on the very dates when the disturbed sections were in process of being brought into view by rotation. Perhaps the most striking illustration of the whole matter in a single instance is to be found in the history of a great disturbance upon the sun in January, 1886. Upon the 12th of that month spots suddenly began to form almost precisely at the meridian and about 10° south of the sun's equator. Upon the four days following, these spots became numerous, and some of them very large, covering an enormous area, extending finally from the meridian almost half-way to the western limb. It would seem that if magnetic effects ever proceed from the sun's meridian that this, above every other, should have been a case in point. But there was scarcely any disturbance whatever and no auroras were reported from any source. On Jan. 16 and 17 the magnets were entirely free from disturbance when this great spot-group was undergoing many rapid changes and was generally in the precise location to have a terrestrial magnetic effect according to the idea which Professor Ricco attempted to work out as above described. When, however, this area was at the eastern limb, from Jan. 7 to 11, although it had not yet developed spots and was the seat of groups of brilliant faculæ only, there was an entirely different state of affairs, a great magnetic storm being in progress and auroras being reported generally from localities in high latitudes. Thus it appears that it is not faculæ in general that produce such marked effects, but faculæ in the location of areas frequented more or less persistently by spots, etc.

M. A. VEEDER.

Lyons, N.Y., April 14.

Where is the Litre?

I HAVE read Professor Mendenhall's contribution to *Science* of April 21 with surprise. I did not think it possible for so eminent a man to so entirely miss the point of any article he might condescend to read and criticise. Nor did I think it possible for so keen-witted a controversialist to so entirely forget his own argument as to admit and corroborate the very statements he set out to refute. Yet any reader of *Science* who may take the trouble to read the two articles written respectively by Professor Mendenhall and myself under the heading "Where is the Litre?" will see that both of the unlikely events in question have happened.

I invite my distinguished critic to re-peruse the paper he attacks, and to thus ascertain whether it contains any statements or contentions displaying "ignorance of the recognized principles of metrology," or whether it sets forth "certain conclusions which will generally be harmless on account of the very magnitude of their errors." If he can find any statements, contensions, or conclusions that appear to him to justify such descriptions, let him quote them in their *ipsissima verba*, and let him show in what manner they betray ignorance or error. I will then, in my turn, show the Professor to be mistaken.

This is no over-bold challenge. It is almost self-evident that Professor Mendenhall was unable to find any display of ignorance or any erroneous conclusion in my article; as, in that case, he would naturally have quoted the offending passages in justification of his severe remarks. But his only approach to quotation is worded as follows: "The sermonizing finish to the article, beginning with the sentence, 'In spite of the much lauded simplicity of metric measures,' etc., may, however, mislead a few readers whose ideas have been befogged by the perusal of the previous three pages." Such a reference is too loose, too indefinite, and too general to indicate what particular statements or conclusions are objected to; and the Professor's scornful allusion to easily-befogged readers of *Science* is, perhaps, too donnish.

And now, while leaving my critic to the digestion of my challenge, I may, without impropriety, quote some opinions that have reached me from other authorities.

1. The *Engineering News* of March 30, in an editorial reference to my paper, says: "Different enactments by legislative bodies, errors in measurement and in calculation, difference in weights between bodies weighed in air and weighed in vacuo, and difference in weights between water containing air and water freed from it have conspired to produce these variations. It is true these variations are all so small as not to affect the practical accuracy of any ordinary measurements; but for the exact work of physicists and chemists, and for some of the finer measurements of engineers, these variations are sufficient to affect the results. The moral which Mr. Emmens points is that the author of any paper or treatise claiming scientific accuracy, and dealing in quantities whose exact values may be in doubt, should preface his work with a statement of the constants adopted throughout the work. In a personal letter to us Mr. Emmens makes the further suggestion that the international congress of scientists and engineers at Chicago next summer will afford an excellent opportunity for defining anew the metric standards whose values have become most variable, thus restoring to the system the advantages of simplicity and freedom from ambiguity which it was originally intended to possess. It certainly gives good ground for criticism that in every school in the land pupils are taught that the litre is equal to the cubic decimetre, whereas, in reality, the litre is about 0.1 cubic inches larger than a cubic decimetre, the exact variation depending on what value is chosen for each."

2. Professor De Volson Wood, of the Stevens Institute, writes: "Your article in *Science*, 'Where is the Litre?' is such a model of courteous discussion that I thank you for it. The closing remarks contain sentiments I often advocate, but you have done it so much more completely and in all respects so much better than I could, that I appreciate it."

3. Mr. R. A. Hadfield, of the Hecla Steel Works, Sheffield, England, whose scientific reputation is world-wide, writes: "It appears to me you have touched the weak point of the Metric system, and it was only the other evening, at a lecture on this subject, that I was aware for the first time there was a difference between the litre and the cubic decimetre. No doubt many others are in the same way, and it would therefore be specially desirable to have some common understanding on this matter."

4. Mr. Latimer Clark, F.R.S., writes: "I will see the Board of Trade with your letters. They are as anxious as you or I can be to help in such a cause, and would do anything to promote it. The Chicago conference would afford a capital opportunity for raising the question, and I will do anything required if you will point out what you recommend. The difference between the litre and cubic decimetre is simply one of popular belief and teaching, and it arises from the French Bureau having decided to adopt the bulk of the kilogramme of water as the bulk of the litre. I may perhaps add that the Warden of the Standards here has written me that he acknowledges my dictionary as correctly setting forth the values they have adopted and are employing, and he adds that he recommends the book to all enquirers on the subject."

I refrain from adducing further evidence lest I should put Professor Mendenhall in the position of the dissentient juryman who complained that "he had never before, in the whole of his life, met with eleven such obstinate fellows."

STEPHEN H. EMMENS.

Youngwood, Pa., April, 25.

Sham Biology in America.

MR. CONWAY MACMILLAN has shown more enthusiasm than discretion in his recent article. He is writing in a good cause, namely, the elevation of botany to an equal rank with zoölogy in biological teaching in universities. Biology, however, is not the science of animals and of plants, as Mr. MacMillan maintains, it is rather the science of life; and I am not aware that biology is taught in any large institution in this country without taking advantage of the fact that certain laws and principles of life are, for purposes of practical study, far better shown in plants than in animals. Plant biology is therefore extensively taught upon the lines laid down by Huxley and Martin, and on such lines we simply select the organism which best demonstrates a certain principle. If the botanists of this country allow the zoölogists to take the lead as *biologists*, that is, in setting forth the fundamental principles of life from their observations upon animals, it will naturally follow that zoölogy will occupy the leading position in the universities. Mr. MacMillan's argument should therefore be directed to the botanists and not to the zoölogists, who are in no

way responsible for the alleged one-sided state of biological education.

While Mr. MacMillan's enthusiasm is in a good cause, he has allowed it to run away with his discretion. Without sufficient reflection or inquiry, he has, unintentionally, I am sure, given an entirely wrong impression of the character of work done in several institutions; this is done under a very sensational title and in a style of questionable taste. As it is desirable that this impression should not spread, and as the arrangement of courses in Columbia is cited by Mr. MacMillan as a leading example of the manner in which botany is subordinated to zoölogy, let us see what the Columbia courses are, as announced in the circular of the faculty of pure science:—

17 *Courses in Botany*, in A. Department of Botany.	1. Elementary Botany. 2. Elementary Botany. 3. General Botany. 4. Vegetable Anatomy. Cells and Tissues. 5. Morphology and Determination of Flowering Plants. 6. Economic Botany. 7. Cryptogamic Botany. 8. Advanced Vegetable Anatomy. 9. Natural Orders of Flowering Plants. 10. Advanced Cryptogamic Botany. 11. Comparative Study of Tissue of Twelve Species. 12. Comparative Study of Plants from a Certain Area. 13. Critical Study of a Genus.
in B. Department of Geology.	1. Palæobotany. 2. Study of Flora of Certain Geological Horizons.
3 *Courses in Physiology*, in Department of Physiology.	1. General Physiology. Lower Animal Types. 2. Human Physiology. Man and Lower Animals 3. Laboratory Physiology.

There are altogether eleven courses in zoölogy under the Department of Biology, two of which, namely, "Elementary Biology" and "Cellular Biology" are taught in part from plants.

It does not appear that botany is ignored in this programme of biological courses of study in this institution. The fact that the botanical courses are not arranged under the Biological Department is a mere technicality of administration, which raises no confusion in the minds of students, any more than does the separation of the Department of Physiology, which is equally cognate to biology. The separation of these three departments is simply owing to the fact that botany and physiology were already well established when the trustees decided to found a distinct department in which biology would be taught especially as illustrated in animal types.

HENRY F. OSBORN.

Columbia College, New York, April 13.

Cedar Waxwings,

Mr. Edwin M. Hasbrouck's "Presumably new fact relative to the Cedar Waxwings (Amp. Ced.)" in the issue of the 17th ult., is a very interesting discovery. The observations from which his conclusions were obtained, are familiar to modern ornithology, while his inductions are assuredly new to me. Whether they are accepted or not, his views of the importance of carefully studying the *first* plumages of birds will scarcely fail of universal acceptance. I have no criticisms, but wish to add an observation concerning the wax tips of the secondaries and retrices of the species which I am inclined to think will favor his conclusions.

I have made the ultimate anatomical structure of feathers a special study for many years, during which I have given those of the period before the first moulting special consideration, and have met with some extremely interesting things.

I have never been so fortunate as to meet with a wax tip while the young bird was still in the nest, but have occasionally seen them in very fresh subjects, or as early as the 25th of July. The development of the appendage, after it has commenced to appear, is very rapid indeed, resembling the process of the growth of the new antlers of a buck. I cannot yet state definitely the length of time, but from three to five days ordinarily, and doubtless sometimes a little more. In a work devoted to the Birds of Minnesota, I have made some references to my familiarity with the species, to which I might add many more notes, since that went out of my hands, that are even more in point, but suffice to say, the red wax is secreted in the ciliohamular portion of the barbules of the terminal barbs of the feather.

The rapidity of the development of the appendage is such that occasionally it results in doubling the whole series of barbs with their barbules, back upon the rachis of the feather, and reveals the fact that the horny material constituting the wax-like mass is filled from the tip, shaftward, as if in fact, as in appearance, it consists of genuine red sealing-wax, which has become so thickened or condensed as to cease flowing before quite reaching the point of union of the barb with the delicate, overladen rachis The naked portion of those barbs becomes an easy object of observation under low powers of the microscope, and under supremely good light and a higher magnification, the reflected portions of the barb with its barbules, and even the barbicels, may be seen resting upon the unreflected portion of the barbs and rachis. That there is some special condition very temporarily involved, that produces these decorations, there can be no doubt. I have never yet succeeded in seeing a wax-tip on a waxwing reared in captivity, excellent as has been my opportunity. Who next has something new about the Cedar Waxwing?

P. L. HATCH.

An Appeal to Naturalists.

MAY I appeal through your valued columns for the coöperation of the naturalists of the country? The following letter from Professor Kölliker of Würzburg is the occasion of my appeal:—

WÜRZBURG, April 4, 1893.

MY DEAR PROFESSOR MINOT:

May I ask you if you could procure for me some rare American forms of fishes and amphibians, preserved in Müller's fluid, so as to be investigated microscopically after Golgi's and Weigert's method? Larger animals should be cut transversely, so that the fluid can enter the spinal canal and act upon the spinal marrow. At the same time the head or body should be opened and the brain acted upon.

The list of my wishes is very large, but I shall be very glad, if I get only some of the animals mentioned. It includes, among the amphibia and reptiles, Amphiuma, Siren, Menobranchus, Menopoma, full-grown and larval, young alligators and tortoises; among the fishes, Lepidostens, Amia, Spatularia, Scaphyrhynchus, full-grown and also very young. . . . I am working just now at the microscopic anatomy of the nervous system, and have begun to extend my investigations to the comparative part also. Unfortunately, specimens in spirit only are worth very little, and the only good methods are those of Golgi and Weigert. But even Golgi's is only useful on embryos and young animals, and you know that both these methods demand a previous preservation in Müller's fluid.

Believe me, etc.,

A. KÖLLIKER.

In view of Professor Kölliker's distinguished services to science, covering a period of over fifty years, and of his undiminished activity in research, every one must feel a wish to promote any investigation Professor Kölliker undertakes. In order to secure the material for which Professor Kölliker asks, I seek for contributions from my American colleagues. I request that all specimens may be sent to me at the Harvard Medical School, so as to be in my hands by May 30. All material thus obtained can be packed and forwarded to Professor Kölliker, together with the list of contributors.

The specimens should be kept in the Müller's fluid until they reach Würzburg. In order to secure a good result with the fluid, it must be used in large quantities, and should be changed every day for the first week, and twice during the second week. Müller's fluid will not penetrate hard tissues, such as bone, for more than a quarter of an inch, and soft tissues for more than three-