quickly followed by re-absorption by the roots, or to (*b*) the excreted substances in the soya bean differing from those in the types examined by Virtanen in that they are volatile—the sand was dried at 30°–40° C. prior to analysis. These two and other possibilities are being examined.

The earlier work of Stallings[4], who concluded that there is an excretion from soya bean nodules of ammonia, presumably in organic or inorganic combination, will be considered in a later fuller publication, together with other literature.

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March 23.

[1] Virtanen, A. I., NATURE, **138**, 880 (1936).
[2] Virtanen, A. I., *J. Soc. Chem. Ind.*, **54**, 1015 (1935).
[3] Bond, G., *Ann. Bot.*, **50**, 559 (1936).
[4] Stallings, J. H., *Soil Science*, **21**, 253 (1926).

The Crayfordian and Boltonian Industries

THIS note refers to the nomenclature of the prehistoric flint industries found in (*a*) the basal gravel at Crayford and of the Ebbsfleet Channel, Kent, (*b*) the Upper and Lower floors in Bolton and Co.'s brickfield, Ipswich, and (*c*) similar floors in the subaerial loams of the Bean Valley and Ingress Vale, Kent. We propose in future to describe the specimens classified under (*a*) as *Crayfordian*, and those classified under (*b*) and (*c*) as *Boltonian*.

From a prolonged examination of the industries mentioned, we are satisfied that they cannot, without prejudice, be included in any of the ordinary subdivisions of prehistoric flint implements. Thus, the industries in question each includes Clactonian, Levalloisian, and hand-axe features, and it is not possible, therefore, to describe them by the usual nomenclature. The characteristics and geological age of these industries will be described before long, but we think it desirable that their new titles, which we hope will be generally adopted by archæologists, should be made public now.

We would point out that the specific names *Crayfordian* and *Boltonian* are based solely upon the characters of the flaked flints of the two industries, and do not connote any particular geological age for the specimens.

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Magnetic and Electrical Dimensions

MY chief reason for desiring to reply to Prof. Howe's letter on this subject in NATURE of March 13 is that the last paragraph of the letter gives the impression, inadvertently I presume, that the implications of the adoption of Ampère's theory of magnetism had been considered fully by the International Committee when they made the recommendation that B and H were to be considered respectively in the roles of effect and cause, and their ratio μ therefore as an entity having physical dimensions. This is not the case.

Prof. Howe and I and all other members of the British Association Committee upon the subject were invited in 1934 to attend the meeting of the International Committee in London by the courtesy of the late Sir Richard Glazebrook, chairman of the International Committee. We were all made parties to the above recommendation, and at the time there seemed to be no alternative to an arbitrary resolution, although had we studied Maxwell's treatise more carefully we might have been more cautious.

It was not until January 19, 1935, that I showed in a letter to NATURE that the adoption of Ampère's theory opened up an alternative mathematical solution which makes μ undimensional, and I followed this by a paper to the British Association meeting at Norwich confirming this result and showing that I got complete support from Maxwell.

I have received a large correspondence about the conclusions of this paper. In September 1935 I received from Sir Richard Glazebrook a letter in which he writes: "To my mind Maxwell's theory, as developed in his book, is based on the two-fluid theory of magnetism. I have not considered how far the adoption of Ampère's theory, which no doubt is nearer the truth, modifies this but will consider the matter when I have a little leisure." It is greatly to be regretted that Sir Richard's untimely death deprived us of his considered opinion upon this important question.

From my correspondence with Prof. Howe, I can see that his main objection from the first to my conclusions has been the mathematical nature of the arguments. To my mind, the whole electrical science is the mathematical machine which he derides in his last letter, that wonderful machine which has produced the electromagnetic theory of light, to which is due the discovery of the quantum theory and all the other developments of modern theoretical physics. All that is fed into this machine consists of the empirical laws and the definitions of the various entities and constants. Now Prof. Howe would have us jettison the machine and the unit pole upon which several of our definitions are founded, and does not propose to provide us with equivalents. Even if equivalents were ready for adoption, is there any possibility of getting them adopted by the scientific world in the month or two before the fate of the dimensions of μ is decided?

Dealing with the purely physical character of μ, consider the coils of a wattmeter. If the current coil is energized, we get a magnetic field surrounding it, which we denote by H at every point. If now we energize the voltage coil, the nature of the magnetic field has not altered but only its magnitude and direction. If, on the other hand, we introduce a little piece of iron instead of energizing the voltage coil, this little piece of iron, according to Ampère's theory, consists of a multitude of atomic electrical circuits, each of which is equivalent to a voltage coil. In what respect does the physical nature of the magnetic field then differ from that which existed when the voltage coil was excited? According to the two-fluid theory of magnetism, they are different *in the iron*, but now that the two-fluid theory is discarded and there is only one medium to be considered, namely, empty space, is there any reason for maintaining the results of a discarded theory? According to Ampère's theory, B and H differ only in magnitude and direction, but are physically identical.

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