IX. "On the Existence of a Magnetic Medium." By T. A. Hirst, Esq. Communicated by Dr. Tyndall, F.R.S. Received April 14, 1855.

In a note on the above subject, communicated to the Royal Society on March 16, 1855, Professor Williamson objects to a certain conclusion deduced by Professor Tyndall in a letter addressed by the latter to Mr. Faraday and recently published in the Philosophical Magazine. Professor Tyndall's conclusion was that, according to the hypothesis of the existence of a magnetic medium in space and of the identity of magnetism and diamagnetism, a compressed diamagnetic cube ought to be less repelled when the magnetic force acts on the line of compression than when it acts at right angles to that line; a result which his own experiments have contradicted. Against the legitimacy of this conclusion Professor Williamson urges that "Dr. Tyndall seems to have assumed, that on the compression of an aggregate of particles of a diamagnetic substance, the medium is not displaced by the particles in their change of position." We shall be better able to estimate the value of this objection by recalling the steps of Professor Tyndall's argument.

A magnetic cube was taken which had *already* been compressed: its deportment before a magnet was experimentally examined, and deductions drawn concerning the changes that would occur in that deportment by merely conceiving the magnetic capacity of the material particles to be diminished, without in any way altering the distances between those particles, and consequently without in any way displacing the magnetic medium in the interstices of the body.

Instead of the assumption attributed to Dr. Tyndall, he might, with greater justice, be accused of having disregarded the possible presence of the medium within the body; but in his own defence he may with perfect justice reply, that in Mr. Faraday's experiments, which originally gave rise to the discussion, no such interpenetration of two media existed.

Admitting, however, that the interstices of a body are occupied by the medium, it may be interesting to inquire whether, from an argument similar to Professor Tyndall's, the same decided conclusion could, with equal accuracy, be deduced. To answer the in-

quiry, it must be remembered that the force of the argument in question depends essentially upon the justness of the supposition, that a diamagnetic cube may, theoretically, be produced from a magnetic one by conceiving the magnetic capacity of the particles of the latter to be sufficiently diminished. It is evident that the total attraction of the cube by a magnet will be equal to the sum of the attractions of the material particles, and of the medium contained in its interstices. If this sum be greater than the attracting force upon the quantity of medium which the cube *and its contents* displace, the substance is called magnetic, for it will be drawn towards the magnet; if less, it is called diamagnetic, for it will be repelled from the magnet. But in our present knowledge of the properties of the medium there is nothing incompatible with the supposition that the density of the internal medium may so far exceed that of the external, that the attraction of the former by the magnet is *itself* greater than the attraction of the medium displaced by the cube and its contents. If so, however, no conceivable diminution of the magnetic capacity of the material particles could possibly render such a cube diamagnetic.

This is sufficient to show that, admitting the presence of the medium within the cube, the method of argument adopted by Professor Tyndall would not be strictly applicable, unless the density of the internal medium were subjected to limits which the advocates of its existence might possibly be unwilling to grant.

But it may be asked, if, whilst admitting that the medium may exist in the interstices of the body, it be granted that a diamagnetic may be produced from a magnetic cube in the manner assumed by Professor Tyndall, does it still follow, necesarily, that attraction is always greatest—repulsion least—when the force acts in the line of compression? In other words, can a conclusion contradictory to experimental facts be *then* legitimately deduced?

In attempting a reply to this question, it will, perhaps, be best to employ the following symbols. Let W represent the attracting force of the magnet upon the medium displaced by the cube and its contents. The value of W will, of course, be unaltered, no matter whether the force acts in, or at right angles to the line of compression. When the force acts in the line of compression, let P_1 represent the attracting force upon the particles, W_1 the attracting force

upon the internal medium, and let F_1 be proportional to the resultant attraction of the cubical mass towards the magnet. Let P_2, W_2 and F_2 have similar significations when the force acts at right angles to the line of compression. Then we may put

$$F_1 = P_1 + M_1 - W,$$
$$F_2 = P_2 + M_2 - W.$$

Now, in a compressed magnetic cube, experiment proves that

$$F_1 > F_2,$$

or $$P_1 + M_1 > P_2 + M_2,$$

i. e. $$P_1 - P_2 > -(M_1 - M_2).$$

As long as we are ignorant of the properties of the medium within the body, we will, for the sake of completeness, consider the following three distinct cases.

I. The attracting force upon the medium within the cube is the same when the force acts in either the one or the other of the two directions, with respect to the line of compression. Here

$$M_1 = M_2,$$

hence $$P_1 > P_2.$$

II. In whichever of the two directions of the force the attraction of the particles may be greatest, the attraction of the internal medium is also greatest in the same direction. Here, according as P_1 is greater or less than P_2, M_1 is greater or less than M_2; hence, inasmuch as

$$P_1 + M_1 > P_2 + M_2,$$
$$P_1 > P_2 \text{ and } M_1 > M_2.$$

III. In whichever of the two directions of the force the attraction of the particles may be greatest, the attraction of the internal medium is greatest in the direction perpendicular to the same. Here, according as P_1 is greater or less than P_2, M_1 is less or greater than M_2, so that the two hypotheses

(1) $P_1 > P_2$ and $M_1 < M_2$, and

(2) $P_1 < P_2$ and $M_1 > M_2$

are both compatible with the sole condition,

$$P_1 + M_1 > P_2 + M_2.$$

In order to test the applicability of Professor Tyndall's method of argument in each of these cases, let us conceive, with him, that the magnetic capacity of the particles is so far diminished that their attractions P_1 and P_2 are reduced to p_1 and p_2. This may evidently be done by making $p_1 = aP_1$ and $p_2 = aP_2$, where a represents a positive fraction whose magnitude can be diminished indefinitely. If, under this supposition, the resultant attractions F_1 and F_2 become f_1 and f_2, then it can easily be proved that, however small the value of a may be, we shall always have

$$f_1 > f_2$$

in the cases I., II. and III. (1). That is to say, with the distributions of the internal medium assumed in these cases, the resultant attraction of the cubical mass will always be greatest, or repulsion least, when the force acts in the line of compression, no matter how diamagnetic the cube may have become by diminishing the magnetic capacity of its particles. It may just as easily be proved, however, that in case III. (2) a value of a may be chosen sufficiently small to make

$$f_1 < f_2,$$

that is to say, with the distribution and properties of the internal medium here supposed, it is quite possible so far to diminish the magnetic capacity of the particles as to obtain a cube which will be attracted least, or repelled most strongly when the force acts in the line of compression. This conclusion involves nothing contradictory to experimental facts, whereas the former one does.

I will not here enter into the question of the relative probability of these three cases, supposing the medium to exist. My sole object has been to show that, although the method of argument adopted by Professor Tyndall is strictly applicable in a great number of cases, even when the medium is supposed to fill the interstices of the body, yet it is possible to attribute properties to this medium of such a nature as to avoid the conclusion, contradictory to fact, which he has deduced. This may be done in two ways. *First*, the density of the internal medium may be such as to render it impossible to produce a diamagnetic from a magnetic cube in the manner assumed, *i. e.* by diminishing the magnetic capacity of the material particles. *Secondly*, granting that a diamagnetic cube may be so

produced, the distribution and properties of the internal medium may still be such as to cause the cube to be attracted least, or repelled most strongly when the force acts on the line of compression, and thus, if the substance be diamagnetic, to cause it to agree, in its deportment, with experimental results. On the other hand, if these hypothetical properties of the internal medium be discarded as artificial or inadmissible, then at present I see no way of escaping the conclusion of Professor Tyndall's argument.

With regard to the explanation given by Professor Williamson, it will be observed that he pursues a path quite different from that of Professor Tyndall, when he considers the effects produced by compressing a number of particles surrounded by a magnetic medium. This compression, he states, may alter the attraction of the mass by a magnet in two ways;—"first, by altering the density of the matter; secondly, by altering the density of the medium." By the term '*density of matter*' is usually understood the relation which exists between the quantity of matter which a body contains, and the volume of the space enclosed by its external surface. But in the present case, where a comparison is instituted between the matter of the body and the medium which is supposed to fill all its pores, we must, I imagine, understand by the term 'density of matter,' the relation which exists between the sum of the masses of the particles, and the sum of their volumes; but if so, then, the particles themselves being incompressible, it is clear that compression could not alter the 'density of matter.'

As to the second effect of compression, viz. an alteration of the density of the medium, it may be quite conceivable, although I do not find that Professor Williamson has any where taken it into consideration. The effects of compression may, therefore, be more correctly described as either—*first*, a diminution of the interstices of a body, without altering the density of the medium which fills them; or *secondly*, a diminution of the interstices, accompanied by an alteration of the density of the medium within them.

Let us assume, as Professor Williamson has virtually done, that the first of these effects takes place; then, if we admit that "in a cubical mass of carbonate of iron the material particles are more magnetic than the medium which they displace, and the force with which it is attracted is proportional to this excess," we can by no

means admit that, because "it becomes more magnetic by compression, we must conclude that the loss of magnetic medium *from its interstices* is more than supplied by the magnetic matter which takes its place;" for, according to what has already been advanced, the excess of the attraction of the material particles above that of the medium they displace is the same after, as it was before compression; inasmuch as compression merely changes the relative *situations* of the particles, by bringing them closer together, but does not in the least alter their volume, and consequently does not in the least alter the quantity of medium they displace.

With respect to carbonate of lime, Professor Williamson's conclusion is, of course, untenable, because it is based upon the foregoing one. He says, "when these particles are brought closer together by pressure, with diminution of the intervening spaces occupied by the medium, the mass becomes more diamagnetic, because a certain quantity of the magnetic medium is thus replaced by the less magnetic matter." It is, however, manifest that exactly the same quantity of magnetic medium is displaced by the less magnetic matter after, as there was before compression. Why, then, should diamagnetic action be increased by compression?

Lastly, with respect to the crystals of carbonate of iron and carbonate of lime, Professor Williamson's explanation, although ingenious, is liable to the same objections as those already mentioned. It cannot, in fact, be said that the functions of matter predominate most strongly over those of the medium they displace in any one direction, merely because the particles may be closer together in that direction; for, as long as each particle is surrounded by the medium, the predominance of that function of the particles with which we are concerned, *i. e.* their attraction, over that of the medium they displace will be the same, whatever may be the distances of the particles asunder.

From the foregoing remarks, therefore, it is manifest that, if Professor Tyndall has not yet succeeded in demonstrating that the hypothesis of the existence of a magnetic medium and of the identity of magnetism and diamagnetism is *necessarily* at variance with experimental facts, neither has Professor Williamson succeeded in proving that this hypothesis is in accordance with those facts. The question of the existence of a magnetic medium is still an open one,

and will continue to be so until the many important principles which it involves, but which have not been introduced into the present discussion, have been further elucidated by new investigations and new thoughts.

X. "On the ultimate arrangement of the Biliary Ducts, and on some other points in the Anatomy of the Liver of Vertebrate Animals." By LIONEL S. BEALE, M.B., Professor of Physiology and Morbid Anatomy in King's College, London. Communicated by F. KIERNAN, Esq., F.R.S. Received June 14, 1855.

In his valuable communication to the Royal Society in 1833, Mr. Kiernan describes and figures anastomoses between branches of the biliary ducts in the left triangular ligament of the human liver. The same author considered that the interlobular ducts anastomosed with each other, and communicated with a lobular biliary plexus, although he had never succeeded in injecting this plexus to the extent shown in his figure, neither had he directly observed the anastomoses between interlobular ducts. It must be borne in mind that these observations were made before the liver-cells had been described.

Since the appearance of Mr. Kiernan's paper, various hypothetical views have been advanced by different observers, with reference to the arrangement of the minute biliary ducts and the relation which the liver-cells bear to them. These points, however, have not yet been decided by actual observation.

Müller considered that the ducts terminated in blind extremities. Weber showed that the right and left hepatic ducts anastomosed by the intervention of branches in the transverse fissure of the liver, which he described under the name of Vasa aberrantia.

Krukenberg, Schröder Van der Kolk, Retzius, Theile, Backer, Leidy and others have adopted the view that the liver-cells lie within a network of basement membrane. On the other hand, Handfield Jones and Kölliker describe the liver-cells as forming a solid net-