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Ideological crests versus empirical troughs: John Herschel's and William Radcliffe Birt's research on atmospheric waves, 1843–50

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INTRODUCTION

The year was 1843, and the theme of English meteorology was measurement. Sir 'Thunder-and-lightning' William Snow Harris was given his last British Association for the Advancement of Science grant to complete the Plymouth series of over 120,000 thermometric observations, publication of which proved a costly venture, not least because the series implied no meteorological theory whatsoever.

In July of that year, however, John Herschel wrote to William Radcliffe Birt that the atmosphere might be considered 'a vehicle for wave like movement which may embrace in their single swell & fall a whole quadrant of a globe'. The idea of 'atmospheric wave', thought Herschel, might well make sense of the odd series of London barometric readings made in September 1836, but, more significantly, it might also lead towards solving the notorious 'storm controversy' of the 1830s between the American meteorologists William Redfield and James Pollard Espy. If Birt would accept, Herschel would propose him to the British Association as the director of the new project to discover laws of weather behaviour.[1]

Birt had already acted as Herschel's computing assistant in the arrangement and reduction of many series of barometric measurements and was enthusiastic to accept the offer. The British Association eventually entrusted him to investigate the properties of atmospheric waves, but in doing so, however, it unwittingly prepared the ground for the failure of what Joseph Agassi once called Herschel's 'philosophy of success'.[2] After five reports for the British Association and several contributions to the *Philosophical Magazine*,

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1 John Herschel to William R. Birt, 28 July 1843, Royal Society of London, Archives, Herschel Papers 19.106 (hereafter RS:HS).

2 Joseph Agassi, 'Sir John Herschel's philosophy of success', *Historical Studies in the Physical Sciences* (1969), **1**, 33, 34.

Birt dropped the research in 1849 without a conclusive explanation of mid-latitude atmospheric disturbances. He declared that the six-year effort was less than rewarding and subsequently returned to astronomy, his first scientific love.

Some time ago, Bruno Latour and Steve Woolgar suggested that scientists 'invest their credibility where it is likely to be most rewarding', that is, where their intellectual capital immediately acquires a high value owing to the supply and demand of the scientific 'market'. They add that a scientist's assessment of the fluctuations of the market explains his or her reference to 'interesting problems' and moves between 'problem areas', in which a scientist enters into collaborative projects, grasping and dropping hypotheses as the circumstances demand.[3] I will not here be so concerned with economic models of scientists' professional behaviour, but the metaphor of credibility-investment seems a fruitful way to make sense of what motivated the research on atmospheric waves undertaken by Herschel and Birt during the 1840s. The state of the sciences in the 1840s, as Cannon has observed, was characterized by 'unexplained phenomena which were powerful enough to compel some sort of theories, and scientists were available adequate to supply theories'.[4]

Indeed, Birt's and Herschel's publications and correspondence reveal the complex of interests that in the early years of the British Association created an increased demand for meteorological research among the members of the British scientific clerisy. Schemes for gathering observations were drawn up and, when the rhetoric of method changed to emphasize the value of hypothesis in science, theoretical concepts were advanced to explain data. Herschel may thus be said to have invested his intellectual credibility in 'atmospheric waves' when he claimed them a candidate for the explanation of large atmospheric disturbances. For the reasons examined below the idea proved increasingly difficult to present as hard scientific currency. Its explanatory value progressively decreased. Eventually it was hard to maintain and impossible to sell: in 1850, the Royal Society of London did not think the idea of atmospheric waves worth even a £50 government grant.

In this paper I will propose that the Birt–Herschel research into atmospheric waves emerged as a response to the contemporary rhetoric about the unsophisticated state of British meteorology and the efforts of the British Association to ameliorate this state. The failure of the research to make an impact, especially on the theory of storms, may perhaps be explained not only by reference to the theoretical problems that plagued the research from the beginning, but also as a result of the changed public legitimation of meteorology, which in the early 1850s sought practical rather than theoretical results.[5]

Even though the prominence of the Humboldtian sciences during the early years of the British Association has often been stressed, meteorology has received a relatively meagre treatment both within and without that general agenda.[6] This historiographical lacuna is

3 Bruno Latour and Steve Woolgar, *Laboratory Life. The Social Construction of Scientific Facts*, Beverly Hills, 1979, 206–7.

4 Susan Faye Cannon, *Science and Culture: The Early Victorian Period*, New York, 1978, 237.

5 See Jim Burton, 'Robert Fitzroy and the early history of the Meteorological Office', *BJHS* (1986), **19**, 147–76.

6 On terrestrial magnetism see J. Cawood, 'The magnetic crusade: science and politics in early Victorian Britain', *Isis* (1979), **70**, 493–518, and 'Terrestrial magnetism and the development of international collaboration in the early nineteenth century', *Annals of Science* (1977), **34**, 551–87. Apart from Burton's article (op. cit. (5)), aspects of meteorological research are discussed in John L. Davis, 'Weather forecasting and the development of

unjustified in the light of nineteenth-century activities. In his *Preliminary Discourse* (1830) Herschel described meteorology as 'one of the most complicated but important branches of science' in which 'any person who will attend to plain rules' might do effective service. With Charles Babbage he tinkered with barometers and took his turn in the Grand Tour 'barometrizing' in the Italian Alps; in 1833, on the way to the Cape of Good Hope, he incidentally rediscovered the equatorial depression in atmospheric pressure.[7] In South Africa Herschel campaigned to establish a network of observers; he published on matters of instrumental accuracy, measurement error and reduction procedures. In 1836 he gave instructions for registering horary observations at the solstices and equinoxes, hoping to see them become 'great meteorological festivals when every observer must be at his post, over all the world'.[8] It was owing to this cosmopolitan enthusiasm that he began proselytizing for an extensive empirical enterprise directed toward understanding non-periodical barometric fluctuations, or what a German contemporary called the mutual reaction between the greater atmospheric currents over large geographical regions.[9] The great unperiodic gales ('cyclones', or 'storms' in modern vocabulary) were on that view considered to depend on causes extrinsic to the regular barometric oscillations, that is, on those of 'local and transient nature', which an organized weather record would unmistakably bring to light. 'A working society', enthused Herschel, 'is wanted to go into all details and dwell upon the subject *till something comes out*'.[10]

Put in context, Herschel's proposals for organizing the recording and reduction of observations, his plea for standardization and his later active role in promoting meteorological theory were reflections of a general discontent that the men of science had been for some time expressing in respect to the status of meteorology. While this discontent may be seen as a side-effect of the notorious debate on the decline of English science, it is more important to emphasize that meteorology suffered critical blows from a much earlier period. In particular, the prediction of weather was a traditional target of criticism and doubt. In 1763, the Reverend William Borlase announced the impossibility of both

meteorological theory at the Paris Observatory, 1853–1878', *Annals of Science* (1984), **41**, 359–82; J. M. Walker, 'The meteorological societies of London', *Weather* (1993), **48**, 364–72; P. J. Cockrell, 'The Meteorological Society of London: 1823–1873', *Weather* (1968), **23**, 357–61; George J. Symons, 'The history of English meteorological societies, 1823 to 1880', *Quarterly Journal of the Royal Meteorological Society* (1881), **7**, 66–8; R. P. W. Lewis, 'The founding of the Meteorological Office, 1854–55', *Meteorological Magazine* (1981), **110**, 221–7. A recent comprehensive treatment of the meteorological theory is Gisela Kutzbach, *The Thermal Theory of Cyclones: A History of Meteorological Thought in the Nineteenth Century*, Boston, MA, 1979.

7 Alexander von Humboldt was credited with being first to describe the phenomenon and Herschel acknowledged his priority. John Herschel, *Meteorology. From the Encyclopaedia Britannica*, Edinburgh, 1861, 54.

8 John Herschel, *Instructions for Making and Registering Meteorological Observations at Various Stations in South Africa and Other Countries in the South Seas, as also at Sea*, 1835. Quoted from Jack Morrell and Arnold Thackray, *Gentlemen of Science. Early Years of the British Association for the Advancement of Science*, Oxford, 1982, 519 n283.

9 For discussion of the theoretical developments, in early nineteenth-century Europe see Kutzbach, op. cit. (6); A. Kh. Khrgian, *Meteorology: A Historical Survey*, Jerusalem, 1970. Earlier researches are surveyed in Theodore Feldman, 'Late Enlightenment meteorology', in *The Quantifying Spirit in the Eighteenth Century* (ed. T. Frangsmyr, J. L. Heilbron and R. E. Rider), Berkeley, 1990, 143–79.

10 [John Herschel], *Report of the Meteorological Committee of the South African Literary and Philosophical Institution*, Cape Town, 1836, 246.

understanding and foretelling the atmospheric changes 'from analogy and review'.[11] In 1807, Louis Cotte asserted that the physics of meteors is 'the least advanced part of meteorology. The useless attempts so far to find a theory to predict the weather from existing series of observations – these futile attempts, I say, prove that we do not know well enough the history of atmospheric phenomena.'[12] In 1839, Henry Ince said that 'this dubious and very limited anticipation of the changes, which occur in the medium we breathe, is merely the fruit of personal assiduity and application … it receives, therefore, no accession in the progress of ages, but perishes with the individual'.[13] Whether fairly or not, early nineteenth-century commentators then erupted with criticisms of a general lethargy that supposedly prevailed in the investigation of weather-systems, of the insufficiency *and* profusion of observations, of the public uselessness of the existing stock of facts, and of the imprecision of means for standardizing and using meteorological instruments.

For instance, the meteorological committee appointed in 1814 by the Royal Society was so careless in overseeing the accuracy of instruments and the reduction of measurements, that Luke Howard, the Quaker cloud-classifier, suggested that if 'this learned and highly respectable body feels the subject of the weather no longer worthy of notice, would it not be better, at once to dismiss the register from its transactions?'[14] Worse yet, the register was the only part of the *Philosophical Transactions* acknowledged by the Council of the Royal Society, all other meteorological contributions being officially disavowed by the body. John Frederick Daniell, arguably the leading meteorologist of the day, added in 1823 that the weather record was so infamously inaccurate that its publication on the Continent would amount to 'the almost national disgrace'.[15] To help improve the status of British meteorology, a group of metropolitan doctors led by George Birkbeck gathered the same year at the Ludgate Coffee House and founded the Meteorological Society of London. Daniell, Howard and the eccentric polymath Thomas Ignatius Maria Forster were among Council members, but their prestige and eagerness did not matter much as the Society had its last meeting as early as May 1824. Howard had moved to Yorkshire; Birkbeck had become busy with the London Mechanics' Institution.[16] When the Society was revived in 1836, it was taken over by astrometeorologists and continued its activities only until 1843.[17]

In fact the British Association claimed exclusive rights on meteorological research. William Harcourt and William Whewell, two visionaries of a provincially based meteorological research, had asked young James Forbes for the first official British Association report on meteorology. Forbes spoke for the entire scientific establishment

11 Borlase's remark is in a letter of 18 July 1763 to Henry Baker, quoted in J. Oliver, 'William Borlase's contribution to eighteenth-century meteorology and climatology', *Annals of Science* (1969), **25**, 291. On the 'declinist' arguments of Charles Babbage and David Brewster, see Morrell and Thackray, op. cit. (8), 47–52.

12 Quoted in Theodore S. Feldman, 'The History of Meteorology, 1750–1800: A Study in the Quantification of Experimental Physics', Ph.D. dissertation, University of California, 1983, 233.

13 Henry Ince, *The Wonders of the World in Nature and Art*, London, 1839, 233.

14 Quoted in John Frederick Daniell, *Meteorological Essays and Observations*, London, 1823, p. ix.

15 Daniell, op. cit. (14), p. x.

16 Walker, op. cit. (6), 365–6.

17 Even as a member of its Council (1838–41) Herschel never attended its meetings. When the Society was re-established in 1848 he thought it might assist in the research on atmospheric waves. Birt doubted this by pointing out the Society's prevailing astrometeorological interest.

when lobbying for 'a total revision' of the 'infant science of Meteorology [which resembled] patches of cultivation upon spots chosen without discrimination and treated on no common principle'.[18] Whewell wrote to Roderick Murchison that meteorology required 'multiple and extensive fagging'. Harcourt envisaged any Association member as becoming, eventually, a useful weather-reporter and Herschel agreed: revamping was a pressing problem especially as the world was not 'yet civilized enough for meteorology'.[19] More to the point, there was a sense that meteorology itself might become a civilizing force as the tenets of the early meteorological agenda, suiting the Association's constituency of provincial 'soldiers of science', managed to secure the ideals of social utility and collaboration. Meteorology appeared to them full of socio-political benignity.[20]

It is not surprising that in the context of the British Association's enlightened conservatism, meteorology's niche in the classification of science acquired more luster than in the recent past. Earlier in the century, meteorology was classified as a subspecies of either natural history, chemical philosophy, or physiology (meaning general inquiry into the natural world);[21] now it slipped into Section I of the British Association's map of knowledge – the section concerned with general physics. This elevated position was underscored by James Forbes's wish to see the scattered empirical work strengthened by a species of mathematical physics modelled on Fourier's work on propagation of heat. Roderick Murchison and Edward Sabine went so far as to say David Brewster's observations in Scotland were of the highest value to meteorology, 'the most interesting department of physics'.[22]

Yet even the most interesting departments are sometimes plagued by prosaic snags; in the case of the early Victorian science of weather the two most prominent were the fiscal and physical inability to process observations already in existence. Herschel continually relayed his doubts about the British Association's ability to pay observers, computers and

18 J. D. Forbes, 'Report upon recent progress and present state of meteorology', *Report of the British Association for the Advancement of Science* (hereafter *Report*), London, 1832, 7.

19 Herschel to Forbes, 15 November 1836, quoted in Morrell and Thackray, op. cit. (8), 519. In the same letter, Herschel wrote: 'I can see abundance of good things that such a society may do: one matter which requires multiplied and extensive fagging is meteorology, which I hope Dalton will set them to work upon.'

20 The vice-president of the Meteorological Society of London encapsulated such an ethics of inquiry writing that 'the moral advantages, too, which must necessarily arise from the increasing multitude of intelligent minds that hold constant and intimate intercourse on subjects of rational curiosity and philosophical interest, cannot be too highly appreciated. But if any department of science is pre-eminently distinguished in this respect, it is surely meteorology – the connector as it were, of the heavens and the earth, the regulator of life and health, the grand medium of operation for the mighty agents already mentioned, whether they are the unerring ministry or the immediate execution of their Creator's will.' Æneas M'Intyre, 'Introductory remarks', *Transactions of the Meteorological Society* (1839), **1**, p. xii. Morrell and Thackray, op. cit. (8), 450, also discuss the Association's ideological ambitions.

21 E. Chambers and A. Rees, *Cyclopaedia*, London, 1784; Henry Robertson, *A General View of the Natural History of the Atmosphere*, Edinburgh, 1808. For a more general treatment of the classification of science see Richard Yeo, 'Reading encyclopedias. Science and the organization of knowledge in British dictionaries of arts and sciences', *Isis* (1991), **82**, 24–49.

22 Forbes, op. cit. (18), 196, 197, 200–4. Roderick Impey Murchison and Edward Sabine, 'Presidential Address', *Report*, London, 1840, pp. xliv–xlv. In the period between 1834 and 1849, the grants the British Association allotted to meteorological research and the supply of meteorological instruments amounted to at least £1444 (or 8.75 per cent of the total expenditure, approximately £16,500).

printers.[23] Along with David Brewster, he warned that misapplied Baconianism would lead to scientific mediocrity: 'perfect spontaneous freedom of thought is the essence of scientific progress', and activity carried out along Harcourt's lines would only make science 'a democratic tyranny'.[24] Meteorological observations made in the decade after this pronouncement brought the point home: 'witness the piles of unreduced meteorological observations which load our shelves and archives', lamented Herschel, 'what we now want is thought, steadily directed to single objects, with a determination to eschew the besetting evil of our age – the temptation to squander and dilute upon a thousand different lines of inquiry'. So great was the concern at a runaway 'factology', that Herschel advised South African observers to look 'with some complacency to their discussion and [be] induced to theorize and combine'. Several years later, he recommended narrowing the field of inquiry, and concentrating on a distinct point in meteorology, such as tracing 'atmospheric waves'.[25]

THE STORM CONTROVERSY

Herschel's suggestion came at a time of great discussion taking place in America of the dynamic principles of mid-latitude and tropical storms. The 'storm controversy', besides being a scientific exchange of considerable weight, brought the importance of meteorological research to the public eye more than any of the utilitarian words in meteorological publications of the period.

In 1801 James Capper, a Cardiff-based amateur, retired from the East India Company, claimed that his experience with Caribbean storms convinced him that 'a water-spout [was] only a whirlwind on a small scale, and a hurricane a whirlwind of an immense scale'.[26] The 'rotatory theory of storms', as the idea was elaborated during the 1830s, held that the velocity of wind was a function of rotation of a vortex of fluid combined with its progressive motion. The theory was soon understood to be the property of William Redfield, a New York transportation businessman and rising star of American meteorology, who in a paper on North American and Atlantic storms (1831) argued with

23 Herschel to Wheatstone, 17 June 1842, cited in Morrell and Thackray, op. cit. (8), 522.

24 Herschel to Whewell, 20 September 1831, Morrell and Thackray, op. cit. (8), 67.

25 John Herschel, 'Address', in *Report*, London, 1846, 43; 'Second report of the South African Literary and Scientific Institution', *Proceedings of the South African Literary and Scientific Institution* (1836), 243; and 'Report on the reduction of meteorological observations', *Report*, London, 1843, 97. Elsewhere, Herschel desired 'combination and concert to predominate over extent and diffusion', see his 'Terrestrial magnetism', in *Essays from the Edinburgh and Quarterly Reviews*, London, 1857, 64. For changing attitudes to methodology see Richard Yeo, 'Scientific method and the rhetoric of science in Britain, 1830–1917', in *The Politics and Rhetorics of Scientific Method* (ed. J. A. Schuster and R. R. Yeo), Dordrecht, 1986, 271; for Herschel's view on the role of hypothesis in science see Richard Yeo, 'An idol of the market place: Baconianism in the nineteenth century', *History of Science* (1985), **23**, 267–72, and Larry Laudan, 'Thomas Reid and the Newtonian turn of British methodological thought', in *Science and Hypothesis: Historical Essays on Scientific Methodology* (ed. Larry Laudan), Boston, MA, 1981, 86–110. On problems associated with Baconian fact-collecting in America, see George H. Daniels, *American Science in the Age of Jackson*, New York, 1968, chs. 5 and 6.

26 James Capper, *Meteorological and Miscellaneous Tracts*, Cardiff, 1808, 127. My survey of the storm controversy is largely based on a definitive account by James Rodger Fleming, *Meteorology in America, 1800–1870*, Baltimore, 1990.

considerable empirical evidence that mid-latitude storms blow in circles counter-clockwise around the centre, which itself advances in the direction of the prevalent wind. In the same year, the havoc wrought by a hurricane on the island of Barbados induced William Reid, a lieutenant-colonel of the Royal Engineers responsible for rebuilding damaged government buildings, to spend his leisure time contemplating the kinematics of storms. By 1838, Reid realized that Redfield's rotatory theory coincided with his own, and instantly wrote to John Herschel in the hope that Redfield would soon 'have the great satisfaction of seeing [his] labours successful in proving the true nature of storms'.[27] In August 1838, Reid appeared at the British Association meeting in Newcastle and opened John Herschel's section with a paper expository of Redfield's 'Law of Storms'. The reception was positive and Redfield's and Reid's proposals began to play a prominent role among British weather scientists.

In North America, however, Redfield's position was far from self-evident. James Espy, Redfield's most outspoken opponent, was at the time director of the Joint Committee on Meteorology of the American Philosophical Society. Espy insisted on the thermal causes of storms: in his view, air was violently sucked towards the storm's centre, raised, and its vapour content condensed so that the released latent heat enhanced convection, which in turn accelerated centripetal movement of the air at the surface. Whereas Espy's ideas fell on fertile soil within the French Academy of Science, the theory was not popular in Britain. Alexander Bache did his best to promote it among the British in 1836, but his efforts had no effect; in fact, John Dalton's reaction was plain negative.[28] Nor was the subsequent visit to Britain of Joseph Henry more helpful; the meteorological doyen John Daniell, as Henry's diary shows, had already made his judgement in favour of the rotatory theory. Herschel inclined in the same direction: at the Newcastle meeting, the Reid/Redfield alliance owed its triumph in part to Herschel's own criticism of Espy's model. If the air was rushing toward the centre of storm, wondered Herschel, why should the centre exhibit the lowest atmospheric pressure?

Espy was particularly troubled by this objection. In 1839 he pleaded for Herschel's caution, 'for such is the weight of his name, that many will think it not worth while to examine a system which has been condemned by Sir John Herschel'.[29] Espy thought a personal appearance at the Glasgow meeting of the British Association in 1840 might help. He gave two talks, but each was received with considerable reservation.[30] Espy acknowledged the criticisms and respectfully dissented.

While a consensus favouring 'whirling' over centripetal kinematics was gaining strength, dynamic issues were tackled with more diffidence. Redfield repeatedly invoked evidential support and argued for *quo modo* inquiry rather than rash speculation on *verae causae*. The only known cause of tornadoes and larger storms lay, in his view, in the tendency of all fluids to run in circuits when subject to opposing forces: 'all aerial fluids and fluviate bodies abound with the incipient rudiments of nuclei of gyration, in various

27 Reid to Redfield, 1 February 1838, cited in Fleming, op. cit. (26), 37.

28 Fleming, op. cit. (26), 46.

29 James P. Espy, 'Examination of Col. Reid's work on the law of storms', *Journal of Franklin Institute* (1839), **23**, 38, quoted in Fleming, op. cit. (26), 39.

30 A summary of the reaction is given in *Athenaeum*, 10 October 1840, 795.

stages of development, and these rudimentary gyrations seem to be the necessary results of variable and unequal corpuscular motion'.[31] But to say that the 'gyration' could be a cause of 'gyratory' motion in storms was to argue in circles, some protested. Espy's thermal processes seemed a good alternative. Or was it perhaps electricity?[32]

SURGES OF OPTIMISM

Herschel's understanding at this juncture was that the true theoretical candidate was not yet in the game. It was at this time that he and Birt, his computer between 1839 and 1843, began discussing alternatives, as their correspondence reveals. Birt had previously published on the periodical variations of β-Lyrae and α-Cassiopeiae in the Astronomical Society's *Notices*. Living near Bethnal Green in London, in 1831 and 1832 he was engaged in systematic astronomical observation; he communicated to John Lubbock a dozen corrections to the celestial map prepared for the Society for the Diffusion of Useful Knowledge and even projected a survey of the Milky Way. Some time later he wrote on cloud-formation and summarized Wilhelm Dove's account of the aerial currents of the temperate zone. Herschel seemed to like Birt's thoroughness and after returning from the Cape employed him for reducing barometric observations. From this time on, Birt's primary scientific interests lay in meteorology.

In the early 1840s, Birt and Herschel worked jointly on the reduction, tabulation and graphical representation of barometric data; the search for diurnal and monthly regularities was their priority since Herschel hoped to lay down the empirical laws of the atmosphere and perhaps extend them into a comprehensive meteorological theory. But he also seemed particularly uneasy about the marginal role British gentlemen of science had in the storm controversy. The concern over a meagre 'expenditure of thought' explains Herschel's call at the Newcastle meeting (1838) for theoretical speculation of *any* kind; if nothing else, speculation would serve as a temporary ferment for a controversy that 'might produce brilliant results, by the very collision of intellect'. Herschel illustrated his plea with a remark indirectly addressed to Reid, the only Englishman partly involved in the storm debate: he asked if the spots on the sun were tornadoes of the solar atmosphere and would it be possible to imagine trade winds in the solar atmosphere?[33]

This was not what Herschel and Birt discussed in private, however. In 1843 Herschel came forward with a concept that would not only make sense out of what had already been tabulated but also have implications for the storm controversy itself:

> Collingwood, July 28, 1843
>
> Dear Sir,
>
> …
>
> I have already come to the conclusion that our conception of a barometric wave can be easily enlarged (i.e. if we set out with my notion of their analogy to sea-waves) so as to take in vast (?) flattened (blanket-like if I may be allowed such a homely comparison) heavings more like the tide waves of the ocean – one such wave of at least 1000 miles in breadth is very distinctly traced over

31 Fleming, op. cit. (26), 28–9.

32 Robert Hare, 'Objections to Mr. Redfield's theory of storms with some strictures upon his reasoning', *American Journal of Science* (1841), **42**, 140–7, and Fleming, op. cit. (26), 52, 34.

33 Fleming, op. cit. (26), 39.

> London in the September of 1836, at a rate of 21 miles per hour from NW to SE, and perfectly well made out … And if we consider (which is already well-known of the phenomena of the barometer) that we have often a rising glass for whole month and vice versa – and sometimes a general high or low level … for one or two months, *it will be clear that our atmosphere is a vehicle for wavelike movements which may embrace in their single swell & fall a whole quadrant of a globe*.
>
> …
>
> My view of the matter to which I have alluded is that every barometric movement is essentially and intimately connected with an appropriate and corresponding wind – as … every wavelike movement in a fluid consists of two distinct things – an advancing form and a molecular movement – … the *advancing form* is indicated by the barometer – the *molecular movement* by the wind, and between these two phenomena there exists of necessity a close and purely dynamical connection. (Knowing barometrical fluctuations one would be able to predict wind) for this could lead to prediction of great storms.[34]

Later that year he repeated that it would be 'no small meteorological discovery' if by the study of barometric fluctuations one could formulate a law that would predict them, 'as by this we might possibly be led to the prediction of great storms'.[35]

As for the 'the revolving gales' or 'revolving storms', Herschel trod cautiously: he was not 'fully prepared to speak' on the subject, 'but some of the principal of their phenomena would seem capable of explanation in this way of conceiving winds of oscillation [those produced locally and non-periodically], and in which they would become traced up, not to "funnel-shaped revolving depressions" [Reid–Redfield's hypothesis] in the nature of watersports, *but simply to the crossing of two large long waves moving in different directions*'.[36] See Figure 1.

On this proposal, the possibility of explaining and forecasting revolving storms hinged on an accurate identification of the time and place of the waves' intersection, an idea that was to remove all the theoretical entanglements developed in the Redfield–Espy dialogue. Empirically, Herschel thought the barometric record obtained at British magnetic and meteorological observatories already sufficed for a global wave dynamics. He himself would be unable to undertake this 'most interesting inquiry', but was authorized by 'Mr. Birt to state, that should it be the pleasure of the British Association to entrust the subject to his inquiries by appointing him a committee for that purpose, he is prepared to do it if provided with a moderate grant to clear unavoidable expenses'.[37]

Birt's first report for the British Association was published in 1844. He reduced a wide variety of observations to sea level and standard temperature, calculated their daily means, transformed values into curves, and compared the results.[38] Early curves confirmed the existence of a 'beautifully symmetrical wave' taking 13 days for a complete rise and fall.[39]

34 Herschel to Birt, 28 July 1843, RS:HS 19.106.

35 Herschel, *Report*, 1843, op. cit. (25), 99. The idea is based on the phenomenon of the wind-direction reversal during the summer thunderstorm, suggesting the existence of the atmospheric node acting as centre of the observed annual wind-direction revolutions.

36 Herschel, *Report*, 1843, op. cit. (25), 100, my italics.

37 Out of £100, Herschel had spent £55; the rest he desired to be partly allotted to lithographing Birt's wave curves. Herschel, *Report*, 1843, op. cit. (25), 98. The appointment was described in a letter from Herschel to Birt, op. cit. (34).

38 Birt began with sixteen British stations, eleven Continental, and four Colonial; the number rose to thirty-four in the next year, as indicated in Birt's letter to Herschel, 5 November 1845, RS:HS 4.112.

39 Herschel, *Report*, 1843, op. cit. (25), 98.

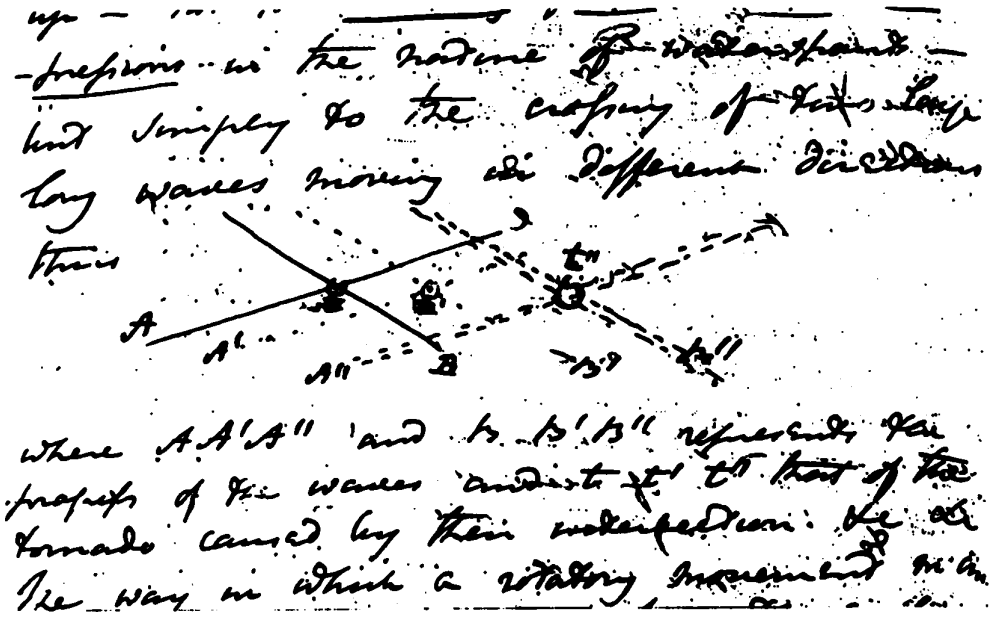

Figure 1. John Herschel's sketch of the crossing of two large atmospheric waves. The lines $AA'A''$ and $BB'B''$ represent the progress of waves; the tornado, represented at two consecutive points, t' and t'', is caused by the intersection of waves. Herschel to Birt, 28 July 1843, Royal Society:HS 19.106. Reproduced by permission of the Royal Society of London.

The evidence of its motion from Dublin to Munich promised 'a rich harvest of results'.[40] Herschel, in another paper delivered at the same meeting, was optimistic regarding Birt's method of 'combining barometrical observations', which he thought was so successful as to prove 'we are dealing with *realities* with forms of a higher order than those which have hitherto been subjected to meteorological observation', forms he believed analogous to the elliptic orbits astronomers infer from the data of successive planetary positions.[41]

The phenomenon under close examination was the 'Great November Wave', which swept Europe between 11 and 25 November 1842 (see Figure 2). When he first saw it outlined in Birt's graphs, Herschel claimed never to have seen anything 'more scientific'. The symmetrical curve was 'a simple result of direct observation. Such fact *must have a meaning*', prophesied Herschel. It was a well-defined specimen but, more importantly, it seemed to be a recurring phenomenon, as Birt was gratified to inform Herschel in November 1845: 'I have much pleasure in acquainting you that I have great reason to believe the return of another extensive undulation [in November 1843].'[42] He wrote of yet another recurrence, in November 1844, and in the Postcript to the Second Report written on 27 November 1845 he announced that his hope to observe the wave again that year was 'fully realized [as] the symmetrical wave has returned and has exhibited all its essential

40 W. R. Birt, 'Report on atmospheric waves', *Report*, London, 1844, 267.

41 Herschel to Birt, 1 September 1844, RS:HS 19.114.

42 Birt to Herschel, op. cit. (38). Also Birt to Herschel, 3 December 1845, RS:HS 4.113.

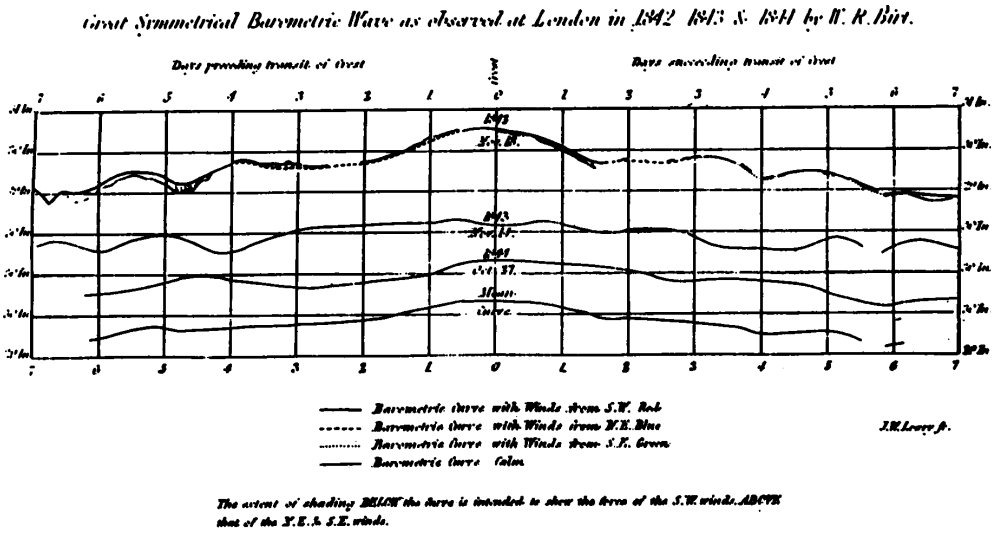

Figure 2. 'Great Symmetrical Barometric Wave at London in 1842, 1843 & 1844 by W. R. Birt'. From the *Philosophical Magazine* (1846), **29**, 357.

features'.[43] Herschel was 'delighted': he saw these 'periodical *surges*' of atmospheric pressure as closely associated with and hence predictive of the 'revolving storms'.[44]

Under the superintendence of Trinity House, men of science already made weather observations at what the Royal Society had recommended as appropriate meteorological hours: 3 a.m., 9 a.m., 3 p.m. and 9 p.m. In 1845 Birt decided that as the November barometric oscillations appeared to occur annually, observations should be undertaken on a larger scale during that period by interested individuals, keepers of the lighthouses and, on the recommendation of Captain Beaufort, by the officials of the surveying vessels, all 'with a view to observe this particular wave'. He announced that any amateur gentlemen who would apply to Birt would be furnished with copies of instructions and forms for recording the observations. These observations would include readings of the barometer, dry and wet bulb thermometer, the direction and force of the wind, and the weather at the set times.[45]

These plans came to naught. Birt soon realized that 'private enthusiasm and present body of observers' would not meet the requirements necessary for discovery of any global wave-system. Despite the generous financial support that the British Association provided, he came to believe that the time had come for a more active involvement on the part of the authorities. Writing to Herschel three years later, in June 1847, about the lack of data for the identification of south-westerly movements, he wrote:

> It has struck me that by the recommendation of the British Association Her Majesty's Government might be induced to direct that on a certain occasion hereafter to be determined upon observations should be made by the officers of H. M. Ships at certain of our foreign stations … The

43 W. R. Birt, 'Second report on atmospheric waves', *Report*, London, 1845, 129.

44 Herschel to Birt, 7 November 1845, RS:HS 19.120.

45 W. R. Birt, 'The great symmetrical wave', *Philosophical Magazine* (1845), **25**, 237–9.

> expense attending such a series of observations would be comparatively trifling as we may presume Her Majesty's Vessels are furnished with Barometers and the observations may be made and recorded four times each day by the officers of the watch.[46]

Birt sent essentially the same message to Lieut.-Col. Edward Sabine, who at that time was the general secretary of the British Association and foreign secretary of the Royal Society with important ties with the Admiralty.[47] In July 1847, Birt also addressed Dr John Lee, an eminent patron of astronomy and meteorology. In a long letter to Lee he described the chronic lack of data, which prevented identification of atmospheric waves and discovery of the nature of winds. In Birt's view, the beneficiaries of such a research would be the naval, merchant and military services, as well as members of the learned societies and directors of observatories. They all would have access to standardized observations regularly deposited in a central meteorological office and arranged under the superintendence of a keeper.

> To show that I am not overrating the advantages to be gained, we have only to contemplate the results which our Transatlantic Brethren are arriving at in reference to those vast gyrative movements known as revolving storms or hurricanes by means of the system of observations established at the Military Posts of the US as well as by the interests taken by the Nautical Men of that country.[48]

Both Herschel and Lee were enthusiastic about Birt's scheme. Herschel endorsed the idea of a central government office headed by a scientific meteorologist and employing 'a number of salaried computers', provided '1st, that the Nation can afford it, – 2nd, that provisions should be made for the reduction and discussion of the observations so collected, and the regular publication of the results'. Lee saw no difficulty in meeting the conditions: 'My reply to his [Herschel's] first provision – I will venture to say that the Nation can readily, and ought cheerfully to afford it.' He thought the funds might be supplied by curtailing one regiment of cavalry and one of infantry as well as by commissioning one frigate of the Navy fewer in the year: 'if one million of the annual expenditure of £20,831,077 now spent on the Army, Navy and Ordnance were spent on the Arts and Sciences the public would readily approve of the appropriation of the funds'.[49]

Birt cited Herschel's and Lee's favourable opinions in a letter to the Marquis of Northampton, the President of the Royal Society. He argued that the dearth of observations had previously impeded Reid's inquiry into the nature of 'Rotatory Gales' and was a major obstacle in the wave research too. However, the problem was now even more pressing as the 'atmospheric wave as a meteorological phenomenon is altogether of a higher order than the revolving hurricane and that the latter is an immediate consequence of the crossing of two large waves, the tornado raging in the point of intersection'.[50]

During this period, Birt also preached for the wave cause from the pulpit of the

46 Birt to Herschel, 17 June 1847, RS:HS 4.116.

47 Birt to Sabine, 22 June 1847, Royal Society of London, Archives, Sabine Papers 160 (hereafter RS.Sa).

48 Birt to Lee, 24 July 1847, RS.Sa.161.

49 Herschel and Lee to Birt, August 1847; quoted in Birt's letter to the Marquis of Northampton, 23 October 1847, RS.Sa.162.

50 Birt to Marquis of Northampton, 23 October 1857, RS.Sa.162; Birt to Lee, 24 July 1847, RS.Sa.161. Birt informed the Marquis of his intention to contact the Duke of Wellington, the Commander in Chief of Her Majesty's Forces, and the Earl of Auckland, the First Lord of the Admiralty. No such letters are extant in Birt's correspondence at the Royal Society.

Philosophical Magazine. He directed the attention of potential observers to the importance of observations coming from 'any locality' and he also gave a brief account of what the symmetrical wave meant at a presentation at the British Association meeting at Cambridge.[51] It is not known whether at this time anyone followed Birt's instructions and what the return of the forms might have been. It is known, however, that in December 1847 Birt learned that George Eden, Lord Auckland, had asked Herschel to edit an instruction manual for officers on surveying and exploring expeditions. Herschel acted immediately and asked Charles Wheatstone for meteorological instructions of a general kind and Birt if he were willing to write on atmospheric waves. Of course, the two should stay in touch to avoid duplication.[52] Birt replied that he was honoured by Lord Auckland's request and would do everything in his power to comply.[53]

Birt's appeals to individuals in government offices and scientific bodies were evidently bringing wave research into the public eye; the empirical basis of the project seemed ensured and the results would appear only as a matter of time. Problems had not yet arisen: Birt's structural analysis of the Great November Wave of 1842 based on anenometric and barometric records gave strong probability to Herschel's idea that the molecular movement (a component of any wavelike movement) must be dynamically connected with the wind. Indeed, the molecular movement was asserted to be strongest in the wave troughs and directed towards them from each side, as Edward Sabine had already suggested in the case of data from Toronto. This would explain the observed reversal of wind direction at the stations below the trough or the crest of the moving wave.[54] If such were the case, the wind patterns, perhaps the greatest point of contention in the storm debate, could in principle be associated with readings of atmospheric pressure and thus connected with the transit of waves.

Birt was assertive on this issue in a review he wrote in 1846 of Redfield's account of the paths of East-American storms.[55] Birt described how storms

> may be produced by the crossing of two large atmospheric waves moving in different directions. Data collected during the American storm of February 1842 strongly implied two waves passing over the country with the storm raging in their intersection. Should the entire barometric observations taken over the United States on that occasion support the theory of atmospheric waves, I apprehend Sir John Herschel's suggestions will be partly realized.[56]

51 According to Birt, during fourteen days of November the oscillations of the barometer exhibited a symmetrical character, which he believed resulted from a transit of a large wave; but, as the curve had five distinct elevations, the same result would have been obtained if the '*five* secondary superposed waves' were simultaneously passing over a given location. W. R. Birt, 'On the great symmetrical barometric wave of November', *Philosophical Magazine* (1846), **29**, 357.

52 Herschel to Birt, 23 December 1847, RS:HS 19.127.

53 Birt to Herschel, 27 December 1847, RS:HS 4.119.

54 Birt, op. cit. (43), 123.

55 W. R. Birt, 'On the storm-paths of the eastern portion of the North American continent', *Philosophical Magazine* (1846), **28**, 380.

56 Birt, op. cit. (55), 381. Birt always credited Herschel for the wave theory, and always spoke of it with reservation: 'if Sir John Herschel's suggestion is correct that the circular storm results from the crossing of two large atmospheric waves', the meteorologists should more fully investigate the theory of storms, as a 'knowledge of the laws of atmospheric waves must greatly contribute to our knowledge of the circular storm'. W. R. Birt, 'On the great symmetrical barometric wave, November 1847, and other important undulations', *Philosophical Magazine* (1848), **32**, 49.

Similar faith in the wave theory was displayed in Birt's contribution to *A Treatise on Atmospheric Phenomena*, a volume compiled in 1846 by Edward Lowe, a halo-*aficionado* living near Nottingham. To be sure, Birt said, Redfield's account was path-breaking in the discussion on barometric oscillations associated with storms, but there was more to the phenomena of the weather than one fashionable if unavoidable topic. The more extensive undulations – waves – 'were left unexplained either by Mr. Redfield or Colonel Reid' and it was presumably up to their less lionized colleague to decide whether there was a similar arrangement of winds associated with wave-passage to the one identified in gyrating storms.[57] Birt's enthusiasm turned tentative proposals into certainties when he pronounced that in the face of evidence 'a rotation of a greater or less extent and force *must* result from their [waves'] intersection; there appears to be a close relation between the rectangularly posited currents and the rotatory gales'.[58]

These pronouncements mark the apex of the optimism Birt and Herschel nurtured in respect to wave research and its potential in the study of revolving storms. It should be pointed out that from the beginning both had presented the research as a primarily theoretical enterprise. In a general sense, it was developed in the context of and in response to the status-of-meteorology rhetorics: such a large-scale empirical enterprise was thought tantamount to discovering laws of atmosphere. More particularly, it promised to facilitate the solution of the storm controversy through an alternative conjecture. It is in this context that we should understand Birt's governmental lobbying: it was concerned mainly with organization of data collection with relatively little to promise in the way of immediate public benefits, such as weather forecasting. In mid-nineteenth-century Britain, this lack of public benefit could easily prove detrimental to research that threatened to consume large funds and required considerable administrative engagement. But, as the next section endeavours to show, problems emerged within the theory itself as well, and eventually eroded the legitimacy of the project.

MOUNTING OBSTACLES

The idea of atmospheric waves was so simple that it could develop and be used almost colloquially. After a violent storm in 1763, for example, William Borlase, perhaps the most conscientious weather observer during the eighteenth century, commented: 'By this violent and sudden alteration in the Mercury it appears that the atmosphere at these times must have been greatly agitated and proceeded over the earth in vast waves; low, deep, and hollow, when the Mercury fell: and mountainous afterwards, which occasioned as sudden rise.'[59] Herschel's 1843 proposal did not diverge much from this commonsensical image of the heaving atmosphere: the wave was a representation of the barometric variation in amplitude as a function of time, featuring such *ad hoc* terminological inventions in description of barometric curves (that is, waves) as 'sudden undulatory movements', 'abnormal protuberances', 'trepidations', or 'bulges'. Herschel suggested that the analogy

57 W. R. Birt, 'On barometric undulations', in Edward Lowe, *A Treatise on Atmospheric Phenomena*, Nottingham, 1846, 356.

58 Lowe, op. cit. (57), 360.

59 William Borlase, *Natural History of Cornwall*, Oxford, 1758, 19.

ought to be enlarged 'till it approaches in some degree, in the extent of its sweep, and the majestic regularity of its progress, to those of the tide waves in the ocean'.[60]

With Birt's second British Association report (1845), problems appeared. Some barometric readings could be accounted for only as result of the *superposition* of two or more waves moving in different directions and having different characteristics of the slopes. Some anomalies could be accommodated in this way, but more waves meant more questions about their origins, extent, velocity and so on.

Birt's strategy was to diminish a theoretical rift between Redfield's rotatory hypothesis and Dove's two-current theory. The latter presupposed storms as a result of the interaction of two contrary currents, which Birt had already shown to be associated with the wave phenomena; Dove's theory was to link phenomena of gyrating storms with the 'wave-crossing' hypothesis. He thus associated anemonal data with the two-current picture and argued that the difference between rotatory gales and currents was in the 'first being a cylindrical body of air revolving around a central axis, while in the [latter] currents, or moving masses, are disposed longitudinally, and not circularly'.[61] Both hypotheses would, however, predict similar phenomena: as the trough of atmospheric pressure approached an observer the velocity of the wind should increase, and after passage of the trough the wind should rapidly shift in direction. Thus in his third report for the British Association, Birt commented on the work of William Brown, who in 1846 explained barometric oscillations as a result of 'the meeting of opposite or nearly opposite aerial currents'.[62] Birt took some of Brown's data and decided to 'regard the progress of barometric and anemonal phaenomena as the progress of waves'.[63]

At this juncture, however, Birt seems to have realized that the concept of atmospheric wave might in the end fail to refer to reality.

> The term atmospheric wave has been used to designate *ideal individuality* which the mind attributes to the process which it observes of the successive change of place which the barometric maxima and minima undergo…We readily see that the wave is a convenient method of representing the barometric fluctuations (and the barometric curve) *does not represent the form of any reality in nature*.[64]

Birt's struggle to disentangle the causes of minute irregularities on the wave-curve converted him into an awkward instrumentalist: the 'ideal individuality' was certainly a far cry from Herschel's Borlaseian naïvety. In addition, Herschel's idea of explaining storms as the result of wave-crossing was automatically reduced to the level of a mere model. On the empirical side, Birt found that the Great November Wave was to a certain

60 John Herschel, *Report*, 1843, op. cit. (25), 97. Herschel strongly encouraged analogical thinking in science, see his *Preliminary Discourse on the Study of Natural Philosophy*, reprint edn, Chicago, 1987, 149ff.

61 Lowe, op. cit. (57), 359.

62 William Brown, 'On the oscillations of the barometer, with particular reference to the meteorological phenomena of November 1842', *Philosophical Magazine* (1846), **28**, 241–87.

63 W. R. Birt, 'Third report on atmospheric waves', *Report*, London, 1847, 133.

64 Birt, op. cit. (63), 134. Birt, however, subsequently laboured to maintain the ontology of waves: 'The establishment of the parallel currents will give the air motion; the diminution of pressure towards the lines of greatest velocity will give the wave form, and the drawing off of air from the S.E. will induce the wave motion', ibid., 130. The third report was adapted for publication in Lowe's treatise where Birt wrote that 'The *individuality* which the mind attributes to this collection of distinct and separate phenomena, by which they are combined into a single *whole*…has been termed an *atmospheric wave*.' Lowe, op. cit. (57), 360.

extent erratic as to the period of its transit; the crest typically passed in mid-November, but in some years it occurred in October, in others in late November.[65] As the initial and terminal minima of the observed readings were discovered to differ in no predictable manner, Birt warned that 'great difficulty must exist in the observation of the wave. If, for instance, any particular curve is expected to return, the observer in most cases will be disappointed',[66] especially if situated away from the locality of greatest symmetry. This qualification called into question the usefulness of proposed 'wave-manuals'. But it made them obsolete when Birt came to claim that even though a similar set of aerial currents recurred at the same time of the year, the Great November Wave 'has no *real* existence: there is not a distribution of pressure over Europe at all similar to it'.[67] The initial identification of the Great November Wave was due to a short series of observations; now, it was apparent that the distribution of atmospheric pressure was the result of two sets of parallel currents.

Of course, there was a great deal of tinkering in the way of securing the original idea. Recurrence of the waves indicated a seasonal disposition of the atmosphere and could perhaps be used to probe into the features of Dove's currents. But nothing of the kind was executed. There was also a lingering hope of achieving a breakthrough in the storm controversy. In Herschel's *Manual of Scientific Inquiry* (1849) Birt proposed that the observations should be taken as 'capable of throwing light, not only on the most important desiderata as connected with storms, but also their connection or non-connection with atmospheric waves'.[68] He used Redfield's observations on the occasional occurrences of the unusually high atmospheric pressure prior to a storm to point out that the event had to be associated with the wave-crest 'rolling forward'. But as for the question of why the crest would precede a storm, or what could cause the wave itself, Birt could not give any specific answer.

The time had come for major disclaimers. We are fortunate to have two versions of Birt's final perception of the shortcomings of the wave concept. The first appeared in the essay for Lowe's *Treatise on Atmospheric Phenomena*. In Lowe's work, Birt urged the reader to apprehend 'the precise idea attached to the term *wave*'. While the 'wave motion' and the 'wave form' might be conveniently represented by waves, it was

> not so when we treat of the molecular motions or wind: this is so entirely opposed to all our notions of molecular motions of waves, that we are called upon to hesitate before we designate the two…a wave, merely because they exhibit, when contemplated as a whole, some wave phenomena…In retaining the term wave until one more suitable is found, it is desirable that the idea of the molecular motion should be restricted to the longitudinal direction parallel to the direction of the crests and troughs.'[69]

The second comes in a letter to Herschel, where Birt wrote:

65 W. R. Birt, 'On the remarkable barometric depression of the 25th December 1821', *Philosophical Magazine* (1847), **30**, 21.

66 W. R. Birt, 'On the great symmetrical barometric wave, November 1847, and other important undulations', *Philosophical Magazine* (1847), **32**, 40.

67 Lowe, op. cit. (57), 361.

68 John Herschel (ed.), *Manual of Scientific Enquiry; Prepared for the Use of Her Majesty's Navy: and Adapted for the Travelers in General*, London, 1849, 335.

69 Lowe, op. cit. (57), 361.

> the principal objection to this definition of an atmospheric wave consists I apprehend in its non-agreement with hydrodynamical principles. So far as the term *wave* has been employed *by myself* in immediate reference to these particular phenomena I most readily admit it was *ill-chosen* and fails to convey a clear notion of such phenomena to the mind.[70]

In both of these statements Birt realized that the notion of 'atmospheric wave' could not account for the observations of the direction and force of the wind ('molecular movement'). Herschel and Birt had at first regarded 'atmospheric waves' as dynamic entities explicable in terms of the wave-mechanics. This, however, proved not to be the case: the 'waves' were not dynamic in nature because, by definition, they referred merely to a wavelike form of the curve representing changes in barometric pressure. Birt's recognition that the 'atmospheric wave' was a purely descriptive concept made him also realize its utter inadequacy in solving the problems addressed in the storm debate.[71]

Besides empirical and conceptual difficulties, in the late 1840s Birt also faced both professional and personal problems. In this period he searched for a post at various places (at the Observatory at the Cape of Good Hope, in Madras and at the Kew Observatory). In the end he began investigations into atmospheric electricity at Kew: in 1848 he was requested by the British Association to undertake the reduction and discussion of the electrical observations made at Kew, and the results formed a report of nearly ninety pages in length, which was published in 1849. After some alarms about discontinuing maintenance of the Kew observations, the British Association voted for their continuance, and Birt moved into premises in the observatory on 2 November 1849. His duties were initially limited to the discussion of electrical measurements, but at Francis Ronalds's request he also commenced daily observations. He soon found himself 'become a mere machine doing work at such epochs which recording instruments are specially designed to accomplish'.[72] Early morning observations and the personal dislike that soon developed between him and Ronalds led to a nervous breakdown in 1850. On 5 June, Birt jotted down a brief note to Sabine saying he was

> unwilling to undertake the duties of the Observatory another twelve months under present arrangements, these arrangements are not satisfactory to me nor are they such as I expected them to have been made.[73]

Birt learned with consternation that the note was interpreted by the Magnetical and Meteorological Committee as his official resignation from the 'electrical' post at Kew. He immediately wrote an explanatory letter to the Visiting Committee. But after failing to obtain an interview with Sabine and the British Association's terminating resolution he decided to pull out. This falling out between him and the British Association was further

70 Birt to Herschel, 1 April 1848, RS:HS 19.207.

71 A review in the *Philosophical Magazine* delivered the final blow to the wave concept: 'The fourth section', wrote the reviewer (likely to have been Herschel himself), 'speaks of periods of extra observations, and particularly of that of the great atmospheric wave of November. Our experience does not confirm the existence of this particular wave.' *Philosophical Magazine* (1850), **36**, 395. Birt's letters suggest he understood this failure as primarily empirical in nature. In 1847 he wrote that he 'failed in contemplating any single wave in its totality' owing to the lack of observations (Birt to Lee, RS.Sa.162). In 1849 he was in need of observations from Yorkshire to identify the Great November Wave's commencement (Birt to Phillips, RS.Sa.169).

72 Birt to the Visiting Committee of the Kew Observatory, 11 July 1850, RS.Sa.177.

73 Birt to Sabine, 5 June 1850, RS.Sa.179.

exacerbated by the Association claiming to have rights over his most recent report on luminous meteors, which he submitted *after* the involuntary termination. In his most dispirited moment, Birt wrote to Herschel in June 1850 to inform him of his complete retirement from any scientific work whatever and to thank him for ten years of assistance and kindness.[74]

Notwithstanding circumstances, Birt wished to continue investigation on atmospheric waves. In 1849 he sought advice from Herschel and John Phillips about submitting a proposal to the Royal Society for a government grant,[75] which entered the Royal Society Council Minutes as a request 'for the allotment of a sum of 50 pounds to enable him [Birt] to prosecute his inquiries into the nature and progress of atmospheric waves'. Whether because of an obvious bias in favour of Fellows of the Royal Society or because the proposal was thought 'too theoretical or too philosophical to be ''good science''', on 25 June 1850 the Committee voted against funding Birt's proposal.[76] In November 1851 he repeated the application and again was refused.[77]

Birt's last meteorological work was the *Handbook of the Law of Storms* (1856), a digest of storm researches intended for use on board ships 'without the trouble of consulting logs, or arranging isolated records of the wind, weather, etc.'[78] The book may be regarded as a testimonial to utilitarian concerns increasingly informing meteorological investigation. Any inclusion of theory seemed to have been in need of apology:

> Impressed with the idea that 'abstract science', under all circumstances, bears remotely, if not immediately, on the well-being of society, we have not shrunk from placing before the public our views of the storm paths in all localities, even at the risk of giving to our otherwise practical volume the appearance of a scientific volume, and incurring the oft-repeated cry, when immediate benefit does not appear, of *Cui bono*?[79]

In the section of this volume devoted to atmospheric waves Birt accordingly eschewed the 'wave-intersection' hypothesis and discussed waves as 'bearing somewhat on storms' to the extent that their troughs were now defined as rectilinear atmospheric streams of hurricane strength. The ability to predict these would be of considerable importance to seamen, but 'at present so little is known of the nature of atmospheric waves on the open ocean, that it would be premature to offer any further remarks on them'.[80]

74 Birt to the Visiting Committee, 11 July 1850, RS.Sa.177; Birt to Herschel, 14 June 1850, TxU:H/M-0103; Birt to Herschel, 19 June 1850, TxU:H/M-0104. Letters from the collection of the Harry Ransom Humanities Research Center, The University of Texas at Austin.

75 Birt to Herschel, 21 December 1849, RS:HS 4.135. On the origins of the government grant see R. M. MacLeod, 'The Royal Society and the government grant: Notes on the administration of scientific research, 1849–1914', *Historical Journal* (1971), **14**, 323–58.

76 MacLeod, op. cit. (75), 328; *Minutes of Council of the Royal Society*, 1846–58, London, 1858, 147. One should take Sabine's secretaryship as an additional factor in the decision, especially in the light of the fact that he was one of the successful candidates for a grant: Sabine received £100 for the purchase of new instruments for the Kew Observatory.

77 Birt to President and Council of the Royal Society, 5 November 1851, Royal Society of London, Miscellaneous Correspondence, 5:23.

78 William Radcliff Birt, *Handbook of the Law of Storms; being a Digest of the Principal Facts of Revolving Storms for the Use of Commanders in Her Majesty's Navy and Mercantile Marine*, London, 1856, p. iv.

79 Birt, op. cit. (78), p. v.

80 Birt, op. cit. (78), 120.

Birt's final scientific contribution was in selenography, to which he turned after the hapless accident with the British Association's managerial machine. Despite that episode, in the period between 1865 and 1869, the Association published five of his reports on mapping the surface of the moon. His other writings dealt with lunar activity and the identification and cataloguing of smaller features on the moon's surface. Three years before his death, in 1878, he was elected the first president of the Selenographical Society.[81]

CONCLUSION

The meteorology of atmospheric waves received relatively little attention among nineteenth-century scientists. Birt's definitions were generally ignored. In 1855, for example, Urban Leverrier, the Director of the Imperial Observatory in Paris, reported the movements of the atmospheric wave associated with the great storm of 1854 that fell upon the French fleet stationed in the Black Sea. E. Liais, Leverrier's assistant, concluded that the direction and speed of the wave had no relationship to the direction and speed of its accompanying winds – a conclusion that Birt had already shown resulted from the incorrect use of the term 'atmospheric wave'. Leverrier hoped A. L. Cauchy would undertake a mathematical analysis of the wave movements but there is no evidence it was ever done.[82] Neither was Birt's research widely recognized. Henry Piddington, a storm theorist known for the invention of the term 'cyclone', noted in 1860 that 'Sir J. Herschel proposed the idea, that two or more extensive atmospheric undulations, or barometric waves, from traversing in different directions, intersect each other, and from their opposing forces cause the phenomena of hurricanes or rotatory storms.'[83] In this century, Isaac Cline, the Principal Meteorologist of the US Weather Bureau, wrote as if Birt's 'wave period' never took place; referring to 'The Hurricane Guide', Cline wrote that 'W. Radcliffe Birt, as early as 1852 showed an unusually complete grasp of the movement of winds in cyclones.'[84] At that time, as this analysis shows, Birt's research into these questions was practically over.

If it was Birt who in his late contributions to the British Association reports had already cast a shadow on the validity of the wave research, it was Robert FitzRoy who in 1863 produced the most scathing criticism of it. FitzRoy claimed that 'what are commonly called "atmospheric waves" are delusive; and that, although there are waves in any line indicating oscillations of the barometer, there are not such movements in the atmosphere itself, as are usually adverted to by expressions "trough" and "crest"'.[85]

81 'William Radcliff Birt' [obituary], *Monthly Notices of the Royal Astronomical Society* (1882), **42**, 142–4. See also Eric G. Forbes, 'Birt, William Radcliffe', *DSB*.

82 Davis, op. cit. (6), 364–5.

83 Henry Piddington, *The Sailor's Horn Book for the Laws of Storms*, London, 1860, 6.

84 Isaac Monroe Cline, *A Century of Progress in the Study of Cyclones*, 3rd edn, New York, 1942, 7.

85 Robert FitzRoy, *The Weather Book. A Manual of Practical Meteorology*, London, 1863, 80. FitzRoy there claimed that with any rising of masses of air the lighter or 'tropical' portions of winds should rise the highest; 'but according to the "Wave Theory" (here controverted) the reverse is asserted; the lowest part of the apparent troughs of the wave occurs with the lowest barometer, that is, with the air, which is the lightest and most expanded and ought to rise up the highest'. 'Such "waves" are more correctly designated "barometric curves"', ibid.

The organization of meteorological research in Britain during the 1850s ultimately emerged as a more pressing issue than Birt's theoretical investigations. In 1850 the British Meteorological Society was founded and was instrumental in mobilizing a number of voluntary weather observers. By 1851 William Reid influenced John Fox Burgoyne, the inspector-general of fortifications, to authorize the setting up of additional meteorological stations in the colonies and in Britain; Burgoyne went forward with an idea of co-operation between Britain and the United States. Disagreements about observational standards led to an international conference on meteorology in Brussels in 1853, which was a great success. In Britain, in August 1854, Robert FitzRoy was nominated president of the newly formed Meteorological Department of the Board of Trade. But as Jim Burton has observed, practical, not theoretical issues took priority in these proceedings: the government addressed the safety of sea transport and profitability of trading, which depended on reliable wind and sea-current maps, and the prospect of military advantages. 'Last was the interest of men of science who favoured the study of meteorology for its own sake.'[86] In this situation, Herschel's initial appeal for the introduction of some theory proved pointless as new meteorological 'commodities' began to attract the attention of governmental authorities and scientists alike. Herschel's project to *explain* atmospheric disturbances appeared increasingly misplaced and even irrelevant.

In addition, as the wave episode suggests, the scientific patriotism that in the once-ebullient market of the storm debate initiated Herschel's hopes of success and opened pathways for Birt to explore, in the end failed primarily because of its daunting empirical cargo and associated conceptual problems, and only secondarily through the tension existing between Birt and the British Association. In an ironic turn of events, the theory that Herschel's reputation and the British Association's financial solvency helped push to the forefront of English meteorology, lost in the face of the very phenomena it was contrived to account. FitzRoy made a shrewd retrospective judgement that 'great authorities then countenanced the theory and apparently sanctioned such opinions. Yet there is so much argument against these views, that even the highest names may scarcely warrant their implicit adoption.'[87] Nature, however loosely one may want to associate it with the business of observation, appeared to have had the upper hand in a contention with ideological powers that be.

86 Burton, op. cit. (5), 150.
87 Fitzroy, op. cit. (85), 95.