form for the law rather than determining it by mathematical development, which the state of experimental knowledge at the time could scarcely justify.

In the more recent development of Weber's ideas the question has been again raised whether the internal molecular magnetic field is sufficient to account for observed phenomena. Thus Weiss, who adopted Weber's assumption regarding the internal field to fit it for application to crystalline media, was led by thermomagnetic phenomena to ascribe very high values to the internal field relatively to even strong external fields. He afterwards pointed out that the high values may include equivalent values of fields which are actually non-magnetic, but may, for example, be electrostatic if the molecular magnet is also an electric dipole; and this view leads to values of the molecular electric susceptibility which are consistent with results of observation.

Now an application of Weber's theory to a determination of the actual law of force, due to the mutual actions of the molecular magnets in a homogeneous crystal, readily indicates that the magnitude of the internal field is of the same order as that of fields which are normally used in the investigation of the magnetic properties of substances. It shows, even without numerical evaluation, that the least possible value of an external field which is able to magnetise a cubic crystal in any direction relative to its crystalline structure is equal to five-eighths of the maximum internal field. That is to say, the maximum internal field acting upon a molecular magnet is not twice as strong as the external field which is just able to turn the molecular magnets out of their stable directions, and so to magnetise the crystal in any direction. *This is true whatever be the nature of the internal directive field which tends to maintain the magnets in their stable positions.* If that internal field has in part an electrostatic origin, the remaining magnetic part is proportionately smaller.

This is in accordance with the observations, described in a recent issue of NATURE (Mar. 5, p. 353), on the deflexion of β-particles in their passage through thin magnetised nickel foil.

If we postulate that there is equipartition between the average translational energy per degree of freedom of the molecules and the average rotational energy of a molecular magnet, the axis of which is maintained, in consequence of the heat motions, on the average at an angle ϕ with the direction of the resultant field, we find, on evaluation of the internal field, that, at ordinary temperatures, this postulate is not satisfied. The change of potential energy of a molecular magnet, due to the rotational effect of heat motions, amounts only to about 1 per cent. of the energy per degree of freedom. This seems to indicate that the internal structure of the molecule is such that, in the collisional interchange of energy amongst molecules, only about 1 per cent. of the whole is communicated to the subatomic portion of the structure which is concerned with the manifestation of magnetic quality.

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Evolution: Emergent and Resultant.

THE recent articles by Dr. P. Chalmers Mitchell and Prof. C. Lloyd Morgan (NATURE, May 21, p. 748, and May 28, p. 786) clearly show the increasing importance of the problem of emergence. But it seems to me that Prof. Morgan advances a criterion of emergents which is seriously defective, and so prevents any reconciliation of the opposed viewpoints. In the first place, he appeals to "matters as they now are," and quite apart from what future discovery may reveal. It follows, therefore, that as knowledge expands, much that is now regarded as emergent may prove to be resultant, since it will become deducible from the phenomena of some "earlier phase"; and to this progress no limits can be assigned in advance.

This attitude is plainly an appeal to the ignorance which prevails at any given moment; and it at once destroys any *absolute* distinction between the emergent and the resultant. Now the trend of research, in my opinion, undeniably involves this loss of absoluteness, as Dr. Chalmers Mitchell maintains. For while it will always be impossible to deduce the macroscopic qualities of combinations from the macroscopic qualities of their elements, the more complete knowledge of microscopic and ultra-microscopic qualities does enable the qualities of combinations to be both explained and predicted. In this respect success depends on the capacity of the inquiring mind; so that as mind evolves, emergents must give way to resultants. If, for example, we accept Prof. Morgan's criterion, then to Galileo electromagnetic storms, due to solar radiation, would be emergent, while to us they are resultant. Similarly, many of the phenomena presented by vitamins, not being as yet deducible, are still emergent, but will probably be resultant for future bio-chemistry.

The criterion of being, or not being, deducible is thus wholly relative and transient; and it obscures what I take to be the sole genuine attribute of all emergents, whether deducible or not; that is, uniqueness, or the possession of characters previously unprecedented. From this more inclusive and permanent viewpoint, atoms and crystals emerged, exactly as did life and sentience at still later stages; and this quite apart from the partial, or complete, explanation of their origin. For each of these was, when it first appeared, in its own specific way unique, exactly as "Hamlet" would remain unique even though it could be fully accounted for in terms of Shakespeare's life and character. Such absolutely unique combinations occur, of course, throughout the entire universe, and present one of its most marvellous and significant features. So that although "out of three sounds he frame, not a fourth sound, but a star," still

A star's a star for a' that.

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IF there be a valid distinction between resultant and emergent advance the question arises: How may this distinction be expressed with precision and clearness? One way of expressing it is that developed by Dr. Broad. It comes to this. There are certain integral wholes, composed of constituents in specific relations, of which it may be said that their characterising properties are not deducible from the most complete knowledge of the properties of the constituents taken severally in isolation, or taken collectively in some other set of specific relations. Such a whole is said to be emergent. The theory of emergence is on trial. Of it Dr. Broad says that "it is a matter of controversy whether it actually applies to anything"; but he adds that it embodies "a logically possible view with a good deal in its favour." If, then, the theory be on trial as a scientific proposition —and such it purports to be—it must, I submit, be tried out on the basis of existing scientific knowledge. I should not designate this as an appeal to ignorance.