fluid when sent, and that evening sections were cut and mounted after short treatment with picro-carmine. Without examination two slides were sent to Sir Robert (we were busy on small-pox), who returned them with the remark, "Only muscular fibre."

Dr. Bookey looked at me and I gazed upon him, we then subjected the slide to examination with $\frac{1}{16}$ water-immersion Powell and Leland and No. 2 eye-piece, all apparatus being Powell and Leland. I have seen reticulation since, but in a tumor purely epitheleomatous; it was simply wonderful. The cells were perfectly differentiated, and the reticulation was so regular that we at once forgave Sir Robert for his hasty conclusion.

We hope to continue our investigations on amœboid organisms; but, as the process is so long, my colleague persuaded me to send you these remarks. A. COWLEY MALLEY.

Munslow, England.

The Fundamental Hypotheses of Abstract Dynamics.

I HAVE been prevented from making earlier reference to Mr. Dixon's letter in *Science* of Sept. 9, p. 149, criticising my address on the above topic, *Science*, Aug. 5, p. 71. The letter was especially interesting to me as I had not seen his paper, "On the Logical Foundations of Applied Mathematical Sciences," communicated to the Mathematical Society of London some few months ago.

Mr. Dixon, taking the relativity of direction into account, seems to me to have proved that the Laws of Motion may be regarded as forming a definition of force. My argument to show that if they be so regarded, they are not in general consistent with one another, involved the specification of accelerations by reference to a single point, and thus assumed the possibility of determining directions absolutely. While valid, therefore, as against the writers to whom I referred, who make the same assumption, it has not the more general validity which I supposed.

That I have regarded force as a non-relative conception, while Mr. Dixon has thus shown that it may be regarded as relative, would seem at first sight to place us in antagonism. It does not, however; for I have merely discussed certain points in connection with the laws of motion, employing the ordinary conception of force, and making no inquiry as to the assumptions involved in it, while Mr. Dixon proves that this conception must involve certain assumptions, and seeks to determine what they are.

Mr. Dixon points out that it is the law of the conservation of mechanical energy only which is deducible from the assumption that stresses are functions of the distance between the particles on which they act, and that this law would not include the general law of the conservation of energy until all energy was shown to be mechanical. That is quite true; but it does not seem to affect my contention, that, since we are now so sure of the conservation of all forms of energy that the law of the conservation of mechanical energy is frequently assumed as itself axiomatic, the laws of motion, if they are to be retained as dynamical axioms, should be supplemented in such a way that this law would be deducible from them. Nor does the fact that the law of the conservation of energy is usually expressed at present in a form which is probably temporary seem to me to make this any the less desirable. The conception of potential energy may lose its utility as we gain clearer insight into dynamical phenomena. When that time comes we may have to modify our fundamental hypotheses to suit the clearer views which will have been gained; but in the meantime it seems none the less desirable that we should have axioms sufficient for the deduction of the law of conservation in its present form.

There is, as Mr. Dixon supposes, an omission in the sentence of my paper which he found unintelligible. If commas be inserted after the words *sum* and *masses*, it will be found to state that, if m_1 and m_2 be the masses of two particles, and a the relative acceleration produced by a stress between them, this stress may be shown to be proportional to

$$a\ m_1\ m_2 \div (m_1 + m_2).$$

It follows that, if one of the particles be of infinite mass, the stress is proportional to the mass of the other multiplied by the relative acceleration. When I conclude from this that "if, in applying the second law of motion, a particle of infinite mass be chosen as point of reference, all the forces acting on a system of particles, may be regarded as exerted upon them by the particle of infinite mass," these forces are supposed to be exerted in accordance with the third law of motion, which asserts action and reaction to be equal and opposite, but not to be in the line joining the particles acting on one another. I do not myself regard this fiction as of any importance. I mentioned it in passing because I wished to refer subsequently to Newcomb's assertion that the law of the conservation of energy assumes it.

Mr. Dixon considers it inconvenient to include in one law of stress two statements resting on such very different evidence as that forces may be considered to be attractions or repulsions and that their magnitudes depend solely on the distances between the particles on which they act. I need hardly say, however, that I see no objection to enunciating the two statements in separate sentences. For educational purposes, indeed, it would certainly be well to enunciate what I have called the law of stress, piecemeal, as is invariably done in the case of what I have called the law of force. J. G. MACGREGOR.

Dalhousie College, Halifax, N. S., Oct. 4.

The Libyan Alphabet.

I GLADLY accept Dr. Brinton's offer (*Science*, Sept. 30); only, if his object is truth rather than the scoring of a point, he will place in the editor's hands, not the *Grammaire tamachek*, which would be useless for the purpose, but the *Grammaire kabyle*, which alone contains the full forms of the three Berber alphabets, but which Dr. Brinton appears never to have seen. Even the *Grammaire tamachek*, now that he has got hold of it, he seems incapable of understanding. The other day he mistook diacritical marks for accents, and now he tells us that Hanoteau connects the Libyan and Semitic systems "solely" because both are read from right to left, even charging me with disingenuousness for suppressing this fact. The charge might stand, had I made the assertion, which is as wide of the mark as is Dr. Brinton's appeal to Hanoteau, on the question of accent. The very Berber name *asekkil* (pl. *isekkilen*) of the letters is equated by Hanoteau (p. 5) with the Arabic *shakl* and the Hebrew *sākal*, "forme, figure, dont les Grecs ont fait σιγλαὶ," hence the French *sigle*. I am not defending these equations, but merely give them to show how ignorant Dr. Brinton still is of the contents of the *Grammaire tamachek*, which he had the temerity to insinuate I had never seen (*Science*, Aug. 19). May I ask Dr. Brinton who are the "French scholars" that regard the initial *t* as radical in the word *tifinar*, and that accent the word differently from Barth, for this also appears to be again insinuated? The recent death of M. E. Renan reminds me that that illustrious "French scholar" is also arrayed against Dr. Brinton, holding that the Punic origin of the Libyan alphabet is an established fact (*Histoire des langues semitiques*, 2d ed., p. 194. *et seq.*). Dr. Brinton is to be envied his possession of "plenty of documents in *tifinar*." Such documents are excessively rare in Europe, and even amongst the Tuaregs themselves, who, apart from rock inscriptions, have never made any extensive use of this old and defective script. Considering the weakness of his position, Dr. Brinton shows as much want of tact as of bad taste in charging his opponent with lack of candor. A. H. KEANE.

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Is There a Sense of Direction?

IN his article on the "Sense of Direction," in *Science* of Oct. 7, Dr. Work says, "It is very well known that an unguided horse returning to familiar haunts will do so over the same route by which he left them, rather than in a direct line by sense of direction." An incident which came under my observation some six years ago directly contradicts this theory. My father had purchased a very intelligent mare about a month before, and on this occasion I hitched her single to a carriage, and drove to a town about fourteen miles distant. As the direction was almost due north-west, the road ran alternately west and north, there being about eight corners to turn. Although the mare might have been through the same region before, it is pretty certain that she had never travelled just the same road. Coming back I gave her her head, and she made every turn so as to keep the same road as on the going trip, with one exception. In that case she made a short-cut by a diagonal road across a quarter-section, striking the regular road a mile further on, and saving about a quarter of a mile. In going up I should have taken the same route, had I not had some business which required me to go the longer way. At the point where this road turned off, it led toward a hill which concealed its junction with the regular road. I certainly did nothing to guide the mare, and was astonished to see her take the short-cut.

As Dr. Work has left considerable room for "accident," he may be able to dispose of this circumstance in that way, though I can scarcely accept such an explanation. J. M. ALDRICH.

Brookings, S.D.

THAT the sense of direction is feeble, if indeed present, in civilized man cannot be denied. I have had some experiences which lead me to suspect that it may be obsolescent rather than quite obsolete. It has frequently occurred that in coming into a strange town or city at night, when compelled to abandon all conscious effort to keep my direction, I have found that in some way I had not lost the points of the compass. These may have been happy accidents, but they may have been cases of unconscious orientation.

Again, upon visiting a cave of considerable dimensions, I purposely refrained from any conscious effort in keeping the points of the compass, with the same result as in the preceding cases.

To the foregoing I have added some inquiries, and a few observations upon others, and feel that there is some ground for thinking that there may be a feeble sense of direction still left to us, though so feeble as to be easily overborne by suggestion from the other senses. CHARLES E. BESSEY.

University of Nebraska, Lincoln.

On Biological Nomenclature.

I AM glad to learn, from Dr. Coues's letter in *Science*, that the code of rules promulgated by the American Ornithological Union a few years ago has been rigidly enforced in that branch of biology, and has been found to work admirably in practice. I studied these rules at their appearance with much interest and attention, and have since, so far as possible, endeavored to adhere to them in my own writings, with one exception — that concerning the erection and definition of genera. As I see that the botanists are disposed to accept this same rule, I shall be glad if a wider discussion may be called out before it becomes established. I refer to canon xlii., which recognizes the validity of generic names unaccompanied by definition, if described species are pointed out as types.

Among ornithologists, and perhaps among botanists, such a rule may not be productive of as much confusion and annoyance as is sure to be the case among entomologists. Generic characters are not, and should not be, included in specific descriptions; how then is it possible for the remote student to learn what *nomina nuda* mean, when it is impossible for him to study the types? He who studies only his own immediate fauna or flora, without a knowledge of the allied forms throughout the world, can never be very successful as a systematist, and, if we are to rely upon types, what is the good of a scientific nomenclature? Furthermore, in such a science as entomology, where there is still a tendency to look upon the manufacture of species and genera as the *ultimum bonum* of the systematist, the mere possibility of such a rule obtaining currency must have a tendency to foster superficiality, incompetence, and ignorance. While I do not agree wholly with those who look upon the genus as an abstract thing, over and above types, I do protest strongly against the acceptance of a rule that will relieve the namer from the necessity of knowing anything about the things he names.

The fear of evil results is not a groundless one. Some years ago an Italian writer, with an assurance as boundless as his ignorance, brought forth a new "system" of dipterological classification, with hundreds of new names. Not the slightest attention has ever been paid to his "system;" but, with this rule in force, one would be bound to torture himself in trying to unravel its vagaries. The careless writer should have no such rule, the careful writer needs none. S. W. WILLISTON.

University of Kansas, Oct. 18.

Solid Glycerine.

CAN you inform me, through your magazine, by what chemical, or by what process, glycerine may be solidified, retaining its transparency? Can any reader answer? C. C. SMITH.

New York, Oct. 31.

BOOK-REVIEWS.

Fourteenth Annual Report of the State Board of Health of the State of Connecticut for the Year Ending November 30, 1891. New Haven, 1892.

THIS report presents fresh evidence that the work undertaken by the various State boards of health is steadily increasing both in scope and in value. This encouraging condition of things has been brought about largely by the adherence of several States to the policy of employing competent expert service. The authorities of these States consider that scientific problems can be successfully attacked only by the most advanced scientific methods, and have in consequence availed themselves of the aid of highly trained chemists, biologists and engineers. A great impetus has been given in this way to the best kind of sanitary work.

The Connecticut report contains, besides the usual reports from local boards of health and the annual statistics of births and deaths, several special features of more than ordinary importance and interest. Dr. H. E. Smith presents a special report upon "The Origin of Certain Cases of Typhoid Fever from Money Island." Twenty-one cases of typhoid, one of which proved fatal, were traced to the contaminated water used at a hotel on Money Island. From . . . facts concerning the sources of the water used, it appears that during the period August 11 to 14, at which time all of those subsequently taken ill were at the inn together, the drinking-water was obtained from the billiard-hall cistern." Dr. Smith shows further that abundant opportunity existed for the infection of this particular cistern water, and adduces convincing evidence that the water was actually infected by a case of "walking typhoid," and that the water thus infected spread the disease.

Dr. L. S. DeForest, in his article upon "Tuberculosis as a Local and Contagious Disease in New Haven" discusses the interesting question of infected dwellings. Dr. DeForest found from the data of 1876–1890 three principal districts of concentration of tuberculosis in New Haven. From a detailed study of house cases he arrived at the conclusion that houses sometimes became true foci of infection. "We think that the accompanying maps and tables go far to show that consumption is endemic in certain parts of the city; that in these parts there are many houses in which it is distinctly dangerous to live." The value of Dr. DeForest's interesting paper would be considerably enhanced by the addition of exact references to the writings of Flick, Cornet, and the other workers in this same field.

The report of the "Examination of Certain Connecticut Water Supplies," by Drs. Samuel W. Williston, Herbert E. Smith, and Thomas G. Lee, covers some two hundred pages and is illustrated with a number of well-arranged charts showing the monthly variations of the analyses. In some respects the report merely confirms the previous work of the Massachusetts State Board of Health, but in other respects it improves upon and extends the latter. Fifteen different water supplies were selected for study, and monthly examinations were made of most of these during a period of twenty-three months.

The special report on the chemical examinations is by Dr. Smith, who in his methods follows closely the chemists of the Massachusetts staff. He, however, expresses his results in milligrams per litre rather than in parts per hundred thousand, and makes a few other minor clerical changes. The limited resources at his command did not permit him to take up carefully the interesting and important question of "normal chlorine;" but his chlorine determinations, so far as they go, support the work of