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18 Why Stokes never wrote a treatise on optics

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With Stokes, mathematics was the servant and assistant, not the master. His guiding star was natural philosophy. Sound, light, radiant heat, chemistry, were his fields of labour, which he cultivated by studying properties of matter, with the aid of experimental and mathematical investigation. [From Kelvin's posthumous tribute to Stokes in *Nature*.]

The indiscretion of plausible conjecture

In the fall of 1878 George Gabriel Stokes had been secretary to the Royal Society of London for nearly a quarter-century and Lucasian Professor of Mathematics at Cambridge for five years beyond that. Well-known in Britain and on the Continent for his papers on mathematics, hydrodynamics and optics, as well as for his experimental investigations of fluorescence, at the age of fifty-nine Stokes was nevertheless something of a disappointment to his British colleagues. Older than William Thomson (later Lord Kelvin) by four years, and than James Clerk Maxwell by fourteen, Stokes had failed to produce a major text. Thomson (together with Tait), had created the immensely influential *Treatise on Natural Philosophy* in 1867.[1] Maxwell's 1873 *Treatise on Electricity and Magnetism* already informed the understanding of a generation, and Rayleigh's 1877 *Theory of Sound* treated that subject in ways that often paralleled the two *Treatises*. The acknowledged master of hydrodynamics and optics, Stokes had produced nothing similar, despite the fact that during the 1860s 'the scientific world expected from him a systematic treatise on Light, and indeed a book was actually advertised as in preparation'.[2]

Not only did Stokes fail to write the awaited *Treatise*, by the late 1870s his overall scientific productivity was not what it had been a decade before. Rayleigh, no doubt reflecting a rather common opinion, attributed the absence of the book on light to 'pressure of work, and perhaps a growing habit of procrastination'. There is ample evidence in his *Memoir and Scientific Correspondence* that Stokes did often put things off, and he was certainly

1 2 vols. (Cambridge University Press).

2 Lord Rayleigh (J. W. Strutt), 'Obituary notice of Sir George Gabriel Stokes, Bart. 1819–1903', *Roy. Soc. Proc.* 75(1904): 199–216, on p. 211.

burdened by his long secretaryship of the Royal Society, which he took quite seriously. However he could at any time after the early 1860s easily have produced a text in optics that presented a unified view of the subject based in major part on two important memoirs that he had published in 1849 and 1862. But he never did, and in Britain there was no one of comparable stature and knowledge to produce the missing text. Consequently for nearly forty years British students lacked a modern treatment of optics whereas they were well-supplied in the books of Maxwell, Thomson and Tait and of Rayleigh with elaborate, highly-developed discussions of electromagnetism, mechanics and sound.

Stokes's failure to put pen to paper does not reflect the absence of a coherent view of the subject on his part. On the contrary, his published articles present a unified scheme for optics, one which remained surprisingly constant. Moreover Stokes lectured on the subject at Cambridge from 1849 for nearly half a century. And for at least half of this time many future British mathematicians and physicists heard him speak, gleaning a substantial part of their understanding of optical principles from him.[3] Yet Stokes's career ceased being on the forefront of research sometime between the mid 1850s and the early 1860s. From that time on Stokes began increasingly to sit on the sidelines, his influence bracketed on one end by his often detailed critiques of papers submitted to the Royal Society and on the other by the extensive correspondence that he maintained.

Rayleigh perceptively captured the character traits that, in part, pulled Stokes away from novel experimental and mathematical research after his fortieth birthday:

> Perhaps [Stokes] would have been the better for a little more wholesome desire for reputation. As happened in the case of Cavendish, too great an indifference in this respect, especially if combined with a morbid dread of mistakes, may easily lead to the withholding of valuable ideas and even to the suppression of elaborate experimental work, which it is often a labour to prepare for publication.[4]

Lack of competitiveness, fear of error, and a certain laziness – all of these traits were remarked by Stokes's friends and family or even by himself, and they do go far in explaining why he did not fulfil his early promise, and even why he

3 David B. Wilson, *Kelvin and Stokes. A Comparative Study in Victorian Physics* (Bristol: Adam Hilger, 1987), pp. 42–53 nicely discusses Stokes's lecturing career, and points out that 'Stokes's course was one for the best students until the mid-1870s, with about 80 per cent of the top ten wranglers during that time enroled' (p. 45). After that time enrolment increasingly declined, until by the early 1890s Stokes's course had apparently become almost completely irrelevant.

4 Rayleigh, 'Obituary notice', p. 210.

never produced a text in optics or in hydrodynamics. But in the case of optics at least he had all the material ready to hand by the early 1860s, and it would not have been an immense labor to write the book.

Why, then, did he never write the text? In the absence of definite evidence one can of course only conjecture, but it seems probable that Stokes never produced the book because he feared that he did not have a proper subject to write about, and that to attempt to create the subject might lead him into what he most feared: public error. A hint of his attitude can perhaps be gleaned from his much later (1887) Burnett Lectures, which were published in the *Nature* series of science popularizations.[5] The lectures were divided into three 'courses', the courses being given in separate years (1883, 1884 and 1885). The first one, entitled 'On the nature of light', contained the substance of physical optics; the second concerned 'investigations carried on by the aid of light' (including absorption and emission, as well as spectral phenomena); the third dealt with light's 'beneficial effects'. In the third lecture of the second course Stokes made the following remark, which reveals something of the difficulty he had had for over two decades in producing a mathematical account of contemporary optics as a unified science:

> In all the phenomena which I have brought before you in my last lecture and in this, and indeed in all that I shall have occasion to mention in this year's course, there is a very intimate relation between molecular grouping and the optical features observed. We touch here on the boundaries of our present physical knowledge. That light consists in the vibrations of a subtle medium or *ether*, that self-luminous bodies, including phosphorescent bodies, which are for the time being self-luminous, are in a state of molecular agitation which they are capable of communicating to the ether, that consequently in the phenomenon of absorption molecular disturbance is excited in bodies at the expense of etherial vibrations – all this is so well established as to leave no reasonable room for doubt. But what may be the mode of connexion by which the vibrations of the ether agitate the

[5] George G. Stokes, *Burnett Lectures on Light. In Three Courses* (Nature Series. London: Macmillan and Co., 1887). Like other texts in the series Stokes's lacked mathematics but was nevertheless written at a very high level – sufficiently high, in fact, that one doubts that most of his auditors could have gleaned much from it. Stokes was the first Burnett lecturer, having no doubt been chosen both for his eminence and (even more so) for his overwhelming (if rather idiosyncratic) interest in revelation, since the terms of the lectures included that '...the lecturer [shall have] regard, in treating of the special subject prescribed, to the illustration afforded by it of the theme proposed by the testator', which was 'That there is a Being, all-powerful, wise, and good, by whom everything exists...' and so on in like vein. The Burnett trust was originally established in 1784 for 'various charitable and pious objects', in Stokes's words, including essays on this theme. Its terms were revised to form the lecture series that began in 1878.

molecules, or the molecules in their turn are able to agitate the ether, what may be the cause of the diminished velocity of propagation in refracting media, what may be the mechanical cause of the difference of velocity of propagation of right and left-handed circularly polarized light in media like sirup of sugar, which is manifested by a rotation of the plane of polarization of plane-polarized light through bodies – all these are questions concerning the true answers to which we can affirm nothing, though plausible conjectures may in many cases be framed.[6]

'Plausible conjecture' – *that* was Stokes's intellectual problem, because he refused to commit himself in print to anything that he felt to be merely 'plausible',[7] and yet he was also unwilling to treat a subject less than thoroughly. For him the *causes* (in the sense of the quotation) of phenomena as fundamental as refraction would necessarily have formed an important part of the subject's textual treatment, and in their absence a fully-fledged subject simply did not exist – and so no reasonable text could be written for it. Moreover in the case of optics the need for the kind of text that Stokes might have written – one without causal conjecture – was not so pressing as it was, e.g. in electricity and magnetism, since the fundamental physical and analytical structure of optics had been reasonably well fixed since the late 1820s, whereas in electricity and magnetism the structure had undergone wholesale revision in the hands of Faraday, Thomson and Maxwell over a forty year period that ended with the production of the requisite text (Maxwell's *Treatise*).

By the late 1870s Stokes was no longer contributing in a substantial way to contemporary optics – to the extent that Richard Tetley Glazebrook, instead of Stokes, wrote the major report on the subject for the British Association in 1885. The previous report in 1862 had indeed been written by Stokes, and it had dealt fully with double refraction.[8] But, Glazebrook revealingly noted a quarter-century later, 'unfortunately [Stokes] confined himself to that one

6 Stokes, *On Light*, pp. 166–7.

7 This could go quite far. During 1882 he and Rayleigh corresponded about viscous effects at the boundary between a solid and a fluid. Stokes felt that eddies would necessarily form in the boundary layer which would expel dust particles from the region. This affects Rayleigh's analysis of 'the Dark Plane which is formed over a heated wire in dusty air', which he had sent to Stokes for publication in the *Proceedings of the Royal Society*. Stokes wanted to add what would have been an interesting but innocuous note on the point, but he could not bring himself to do so: 'I have abstained from putting in a note about this', he wrote to Rayleigh, 'because the existence of such a narrow stratum of eddies is at present only conjectural'. See George G. Stokes, *Memoir and Scientific Correspondence*, selected and arranged by Joseph Larmor, 2 vols. (Cambridge University Press, 1907. Reprinted New York: Johnson Reprint Corporation, 1971), vol. 2, p. 109.

8 George G. Stokes, 'Report on double refraction', *British Association Reports* (1862), pp. 253–82.

branch of the subject'. What had Stokes *not* treated in 1862 that he might have? He could not very well have dealt with anomalous dispersion (as Glazebrook did in 1885), since it was not discovered until 1870, but there were two areas he certainly could have treated in some detail but did not: reflection and dispersion. Instead he chose to discuss only double refraction – though the previous British Association *Report*, written by Humphrey Lloyd in 1834 on the very eve of the wave theory's wide dissemination, had discussed nearly every possible topic that the subject might embrace. In 1862 Stokes had limited himself severely indeed, and this I believe was already a symptom of his growing unwillingness, or temperamental inability, to stay on the cutting edge of research. To do that he would *in print* – and not just in correspondence – have had to say much that was merely plausible.[9] And that he refused to do.

Analysis and experiment

> If you gave Stokes the Sun there was no experiment he could not do for two-pence.[10]

Despite the fact that Stokes chose in 1862 to write a *Report* solely on double refraction his own reputation in optics at the time had little to do with the subject, though he had certainly thought much about it. Stokes had instead worked primarily on three other, distinct topics, two of which were closely bound to laboratory investigation. The earliest (1845), which had little to do with contemporary experiment, involved the conditions that should be imposed on the ether's motion in the vicinity of the earth in order to yield a correct account of stellar aberration.[11] After 1849 he rarely discussed this topic, but it was important early in his career in that, through an extended and public controversy with Challis to which it led, Stokes had a lesson in how dangerous premature assertion can be.[12]

9 Stokes began the *Report* by 'regretting' that 'in consequence of other occupations the materials for a complete report on Physical Optics, which the British Association have requested me to prepare, are not yet collected and digested'. He offered a report on the 'single branch' of double refraction alone, which we shall see could be treated analytically with results that gave nothing away to mere plausibility.

10 Saying round Cambridge: Stokes, *Memoir*, vol. 1, p. 19.

11 On which see Wilson, *Kelvin and Stokes*, chap. 6 for details. Stokes required that the ether have no slip relative to the earth at its surface, that its velocity must lack divergence, and that it possess a velocity potential. In 1886 Lorentz demonstrated that Stokes's conditions are inconsistent (on the effects of which see Jed Z. Buchwald, 'The Michelson experiment in the light of electromagnetic theory before 1900', *AIP Conference Proceedings* 179(1988):55–70).

12 During their controversy Stokes was forced to admit on one critical point that Challis had the better of him (Wilson, *Kelvin and Stokes*, p. 141). Then, in 1849, Stokes became Lucasian professor at Cambridge, where Challis lectured on all of physics. On

Shortly before Stokes became Lucasian professor at Cambridge he finished his first major analytical work in optics, which was entitled *On the dynamical theory of diffraction*.[13] In it Stokes penetrated very far into the mathematical core of the contemporary wave theory – farther than anyone had since Fresnel – and he also derived an important new result that he immediately sought to confirm in the laboratory. It is worth spending a moment on Stokes's theory of diffraction, both because he did *not* apparently discuss the subject in detail in his optics lectures,[14] and because it illustrates the way in which, when convinced that he was on the firmest ground, Stokes could nicely draw physical conclusions with important and direct empirical significance from intricate analysis.

Stokes's purpose was to uncover the function that governs the dependence of the amplitude of secondary waves on the direction with respect to the normal to the front which they form.[15] To do so he began at once with the general differential equation of motion for an isotropic elastic solid:[16]

$$\partial^2\mathbf{u}/\partial t^2 = b^2\nabla^2\mathbf{u}+(a^2-b^2)\nabla(\nabla\cdot\mathbf{u})$$

where $\mathbf{u}$ is the displacement, and a, b are elastic constants. He then separated the equation by defining 'for shortness' δ as the negative compression $\nabla\cdot\mathbf{u}$ (or 'dilatation' as he called it), and ω as the rotation (or, again in Stokes's terminology, the 'distortion') $(\frac{1}{2})\nabla\times\mathbf{u}$:

$$\partial^2\delta/\partial t^2 = a^2\nabla^2\delta,$$

$$\partial^2\omega/\partial t^2 = b^2\nabla\omega.$$

The single equation for the compression, and the three for the components of the rotation, all have precisely the same form, and Stokes at once wrote down the following solution for them, which he obtained from Poisson:

$$U = (t/4\pi)\iint F(at)\,\mathrm{d}\sigma+(1/4\pi)(\mathrm{d}/\mathrm{d}t)\{t\iint f(at)\,\mathrm{d}\sigma\}.$$

Stokes's arrival Challis climbed to the lofty perch of astronomy, handing fluid mechanics and optics over to him (Wilson, *ibid.*, p. 44).

13 According to Rayleigh – who himself had a rather jaundiced view of Stokes's conclusions (see Stokes, *Memoir*, vol. 2, pp. 110–11) – this work guaranteed him the Lucasian [Rayleigh, 'Obituary notice', p. 204].

14 As is apparent from John Ambrose Fleming's notes of the lectures given in 1879: Papers of John Ambrose Fleming, 1879, MS ADD 122/36. The Library. University College, London.

15 This was an important question that had much vexed Fresnel over thirty years before, on which see Jed Z. Buchwald, *The Rise of the Wave Theory of Light* (The University of Chicago Press, 1989), pp. 194–6.

16 As developed in this form by Stokes himself in 'On the theories of the internal friction of fluids in motion, and of the equilibrium and motion of elastic solids', *Cambridge Philosophical Society Transactions* 8(1845):287–319.

Here U is the solution at some point P, t is the time, and f, F are respectively the initial values of the function and its time derivative. The integrals are taken over a surface of radius at (or bt) that surrounds the field point P.[17]

It is particularly ironic that Stokes took this from Poisson, because he at once used it to argue that the amplitude of the secondary waves varies with direction in a fashion that Poisson himself would probably not have accepted.[18] The solution expresses the value of the function at a given place and time in terms of integrals over the region that contained the initial disturbance.[19] Through clever manipulation Stokes was able to conclude from it that the integral for the pulse of distortion contains the factor $1+\cos\theta$, where θ is the angle between the normal to the pulse at the element of integration and the line from there to the field point. Significant though this was – since it justified in retrospect Fresnel's assumptions – it was not the centerpiece of Stokes's analysis.[20]

17 Here $d\sigma$ is $\sin\theta\, d\theta\, d\phi$ where θ, ϕ are the angular spherical coordinates for the surface surrounding the field point P. The integrals therefore correspond to the *mean values* of the functions over the surfaces: see B. B. Baker and E. T. Copson, *The Mathematical Theory of Huygens' Principle* (Oxford: Clarendon Press, 1939), chap. 1 for details. Poisson's solution has the peculiarity of representing the effect at P in terms of a time-dependent radius that is drawn from P. Instead, that is, of following a pulse as it expands outwards, with Poisson's solution we start at a given point and cut space with surfaces drawn about it until we find surfaces that pass through the regions which contain the initial disturbance. Poisson, as it were, held fixed the initial disturbance and went looking for it from the field point.

18 Buchwald, *Wave Theory*, p. 192.

19 Baker and Copson, *Huygens' Principle*, pp. 12–15 discuss Poisson's solution, which is difficult to formulate in a rigorous manner. Note that the solution does not require periodicity but applies to any disturbance that begins at some moment, i.e. to pulses. Stokes justified the extension to infinitely long wave trains from pulses in the following way:

> In the investigation it has been supposed that the force [disturbance] began to act at the time 0, before which the fluid was at rest, so that $f(t) = 0$ when t is negative. But it is evident that exactly the same reasoning would have applied had the force begun to act at any past epoch, so that we are not obliged to suppose $f(t)$ equal to zero when t is negative, and we may even suppose $f(t)$ periodic, so as to have finite values from $t = -\infty$ to $t = +\infty$.

Stokes is a bit disingenuous here, since not only his investigation, but Poisson's solution, requires the limitation. Stokes's quick attempt to extend the class of allowable functions to cover those which are not temporally delimited requires a great deal more justification than this.

20 Neither Rayleigh (*Theory of Sound*, 2 vols. (Cambridge University Press, 1894. Reprinted New York: Dover Publications, Inc., 1945), vol. 2, chap. 14) nor Todhunter (*A History of the Theory of Elasticity and of the Strength of Materials from Galilei to Lord Kelvin*, edited and completed by K. Pearson, 3 vols. (Cambridge University Press, 1886–93. Reprinted New York: Dover Publications, Inc., 1960), vol. 1, secs. 1263–75)

In Stokes's eyes and in the eyes of his contemporaries the core result of his investigation – the one that had great importance for ongoing controversies – concerned a relation that he obtained between diffraction and the plane of polarization.[21] If α_i, α_d are respectively the angles between the plane of *oscillation* and the plane normal to that of diffraction for the incident and diffracted rays then with θ the angle of diffraction Stokes found:[22]

$$\tan \alpha_d = \cos \theta \tan \alpha_i.$$

This could be examined in the laboratory. Stokes performed a series of difficult experiments in his Cambridge rooms, followed by complicated estimations of error, which – though not without a great deal of discussion concerning disturbing effects – supported the oscillation's being normal to, rather than in, the plane of polarization. The conclusion did not achieve immediate assent, in part because the experiment was extraordinarily difficult to replicate, but also because Stokes's analysis was itself hard to penetrate and also because it seemed critically to depend upon the propagation equation that

mentions Stokes's discovery of the inclination factor, though both point out his use of Poisson's solution. Baker and Copson (*Huygens' Principle*, sec. 4.6) approximate in Helmholtz's diffraction integral for a *periodic* disturbance (to small wavelengths) and obtain thereby the inclination factor. They write that this solution is 'due to Stokes', referring to his 'On the dynamical theory of diffraction', *Transactions of the Cambridge Philosophical Society*, 9(1849).
Stokes certainly did obtain the inclination factor, and he did also deploy Poisson's solution. But it is extraordinarily difficult to see how he obtained the former from the latter when the *only* way that it has been obtained elsewhere is through Helmholtz's integral for *periodic* disturbances. Stokes's solution was apparently much more general, embracing any retarded function at all. These are difficulties here which badly need clarification, but this will require a thorough study of the subsequent history of diffraction theory, as well as a detailed analysis of Stokes's own *Dynamical theory*.

21 The best, and most comprehensive, treatment of this and related issues concerning ether dynamics remains E. T. Whittaker's *A History of the Theories of Aether and Electricity*. 2 vols. (New York: Humanities Press, 1974), vol. 1, chap. 5. As always with Whittaker, however, the discussion reads very much like a retrospective British Association *Report* on a newly-deceased issue. Great technical insight can be gleaned from such things, but equally great care must be taken to maintain a critical, historical distance from the argument. Whittaker's *History*, as I have previously remarked, must itself be treated as a kind of primary material.

22 The plane of polarization was conventionally specified by its angle with respect to the plane of diffraction. Consequently if the direction of oscillation is *perpendicular* to the plane of polarization then the angles α in Stokes's equation correspond directly to that plane, but if the oscillation is *in* the plane of polarization then the α are the complements of the polarization angles. Stokes's α can therefore always be treated as conventional polarization angles if, in his formula, $\cos \theta$ is used for oscillations perpendicular to the plane of polarization, and $\sec \theta$ is used for oscillations in that plane.

he had chosen.[23] Indeed, during the next forty years a great deal of experimental and theoretical attention was devoted to this topic, one that nicely mixed physical and mathematical complexities with laboratory intricacy.[24]

Stokes devoted nearly half of his paper to an elaborate discussion of the experiments, for here he hoped to present a new 'discovery' and not merely a new result of analysis. Despite Stokes's present (and indubitably deserved) reputation as a master of analysis, he himself was most strongly attracted to the unearthing of novel experimental lore. This is hardly surprising given the temper of the era, which placed an extremely high premium on discovery, but his desire for great success in the optical laboratory was only partially fulfilled, and in any case not by these experiments on polarization and diffraction. They remained controversial, both on theoretical and on experimental grounds, so that Stokes had no hope of claiming here a universally-recognized discovery.

This changed dramatically in 1852, for in that year Stokes discovered something that rapidly extended his reputation from mathematics into natural philosophy. In his own words, written at the time:

23 In a note appended to the version printed in his *Memoir* Stokes noted that Holtzmann in 1856 obtained results that led to the opposite conclusion from his own, but that Lorenz in 1860 – while rejecting Stokes's analysis – obtained the same empirical result that Stokes had. Clearly the boundaries of contemporary optical technique were stressed by the polarization of diffracted light. Glazebrook, e.g., remarked in 1885 that the 'experiments are troublesome, and the comparison of the results with theory is complicated by the fact that the refraction through the glass plate on which the [diffraction] grating is ruled also produces a change in the position of the plane of polarisation' (R. T. Glazebrook, 'Report on optical theories', *British Association Reports* 1885:157–261, on p. 203). M. E. Mascart, *Traité d'Optique*, 3 vols. (Paris: Gauthier-Villars et Fils, 1893), vol. 3 still regarded the experiments as doubtful, remarking 'It does not seem to me that the agreement of the measurements [with theory] can authorise [Stokes's conclusion]'. The question was eventually settled when the electromagnetic theory of light achieved widespread acceptance. According to it the Fresnel sine law for reflection applies to the electric field vector when the latter is *normal to* the plane of reflection. Since, empirically, that law holds for light polarized *in* this plane, then it follows that the direction of oscillation must be normal to the plane of polarization *if* the electric field is the optical vector. It seems that the latter question was reasonably well-settled by Otto Wiener in 1890, who produced standing waves in photographic emulsions that were only fractions of a wavelength in thickness (see M. Born and E. Wolf, *Principles of Optics*, Fifth edn (Oxford: Pergamon Press, 1975), pp. 279–80 for a brief discussion of Wiener's experiments). Of course once the electromagnetic theory prevailed this issue was no longer particularly interesting since the relationship of the optically-effective vector to the plane of polarization no longer had any importance for distinguishing between equally viable, alternative theories.

24 See the lengthy discussion in Glazebrook's 'Report', chap. 6 for details. Some of the conceptual problems that arose concerned the applicability of Stokes's solution to wave trains, as well as problems in understanding how to use Huygens's principle.

> I discovered on Monday, April 28th, 1852, that in the phenomenon of interior dispersion a ray of light actually *changes its refrangibility*. In sulphate of quinine ... the violet rays of a certain refrangibility produce the interior dispersion noticed by Sir D. Brewster, while the invisible, or at any rate barely visible, rays beyond the extreme violet produce the narrow band of light described by Sir J. Herschel ...[25]

The following September Stokes presented his discovery of fluorescence at the British Association meeting in Belfast, where, following Stokes's talk, the Association's president, Col. Edward Sabine, effusively remarked:[26]

> many would look back with delight to their presence there that evening, as they watched the onward progress of him whose present discovery was but a first step, of him who, if God is pleased to spare his life, promises to be one of the first scientific men of his age or of any other; that his countrymen have good reason to be proud of him ...[27]

John Herschel, in his report for the Royal Society on the formal paper that Stokes submitted, considered it 'to be one of the most remarkable and important contributions to physical optics which have appeared for a long time'.[28]

Stokes many years later described his discovery as having thrown open 'a new field of research',[29] and he had great hopes for continuing in the same vein, as his reply to a question concerning his 'favorite occupation' from his prospective wife, Mary Robinson, nicely shows: '8. Occupation. Scientific investigations, especially when they lead to discoveries.'[30] Stokes's preoccupied

25 Stokes, *Memoir*, vol. 1, p. 9. Herschel called his observation of what, after Stokes, was understood to be fluorescence, 'epipolic dispersion', and it had not occurred to him that a substance could actually change the wavelength of light. Stokes noted in his Burnett Lectures that he had arrived at his discovery by 'reflecting' on Herschel's epipolic dispersion.

26 According to the recollection of Stokes's sister Elizabeth.

27 Stokes, *Memoir*, vol. 1, p. 10.

28 Stokes, *Memoir*, vol. 1, p. 128. Stokes's discovery stimulated a great deal of interest indeed, including a request from the Prussian Prince of Salm-Horstmar for an appropriate bit of glass to carry out the experiments (p. 136). The correspondence between Stokes and Salm-Horstmar went on for a decade.

29 Stokes, *Memoir*, vol. 1, p. 9.

30 Stokes, *Memoir*, vol. 1, p. 62. His daughter draws an evocative image of Stokes at experiment:

> As a child I loved to watch him working at experiments in his study; I can still see the Rembrandt effect of the strong light and shade cast upon his face, when he opened the shutter from time to time to alter the position of the things resting on the bracket, and the absorbed and delighted expression of his countenance. (*Ibid.*, p. 19)

abstraction, no doubt enhanced by his long bachelor years and his recent success, nearly repelled the young Miss Robinson, who was looking for romance and close companionship. He, at least, found family life extremely congenial, but after his marriage in 1857 he produced nothing quite so original as his *Dynamical theory* nor did he ever again generate something new in the laboratory.[31] His career at the cutting edge of physical research was over by the mid-1850s, even though it had come into full bloom only in 1852.

Psychological and social factors were unquestionably instrumental in collapsing Stokes's career into a comparatively small compass by 1860. Marriage and family life blunted the edge of his none-too-sharp ambition; he immersed himself in Society business to the detriment of research. Most important of all, Stokes mightily feared public commitment, and not only in science. These factors operated in conjunction with his overpowering conviction that partial theories were not enough, that there was little point in developing accounts that had to stop just where they became most interesting. His daughter reminisced that Stokes 'could not bear "scientific romancing", as he called it'.[32] This was already apparent in his work on fluorescence, which, in print, he did not pursue far beyond the discovery of the change in wavelength itself. We can see an oblique reflection of his objection to 'romancing' in remarks he made many years later in his Burnett Lectures:

> in speaking of a change of refrangibility I would guard against being misunderstood. All that is intended is that light of one refrangibility being incident on the substance, light of a different refrangibility is emitted so long as the first remains in action. It is not to be supposed, according to a view which has erroneously been attributed to me by more than one writer, *but which I never for a moment entertained, much less published*, that the refrangibility is changed in the act of reflection from the molecules. The view which I have all along maintained is that the incident vibrations caused an agitation among the ultimate molecules of the body, and that these acted as centres of disturbance to the surrounding ether, the disturbance lasting for a time which, whether it was long enough to be rendered sensible in observation or not, was at any rate very long compared with the time of a single luminous vibration.[33] [emphasis added]

The distinction here between views entertained and views published corresponds to Stokes's lifelong refusal to commit himself in print to anything that seemed to be even slightly speculative, or that lacked analytical and

[31] Of course he was in any case nearly forty when he married and already deeply involved as a secretary in the Royal Society.

[32] Stokes, *Memoir*, vol. 1, p. 33.

[33] Stokes, *On Light*, p. 150.

conceptual rigor.[34] Correcting a claim erroneously attributed to him, Stokes explains his own 'view', one that he never developed in any detail at all.

With an attitude like this, Stokes could hardly have written anything like Maxwell's *Treatise*, nor would he ever have indulged in public talks such as Kelvin's 1884 *Baltimore Lectures* (much less allow them to see print). Maxwell's *Treatise*, which Stokes scarcely ever mentioned, disavowed any claim to explicating through a model the concept of electric charge, and reconstructed electromagnetics in radical fashion with little contemporary support from the laboratory.[35] Kelvin's *Baltimore Lectures*, conversely, went into exactly the kind of speculative detail that Stokes abhorred; he surely regarded many of the lectures as 'scientific romances'. Stokes balanced uncomfortably between these two extremes, unable or unwilling to strike out in either direction. Of course in the early 1860s the extreme *Maxwellianism* of the later *Treatise* was scarcely developed, and Maxwell's reputation was high but not overpowering. Moreover Maxwell had engaged in careful, detailed model-making at just this time in discussing the electromagnetic field. And yet when Stokes produced his own *Report* for the British Association in 1862 he failed to include in it the model that he had himself invented and that, as far as he then knew, was apparently as good as the alternatives that he *did* discuss. Though he excluded the model itself, he nevertheless *did* include its implication for the wave surface: even slightly doubtful models were never to be discussed; statements with direct laboratory consequences could be, albeit tentatively. This episode nicely illustrates Stokes's extreme reluctance to delve publicly into something about which he was not thoroughly certain.

Stokes's *Report* rather neatly divides his career. His seminal work in hydrodynamics, elasticity and diffraction, as well as his discovery of

[34] Stokes passionately insisted on the absolute distinction between private and public views. An apposite example concerns his verbal suggestion to William Thomson concerning the link between the absorption and emission D line for sodium. 'On the strength of this conversation', Stokes wrote to John Lubbock in 1881,

> and of his having mentioned the thing is his lectures to his class, he tried to make out that I was the first to point out the existence of sodium in the sun. I think he was quite wrong; *for if a man's private conversations with his friends are to be brought in, there is an end to all evidence that such a man suggested or pointed out in such a thing*. [emphasis added. Stokes, *Memoir*, vol. 2, p. 76]

The words of disclaimer in this letter are *almost verbatim* the same ones that he had used twenty years before in a letter to Henry Roscoe (viz. 'if a man's conversations with his friends are to enter into the history of a subject there is pretty nearly an end of attaching any mention or discovery to any individual' [p. 83]).

[35] See Jed Z. Buchwald, *From Maxwell to Microphysics* (The University of Chicago Press, 1985) for details. On the mathematical structure of the *Treatise* see Peter Harman, 'Mathematics and reality in Maxwell's dynamical physics', in *Kelvin's Baltimore Lectures and Modern Theoretical Physics*, eds. Robert Kargon and Peter Achinstein (Cambridge, Mass.: The MIT Press, 1987), pp. 267–97.

fluorescence, all precede it. Afterwards, though he wrote many influential papers on limited subjects, he never again achieved the breadth of his previous analytical work nor did he again feel the exhilaration of experimental discovery. The *Report* bears the traces of Stokes's growing recognition that he could not himself see how to unify optics, a recognition that almost certainly acted to dampen his enthusiasm – always well contained in any case – for the kind of 'scientific romancing' that might integrate the subject's disparate branches.[36]

The *Report on double refraction*

Stokes's *Report* was probably the most widely-read article of his career. It dissected a generation of work at the frontiers of optical theory, pointing out where the several theories either failed or where they were, in his view, unsatisfactory. It is worthwhile examining Stokes's remarks here, both for what they tell us about his own outlook in the early 1860s (which froze solidly in place at just about that time), and for what they have to say about research concerning the problem that is so often taken to encapsulate the entire nineteenth century – the structure of the ether.

Stokes quickly rehearsed Fresnel's failure adequately to have derived his several surfaces for birefringence from his ether structure – from the supposition that the ether consists of particles that are mutually repulsive.[37] Cauchy, Stokes continued, remedied this defect in Fresnel's analysis by properly constructing the equation of motion for such a medium.[38] Assuming that the ether's particles are distributed symmetrically with respect to three orthogonal planes, Stokes continued, Cauchy obtained a differential equation with nine disposable constants – the constants being extremely complicated functions of the forces and the spacing of the particles in equilibrium. Three of the constants represent the equilibrium pressures, and Cauchy at first assumed that they vanish. This led to a formula that determines the wave speeds as a function of the direction of the wave normals, or for what one may call (using modern terminology) Cauchy's version of the 'normal surface'. The formula still contained six disposable constants, whereas Fresnel's normal surface contained three, and so Cauchy constrained his expression by requiring a priori that it must have the same sections as Fresnel's surface in the three coordinate planes.

Though Cauchy's result was *not* Fresnel's surface it differed from Fresnel's by amounts that were vastly too small to be detected in the contemporary

[36] And that, according to Glazebrook, *did* nearly succeed in so doing at the hands of Kelvin, a point we will shortly return to.

[37] For details of Fresnel's difficulties see Buchwald, *Wave Theory*, chap. 11.

[38] For details of Cauchy's theory see Jed Z. Buchwald, 'Optics and the theory of the punctiform ether', *Arch. Hist. Exact Sci.* 21(1980):245–78.

laboratory.[39] Despite this apparent empirical success Stokes felt that the theory was unsatisfactory in two respects. First, the relations that Cauchy established by requiring his surface and Fresnel's to have the same sections in the coordinate planes are 'forced', by which he meant that they have no external warrant beyond the necessity of experiment. Second, as in all molecular theories, incompressibility cannot be imposed on Cauchy's structure because its reaction to compression is ineluctably tied to its reaction to shear. Consequently pressure (or longitudinal) waves with finite speeds must necessarily exist in such a medium, whereas optics has to do only with transverse waves.

Stokes offers four reasons that such waves – at least in Cauchy's form – must not occur. First, strictly speaking Cauchy's theory does not imply the existence of *either* transverse (torsion) or longitudinal (pressure) waves, because according to it in every wave the oscillation must have components both in and normal to the front. The waves however divide into two types, for one of which the oscillation is nearly in the front, for the other of which it is nearly perpendicular to the front. Although the former's obliquity is hardly insignificant (amounting to ten degrees for propagation in the principal section of Iceland spar) in fact no empirical test could possibly detect it because observations are made in air, not within the crystal.[40] Stokes nevertheless regarded the implication as objectionable. He felt that a mere turning of the oscillation (from being nearly in to being nearly normal to the front) should not transform it from something that could be detected optically to something that could not be.[41]

This objection to Cauchy's theory was hardly water-tight, but Stokes had other, stronger ones, based on what occurs during crystalline reflection, which necessarily involves both types of waves (the nearly-normal and the nearly-transverse). The boundary conditions that govern this particularly difficult problem were quite uncertain, but Stokes was nonetheless able to provide a rough estimate of the amount of energy that would be carried off by the nearly-normal wave within the crystal.[42] Using Cauchy's implication that the nearly-

39 Stokes found, e.g., that in Arragonite the difference in velocity for waves travelling at equal angles to the principal directions appears only in the 10th decimal place, unity being the velocity in air. 'Such a difference as this', he remarked, 'would of course be utterly insensible in experiment'.

40 So that, e.g., an interference experiment *within a crystal* could not easily be conceived that might detect the obliquity.

41 In his words: 'We can hardly suppose that a mere change of inclination in the direction of vibration of from 10° to 80° with the wave-front makes all the difference whether the wave belongs to a long-known and evident phenomenon, no other than the ordinary refraction in Iceland spar, or not to any visible phenomenon at all.'

42 To do so he chose a plane of incidence such that the oscillation (which, remember, is *in* the plane of polarization) is inclined at 10 and 80 degrees respectively to the nearly

normal wave has a speed equal to $\sqrt{3}$ times that of the nearly-transverse wave, Stokes deduced from this that the former's intensity must be $\frac{1}{26}$ that of the latter which, he continued, 'is by no means insignificant, and therefore it is a very serious objection to the theory that no corresponding phenomenon should have been discovered'. Even if the phenomenon itself had not been observed the loss of energy involved in its existence would unquestionably have shown itself in many different kinds of experiments.[43]

Clearly any pressure wave that would carry off, or generate, a detectable amount of energy was empirically unacceptable. However the greater the speed of the normal wave the *less* energy it carries off, so that if it were possible to make the speed sufficiently large then the empirical difficulty could be avoided. This is precisely what can be done, it turns out, if in Cauchy's equations the equilibrium pressures do not vanish, and the oscillation is in consequence normal to the plane of polarization. Here again Fresnel's normal surface does not emerge exactly, but the difference is once more undetectable. This theory – which can grant the normal wave high speed in virtue of an extra disposable constant – 'to a certain extent' alleviates the difficulty, Stokes admitted, though it still suffers from arbitrariness.

Only George Green's analysis came close to satisfying Stokes's rigorous strictures on what a satisfactory theory for double refraction would be, and even it failed. Green took his stand on what Stokes termed 'the method of Lagrange': he developed a potential energy density for a completely general strain and applied what Green termed d'Alembert's principle' to it to obtain differential equations of motion and boundary conditions (the latter through partial integration). The general density contains twenty-one coefficients among which relations must be established to obtain the normal surface. This would seem to make it even *more* arbitrary than Cauchy's equations, but Stokes thought not. On the contrary, he felt that Green's potential suffered from a great deal less arbitrariness because it could be constrained in a manner much more closely tied to the basic requirements of the wave theory than Cauchy's equations could be.

The relations required by Cauchy are arbitrary in that their *sole* justification is the necessity to reach Fresnel's normal surface as closely as possible, and even then the result only approximates Fresnel's surface, at the further cost of incompletely satisfying transversality within the crystal. Green's method for

transverse and the nearly normal oscillations that occur within the crystal. Stokes then takes refracted amplitudes to be approximately proportional to the projections of the incident amplitude onto their directions. This amounts to a tacit boundary condition, albeit an admittedly approximate one.

43 Stokes also brought up Green's old objection to normal waves – that, if they do exist, then one would expect them to generate nearly-transverse waves on reflection, and yet no unaccounted-for waves had ever been observed.

constraining his coefficients is thoroughly different from Cauchy's, because he did not have to refer in any way at all *ab initio* to Fresnel's normal surface. Instead, Green required that, of the three possible waves which his equations permitted in general, two must have their oscillations restricted entirely to the wave front. In other words Green took exact transversality within the crystal as the constraint, whereas Cauchy had abandoned transversality and used instead the sections of Fresnel's normal surface. In this way Green was able *exactly* to obtain Fresnel's surface, but only at the cost of having the oscillation *in* the plane of polarization and not normal to it, supposing the equilibrium pressures to vanish.

Stokes felt that Green's method approached perfection, but that there were unfortunately other arguments against the particular theory that he had developed:

> Were it not that other phenomena of light lead us rather to the conclusion that the vibrations are perpendicular, than that they are parallel to the plane of polarization, this theory would seem to leave us nothing to desire, except to prove that we had a right to neglect the *direct* action of the ponderable molecules, and to treat the ether within a crystal as a single elastic medium, of which the elasticity was different in different directions.

Green, like Cauchy, also essayed a theory in which the equilibrium pressures do not vanish – and so in which the oscillation is *normal* to the plane of polarization. He was again able to obtain Fresnel's surface exactly, but this time only at the cost of introducing a certain arbitrariness (in Stokes's sense) into the constraints that governed his coefficients, for he had to introduce a condition that derived from his goal – Fresnel's surface of elasticity.[44]

The only other theory that Stokes considered had been developed by the Irish mathematician James MacCullagh. It became extremely influential twenty years later, when its analytical structure proved to be similar to that required by Maxwell's electromagnetic field theory.[45] At the time, however, Stokes thought it to be unacceptable because, though it did give Fresnel's normal surface exactly, and though it did (like Green's) utilize a potential in the fashion that Stokes approved, nevertheless it was not 'mechanically' acceptable.[46] Unlike the later Maxwellians, Stokes always felt that dynamical

44 This surface is such that the semi-axes of its sections by a plane give the directions of polarization and the wave speeds for fronts that are parallel to the section. The normal surface follows easily from it. See Buchwald, *Wave Theory*, appendix 5 for details.

45 See Buchwald, *From Maxwell*, appendices 2 and 4 for details.

46 The difficulty amounted to this: MacCullagh had not employed Green's potential, but had rather developed one that seemed to be completely different from it (though in fact MacCullagh's potential can be obtained from Green's by dropping certain terms from the latter). The resulting energy density is proportional to the square of the medium's absolute rotation at a point ($\nabla \times \mathbf{u}$). This, Stokes could easily show, meant that the

structure in itself – that is to say, workable potential and kinetic energy densities – did not suffice, that the structure had to be 'mechanically' realizable. He accordingly remarked that MacCullagh's 'methods have been characterized as a sort of mathematical induction', and, though useful, they were not in themselves sufficient for theory construction.

So even here, in the highly limited area of double refraction, Stokes was unsatisfied. And yet one might easily wonder *why* he seems to have been so convinced that there was little purpose in pursuing the subject. For even in 1862 he himself suspected that there might be a satisfactory route to a theory of double refraction. Indeed, he more than suspected it, he even knew what it was and how to formulate it. We shall now see that, had he pursued the issue, he might in the early 1860s have discovered what twenty-six years later (when the point had in any case become essentially moot) *did* prove to be a satisfying unification of optics on mechanical (and not merely dynamical) principles.

Stokes's failure to seize the moment

Although Stokes had carefully limited his report to double refraction, he was well aware that reflection theory was, if anything, an even more difficult subject. In double refraction, as we have seen, there were ways to generate the results that experiment demanded. Reflection theory failed even in this minimally-necessary respect.[47] Yet Stokes had to hand an idea that could be applied successfully to *both* double refraction and to reflection, an idea whose consequences for double refraction he mentioned in 1862. That theory does not lead to Fresnel's surface of elasticity but to one that is its reciprocal.[48] 'In the present state of the theory of double refraction', Stokes insisted, 'it appears to be of especial importance to attend to a rigorous comparison of its laws with actual observation' – that is, to find out whether the replacement works.

Yet Stokes did not undertake the investigation for five years, despite the fact that he loved experimental work, that he had conceived the theory behind the new surface nearly *twenty years before* (in 1843), and that Rankine had independently published much the same thing shortly after Stokes's *Report on*

medium as it stood violated conservation of angular momentum (since the corresponding 'stress' tensor is not symmetric). This was of course unacceptable, yet there was an escape that Stokes himself recognized. If the continuum itself produces a torque in reaction then conservation can be maintained. But Stokes did not see how this could occur, and he was not willing simply to assume that it did.

47 For a brief discussion see Buchwald, *From Maxwell*, appendix 2. Whittaker, *History*, vol. 1, chap. 5 provides a great deal of insight into the many difficult questions of the period, though it often elides problems that arose decades apart.

48 Fresnel's surface has the form $a^2x^2+b^2y^2+c^2z^2 = 1$; Stokes's suggested replacement for it is $x^2/a^2+y^2/b^2+z^2/c^2 = 1$.

double refraction was printed. In fact, the idea was sufficiently obvious that the young Rayleigh, unaware of either Rankine's *or* Stokes's work on the point, himself re-invented the theory in 1871. In March of that year Stokes wrote him the following letter, which is worth quoting at length since it is the only detailed evidence that he ever provided concerning these developments:

> Prof. Maxwell called my attention a day or two ago to your paper on double refraction ... I have just been reading it.
> In a paper of mine on some cases of fluid motion [... about 1843 ...] I obtained an expression for the equivalent inertia of an incompressible fluid moving relatively to a solid ... I saw at the time this would lead to a theory of double refraction, differing from Fresnel's in having reciprocals of velocities in place of velocities themselves. But having calculated the velocity at 45° to the axis in Iceland spar, I found it to differ from that given by Huyghens' construction by a quantity large enough to deter me from publishing the result without a careful scrutiny of the observations of Wollaston and Malus to see whether such an error could be tolerated. I had always a hankering after this theory, and developed it for myself much as you have done, and even investigated the form of the wave surface. After my experiments on diffraction came out Rankine published in the *Phil. Mag.* a similar theory. About four years ago I carried out the suggestion in my Report for examination by prismatic refraction on a crystal of Iceland spar ... The result was perfectly decisive. The *difference of inertia theory must be rejected, and Huyghens' construction adhered to.* The difference between the results of the two theories is something like 100 times the probable errors of observation. I ought to have published the results before this.[49]

So we see that by the time of his *Report on double refraction* Stokes had had a theory in hand for nearly twenty years that *might* have worked (he only suspected that it would not), that he discussed its consequences in his *Report*, that he there urged new empirical investigations, that he did not himself undertake them for five years, and that he did not publish his results until Rayleigh revived the theory a few years later.

This 'difference of inertia' theory, as Stokes called it, differs fundamentally from every contemporary alternative because it does not at all alter the ether's coefficients of elasticity. Instead, it transforms (in effect) the ether's density into a symmetric tensor, which is to say that it makes the ether's inertial reaction depend upon direction.[50] But why was he so attracted by this theory?

49 Stokes, *Memoir*, vol. 2, pp. 99–100. See J. W. Strutt, 'On double refraction', *Phil. Mag.* 48(1871):369–81 for his independent discovery of the Stokes–Rankine inertia theory.

50 The difference between the inertia and elasticity theories can be formulated in the following way. Suppose that we require (as Stokes did) that every theory must be derivable from variational principles ('the method of Lagrange') applied to the ether's

The answer is, I believe, quite simple and can be divined from remarks that Stokes made in his refraction report. After criticizing the elasticity theories, but before presenting the results (not the substance) of the inertia alternative to them, he concluded:

> The various theories which have just been reviewed have this one feature in common, that in all, the direct action of the ponderable molecules is neglected, and the ether treated as a single vibrating medium. It was, doubtless, the extreme difficulty of determining the motion of one of two mutually penetrating media that led mathematicians to adopt this, at first sight, unnatural supposition; but the conviction seems by some to have been entertained from the first, and to have forced itself upon the minds of others, that the ponderable molecules must be taken into account in a far more direct manner.
>
> ... In concluding this part of the subject, I may perhaps be permitted to express my own belief that the true dynamical theory of double refraction has yet to be found.

That missing 'true dynamical theory', Stokes evidently hoped, lay in the idea of the ether's anisotropic inertia because that was how material particles could most naturally act upon the ether.

What most troubled Stokes about the elasticity theories had little to do with their empirical adequacy since all of them seemed to yield reasonably accurate results in double refraction. Nor was he profoundly troubled by the difficulty of melding them with an adequate account of reflection, since the latter was in any case missing because (Stokes felt) of difficulties in forming appropriate boundary conditions. Rather, in his eyes their inadequacy derived primarily from what he felt to be their arbitrary assimilation of matter's affect on ether to a change in the internal forces that govern the latter.

Stokes was hardly alone in this sentiment, and it is not at all surprising that he first strongly felt it as early as 1843. In 1842 Matthew O'Brien, whom Stokes probable knew quite well,[51] had proposed an intricate scheme, modeled on Cauchy's, in which two interpenetrating systems of particles – the one representing ether, the other matter – act upon one another. The resulting equations were extremely complicated, and O'Brien was not able to obtain much from it.[52] Stokes knew also that Cauchy had attempted to develop such

energy density. The elasticity theories begin with a hypothetical expression for the density of the potential energy, but they leave the kinetic density alone. The inertia theory does the reverse: it introduces a new form for the kinetic density, but it leaves the potential energy untouched.

51 Stokes thanked 'his friend' O'Brien in the 1849 *Dynamical theory* for providing him with certain instruments.

52 Though it sufficed to immerse him in controversy with Kelland and Earnshaw over precisely how to represent the effect of the material on the ethereal particles. Stokes never mentioned the controversy, which he would certainly have found distasteful,

a theory (in ultimate response to an extremely damaging critique developed by MacCullagh), and he was no doubt thinking of all such theories when he remarked in the *Report* that Cauchy 'does not seem to have advanced beyond a few barren generalities, towards a theory of double refraction founded on a calculation of the vibrations of one of two mutually penetrating media.'[53]

Instead of these flamboyant schemes, which invented forces and spread particles about according to whatever hypothetical symmetries seemed to be necessary, Stokes's inertial theory made the simplest possible assumption. Namely, that whatever else they may do to the ether, *material particles act to load its motions* since both they and the ether posses inertia, much as a grape embedded in a jelly would load it: the vibrating ether (jelly) must carry the material particle (grape) along. In crystals the loading would be anisotropic (due to some sort of anisotropy in *material* – not ethereal – structure), and elaborate, arbitrary mathematics are not necessary to represent the possibility.

While O'Brien and others in the early 1840s were avidly publishing their blueprints for complicated ether–matter lattices of point particles, Stokes remained silent. He had an alternative, though one that might not work empirically. Yet he did not examine it in the laboratory, nor did he even publish it. Twenty years later he mentioned the possibility in print, called for experiments, and then he did not perform them. When he did finally carry them out he remained silent until someone else rediscovered the theory.

Stokes's initial reticence fits his lifelong pattern of refusing even to mention in print something about which he was at all doubtful. His subsequent failure to test the hypothesis shows something else as well. It shows that Stokes would generally not carry through an experimental investigation unless he felt that it would produce novel results, new discoveries. Both his examination of polarization in diffraction and his discovery of fluorescence five to ten years later *did* produce something new. At best, he knew, an examination of double refraction would indicate either that the inertia theory was unacceptable or else that contemporary technique could not tell the difference between its requirements and Fresnel's normal surface.[54] There was, consequently, little

and in any case he felt that the entire scheme was thoroughly misguided. For details see Buchwald, 'Punctiform ether' and 'The quantitative ether in the first half of the nineteenth century', in Cantor and Hodge (eds.), *Conceptions of Ether* (Cambridge University Press, 1981), chap. 7. Ironically, considering Stokes's critical remarks about his work, MacCullagh was motivated by very much the same distaste for that kind of theorizing.

53 This was not entirely fair, since Cauchy *had* developed an extremely elaborate scheme, although it was based rather on a new form of differential equation (one with periodic coefficients) than it was on a specific mechanical structure. See Buchwald, 'Punctiform ether' for details.

54 Fresnel's surface worked extremely well at the accuracies of Wollaston's and Malus's old experiments, which were about 1 % or so (see Buchwald, *Wave Theory*, chaps. 1–2

incentive for Stokes, who was in any case so prudent, to spend his time pursuing the issue, particularly since any publication about it in the early 1840s was certain to land him in the midst of controversy.

The situation had however changed by the early 1860s. The danger of angry controversy was past, in part because the old issues were dead, and in part because Stokes's reputation could easily suppress it. Yet he still refused to publish more than a snippet or, for five years, to try it in the laboratory. No doubt he continued to feel that the likelihood of success in such an investigation was small. This – together with Stokes's acknowledged tendency to procrastinate and his immersion in Society business – again put the issue aside until he found the time to look into it, which evidently occurred sometime during 1867. And then the results were just what he had long anticipated – the inertia surface failed abysmally.

We might end the story there – what after all is there to say after such a definitive result from the laboratory? – were it not for one thing. Stokes was wrong. Not about the failure of the *surface* that he had deduced: it certainly was frustrated by experiment. Rather, the *theory* he had developed did not have to be abandoned. It could have been evolved into something extraordinarily successful had Stokes persevered with it – if he had had the confidence, and perhaps the competitive desire, to examine precisely *why* the theory led to the empirically-unacceptable surface.

The belated success of mechanical optics

A quarter-century after Stokes closed the book on contemporary optical theory his old friend, William Thomson (by then Lord Kelvin), re-opened it in a remarkable article that was published in the *Philosophical Magazine*. Thomson remarked:

> Having ... after a great variety of previous efforts which had been commenced in connexion with preparations for my Baltimore Lectures of this time four years ago, seemingly exhausted possibilities in respect to *incompressible* elastic solid [for generating a theory of reflection], without losing faith either in light or in dynamics, and knowing that the condensational-rarefactional wave disqualifies any elastic solid of *positive*

for details). Any new surface had to be at least this accurate, so either it would rapidly prove inadequate, or else (in these antique experiments) no better than Fresnel's. Given no difference between them in these experiments, new ones would have to be performed, and these would not likely uphold the inertia surface *over* Fresnel's since the latter had been successfully used in a wide variety of experiments. At best, then, more accurate experiments would show only that the new surface was tenable – and so that the difference between it and Fresnel lay at the boundary of contemporary technique. In other words the most that could be hoped for from experiment would be a demonstration of *possibility*.

> compressibility, I saw that nothing was left but a solid of such negative compressibility as should make the velocity of the condensational-rarefactional wave zero. So I tried it ...[55]

The first point to remark here is that Thomson in the 1880s, unlike Stokes in the early 1860s, was particularly concerned to retrieve Fresnel's reflection laws from ether structure. Second, Thomson's own lectures in Baltimore had stimulated him to probe deeply *why* only one of the two laws could be deduced in this way.[56] Third, Thomson was well prepared to formulate a new attack because he was convinced throughout his life that the ether must be able to sustain pressure waves.[57]

Since the time of Green, and particularly with the advent of Maxwellian electrodynamics, British physicists had tacitly assumed that the ether is incompressible. The first stimulus to this belief had been Green's apparent demonstration that otherwise it would be unstable, that a slight disturbance would cause it to collapse. Thomson, convinced for decades for a complex of reasons that the ether should be compressible, began in the mid 1880s to wonder about Green's demonstration of instability. He discovered a hidden flaw, or, rather, that it contained a hidden supposition. If, he demonstrated, we take Green's potential and perform a partial integration, with a boundary at infinity, then Green's conclusion – that an ether without compressibility must be unstable – fails provided that we ignore the resulting surface integral.[58] This permitted Thomson to assume the precise opposite of Green: namely, that, far from being incompressible, the ether is infinitely compressible (i.e. that it stores no energy in compression). This 'labile' structure, he went on to show, successfully reproduces both of Fresnel's reflection laws provided, significantly, one assumes that the elasticities remain the same across media boundaries but that the densities alter.

But what of double refraction? Thomson did not treat the subject, but later that same year the British Maxwellian and optical specialist, Richard Tetley Glazebrook did. Glazebrook was particularly well-prepared to take up the subject since his first major published work concerned experiments on birefringence that were directly motivated by Stokes's 1862 *Report*. That work

55 W. Thomson (Lord Kelvin), 'On the reflexion and refraction of light', *Phil. Mag.* 26(1888), pp. 414–25, on p. 414.

56 See W. Thomson, *Baltimore Lectures on Molecular Dynamics and the Wave Theory of Light* (1884). Stenographically reported by A. S. Hathaway. In particular, it was quite simple to obtain Fresnel's sine law for light polarized in the plane of reflection, but no one had succeeded in deducing his tangent law for light polarized in the perpendicular plane, at least not without introducing controversial principles that went beyond the normal constraints imposed by elasticity. See Buchwald, *From Maxwell*, Appendix 2 for details.

57 See, e.g., Norton Wise, 'Mediating machines' in *Science in Context* 2(1988):77–113, on p. 107.

58 Which means that there is no displacement at infinity.

was done at the Cavendish Laboratory, where he was a demonstrator, and was communicated to the Royal Society in 1878 by Maxwell himself. At the time Glazebrook was a fellow of Trinity College. He knew Stokes (who guided his work) and had attended his lectures on optics while an undergraduate. It seems that a major purpose of Glazebrook's work was to see just how badly the old Stokes–Rayleigh theory for double refraction, which required the density to become anisotropic, failed. He remarked after presenting an elaborate series of data:

> though some of the apparent difference [between the Rayleigh–Stokes equation and experiment] may be due to the error made in assuming the principal plane of the prism to coincide with one of the crystal, that cannot account for the whole; for we have seen that in Fresnel's surface the error made by the same assumption appears only in the fourth place of decimals, in the value of the refractive index, while the differences between Lord Rayleigh's theory and experiment show themselves in the third place, and tend to increase [with the incidence].
>
> Thus it seems that Lord Rayleigh's theory will not account for the phenomena of double refraction in arragonite. This result agrees with that arrived at by Professor Stokes for Iceland spar.[59]

But in 1888 Glazebrook, thoroughly familiar with the structure of the Rayleigh–Stokes theory, now saw that Thomson's referral of optical processes to changes in the effective ether density could be used to rescue it from empirical disaster. He easily demonstrated that Fresnel's original surface will emerge *exactly* if, as Thomson required, the ether has no resistance to compression.[60] This would have been a stunning result in the early 1860s, for it meant that one could generate a consistent theory for reflection and double refraction on the basis of Green's potential – on the basis, that is, of an elastic solid – provided densities, but not elasticities, are manipulated. However by the late 1880s this was hardly enough. By then anomalous dispersion, absorption and metallic reflection were at the center of many optical physicists' attention. Glazebrook, who had written the century's third *Report on optics* for the British Association in 1885, knew in detail how continental physicists, particularly in Germany but also elsewhere, had created structures for optics that embraced the new phenomena. They had done so by building two equations of motion. One, for the ether, never changed its form, but it contained a term that linked it to the second equation, which governed matter.

59 R. T. Glazebrook, 'An experimental determination of the values of the velocities of normal propagation of plane waves in different directions in a biaxal crystal, and a comparison of the results with theory', *Phil. Trans.* (1879):287–377, on p. 318.

60 R. T. Glazebrook, 'On the application of Sir William Thomson's theory of a contractile aether to double refraction, dispersion, metallic reflection, and other optical problems', *Phil. Mag.* 26(1888):521–40.

That equation could be manipulated according to necessity, and German physicists in particular had built a cottage industry on doing so.[61] Glazebrook now sought to *adapt* their methods to Thomson's labile ether. He remarked:

> Refraction occurs because the optical density of the aether is different in different media; double refraction, because in a crystal the optical density is different in different directions.
> It remains now to consider what is meant by the optical density of the aether, and how it can vary in different media, or in different directions in the same medium. The phenomena of aberration and the other optical effects produced by the motion of transparent bodies are more easily explicable if we suppose the actual density of the aether as well as its rigidity to be the same in all bodies.[62] Let us make this supposition for the present. Now the motion of the aether within a transparent body is not free; in addition to the forces arising from its own rigidity there must be others arising from the action of the transparent matter; and though we are ignorant of the nature of this action we can show, remembering that light-waves travel through the medium with a velocity which is independent of the amplitude, that the forces resolve themselves into two sets. One of these makes its appearance in such a way as to be equivalent to an increase in the density of the aether, while the other is equivalent to an increase in its rigidity.[63]

Where German physicists had taken their stand on manipulating the ether–matter link, and the material equation, in a way that changed the ether's effective *elasticity*, and with only partial success, Glazebrook now demonstrated that modifying the effective *density* leads to a thoroughly comprehensive optics.

Glazebrook's demonstration, as well as Thomson's original paper, generated very little contemporary reaction.[64] It is as though British physicists

[61] For a brief discussion of the origins of this 'twin equation' structure, which derives from Helmholtz's response to the discovery of anomalous dispersion, see Buchwald, *From Maxwell*, chap. 27.

[62] Referring implicitly here to Stokes's theory for aberration, which required the earth completely to drag the ether along. That theory was already in trouble, though Glazebrook was not aware of the fact, and in any case he was not interested in aberration *per se*; he wanted only to provide a nice, extrinsic reason for accepting what he was about to base an entire new optics on. In general the problem of optics for moving bodies held very little interest for most physicists, in Britain or on the Continent, until well into the 1890s: see Buchwald, 'Michelson experiment'.

[63] Glazebrook, 'On the application', pp. 530–1.

[64] One major exception to the silence was Gibbs, who regarded the labile ether as a major accomplishment, who wrote that 'A REMARKABLE [*sic*] paper by Sir William Thomson ... has opened a new vista in the possibilities of the theory of an elastic ether'. (J. W. Gibbs, 'A comparison of the electric theory of light and Sir William Thomson's theory of a quasi-labile ether', *American Journal of Science* 37(1889): 139–144). Gibbs

regarded what was apparently a long-sought unification with collective apathy. The reason was that, as Glazebrook himself pointed out, one could – analytically at least – do just as well with Maxwell's electromagnetic field theory.[65] Interesting but anti-climactic, the Thomson–Glazebrook unification of optics was simply not at the forefront of contemporary research. Stokes made no public remarks about it at all, though he was hardly a convinced Maxwellian.

For our present purposes three aspects of the unification are particularly striking. First, it derives ultimately from an *analytical* perception on Thomson's part which was based on a prior belief that the ether must be compressible. Second, the account of double refraction to which it naturally leads amounts to the one that Stokes had himself quietly developed nearly a half-century before. Third, Glazebrook's extension of the scheme was founded on the concept that ether and matter interact without altering one another's inherent structure, that their mutual affects are to be sought in a single mathematical link that expresses their dynamical connection. Of these three, the first two were available to Stokes in the early 1860s, and he was himself instrumental in suggesting the third.[66] Stokes's enduring belief that the ether is

went on to demonstrate that Thomson's labile ether satisfies the same equations as the electromagnetic field in non-conducing media. This is perhaps obvious in retrospect because the labile ether's potential function is essentially the same as MacCullagh's, and the latter governs the electromagnetic field (see Buchwald, *From Maxwell*, Appendix 2 for further details). There is however a difference between the two subjects' energy functions which involves the divergence of the displacement. Gibbs pointed out that crystalline refraction could be used to probe the disagreement, but that recent experiments on birefringence in Iceland spar 'do not encourage us to look in this direction for the decision of the question'. Gibbs felt in the end that the 'electrical theory' remained superior to the mechanical because 'it is not obliged to invent hypotheses, but only to apply the laws furnished by the science of electricity'. (I thank Martin Klein for emphasizing to me the importance of Gibbs's paper in this

65 Specifically, Glazebrook noted with some exaggeration:

> There seems... to be no reason – as has been pointed out by Professor Fitzgerald – against applying to the oscillations of the electro-magnetic field the methods and reasoning [of the twin-equation system]. Almost the whole of the work can be translated into the language of the electro-magnetic theory at once. Periodic electric displacement in the ether will produce periodic electric displacement in the matter, and the relations between the two will depend on the ratio of the period of the ether vibrations to the possible free periods of the electric oscillations in the matter molecules; and it is not difficult to see how the action between the two might depend on the relative electrical displacements and their differential coefficients. (Glazebrook, *Report*, p. 256)

66 Since it was Stokes who had insisted on the necessity of considering the ether–matter connection in optics, and who had himself devoted much attention to it in his discussion of fluorescence, which he suggested might involve an anharmonic material restoring force.

incompressible goes far to explain why he did not see what Thomson, who thought otherwise, found many years later. Nevertheless physicists do often play with their mathematics and concepts in restricted ways when faced with empirical recalcitrance, particularly when they are convinced that their overall approach requires preservation. Stokes did not play; he did not attempt to preserve. He made no effort *at all* to manipulate his equations. I believe that the primary reason for Stokes's apparent refusal to carry on, to probe further was his nearly palpable distrust of 'scientific romance'. As late as 1883, when he undoubtedly knew that there were excellent reasons for thinking that paths to a comprehensive optics had opened, he still refused to move very far from certainty, remarking in his Burnett Lectures:

> It may readily be imagined, as more probable than the contrary, that the presence of the ponderable molecules interspersed through the ether... may have the effect of altering the velocity of propagation of the ethereal disturbances ... and very probably diminish it. But what may be the precise mechanism by which this result is brought about we do not know. It is easy to frame plausible hypotheses which would account for the result, but it is quite another matter to establish a theory which will admit of, and which will sustain, cross-questioning in such a variety of ways that we become convinced of its truth.[67]

The foundation of Stokes's variable-density formulation for ether dynamics was this very belief that matter does not affect the structure of the ether, but rather that it merely adds to the mass that moves *with* the ether. That much Stokes always admitted to be 'probable', but he never pursued it intensely at any time, and when the experiments that he finally performed did not favor it, he let the theory die. Stokes stimulated new physics but he rarely produced it after the mid-1850s.

[67] Stokes, *On Light*, p. 81.