has any influence in modifying animals. The real trouble is the temptation to indiscreet or over-ambitious specialists to give names to too many connectant forms that would be better left unnamed.

Experience shows that the 'characters' claimed by describers for their new forms are rarely without basis; when the same material is independently examined by several different experts they generally agree as to whether or not certain alleged differences exist, but they may, and often do, differ in their estimates of the nomenclatorial value of the differences. This, as before intimated, is a condition of things beyond present remedy. As regards North American birds, the aspiring young ornithologist has now a comparatively barren field so far as the discovery of well-marked new forms is concerned and the tendency is to name forms not fairly entitled 'to bear the weight of a name.' His 'discoveries' are often not new zoological facts, but a reestimate of the nomenclatorial value of facts long known to the older, more experienced, and more conservative workers, who have simply not deemed them entitled to serve as the basis of a name. But there are many exceptions; as material collected in the breeding season from many and widely separated regions becomes available for comparison, it not infrequently happens that differences previously unnoticed, or if noticed incorrectly attributed to seasonal or individual variation, are found to have a local habitation and to characterize distinct geographic areas. Although such differences are commonly slight, at least from the layman's point of view, they are zoological facts that may well be recognized by making them the basis of a name.

In this connection it may be well to recall the fact that not all of the many new 'subspecies' of North American birds proposed in recent years are admitted to recognition by the American Ornithologists' Union Committee on Nomenclature, whose duty it is to examine the merits of each and rule upon their admissibility to the A. O. U. 'Check-list of North American Birds'; at least one third having been 'turned down' or disapproved by the A. O. U. Committee, while many more are still in abeyance awaiting further investigation by the Committee. But the adverse ruling of the Committee does not always result in their effectual suppression, as their authors, with a small personal following, sometimes continue indefinitely to recognize in their own writings some at least of the discredited names.

As already said, Mr. Clark's article is timely and voices a widespread feeling among laymen, but who, it is not too much to assume, are necessarily poorly equipped to render a proper verdict in a field where expert knowledge is necessary. Yet it must be conceded that the laymen are in part right; 'splitting' is undoubtedly carried too far, and that the fact is well recognized, and the practice esteemed a great evil by competent judges, is evidenced by the decisions made each year by the A. O. U. Committee. On the other hand Mr. Clark's presentation of the case, if allowed to pass without comment, might lead to erron-

eous inferences, prejudicial to a correct understanding of what is really taking place and to the setting up of wrong standards in respect to the degree of difference legitimately open to recognition by name. J. A. Allen.

PRESIDENT MINOT ON 'THE PROBLEM OF CONSCIOUSNESS IN ITS BIOLOGICAL ASPECTS.'*

Sciences, like human beings, are seldom indifferent to the good opinion of others. Even age and great respectability never wholly dull the moral consciousness of a science to the approval and disapproval of its neighbors. This sensitiveness is, however, keenest and most easily wrought upon in the younger sciences, for the reason that these are most frequently challenged to defend their right to exist. Self-consciousness—provided it does not approach morbid embarrassment—is by no means a misfortune to the youthful science. It clears up its concepts, gives self-confidence and helps it to get on with its fellows. Psychology has had, more than most sciences, to give a strict account of itself and of its methods, both because it has had an unusual amount of prejudice to overcome and because it has developed in an unusually critical and criticising period of thought. The social pressure has, however, served its purpose, so far as psychology is concerned; for psychology —even as an experimental science—has passed its majority and knows perfectly well what its task is and how it means to perform it. But, while this is true, and while one science is never, within its own borders, responsible to any other coordinated branch of knowledge, there is, as I have intimated, the temptation to stop and listen when one's character and obligations are discussed in a convocation of the sciences. The temptation is not to be withstood when the discussion turns out to be the authoritative opinion of a near neighbor with whom important and amicable relations have, for some time, been sustained. Professor Minot, in his recent address at Pittsburgh, indicates what he conceives to be the most natural and the most profitable attitude of the biological sciences toward psychology. His outline involves a definition of mental phenomena, a statement of the part that consciousness plays in bionomics, and an appeal to psychology to employ the comparative method. The argument of the address runs as follows:

Consciousness may be regarded either as a real phenomenon in the world or as an epiphenomenon. The 'epiphenomenon hypothesis of consciousness' is, according to the author, 'an empty phrase, a subterfuge.' "Consciousness ought to be regarded as a biological phenomenon, which the biologist has to investigate in order to increase the number of verifiable data concerning it. In that way, rather than by speculative thought, is the problem of consciousness to be solved, and it is precisely because biologists are beginning to study consciousness that it is becoming, as I said in opening, the newest problem of science." * * * "For the present, it is more important to seek additional positive knowledge than to hunt for ultimate interpretations." The 'younger science of experimental psychology' is, therefore, to be welcomed. "It completes the circle of the biological sciences." The most striking peculiarity of consciousness—a peculiarity which is common to biological processes—is that it is teleological. "We do not know what it is, we do not know how it functions, but we do know why it exists." The essential 'function of consciousness is to dislocate in time the reactions from sensations.' The evolution of consciousness is a strong indication of its usefulness to the organism. If it had not been useful it would have disappeared. It is useful because it permits the individual to react on his accumulated experiences. Sensations recur in memory and increase the scope of possible adjustments. Sensations are only symbols of 'objective phenomena.' We 'see' colors, but light—the 'external reality'—is undulations. "Objectively, red, yellow and green do not exist." These symbols are, nevertheless, convenient labels, for by means of them the individual reacts appropriately on every occasion. They are 'bionomically sufficient because they are constant.' 'They enable consciousness to prophesy or foresee the results of the reactions of the organism,' and, hence, to maintain adjustment. Animal conscious-

* Science, July 4, 1902.