

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 epitheliomatous; 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 amoeboid organisms; but, as the process is so long, my colleague persuaded me to send you these remarks.

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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.

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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 *sakal*, "forme, figure, dont les Grecs ont fait *σικλαι*," 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.

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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