

Now the alarming discovery has been made that a great many of what we have heretofore called "palæolithic implements" display with fatal clearness the peculiar earmarks of these "quarry-rejects," hinting, therefore, that they never were real implements at all. What is worse, like the rejects, they show no signs of use, and clearly never could have been employed as implements, and consequently do not in any way illustrate the industry of the chippers, no matter of what age they are. If found in gravels, the gravel-bed was the quarry, and they the refuse. It has even been hinted that the famed gravels of the Somme and the "palæolithic floors" of the Thames and the "Trenton gravels" of our own land, may have to lose their laurels in the light of this discovery.

#### The Builders of the Southern Mounds.

Those who have looked at the archæological collections of the Smithsonian with any attention, cannot fail to remember the extraordinary specimens of copper work from the Etowah valley mounds, in northern Georgia. The figures they delineate have an unquestionable family resemblance with those inscribed on shells obtained on the lower Mississippi, so accurately presented in Mr. Holmes's essay in the Report of the Bureau of Ethnology, 1880-81. Both present curious analogies to Mexican and Maya art, and I have been almost constrained to believe in a connection, either ethnic or commercial, between these peoples.

Dr. Eduard Seler, however, who is a most competent authority on these questions, expresses a different opinion in a recent article in *Globus*, Bd. LXII., No. 11. He analyzes with care the mode of wearing the hair, the headdress, the clothing, and the weapons of the figures, and shows that in several of these points they correspond with the descriptions of the early voyagers of the natives they found in these localities. He also compares the same features with similar relics from ancient mounds in the Ohio valley. The conclusions he reaches are, that the builders of the Etowah mounds and the artists of the inscribed shells were probably related to the builders of the Ohio mounds; that they were not the direct ancestors of the tribes found in Georgia at the discovery; that there is not sufficient reason to suppose connection with Mexico or Yucatan; that probably the mound-building and copper-working tribes were destroyed or driven to the remote sea-coast by invasions from the north and west at a period not very remote from that of the discovery of the continent.

#### 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.**

#### "Ancient Mexican Heraldry."

PERMIT me to place before the readers of *Science* an interesting fact kindly communicated to me on Oct. 17 by the Rev John Woodward, LL.D., F.S.A., the learned author of "A Treatise on Heraldry, British and Foreign," referred to in my article on "Ancient Mexican Heraldry" in *Science*, No. 503, Sept 23. That gentleman has just informed me "that the late Mr. Ellis, in his book on 'The Antiquities of Heraldry,' 1869, made reference to the Armory of the Ancient Mexicans and gave a plate (iv.) of the symbols from the works of Lord Kingsborough and Mr. Stephens." "There is not, however," my courteous correspondent adds, "any evidence that the use of such devices was hereditary; they were, so far as we know, merely personal distinctions." This agrees with the general conclusions, based on special investigations, of Mrs. Zelia Nuttall, who was doubtless, like myself, unaware of the publication of Mr. William Smith-Ellis's views on Mexican heraldic emblems nearly a quarter of a century ago. I endeavored in vain to consult a copy of his work when studying the subject of my communication to *Science*; and, as other students may experience the same difficulty, I will reproduce the Rev. J. Woodward's observations respecting ancient heraldry on page 26 of the Introduction to Vol. I. of his recent Treatise on Heraldry in general.

"Mr. W. G. (sic) Ellis, in his 'Antiquities' of Heraldry," has collected a mass of interesting matter relating to what he calls the heraldry of ancient times and of all nations of the world, and he certainly succeeds in showing to how great an extent pictorial symbols, which had originally a meaning, have been in use among all nations of mankind, civilized and savage. His plates are curious as showing the occasional occurrence among these manifold devices of some resembling modern figures of blazon. The crescent, the mullet, the lozenge, the quatrefoil, and the fleur de lis are traced by him to counterparts existing among Egyptian, Chinese, Indian, and Japanese emblems; and among the figures on Etruscan vases he shows us what, in heraldic language, would be called a bull's head caboshed and a not unheraldic-looking demi-boar."

Furthermore, it is noteworthy that the Rev. John Woodward considers "there is some reason to believe that the use of the hereditary badge must have long preceded hereditary heraldry" (p. 589). Additional instances of their use as military distinctions rewarding the capture of prisoners in European warfare may be cited from his interesting work. Two fleur de lis with other "augmentations" were granted to Sir John Clerke because he took captive Louis Duke de Longueville on the field of Therouenne, better known as the battle of the Spurs, and fought a month before the eventful fight of Flodden. A still more interesting case is that of the "Crampet," or metal termination of the scabbard of the sword, sometimes borne as a device. One was assigned to Sir Roger La Warr about the same time as the buckle was granted to Sir John Pelham in recognition of his aid in the capture of King John of France. It is somewhat remarkable that the descendants of these gallant knights, who fought side by side at Poitiers, still hold lands and earldoms in the same County of Sussex. Some members of the ancient house of De la Warr passed over to America in the fifteenth and sixteenth centuries, hence the more familiar name of Delaware.

AGNES CRANE.

Brighton, England, Oct. 21.

#### Reticulated Protoplasm of *Pelomyxa*.

SINCE the publication of Dr. Stokes's article myself and colleague, Mr. W. F. Pentland, have had several opportunities of examining *Pelomyxa Palustris*. The difficulties of the investigation are so great that at his suggestion I tabulate the methods and their results.

1. Examination of unstained preparations (50 slides). Utterly useless as far as the detection of reticulation is concerned.
2. After treatment with osmic acid, usually 1 per cent solution, one organism was found destitute to a great extent of ingested material. An  $\frac{1}{2}$  Powell and Leland water-immersion and Zeiss 12 compensating ocular failed even with critical light, with an immersion condenser, and ammonia sulphate of copper solution as modifier, to detect the slightest trace of reticulation.
3. In the same preparation we found some *Amœbæ* resembling *Proteus*. The coarseness of the enclosure in these specimens we found would lead a neophyte astray, as it frequently resembled reticulation.
4. Determined to leave no stone unturned, we tried the usual aniline and carmine dyes, with no result.
5. One-half per cent solution of chloride of gold (no osmic acid) was tried on over twelve slides. I must certainly confess I glimpsed reticulation in two specimens, but owing to the protracted investigation was compelled to desist.

So far with regard to amoebic organisms. It is in the field of pathology that reticulation of protoplasm is most frequently observed, as far as my experience goes. The more rapid the morbid process the greater certainty of reticulation. Fifteen years ago I was working with my lamented colleague, Dr. Booke of Dublin, on the effects of bichloride of mercury on blood corpuscles; but fortunately we did not follow out Dr. Klein's suggestions to the letter, as we found epithelium cells beautifully reticulated as described by Dr. Stokes. We found the nucleus in the blood discs, but, as usual in scientific investigations, forgot to look for reticulation. However, on a future occasion the late Sir Robert Macdonald submitted to us for examination a portion of tumor of then unknown nature. The portion was placed in Muller's

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.

A. COWLEY MALLEY.

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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.

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 *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