the same temperature have the same number of molecules to the cubic centimetre, this shows that it is not the number but the kind of molecules which determines the scattering. But perhaps the most important experiments were those in which the discharge was allowed to pass into another tube which had been exhausted so far as possible. It was argued that if the cathode discharge was due to the projection of atoms from the cathode that it could not take place in an absolute vacuum. The tube into which the discharge was to pass was exhausted as far as possible, i.e., until a twenty-centimetre spark would not pass from one electrode of the absolute vacuum tube to the other. Notwithstanding this extreme exhaustion, the discharge passed freely through, as was shown by the phosphorescence of substances placed at the other end. The conclusion which Dr. Lenard draws from this experiment is that the cathode rays are really processes in the ether, and not due to the movement of atoms.

On account of the difficulty of obtaining an absolute vacuum, Dr. Lenard's results cannot be accepted as final. Even at the exhaustion obtained by him it may be calculated that there are quite a sufficient number of atoms left to produce the phenomenon (using the results of J. J. Thomson and Chattock in the calculation), even neglecting the number contained in the layer of air on the sides of the tube, and which would be driven off into the tube so soon as the discharge began to pass. Moreover, it is quite possible to conceive that a discharge of atoms from the cathode, on reaching a thin metal sheet, and being abruptly stopped by it, might propagate an electric disturbance proceeding from the other side of the sheet of metal, and so drive off another set of charged atoms. If there were any way of obtaining an absolute vacuum, of course the question could be answered definitely, but this is impossible, and we must wait for further results before attempting an explanation. R. A. F.

LETTERS TO THE EDITOR.

*** Correspondents are requested to be as brief as possible. The writer's name is in all cases required as proof of good faith.*

On request in advance, one hundred copies of the number containing his communication will be furnished free to any correspondent.

The editor will be glad to publish any queries consonant with the character of the journal.

Low Temperatures.

IN your issue of Jan. 27, page 50, it is stated that the Franklin Search Expedition, under Lieutenant Frederick Schwatka, in 1879–80, experienced a temperature of — 71° C.

This is an error, as I have heard Lieutenant Schwatka in many conversations refer to it as "seventy-one degrees below zero, Fahrenheit."

I enclose a copy of a letter now in a collection belonging to my brother:—

TACOMA, WASH., Sept. 15, 1892.

On the third of January, 1880, my Arctic exploring party encountered a degree of cold of seventy-one below zero, Fahrenheit, or one hundred and three degrees below the freezing-point of that scale, the coldest we noted on the trip, and the coldest ever encountered by white men travelling in the field, for that day we moved our camp some twelve miles. It will be a cold day when that record gets left. FREDERICK SCHWATKA.

FRED. G. PLUMMER.

Tacoma, Wash., Feb. 11., 1893.

Where is the Litre?

IT must be a source of regret to all interested in metrology that so much time was expended in the preparation, and so much space in the publication of the leading article in *Science* for March 17, entitled "Where is the Litre?" etc. Even if the instruction contained in the article be reinforced by the amusement which it furnishes, the result is quite incommensurate with the labor which must have been involved in its production.

Ignorance of the recognized principles of metrology has led to certain conclusions which will generally be harmless on account of the very magnitude of their errors. 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. It will be well to remind them, therefore, that the apparent bewildering confusion as to the value of the litre has no relation whatever to the "simplicity of the metric system." Indeed, the confusion might have been rendered vastly greater, the alleged case against the metric system much stronger, and the entire article more picturesque, if the author had introduced the "gus" of Arabia, the "pik" of Egypt, and the "sun" of Japan, the value of each of which in metres must always be a matter of considerable uncertainty.

The following simple statements may be of value. It is generally agreed among metrologists that *natural* standards of length and mass are not at present easily attainable. Our knowledge of physical or astronomical constants must continually increase in precision as methods and instruments are improved. Such constants are, therefore, unsuitable for standards, because standards should, first of all, be invariable as far as possible. Artificial standards can be made of more convenient dimensions, can be multiplied with almost any required degree of precision, and their invariability is perhaps as well assured as that of any suggestive national standard.

It was originally proposed to derive the metre from the dimensions of the earth. We know that the metre is not the one ten-millionth of the quadrant of the meridian passing through Paris, but that fact does not in the slightest degree lessen the value of the metre as a unit of length. Its value is so nearly that, that it is exceedingly convenient to use in ordinary calculations relating to the earth, not requiring a high degree of precision.

It was also proposed originally to establish some sort of a simple relation between the unit of length and the unit of mass. As length and mass have no natural relation to each other, any numerical ratio must depend on a physical constant, namely, the density of some selected kind of matter. The determination of this must be a matter of experiment, and its value can never be absolutely known. For this reason any relation between the unit of length and the unit of mass must always be an approximation. The unit of mass must, therefore, be an artificial, independent unit.

The new international prototype of the metre is, in length, an exact reproduction of the old metre of the archives, as far as can be determined by the most recent and most perfect means of comparison. The new international prototype kilogramme is identical, in mass, with the old kilogramme of the archives, as far as can be determined by the most precise and delicate weighings ever made.

It was originally intended that the mass of the kilogramme of the archives should be that of a cubic decimetre of pure water at its maximum density. As this involves the knowledge of a physical constant, it was not possible to realize this relation exactly, and it never will be possible.

In determining volumes which do not exceed a certain limit, it has been found that greater accuracy can ordinarily be secured by the indirect method of determining the mass of a liquid of known density, than by direct geometrical processes. The application of the latter requires simple forms whose linear dimensions may be easily and accurately measured. The former depends only on the accuracy attainable in mass measurement and density determination.

This method of volume measurement has usually been regarded, however, as a matter of convenience only. Thus, the U. S. gallon is defined as a volume of 231 cubic inches; in standardizing measures of capacity in gallons, it has always been customary to use the indirect mass-density method. The mass of water which has been assumed to represent this volume has varied from time to time as our knowledge of the physical constants involved advanced.

The litre was originally assumed to be identical in volume with the cubic decimetre, and there could be no possible objection to confining the term *litre* strictly to this meaning. But, as noted above, it being vastly more convenient to use the mass-density method in determining volumes, much of the uncertainty of precise volumetric work would be avoided by defining the litre as the volume of a kilogramme of water at maximum density.

Recognizing the wisdom of this course, the International Committee of Weights and Measures, in October, 1880, resolved that in its publications and in its official use the term *litre* should be used to express the volume of a kilogramme of pure water at maximum density. The one-thousandth part of this, that is to say, the volume of a gramme of pure water at maximum density is called the *millimetre*, and the abbreviation *ml.* is used to stand for it.

The *litre* and the *millilitre*, therefore, are not precisely identical with the cubic decimetre and the cubic centimetre. The difference, however, is very small, and may safely be neglected in all ordinary operations. Where a high degree of accuracy is required, it will usually be found that the results are primarily obtained by the mass-density method, and that no correction is required.

The International Bureau is engaged in an elaborate investigation of the relations of mass, volume, and density in pure water, and, when the results are available, they will doubtless satisfy the most exacting demands. T. C. MENDENHALL.

Washington, D.C., April 14.

On the Teaching of Biology.

IF the article "On the Emergence of a Sham Biology in America," by Mr. Conway MacMillan, printed in *Science* for April 7th, 1893, had appeared in a special journal, it would not be worth while to notice it, but since *Science* reaches many people who are not specialists in any branch of biology, it may not be a waste of time to point out some of its special merits.

The author of the article looks over the courses offered in biology in some of the leading universities of the country, and, finding that botany does not receive adequate treatment, he apparently becomes fired with the serious purpose of exposing what he illogically calls a "sham" science.

The Johns Hopkins University, which has done as much as, if not more than, any other single institution in the country, for the advancement of biological science in America, during the last seventeen years, is stigmatized in a way which will highly amuse those who are acquainted with its work. This institution is accused of dishonesty in naming its zoölogical courses. "Injustice," "wrecker-light use of the word 'biology,'" "protective mimicry in a university curriculum," "perpetrating a confidence game upon a board of trustees," are some of the choice phrases which are indulged in. These flattering remarks are not limited to the institution; they extend even to its graduates. "The cool effrontery of this would have surprised me had I not known the marvellous, sometimes continuous, sometimes sporadic, always insular capabilities of the Johns Hopkins biologist for blatant philistinism in regard to things botanical."

Of course it is not necessary to take such criticism as this seriously. The tone of the article is so thoroughly bad, and the looseness of statement so completely inconsistent with anything bordering on scientific accuracy, that sober criticism is well nigh impossible.

The chief merit of the paper lies in pointing out the great value which a good course in general biology, such as that given for many years at the Johns Hopkins University, may possess for an average student, who will follow it with a fair degree of fidelity. Such a student would have learned what Lamarck, Treviranus, and Bichat comprehended, and what Huxley and the school of biologists who have been inspired by his teaching have striven with signal success to inculcate,— that the study of biology is not, as this erratic writer supposes, two disciplines, but *one discipline*, the study of living phenomena, in which the distinction between plant and animal, in the widest sense, is one of secondary importance.

A student who had followed this general biological course with a fair degree of success would have learned that "biological science is *not* to be set over against physical science in the broadest sense," but that in this broadest sense biology is a physical science, coördinate with chemistry and physics. In biology there is no natural cleavage into two branches, botany and zoölogy, any more than there is a natural constriction of chemistry into the studies of minerals and the compounds of carbon, because the plane of division in either case would be a purely imaginary one. An appreciation of this truth does not conflict with the obvious fact that biologists in general find it convenient to specialize either in the direction of the study of plants or the study of animals. Biology is often primarily divided, for convenience, into study of living structure and study of function, or into morphology and physiology, because the study of living structure is one subordinate discipline, and the study of function is another. For further convenience we may further classify these sub-sciences, according to their subject-matter, into vegetable morphology and animal morphology on the one hand, and into vegetable and animal physiology on the other.

Let an institution that sets about to teach biology do all it can to put before its students the principal facts of morphology and physiology of both plants and animals, but to pronounce its work, if well done, a "sham," through its inability to cover the whole field, is, to say the least, a very flagrant misuse of language. The title of Mr. MacMillan's article is misleading, and the whole tone of it is characterized by this glaring misuse of words. He does not distinguish between a "sham" science and a science too much "restricted" or "narrowed." Even if we grant the most that is said in regard to the teaching of biology at some of the institutions named, all that would be proved would be that the science of biology had been too much restricted at these places, not that there was any element of "sham" in it. The work which the Johns Hopkins University has done for the study of biology in this country proves conclusively that there has been no element of "sham" in its methods.

I find in the Johns Hopkins University Circulars for March, 1893, No. 104, eleven courses offered to students in the biological department, including seminaries and clubs. One course is announced in "Cryptogamic Botany"; the rest have reference almost exclusively to animal physiology and morphology. An elementary course in botany has been given at this university for years, and lecture courses in vegetable morphology and physiology of a more technical nature have been offered from time to time, showing that the study of plants is far from being ignored. The biological work of this university, as is well known, has been chiefly devoted to the study of animal physiology and morphology, and the work that it has undertaken it has done eminently well. Nothing could be more unjust than any inference that this university has encouraged its students to undervalue the study of plants. On the contrary, it has regretted that it has had no fully equipped botanical laboratory to offer its students, and it has uniformly advised them to go to institutions better equipped in this department for the special study of plants.

It is not possible for every institution to take the same color with reference to the special lines of scientific investigation, but this is a different thing from saying that it is not desirable for every institution to have a well-balanced curriculum. In most of the smaller colleges the man at the head of his department is the only teacher in it, and if he is a botanist his work will soon take on a botanical tinge; if morphologist or physiologist, his special work is sure to come to the front. This explains a good deal of the "sham" element that Mr. MacMillan has discovered in American biological teaching.

The stimulus which comes from the association of specialists in a large educational centre is undoubtedly very helpful, but as soon as students commence to leave the elementary stages of their work, and to enter upon special lines of investigation, their sympathies immediately diverge with increasing rapidity. It is therefore desirable that this loss of sympathy on the part of one specialist for the work of another, should be postponed as long as possible. One means of accomplishing this in a large university, in the case of biology for instance, is undoubtedly to present the whole subject in the fullest manner, especially in the elementary courses.

There is no doubt that every biologist, whatever the special line of work to which he devotes himself, should have the same training up to the point of specialization, in at least chemistry, physics, morphology, and physiology. The attitude of mind which Mr. MacMillan displays comes from a lack of this early compre-