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XXII. On the theory of dispersion

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which I believe to be incontestably true) that the *distinction* is not between *luminous* and *obscure* sources of heat, but between the kind of rays of heat emitted from bodies at different *temperatures*; and that the *accident* (as I may term it) of the bodies being at such a temperature that rays of light accompanied the rays of heat, has nothing whatever to do with the fact of the different transmissibilities of the calorific rays: 1st, Because the same difference of transmission exists between sources altogether *obscure*; 2nd, Because this *difference* (between luminous and obscure sources) does not exist with reference to some bodies, *e.g.* *rock salt*; and, thirdly, in bodies emitting light, the quantity of heat transmitted is in no way *proportional*, *either* to the degree of light which accompanies it in the first instance, or to the quantity of light which passes through along with it.

In reference to this subject, it is to be observed, that it is altogether erroneous to consider "diathermancy" in the science of heat as *analogous* to "transparency" in optics; for that property of bodies by which they *stop* (absorb) or *transmit* rays of a *particular refrangibility or colour* is the true analogue in the latter science.

I suspect that this necessary distinction escaped Professor Powell's attention when he alluded to M. Melloni's *hypothesis* as "needless and contrary to all analogy;" for in this view of the subject, the explanation which M. Melloni has given of the heat being more abundantly transmitted through successive plates (of similar natures) is *perfectly analogous* to the effect of a succession of screens (of the *same colour*) on common light. The "diathermancy" of *rock salt* alone appears entitled to be compared with "transparency" as used in optics.

Stephen's Green, Dublin, Jan. 9, 1836.

XXII. *On the Theory of Dispersion.* By the Rev. B. Powell, MA., F.R.S., Sav. Prof. of Geometry, Oxford.

LEARNING that there is not room in this Number for the continuation I had proposed of the researches commenced in the last, I am anxious meanwhile to make a brief remark on two points referred to in the last Number.

I. Mr. Tovey in an excellent paper on the formula for dispersion, introduces a most material simplification on M. Cauchy's process. I allude to this more particularly now, because the writer refers to the importance of not omitting the summation. He will, I trust, find that the introduction of it as discussed in my paper (and still more when the continuation appears,) will produce an entire accordance in our results.

II. In the Editors' note appended to my paper (p. 28.) there is a reference to some investigations of M. Rudberg, published in a former volume of this Journal. I ought, perhaps, to have referred to those curious researches at an earlier period: but it will readily appear that they are quite distinct from mine. The author states at the commencement of his paper: "In investigating the principle, according to which, for the explanation of the dispersion of light on the system of undulations, we must suppose that when the light passes from the air into a more refractive medium the length of the undulations are much contracted, in fact, much shorter,—I have found that the following relation appears to exist between the length of the undulation of a certain colour in the air and the corresponding one in any other substance:" $L = a \cdot l^m$; l being the length in air, L in the medium, a and m constant depending on the medium.

He then takes Fraunhofer's value of l for each ray, and assuming that they are diminished in proportion to the refractive power, proceeds to calculate L for the different media examined by Fraunhofer: and thence the refractive indices by the formula, which on this assumption follows from the former (N being the index)

$$N = \frac{1}{a l^{m-1}},$$

and the resulting numbers certainly exhibit a very good agreement with those of observation.

Now, with regard to the nature of the formula, it is to be observed that the author neither gives any theory from which it is deduced, nor a reference to any other paper, or investigation of such theory; and the form of it is such as would appear extremely unlikely to have any connexion with the analysis of undulations. Again, had any such investigation either of the author or of any other philosopher been in existence, it is hardly conceivable that it could have remained since 1827, without becoming known to some, at least, of the numerous mathematicians in all parts of Europe who have since that period been directing their attention to the subject.

But further, (unless I greatly mistake the author's meaning,) it appears to me from the very form of expression used in the passage above quoted, that *the formula is not derived from any theory of undulations*: for when, he says, "In investigating, &c..... I have found," the meaning really seems to me simply this; that in *attempting* such an investigation on the undulatory theory he had *not* been able to *succeed* in obtaining any *theoretical relation* between the velocity of a wave, and

its length: but that EMPIRICALLY he found upon a comparison of numerical results, "that this relation appears to subsist," which is expressed by the above formula. Such is the impression which the passage conveys to my mind; and, indeed, the tenor of the whole confirms it. I can only add, that I should be truly glad to have pointed out any deduction *from theory* which would give so simple a formula.

Considered as an *empirical law*, it certainly merits great attention. But thus much will be at once evident,—it is totally *independent* of M. Cauchy's *principles*, and of my *results* deduced and calculated on those principles.

Oxford, Jan. 6, 1836.

XXIII. *Experimental Researches in Electricity*.—*Tenth Series*. By MICHAEL FARADAY, D.C.L. F.R.S. Fullerian Prof. Chem. Royal Institution, Corr. Memb. Royal and Imp. Acad. of Sciences, Paris, Petersburg, Florence, Copenhagen, Berlin, &c. &c.*

§ 16. *On an improved form of the Voltaic Battery.* § 17. *Some practical results respecting the construction and use of the Voltaic Battery.*

1119. I HAVE lately had occasion to examine the voltaic trough practically, with a view to improvements in its construction and use; and though I do not pretend that the results have anything like the importance which attaches to the discovery of a new law or principle, I still think they are valuable, and may therefore, if briefly told, and in connexion with former papers, be worthy the approbation of the Royal Society.

§ 16. *On an improved form of the Voltaic Battery.*

1120. In a simple voltaic circuit (and the same is true of the battery) the chemical forces which, during their activity, give power to the instrument, are generally divided into two portions; the one of these is exerted locally, whilst the other is transferred round the circle (947. 996.†); the latter constitutes the electric current of the instrument, whilst the former is altogether lost or wasted. The ratio of these two portions of power may be varied to a great extent by the influence of circumstances: thus, in a battery not closed, *all* the action is local; in one of the ordinary construction, *much* is in circula-

* From the Philosophical Transactions for 1835, Part II. This paper was received by the Royal Society June 16th, and read June 18th, 1835.

[† The paragraphs of the author's former series of Researches here referred to, from 875 to 1047 both inclusive, belong to his Eighth Series, reprinted in Lond. and Edinb. Phil. Mag., vol. vi. p. 34 *et seq.*—EDIT.]