IT is stated in the daily papers that Dr. Antoneo Crocichia has been elected to the chair of biology in the Catholic University, Washington.

DR. J. S. ELY, professor in the Woman's Medical College, New York, has been elected professor of the theory and practice of medicine in the Medical School of Yale University.

WE learn from the *Botanical Gazette* that Dr. E. B. Copeland has been appointed assistant professor of botany in the University of Indiana in place of Dr. Geo. J. Peirce, who resigned to accept a similar position, in charge of plant-physiology in the Leland Standford Junior University.

DR. BECK, of the University of Lemberg, has been promoted to a full professorship of physiology, and Dr. Konrad Zeisig has been made second professor of physics in the Polytechnic Institute of Darmstadt. Dr. Deichmüller, observer in the observatory at Bonn, has been appointed associate professor. Dr. Ludwig Heim, of Würzburg, has been called to an associate professorship of bacteriology in the University of Erlangen. Dr. Hillebrand has qualified as docent in astronomy in the University at Vienna.

DISCUSSION AND CORRESPONDENCE.

THE DISCRIMINATION OF SPECIES AND SUBSPECIES.

DR. MERRIAM'S paper in SCIENCE for May 14 (N. S., Vol. V., No. 124, pp. 753–758), entitled 'Suggestions for a New Method of Discriminating between Species and Subspecies,' opens up a question of immense interest and far-reaching importance, respecting which there is room for two widely divergent opinions, both susceptible of support by arguments of considerable weight. Dr. Merriam cites the purely conventional and arbitrary rule adopted in the A. O. U. 'Code of Nomenclature' for deciding the status of closely related forms with reference to whether they are to be ranked as species or subspecies, and calls attention to the well-known inconsistencies sometimes resulting from its use. The failure of the rule to yield always satisfactory results is not due to the principle involved, but to the imperfection of our knowledge respecting what closely related forms intergrade and what do not. Consequently, it is urged, a stable nomenclature for such forms cannot be attained under this rule till we have a complete knowledge of the relations of such forms; in the meantime their status will be unstable, and their nomenclature, in this respect, subject to change as our knowledge of them increases.

The first part of the rule as summarized by Dr. Merriam (l. c., p. 753)—to wit: "Forms known to intergrade, no matter how different, must be treated as subspecies and bear trinomial names"—presents no difficulty of application and can be carried into effect without imperiling stability of nomenclature. The second part —namely, "forms not known to intergrade, no matter how closely related [or, rather, how closely they resemble each other], must be treated as full species and bear binomial names" —is difficult to apply always consistently. As Dr. Merriam says, "only in a small percentage of cases does an author have at his command a sufficiently large series of specimens, from a sufficient number of well-selected localities, to enable him to say positively that related forms do or do not intergrade;" and that consequently "authors usually exercise their individual judgment as to the *probable* existence or non-existence of intergradation," based, of course, on the nature of the differences, the geographical relationship of the forms, and on general grounds —on what is known to happen in other similar cases. Hence, naturally, some degree of inconsistency results in the use of trinomials, they being frequently employed where conclusive evidence of intergradation is lacking, though strongly indicated by the circumstances of the case. When later information shows that the true relationship of the forms in question has not been correctly indicated, their status must be changed, either from that of a species to a subspecies, or the reverse, as the case may require. But this, while undesirable, is not a serious change, since the 'special' name (specific or subspecific) is necessarily retained—a change far less important than the substitution of one name for another, as not infrequently becomes imperative from other causes.

The real question, then, is whether we can

not well afford to wait for any necessary rectifications of this sort rather than to adopt any alternative thus far suggested. I am heartily in sympathy with any effort to improve the present somewhat unsatisfactory method, and am glad Dr. Merriam has raised this important question for discussion, respecting which the comparison of opinions of experts in this line cannot fail to be interesting and profitable. I am also glad that Mr. Roosevelt has presented 'A Layman's Views on Scientific Nomenclature' (SCIENCE, N. S, Vol. V., No. 122, April 30, 1897), believing that such discussions have value in rendering clear the 'reason of things' under our modern phase of systematic zoölogy, as it has drawn forth Dr. Merriam's admirable exposition of the 'other side.'

I cannot, however, quite subscribe to Dr. Merriam's proposed remedy for this 'incurable inconsistency' and these 'inevitable changes' contingent on increase of knowledge. It is proposed that we base our recognition of species and subspecies on 'the degree of difference between related forms,' principally on the ground that "a knowledge of this is infinitely more important than a knowledge of whether or not the intermediate links connecting such forms happen to be living or extinct." In other words, it would be more useful "if the terms species and subspecies were so used as to indicate degree of difference, rather than the author's opinion as to the existence or non-existence of intergrades." On this point there is obviously room for difference of opinion. This phase of the subject, however, may be waived as aside from the main point, which I take to be the feasibility or non-feasibility of adopting the 'degree of difference' standard for species and subspecies. Yet I would like to add, in passing, that to me it is of far greater interest to know that the connecting links between quite unlike forms still exist, and that we have thus positive evidence of their genetic relationships, than to know that these forms, in their extreme phases, have become so far differentiated as to present differences as great as ordinarily characterize closely related species.

The real difficulty with the degree of difference principle is its elasticity; it enlarges to the widest possible extent the personal equation element, which is one of the alleged sources of dissatisfaction with our present system. Dr. Merriam's paper, taken as a whole, so far shows the wide influence of 'personal equation' in such matters that little need be added on this point, except by way of further illustration. The diversity of opinion respecting the amount of difference required to distinguish genera and subgenera is notorious; is it likely to be any less in the case of species and subspecies, in case degree of difference is taken as the basis of their recognition? In reply to this, reference may be made to the treatment of North American birds by the authors of the 'British Museum Catalogue of Birds,' on the one hand, and of the 'A. O. U. Check List,' on the other. In the former work some of the most worthless subspecies are given the rank of full species, while, on the other hand, many of the most strongly marked subspecies, and even some species, are wholly ignored, being reduced to synonyms, with often not a word of comment. And this is done not in one group, nor by one author, but in all groups and by each of the half-dozen or more eminent ornithologists who have contributed to this monumental work. This does not foreshadow any 'narrowing of bounds' of the personal equation element, nor give much hope of agreement on any 'degree of difference' standard for the basis of species and subspecies.

As is well known, not only a great deal depends on 'the point of view,' but also on the quantity and character of the material different authors may have before them, in relation to their conclusions on identical questions. The point of view, expertness and amount and kind of material are thus factors in the case, so that, whether we adopt the intergradation test or the degree of difference test, we are not likely to reach unanimity of opinion on such matters for a long time to come.

But there are other points that demand consideration. The advocates of the 'intergradation' test claim that it is based on a philosophic principle, and that the use of a binomial term means one thing and the use of a trinomial term means another and a very different thing.

Binomial names are given only to forms known or supposed to be non-intergrading—to

fully segregated species; trinomial names and forms known or supposed to intergrade, to incipient species or geographical forms, to species still in process of evolution. As said by the present writer some years ago (*Auk*, I., 1884, 201): "It hence follows that the terms species and varieties [or subspecies] are not interchangeable at will, but expressions for certain definite and known facts in nature, grounded on a philosophic principle, to ignore which is not only unscientific, but is to deprive us of a means of precise definition at a point where precision is of high importance."

It thus seems to me better to maintain our theoretically hard-and-fast standard for regulating the status of closely related forms than to adopt so elastic and unphilosophic a basis, and one withal so eminently open to the influence of personal equation, as the 'degree of difference' criterion must inevitably be, and allow time and research to correct the lapses that may occur under our present system.

J. A. ALLEN.

TO THE EDITOR OF SCIENCE: I have been greatly interested in Dr. Merriam's article as to discriminating between species and sub-species. With his main thesis I entirely agree. I think that the word 'species' should express degree of differentiation rather than intergradation. I am not quite at one with Dr. Merriam, however, on the question as to how great the degree of differentiation should be in order to establish specific rank. I understand entirely that in some groups the species may be far more closely related than in others, and I suppose I may as well confess that I have certain conservative instincts which are jarred when an old familiar friend is suddenly cut up into eleven brand new acquaintances. I think he misunderstands my position, however, when he says, "Why should we try to unite different species under common names?" He here assumes, just as if he were a naturalist of eighty years ago, that a 'species' is always something different by its very nature from all other species; whereas the facts are that species, according to his own showing in the beginning of his article, are merely more or less arbitrary divisions established for convenience's sake by ourselves, between one form and its ancestral and related forms.

I believe that with fuller material Dr. Merriam could go on creating new 'species' in groups like the bears, wolves and coyotes until he would himself find that he would have to begin to group them together after the manner of the abhorred 'lumpers.' His tendency to discover a new species is shown by the allusion in the last part of his article to the 'unknown form of wapiti,' which has been exterminated from the Allegheny country. The wapiti was formerly found in the Allegheny regions; there it was beyond a doubt essentially the same animal that is now found in the Rockies. Probably it agreed more closely with the wapiti of Minnesota, which still here and there survives, than the latter does with those of Oregon. It may have been slightly different, just as very possibly a minute study of wapiti from the far south, the far north, the dry plains, the high mountains and the wet Pacific forests might show that there were a number of what Dr. Merriam would call 'species' of wapiti. If this showing were made, the fact would be very interesting and important; but I think it would be merely cumbrous to lumber up our zoological works by giving names to all as 'new species.' It is not the minor differences among wapiti, but their essential likenesses, that is important.

So with the wolves. Dr. Merriam has shown that there are different forms of wolf and coyote in many different parts of the country. When he gets a fuller collection I am quite sure he will find a still larger number of differences and he can add to the already extensive assortment of new species. Now, as I have said before, it is a very important and useful work to show that these differences exist, but I think it is only a darkening of wisdom to insist upon treating them all as a new species. Among ordinary American bipeds, the Kentuckian, the New Englander of the sea coast, the Oregonian, the Arizonian, all have characteristics which separate them quite as markedly from one another as some of Dr. Merriam's bears and coyotes are separated; and I should just as soon think of establishing a species in the one case as in the other.

Some of the big wolves and some of the coyotes which Dr. Merriam describes may be entitled to specific rank, but, if he bases separate species upon characters no more important than those he sometimes employs, I firmly believe that he will find that with every new locality which his collectors visit he will get new 'species,' until he has a snarl of forty or fifty for North America alone; and when we have reached such a point we had much better rearrange our terminology, if we intend to keep the binomial system at all, and treat as a genus what we have been used to consider as a species. It would be more convenient and less cumbersome, and it would be no more misleading.

Dr. Merriam states that the coyotes do not essentially resemble each other, or essentially differ from the wolves. It seems to me, however, that he does, himself, admit their essential difference from the wolves by the fact that he treats them all together even when he splits them up into three supra-specific groups and eight to eleven species. He goes on to say that there is an enormous gap between the large northern coyote and the small southern coyote of the Rio Grande, and another great gap between the big gray wolf of the north and the big red wolf of the south, while the northern coyote and the southern wolf approach one another. Now I happen to have hunted over the habitats of the four animals in question. I have shot and poisoned them and hunted them with dogs and noticed their ways of life. In each case the animal decreases greatly in size, according to its habitat, so that in each case we have a pair of wolves, one big and one small, which, as they go south, keep relatively as far apart as ever, the one from the other. At any part of their habitat they remain entirely distinct; but as they grow smaller toward the south a point is, of course, reached when the southern representative of the big wolf begins to approach the northern representative of the small wolf. In voice and habits the differences remain the same. As they grow smaller they, of course, grow less formidable. The northern wolf will hamstring a horse, the southern carry off a sheep; the northern coyote will tackle a sheep, when the southern will only rob a hen-roost. In each place the two animals have two different voices, and, as far as I could tell, the voices were not much changed from north to south. Now, it seems to me that in using a term of convenience, which is all that the term 'species' is, it is more convenient and essentially more true to speak of this pair of varying animals as wolf and coyote rather than by a score of different names which serve to indicate a score of different sets of rather minute characteristics.

Once again let me point out that I have no quarrel with Dr. Merriam's facts, but only with the names by which he thinks these facts can best be expressed and emphasized. Wolves and coyotes, grizzly bears and black bears, split up into all kinds of forms, and I well know how difficult it will be and how much time and study will be needed, to group all these various forms naturally and properly into two or three more species. Only a man of Dr. Merriam's remarkable knowledge and attainments and ability can ever make such groupings. But I think he will do his work, if not in better shape, at least in a manner which will make it more readily understood by outsiders, if he proceeds on the theory that he is going to try to establish different species only when there are real fundamental differences, instead of cumbering up the books with hundreds of specific titles which will always be meaningless to any but a limited number of technical experts, and which, even to them, will often serve chiefly to obscure the relationships of the different animals by over-emphasis on minute points of variation. It is not a good thing to let the houses obscure the city.

Theodore Roosevelt.

Washington, D. C.

GLACIAL MAN IN OHIO.

I have read 'Human Relics in the Drift of Ohio' and Dr. Brinton's criticism of the same in Science of February 12th.

The gist of Professor Claypole's paper is based upon the discovery of a polished stone axe, made by a well-digger in Ohio ten years before.

Not with especial reference to this discovery, but *apropos* of the danger of accepting any statement at second hand even from the most veracious person (for we are all liable to error), I would like to cite two personal experiences