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established a connexion of the closest order between the aromatic portion of the molecule of lignin (bastose) and the tri-atomic phenols—a fact which considerably strengthens the views advanced by physiologists as to the correlations of the carbohydrates with the aromatic group, and the reasoning by which we have sought to emphasize them. The researches in question, on this point, will be published in due course.

XL. *On the Dimensions of a Magnetic Pole in the Electrostatic System of Units.* By OLIVER J. LODGE, D.Sc.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

THE discussion which has been carried on in your pages* respecting the dimensions of a magnetic pole serves to illustrate the divergency of thought between those in this country who have been brought up, electrically, under Faraday and Maxwell, and the continental philosophers so eminently represented by Prof. Clausius. From one point of view the discussion may be said to have been roused by a simple mare's nest constructed by dropping a factor out of one side of an equation (as was pointed out at once by Prof. Fitzgerald and by Mr. J. J. Thomson); but from another point of view it is the natural and inevitable consequence of the different aspects from which these matters can be regarded:—the English standpoint, in which the medium is recognized as the active agent, and is continually present both in the mind and in the formulæ; and the continental standpoint, from which the medium is perceived as so much empty space, and is taken account of as such in the formulæ. Both these aspects of the subject are worth consideration; and it may be conducive to future clearness to discuss them at moderate length.

Coulomb's measurements provisionally established the fact that in air the mechanical force between two electrically charged bodies was proportional to ee'/r^2 ; but the subsequent researches of Faraday proved that this proportionality only holds so long as the medium enveloping the bodies is unchanged, and that the above quotient must be multiplied by different factors in order to give the force exerted in different media. Thus if the same two charged bodies were immersed in bisulphide of carbon, they would repel one another with much less vigour than they do in air.

Introducing therefore as a factor the electric inductive

* Phil. Mag. [5] vol. xiii. pp. 376, 381, 427, 429, 431, 530; and vol. xiv. pp. 124 & 225.

capacity K , we have the general equation

$$F = \frac{ee'}{Kr^2}.$$

Now all that is solemnly essential with respect to the dimensions of the quantities here involved is that e^2/K must be a force into an area, or that

$$[e] \equiv [L][KF]^{\frac{1}{2}}.$$

If we proceed to define the unit of electricity so as to make K of no dimensions and to equal 1 for air, that is a convention, and it is the basis of the electrostatic system; but the above statement is no convention, but a natural truth.

Precisely the same kind of thing is true in magnetism; and we now know that the force between two magnetic poles is not independent of the medium surrounding them, but that if the torsion-balance were full of, say, ferric chloride, the force between the two poles would be measurably less than if it were full of mere air. Thus we again need a factor for completeness; and the real law is that

$$F = \frac{mm'}{\mu r^2},$$

or that

$$[m] \equiv [L][\mu F]^{\frac{1}{2}}.$$

The conventional *magnetic* system of units is based on the definition of m in such a way that μ , the magnetic inductive capacity of the medium, shall be of no dimensions, and shall for air be simply 1.

All then that is objectively and physically fixed about the matter is that the dimensions of $e/\sqrt{K}$ and of $m/\sqrt{\mu}$ are absolutely and mechanically definite, being each of them a length into the square root of a force, or

$$M^{\frac{1}{2}} L^{\frac{3}{2}} T^{-1}.$$

But observe that the two system of units, the electrostatic and the magnetic, the arbitrary definition of e and the arbitrary definition of m , are in their origin utterly independent; not that they are *unrelated*, but their relation must be a matter for future and experimental investigation. All we can so far say about them is, that, in every possible consistent system that can be adopted,

$$[eK^{-\frac{1}{2}}] \equiv [m\mu^{-\frac{1}{2}}] \equiv [LF^{+\frac{1}{2}}] \equiv [Mk^{-\frac{1}{2}}]. \quad . \quad . \quad (1)$$

(The last term of this triple identity is added for the sake of completeness, though it does not directly belong to the present

subject : the letter k is intended for the gravitation-constant as determined by the Cavendish experiment. I am not aware whether the question of the possible dependence of this constant on the optical density of the medium surrounding the attracting masses has ever been considered; but I feel sure that a direct experimental attack on this question would not be uninteresting, and it might lead to important results.)

We now come to the Ørsted-Ampère discovery of the connexion between m and e —the form of the connexion being that an electric current flowing in a closed circuit can produce a magnetic potential, and therefore of course can act on magnets, precisely as if it itself were a magnet of a certain strength and form. The potential so caused at any point in air is found to be simply proportional to the strength of the current and to the solid angle which the circuit subtends as seen from that point; or, in other words, the moment of the magnet which is equivalent to the current is simply proportional to the strength of the current and to the area of the contour round which it flows.

The unit of current most simply and directly applicable to these electromagnetic phenomena is not the old electrostatic unit at all, but a new unit which may be defined in many ways—as, for instance, these :—

The electromagnetic unit of current is that which produces unit magnetic potential at a point whence its circuit subtends unit solid angle;

It is also that which produces unit magnetic intensity, in a given direction, at a point whence the solid angle subtended by its circuit is changing at unit rate, per unit displacement, in that direction;

And, again, it is that current which when flowing round a contour of unit area is equivalent to a magnet of unit moment,—

all these statements being derived directly from the unit magnetic pole thus :—

Unit magnetic potential is defined to exist wherever a solitary and stationary unit pole would possess unit energy ;

Unit magnetic intensity exists wherever unit pole would experience unit force ; and

Unit magnetic moment is that possessed by two unit poles of opposite sign rigidly connected by a bar of unit length.

(The connexion between the old electrostatic unit and this new electrical unit thus magnetically defined may be expressed, if I am not mistaken, by saying that a ring charged with the electrostatic unit of electricity would have to revolve in its own plane with an angular velocity of about 3×10^{10}

radians per second in order to produce the same magnetic effects as the electromagnetic unit of current flowing in the same ring.

Or, conversely, the electrostatic unit magnetic pole would be that which would experience unit force if placed at the centre of a circle of unit radius in which the electrostatic unit of electricity was moving with unit velocity.

This definition I believe to hold equally well in any homogeneous medium; for it is pointed out below that the electromagnetic effect of a current is independent of μ ; while as regards K , a quantity which we might perhaps think would be likely to affect the result, we must remember that electric displacement is totally independent of any such circumstance. So, corresponding to the common electrokinetic equation,

$$\text{Force} = m \int \frac{Cds}{r^2},$$

we shall have, for a moving charge,

$$\text{Force} = \frac{nev}{r^2},$$

whence

$$[me] \equiv [ML^2T^{-1}]. \quad . \quad . \quad . \quad . \quad (2)$$

If statements like these are in the main correct, and after the experiments of Rowland we are bound, I suppose, to believe in the truth of something of the kind, they ought to remove Dr. Everett's objection (*Phil. Mag.* June 1882, p. 431) as to the introduction of electrostatic units into magnetism; unless indeed he maintains the thesis—no doubt a tenable one—that directly you begin to carry a charged body about, the discussion of its performances no longer belongs to *electrostatics*.)

Returning from this digression, we have now to ask whether the statements above made are really definite and independent of the magnetic properties of the medium surrounding the circuits, or must we introduce a factor to express the influence of this medium when it is other than air?

Mr. J. J. Thomson has instructively raised this question (*Phil. Mag.* for June); and he and others at Cambridge consider it a matter to be settled by experiment; and they further consider that, in order to agree with Maxwell's view, experiment ought to make the magnetic effect of a solenoid and its air-equivalent magnetic shell differ, as soon as they are both introduced into some medium for which μ is not unity. Now, though agreeing with this as far as it goes, I venture with diffidence to think that Maxwell would have drawn a distinc-

tion between the medium inside the region of the solenoid corresponding to the substance of the magnetic shell, and that outside. He over and over again lays stress upon the fact that artificial solenoids can only be compared with magnetic shells for the space outside the shells, and that the line of integration must never be allowed to thread the circuits. Let us follow this out and see what it means when applied to the above question.

I will assume it possible (for it certainly is theoretically possible) to imitate any steel magnet whatever by a proper arrangement of electric circuits, both being at present immersed in a non-magnetic (*i. e.* non-magnetizable or $\mu=1$) medium. The two arrangements are completely equivalent for all the region outside both—the region outside both being defined by the shape of the steel. For the comparison is not to be urged within the steel, because of the magnetized surfaces, which would have to be cut through, a circumstance which would completely alter all the conditions; and it is not to be urged within the region of space near the solenoid which is the counterpart of the steel-occupied region, simply because here there are no magnetized surfaces to be cut through, and therefore the conditions will be continuous.

Now take some non-magnetic medium, which for shortness I will call “clay,” mould it into the shape of the steel, and place it in or around the solenoid so as to mechanically define the limits of the “outer region.” And now immerse both magnet and solidified solenoid in any medium for which μ differs from unity: I venture to assert that the equivalence which existed in air will be entirely maintained in the magnetic medium—even though that medium be iron or bismuth,—and that, for both, the magnetic intensity at any point will be its air-value divided by μ .

Still keeping both the things in the magnetic medium, remove the clay from the solenoid and permit the medium to flow into the space it occupied. If what I said before is true, the solenoid will now be too strong for the magnet, for the magnetic permeability of the interior will increase its effect μ times, while that of the exterior will, as before, diminish it $\frac{1}{\mu}$ -th; so that the effect of the solenoid completely immersed in the medium is precisely the same as it was when in air, while the effect of the magnet, from whose interior the magnetic substance is of necessity excluded, is still $\frac{1}{\mu}$ -th of what it was in air. This latter seems to be the kind of experiment which Mr. J. J. Thomson suggests in his June letter (p. 429), and
Phil. Mag. S. 5. Vol. 14. No. 89. Nov. 1882. 2 B

which, he says, Mr. Sargent then intended (and I hope still intends) to carry out.

The solenoid being now completely surrounded with homogeneous magnetic substance, wall a portion of it in with paper or glass to the shape of the steel of the magnet, and then pull both magnet and solenoid out into the air again. Naturally the solenoid will still be μ times too strong for the magnet, but no further discrepancy need be expected; and if the current of the solenoid has been weakened when inside the magnetic medium so as to restore the disturbed equivalence, they will remain equivalent when the region external to both is again filled with common air.

These statements, if in their essence granted, require to make them complete certain provisos about the boundary of the vessel containing the magnetic medium, unless it be infinitely large, and also a discussion of what happens in the case of more than one magnetic medium. But the magnetization of bounding surfaces, and the accidents which happen to lines cutting surfaces of discontinuity, are perfectly understood and need not be here entered into.

Moreover, in making these statements I am merely saying what one would *expect* to happen without evidence to the contrary; but I am not for an instant implying that direct experimental investigation is unnecessary and would not be highly desirable. On the contrary, I think it would be most desirable and satisfactory to have the matter thoroughly sifted.

Supposing, then, that I have so far made no mistake, we can make the general statement of the equivalence of a current and a magnet thus—

The magnetic moment of a circuit is equal to the strength of its current multiplied by its effective area and again multiplied by the magnetic inductive capacity (or permeability) of the medium in the interior of the region enclosed by the contour (which region for a simple plane circuit is a mere shell), but is wholly independent of the magnetic properties of all the rest of the surrounding medium.

The corresponding dimensional equation is

$$[m] \equiv [L\mu eT^{-1}].$$

Substituting in this the value of $[m]$ from (1), we obtain the relation

$$[\mu K] \equiv \frac{1}{v^2}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

whence

$$\left[\frac{e}{m}\right] \equiv \left[\frac{K}{\mu}\right]^{\frac{1}{2}} \equiv [Kv] \equiv \frac{1}{[\mu v]}. \quad . \quad (4)$$

These relations must all hold in any consistent system of units, since they express physical truths ; but of course they are not all independent. The number of independent relations must be limited by the number of fundamental experiments, viz. three—Coulomb, Coulomb, and Oersted ; and the shortest way of writing the independent relations is this :—

$$\text{and} \quad \left. \begin{array}{l} [\mu e^2] \equiv [K m^2] \equiv [ML] \\ [\mu K v^2] \equiv 1. \end{array} \right\} \dots \dots (5)$$

The electrostatic convention makes $[K]=1$; the electromagnetic convention makes $[\mu]=1$.

So far every thing being pretty clear and straightforward, we have now to ask how it was that Prof. Clausius should have jumped to the conclusion that Maxwell had fallen into error*, or else that he held a theory of magnetism different from (and not merely an amplification of) Ampère's and Weber's†. With this latter horn of the dilemma, by the way, he is half allowed by Mr. J. J. Thomson (September) to have succeeded in transfixing Maxwell; and Dr. Everett (June) is not extremely energetic in his repudiation even of the alternative of the first.

Now it is perfectly true that Maxwell, in stating the current theory of magnetism, says without any kind of retraction or hesitation that "the magnetic action of a small plane circuit at distances which are great compared with the dimensions of the circuit is the same as that of a magnet whose axis is normal to the plane of the circuit, and whose magnetic moment is equal to the area of the circuit multiplied by the strength of the current. . . . and if a magnetic shell . . . &c. be substituted . . . then the magnetic action of the shell on all distant points will be identical with that of the current." And in discussing Ampère's theory, he ignores the existence of magnetic media whose μ does not equal 1 as completely as Prof. Clausius could wish.

But then, according to Weber's extension of Ampère's theory (an extension I suppose universally accepted), the properties of magnetic substances of all kinds are explained by molecular electric currents, and no magnets or magnetic substances other than those consisting of current-conveying molecules exist. [And with reference to a remark of Mr.

* Phil. Mag. June, pp. 387 & 392.

† Ibid. August, p. 126.

Thomson's in the September number, p. 225, I may say in passing that it seems to me that Maxwell held, though no doubt tentatively and hypothetically, the view that electric currents and small magnets are *identical* and not only *equivalent*.]

The coefficient μ is thus foreign to Ampère's theory applied universally; and this is how it has happened that Prof. Clausius has failed to recognize its existence and has been led into error*. A system dealing with Ampèrian magnets in media for which μ does not equal 1 is a mongrel combination which may no doubt be occasionally convenient but which never can be thoroughly satisfactory.

We may accept then without hesitation Clausius's presentation of Maxwell's views, viz. both that a small magnet is an electric current, and that *magnetic moment ALWAYS equals simply integral current $\times$ area*—remembering, however, that there exist currents in molecules besides the gross and artificial currents in our copper wires, that these are directed by our artificial currents and add to their effects, and that in all cases they are most distinctly to be taken into account.

In air, so far as it is non-magnetic, these molecular currents are zero, and the magnetic induction and the magnetic force are everywhere equal; but in media consisting of Ampèrian

* Very many errors, I now find; for he has also ignored K, Faraday's simple old electrostatic constant; and accordingly his equations (1), (2), (4), &c. express mere conventions (if they were any thing more, then truly m_s would have to equal m^2 , and $e_s = ed$, as he begins to perceive in his August letter); while his general equation (3), which is the foundation of his reasoning, is quite wrong, and is indeed at the bottom of the whole confusion. In using the term "error" here, I would be understood to mean rather "divergency from opinions commonly held in this country" than absolute incorrectness as to matter of fact. For it would not be becoming to apply the latter term to views held by Prof. Clausius when the experimental foundation of opposing views is confessedly incomplete. The views held by Prof. Clausius are no doubt perfectly consistent, and would probably be in accord with fact if only the medium produced no effect such as it is here commonly supposed to produce; and whether the medium does or does not produce such an effect appears to some extent at present a subject of legitimate debate and a matter for experimental investigation. It will be understood therefore, that in stating one side strongly I have been influenced by the wish to be clear, rather than with the desire to dogmatize.

Since the above letter was in type Dr. Francis has kindly called my attention to a paper by Prof. Helmholtz in Wiedemann's *Annalen*, No. 9, 1882, to which I might have further referred if I had known of it in time. As far as I can hurriedly understand his position, Prof. Helmholtz in part endeavours to reconcile the views of Maxwell and of Clausius by throwing a doubt upon the Weber-Ampère theory; and in fact he appeals to pure physicists not to abandon the old electrostatic for the more cumbersome and less surely founded electromagnetic system.

[A translation of Professor Helmholtz's paper will appear in our next number.—ED. Phil. Mag.]

molecules there is an extra magnetic induction, due to the pointing of these along the lines of force, which is 4π times the magnetization, and which has to be added to the other, thus making the total magnetic induction at any point μ times the magnetic force there.

The effect of the medium is a physical fact; and no theory can presume really to dispense with the constant μ . All that the Ampèrian theory does is to give a physical interpretation to it, and to render one independent of it so soon as one takes account of every current-conveying circuit, whether molecular or other, existing in the field, and does not arbitrarily elect to deal only with those gross solenoids which we can excite and immediately control by batteries.

There can be no doubt, I think, that the mind of Maxwell on this subject, as on most others, was as clear as daylight; and so far from falling into the least suspicion of an error, he expresses himself in art. 615 (1st edit.) almost as if he were joining in the present discussion, saying:—

“There is one result . . . which is of very great importance. If we suppose that no magnets exist in the field except in the form of electric circuits, the distinction which we have hitherto maintained between the magnetic force and the magnetic induction [and therefore also the difference $\mu-1$] vanishes, because it is only in magnetized matter that these quantities differ from each other. According to Ampère’s hypothesis the properties of what we call magnetized matter are due to molecular electric currents, so that it is only when we regard the substance in large masses that our theory of magnetization is applicable; and if our mathematical methods are supposed capable of taking account of what goes on within the individual molecules they will discover nothing but electric circuits, and we shall find the magnetic force and the magnetic induction everywhere identical. In order, however, [N.B.] to be able to make use of the electrostatic or of the electromagnetic system of measurement at pleasure we shall retain the coefficient μ , remembering that its value is unity in the electromagnetic system.”

I am, Gentlemen,

Your obedient servant,

University College, Liverpool,
September 28, 1882.

OLIVER J. LODGE.