It is a singular fact that, despite all the improvements of the dioptric system and the vital urgency of the matter, the side and mast lights of vessels still remain to a large extent in so imperfect a condition. In Paris and Birmingham, the only seats of the manufacture of dioptric lights, ship lights with true lenses have long been constructed on the same principles of the sea-lights which have a radius thirteen times as great. The writer has long urged, both publicly and privately, the employment of more powerful lights at sea, and more particularly the equalization of the power of these lights by using electricity in incandescent lamps of unequal intensity, in the colored side lights, so that meeting or passing vessels shall understand the course and character of each other at much greater distances than are now sanctioned by statutory rules. At the International Marine Conference in Washington, in 1889, the subject of ship lights was amply discussed with reference to azimuthal ranges and vertical divergences, and the conclusions formulated are being now internationally adopted. But the question of *greater intensity of beam* and of *equality of beam* does not appear to have been considered in relation to the greatly changed conditions of vessels thronging the high and narrow seas in these days, and to the ever-increasing frequency of accidents by collision at night. I earnestly hope that the authorities of the United States will yet again take the initiative in effecting this final improvement in ship lights.

In closing for the present these few remarks on lighthouses it is impossible not to give expression to feelings of admiration for the liberal and enlightened policy of the United States in maintaining the lighthouses of their immense coast-line free of toll to all the maritime world. America sets a shining example to many an older country in this as in many other ways May her maritime prosperity abundantly increase!

A JAPANESE SICK WITH SCARLET-FEVER.[1]

BY ALBERT S. ASHMEAD, M.D., NEW YORK CITY.

I HAVE been introduced to a Japanese gentleman, aged 23, living in Brooklyn, who is undergoing treatment by Dr. Benjamin Ayres for scarlet-fever. As this is the first case of scarlet-fever I have ever seen in a Japanese, I report it to you. To-day is the twenty-eighth day of the disease. There has been no temperature during the last two weeks. Desquamation has been general for three weeks, mostly behind the knees and about the shoulders. He has now scaly desquamation on the palms and soles; noticed first by the patient on the backs of the hands. The throat showed very marked symptoms and is even now very distinctly red and inflamed. Highest temperature 103½; no albumenaria.

I content myself with this short sketch, as, I think, Dr. Ayres will make a more complete report.

I am the more interested in this case, as it is supposed that the Japanese have an immunity from scarlet-fever. I have tried, without success, several times to inoculate a Japanese subject with the disease, in the hope of producing a protective virus. More recently I inoculated two children who had been exposed to the contagion of scarlet-fever with the blood-serum from a blister on the body of a child who, having had scarlet-fever previously, was artificially immune.

These children, whether protected or not, did not take the disease. More recently still, I have inoculated two cases of scarlet-fever with pure blood-serum from a blister on the body of an adult, who was also artificially immune. The inoculations were made in the arms on the third, fourth, and fifth days. In these latter cases there was no effect if diminished desquamation is not to be considered as one. Both cases ran a mild course. It is my opinion, on which, having so little to go upon, I would not insist too strongly, that blood-serum from an artificially immune subject has a virtue, if not curative, at least preventive. Dr. Seward of the Willard Parker Hospital promised me to make a further investigation in the scarlet-fever ward of his hospital.

I have given you these facts to show you what reasons I have to be particularly interested in the case on which I have summarily reported.

[1] Communicated to the Tei-I.-Kwai.

ELECTRICAL NOTES.

IF a student of molecular physics had been asked a few months ago for an explanation of the phenomenon seen when an electrical discharge is passed through a Geissler tube, he would not have hesitated in his reply. He would have shown, from the researches of J. J. Thomson and others, that the phenomenon, in the case of the non-striated discharge, is akin to that of electrolysis, that disassociation was a necessary accompaniment; that, in the case of the striated discharge, the electricity was carried partly by convection and partly by electrolysis, that this was shown by the fact that the conduction did not proceed with the velocity of light, that each stria was a place where electrolysis was taking place, and each dark band a place where the electricity was carried by convection, that the reason why the discharge was not produced with mercury vapor is that it cannot be disassociated, and that the reason that it takes place so readily with other gases is that the converse is the case.

But the recent work of Herr Hertz and Dr. Lenard has caused considerable doubt to be thrown on some parts of this theory. Not that the theory as given above may not be true after all, but it must first explain the phenomena discovered by the above-named scientists, and at present this seems difficult.

A short account of them is as follows: If we take a Crookes tube, i.e., a tube in which exhaustion has been carried to such an extent that the discharge is no longer visible, except where it strikes upon the glass, or some other solid or phosphorescent substance, we find that, as the exhaustion progresses, the rays issuing from the cathode, and producing incandescence or phosphorescence, instead of passing directly from the cathode to the anode, tend to move in a straight line, normal to the cathode. This discharge has been supposed, one might almost say proved, by Crookes, in a series of most masterly experiments, to consist of highly charged atoms of gas, repelled with great violence from the cathode. As the exhaustion becomes more and more thorough, fewer and fewer atoms are left in the tube, and consequently the trajectories of the atoms become more and more nearly straight lines, and, if the tube is bent at an angle between the electrodes, the discharge will strike against the glass.

If this is the real nature of the discharge, it would seem on first sight that it should not be able to pass through a metallic substance. Yet it has been discovered by Herr Hertz that this is not the case, that it passes readily through thin metal plates. From these two facts, that the discharge takes place in straight lines, and that it passes through thin metal plates, Dr. Lenard conceived the idea that it should be possible to produce the discharge in a Crookes tube and make it pass out into the air, and the experiment, when tried, proved successful.

The apparatus used was as follows: A Crookes tube, whose two ends we will call A and B, had the cathode electrode sealed in at A. This was of the usual form, and projected some distance into the tube. The anode consisted of a tube of aluminium, only a little smaller than the size of the glass tube containing it, and surrounding the cathode. On the discharge taking place it would, instead of passing directly from the cathode to the anode, as in the case where the gas was not so much rarefied, proceed normally from the cathode and out of the open end of the aluminium tube constituting the anode and strike against the glass at the other end of the Crookes tube. In these experiments, that end was cut off, and a metal plate cemented across the opening. In the middle of this metal plate a small hole, 1.7 millimetres, was drilled, and this was covered by a sheet of aluminium, .0003 millimetres thick. Consequently, when the discharge struck against the aluminium plate, the latter being permeable to it, it passed out into the air. This was shown by a luminous discharge just outside the sheet of aluminium, and by the fact that phosphorescent substances placed there behaved in the same manner as when exposed to the cathode discharge in a Crookes tube. If, in place of air, other gases were made to surround the aluminium plate, very different effects were obtained. If the gas was hydrogen, the discharge, after passing through the aluminium window, was not scattered so much. If carbonic acid gas, the scattering was much greater. Dr. Lenard points out that, as all gases at 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.