XLII. Observations on some points in the theory of the dispersion of light

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right; and a grete hole theirin, whereof the flawme cam oute of. And after a vj or viij dayes it aroose North-Est, and so bakkere and bakkere, and so enduryd a xiiij nyghtes full lytell chaungynge, goynge from the North-Este to the Weste; and sometyme it wuld seme a quenched oute, and sodanly it brent fervently ageyn. And then it was at one tyme playne northe, and then it compassed rounde aboute the lode sterre, for in the evynynge the blase went ageyns the Southe. And in the mornynge playne northe, and then afterward West, and so more West flamyng vpryght, and so the sterre continued iiij wekys tyll the xx day of ffeveryere; and when it appered West in the fymament, then it lasted alle the nyght, somewhat discendynge with a grettere smoke on the heyre; and som men seyde that the blassynges of the seide sterre was of a myle lengh; and a xij dayes afore the vanyschyng therof, it appereryd in the evenyng, and was down anon within two oures, and evyr of a colour pale stedfast, and it kept his course rysynge west in the northe, and so every nyght it apperid lasse and lasse tyll it was as lytell as a hesyll styke, and so at the laste it vanesched away the xx day of ffebruarii. And some men saide that this sterre was seen ii or iij oures afore the sunne rysynge in Decembre iiij days before Chrystynmasse in the Southwest, so by that reasoun it compassed rounde abowte alle the erthe alleway chaungynge his cours as is afore rehersed."

The observations of Johannes Regiomontanus upon the same comet are recorded in the Nuremberg Chronicle. Warkworth's description was lately communicated to the Society of Antiquaries by my friend Mr. Bruce, and my object in sending it to you was to afford an opportunity for those of your readers to peruse it who are not likely to meet with it through any other source.

Your obedient servant,

J. O. HALLIWELL.

XLII. *Observations on some points in the Theory of the Dispersion of Light.* By the Rev. BADEN POWELL, M.A., F.R.S., F.G.S., F.R.A.S., Savillian Professor of Geometry, Oxford.*

THE problem of the *dispersion* was, for a long time, confessedly the opprobrium of all theories of light, but more especially, in proportion to its higher pretensions,—of the undulatory, not simply because it *had not explained* those phenomena, but because, according to the received views, it was *at variance* with them. It has only been within the last few years, that, by a modification of the principles of this

* Communicated by the Author.

theory, it has been brought to bear on the question of the dispersion. Yet there are still those who are so unreasonable as to inveigh against the theory, because it has not, in that comparatively short period, succeeded in clearing the subject of all its difficulties; and who, instead of any degree of satisfaction at what *has* been done, express only displeasure at what *has not* been effected. Passages might be cited (were it to any purpose to do so) from the writings of some philosophers of the present day, in which complaints of this nature occur, involving remarks and reflections which we cannot well set down to ignorance, and which, in any other point of view, can neither be regarded as peculiarly creditable to the taste of the writers, nor to their appreciation of what is justly due to a series of researches of such a character as must fairly be allowed to belong to those in question, even if they could be shown to have failed in their object.

It is not however my design in the present observations to enter into controversy, but merely to offer a few remarks on one or two points connected with that part of the theory which at present seems most open to difficulty and objection; and to which the attention of those who are in a condition to grapple with the difficulties of this intricate portion of dynamical research, is at present most powerfully called.

In my first deductions from the theory of M. Cauchy, and my earliest calculations to compare it with observation, I made use of a formula which was allowed all along to be but an imperfect and approximate one, and which was applied numerically only by the aid of an indirect and tentative process. A *direct* mode of calculation by it, was indeed stated and explained (under a certain material limitation) (see London and Edinburgh Journal of Science, April 1836, vol. viii. p. 309), but this was in practice not less troublesome than the former.

The other and more exact methods proposed by Sir W. R. Hamilton and Mr. Kelland, by which my later computations were carried on, involve a greater number of constant coefficients; which by consequence, directly or indirectly, must be assumed from observation. The nature of these assumptions, and the precise difference between the two formulas, I pointed out in a paper in the Lond. and Edinb. Journal of Science, January 1836, (p. 26,) and March 1836, (p. 206). It will be desirable here briefly to restate them:

The exact formula deduced immediately from M. Cauchy's theory is,

$$\frac{1}{\mu^2} = \Sigma \left\{ H^2 \left(\frac{\sin^2 \left(\frac{\pi \Delta x}{\lambda} \right)}{\left(\frac{\pi \Delta x}{\lambda} \right)^2} \right) \right\} \quad (1.)$$

This, it may be well to observe, is equivalent to the sum

of a number of like terms, for the same value of μ and λ , such as,

$$\frac{1}{\mu^2} = \left\{ \begin{aligned} & H_I^2 \left(\frac{\sin^2 \left(\frac{\pi \Delta x_I}{\lambda} \right)}{\left(\frac{\pi \Delta x_I}{\lambda} \right)^2} \right) \\ & + H_{II}^2 \left(\frac{\sin^2 \left(\frac{\pi \Delta x_{II}}{\lambda} \right)}{\left(\frac{\pi \Delta x_{II}}{\lambda} \right)^2} \right) \\ & + H_{III}^2 \left(\frac{\sin^2 \left(\frac{\pi \Delta x_{III}}{\lambda} \right)}{\left(\frac{\pi \Delta x_{III}}{\lambda} \right)^2} \right) \\ & + \&c. \end{aligned} \right. \quad (2.)$$

The summation being extended to all the values of Δx which it may be necessary to include.

My first and approximate attempt at calculation was effected by taking a *single term* of such a series, with some constant coefficient, which might be a sort of mean among all the similar terms, and which (on extracting the root) would be,

$$\frac{1}{\mu} = H \left(\frac{\sin \left(\frac{\pi \Delta x}{\lambda} \right)}{\left(\frac{\pi \Delta x}{\lambda} \right)} \right) \quad (3.)$$

The peculiar function involved is easily expanded into a series; and in this way the *exact formula* (1.) may be expressed by,

$$\frac{1}{\mu^2} = P - Q \frac{\mu^2}{\lambda^2} + L \frac{\mu^4}{\lambda^4} - \&c. \quad (4.)$$

Supposing 3 terms sufficient, this is the formula of Mr. Kelland: in which the coefficients can only be found by *assuming 3 indices from observation*. An objection has also been urged by Professor Lloyd, that, on pursuing the investigation, from the relations which subsist between these constants, values of them are given by calculation which are at variance with those derived from observation. I have also remarked in referring to this point at the end of my paper (Phil. Trans. 1838, part i.) that to propose as a sufficient theory one which requires so large an assumption of data from observation, appears hardly consistent with the just demands of what ought to constitute a legitimate mathematical explanation of the law of experimental results.

Now to all who have attentively considered the nature of those remarkable optical properties of bodies which are called their *refractive* and *dispersive powers*, it will be evident that we have a *very peculiar* case to consider. The problem we have to solve is rather a *combination of two distinct problems*. The dispersive and refractive powers follow no proportion to each other, and it is almost impossible to conceive any theory, or even any empirical mathematical law, which could connect the two together. For example, we have diamond and water, with refractions nearly double the one of the other, and dispersions nearly the same. Flint glass and oil of cassia with the same refractions nearly, and dispersions as about 1 to 3. In a word, the absolute magnitude of the deviation of any given ray, or of white light, bears no relation whatever to the difference of deviation between the extreme rays of the spectrum, in different media. If then we seek a theory to explain the facts, it would be not only unreasonable to expect it to connect such obviously incongruous phænomena, but it ought most rationally to involve *two distinct constants*, one belonging to the *refractive POWER*, the other to the *refractive CHARACTER* of the medium. And in conformity with the general conditions of formulas of this kind, we might expect that, directly or indirectly, we should have to assume *two* quantities as given by observation, in any calculation to compare theory with observation, for the spectrum of a particular medium.

Now if we take the formula (1.) above stated, it is at once manifest that if Δx be very small compared with λ , we have for all values of λ very nearly,

$$\frac{\sin\left(\frac{\pi \Delta x}{\lambda}\right)}{\left(\frac{\pi \Delta x}{\lambda}\right)} = 1 \quad (5.)$$

or there is no dispersion, and the formula is reduced to

$$\frac{1}{\mu^2} = \Sigma . H^2 \quad (6.)$$

Now, in any medium, if Δx be *not* very small compared with λ , we shall have different refractive indices for each ray, which will differ less as λ becomes greater; and if we suppose λ to increase indefinitely, (Δx remaining finite) the above expression (6.) will be the *limit* to which the refractive index constantly tends, and from which it does not sensibly differ when λ is supposed large; that is, for some ray beyond the red end of the spectrum (and such may exist though not sensible to the eye) there is an absolute limit to all refraction,

different in different media; this may be called *the refraction constant*. If we had any means of determining it by experiment, it would be a most important step for the theory. But it is evidently a datum quite independent of the dispersion. This quantity corresponds to (*h*) in Mr. Tovey's notation, and to (*P*) in Mr. Kelland's, who has determined it for Fraunhofer's media; as I have done for several of those I have examined. (Phil. Trans. 1838, part i.) It is an index not greatly below that for the red extremity of the visible spectrum. Whether it may *possibly* be connected with the refraction of *heat* may become an interesting question.

To return to the dispersion:—according to the exact method we have still two more constants to determine, viz. in Mr. Kelland's notation (*Q*) and (*L*); in Mr. Tovey's (*h'*) and (*h''*): these like the former must be ultimately derived from observation, and they are in fact obtained from the assumption of *two more* observed indices.

But pursuing the same idea as that just adverted to, it would seem reasonable that as *one* datum for each medium is the *refraction constant*, so *one* more should supply a *dispersion constant*; and, on looking at the nature of the formula, it would seem that this should depend on the arc $\pi \Delta x$. Now the coefficients *Q* and *L* arise from the summation of the various combinations of H_i and Δx_i ; H_{ii} and Δx_{ii} ; H_{iii} and Δx_{iii} , &c. And if we recur to the simple consideration of one term of the summation, which furnished my first approximate formula, we shall see that we here avoid this difficulty, and in fact have only the *two* constants of refraction and dispersion to derive from observation. And though it may be true in reference to the physical theory that for the adoption of that formula no legitimate ground appears, yet for the reasons already adduced it may be worth while to examine the application of it more closely.

If we take the simple formula (3.), or what is the same thing, on developing, (see Lond. and Edinb. Journal of Science, Jan. 1836) and substituting the value of λ in air,

$$\frac{1}{\mu} = H \left\{ 1 - \frac{1}{6} \frac{\mu^2}{\lambda^2} (\pi \Delta x)^2 + \frac{1}{120} \frac{\mu^4}{\lambda^4} (\pi \Delta x)^4 \text{ \&c.} \right\} \quad (7.)$$

writing for abridgement, $\pi \Delta x = \theta$,

$$p = \frac{1}{6} \frac{\mu^3}{\lambda^2}, \text{ and } q = \frac{1}{120} \frac{\mu^5}{\lambda^4},$$

we shall have, for any one ray,

$$\frac{1}{H} = \mu - p\theta^2 + q\theta^4 \quad (8.)$$

and for any other ray of the spectrum,

$$\frac{1}{H} = \mu_l - p_l \theta^2 + q_l \theta^4 \quad (9.)$$

Then eliminating H , we have

$$0 = (\mu - \mu_l) - (p - p_l) \theta^2 + (q - q_l) \theta^4 \quad (10.)$$

and

$$\frac{-(\mu - \mu_l)}{(q - q_l)} = \theta^4 - \left(\frac{p - p_l}{q - q_l}\right) \theta^2 \quad (11.)$$

or for abridgement

$$-m = \theta^4 - r \theta^2 \quad (12.)$$

whence by solving the quadratic we obtain

$$\theta^2 = \frac{r}{2} \pm \sqrt{-m + \frac{r^2}{4}} \quad (13.)$$

in which

$$\frac{r}{2} = 10 \frac{\left(\frac{\mu^3}{\lambda^3} - \frac{\mu_l^3}{\lambda_l^3}\right)}{\left(\frac{\mu^5}{\lambda^4} - \frac{\mu_l^5}{\lambda_l^4}\right)} \quad (14.)$$

$$m = \frac{(\mu - \mu_l)}{\frac{1}{120} \left(\frac{\mu^5}{\lambda^4} - \frac{\mu_l^5}{\lambda_l^4}\right)} \quad (15.)$$

If then we take the *two* values μ and μ_l from observation, we readily find θ , and thence again obtain the value of H , by substituting in either of the expressions (8.) (9.). Then, in the same expression for every other ray of the spectrum, substituting θ , we shall find H ; which, if the formula be correct, ought to result the *same* for every ray.

This process then, if it be considered allowable, affords this material advantage, that it requires only the assumption of *two* values as given by observation for each medium, which we have just seen is exactly the improvement required in theory. Now though I have not as yet tried the result of calculation precisely in the way above stated, yet it is evident that all the calculations I made in my two first papers in the Phil. Trans., including all the results of Fraunhofer and Rudberg, were conducted on an hypothesis which is in principle identically the same: and in all those it is universally allowed the coincidences are as close as could be desired: so that it is evident that this supposition cannot be very far from the truth; calculation grounded upon it has not yet been applied to more highly dispersive media. But it becomes extremely important to see whether it may be so applicable; whether it may apply even *as well as* the methods proposed on the less restricted hypotheses, such as I have em-

played in my later calculations; but which essentially involve the fault of requiring too large assumptions from observations, and these too, according to the remarks of Prof. Lloyd, leading to conclusions inconsistent with truth: this he has pointed out in a letter to myself; the details of his investigation have not yet been published; but there is a short abstract of them in the Proceedings of the Royal Irish Academy, No. 1. p. 10, and some mention of the subject was, I believe, made at the British Association 1838, in the Physical Section. I have reason to hope that the paper will before long be published, meanwhile it may not be irrelevant to offer one observation more on the subject.

Since such a formula as the above is unquestionably very nearly accordant with the truth, through, at least, a considerable range of media, it certainly becomes important to examine, whether any *theoretical* conditions are conceivable, which may warrant the adoption of it, as at least a good approximation; and the question obviously reduces itself to this: whether in taking the sum of an indefinite number of terms which are combinations of H_I and Δx_I , H_{II} and Δx_{II} , H_{III} and Δx_{III} , &c. (the form of which is seen in my paper, Phil. Trans. 1838, part ii.), we may not discover some ground for justifying the adoption of a *single term* containing some mean value of Δx combined with a constant coefficient H , such, that we shall have accurately or nearly,

$$\left\{ \begin{aligned} & \left\{ H_I^2 \frac{\sin^2 \left(\frac{\pi \Delta x_I}{\lambda} \right)}{\left(\frac{\pi \Delta x_I}{\lambda} \right)^2} \right\} \\ & + \left\{ H_{II}^2 \frac{\sin^2 \left(\frac{\pi \Delta x_{II}}{\lambda} \right)}{\left(\frac{\pi \Delta x_{II}}{\lambda} \right)^2} \right\} \\ & + \left\{ H_{III}^2 \frac{\sin^2 \left(\frac{\pi \Delta x_{III}}{\lambda} \right)}{\left(\frac{\pi \Delta x_{III}}{\lambda} \right)^2} \right\} \\ & + \&c. \end{aligned} \right\} = H^2 \frac{\sin^2 \left(\frac{\pi \Delta x}{\lambda} \right)}{\left(\frac{\pi \Delta x}{\lambda} \right)^2}$$

How far the dynamical conditions of a system of molecules such as that supposed by M. Cauchy and the other eminent mathematicians who have treated on the subject, may be found susceptible of leading to any such deductions (in which the process would be perhaps somewhat analogous to finding the expression for the centre of gravity of a system of particles,)

I would leave to the meditations of those more familiar with the analysis of dynamics. But I trust some of those readers of this Journal, who are so able to discuss such a topic will not think it unworthy of their attention.

XLIII. *Meteorological Observations made during Voyages in the Atlantic and South Pacific Oceans; and Altitudes in the Vicinity of Lima measured by the Sympiesometer.* By JOHN MACLEAN*.

Meteorological Register from the Cape de Verd Islands to Callao. Thermometer in the Cabin, and a fair exposure.

Date.	Thermometer 8 a.m. Noon.		Latitude.	Wind and Weather.	Observations.
1821.					
Sept. 22.		85		calm and clear.	off Cape Arit. Cape de Verd.
23.		82		light air.	
30.		83			
Oct. 1.		83	7° 3'	N. lat.	long. 17° 30'
2.	76	78†	4 59	wsu., rainy.	
3.		82‡	4 44	s., clear.	
4.		82§	4 4	s., clear.	long. 14°
5.		82	3 34	southerly.	
6.		81	3 4	"	
7.		81	2 46	"	
8.	79	81	2 25	ssw.	
9.	78	81	1 47	ssw., breezes.	
10.	78	79	1 37	ssw.	
12.	78	80	1 0	s. by E.	
13.	78	79	0 20	s. lat.	
14.	78	80	1 25	s. by w., light.	
15.	77	79	2 42	se. by E., breezes.	
16.	77	79	4 18	cloudy.	
17.	77	80	6 32	"	
18.	76	79	8 48	"	
19.	76	79	10 47	se. by E.	
20.	76	79	12 43	"	many birds.
21.	76	79	14 55	"	
22.	76	79	16 48	NE.	
23.	77	80	18 38	N. by E.	
24.	77	80	20 40	variable, rainy.	
25.	74	74	21 13	s.	
26.	74	73	22 21	ese., cloudy.	

* Communicated by Professor Sir W. J. Hooker, LL.D., F.R.S. The author observes, with respect to his Meteorological Observations: "My chief object in sending these is to show the trifling change in the thermometer during winter and summer in the extreme southern latitude, where it appears the cold is only severe when the wind is from the southward."

† At 4 p.m. 79°. ‡ At 6 a.m. 80°; 8 p.m. 82°. § At 6 a.m. 79°.