The Origin of Magnetism.

WHEN the proposal was first made to hold in Section A of the British Association at Hull this year a discussion on "The Origin of Magnetism," it was met with the criticism from eminent quarters that the time was not yet ripe for the consideration of this subject. Those who attended the meeting will probably agree that this view was justified, for it can scarcely be said that the position was advanced appreciably, or that any real, or even plausible, answer was given to the main question involved. Perhaps this was in some measure due to the regrettable absence of Prof. Langevin, who had promised to make the opening remarks, and had expressed his intention of using the opportunity for a critical survey of the whole subject. But a recurrence of the ill-health from which he has intermittently suffered for a long time deprived the Section of Prof. Langevin's presence and his eagerly anticipated contribution to the discussion. As it was, the discussion lacked co-ordination; the remarks of the various speakers bore little relation to one another. There was the exposition by Prof. Weiss of his theory of the molecular field and the existence of magnetons; then Sir J. A. Ewing's description of his new molecular magnet models; then the remarks of Dr. A. E. Oxley on the changes of susceptibility imparted to platinum and palladium by the occlusion of hydrogen; and, finally, an account by Mr. L. F. Bates of the measurements of the Richardson effect recently carried out by Dr. Chattock and himself,—all contributions of considerable individual interest, but not closely related to one another nor providing an answer to the essential question of the *origin* of magnetism.

In spite of the comparative failure of the discussion in its wider aspects, one felt that the time had not been wasted, principally because it afforded an opportunity for Prof. Weiss to give a most interesting account of his work in connexion with ferromagnetism and paramagnetism, which is not too well known in this country. Prof. Weiss at very short notice undertook to open the discussion in place of Prof. Langevin, and a fairly complete account of his remarks will eventually appear in the Report of the Association. An outline of this exposition may be profitable here.

Starting from the analogy of the difference between the laws of fluid compressibility for low and high densities, Prof. Weiss showed how Langevin's kinetic theory of paramagnetic substances may be modified so as to include strong magnetism—or ferromagnetism—by the assumption of the existence of a *molecular field* analogous to van der Waal's internal pressure in fluids. A whole array of experimental facts was brought forward in support of this theory of the molecular field. It provides an explanation of the variation of magnetic saturation with temperature; it accounts precisely for the transformation of ferromagnetism to paramagnetism at the temperature of the Curie point, and for the observed law of this paramagnetism. The theory also points to a discontinuity of specific heat at the Curie point, and the magnitude of the discontinuity, calculated from magnetic data, agrees with calorimetric measurements. Still more interesting is the recently discovered magneto-caloric phenomenon, which consists of a reversible temperature variation accompanying magnetisation. This differs from the ordinary hysteresis effect, which is irreversible, and always involves heating. In the reversible effect, magnetisation produces a rise of temperature and demagnetisation a fall. At the Curie point the change is by no means negligible, reaching, as it does, a value of about 1° in fields readily attainable. The extent of temperature variation calculated by means of the molecular field theory agrees with that observed.

When one comes to calculate from various experimental data the numerical value of the molecular field, it proves to be of the order of magnitude 10^7 gauss, which is far in excess of the magnetic field which might in the most favourable circumstances be produced by the magnetic moments of the molecules of a ferromagnetic body, namely, 10^4 gauss. This remarkable result indicates that the so-called molecular field has not itself a magnetic origin. In this connexion Prof. Weiss's own (translated) words are worth quoting:—

"It is therefore impossible for the mutual actions represented by the molecular field to be of a magnetic nature. It is just a notation for forces of a non-magnetic character, with a symbol borrowed from magnetism. I prefer, in place of the primitive definition given earlier, the equivalent definition

$$H_m = -\frac{\delta U}{\delta I},$$

where U is the intrinsic energy per unit volume, and I the intensity of magnetisation. This definition is advantageous in that it does not prejudge the nature of the forces. . . . It does not appear to be impossible that the forces may be electrostatic; that, however, is at present a pure supposition."

In the second part of his address Prof. Weiss directed attention to another important aspect of the combined kinetic theory of Langevin and his own theory of the molecular field. The possession of these theories permits the calculation of the values of the molecular or atomic magnetic moments which have been the underlying assumption in all theories of magnetism. A great number of atomic moments have thus been evaluated from many experimental sources, such as the measurement of the magnetisation of ferromagnetic substances and their alloys both in the neighbourhood of absolute zero and above the Curie point, the investigation of the paramagnetism of solutions of salts, and the like. The general law which emerges is that "all atomic moments are integral multiples of the same elementary moment, to which the name *magneton* has been given." For example, six different and independent observers have found for nickel, over a temperature interval of about 400°, 8·03, 7·99, 8·04, 8·05, 8·03 and 7·98 magnetons respectively, numbers which, it will be seen, are in the immediate neighbourhood of the integer 8. It is, besides, a general property of atoms to possess different integral numbers of magnetons according to various conditions, such as their state of chemical combination, or their temperature, whether in the ion, or in the undissociated molecule. Prof. Weiss affirms that the magneton is a real entity, and he pointed to the fact that the Rutherford-Bohr atom, together with Planck's quantum theory, actually does indicate the existence of a universal elementary magnetic moment, which, however, proves upon calculation to be almost exactly five times as great as the magneton.

Prof. Weiss's general conclusions may be summed up by quoting him again:—

"1. One of the essential conditions for the production of strong magnetism—or ferromagnetism—is the existence, between molecules possessing magnetic moments, of important mutual actions which are numerically expressed by the molecular field, and are certainly of a non-magnetic nature.

"2. The appearance of atomic moments as integral multiples of the same elementary moment—the magneton—is thus one of the important aspects of magnetic phenomena."

Altogether a convincing exposition, in spite of Sir Ernest Rutherford's amusing allusion to the fascination which *whole numbers* have for physicists.

A. O. RANKINE.

Man and the Ice Age.

OF the many discussions which took place during the recent meeting of the British Association at Hull, few are likely, on purely scientific grounds, to prove of more importance than that on the relation of man to the ice age in Britain, in which the sections of geology, geography, and anthropology took part. It cannot be said that any agreement was reached; but the significance of the discussion lies in the fact that protagonists of different schools of thought in geology were brought face to face, while archæologists and geographers were able to formulate and lay before them problems for the solution of which they await the assistance of geologists. In considering the problems of the ice age, geologists and archæologists are dealing with the same material, but each from their special point of view. The result has been a difference in nomenclature and method of classification: the geologist thinks in terms of the deposits; the archæologist in terms of the artefacts found in them. Consequently, as Prof. P. F. Kendall pointed out, any discussion between them is likely to come to a deadlock through disparity of nomenclature. This discussion, however, showed that the difficulty is by no means insuperable.

It was apparent at an early stage in the discussion that there existed a clear-cut difference of opinion as to the method of approach in attacking the problem. Indeed the title of the discussion, in suggesting a restriction of the subject matter to Britain, was a challenge which Prof. W. J. Sollas was not slow to take up, when at the outset he maintained that it was impossible to consider the evidence in Britain apart from conditions on the Continent. Prof. Kendall, on the other hand, held that not merely must consideration be confined to the evidence as it is presented in the British area alone, but that the solution of the problem must be sought in East Anglia in the relation of the northern drift to the chalky boulder clay. On this point, Prof. Kendall's lucid summary of the evidence gave his audience a clear indication of the nature of the problem and of the extent to which the British data may be expected to throw light upon the problem as a whole. It turns to a great extent upon the view which is taken of the relation of the glacial deposits of Yorkshire to those of East Anglia. The chalky boulder clay of East Anglia was carried down by ice from north of the Wash and the fens. In Yorkshire there is a clear glacial sequence of at least three boulder clays, in the lowest of which is a Scandinavian element. In Prof. Kendall's opinion the hope of correlating the Yorkshire evidence with that of East Anglia is to be found in the Wolds, on the west of which is found the purple clay of Yorkshire, and on the east, the chalky boulder clay. Was it possible, he asked, that the latter might be the purple clay transformed by its passage over the Wolds?

The trend of the discussion was to show that the archæological problem is narrowing down to the question of the relation of the gravels containing Chellean and Acheulean implements to the boulder clay, a definite issue for solution by excavation. At Hoxne, such implementiferous gravels were found to overlie a boulder clay, but the evidence is by no means entirely conclusive and appears to conflict with that from elsewhere. Prof. Boswell had hoped to be in a position to place before the sections the results of excavations undertaken to determine this point, but, unfortunately, they had not been completed in time. On the other hand, Mr. Hazzledine Warren showed himself an uncompromising opponent of anything but a post-glacial date for the palæolithic gravels, on the ground that they are conformable to the holocene alluvium, a condition which would be impossible had they been subjected to glacial action. The general disposition appeared to be, however, that further evidence on this clear crucial point must be awaited. On the whole, this would appear to be in agreement with the tendency of the opinions which have been elicited by the British Association Committee appointed at the Cardiff meeting to report on the relation of early types of palæolithic implements and glacial deposits. Of these some have appeared in *Man*; others await publication.

The interest of archæologists and geographers, however, is not bounded by the position of man in relation to glacial deposits in this country. They would wish to know how far conditions in this country can be equated with conditions in the Continental area, extending this term to include North Africa, and how far it is possible by geological evidence to link up the palæolithic cultures of this country with the cultures of these areas. They welcomed, therefore, the opening remarks of the president of the anthropological section, Mr. Peake, in which he referred to the tentative scheme for effecting this which he had put forward,[1] and the pronouncement of Prof. Sollas that the British evidence could not be considered apart from the Continental evidence. Prof. Sollas ably summarised Penck's views, and pointed out how the differences between the French and German geologists might be reconciled—differences however, which did not affect the question of the geological age of man. Penck's four great periods of glaciation in the Eastern Alps could be correlated with the river gravels, while in France glaciation could be brought into relation with raised beaches. As a result of such a correlation, it appeared that the Chellean implements belonged to a warm period, the Riss-Würm, the Mousterian straddled the Würm, and the Aurignacian and later phases of palæolithic culture were post-Würm.

The point of view of the archæologist and geographer was well put by Prof. H. J. Fleure. The archæologist in particular has arrived at certain conclusions on purely archæological evidence, for which he looks to the geologist for confirmation or the reverse. Prof. Fleure pointed out that the three centres of glaciation, Scandinavia, Britain, and the Alps, could not be considered apart. Any change in the distribution of ice in one area was bound to affect the climate and distribution of ice in the others. It was therefore incumbent upon the geologists to produce a scheme applicable to all areas.

An interesting question to which Prof. Fleure alluded is raised in the relation of the Bühl period, which was marked by a readvance of the ice, to the conditions in Scandinavia described by de Geer. The study of climatic conditions may also be expected to throw light upon the problem. Prof. Fleure pointed out that a constant anticyclone over the

[1] *Man*, 1922, No. 5.