

## John Tyndall and the Early History of Diamagnetism

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### Summary

John Tyndall, Irish-born natural philosopher, completed his PhD at the University of Marburg in 1850 while starting his first substantial period of research into the phenomenon of diamagnetism. This paper provides a detailed analysis and evaluation of his contribution to the understanding of magnetism and of the impact of this work on establishing his own career and reputation; it was instrumental in his election as a Fellow of the Royal Society in 1852 and as Professor of Natural Philosophy at the Royal Institution in 1853. Tyndall's interactions and relationships with Michael Faraday, William Thomson, Julius Plücker and others are explored, alongside his contributions to experimental practice and to emerging theory. Tyndall's approach, challenging Faraday's developing field theory with a model of diamagnetic polarity and the effect of magnetic forces acting in couples, was based on his belief in the importance of underlying molecular structure, an idea which suffused his later work, for example in relation to the study of glaciers and to the interaction of substances with radiant heat.

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## 1. Introduction

The period of 10 years following Michael Faraday's discovery of diamagnetism in late 1845 was a critical one for the development of the understanding of magnetism and for Faraday's emerging field theory. John Tyndall played a significant role in this period which has not been properly recognised. It is the aim of this paper to identify Tyndall's contribution as an experimentalist and theoretician, to demonstrate how his own ideas of matter and force developed, and to indicate the impact that this work had on his own later researches, his reputation and his career. It can fairly be said that diamagnetism both launched Tyndall's scientific career and gave him the platform for his entry into elite Victorian Society, yet the significance of his work on diamagnetism has been underplayed in the literature. In the most recent extensive biographical study, Ursula DeYoung gives barely a mention to diamagnetism, dividing his career into three major categories of research - glaciology, molecular behaviour in varying atmospheric conditions, and bacteriology.<sup>1</sup> Diamagnetism is in reality the first of four major areas of his research, and the significant one for establishing his career and early reputation, scientifically and socially.<sup>2</sup>

The phenomenon of diamagnetism is complex and very weak compared to paramagnetism or ferromagnetism, requiring powerful magnets and sensitive, and expensive, apparatus. There was much uncertainty and disagreement in this period both about the experimental 'facts' and about the conceptual frameworks brought forward to explain them, with many savants in Britain and on the Continent actively involved. Understanding these developments, and Tyndall's specific contributions, requires a detailed analysis of the practical and theoretical developments and the interactions of the key participants, which is given first. The final part of the paper assesses the significance of Tyndall's contribution to our understanding of magnetism, polarity, matter and force.

## 2. The initial exploration of diamagnetism

### 2.1 Faraday's discovery of diamagnetism

'The effects to be described require magnetic apparatus of great power, and under perfect command'. So stated Faraday in his paper read before the Royal Society on 20 November 1845, announcing the discovery he had made on 4 November of a new but very weak magnetic property of matter.<sup>3</sup> Faraday had demonstrated in September with his

<sup>1</sup> U. DeYoung, *A Vision of Modern Science; John Tyndall and the Role of the Scientists in Victorian Culture* (New York: Palgrave Macmillan, 2011), 28

<sup>2</sup> An outline of Tyndall's work on diamagnetism is given in A. S. Eve, and C. H. Creasey, *Life and Work of John Tyndall*, 290–7 (London: Macmillan, 1945) and in W. H. Brock, N. D. McMillan and R. C. Mollan, *John Tyndall; Essays on a Natural Philosopher*, 82–6 (Dublin: Royal Dublin Society, 1981).

<sup>3</sup> M. Faraday, 'On new magnetic actions, and on the magnetic condition of all matter', *Philosophical Transactions of the Royal Society of London* (1846), 136, 21–62 (§2245; this and subsequent such references refer to Faraday's paragraph numbering).

'heavy glass' that a magnetic force could cause the rotation of polarised light travelling through the glass. It was important to him to show that magnetism was a universal property of matter,<sup>4</sup> and he now examined the effect of magnetic force directly on the glass and then on many other materials, by suspending the chosen material between or close to the poles of a powerful magnet. This resulted always in a repulsion from the poles or from a single pole<sup>5</sup> 'remarkably like a case of weak electrostatic repulsion',<sup>6</sup> so that a bar of the material placed between the poles would set at right angles to the line joining the poles, whereas a normal magnetic substance, which we now call paramagnetic, would be attracted and set in line with the poles. Faraday termed these the 'equatorial' and 'axial' positions respectively, and called the substances which behaved like this 'diamagnetics'.<sup>7</sup> He understood this effect in terms of lines of force, with the bar moving from the stronger to the weaker part of the magnetic field, or across the lines of force, following what he termed 'diamagnetic curves' in contrast to the magnetic curves followed by paramagnetic substances.<sup>8</sup> Faraday also stated that 'we have magnetic repulsion without polarity, i.e. without reference to a particular pole of the magnet, for either pole will repel the substance, and both poles will repel it at once'.<sup>9</sup> Extending the work in detail to solids, liquids and gases, Faraday concluded that 'substances appear to arrange themselves into two great divisions; the magnetic, and that which I have called the diamagnetic classes',<sup>10</sup> though these substances affect the rotation of polarised light in the same manner.<sup>11</sup> William Thomson soon gave mathematical rigour to this discovery, showing in May 1847 that the equations governing the behaviour of (para)magnetic and diamagnetic substances under the influence of a magnet are the same but of opposite sign, illustrating Faraday's conclusion that a diamagnetic substance tends to move from stronger to weaker places or points of force.<sup>12</sup> Both Faraday, conceptually, and Thomson, more mathematically, demonstrated clearly the effect in three dimensions of the strength of the magnetic force at any particular place, when the force (or field) is not uniform in space. Incidentally, in this paper Thomson also predicted the possibility of stable magnetic levitation of diamagnetic substances, wonderfully exemplified in the 20<sup>th</sup> century by Geim and Berry with a levitated frog.<sup>13</sup>

<sup>4</sup> See F. A. J. L. James, *Michael Faraday: A Very Short Introduction* (Oxford: OUP, 2010), 76–82. Faraday's 'heavy glass', a lead borosilicate, had a much greater ability to rotate plane polarised light than ordinary glass, making the effect readily demonstrable.

<sup>5</sup> Faraday subsequently learnt that the repulsion by a single magnetic pole had been observed previously but not taken very seriously by the Dutchman S. J. Brugmans (1763–1819), the German T. J. Seebeck (1770–1831) and the Frenchman Alexandre Claude Martin le Bailif (1764–1831), a physician; see the letter from A. de la Rive to Faraday, 25/12/1845 (Letter 1809 in F. A. J. L. James *The Correspondence of Michael Faraday, Volume 3, 1841–1848* (London, 1996) and a footnote dated 2 February 1846 in M. Faraday, *Experimental Researches in Electricity* (London, 1855), vol. III, 82. Le Bailif seems to have been the first to note the (relatively) great strength of the diamagnetism of bismuth.

<sup>6</sup> M. Faraday (note 3), 25 (§2268).

<sup>7</sup> M. Faraday, 'On the magnetisation of light and the illumination of magnetic lines of force', *Philosophical Transactions of the Royal Society of London* (1846), 136, 1–20 (§2149).

<sup>8</sup> M. Faraday (note 3), 26 (§2270).

<sup>9</sup> M. Faraday (note 3), 26 (§2274).

<sup>10</sup> M. Faraday (note 3), 53 (§2420).

<sup>11</sup> M. Faraday (note 3), 55 (§2427).

<sup>12</sup> W. Thomson, 'On the forces experienced by small spheres under magnetic influence; and on some of the phenomena presented by diamagnetic substances', *Cambridge and Dublin Mathematical Journal* (May 1847). See also *Reprint of papers on Electrostatics and Magnetism* (London, 1884), 2nd ed., 499–505.

<sup>13</sup> M. V. Berry and A. K. Geim, 'Of flying frogs and levitrons', *European Journal of Physics* (1997), 18, 307–13.

## 2.2 Defining diamagnetism

What we now call ‘paramagnetism’ was originally called ‘magnetism’ and its opposite was termed ‘diamagnetism’. Faraday’s first suggestion for the property was the word ‘dimagnetic’, based on the electric word ‘dielectric’, but the current form ‘diamagnetic’ was suggested to Faraday by William Whewell in a letter of 10 December 1845,<sup>14</sup> as was the term ‘paramagnetic’ and ‘paramagnetism’ (but not ‘diamagnetism’). Faraday adopted the term diamagnetic from 1846<sup>15</sup> and paramagnetic from 1851<sup>16</sup> leaving thereafter the word ‘magnetic’ for the phenomenon in general. The OED at the time of research (June 2013) gave the first use of the term ‘diamagnetism’ in 1850,<sup>17</sup> but this is a footnote in a reference to Faraday, and the first use of the word in print by Faraday appears to be in a letter published in *Philosophical Magazine* dated 8 November 1847,<sup>18</sup> then in a letter to Whewell on 13 December 1847,<sup>19</sup> although it seems that he then did not use the term in print again until 1854,<sup>20</sup> preferring to refer to ‘diamagnetics’. The first written use of the word by Faraday is in his experimental notebook for 5 November 1847.<sup>21</sup> However, Julius Plücker used the term diamagnetism (in German: Diamagnetismus) earlier, in his first two papers published in *Poggendorff’s Annalen* in October 1847.<sup>22</sup> He sent these papers to Faraday with a letter dated 3 November in French, using the word ‘diamagnétisme’.<sup>23</sup> In his Bakerian Lecture of 1855, Tyndall stated that Faraday gave the name of diamagnetism to the effect of repulsion by a single pole.<sup>24</sup>

## 2.3 Practical and theoretical challenges of diamagnetism

The subsequent study of diamagnetism was bedevilled by both practical challenges and theoretical differences. From a practical perspective, diamagnetism is an extremely weak and complex property of matter, easily overpowered by contamination with minute amounts of paramagnetic materials and dependent on the nature of the magnetic field in relation to the size and shape of substances. The questions of theory at the root of disagreements concerned whether diamagnetism is or is not ‘polar’, and whether it can best be explained in terms of action at a distance between magnetic poles or in terms of a magnetic field that fills all space. Tyndall’s contributions to each were striking, and the theoretical position he took, in opposition to Faraday, underlies all his subsequent thinking about the constitution of matter and its relationship to force. He challenged Faraday’s interpretation from the outset of his researches. Faraday developed his thinking in terms of a field filling space, with physically real lines of force having certain

<sup>14</sup> Whewell to Faraday, 10 December 1845 (Letter 1798 in F. A. J. L. James (note 5)).

<sup>15</sup> M. Faraday (note 3), 2 (§2149).

<sup>16</sup> M. Faraday, ‘On the magnetic and diamagnetic condition of bodies’, *Philosophical Transactions of the Royal Society* (1851), 141, 7–28 (§2790).

<sup>17</sup> W. Gregory, *Letter to a Candid Admirer, on Animal Magnetism* (Philadelphia: Blanchard and Lea, 1850).

<sup>18</sup> M. Faraday, ‘On the diamagnetic conditions of flame and gases’, *Philosophical Magazine* (1847), 401–21.

<sup>19</sup> Faraday to Whewell, 13 December 1847 (Letter 2034 in F. A. J. L. James (note 5)).

<sup>20</sup> M. Faraday, ‘On Magnetic Hypotheses’, *Proceedings of the Royal Institution of Great Britain* (1854), 1, 457–9. See also M. Faraday, ‘On some points of magnetic philosophy’, *Philosophical Magazine* (1855), 9, 81–113 (§3309).

<sup>21</sup> M. Faraday, *Faraday’s Diary* (1934), vol. 5, paragraph 9196.

<sup>22</sup> J. Plücker, ‘Über die Abstossung der optischen Axen der Krystalle durch die Pole der Magnete’, *Annalen der Physik und Chemie* (1847), 72, 315–43 and J. Plücker, ‘Über das Verhältnis zwischen Magnetismus und Diamagnetismus’, *Annalen der Physik und Chemie* (1847), 72, 343–52.

<sup>23</sup> Plücker to Faraday, 3 November 1847 (Letter 2024 in F. A. J. L. James (note 5)).

<sup>24</sup> J. Tyndall, ‘On the Nature of the Force by Which Bodies Are Repelled from the Poles of a Magnet; to Which is Prefixed, an Account of Some Experiments on Molecular Influences’, *Philosophical Transactions of the Royal Society of London* (1855), 145, 1–51.

properties, but without clearly specifying the underlying mechanisms. Tyndall once described this as Faraday's 'mistiness',<sup>25</sup> since his own focus was firmly on clear physical explanations.

#### 2.4 Early experiments of Faraday, Plücker and Weber

The German-speaking physicists had ready access to Faraday's work through its translation in *Poggendorff's Annalen*, and it stimulated considerable experimental and theoretical interest. In Bonn, Julius Plücker took up the study of diamagnetism around June 1847. Plücker was a geometer turned physicist, who eventually published some 59 papers on physics, the magnetic properties of gases and crystals, and electric discharge in evacuated gases.<sup>26</sup> His experiments, initially with vegetable materials, led him to suppose that the alignment of fibres might influence the magnetic behaviour of matter and that the structure of crystals might produce a similar effect. In his work on crystals, published in *Poggendorff's Annalen*,<sup>27</sup> he found that the optic axes of crystals are repelled by the poles of a magnet, that the force is independent of the magnetic or diamagnetic condition of the crystal, and that it diminishes less, as the distance from the poles increases, than the magnetic or diamagnetic forces. In other words, he suggested that there is a new repulsive force at work. The question of polarity remained elusive, Plücker commenting 'I have made many but unsuccessful experiments to discover a diamagnetic polarity'... 'The simplest hypothesis... that in which diamagnetism is regarded as a general repulsive force of nature'.

He then described, in the next article in the same issue of *Poggendorff's Annalen*,<sup>28</sup> the apparently anomalous results for cherry bark, which set equatorially if placed close between the poles but axially if the poles are wider apart or if placed above or below the line between the poles, noting that De la Rive had made similar observations with charcoal. He explained this in terms of the magnetic force diminishing less than the diamagnetic in proportion to the increase of distance from the poles. Plücker wrote to Faraday on 3 November sending copies of both papers and summarising his findings.<sup>29</sup> Faraday replied on 11 November regretting his inability to read German and sending him a piece of heavy glass for experiments.<sup>30</sup> Plücker wrote again on 6 February claiming to have shown air to be diamagnetic,<sup>31</sup> although there is no recorded reply.

In January 1848, Wilhelm Weber published his related work in *Poggendorff's Annalen*. Weber was a key figure in both the experimental and theoretical understanding of diamagnetism, extending Ampère's theory to cover diamagnetism, arguing that it is

<sup>25</sup> Tyndall to Hirst, 5 November 1855, RI MS JT/1/T/935.

<sup>26</sup> Julius Plücker (1801–1868). It is arguable that Plücker's accomplishments were appreciated more by English savants than by his compatriots (Dictionary of Scientific Biography, hereafter abbreviated DSB). His relationship with Tyndall was acrimonious until they mended fences in 1858 at an encounter brokered by August Hofmann (Tyndall, Journal, 10 April 1858). Plücker was elected a foreign member of the Royal Society in June 1855 (Tyndall did not sign the nomination certificate) and was awarded the Copley Medal in 1866. For more on Plücker's work see C. Jungnickel and R. McCormach, *Intellectual Mastery of Nature, Theoretical Physics from Ohm to Einstein Vol. 1, The Torch of Mathematics 1800–1870* (Chicago: University of Chicago University Press, 1986), 234–8.

<sup>27</sup> J. Plücker (note 22).

<sup>28</sup> J. Plücker (note 22).

<sup>29</sup> Plücker to Faraday, 3 November 1847 (note 23).

<sup>30</sup> Faraday to Plücker, 11 November 1847 (Letter 2025 in F. A. J. L. James (note 5)).

<sup>31</sup> Plücker to Faraday, 6 February 1848 (Letter 2051 in F. A. J. L. James (note 5)). Faraday had in fact shown this in 1847 (see note 36).

caused when resistanceless molecular currents are induced in diamagnetic substances. His lasting impression on physical theory was his atomistic conception of electric charge and its role in determining the electrical, magnetic and thermal properties of matter.<sup>32</sup> In this paper,<sup>33</sup> Weber raised the question of action at a distance, saying ‘were we to admit that the diamagnetic force has its origin in the unvarying metallic particles of the bismuth itself,...it would be the first case in which the action of a ponderable upon an imponderable body [meaning magnetic fluids] at a distance had been observed’. Weber in this paper was explaining the effect of opposite magnetic poles on the same side of a piece of bismuth, which is subtractive not additive,<sup>34</sup> as due to distribution of the ‘imponderable constituents’ i.e. north and south magnetic fluids, and that on Ampère’s theory currents induced in diamagnetics are in the contrary direction (whereas in magnetics they would be in the same direction), as Faraday had pointed out.<sup>35</sup> So, ‘if the two magnetic fluids, or their equivalents, Ampère’s currents, are really present in the diamagnetic bodies, which are set in motion or rotated under the influence of a powerful magnet, they must induce an electric current in a neighbouring conductor at the moment this change takes place’. Weber designed experiments to observe these induced currents and to show that those induced in bismuth are opposite to those in iron. He explained that the molecular currents exist in iron independently of any external excitation, whereas those in bismuth are entirely induced.

In March 1848 Plücker published his paper exploring diamagnetic polarity in *Poggendorff’s Annalen*.<sup>36</sup> ‘In bismuth every north pole of a magnet induces a north pole, each south pole a south pole. Diamagnetic polarity is a consequence of this explanation. I then tried in vain to detect this polarity’. In this paper he claimed he had, using single poles. In addition he concluded that ‘...the augmentation of the force of the poles of the magnet converts the magnetism of wood-charcoal into diamagnetism’. At the end he claimed to confirm ‘the theory of diamagnetism adopted by Faraday, Reich, Weber and Poggendorff’,<sup>37</sup> in which I now entirely coincide’. (Poggendorff had concluded that a bar of bismuth in an equatorial position was a real transversal magnet, which turns its north pole to the north pole). Thomson was sceptical about this supposed conversion of

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<sup>32</sup> Wilhelm Weber (1804–1891) is best known for his *Elektrodynamische Maassbestimmungen*, seven long works published between 1848 and 1878. He was elected a foreign member of the Royal Society in 1850 and was awarded the Copley Medal in 1859. See also C. Jungnickel and R. McCormack (note 26), 143.

<sup>33</sup> W. Weber, ‘Über die Erregung und Wirkung des Diamagnetismus nach den Gesetzen der inducierten Ströme’, *Annalen der Physik und Chemie* (1848), 73, 242–56.

<sup>34</sup> Reich had shown this repulsion (F. Reich, ‘On the repulsive action of the pole of a magnet upon non-magnetic bodies’, *Philosophical Magazine* (1849), 34, 127–30) and is referenced in the translation of Weber’s article in Taylor’s *Scientific Memoirs* (W. Weber, ‘On the excitation and action of diamagnetism according to the laws of induced currents’, *Taylor’s Scientific Memoirs* (1859), vol. 5, 477–88). Poggendorff had also described two experiments demonstrating diamagnetic polarity (J. C. Poggendorff, ‘Ueber die diamagnetische Polarität’, *Annalen der Physik und Chemie* (1848), 73, 475–9).

<sup>35</sup> M. Faraday (note 3), 56 (§2430).

<sup>36</sup> J. Plücker, ‘Über ein einfaches Mittel, den Diamagnetismus schwindiger Körper zu verstärken. Diamagnetische Polarität’, *Annalen der Physik und Chemie* (1848), 73, 613–8. Plücker, along with Zantedeschi, Bancalari and Faraday also explored the diamagnetism of flames and gases; see G. Boato and N. Moro, ‘Bancalari’s role in Faraday’s discovery of diamagnetism and the successive progress in the understanding of magnetic properties of matter’, *Annals of Science* (1994), 51, 391–412.

<sup>37</sup> Johann Poggendorff (1796–1877) was a physicist at the University of Berlin who edited *Annalen der Physik und Chemie* for more than half a century. He was an excellent experimenter, concentrating on electrical phenomena (DSB 1981).

magnetism into diamagnetism as he described in his paper to the British Association in Edinburgh ‘On the Theory of Magnetic Induction’.<sup>38</sup>

Plücker wrote to Faraday on 5 June 1848, sending his paper on diamagnetic polarity which, he stated, ‘cannot now be doubted’.<sup>39</sup> Faraday, without Plücker’s permission, had this letter published in *Philosophical Magazine*,<sup>40</sup> which surprised but pleased Plücker. In this letter, Plücker reiterated his conclusion that the intensity of the diamagnetic force increases more rapidly than the magnetic when the force of the electromagnet is increased, using bodies of mixed magnetic and diamagnetic material, and gave some initial results of the effect of chemical composition and temperature.

Plücker visited Faraday on 7 and 25 August 1848, before and after the meeting of the British Association in Swansea, which both attended. In Swansea, Plücker presented his finding that the optic axis, or its resultant if there are two axes, sets equatorially, pointing out also that this could be used in non-transparent crystals to find the optic axis. Though the formal report is brief, *Athenaeum* published a summary of the discussion, in which Faraday illustrated Plücker’s experiment with pieces of potatoes for the poles and another for the crystal with a quill stuck through it to represent the axis.<sup>41</sup> After the meeting Stokes wrote to Thomson, who had not been present, describing Plücker’s presentation and evincing his surprise at an experiment on mercury which Plücker maintained showed that the diamagnetic force decreases faster than the magnetic as the distance increases.<sup>42</sup>

Plücker wrote on 28 September to thank Faraday,<sup>43</sup> still firmly sticking by his position on the different laws of intensity for magnetism and diamagnetism. Faraday replied on 14 December, describing his identification of the magnecrystallic axis as a line in a crystal tending to place itself in the magnetic axis, analogous to Plücker’s effect of the optic axis, and sending Plücker his two papers, including the Bakerian Lecture, on the crystalline polarity of bismuth.<sup>44</sup> In a letter of 15 December 1848 to Schoenbein he explained the effect of the magnecrystallic force as ‘not one of *attraction* or of *repulsion* but of *position only*, and is as far as I can see a new effect or an exertion of force new to us’.<sup>45</sup> He had become firm in this view by the end of October 1848 and described it in a letter to Whewell on 7 November, with a description of critical experimental results outlining his identification of the magnecrystallic axis and the induced ‘Magneto crystallic’ force.<sup>46</sup>

Faraday gave the Bakerian Lecture on 7 December 1848. He showed that the crystallisation of bismuth affects the position it takes up in a magnetic field, and using poles which give a uniform magnetic field he demonstrated that crystals align themselves axially in the lines of force in a ‘magnecrystallic’ manner, which appeared to present a

<sup>38</sup> W. Thomson, ‘On the theory of magnetic induction in crystalline and non-crystalline substances’, *British Association Report, Notes and Abstracts of Miscellaneous Communications to the Sections* (London, 1850), 23. See also the report in *Athenaeum*, 17 August (1850), 877.

<sup>39</sup> Plücker to Faraday, 5 June 1848 (Letter 2086 in F. A. J. L. James (note 5)).

<sup>40</sup> J. Plücker, ‘On Diamagnetism’, *Philosophical Magazine* (1848), 33, 48–9.

<sup>41</sup> J. Plücker, ‘On some new relations of the diamagnetic force’, *British Association Report* (London: Murray, 1848) Part 2, 2; *Athenaeum*, 17 August 1850, 877.

<sup>42</sup> Stokes to Thomson, 21 August 1848 (Letter 29, *The Correspondence between Sir George Gabriel Stokes and Sir William Thomson* (Cambridge: Cambridge University Press, 1990)).

<sup>43</sup> Plücker to Faraday, 28 September 1848 (Letter 2108 in F. A. J. L. James (note 5)).

<sup>44</sup> M. Faraday, ‘On the crystalline polarity of bismuth and other bodies, and on its relation to the magnetic form of force’, *Philosophical Transactions of the Royal Society of London* (1849), 139, 1–41.

<sup>45</sup> Faraday to Schoenbein, 15 December 1848 (Letter 2138 in F. A. J. L. James (note 5)).

<sup>46</sup> Faraday to Whewell, 7 November 1848 (Letter 2118 in F. A. J. L. James (note 5)).

new form of force in the molecules of the matter, the ‘magnecrystallic force’, different from Plücker’s action of the optic axis force. The crystal can set either way axially, so the words ‘axial’ and ‘axiality’ were preferable to Faraday than ‘polar’ and ‘polarity’. The line of magnecrystallic force is perpendicular to one particular line of cleavage, and ‘the line or axis of magnecrystallic force tends to place itself parallel, or as at a tangent, to the magnetic curve or line of magnetic force, passing through the place where the crystal is situated’.<sup>47</sup> In all this work, Faraday continued to explain diamagnetics as moving from the stronger to the weaker part of the field, and the magnecrystallic force as tending to line up with the magnetic field (or resultant of magnetic force), rather than attraction or repulsion. He demonstrated the same with antimony and with arsenic. He did try to see if a magnetic field affected the crystallisation of bismuth, as might have been expected, but could not show it.<sup>48</sup>

In the continuation paper Faraday explored various other metals and compounds. Most did not show magnecrystallic action but a few did, as did sulphates of iron and nickel. His theoretical understanding was that ‘the magnecrystallic force is a force acting at a distance’<sup>49</sup> ... ‘...still it is due to that power of the particles which makes them cohere in regular order...which we call...attraction of aggregation, and ... as acting at insensible distances’.<sup>50</sup> He asked if the magnecrystallic force is inherent in the crystal or induced<sup>51</sup> and showed by various experiments that it seems to be induced, so should probably be called ‘magnetocrystallic’;<sup>52</sup> while the magnecrystallic (axis) belongs to the crystal itself. But he was puzzled, stating ‘I do not remember heretofore such a case of force as the present one, where a body is brought into position only, without attraction and repulsion’.<sup>53</sup> He was further puzzled by Plücker’s results,<sup>54</sup> in that a determining line of force should not as its full effect have the result of going into a plane indifferently as to direction (i.e. equatorially), and suggested that his effects and Plücker’s had a common cause. Finally he restated his view that ordinary magnetic action is polar and magnecrystallic is only axial in character;<sup>55</sup> if a piece of magnetic iron is placed in the magnetic field it immediately becomes polar, with the ends of different qualities, but this is not so with the magnecrystallic force.

Plücker continued to work with crystals, writing to Faraday on 20 May 1849<sup>56</sup> to convey his findings that the optic axis was repelled or attracted depending on whether the crystal was negative or positive respectively, a letter which Faraday again had published in *Philosophical Magazine*.<sup>57</sup> Faraday also proposed him as a member of the Royal Institution in June, for which he was most appreciative.<sup>58</sup> So, by the time Tyndall started

<sup>47</sup> M. Faraday (note 44), 7 (§2479).

<sup>48</sup> M. Faraday (note 44), 12 (§2503).

<sup>49</sup> M. Faraday (note 44), 25 (§2564).

<sup>50</sup> M. Faraday (note 44), 26 (§2568).

<sup>51</sup> M. Faraday (note 44), 28 (§2576).

<sup>52</sup> M. Faraday (note 44), 30 (§2585).

<sup>53</sup> M. Faraday (note 44), 31 (§2589).

<sup>54</sup> M. Faraday (note 44), 33 (§2600).

<sup>55</sup> M. Faraday (note 44), 38 (§2624).

<sup>56</sup> Plücker to Faraday, 20 May 1849 (Letter 2183 in F. A. J. L. James, *The Correspondence of Michael Faraday, Volume 4 1849–1855* (London: Institution of Electrical Engineers, 1999). As Jungnickel and McCormach discuss, the German physicists considered that magnetism acted on the molecules of the transparent body and not directly on light as Faraday thought, hence Plücker’s belief that crystal forms could be determined by magnets. See C. Jungnickel and R. McCormach (note 26), 126, fn48.

<sup>57</sup> J. Plücker, ‘On the magnetic relations of the positive and negative optic axes of crystals’, *Philosophical Magazine* (1849), 34, 450–2.

<sup>58</sup> Plücker to Faraday, 10 August 1849 (Letter 2214 in F. A. J. L. James (note 56)).



his work, Faraday had established the existence of diamagnetism as a weak property demonstrable for all substances which are not paramagnetic – we now know that it is a universal property, as Faraday had inferred, but that could not be determined with certainty at the time given the relative weakness of diamagnetism. Faraday explained diamagnetism in terms of his lines of force, described mathematically by Thomson, who had also challenged Faraday's theoretical understanding by predicting from his model that diamagnetics should set axially and that findings otherwise were an artefact of the size of the sensor and shape of the magnetic poles.<sup>59</sup> In crystals, Faraday had proposed a new 'magnecrystalline' force. But he was still working through the concepts which eventually became his coherent field theory.<sup>60</sup> Plücker, exploring the effect of structure on the manifestation of the property in fibrous and crystalline solids, had apparently demonstrated the importance of the optic axis in crystals and sought to link this to the underlying structure.

### 3. Tyndall's first phase of work

#### 3.1 *Tyndall and Knoblauch*

On 28 November 1849, and before he had completed his PhD thesis at the University of Marburg, John Tyndall recorded that he had begun his work on diamagnetism in collaboration with Hermann Knoblauch,<sup>61</sup> a similar age to Tyndall, and one of a strong group of German savants including Helmholtz, Du Bois-Reymond, Clausius and Siemens who worked at one time or another in Magnus's laboratory in Berlin. Diamagnetism, this weak and complex physical phenomenon was to be the primary focus of Tyndall's experimental work for several years. It enabled him to develop and demonstrate the painstaking precision of measurement and systematic examination of variables which would later bring him such success in the exploration of radiant heat and putrefaction, very much in tune with, or influenced by, the German approach to accurately 'measure and number' the phenomena. It also rapidly revealed him as a physicist to be reckoned with, prepared from the outset to challenge the established figures such as Faraday and Thomson and the lesser, though extensively engaged, figure of Plücker. Within a few years, in June 1852, Tyndall was a Fellow of the Royal Society, the citation emphasising his work on diamagnetism. Then on 11 February 1853 Tyndall gave his first Discourse at the Royal Institution 'On the influence of material aggregation upon the manifestations of force'; a presentation to a general audience of this difficult topic of diamagnetism. It was a great success, Tyndall showing that he could ally his scientific expertise with an ability to engage and enlighten a broad audience through skills honed as a teacher at Queenwood College. A few months later he was appointed Professor of Natural Philosophy at the Royal Institution and started to form the significant connections into Society which led, in

<sup>59</sup> D. Gooding, 'A convergence of opinion on the divergence of lines: Faraday and Thomson's discussion of diamagnetism', *Notes and Records of the Royal Society of London* (1982), 36, 243–59.

<sup>60</sup> D. Gooding, 'Final steps of field theory: Faraday's study of magnetic phenomena, 1845–1850', *Historical Studies in the Physical Sciences* (1981), 11, 231–75.

<sup>61</sup> Although according to his first paper they had started 'early in the month of November' (J. Tyndall, *Journal*, 28 November 1848, RI MS JT/2/13; hereafter Tyndall Journal entries are referenced 'Tyndall, Journal, date'). Herman Knoblauch (1820–1895) moved to the University of Halle in 1853 where he remained for the rest of his career, and kept in touch with Tyndall over many years.

due course, to the marriage into the aristocracy of this son of a relatively poor Irish shoemaker.

Herman Knoblauch had completed his doctorate in 1847, studying with Magnus<sup>62</sup> in Berlin. Although his major interest was the study of radiant heat, like others in Germany he had become intrigued by Faraday's discovery and had some apparatus made in Berlin to 'repeat and follow out the investigations of Faraday'.<sup>63</sup> However, Knoblauch had little time and the job fell largely to Tyndall, who had arrived in Marburg a year earlier, on 25 October 1848, to start his doctorate in Robert Bunsen's laboratory,<sup>64</sup> under the supervision of the mathematician Friedrich Stegmann. Tyndall wrote a dedication in the back of a surviving laboratory notebook on diamagnetism to Robert Bunsen, in whose laboratory he was working, and 'from whose lips I first heard of diamagnetism'.<sup>65</sup> Tyndall was juggling priorities, since he wrote on 30 November, two days after starting the diamagnetism experiments, that he had finally 'hewed the last difficulty of my dissertation to pieces'.<sup>66</sup>

In the fortnight to 18 December he worked every day on magnetism experiments, considering that Plücker might be right with respect to the optic axis but that 'he never took the time to establish his law'.<sup>67</sup> To test it systematically, Tyndall cut crystals into discs and cubes, parallel and perpendicular to optic axes, finding now that in some cases the optic axis set axially, falsifying Plücker's finding and conclusions. In the last few days of 1849 – he was even at work on Christmas Day and missed going out with his friends Debus and Bromeis on New Year's Eve – it struck him that the cleavage rather than the optic axis of the crystal might be significant, an idea that would become the basis of his beliefs about the importance of molecular structure, and would become particularly significant in his later work on glaciers. By 22 January he could report, after working 'morning, noon and night, Sunday and holiday'<sup>68</sup> that his joint paper with Knoblauch was on the way to Giessen, and from there that it would be taken the next day by his good friend Edward Frankland<sup>69</sup> to England. This paper, Tyndall's first, was published in *Philosophical Magazine* in March.<sup>70</sup> It demonstrated immediately Tyndall's ability to control variables, as he realised that 'no safe inference could be drawn from experiments made with full crystals' and described the use of cubes, discs and thin bars, cut in various ways in relation to the optic axis of the crystal concerned, and then powdered crystals

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<sup>62</sup> Heinrich Magnus (1802–1870), chemist and physicist, moved to Berlin in 1828 after studying with Berzelius in Stockholm. For the significance of his private laboratory see C. Jungnickel and R. McCormach (note 26), 109–12 and 257–9.

<sup>63</sup> Tyndall, *Journal*, 28 November 1849.

<sup>64</sup> Robert Bunsen (1811–1899), chemist, concentrated on inorganic chemistry and analytical techniques. His students included Kolbe, Frankland, Mendeleev and Lothar Meyer. With Playfair, he developed means of efficiently recycling gases in furnaces and he later collaborated with Roscoe 1852–62 on photochemical research, and with Kirchhoff in the 1860s to develop the field of spectroscopy. See F. A. J. L. James, 'The establishment of spectro-chemical analysis as a practical method of qualitative analysis, 1854–1861', *Ambix* (1983), 30, 30–53. He was elected a foreign member of Royal Society in 1858 and awarded the Copley Medal in 1860. Bunsen and Kirchhoff received the first Davy Medal in 1877 (DSB 1981).

<sup>65</sup> RI/MS/JT/3/45.

<sup>66</sup> Tyndall, *Journal*, 30 November 1849.

<sup>67</sup> Tyndall, *Journal*, 18 December 1849.

<sup>68</sup> Tyndall, *Journal*, 22 January 1850.

<sup>69</sup> Edward Frankland (1825–1899) was a chemist and early friend of Tyndall. He discovered organometallic chemistry, publishing an important paper on the subject in May 1852, and made major contributions to the development of valence theory and the chemical bond. He was elected FRS in 1853.

<sup>70</sup> J. Tyndall and H. Knoblauch, 'On the deportment of crystalline bodies between the poles of a magnet', *Philosophical Magazine* (1850), 36, 178–83.

reconstituted into thin bars. Tyndall also realised that contamination with minute amounts of paramagnetic material might be affecting the results, and indeed the Iceland Spar crystals which stood axially, contrary to Plücker, were found to contain traces of iron while those that stood equatorially did not. Tyndall concluded that it was the chemical composition, rather than the optic axis or whether the crystal was positive or negative (as Plücker had concluded) which was the key factor. Then, with gutta percha, he identified the importance of the direction of the fibre and the overall shape of the piece of material, as well as whether it was magnetic or diamagnetic in determining whether it stood axially or equatorially. So Tyndall ruled out the optic axis as the prime agent in determining the response to the magnet and referred in this paper to the ‘magnetic or diamagnetic force’ and ‘the manner in which either force is modified by the peculiar structure of the crystal’, implying that there were two forces at work.

While Tyndall and Knoblauch were at work in Marburg, Plücker, in a letter of 4 December 1849 to Faraday claimed new proofs of diamagnetic polarity and that attraction by the poles is only dependent on the exterior form of the crystal.<sup>71</sup> Faraday in reply, on 11 December 1849,<sup>72</sup> stated that he believed that the subjection of any crystal to the magnetic force depends on its internal structure, or rather the forces which give it its particular structure, and that the line which coincides with the magnetic axis may be called the magnecrystallic axis, which may not coincide either with the crystallographic or optic axis. His letters generally remark on his inability to read German and hence to access the detail of Plücker’s work in this field with its bewildering complexity of results. One senses he is waiting for someone to come and clear up the facts; which Tyndall indeed was to do.

Plücker wrote on 4 January 1850 confirming again, contrary to Faraday, his view of the polarity of diamagnetism and suggesting, contrary to Weber, that the polarity might be permanent.<sup>73</sup> Faraday replied on 8 January 1850 that he retained his view on polarity, though did not consider it proved either way.<sup>74</sup>

After a break of a year from publishing on this subject, Faraday’s paper ‘On the polar or other condition of diamagnetic bodies’ was read on 7 and 14 March 1850 and published in *Philosophical Transactions*.<sup>75</sup> It is unlikely that Faraday was aware of Tyndall’s work at this point. The paper was received on 1 January 1850, before publication of Tyndall’s first paper, also in March, and the manuscript shows no signs of significant editing. The earliest known letter between them is dated 19 July 1850. Faraday’s paper was stimulated in particular by Weber’s assertion that diamagnetics are polar in a magnetic field. Faraday stated that a true polarity must be permanent not induced or temporary, and opposite to ordinary magnetic polarity.<sup>76</sup> He set up apparatus very similar to Weber’s but ‘it gives me contrary results’.<sup>77</sup> Indeed he concluded that the effects were due to the conducting power of the substances for electricity and to induced currents, not to any polarity of their particles.<sup>78</sup>

<sup>71</sup> Plücker to Faraday, 4 December 1849 (Letter 2237 in F. A. J. L. James (note 56)).

<sup>72</sup> Faraday to Plücker 11 December 1849 (Letter 2239 in F. A. J. L. James (note 56)).

<sup>73</sup> Plücker to Faraday 4 January 1850 (Letter 2249 in F. A. J. L. James (note 56)).

<sup>74</sup> Faraday to Plücker 8 January 1850 (Letter 2250 in F. A. J. L. James (note 56)).

<sup>75</sup> M. Faraday, ‘On the polar or other condition of diamagnetic bodies’, *Philosophical Transactions of the Royal Society of London* (1850), 140, 171–88. The original manuscript is RS RR/PT/37/6.

<sup>76</sup> M. Faraday (note 75), 171 (§2642).

<sup>77</sup> M. Faraday (note 75), 173 (§2646).

<sup>78</sup> M. Faraday (note 75), 175 (§2656).

### 3.2 Tyndall's 'First Memoir' and the British Association Meeting in Edinburgh, 1850

On 1 June Tyndall posted his 'memoir' to his friend Thomas Hirst<sup>79</sup> for publication.<sup>80</sup> This was the first major paper, later referred to as the 'First Memoir',<sup>81</sup> taking up 33 pages in *Philosophical Magazine* in July,<sup>82</sup> and again published with Knoblauch as the joint author – every other paper in his lifetime was attributed to Tyndall alone, apart from the first paper on glaciers with Thomas Huxley. Having demolished, in their original paper, Plücker's statement that that optic axis alone determined the orientation of the crystal in the magnetic field, Tyndall and Knoblauch proceeded in this paper to show that Plücker's new law of the behaviour of optically positive and negative crystals was invalid too. They did this both by demonstrating mistakes in his classification and by using a wider range of crystals; by chance it appeared that Plücker had chosen only crystals which confirmed his theory, and had thereby been led to an incorrect conclusion.

They turned next to Faraday's experiments, and to his positing of the magne-crystallic force (inherent in the crystals) and the magneto-crystallic force (induced by the magnetic field) which, with Plücker's optic axis force, added up to three new forces. Tyndall had no issue with Faraday's experimental results but found difficulty in obtaining a clear notion of a force 'capable of producing such motions in the magnetic field, and yet neither attractive nor repulsive' (indeed Faraday had made a similar comment, resolved ultimately through his field theory). Instead, Tyndall showed that with the right geometry a repulsion could cause the 'approach' (or apparent attraction) of a bismuth crystal and an attraction the 'recession' (or apparent repulsion) of iron sulphate (eisenvitriol) which Faraday had found. He appears to have established this on 30 March when he noted in his journal that he had 'solved the paradox of eisenvitriol completely'.<sup>83</sup> He then suggested that the effect might be due to the closer contact of particles in one direction of the crystal than another and that the force would be exhibited most strongly in the former case, demonstrating this possible explanation by powdering crystals of bismuth and iron carbonate, reconstituting them in a vice, and showing that they behaved as expected with the line of closest contact axial or equatorial depending on whether the material was magnetic or diamagnetic. So there was a directive force, but not as suggested by Plücker or Faraday, and Tyndall termed it the 'line of elective polarity'. This effect was shown in reconstituted powdered substances as well as in crystals, which implied no need to identify a new 'magne-crystallic' force. The question then became one of whether there is 'any discoverable circumstance connected with crystalline structure...upon which the difference of proximity depends; and, knowing which, we can pronounce with tolerable certainty, as to the position which the crystal will take up in the magnetic field'. The cleavage plane or planes of the crystal offered one possibility, and Tyndall showed that the cleavage planes stand equatorial with diamagnetic specimens and axial with magnetic. At this point Tyndall made explicit his model of structure, with plates of material alternating with unfilled spaces ('expansion and contraction by heat and cold compel us to assume that the particles of matter do not in general touch each other') through which the magnetic force

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<sup>79</sup> Thomas Hirst (1830–1892) was a mathematician and friend of Tyndall since their days surveying the railways in northern England in 1845. He was elected FRS in 1861.

<sup>80</sup> Tyndall, Journal, 2 June 1850.

<sup>81</sup> Tyndall published the six main papers and supplementary material as *Researches on Diamagnetism and Magnecrystallic Action* (London: Longmans, 1870).

<sup>82</sup> J. Tyndall and H. Knoblauch, 'On the magneto-optic properties of crystals, and the relation of magnetism and diamagnetism to molecular arrangement', *Philosophical Magazine* (1850), 37, 1–33.

<sup>83</sup> Tyndall, Journal, 30 March 1850.

might be preferentially directed. Indeed, ‘anything that affects the mechanical arrangement of the particles will affect...the line of elective polarity’, and in crystals or other substances where there are several different ‘lines of elective polarity’ of different strengths the actual behaviour of a piece of matter will be complex.

In the final part of the paper, Tyndall demolished Plücker’s argument that the magnetic attraction decreases in a ‘quicker ratio’ than the repulsion of the optic axis, noting the importance of the degree of uniformity of the magnetic field in which the substance is placed, with flat poles equivalent to point poles withdrawn at a distance. He again used the method of powdering a crystal, in this case Iceland spar, reconstituted with gum and squeezed under pressure in one direction. It behaved just as the crystal, and any ‘optic axis’ force must surely have been absent. The conclusion was that the idea of structure and lines of ‘elective polarity’ were sufficient to explain all the effects of orientation in the magnetic field of magnetic and diamagnetic substances, whether crystalline, fibrous or amorphous, and that the relationship of the shape of the substance to the extent of uniformity of the field are critical.

Tyndall met the staff of *Philosophical Magazine* in late June, with his paper due to appear on 1 July. He also saw Faraday in June but, strangely for such a significant meeting, there is no note of it in his journal until 7 August, during his account of the discussion with Thomson at the British Association.<sup>84</sup> On 19 July Faraday sent a brief, friendly letter (the earliest recorded between them) thanking him for the samples of calcareous spar and remarking that he was now working on other aspects but would very likely turn to this again.<sup>85</sup> Knoblauch sent him a long letter on 21 July,<sup>86</sup> giving intelligence from his cousin in Bonn that Plücker still stuck fast to his theory of the optic axis, commenting ‘This holding fast to the optical axis with respect to these effects seems to me as if one wanted to fight with Newton’s fits of light against the wave theory’. He was suspicious of the purity of Plücker’s samples, gave information about the means of classifying optically positive and negative crystals, and suggested Tyndall should travel back via Göttingen to discover from Gauss the method of measuring crystal angles very accurately so they could complete their planned experiments on further crystals. He also mentioned that Plücker was planning a mathematical paper in *Crelle’s Journal* which would explain his phenomena.

In late 1850 Plücker published an extensive paper with Beer, in *Poggendorff’s Annalen*<sup>87</sup> - an abridged version of this paper appeared in June 1851 in *Philosophical Magazine* (it is not clear who abridged it).<sup>88</sup> In this paper Plücker asserted that magnetism and diamagnetism are caused by induction, with their induced currents opposite, and that diamagnetism is polar as shown by Reich, Weber and Poggendorff. He reiterated his belief that magnetic induction decreases more with distance than diamagnetic, which he put down to greater coercive force in diamagnetics, i.e. that the effect of induction lasts longer, and reemphasised the optic axis effect in positive and negative crystals, with extensive examples summarised at the end of the paper. Plücker referred to two memoirs by Tyndall and Knoblauch in *Philosophical Magazine*, and quoted from his own paper in

<sup>84</sup> Tyndall, Journal, 7 August 1850.

<sup>85</sup> Faraday to Tyndall, 19 July 1850 (Letter 2308 in F. A. J. L. James (note 56)).

<sup>86</sup> Knoblauch to Tyndall, 21 July 1850, RI MS JT/1/K/14.

<sup>87</sup> J. Plücker and A. Beer, ‘Ueber die magnetischen Axen der Krystalle und ihre Beziehung zur Krystallform und zu den optischen Axen’, *Annalen der Physik und Chemie* (1850), 81, 115–62.

<sup>88</sup> J. Plücker and A. Beer, ‘On the magnetic axes of crystals, and their relation to crystalline form and to the optic’, *Philosophical Magazine* (1851), 1, 447–57.

Latin, but also mentioned that the latest memoir from them had arrived too late to be referred to.<sup>89</sup> Plücker, though admitting that ‘many of my old opinions must now be modified’, claimed he had been misunderstood as to the meaning of ‘attraction’ or ‘repulsion’ of the optic axis when he clearly meant that the resultant of mechanical action coincides with it, not that it itself is physically attracted or repelled, and he spoke more of ‘magnetic axes’ than optical in this paper. He mentioned a forthcoming paper in *Crelle’s Journal* ‘Théorie Mathématique de l’Action des Aimant sur les Crystaux non appartenant au Système Tesséral’, which would give further explanation. Plücker explained that though Tyndall and Knoblauch agreed with him in many respects, their fundamental view was different; he believed that the three axes of elasticity of the aether of Fresnel produced the modification of magnetism as well as light.

On 31 July Tyndall travelled from Halifax to Edinburgh for his first British Association meeting, staying at a temperance hotel recommended by a fellow traveller. He arranged with the Secretaries of Section A that his paper should be heard the following day ‘in the respectable company of Sir David Brewster,<sup>90</sup> [who was President of the Association that year, with J D Forbes<sup>91</sup> President of the Section] and others’. On 2 August (his birthday, though he made no note of this) his paper was the second of the day, after one on meteorology, and he read poems by Emerson to calm his nerves. Only the title of the paper was published in the Report of the meeting,<sup>92</sup> but a full report was given in *Athenaeum*, Tyndall having been asked for abstracts after the session. According to *Athenaeum* it gave rise to ‘a very animated discussion’,<sup>93</sup> which Tyndall described as ‘a hand to hand fight’,<sup>94</sup> and was the first occasion, but by no means the last, on which Tyndall took on William Thomson, younger than Tyndall but already with a significant reputation, and a Vice-President of Section A that year. Tyndall had been able to have a quick word with Thomson before the session, which was invaluable ‘as I had time to turn his objections over in my mind instead of having to combat them impromptu’.<sup>95</sup> The session was chaired by Forbes, and Tyndall was flanked by Lord Wrottesley<sup>96</sup> to his immediate left, then Brewster and Forbes, with Thomson and Stokes<sup>97</sup> to his right, and ‘just before me were a bunch of ladies with mild brown eyes and every time I raised mine I found theirs fixed on me as if I had been reading the story of Jack and the beanstalk or something else equally interesting - It must have been the half dare-devil way in which

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<sup>89</sup> Since they only wrote two together, and Tyndall’s next paper was published after this one of Plücker and Beer, this is somewhat confusing.

<sup>90</sup> David Brewster (1781–1868), devout evangelical Presbyterian, concentrated on the study of optics and was an adherent of the emission theory of light. He was an editor of magazines and a prolific writer, and one of the founders of the British Association (DNB).

<sup>91</sup> James Forbes (1809–1868) was Professor of Natural Philosophy at Edinburgh from 1833 and Principal of St Andrews from 1860. He discovered the polarisation of radiant heat and, from 1840, carried out extensive work on the structure and motion of glaciers, a topic on which he later clashed substantially with Tyndall.

<sup>92</sup> J. Tyndall, ‘On the Magneto-Optical Properties of Crystals’, *British Association Report, Notes and Abstracts of Miscellaneous Communications to the Sections* (London: Murray, 1850), 23.

<sup>93</sup> *Athenaeum*, 10 August 1850, 842.

<sup>94</sup> Tyndall, *Journal*, 7 August 1850.

<sup>95</sup> Tyndall to Hirst, 4 August 1850, RI MS JT/1/T/530.

<sup>96</sup> John Wrottesley (1798–1867), landowner and astronomer, became President of the Royal Society in 1854.

<sup>97</sup> George Stokes (1819–1903), mathematician, was Lucasian Professor at Cambridge and is particularly known for his work on hydrodynamics. He was a Secretary of the Royal Society from 1854 until he became President in 1885. Like his friend William Thomson, and unlike Tyndall, he was devoutly religious.

I spoke and not the subject itself that interested them'.<sup>98</sup> The presentation lasted about 45 minutes and the discussion a similar time.<sup>99</sup> Forbes, impressed by the paper, and who had seen and been satisfied by Plücker's experiments in Bonn said 'Here we have a memoir which tends directly to invalidate the views of Faraday and Plücker' and invited comments. Thomson praised the 'beauty and ingenuity' of the experiments, emphasised that he believed Poisson's theory would be corroborated and supported by these results, and queried whether the use of the pressed dough showed that the action was not due to crystal structure, since smaller crystals might be oriented by pressure, and then defended Faraday's view of the 'directive force'. No-one else tackled Tyndall and the discussion continued between him and Thomson, while people kept thronging into the room. Tyndall noted that he had seen Faraday a few weeks before, telling him he felt compelled to differ, and Faraday had replied 'No matter, you differ not as a partisan, but because your convictions compel you'. Tyndall rebutted Thomson's claim that magnetic action in crystals can be reduced to three and only three lines of equilibrium, and that his dough experiments were open to Thomson's objection, using the example of powdered calcareous spar to counter it. Brewster seemed to support Tyndall on this, and Thomson finally said that he believed Tyndall would find his views and the theory of Poisson in harmony with Thomson's. Tyndall had thoroughly enjoyed the occasion, and also the response of the old door keeper of the Section who commented 'really Sir Professor Thomson could make nothing of it. He war (sic) completely under'.<sup>100</sup>

On 7 August 1850, having left Edinburgh and missed Thomson's paper 'On the theory of magnetic induction in crystalline and non-crystalline substances', Tyndall wrote most politely to Thomson: 'Would Professor Thomson have the kindness to write down in the margins the title of any book where I might find a statement of the magnetic theory of Poisson? If after having done this, the Professor would be good enough to return me this leaf I should feel very much obliged indeed'.<sup>101</sup> Thomson replied with an 18 page letter<sup>102</sup> giving the references for Poisson and a preview of his paper of 1851,<sup>103</sup> based on the presentation in Edinburgh that Tyndall had missed, and suggesting experiments Tyndall might make. Tyndall replied with thanks, adding somewhat ingratiatingly that he would 'feel truly happy to make whatever experiments you may require'.<sup>104</sup> He also noted, to Hirst, that he thought he had an edge 'I dont know whether I mentioned to you before that I have been favoured with a letter 18 pages long from the Professor of Natural Philosophy in Glasgow, my opponent in Edinburgh - He is working at the same subject, in fact every body will be having a trial at it as they see that a new field of speculation and experiment is opened. But it takes long preliminary discipline before man can get thoroughly into such a subject and in this respect <I> am a certain distance ahead, which advantage by the favour of the immortals I intend to maintain'.<sup>105</sup> Tyndall, as it happens, was still exploring alternatives to an uncertain scientific career at this moment, receiving a

<sup>98</sup> Tyndall to Hirst, 4 August 1850, RI MS JT/1/T/530.

<sup>99</sup> Tyndall to Mrs Stueart, 5 August 1850, RI MS JT/1/TYP/10.

<sup>100</sup> Tyndall to Mrs Stueart, 5 August 1850, RI MS JT/1/TYP/10.

<sup>101</sup> Tyndall to Thomson, 7 August 1850, CU Add 7342/T623, Kelvin Correspondence.

<sup>102</sup> Thomson to Tyndall, 14 August 1850, RI MS JT/1/T/9.

<sup>103</sup> W. Thomson, 'On the theory of magnetic induction in crystalline and non-crystalline substances', *Philosophical Magazine* (1851), 1, 177-86.

<sup>104</sup> Tyndall to Thomson, 31 August 1850, CU Add 7342/T624, Kelvin Correspondence.

<sup>105</sup> Tyndall to Hirst, 11 September 1850, RI MS JT/1/T/1013.

letter from his friend, and mentor, George Wynne giving advice in relation to his expressed wish ‘to be employed for the present on the Ordnance Survey’.<sup>106</sup>

On 25 September 1850, Knoblauch updated Tyndall on his progress back in Marburg, mentioning, from his cousin in Bonn again, that ‘Plücker will probably drop the law of positive and negative crystals, but still sticks fast to the idea that all effects which the crystal shows between magnetic poles are to be derived from the perspective of the optical axis. Further, he lays great emphasis (as counter-experiment to our results) on the observation that a diamagnetic calcareous spar (that is, one repelled from 1 pole) may stand with the optical axis from pole to pole and a magnetic one sometimes with the optical axis equatorial’.<sup>107</sup> Knoblauch saw no problem in countering this and stood ready to respond in *Poggendorff’s Annalen* as soon as Plücker’s threatened mathematical paper appeared in *Crelle’s Journal*, which in fact it never did. He authorised Tyndall to spend up to 150 Thalers for instruments he judged useful for their studies, such as pursuing Brewster’s method of identifying positive and negative crystals.

On 10 October 1850 Tyndall was back in Marburg, restarting his experiments on 18 October after a week occupied primarily with translations and an article for the *Leader*. On 21 October he noted receipt of the latest issue of *Poggendorff’s Annalen* with 60 pages from Plücker attempting to dispose of their first memoir (the second having arrived too late) – this was presumably the first Plücker and Beer paper, actually nearer 50 pages, which appeared in late 1850<sup>108</sup> – and he wrote to Faraday on 24 October sending magnetic crystals of calcareous spar (the ones he had sent earlier had proved not to be) of which the optic axis of a rhomboid will set pole to pole, unlike the diamagnetic ones.<sup>109</sup> Tyndall explained this ‘by reference to a principle which you were the first to hint at, that is to say “the action of contiguous particles”’. Faraday replied to explain that he was now deeply engaged in terrestrial magnetism, but hoped someday ‘to take up the point respecting the magnetic condition of associated particles’. But he welcomed the work of others: ‘Where science is a republic, there it gains; and though I am no republican in other matters, I am in that’.<sup>110</sup>

Tyndall’s collaboration with Knoblauch was starting to draw to a close, as he noted on 1 November that Knoblauch was appearing to take the credit in front of Bunsen for Tyndall’s work, and on 1 December he ‘made the resolution to dissolve the curious partnership which exists between me and Professor Knoblauch’, though they remained friends over many years. His experiments with diamagnetism at this time were accompanied by experiments on water jets which he did not think had been fully explained and which he was to publish in February 1851.<sup>111</sup> Indeed, on 31 December Tyndall recorded an effusive letter from William Francis,<sup>112</sup> his good friend and publisher of *Philosophical Magazine*, with the proof of the water jet article and a proposal for a

<sup>106</sup> George Wynne to Tyndall, 8 August 1850, RI MS JT/1/TYP/5/1841.

<sup>107</sup> Knoblauch to Tyndall, 25 September 1850, RI MS JT/1/K/15.

<sup>108</sup> J. Plücker and A. Beer (note 87).

<sup>109</sup> Tyndall to Faraday, 24 October 1850 (Letter 2333 in F. A. J. L. James (note 56)).

<sup>110</sup> Faraday to Tyndall 19 November 1850 (Letter 2344 in F. A. J. L. James (note 56)).

<sup>111</sup> J. Tyndall, ‘Phænomena of a water-jet’, *Philosophical Magazine* (1851), 1, 105–11.

<sup>112</sup> William Francis, of the publishing firm Taylor & Francis. Tyndall translated and summarised many papers for Francis, and was appointed one of the ‘conductors’ of *Philosophical Magazine* in early 1854. See W. H. Brock and A. J. Meadows, *The Lamp of Learning: Taylor & Francis and the Development of Science Publishing* (London: Taylor & Francis, 1984).



monthly Report on the Progress of Physics in *Philosophical Magazine* and translations of both French and German papers.<sup>113</sup>

Although those involved in the work at this time referred to magnetic and diamagnetic forces as though they were distinct there was an underlying sense that there might be a common cause. This is exemplified by von Feilitzsch's<sup>114</sup> letter to Faraday on 3 December 1850,<sup>115</sup> which Faraday had published in *Philosophical Magazine*.<sup>116</sup> Von Feilitzsch suggested that the intensity of distribution of magnetism is different in magnetic and diamagnetic substances and linked it to Ampère's theory of currents, with diamagnetism and magnetism manifestations of the same power: 'In the molecules of magnetic and diamagnetic bodies are electric currents'. These currents put themselves parallel to externally acting currents. He argued that there is great resistance in diamagnetics so that the intensity decreases from the centre and the substance is repelled, with the opposite in magnetic substances.

This next substantial piece of work showed Tyndall's strong systematising approach and careful experimentation, as he set out to establish the laws of magnetism, as established by Lenz and Jacobi for bodies not in contact, for those in contact or separated by very small distances, work which was carried out mostly in November and December. He wrote to Faraday on 4 February 1851,<sup>117</sup> and to Thomson in similar vein on 11 February,<sup>118</sup> enclosing the paper which he hoped would be published on 1 March. No reply is extant from Thomson. Faraday replied on 19 April in supportive mode,<sup>119</sup> in a letter which Tyndall received on 28 April in Berlin, a few days after his arrival to work in the laboratory of Magnus: 'I am fully able to appreciate the value of the results you arrive at, and it appears to me that they are exceedingly well established and of very great consequence. These elementary laws of action are of so much consequence in the development of the nature of a force which, like magnetism is as yet new to us'. His paper 'On the laws of magnetism' appeared in the very next issue of *Philosophical Magazine* in April.<sup>120</sup> Tyndall here established the relation of the strength of a magnet and its attracting power in contact and when separated by very small distances since the existing findings were confused, as he had shown in his review paper. He used spheres of material as best suited for experimentation, and improved the sensitivity of the experimental design by changing the magnetic power and seeing its effect on the sphere rather than vice-versa, since that was more controllable. He showed clearly that the mutual attraction of the magnet and a sphere of soft iron, in contact, is directly proportional to the strength of the magnet, unlike the case established by Lenz and Jacobi at a distance, when it is

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<sup>113</sup> The third report, which appeared in July, included a summary of a paper by Knoblauch 'On the department of crystalline bodies between the electric poles' (J. Tyndall, 'Reports on the progress of the physical sciences' *Philosophical Magazine* (1851) 2, 26–36), showing that magnetic crystals, which stand axial between magnetic poles stand equatorial between electric poles, and that diamagnetic crystals and substances artificially compressed stand equatorial in both cases. The latter observation reinforced their conclusion about the influence of the proximity of particles.

<sup>114</sup> Ottokar von Feilitzsch (1817–1885) came from an aristocratic German family. He was Professor of Physics at the University of Greifswald, where he had limited resources, working on magnetism and galvanic currents. He had been trained by Plücker in Bonn and Magnus in Berlin (see C. Jungnickel and R. McCormach (note 26), 228).

<sup>115</sup> von Feilitzsch to Faraday, 3 December 1850 (Letter 2350 in F. A. J. L. James (note 56))

<sup>116</sup> F. C. O. von Feilitzsch, 'On the physical distinction of magnetic and diamagnetic bodies', *Philosophical Magazine* (1851), 1, 46–51.

<sup>117</sup> Tyndall to Faraday, 4 February 1851 (Letter 2379 in F. A. J. L. James (note 56)).

<sup>118</sup> Tyndall to Thomson, 11 February 1851, RI MS JT/1/T/1440.

<sup>119</sup> Faraday to Tyndall 19 April 1851 (Letter 2411 in F. A. J. L. James (note 56)).

<sup>120</sup> J. Tyndall, 'On the laws of magnetism', *Philosophical Magazine* (1851), 1, 265–95.

proportional to the square of the strength. In exploring the relationship without contact he further innovated by using a subtle experimental design of iteratively identifying the required position of the rheostat to avoid any possible effect of the lag of magnetisation, and teased out with great precision the changing relationship as the distance between the sphere and the magnet became extremely small.

Tyndall left Marburg on 6 April for a long walk with Hirst in the area of the Lahn valley, returned for a few days and then left for Berlin, arriving on 21 April. He was soon at work on diamagnetism experiments. He wrote to Hirst 'This morning until 2 o'clock I have devoted to visiting, and have seen many of the great guns of science. I had an opportunity of making the celebrated experiment of du Bois Reymond, of exhibiting an electric current by the action of the muscles of my arm. I have been with Dove,<sup>121</sup> Magnus, Riess, and Poggendorff. Magnus' place is out of order just now but by Wednesday he will arrange a spot for me to work in'.<sup>122</sup>

### 3.3 Tyndall's 'Second Memoir' and the British Association Meeting in Ipswich, 1851

On 11 May 1851 Tyndall noted that he had been 'lucky beyond anticipation. Diamagnetism behaves exactly like magnetism; a double current excites a double amount'.<sup>123</sup> On 26 May he wrote to Faraday, saying that he had been at work on diamagnetism for 5 weeks but that 'It has been again my misfortune to arrive at conclusions very divergent from those of Prof. Plücker. A paper on the subject shall be ready for the British Association at its next meeting'.<sup>124</sup> On 30 May he had the disappointment of reading Becquerel's<sup>125</sup> paper on diamagnetism,<sup>126</sup> 'He has anticipated me in many of his results: that is, he has published before me, but I have got the same'.<sup>127</sup> To Hirst he was sanguine: 'He has not exhausted the matter however, and my method of experimenting is better than his'.<sup>128</sup> He left Berlin around 22 June, having seen little of the city while working so hard, conscious of the value of three years in Germany, of what he had achieved and what he yet might achieve in science. He was heading for the British Association, meeting in Ipswich from 2–8 July, via an unpleasant 50–60 hour journey and three days at Queenwood.

The meeting in Ipswich brought Tyndall into contact with Faraday again, and indicates his friendship with Francis, as they agreed to lodge together. Tyndall also became aware here of the vacant post at Toronto, one of several for which he was to apply over the next few years.<sup>129</sup> The administration of the meeting caused him frustration; he had 'sent in the titles of 4 papers with the probable time to be occupied in reading them',<sup>130</sup> but they were sent to the wrong Section so only three were given in the end.

<sup>121</sup> Heinrich Dove (1803–1879), meteorologist and physicist, was Director of the Prussian Institute of Meteorology from its founding in 1849, and professor at the University of Berlin (DSB). He received the Copley Medal in 1853, the year in which Tyndall declined his Royal Medal.

<sup>122</sup> Tyndall to Hirst, 29 April 1851, RI MS JT/1/HTYP/127–128.

<sup>123</sup> Tyndall, Journal, 11 May 1851.

<sup>124</sup> Tyndall to Faraday 26 May 1851 (Letter 2427 in F. A. J. L. James (note 56)).

<sup>125</sup> Edmond Becquerel (1820–1891), physicist, devoted most of his attention between 1845 and 1855 to the investigation of diamagnetism (DSB).

<sup>126</sup> E. Becquerel, 'De l'action du magnetisme sur tous les corps', *Annales de Chimie et de Physique* (1851), 32, 68–112. Becquerel, referring also to his previous results in *Annales de Chimie et de Physique* (1849) 28, 283, specifically contradicted Plücker's position in this paper.

<sup>127</sup> Tyndall, Journal, 30 May 1851.

<sup>128</sup> Tyndall to Hirst, 7 June 1851, RI MS JT/1/T/542.

<sup>129</sup> Others were Sydney and Galway.

<sup>130</sup> Tyndall to Hirst, 15 July 1851, RI MS JT/1/T/543.

Faraday, whom he had met in the street on 3 July, could only stay until the following day and wanted to hear the paper on diamagnetism which was duly brought forward, although several of those he would have liked to hear the paper were occupied with ‘the Prince and his train of asinine flunkeys’. The paper was given towards the end of 4 July,<sup>131</sup> ‘and the Section was already tired ... this induced me to hurry over the paper more quickly than I otherwise should have done’. Nevertheless, it was well received, and *Athenaeum* reported that Faraday spoke at some length on Tyndall’s contribution, which afforded him ‘...great gratification that there was one at least among us who has followed up this important subject so perseveringly’.<sup>132</sup> He ‘...felt prepared to admit that that some of Dr Tyndall’s results seemed to promise an explanation of Plücker’s perplexing results and conclusions ...’. In this paper, which forms the ‘Second Memoir’, given in detail in *Athenaeum* and published in September in *Philosophical Magazine*,<sup>133</sup> Tyndall used a torsion balance to measure diamagnetism in bismuth and disprove Plücker’s proposition that the laws that govern magnetism and diamagnetism are different, showing the attraction of magnetic substances and repulsion of diamagnetic substances by magnetic poles. Following another administrative blunder on the Monday, Tyndall finally presented two more papers on Tuesday, the last day.<sup>134</sup>

### 3.4 Diamagnetic polarity; The ‘Third Memoir’

On 30 July Tyndall wrote to Faraday<sup>135</sup> sending samples of materials which he invited Faraday to explore to demonstrate his conclusions on diamagnetism, referring to Faraday’s understanding of diamagnetic materials moving from places of stronger to weaker magnetic force. A few days later he wrote again,<sup>136</sup> remarking also in relation to his conclusions on diamagnetism that Faraday had noted with reference to his Bakerian Lecture in 1849 ‘Perhaps these points may find their explanation hereafter on the action of contiguous particles’.<sup>137</sup>

On 1 September he wrote to Thomson with a copy of the paper he had given in Ipswich, regretting that Thomson had not been there.<sup>138</sup> He mentioned that he had not yet read Thomson’s paper on the Theory of Magnetism in *Philosophical Transactions*, and that he was struggling with Poisson’s theory, being ‘...rather rusty at the calculus. I commenced Poisson theory 6 or 8 months ago, but never got through it; he writes with wonderful precision it is true, but he leaves many steps unexplained’. Thomson replied on 10 September with various papers and a commentary on Tyndall’s findings.<sup>139</sup> First, he approved of Tyndall’s conclusions on Plücker’s claim about the relative strengths of magnetic and diamagnetic forces ‘... I have always felt very much inclined to believe that

<sup>131</sup> J. Tyndall, ‘On Diamagnetism and Magnecrystallic Action’, *British Association Report, Notes and Abstracts of Miscellaneous Communications to the Sections* (London: Murray, 1851), 15–8.

<sup>132</sup> *Athenaeum*, 12 July 1851.

<sup>133</sup> J. Tyndall, ‘On diamagnetism and magnecrystallic action’, *Philosophical Magazine* (1851), 2, 165–88.

<sup>134</sup> One was on air-bubbles formed in water (J. Tyndall, ‘On Air-bubbles formed in Water’, *British Association Report, Transactions of the Sections* (London: Murray, 1851), 26–7) which was ‘exceedingly well received - though towards the close of the day, and though the room at the commencement was thin, before I ended every seat was occupied.’. and the other on thermoelectricity: J. Tyndall, Experiment in thermo-electricity with the monothermic pile invented by Prof. Magnus of Berlin’, *British Association Report, Transactions of the Sections* (London: Murray, 1851), 18–9.

<sup>135</sup> Tyndall to Faraday, 30 July 1851 (Letter 2451 in F. A. J. L. James (note 56)).

<sup>136</sup> Tyndall to Faraday, c3August 1851 (Letter 2454 in F. A. J. L. James (note 56)).

<sup>137</sup> Note to §2586 M. Faraday, *Experimental Researches in Electricity* (1855).

<sup>138</sup> Tyndall to Thomson, 1 September 1851, RI MS JT/1/TYP/5/1530-1531.

<sup>139</sup> Thomson to Tyndall, 10 September 1851, RI MS JT/1/T/10.

Plücher's "loi générale" about magnetism decreasing less rapidly than diamagnetism was entirely a delusion, and I am still so inclined after reading your two last papers'. Then he remarked that he was glad to see that Tyndall had '...so amply confirmed the theory of magne-crystallic induction as suggested by Poisson, and by Faraday (§2588), and verified experimentally by Faraday (§2841) for the single case of bismuth'.

But there remained an area of disagreement on the influence of proximity, with Thomson saying:

Ever since May 1847 (See Cambridge and Dublin Math. Journal Vol. II. p. 235 ff 12; or British Association Report Swansea 1848 Physical Section p. 9) I have been prepared to demonstrate that the effect of proximity among the particles of a diamagnetic powder is the reverse of what you assume it to be, but that it is so small as to be insensible in actual experiments. I think the very important experiments you describe in pages 19, 20, 21 of your last paper demonstrate that the effects of compression which you observe are due to a molecular alteration of the substances, and they fully confirm the second of the conjectures which I threw out at Edinburgh last year. I am quite ready to give up the first conjecture, the objection to it stated in p. 17 of your paper having occurred to myself as probably fatal to it, and your measurements (foot of p. 18) being very decisive against it. I hope before long to be able to write a short paper for the *Philosophical Magazine*, explaining my views regarding form and proximity as affecting the bearing of single bodies or of groups, in a magnetic field.

Tyndall held his ground on the effect of proximity in a response on 15 September,<sup>140</sup> although he apologised that due to lack of time for reading he had not referred to 'the close connection which subsists between the theoretic views advanced by you in the March number of *Philosophical Magazine* and my experiments'. He looked forward to Thomson's promised paper in *Philosophical Magazine*.

Following the Ipswich meeting, Tyndall took up the issue of polarity with vigour, and after a gap of a fortnight remarked with relief in his *Journal* on 1 October 'This night finished my memoir on 'Diamagnetic polarity'. I never laid down my pen in greater physical prostration'.<sup>141</sup> This work was published in *Philosophical Magazine* in November,<sup>142</sup> and referred to as the 'Third Memoir' in *Researches on Diamagnetism and Magnecrystallic Action*.

Tyndall set the scene in this paper on the polarity of bismuth by remarking that 'On the one side we have Weber, Poggendorff, and Plücker, each affirming that he has established this polarity; on the other side we have Faraday, not affirming the opposite, but appealing to an investigation which is certainly calculated to modify whatever conviction the results of the above-mentioned experimenters might have created'. He again showed his ability to improve on the experimental sensitivity of previous approaches in the way in which he prepared his sample of bismuth, so that it set axially rather than equatorially, presenting a mechanical couple of far greater power than if it were equatorial. The experiments showed deflection of bars of bismuth in the same direction as those of magnetic shale or of iron, implying that the north pole of the magnet

<sup>140</sup> Tyndall to Thomson, 15 September 1851, RI MS JT/1/TYP/5/1534.

<sup>141</sup> Tyndall, *Journal*, 1 October 1851.

<sup>142</sup> J. Tyndall, 'On the polarity of bismuth, including an examination of the magnetic field', *Philosophical Magazine* (1851), 2, 333–44.

excited a south pole in the bismuth and vice-versa, which is not what Poggendorff had found in 1847. As Tyndall observed, these experiments seemed to bear out the conclusions of von Feilitzsch,<sup>143</sup> though he noted that he saw no way of reconciling the repulsion of the total mass of a piece of bismuth with the idea of a polarity similar to that of iron. Considering that these effects might perhaps be caused by reference to the change effected in the magnetic field when intersected by an electric current, in the context of Faraday's view that 'diamagnetic bodies tend to go from stronger to weaker places of action', Tyndall devised experiments to test this, using a small sphere of carbonate of iron as a sensitive means of testing the relative force at various places. He showed that the changing effect on the magnetic field (a term he was using, and continued to use) explained the movement in Faraday's terms. Nevertheless, the voice of the believer in diamagnetic polarity then raised itself, as Tyndall asked if two opposite poles, acting on a body, do so by annulling each other 'by interference *before they reach the body*; or does one pole induce in a body the certain condition upon which the second pole acts in a sense contrary...if the latter, then we must regard the field as possessing two systems of forces;...'. The latter, Tyndall argues, indicates diamagnetic polarity, and he recalled Reich's experiments in support.<sup>144</sup> He then argued that diamagnetism is induced because, as demonstrated by Becquerel and himself, the repulsion of diamagnetic bodies follows the same law of squares as that of magnetic. Then at the end of the paper, contrasting the 'magnetic fluids' of Poisson with the 'lines of force' of Faraday, Tyndall claimed that Reich's experiments, showing 'that the matter evoked by one pole will *not* be repelled by an unlike pole, compels us to assume the existence of *two kinds* of matter, and this, if I understand the term aright, is polarity'. This indicates Tyndall's use of a two-fluid theory, and indeed in the polarity of opposites. Much later, in 1870, Tyndall reflected that this 'slight paper could have very little influence upon so weighty a question', and that he had therefore resumed the study of diamagnetism in the autumn of 1854.<sup>145</sup>

Faraday appears to refer to a version of this paper (not the one footnoted in the Faraday Correspondence) in a letter to Tyndall of 21 October 1851,<sup>146</sup> mentioning that he was about to submit a paper to the Royal Society touching on polarity.<sup>147</sup> Tyndall also sent this paper to Thomson, who responded on 7 November both thanking him for it and apologising for a mistake in his own paper, 'merely a slip of the pen', which had perplexed Tyndall.<sup>148</sup>

#### 4. The Royal Society, the Royal Institution and the Royal Medal

By early 1852 Tyndall was coming to the end of his first phase of work on diamagnetism, which he would not resume fully until the autumn of 1854. This period of his life, following his work with Knoblauch in Marburg and in Magnus's laboratory in Berlin, saw him seeking to establish himself as a natural philosopher, which he did from a

<sup>143</sup> F. C. O. von Feilitzsch (note 116).

<sup>144</sup> F. Reich (note 34).

<sup>145</sup> J. Tyndall (note 81), 88.

<sup>146</sup> Faraday to Tyndall, 21 October 1851 (Letter 2468 in F. A. J. L. James (note 56)).

<sup>147</sup> M. Faraday, 'On lines of magnetic force; their definite character; and their distribution within a magnet and through space', *Philosophical Transactions of the Royal Society of London* (1852), 142, 25–56.

<sup>148</sup> Thomson to Tyndall, 7 November 1851, RI MS JT/1/T/11.

base in Queenwood College, occupied with some teaching, with some research and with translations and summaries of papers for Francis.<sup>149</sup>

#### 4.1 Election to the Royal Society

Applications for posts in Toronto, Sydney and Galway and a possible post in Cork came to nought, but he had caught the eye of many in Britain, not least Edward Sabine who offered, on hearing that he might go to Toronto, to put in motion his election as a Fellow of the Royal Society.<sup>150</sup> Faraday offered unprompted to sign the certificate and Tyndall secured Sylvester<sup>151</sup> and Huxley as two other signatories.

Tyndall's name was read out at the Royal Society on 6 May as one of 15 candidates recommended by the Council out of an original 34.<sup>152</sup> The certificate states that he is 'The Author of a Mathematical Dissertation on a curved surface:- and of Memoirs on, the Magneto-optic Properties of Crystals, and the relation of Magnetism & Diamagnetism to Molecular Arrangement:- on the phaenomena of a water-jet:- on the laws of Electro-magnetic attraction:- on diamagnetic and Magnecrystallic Action:- on the Polarity of Bismuth:- and of several reports on the progress of the Physical Sciences. - Distinguished for his acquaintance with the science of Natural Philosophy'. Essentially, it was his work on diamagnetism that formed the basis of a record and reputation sufficient for election.

#### 4.2 Early work on the transmission of heat

On 20 January 1852 Tyndall had noted in his journal that he had been experimenting with wood and finished the first part of a paper on molecular influences intended for *Philosophical Transactions*. By 20 February he recorded that he hoped to send the paper off to Sabine in a few days, which he eventually did on 13 May.<sup>153</sup> This paper, which appears to have consisted of two sections (only Part I was later published) was refereed by Thomson, who found 'results on the conduction of heat in wood which I believe to be new, and which are certainly very interesting...ought not to be published without a very distinct reference to M de Lenarmont as the first experimenter who published researches

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<sup>149</sup> In the period, while producing the many translations and summaries, Tyndall's focus was changing from diamagnetism to the transmission of heat, as he sought through both to explore the influence of structure and proximity, although some work on diamagnetism continued, which he was to report at the British Association meeting in Belfast. He also noted on 27 June 1852: 'Reading Plücker's bewildering memoir in the forenoon' (Tyndall, Journal, 27 June 1852). In December 1852 Tyndall published 'On the reduction of temperatures by electricity' (J. Tyndall, 'On the reduction of temperatures by electricity', *Philosophical Magazine* (1852), 4, 412–23), written from Queenwood in November. This was part of a running argument with Richard Adie, who maintained that absorption of heat did not take place at a bismuth antimony joint (R. Adie, 'On the unequal heating effect of a galvanic current while entering and emerging from a conductor', *Philosophical Magazine* (1852), 4, 224–5). Adie also delved into diamagnetism, though without great penetration (R. Adie, 'On the relation of magnetism and diamagnetism to the colour of bodies', *Philosophical Magazine* (1852), 4, 451–2). Tyndall reiterated his bemusement in a note in February 1853 (J. Tyndall, 'On the temperatures of conductors of electrical currents', *Philosophical Magazine* (1853), 5, 147).

<sup>150</sup> Sabine to Tyndall, 6 November 1851; Tyndall, Journal, 6 November 1851.

<sup>151</sup> James Sylvester (1814–1897) was a mathematician working particularly on invariants. He was awarded the Royal Medal in 1861 and the Copley Medal in 1880 (Oxford Dictionary of National Biography, 2004; hereafter abbreviated as ODNB).

<sup>152</sup> Tyndall, Journal, 3 June 1852. Those signing the certificate from general knowledge were Wheatstone, Playfair, Edward Forbes, Henry and Airy; and from personal knowledge Faraday, Grove, Huxley, Sylvester and John Phillips. The original certificate, sent to Sylvester, was lost so Tyndall had to write out his qualifications again and this may explain why the writing on the certificate appears to be Tyndall's own, which is not normal practice (Election certificate, RS EC/1852/13). Sabine also told him that Grove and Gassiot had asked to sign.

<sup>153</sup> Tyndall, Journal, 15 May 1852.

on the unequal conducting powers of bodies in different directions...the method of heating the plates of the substances to be experimented on adopted by Mr. Tyndall, which appears to have considerable advantages over that described by M. Lenarmont...The preamble (pages 1...7) might I think, with advantage to the paper, be omitted'.<sup>154</sup> Bell gave Tyndall the report, with some remarks of Sabine, after his admission to the Royal Society on 17 June. Tyndall remarked 'The report on the whole was a flattering one, but Professor Thomson, as is very natural to a young man, wishes to shew that he knows something about the matter'.<sup>155</sup> It was some time before Tyndall's spikiness towards Thomson dissipated.

On 19 June, two days after Tyndall had received comments from Thomson and Sabine on his paper on the transmission of heat, he sought out Thomson who he found with Faraday. He talked with Faraday about his theory of lines of magnetic force (Faraday lent Tyndall his private copy of his recent paper since Tyndall had been unable to access *Philosophical Transactions*),<sup>156</sup> and then with Thomson about magne-crystalline action and Thomson's statement in the private letter to him that he was prepared to prove the action of proximity the reverse of Tyndall. Tyndall did not believe he could, and arranged to breakfast with him the next week.<sup>157</sup>

Tyndall continued to carry out work during July on the transmission of heat through different substances, using cubes of material. He benefited from the knowledge and contacts with instrument makers of Knoblauch, who wrote to him on 10 August, replying to Tyndall's letter of 5 July, saying that he had ordered a thermopile from Kleimer to the same specification as his own.<sup>158</sup> Knoblauch also sent a number of expensive cubes of crystals on 16 April 1853.<sup>159</sup>

Tyndall's paper on this subject, and indeed his first in *Philosophical Transactions* was submitted in October<sup>160</sup> and read on 6 January 1853.<sup>161</sup> He did not know it was scheduled but happened to be there.<sup>162</sup> When a Mr Brooke<sup>163</sup> made critical comments and asked a question, Hopkins,<sup>164</sup> presiding, who had been told that Tyndall was in the room, invited him to reply which he did, the first time he spoke at the Royal Society and in a way that amused Sabine and others and drew applause.<sup>165</sup> The revised paper was then resubmitted and finally published.<sup>166</sup> The referees, Thomson<sup>167</sup> and W. H. Miller,<sup>168</sup> were

<sup>154</sup> Thomson to Bell, 15 June 1852, RS RR/2/247.

<sup>155</sup> Tyndall, Journal, 17 June 1852.

<sup>156</sup> M. Faraday, 'On lines of magnetic force; their definite character; and their distribution within a magnet and through space', *Philosophical Transactions of the Royal Society of London* (1852), 142, 25–56.

<sup>157</sup> Tyndall, Journal, 19 June 1852. The conversation at that event, on Tuesday 22 June, is not reported in any detail.

<sup>158</sup> Knoblauch to Tyndall, 10 August 1852, RI MS JT/1/K/18.

<sup>159</sup> Knoblauch to Tyndall, 16 April 1853, RI MS JT/1/K/19.

<sup>160</sup> It was formally received at the Royal Society on 20 October 1852.

<sup>161</sup> The only original manuscript with the Royal Society is that of the final published paper (RS PT/46/6).

<sup>162</sup> Tyndall to Hirst, 9 January 1853, RI MS JT/1/T/558.

<sup>163</sup> Possibly Charles Brooke (1804–1879), surgeon, who later seconded Tyndall's nomination for the Royal Medal.

<sup>164</sup> William Hopkins (1793–1866), mathematician and geologist, elected FRS in 1837, was an important figure in Cambridge, particularly in the education of mathematicians such as Thomson and Stokes. He would later have much interaction with Tyndall on the subject of glaciers.

<sup>165</sup> Tyndall, Journal, 6 January 1853.

<sup>166</sup> J. Tyndall, 'On Molecular Influences. Part I. Transmission of Heat through Organic Structures', *Philosophical Transactions of the Royal Society of London* (1853), 143, 217–31.

<sup>167</sup> Thomson 22 March 1853, RS RR/2/248.

<sup>168</sup> William Miller (1801–1880), professor of mineralogy at the University of Cambridge, and Foreign Secretary of the Royal Society 1856–1873.

very supportive, Miller writing ‘his mode of experimenting is new, yet so simple in principle that it probably will be applicable very generally’.<sup>169</sup>

#### 4.3 *British Association Meeting in Belfast, 1852*

Tyndall had not intended to go to the meeting in Belfast but once again Sabine, President that year, intervened in his career by asking if he would accept a Secretaryship of the Physical Section. This he did, commenting ‘Nearly 5 years have elapsed since my foot rested on Irish ground ... Little did I imagine that 5 years ago I should mingle among the magnates of the British Association, and mingle with them by a fairly won right, at my next visit’.<sup>170</sup>

Tyndall arrived in Belfast on the afternoon of Wednesday 1 September, hearing Sabine’s Address that evening. The next morning he was at his post as Secretary, procuring a battery and electromagnet from the Catholic Bishop Danvers, while ‘a skirmishing fire was kept up over every paper’<sup>171</sup>...But the warmest discussion was on Professor Thomson’s paper on the lines of force’.<sup>172</sup> The summary in *Athenaeum* gives no discussion and ends with ‘It would be impossible by a mere abstract to make the communication generally intelligible’.<sup>173</sup> Tyndall had just started to respond when the Lord Lieutenant arrived, accompanied by his Countess and suite but Tyndall ‘managed the matter very coolly’, as Brewster participated too and Sabine sat behind. Tyndall gave a hint of the discussion in a letter to Hirst: ‘With regard to the bismuth I must have expressed myself equivocally. I never meant to say that its polarity is the same as that of iron. I believe it has the polarity which is attributed to it by Weber and my experiments go to prove this. In fact it was on this very point that Thomson and myself had the discussion before the Lord Lieutenant’.<sup>174</sup>

On the Friday Tyndall gave his paper ‘On molecular action’,<sup>175</sup> given as ‘On the Molecular Peculiarities of certain Organic Substances’ in *Athenaeum*,<sup>176</sup> based on his experiments on heat conductivity in 57 different kinds of wood according to the direction of transmission in relation to the direction of the wood fibre and ligneous growth layers. It was cut short by the presiding Thomson, as time was pressing. Tyndall felt, as apparently did several other participants whose views he heard the following day, that ‘had I been in Thomson’s place I think I should have afforded him more time than he afforded me’.<sup>177</sup> Thomson then made a long communication on the Saturday ‘so spun out as to throw me quite to the end of the day’,<sup>178</sup> leaving a very small audience to whom Tyndall, with the

<sup>169</sup> Miller, 28 March 1853, RS RR/2/247.

<sup>170</sup> Tyndall, *Journal*, 26 and 29 August 1852.

<sup>171</sup> Tyndall, *Journal*, 2 September 1852.

<sup>172</sup> W. Thomson, ‘On certain magnetic curves; with applications to problems in the theories of heat, electricity and fluid motion’, *British Association Report, Notes and Abstracts of Miscellaneous Communications to the Sections* (London: Murray, 1852), 18. Thomson, who was President of Section A, gave five papers at this meeting, including one with Joule on what is now known as the Joule-Thomson effect, although no detail of it is given in the *British Association Report*.

<sup>173</sup> *Athenaeum*, 11 September 1852, 978.

<sup>174</sup> Tyndall to Hirst, 19 September 1852, RI MS JT/1/T/553.

<sup>175</sup> J. Tyndall, ‘On molecular action’, *British Association Report, Notes and Abstracts of Miscellaneous Communications to the Sections* (London: Murray, 1852), 20.

<sup>176</sup> *Athenaeum*, 11 September 1852, 980.

<sup>177</sup> Tyndall, *Journal*, 4 September 1852.

<sup>178</sup> Tyndall, *Journal*, 4 September 1852.



help of the bishop for his demonstration, presented his view that Poisson had not really anticipated magne-crystallic action as discovered by Plücker,<sup>179</sup> as Thomson had claimed, since he had suggested that the action would be caused by the ellipsoidal shape of the magnetic elements, whereas Tyndall showed that it was due to the manner of arrangement of the molecules, not their shape, using crystals of calcareous spar, carbonate of iron and white wax of the same shape and size as the spar. Tyndall was already thinking here about the relative arrangement of molecules and not just their supposed shape. Thomson delivered ‘some very eulogistic remarks’, Tyndall remarking that ‘Thomson I believe is a decent soul at bottom but he is greatly afraid of his fame. I think it will never be extraordinary’.<sup>180</sup> *Athenaeum* reporting him saying that ‘Dr. Tyndall’s discoveries...had cleared away a mass of rubbish, and set things in their true light’.<sup>181</sup> Tyndall had his eyes on Stokes too: ‘Stokes has been greatly praised...and he is a proud fellow...the time will come when he can’t afford to be proud to me’.<sup>182</sup> He had the grace to note in October 1853 ‘I will let the record stand to prove what an egotist thou wert’.

#### 4.4 Appointment to the Royal Institution

On 17 October 1852 Tyndall noted a letter from Bence Jones,<sup>183</sup> saying he had mentioned him to Barlow<sup>184</sup> ‘as one likely to give a good course of lectures; and invited me to deliver a lecture on some Friday evening either before or after Easter’.<sup>185</sup> Tyndall wrote positively in response, and on 24 October recorded receipt of a letter from Barlow asking him to fix some Friday in February for his lecture at the Royal Institution.<sup>186</sup> ‘The subject which you are likely to take is an excellent one for our audience and we have the same electric magnet with which Dr. Faraday rotated a ray of light & established diamagnetism’.<sup>187</sup> On 6 January 1853 Tyndall went with Barlow to the Royal Institution to make arrangements, meeting Faraday ‘like an alchemist at work beside the fire’, who told him that ‘he would be obliged to say ‘no’ to some of my results’ (on diamagnetism) in his own lecture, planned for 21 January.<sup>188</sup>

On Friday 11 February Tyndall gave his first Friday Evening Discourse at the Royal Institution ‘On the influence of material aggregation upon the manifestations of force’,<sup>189</sup> having repeated many of the experiments with Faraday beforehand. The lecture was an immediate success, as Tyndall restated his ideas about the influence of molecular arrangement and proximity, instead of relying on the posited ‘optic axis’ force (by Plücker) or ‘magnecrystallic’ force (by Faraday). He paid warm tribute to Faraday, having challenged his interpretation of the phenomena; at the end Faraday crossed the platform

<sup>179</sup> J. Tyndall, ‘On Poisson’s Theoretic Anticipation of Magnecrystallic Action’, *British Association Report, Notes and Abstracts of Miscellaneous Communications to the Sections* (London: Murray, 1852), 20–1.

<sup>180</sup> It was some years until Tyndall seems to have overcome a certain jealousy of Thomson, some years younger yet more established and clearly much superior mathematically.

<sup>181</sup> *Athenaeum*, 18 September 1852, 1010–11.

<sup>182</sup> Tyndall, *Journal*, 4 September 1852.

<sup>183</sup> Henry Bence Jones (1813–1873), physician and chemist, was instrumental in the appointment of Tyndall as Professor of Natural Philosophy at the Royal Institution in 1853. He became a manager of the Royal Institution in April 1853 and was Secretary from 1860–1872 (ODNB).

<sup>184</sup> John Barlow (1798–1869) was Secretary of the Royal Institution from 1843–1860.

<sup>185</sup> Tyndall, *Journal*, 17 October 1852.

<sup>186</sup> Tyndall, *Journal*, 24 October 1852.

<sup>187</sup> Barlow to Tyndall, 21 November 1852, RI MS JT/1/TYP/1/142.

<sup>188</sup> Tyndall, *Journal*, 6 January 1853. M. Faraday, ‘Observations on the Magnetic Force’, *Proceedings of the Royal Institution of Great Britain* (1853), 1, 229–38.

<sup>189</sup> J. Tyndall, ‘On the Influence of Material Aggregation Upon the Manifestations of Force’, *Proceedings of the Royal Institution of Great Britain* (1853), 1, 254–9.

and shook his hand as did the Duke of Northumberland, presiding. Mr Whitbread, ‘the brewer’, waited behind to say ‘anything to surpass your lecture tonight I never heard anywhere’.<sup>190</sup> Frankland, in his obituary of Tyndall, wrote ‘The lecture, although of such an abstruse character, took his audience – mostly popular as it was – by storm’.<sup>191</sup>

Invitations poured in. Bence Jones offered him 4 lectures at the London Institution<sup>192</sup> at 5 guineas a lecture,<sup>193</sup> suggested that he would be offered a Professorship there at £200 a year, and mentioned that there was a vacancy at the Royal Institution at £150 a year which he was trying to enable him to qualify for; Gassiot also wrote to ask him to lecture at the London Institution.<sup>194</sup> Barlow offered him 4 lectures at the Royal Institution at 5 guineas a lecture, and invited him to give a second Discourse on 3 June, an unusual honour.<sup>195</sup> By 25 February he was writing to Hirst ‘It is likely that two openings will [occu]r one at the Royal Institution [one] at the London Institution’.<sup>196</sup> On 15 April Bence Jones offered terms for the Royal Institution, with the first formal duties to be in January 1854.<sup>197</sup>

Tyndall lectured at the Royal Institution on Saturday 21 May,<sup>198</sup> and on 25 May received a further letter from Bence Jones with the formal proposal ‘The managers met today and I am requested to communicate to you officially, that in consequence of a recommendation from Mr. Faraday the managers are desirous of proposing you for election as Professor of Natural Philosophy with £200 a year’.<sup>199</sup> After receiving his response, and discussing it with him at the Royal Society, Bence Jones wrote to confirm the election date of 6 July,<sup>200</sup> with his duties to start in January but the salary immediately.<sup>201</sup>

Tyndall’s second Discourse, ‘On some of the eruptive phenomena of Iceland’, took place on 3 June.<sup>202</sup> He was not entirely happy with it - although it stimulated the directors of the Crystal Palace to have a geyser built<sup>203</sup> - but overall very content with his position ‘I have stood at no man’s door craving admittance, I have been asked in, every external advancement has been given, not sought. I never sought the Royal Society, still it came. I never sought the Royal Institution, but it has come. I never sought the society of the great and eminent, still I have got into such society...’.<sup>204</sup>

6 July 1853 was Tyndall’s last day as a teacher at Queenwood, although he was to return often to visit friends. He had been at the Royal Society for the election of Fellows, including introducing Frankland. Further honours were coming his way too; he had been unanimously elected on 20 May as an honorary member of Société Royal Hollandaise des

<sup>190</sup> Tyndall, Journal, 11 February 1853.

<sup>191</sup> E. Frankland, ‘John Tyndall’, *Proceedings of the Royal Society of London* (1894), 55, xviii–xxxiv.

<sup>192</sup> The London Institution had similar aims to The Royal Institution and was located at Finsbury Square, F. Kurzer, ‘Chemistry and Chemists at the London Institution 1807–1912’, *Annals of Science* (2001), 58, 163–201.

<sup>193</sup> Tyndall, Journal, 16 February 1853.

<sup>194</sup> Tyndall, Journal, 20 February 1853.

<sup>195</sup> Tyndall, Journal, 18 March 1853.

<sup>196</sup> Tyndall to Hirst, 25 February 1853, RI MS JT/1/T/560.

<sup>197</sup> Bence Jones to Tyndall, 15 April 1853, RI MS JT/1/HTYP/239.

<sup>198</sup> Tyndall, Journal, 21 May 1853.

<sup>199</sup> Bence Jones to Tyndall, 23 May 1853, RI MS JT/1/TYP/682.

<sup>200</sup> Bence Jones was mistaken; the election took place on 4 July, Minutes of General Meetings of The Royal Institution, 93, RI MS AD/02/B/01/A06.

<sup>201</sup> Bence Jones to Tyndall, 6 June 1853, RI MS JT/1/TYP/683.

<sup>202</sup> J. Tyndall, ‘The Eruptive Phenomena of Iceland’, *Proceedings of the Royal Institution of Great Britain* (1853), 1, 329–35.

<sup>203</sup> Tyndall to Hirst, 16 June 1853, RI MS JT/1/T/563.

<sup>204</sup> Tyndall, Journal, 26 June 1853. While he made every effort to get himself noticed, through publishing and translating in particular, he does not seem to have actively solicited the support of powerful figures such as Sabine.

Sciences, and a note from Francis intimated he was a candidate for the Royal Society's Royal Medal.<sup>205</sup> He was also becoming a force in the world of scientific publishing, not least as a help to German physicists, given his close relationship with Francis and his *Philosophical Magazine* and Scientific Memoirs. Indeed Francis named him as one of the 'conductors' of the *Philosophical Magazine* in January 1854, in the company of Brewster, Kane, Taylor and himself. Although he was not actively engaged in this period in experiments on diamagnetism it was nevertheless still in his mind; he mentioned in a letter of July 1853 to Francis that he was engaged on one of Plücker's papers on the mathematical theory of diamagnetism,<sup>206</sup> although it is not clear to which paper this refers.

#### 4.5 The Royal Medal

Tyndall left London on 11 July,<sup>207</sup> and would not return until late September. This period in Berlin, and subsequently in Paris, further extended his relationships with German and French natural philosophers.

While Tyndall was in Paris the Annual Meeting of the British Association took place from 7-14 September in Hull, with Hopkins presiding. The programme in Section A was quite light, but both Plücker (who was listed as a member of the Committee of Section A) and Matteucci<sup>208</sup> spoke on magnetism. Plücker reiterated his belief that 'the power of action on diamagnetic bodies augments more rapidly than the action on magnetic ones',<sup>209</sup> giving his conclusions about the limit or saturation of magnetism in different substances. The paper was printed in *Athenaeum*,<sup>210</sup> as were two of Matteucci's three papers, but no discussion was recorded.

After returning to England, staying with Bence Jones in Folkestone followed by a few days at Queenwood, Tyndall reached London on 29 September. On 6 October he was preparing cubes of bismuth at the Royal Institution, fortified by a loaf, two pounds of cheese and a regular supply of porter from Charles Anderson, the laboratory assistant of 25 years. For the next few days he started familiarising himself with the resources of the Royal Institution, writing his introductory lecture and experimenting, working on the rocking of heated metals.<sup>211</sup>

<sup>205</sup> Tyndall, Journal, 6 July 1853.

<sup>206</sup> Tyndall to Francis, undated July 1853, RDS 27/32.

<sup>207</sup> Tyndall, Journal, 11 July 1853.

<sup>208</sup> Carlo Matteucci (1811-1868), electrophysiologist whose major work was on electric discharge of torpedoes, the resting potential of the frog muscle and action currents, which he discovered (DSB). Matteucci was a good friend of Faraday.

<sup>209</sup> J. Plücker, 'On Magnetism', *British Association Report, Notes and Abstracts of Miscellaneous Communications to the Sections* (London: Murray, 1853), 7-8.

<sup>210</sup> *Athenaeum*, 1 October 1853, 1164

<sup>211</sup> He worked all day on this on 14 October, and a few days later he produced distinct tones with bismuth, which Forbes had found completely inert either as a rocker or a bearer in Trevelyan's experiment (Tyndall, Journal, 25 October 1853). Tyndall's paper on 'rockers' (J. Tyndall, 'On the Vibrations and Tones Produced by the Contact of Bodies Having Different Temperatures', *Philosophical Transactions of the Royal Society of London* (1854), 144, 1-10) was read at the Royal Society on 26 January, and he showed some experiments afterwards in the library, commenting 'They all seemed amused at the manner in which I have "demolished Forbes" as they express it. It is just what he would like to do himself!' (Tyndall, Journal, 26 January 1854). The paper for *Philosophical Transactions* was refereed by Wheatstone (C. Wheatstone, 9 February 1854, RR/2/250) and Grove (C. Grove, 15 February 1854, RR/2/251). Wheatstone noted 'Dr Tyndall's memoir derives its whole value from its refutation of a theory subsequently advanced by Prof. James Forbes...'. Grove, perhaps presciently for some of Tyndall's later altercations, including with Forbes, remarked that 'some inconvenience may result from the introduction into the Phil Trans of a paper of a controversial character...Dr Tyndall's objects...equally well effected by communicating the experiments to the Phil Magazine or a similar journal of science'.

Tyndall now had access to Faraday's large electromagnet, and on 18 October he found perplexing results which nevertheless 'will throw some light upon the relation of magnetism and diamagnetism'.<sup>212</sup> The following day he noted that in gypsum the line which set from pole to pole is the line of quickest transmission of heat, which contradicted his conclusion deduced from diamagnetism experiments that the line of greatest density is the line of best heat conductivity, so 'in the case of gypsum the line of least density is the line of best conductivity or my statements regarding magnetic action are not universally true', but 'It does not seem improbable that with a very bad conductor the line of closest proximity may be that of worst conduction'.<sup>213</sup> This would 'open entirely new views on the nature of conduction, and it will at the same time corroborate all I have heretofore said of magnetic action'. He talked with Faraday about diamagnetic polarity on 30 November, although the substance of the discussion is not recorded.<sup>214</sup>

On 4 November Tyndall heard from Bence Jones that he was the elected candidate for a Royal Medal, against Hofmann,<sup>215</sup> Frankland, Cayley<sup>216</sup> and Sylvester, and also heard of the political dealing which had resulted in this outcome; J P Gassiot<sup>217</sup> having proposed him and Charles Brooke seconded, 'for his paper 'On Diamagnetism and Magnecrystalline Action', published in the *Philosophical Magazine* for 1851'.<sup>218</sup> A letter from Gassiot on 9 November indicated that Gassiot had proposed him for a discovery which he considered would help solve 'the true cause of the variation of the magnetic needle'.<sup>219</sup> But matters became complicated, as Gassiot, after speaking with Faraday, told Tyndall that there were objections; people 'say that my investigations were partly conducted along with Knoblauch and partly in the private cabinet of Prof. Magnus in Berlin, and add something regarding Plücker's priority which I do not understand'.<sup>220</sup> Tyndall, after consultation with Faraday and Gassiot, determined not to accept this singular honour, the only time in its history in which a medal has been awarded and not presented.<sup>221</sup>

### 5. Tyndall's second phase of work

Faraday gave a Friday Evening Discourse on 9 June 1854 'On Magnetic Hypotheses',<sup>222</sup> in which he particularly took issue with atomic and molecular theories

<sup>212</sup> Tyndall, *Journal*, 18 October 1853.

<sup>213</sup> Tyndall, *Journal*, 19 October 1853.

<sup>214</sup> Tyndall, *Journal*, 30 November 1853.

<sup>215</sup> August Wilhelm von Hofmann (1818–1892) studied with Liebig in Giessen, and became professor and director of the Royal College of Chemistry on its establishment in 1845. In a series of papers in 1849–1851 on substituted ammonias he laid the basis for the theory of atomic valence, with Edward Frankland and others, and the theory of chemical structure, proposed formally by Kekulé and Couper in 1858 (ODNB).

<sup>216</sup> Arthur Cayley (1821–1895), mathematician, published early in his career on determinants and invariant theory, and was the first to write a paper on quaternions following their discovery by William Rowan Hamilton in 1843 (ODNB).

<sup>217</sup> John Peter Gassiot (1797–1877) was a wealthy wine merchant who had his own laboratory on Clapham Common in which he concentrated on the study of voltaic electricity and discharge of electricity through gases at low pressure (ODNB).

<sup>218</sup> Royal Society Minutes of Council, 23 June 1853.

<sup>219</sup> Tyndall, *Journal*, 9 November 1853. Tyndall's researches were not specifically directed at this question, although he did refer to the connection in his first Discourse (note 189).

<sup>220</sup> Tyndall, *Journal*, 15 November 1853.

<sup>221</sup> A full account of this episode is given in R. Jackson, 'John Tyndall and the Royal Medal that was never struck', *Notes and Records* (2014), 68, 151–64.

<sup>222</sup> M. Faraday, 'On Magnetic Hypotheses', *Proceedings of the Royal Institution of Great Britain* (1854), 1, 457–9.

of electricity and magnetism, as expressed by Ampère, Weber and De la Rive. This seems to have spurred Tyndall back into action, perhaps because he did want to make a case for understanding and visualising phenomena in those terms; he had done little systematic work on diamagnetism since the end of 1851. Tyndall wrote to Faraday on 25 June saying ‘I am now at work, and as usual sadly bewildered – I know nothing of magnetism – the experiments which everybody seems to understand are those which puzzle me most – At least I find the accepted theories of magnetic action no refuge at present’.<sup>223</sup> On 30 June, following a brief response from Faraday on 28 June,<sup>224</sup> Tyndall again wrote describing that during the last week he had been ‘endeavouring to decide a point or two in magnetism and have got myself into a labyrinth of difficulties’.<sup>225</sup> On 27 June Tyndall had applied to the Royal Society for a grant of £50 or £100, and heard the next day from Sabine at Colonel Yorke’s<sup>226</sup> that he had been granted £100 ‘for experimental researches on Heat and Magnetism’.<sup>227</sup> Tyndall wrote to Weber on 3 July and Weber replied on 23 July:

I have sent over to Mr Leyser in Leipzig the order sent to me for the apparatus for the detection of diamagnetic polarity, and have specified exactly the alterations to the instrument that seemed necessary for your purposes, so that he in consequence can deliver to you an instrument completely in accord with your wishes.<sup>228</sup>

Tyndall read critically an article of Plücker’s on 6 August,<sup>229</sup> and was in the laboratory that week, with hints of a breakthrough on 9 August,<sup>230</sup> ‘pondering diamagnetic polarity’ on 10 August,<sup>231</sup> and carrying out some translating for Francis who was short of material for *Philosophical Magazine*. He ordered bismuth on 15 August and continued experimenting that week;<sup>232</sup> by 18 August he had ‘obtained a clearer view of my subject’.<sup>233</sup> On 22 August he had a long discussion with Faraday on diamagnetic polarity, ‘we differed, but differed so cordially, that it was pleasanter than agreement’.<sup>234</sup> He noted the following day that Faraday was at work ‘I believe engaged at the same subject which occupies myself’.<sup>235</sup> Faraday, after a break in his Diary, had noted on 1 August that he was thinking again about the polarity or condition of bismuth when in the magnetic field: ‘is it as I think or as Weber thinks’.<sup>236</sup> On 26 August Tyndall repeated experiments he had previously made on the polarity of bismuth ‘and found them all correct. Von Feilitsch (sic) must have used a rough apparatus when he was unable to reproduce them’.<sup>237</sup> This refers to von Feilitsch’s two papers in *Poggendorff’s Annalen* for 1854,<sup>238</sup> which followed two

<sup>223</sup> Tyndall to Faraday, 25 June 1854 (Letter 2858 in F. A. J. L. James (note 56)).

<sup>224</sup> Faraday to Tyndall, 28 June 1854 (Letter 2859 in F. A. J. L. James (note 56)).

<sup>225</sup> Tyndall to Faraday, 30 June 1854 (Letter 2861 in F. A. J. L. James (note 56)).

<sup>226</sup> Philip Yorke (1799–1874), chemist.

<sup>227</sup> Royal Society Minutes of Council, 29 June 1854, which implies Sabine told him before the decision had been formally made.

<sup>228</sup> Weber to Tyndall, 23 July 1854, R1 MS JT/1/W/13. The letter of 3 July has not been found.

<sup>229</sup> Tyndall, Journal, 6 August 1854.

<sup>230</sup> Tyndall, Journal, 9 August 1854.

<sup>231</sup> Tyndall, Journal, 10 August 1854.

<sup>232</sup> Tyndall, Journal, 15 August 1854.

<sup>233</sup> Tyndall, Journal, 18 August 1854.

<sup>234</sup> Tyndall, Journal 22 August 1854.

<sup>235</sup> Tyndall, Journal 23 August 1854.

<sup>236</sup> Faraday’s Diary, Vol. 6, 288.

<sup>237</sup> Tyndall, Journal, 26 August 1854.

<sup>238</sup> F. C. O. von Feilitsch, ‘Erklärung der diamagnetischen Wirkungsweise durch die Ampère’sche Theorie’, *Annalen der Physik und Chemie* (1854), 92, 366–401 and 536–76.

earlier papers in 1852.<sup>239</sup> Von Feilitzsch sent the latter two papers to Faraday on 11 August.<sup>240</sup> By 2 September, having worked in the laboratory every day apart from Sundays, Tyndall was able to write ‘diamagnetic polarity is secure’, and to start writing his memoir on the subject.<sup>241</sup> He received a letter from von Feilitzsch on 9 September but it appears to have been lost. However, Tyndall refers to such a letter in a letter to Hirst:

I have been engaged in a tolerably exhaustive comparison of magnetism and diamagnetism, and have I trust established a compete [sic] antithesis throughout, and proved beyond a doubt that diamagnetic bodies possess polarity the reverse of that of iron. Weber’s views and mine thus far coincide, but I am firmly convinced that Weber’s hypothesis is false. The experiments please me because they leave no doubt upon the mind and I have strengthened the action, and improved the mode of operation so as to permit me to detect the actions sought in masses of bismuth not a few grains in weight, but in cylinders [14] inches long and an inch in diameter. Feilitsch (sic) has written to me recently and sent me his recent memoirs where he professes to prove that diamagnetic bodies and magnetic ones possess the same polarity. Many of the actions which he failed to obtain I have succeeded in obtaining, and I imagine his experiments cannot stand for an instant in opposition to the evidence which I can bring to bear against them.<sup>242</sup>

### 5.1 British Association Meeting in Liverpool, 1854

Having missed the meeting in Hull in 1853 while in Paris, Tyndall travelled to Liverpool on 20 September for the meeting which took place from 20 to 27 September 1854, for which he was a Secretary to Section A, and took lodgings with Francis. On 21 September, Tyndall gave a paper on the magnetic field.<sup>243</sup> This paper stimulated a discussion in which Whewell, Thomson and Faraday joined,<sup>244</sup> and was reported at length in *Athenaeum*.<sup>245</sup> Tyndall showed the difference between the effect of flat and pointed poles on whether bars of magnetic and diamagnetic substances set axially or equatorially, both between the poles and above or below them, with magnetic substances always behaving the opposite to diamagnetic, and on cubes of different woods. He used the fact that diamagnetic bars set axially between flat poles (though not pointed poles) to deduce that the line joining the centre of two flat poles is the line of minimum force. Faraday remarked that ‘it was conceded on all hands that the explanation was erroneous which Plücker had given’, but in saying that ‘did not mean that as the slightest disparagement to that philosopher’ since ‘it was through the mist of error that the most important discoveries had to be made’. He asked his mathematical friends, Whewell and others, for help in explaining the law of distribution of force in the magnetic field, if it was known. Whewell turned to Thomson, who stated that a completely uniform field, as opposed to an approximation, could only be obtained inside a magnet, but this would be difficult to

<sup>239</sup> F. C. O. von Feilitzsch, ‘Erklärung der diamagnetischen Wirkungsweise durch die Ampère’sche Theorie’, *Annalen der Physik und Chemie* (1852), 87, 206–26 and 427–54.

<sup>240</sup> von Feilitzsch to Faraday, 11 August 1854 (Letter 2874 in F. A. J. L. James (note 56)).

<sup>241</sup> Tyndall, *Journal*, 2 September 1854.

<sup>242</sup> Tyndall to Hirst, undated September 1854, RI MS JT/1/HTYP/359.

<sup>243</sup> J. Tyndall, ‘On some Peculiarities of the Magnetic Field’, *British Association Report, Notes and Abstracts of Miscellaneous Communications to the Sections* (London: Murray, 1854), 16–7.

<sup>244</sup> Tyndall, *Journal*, 21 September 1854.

<sup>245</sup> *Athenaeum*, 30 September 1854, 1174–5.

achieve in practice for strong fields in which an experimenter could also enter, so the approximations Tyndall had used would have to suffice.

On the following day Tyndall gave his paper on the diamagnetic force,<sup>246</sup> the discussion of which was also reported at length in *Athenaeum*,<sup>247</sup> and ‘was surprised to find Thomson backing out of the position he had assumed with regard to diamagnetic polarity’.<sup>248</sup> Indeed, as he wrote to Hirst:

Thompson (sic) completely backed out of the position which he had assumed in Belfast, and completely disowned the interpretation of his views as stated in Faraday’s lecture. Thomson has in fact backed out of almost every position he has assumed in regard to the phenomena of diamagnetism and magnecrystallic action. And he has done so leaving the public to suppose that he had been misconstrued or misapprehended – which tact may possibly increase his reputation with the general public, but in the private opinion of me at least does not add a whit to his nobleness.<sup>249</sup>

This paper presented experiments with bismuth to test whether diamagnetic bodies possess a polarity opposite to iron (Weber) or the same (von Feilitzsch), or have no polarity (Faraday, Thomson). He showed that the repulsive force increases as the square of the strength of the influencing magnet, so it depends on joint action of the magnet and diamagnet, and that the excitement evoked by one pole in a diamagnetic body enables a pole of opposite quality to repel it. He also showed the importance of structure, in that a bar of bismuth with its planes of principal cleavage parallel to its length sets perpendicularly to magnetic lines of force (a ‘normal’ diamagnetic bar), and if transverse sets parallel. The former behaves as the exact opposite of a bar of iron, and there is the same antithesis if the bars are placed in an electrical field inside helical coils. This and similar experiments, according to Tyndall, showed that the diamagnetic force is a polar force the reverse of magnetic polarity. But he stated that this did not prove that the physical theory of Weber is correct, which can be controverted by experiment, by showing that the approximation of diamagnetic bodies has an effect opposite to that deduced from the theory. In discussion, Thomson referred to his paper of 1847,<sup>250</sup> assuming that ‘magnetic force induces upon a fragment of bismuth...a polarity reverse to...a piece of soft iron’, and to his remarks in Belfast which some had taken as opposed to the theory of polarity of bismuth. He explained his conclusion ‘not that bismuth experienced no magnetic polarity, but that the actual magnetization of its substance could not be the reverse of that of soft iron...the definition of an ordinary diamagnetic is, a substance less magnetizable than air’. Nevertheless he agreed completely with Tyndall that the resultant polarity of bismuth, however caused, was the reverse of iron.

## 5.2 *The Bakerian Lecture, 1855 (the ‘Fourth Memoir’)*

Back in London, Tyndall concentrated on his memoir, which he completed on 29 October and handed to Faraday on 30 October.<sup>251</sup> He discussed it at some length with

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<sup>246</sup> J. Tyndall, ‘On the diamagnetic force’, *British Association Report, Notes and Abstracts of Miscellaneous Communications to the Sections* (London: Murray, 1854), 14–6.

<sup>247</sup> *Athenaeum*, 7 October 1854, 1203–4.

<sup>248</sup> Tyndall, *Journal* 22 September 1854.

<sup>249</sup> Tyndall to Hirst, 1 October 1854, RI MS JT/1/T/HTYP/361-363.

<sup>250</sup> W. Thomson (note 12).

<sup>251</sup> Tyndall, *Journal*, 30 October 1854.

Faraday the following day, a conversation which reflected the very different views of Tyndall and Faraday on polarity, force and matter.<sup>252</sup>

According to his journal he handed his paper to the Royal Society on 2 November,<sup>253</sup> though the date of receipt on both the manuscript<sup>254</sup> and published version states 31 October.

Faraday was still pondering polarity and related matters, as he wrote to Tyndall on 11 November:

Reading Matteucci carefully, and also an abstracted translation of Van Rees' paper, is my weighty work; and because of the call it makes on memory, I have now and then to lay them down and cease to the morrow. I think they encourage me to write another paper on lines of force, polarity &c, for I was hardly prepared to find such strong support in the papers of Van Rees and Thomson for the lines as correct representants of the power and its direction, and many old arguments are renewed in my mind by these papers.<sup>255</sup>

On 7 December, at the Royal Society, Tyndall was informed that his paper would be chosen as the Bakerian Lecture, expected to be on 21 December, though it was then postponed to 18 January 1855 to give Fellows more notice,<sup>256</sup> and eventually given on 25 January. But his ideas were still not final, and on 11 December he noted that while pondering by the fire 'I alighted on a proof of diamagnetic polarity which I think must convince everybody'.<sup>257</sup> It clearly did not, since he had 'a hand to hand fight' with Faraday on the subject on 20 December.<sup>258</sup>

On 25 January, Tyndall gave the Bakerian Lecture 'On the Nature of the Force by Which Bodies Are Repelled from the Poles of a Magnet'.<sup>259</sup> It was well received. Lord Wrottesley called it an able lecture. Wheatstone said Lord Ashburton was delighted with it. Lord Harrowby was there, the Astronomer Royal and 'many others of that calibre'.<sup>260</sup> Grove did not see how the arguments could be overcome.<sup>261</sup>

Miller<sup>262</sup> and Thomson<sup>263</sup> refereed this paper for publication in *Philosophical Transactions*. Miller called it 'a large and very important addition to the knowledge of diamagnetism'. Thomson was not so convinced, writing:

...there is so much of important and curious experimental investigation in it...as to fully entitle to a place in the Transactions... Still I think that...Mr Tyndall is frequently contending against an imaginary adversary...Feilitsch (sic) "theory" is founded on a mistake...all Mr Tyndall's experiments and views are in perfect accordance with those indicated by Faraday from the beginning and advocated by

<sup>252</sup> Tyndall, Journal, 31 October 1854. See also note 388.

<sup>253</sup> Tyndall, Journal, 2 November 1854.

<sup>254</sup> The manuscript has small textual differences to the published paper and stops abruptly near the end of p16 '...to the line which united them. The magnet being...', RS PT/50/1.

<sup>255</sup> Faraday to Tyndall, 11 November 1854 (Letter 2921 in F. A. J. L. James (note 56)).

<sup>256</sup> Tyndall, Journal, 12 December 1854.

<sup>257</sup> Tyndall, Journal, 11 December 1854.

<sup>258</sup> Tyndall, Journal, 20 December 1854.

<sup>259</sup> J. Tyndall, 'On the Nature of the Force by Which Bodies Are Repelled from the Poles of a Magnet; to Which is Prefixed, an Account of Some Experiments on Molecular Influences', *Philosophical Transactions of the Royal Society of London* (1855), 145, 1–51. See also RI MS JT 4/5/7.

<sup>260</sup> Tyndall, Journal, 25 January 1855.

<sup>261</sup> Tyndall to Hirst, 29 January 1855, RI MS JT/1/T/592.

<sup>262</sup> Miller, 16 April 1855, RS RR/2/252.

<sup>263</sup> Thomson to Stokes, undated, RS RR/2/253.



myself as early as 1846...The real question is "are the phenomena presented by diamagnetics to be explained by a contrary magnetic action to that of soft iron, or by a less magnetization than that of the medium (air or luminiferous ether) surrounding them".

Thomson wrote that he would wish for some modification to be made 'in the controversial part of the communication' but 'should Mr Tyndall be disposed to make no change, I should advise its publication as it stands'.

In this paper, published as the 'Fourth Memoir' in *Researches on Diamagnetism and Magnecrystalline Action*, Tyndall set out his view of the importance of structure, 'Indeed it may be safely asserted that every force which makes matter its vehicle of transmission must be influenced by the manner in which the particles are grouped together...whether we take the old hypothesis of imponderables or the new, and more philosophic one, of modes of motion', and described in the first part of the paper his experiments on the influence of the molecular structure of wood upon its magnetic deportment. His view on polarity is also stated 'The magnetic force, we know, embraces both attraction and repulsion, thus exhibiting that wonderful dual action which we are accustomed to denote by the term polarity'. Detailed experiments are reported on the movement of bars and spheres of different substances, diamagnetic and paramagnetic, when placed between pointed poles, either directly in line between the poles or above or below them, exploring whether the bars or the crystallographic axes (or axes of compression) of spheres set axially or equatorially. The clear conclusion is that the position taken up by spheres depends on molecular structure, while a further action comes into play with elongated bars, due to the magnetic force, or couple, on the end of the bar, which can overcome the effect of structure. In all cases diamagnetic and paramagnetic substances behave as complete opposites, and Plücker's explanation of a different change in strength of magnetic and diamagnetic force with distance is incorrect. Further experiments are then described with flat poles, which give an approximately uniform magnetic field between them, unlike the pointed poles, with Tyndall showing the field is not entirely uniform but that the straight line which connects the centre of one pole to the other is that of weakest force. Tyndall proceeded to show clearly that diamagnetism is induced, and then turned again to polarity, describing the excitation of diamagnetic bodies to be of a dual nature since the state excited by one pole will prevent the repulsion of a mass by a second opposite pole (which would otherwise repel it on its own). He next described an extensive series of experiments on the effect of electric current and magnet, alone or combined, on magnetic and paramagnetic bars, depending also on their structure ('normal' or 'abnormal' bars), noting also that he had re-affirmed a result which von Feilitzsch had recently disputed.<sup>264</sup> Again, the antithesis between the behaviour of paramagnetic and diamagnetic bars is completely maintained. In the final part of the paper he again dealt with polarity, which was to be the subject of the 'Fifth and Sixth Memoirs' also, reinforcing the concept of 'twoness' of action, with a bar of bismuth like a bar of iron being able to be either attracted or repelled by a magnet depending on its magnetization by a surrounding coil, but always in an opposite manner. He drew the conclusion 'That the diamagnetic force is a polar force, the polarity of diamagnetic bodies being opposed to that of paramagnetic ones under the same conditions of excitement'. But if this is so, Tyndall asked 'how are we to conceive of the *physical mechanism* of this polarity?

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<sup>264</sup> F. C. O. von Feilitzsch (note 238).

According to Coulomb and Poisson it lies in decomposition of the neutral magnetic fluid, but if so how could a north pole excite a north?; for Ampère, the molecular currents would set themselves parallel to and in the same direction as those of the magnet, but that would result in attraction not repulsion, hence perhaps Weber's assumption that diamagnetism is produced by molecular currents not directed but actually excited in bismuth by the magnet, though this requires channels surrounding the molecules of diamagnetic bodies in which the currents can flow without resistance, and one conclusion drawn from his theory is opposed by experimental facts'. So as yet, Tyndall declared 'we know absolutely nothing of the physical causes of magnetic action'. At the end of the paper Tyndall dealt with objections from Matteucci, which he had received via Faraday, and showed at considerable length how the movements of a diamagnetic bar can only be explained on the assumption of diamagnetic polarity. In an endnote in *Researches on Diamagnetism and Magnecrystallic Action*<sup>265</sup> he stated that since his and Weber's experiments had only been made with bismuth, he felt the need to establish the evidence for diamagnetic polarity by using a wider range of substances, which he proceeded to do in the following paper, the 'Fifth Memoir'.

The following evening, 26 January, he gave the paper as a Friday Evening Discourse,<sup>266</sup> writing to Hirst:

I fear I made a slight mistake – I said once that I was compelled to dissent from the views put forward by Faraday in his lecture of the foregoing week. Faraday's own feelings I do not know. He shook hands with me at the conclusion of the lecture, but I know the thing has been commented on by people in the house. If I had known that the matter would have caused such a stir I should probably not have alluded to the difference between us. As it is I am content – I have a clearer appreciation of Faraday's merits than those who would condemn me. I love and reverence the man, but it is with the heart of a freeman who will ever maintain his right to differ from him. The world will yet see that I do not differ from him on insufficient grounds.<sup>267</sup>

The paper was published also in two parts in *Philosophical Magazine* in September and October,<sup>268</sup> with additional commentary.

### 5.3 Exchanges with Faraday and Thomson

On 4 February 1855, in response to Faraday's paper in the February issue of *Philosophical Magazine*,<sup>269</sup> Tyndall wrote to Faraday on the question of whether there is a magnetic medium in space.<sup>270</sup> He published his letter in the March issue and sent copies to his 'foreign friends' as well as to Thomson.<sup>271</sup> He later had it reprinted in *Researches on Diamagnetism and Magnecrystallic Action*. Faraday replied on 14 March, a letter published in the April issue<sup>272</sup> and also reprinted in *Researches on Diamagnetism and*

<sup>265</sup> J. Tyndall (note 81), 153.

<sup>266</sup> J. Tyndall, 'On the Nature of the Force by Which Bodies Are Repelled from the Poles of a Magnet', *Proceedings of the Royal Institution of Great Britain* (1855), 2, 13–6.

<sup>267</sup> Tyndall to Hirst, 29 January 1855, RI MS JT/1/T/592.

<sup>268</sup> J. Tyndall, 'On the Nature of the Force by Which Bodies Are Repelled from the Poles of a Magnet; to Which is Prefixed, to which is prefixed an Account of Some Experiments on Molecular Influences', *Philosophical Magazine* (1855), 10, 153–79 and 257–90.

<sup>269</sup> M. Faraday, 'On some points of magnetic philosophy', *Philosophical Magazine* (1855), 9, 81–113.

<sup>270</sup> Tyndall, Journal, 4 February 1855.

<sup>271</sup> J. Tyndall, 'On the existence of a magnetic medium in space', *Philosophical Magazine* (1855), 9, 205–9.

<sup>272</sup> M. Faraday, 'Magnetic Remarks', *Philosophical Magazine* (1855), 9, 253–5.

*Magnecrystallic Action*. On 9 March Tyndall noted that Thomson had sent him two papers, one for him and one for Faraday, although Thomson's letter to Tyndall as published in *Philosophical Magazine* is dated 12 March.<sup>273</sup> It was also reprinted in *Researches on Diamagnetism and Magnecrystallic Action*. Tyndall described correspondence between Barlow and Airy<sup>274</sup> about Faraday's lecture, Airy apparently writing to Barlow to 'approve the strictness and mathematical conception' of Tyndall's paper, and he also noted that Faraday, concerned that he might have offended Tyndall in remarks made when he was shown experiments with Dove and Sabine, had suggested they should examine the subject together 'to reach the facts of the case'. On 15 March at the Royal Society he heard a paper from Williamson on his letter to Faraday suggesting that 'the facts adduced by Dr Tyndall are not inconsistent with a magnetic medium, but follow naturally from it', arguing that the magnetic medium would be squeezed out by compression, increasing the apparent diamagnetism of a diamagnetic substance.<sup>275</sup> Tyndall had a bad headache and felt unable to discuss it, which he thought may have led people to believe mistakenly that he thought Williamson convincing.<sup>276</sup> He wrote immediately to Thomson in response to a letter from Thomson (which appears to have been lost):

Far be it from me to deny dogmatically the existence of a magnetic medium – I long for more light on the subject, and I have no doubt that the agitation of the question will greatly contribute to advance our knowledge of magnetism...Prof. Williamson of University Coll. has sent a paper to the Royal Society upon the subject of a medium. I heard it read last night, but did not feel able to enter upon the discussion...I hope to be able to send you an account of a great number of facts in connection with the subject of compression as soon as my lectures here are concluded...I have purposely avoided mingling the matter too much with my paper on polarity which is now in the hands of the Royal Society. The desire to keep this latter question as much as possible to itself, and not the want of material, has prevented me from entering more fully upon the Subject of Compression. A short communication on the 'medium' from Mr. Faraday will appear in the next number of the *Phil. Mag.*<sup>277</sup>

He also wrote to Hirst suggesting he might like to combat Williamson by writing a short paper<sup>278</sup> that Tyndall could bring before the Royal Society,<sup>279</sup> sending a note of approval for Hirst's efforts on 3 April.<sup>280</sup> The paper was read and printed in July.<sup>281</sup>

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<sup>273</sup> W. Thomson, 'Observations on the "Magnetic Medium" and on the Effects of Compression', *Philosophical Magazine* (1855), 9, 290–3.

<sup>274</sup> George Airy (1801–1892), astronomer, became Astronomer Royal in 1835 until his retirement in 1881.

<sup>275</sup> A. W. Williamson, 'A Note on the Magnetic Medium', *Proceedings of the Royal Society of London* 7 (1855), 7, 306–8. Alexander Williamson (1824–1904), chemist.

<sup>276</sup> Tyndall, *Journal*, 15 March 1855.

<sup>277</sup> Tyndall to Thomson, 15 March 1855, RI MS JT/1/TYP/5/1538-1539. This term 'medium', with its overtones of the spiritualism that both Faraday and Tyndall abhorred, had different meanings. To Faraday the medium was the lines of force. Tyndall's position is not so clear, although he was a consistent believer in the ether.

<sup>278</sup> T. A. Hirst, 'On the Existence of a Magnetic Medium', *Proceedings of the Royal Society of London* (1854), 7, 448–54.

<sup>279</sup> Tyndall to Hirst, 1 March 1855, RI MS JT/1/T/1007.

<sup>280</sup> Tyndall to Hirst, 3 April 1855, RI MS JT/1/T/596.

<sup>281</sup> Tyndall to Hirst, 26 July 1855, RI MS JT/1/T/609.

On 21 March Tyndall replied to a letter from Thomson, writing very much as an equal compared to his first letters in 1850:

I have read your letter a second time this morning. It appears to me to supply a want in the writings you have hitherto published on the subject of molecular induction in paramagnetic and diamagnetic bodies, on this account if you thought well of it I should be glad to have the portion of the letter which refers to this subject (or the whole letter if you prefer it) published in the next number of the *Philosophical Magazine*.<sup>282</sup>

Thomson replied on 22 March giving permission and describing experiments he had recently carried out with compressed iron filings or small wire pieces in soft wax or dough, when they all set perpendicularly to the lines of force, which he understood were different to those obtained by Tyndall for paramagnetic substances in general.<sup>283</sup>

On 26 April Stokes wrote to Tyndall:

At the last meeting of the Council it was voted, on the recommendation of the referees, that your paper should be printed in the *Transactions*. Both the referees have made remarks in side papers on points here and there in your paper. These I submit to your consideration. ... Thomson in his report seemed to think that you have been contending in part against an imaginary adversary, for with the exception of Von Feilitzsch whose opinion he conceived originated in an obvious mistake, nearly all who had attended to the subject adopted polarity. For my own part (and I conversed with Thomson on the subject almost from the first) I don't think I ever had any other idea about it than that of a polarity (relative or absolute) of a contrary sign to that in a paramagnetic substance. I mention myself merely as a specimen of the public who had paid no particular attention to the subject; but I am certain that was Thomson's and it seems also it was Faraday's view from the first.<sup>284</sup>

Tyndall received the letter and Thomson's remarks as a referee on 1 May. He replied to Stokes on 6 May at considerable length,<sup>285</sup> challenging many of Thomson's comments and interpretations, particularly around his apparent change of position on diamagnetic polarity, but ending 'However all that involves his name in connexion with the question of polarity has been struck out and this makes an end of the discussion'.

On 10 May Tyndall was arranging publication of his paper in *Philosophical Transactions* and making drawings for it, and heard he had been elected along with Stokes and Huxley to the *Philosophical Club*, an exclusive social club set up within the Royal Society in 1847 as part of the internal reform process.<sup>286</sup> On 24 May he dined at the *Philosophical Club* between Huxley and Stokes. He was at the Royal Society afterwards, hearing a paper by Thomson: 'Thomson knows nothing of the divine gift of silence - he is eternally before us - if he continues spurting out in this way I shall begin to think there is nothing great in the man. Indeed I have thought so for some time'.<sup>287</sup>

<sup>282</sup> Tyndall to Thomson, 21 March 1855, RI MS JT/1/TYP/5/1537.

<sup>283</sup> Thomson to Tyndall, 22 March 1855, RI MS JT/1/T/12.

<sup>284</sup> Stokes to Tyndall, 26 April 1855, RI MS JT/1/S/217.

<sup>285</sup> Tyndall to Stokes, 6 May 1855, RI MS JT/1/TYP/4/1462-1466.

<sup>286</sup> Tyndall, *Journal*, 12 May 1855.

<sup>287</sup> Tyndall, *Journal*, 24 May 1855.

#### 5.4 British Association Meeting in Glasgow, 1855

In the week to 31 July Tyndall was working on Weber's experiments on diamagnetic polarity, and 'succeeded in reducing all to certainty'. He obtained precise results with bismuth, remarking 'Weber is right and Feilitsch is wrong',<sup>288</sup> and continued with experiments during the next week, concerned on 6 August that the shellac and resin holding together his bismuth powder might be impure.<sup>289</sup> On 14 August he demonstrated experiments on diamagnetic polarity to De la Rive and Faraday, the latter expressing 'his perfect satisfaction with the experiments'.<sup>290</sup> De la Rive was anxious to see the polar action of a diamagnetic body that was an insulator, and Tyndall demonstrated this to his satisfaction on 17 August with heavy glass, showing it also to Faraday.<sup>291</sup> Tyndall stayed at Queenwood from 18 August working, walking and reading Tennyson until early September, spending much time with Hirst, Debus and Francis, whence he left on 6 September for a walking trip of 5 days in the Lake District with Frankland en route to the British Association Meeting in Glasgow.

Tyndall was now on the Council of the British Association, the Duke of Argyll being President that year, and was again a Secretary to Section A, although with Stevelly absent a heavy share of the duties fell on him.

On Saturday 15 September, Tyndall gave his paper on the polarity of diamagnetic bodies,<sup>292</sup> after papers by James Thomson (brother of William Thomson) and Brewster. The paper referred to his Bakerian Lecture showing that a bismuth bar had the opposite deflection to iron under the same circumstances, though with much less energy. In the latter case the bar was deflected by magnets. Tyndall now showed that magnets could be deflected by bars, overcoming previous objections to Weber's demonstration, and using a magnet produced by Leyser at Leipzig according to Weber's plan. He showed that the effect is not caused by momentary currents of induction, because it is permanent, because there is no deflection with copper and also because it is shown with a glass prism, which is an insulator. Tyndall claimed to have removed the last remaining doubt that diamagnetic bodies under magnetic excitement possess a polarity the reverse of magnetic ones.

The subsequent discussion (not reported in *Athenaeum*),<sup>293</sup> with Kelland in the chair, led to a major altercation with Thomson or at least with Thomson's supporters, as Tyndall related to Hirst:

I have been told indeed that my communication was unequalled. Indeed I got through it entirely to my own satisfaction. Whewell, Sir David Brewster, Sir William (Rowan) Hamilton & numbers of that stamp were present and they all cheered me at the conclusion. Brewster took me by the hand and said "Oh what an admirable lecture you have given us" introducing me at the same time to Sir William Hamilton. Thomson was beside me as I spoke taking notes. I said that I was glad to see him there and hoped that we should finally come to an understanding with each other. He arose and replied. Whewell apparently desirous of throwing the weight of his great authority in favour of Thomson and thus bearing me down rose and spoke. Had he not done so all would have passed off well. But

<sup>288</sup> Tyndall, *Journal*, 1 August 1855.

<sup>289</sup> Tyndall, *Journal*, 6 August 1855.

<sup>290</sup> Tyndall, *Journal*, 14 August 1855.

<sup>291</sup> Tyndall, *Journal*, 17 August 1855.

<sup>292</sup> J. Tyndall, 'Experimental Demonstration of the Polarity of Diamagnetic Bodies', *British Association Report, Notes and Abstracts of Miscellaneous Communications to the Sections* (London: Murray, 1855), 22–3.

<sup>293</sup> *Athenaeum*, 29 September 1855, 1120–1.

I with my sensitive head rose and replied to both him and Thomson speaking I think with considerable power & effect upon the audience. I went just a little too far and for this I feel dissatisfied with myself. I said something to the effect that Thomson had acted a safe part in suffering himself to be guided by Faraday. One of Thomson's friends upon the platform grumbled; Whewell glowered upon me like an enraged lion – indeed Thomson's friends en masse seemed to be offended with me. The great body of the audience was however upon my side and this perhaps made matters worse. When I was aware of the discontent I stopped and said that if I had uttered any thing unbecoming the calmness of scientific discussion, I begged to apologize for it. The Chairman said that every body present felt that there was no need of an apology: the audience applauded loudly. Still the impression which the whole affair has left upon me is by no means a pleasant one. Thomson indeed invited me to dine with him and meet Liebig afterwards: but it will take some time to banish the remembrance of the thing from the minds of all concerned...I am anxious to return & should perhaps have been happier had I not come at all.<sup>294</sup>

Tyndall did not attend a British Association Meeting again until 1858; he had indeed been ambivalent about going to Glasgow since Faraday would not be there, writing on 5 September 'I am not quite sure that I act right in going and your presence there would be a kind of quieter to my conscience'.<sup>295</sup>

Although it does not appear in the published *British Association Report*, Tyndall gave a further paper 'On the comparison of magnetic induction, and calorific conduction in crystalline bodies'.<sup>296</sup> He showed that the line of best calorific conduction in gypsum is that of least magnetic induction (unlike calcareous spar, as found by M Seuermont) so there is not a unity of agency, a finding highly relevant to his emerging thoughts about the relationship of structure to properties.

Tyndall, concerned at the impact of his impulsive remarks about Thomson, wrote to Faraday soon after his return from Glasgow to which Faraday replied on 6 October in a letter full of sensible advice, advising him not to jump to conclusions on people's motives and to be more diplomatic, gently chiding him 'it is better to be blind to the results of partizanship (sic) and quick to see goodwill'.<sup>297</sup> He also mentioned that he was carrying out experiments on magnecrystals and the effects of heat on them.

Tyndall spent several weeks at Queenwood, in a reflective mood after Glasgow. Nevertheless he was content with his achievements, including 'one beautiful problem I believe I have solved and that is the question of slate cleavage'.<sup>298</sup>

### 5.5 Weber, Thomson and the 'Fifth and Sixth Memoirs'

Weber wrote a long letter to Tyndall on 25 September,<sup>299</sup> in response to Tyndall sending him on 3 September a copy of the Bakerian Lecture and a letter giving a sketch of

<sup>294</sup> Tyndall to Hirst, 17 September 1855, RI MS JT/1/T/611.

<sup>295</sup> Tyndall to Faraday, 5 September 1855 (Letter 3023 in F. A. J. L. James (note 56)). Tyndall had sparred with Thomson from their first meeting at the British Association in Edinburgh in 1850, and subsequently in Belfast in 1852, in Liverpool in 1854 and in Glasgow in 1855. Tyndall was particularly sharp in the Glasgow encounter, although Thomson did not respond to the provocation. It seems to have taken some time for a perhaps jealous Tyndall to acknowledge the younger Thomson's true capabilities.

<sup>296</sup> *Athenaeum*, 6 October 1855, 1157.

<sup>297</sup> Faraday to Tyndall, 6 October 1855 (Letter 3027 in F. A. J. L. James (note 56)).

<sup>298</sup> Tyndall, *Journal*, 27 October 1855.

<sup>299</sup> Weber to Tyndall, 25 September 1855, R1 MS JT/1/W/14.

some experiments executed with the instrument Weber had devised for him. Tyndall had the letter published in *Philosophical Magazine* in December,<sup>300</sup> and reprinted in *Researches on Diamagnetism and Magnecrystallic Action*, to which he added his response,<sup>301</sup> also in *Researches on Diamagnetism and Magnecrystallic Action*. In the letter, Weber congratulated Tyndall for his care in separating the fact of diamagnetic polarity from the theory and emphasised his own theory which assumed diamagnetic polarity and Ampère's theory of molecular currents, with Poisson's theory of two magnetic fluids equally admissible. He stated that the excitation of such molecular currents is a necessary conclusion from Ampère's theory, which Ampère himself had not been able to make, because the laws of the voltaic induction that Faraday discovered were not yet known to him. Then he tackled Tyndall's remark that 'M. Weber is obliged to suppose that the molecules of diamagnetic bodies are surrounded by channels, in which the induced molecular currents, once excited, continue to flow without resistance', pointing out that this assumption was already contained in Ampère's theory, since 'a permanent molecular current without such a channel involves a manifest contradiction, according to the law of Ohm'. Weber agreed with Tyndall that this may seem extremely artificial but stressed that he had made no new assumptions. He hoped that in time that mathematics might overcome the limitation to linear currents and the concept of channel-like current beds. 'All our molecular theories are still very artificial: I for my part take less offence at the artificiality of Ampère's theory than at other artificialities of our molecular theories, because in Ampère's theory the basis of the artificiality lies clear and plainly before our eyes, thus opening the outlook and the way to finally eliminate the same'. In a footnote in *Researches on Diamagnetism and Magnecrystallic Action* in 1870, Tyndall heartily endorsed Weber's view of this need for clarity in the description of the physical model.<sup>302</sup>

Tyndall's response, welcoming Weber's points, picked up only on the question of whether the diamagnetism of two bismuth particles lying in the line of magnetisation is diminished by their reciprocal action (as Weber claimed) rather than increased (as Tyndall had claimed in the Bakerian Lecture). Weber had stated that the effect was in any case very weak and might be affected by Tyndall's compression of the bismuth. Experiment, at this point, was unable to decide the facts.

By 3 November, and over the next couple of weeks, Tyndall was writing a portion of his next memoir,<sup>303</sup> presumably the 'Fifth Memoir', published in *Philosophical Transactions*,<sup>304</sup> and also much later, in September 1856, in *Philosophical Magazine*,<sup>305</sup> after the 'Sixth Memoir' had appeared there in February.<sup>306</sup> His disagreement with Faraday continued, as in his letter to Hirst:

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<sup>300</sup> W. Weber, 'On the theory of diamagnetism. Letter from Professor Weber to Prof. Tyndall', *Philosophical Magazine* (1855), 10, 407–9.

<sup>301</sup> J. Tyndall, 'Note on Weber's Paper "On the theory of diamagnetism. Letter from Professor Weber to Prof. Tyndall"', *Philosophical Magazine* (1855), 10, 409–10.

<sup>302</sup> J. Tyndall (note 81), 228.

<sup>303</sup> Tyndall, *Journal*, 3 November 1855.

<sup>304</sup> J. Tyndall, 'Further Researches on the Polarity of the Diamagnetic Force', *Philosophical Transactions of the Royal Society of London* (1856), 146, 237–59.

<sup>305</sup> J. Tyndall, 'Further Researches on the Polarity of the Diamagnetic Force', *Philosophical Magazine* (1856), 12, 161–84.

<sup>306</sup> J. Tyndall, 'On the relation of diamagnetic polarity to magnecrystallic action', *Philosophical Magazine* (1856), 11, 125–37.

It is amusing to see how many write to Faraday asking him what the lines of force are. He bewilders even men of eminence, for the v[er]y fact of his making these lines of force the medium of his theoretic sight and his hav[ing] done so much with them convinces the generality of people that they are the final cause of magnetic phenomena...I heard Biot once say that he could not understand Faraday, & if you look for exact knowledge in his theories you will be disappointed - flashes of wonderful insight you meet here and there. But he has no exact knowledge himself, and in conversation with him he readily confesses this. In my next paper I shall have to say something of these lines of force.<sup>307</sup>

On 9 and 10 November Tyndall was attempting without success to repeat an experiment of Weber's which Faraday had also not been able to repeat. He gave Faraday a draft of his paper on 17 November,<sup>308</sup> and was working on compression experiments during the week of 19 November.<sup>309</sup>

Tyndall wrote to Thomson on 20 November offering assistance for Thomson's forthcoming Friday Evening Discourse and asking for some clarification over Thomson's theory of the magnetic field:

From your proof that the intensity of a magnetic field increases towards the centre of curvature (Phil Mag April 1855) I should infer that if the lines of force were parallel straight lines the intensity at right angles to them would be constant. I have a steel horse shoe magnet here in which the lines of force run sensibly parallel from leg to leg almost from top to bottom, yet such a field is not one of constant intensity, for the force increases [from] the bend towards the poles. When we examine such a field closely we even find that the lines of force are slightly curved, the centre of the curvature being towards the bend, and not towards the poles. According to this the intensity increases as we recede from the centre of curvature...I have just finished a paper on polarity which I purpose sending to the Royal Society in a few days, I am now entangled in compression experiments.<sup>310</sup>

As he finished his memoir - his journal states he wrote 6 pages on 27 November,<sup>311</sup> which may have been the Sixth Memoir since the Fifth was received by the Royal Society on that date - he wrote again to Thomson 'On Reciprocal Molecular Induction',<sup>312</sup> a letter that was published in *Philosophical Magazine* for December,<sup>313</sup> and reprinted in *Researches on Diamagnetism and Magnecrystalline Action*. Thomson replied on 24 December,<sup>314</sup> in a letter which Tyndall had published in *Philosophical Magazine* for January 1856<sup>315</sup> and also reprinted in *Researches on Diamagnetism and Magnecrystalline Action*. At the root of this was an argument stemming from the correspondence with Weber, about whether the effect of bismuth particles on each other was predictable, in that

<sup>307</sup> Tyndall to Hirst, 5 November 1855, RI MS JT/1/T/935.

<sup>308</sup> Tyndall, Journal, 17 November 1855.

<sup>309</sup> Tyndall, Journal, 19 November 1855.

<sup>310</sup> Tyndall to Thomson, 20 November 1855, RI MS JT/1/TYP/5/1544-1545.

<sup>311</sup> Tyndall, Journal, 27 November 1855.

<sup>312</sup> Tyndall to Thomson 26 November 1855.

<sup>313</sup> J. Tyndall, 'Letter to Prof. W Thomson On Reciprocal Molecular Induction', *Philosophical Magazine* (1855), 10, 422-3.

<sup>314</sup> Thomson to Tyndall, 24 December 1855.

<sup>315</sup> W. Thomson, 'Prof. W. Thomson on the Reciprocal Action of Diamagnetic Particles' Induction', *Philosophical Magazine* (1856), 11, 66-7.



it would impair their ‘diamagnetisation’, but was not experimentally verifiable as Thomson claimed. Tyndall replied to this letter:

The people at Red Lion Court [i.e. Taylor & Francis] thoughtlessly forwarded your letter to me without opening it, and thus lost the post which you saved. I took it back immediately and urged Francis strongly to publish it. This however he declares to be impossible this month. He may change his mind. I think the letter will pleasantly close the discussion, and if I have anything else to write about which I expect to have – I think the most satisfactory plan would be to write privately at first, & afterwards we could publish or not publish just as we thought necessary. I have something to say with regard to the law of movement from stronger to weaker places of force & vice versa in the magnetic field; but at present I am too busy to take the matter up.<sup>316</sup>

The exchange illustrates Thomson’s view of a consistent treatment of all magnetic and diamagnetic phenomena, conceptually and mathematically, while Tyndall was concerned to have a clearer physical picture.

A long letter to Grove of 5 December reveals both Tyndall’s perception of constraints at the Royal Institution and the significance of his latest findings.<sup>317</sup> The letter was stimulated by a request from Grove, acting for the Royal Society Government Grant Committee, for Tyndall to justify his expenditure. He argued that the grant was for him personally, not the Institution, to give him the freedom to respond quickly which the management of the Royal Institution might not allow, and especially now when the issue of diamagnetic polarity was still disputed even after his Bakerian Lecture: ‘The question was one which lies at the basis of all enquiries into diamagnetism’. So he had spent £20 of the grant on an instrument, which he offered to return to the Royal Society after the work if requested, which has ‘removed the last trace of doubt and brought complete conviction to the mind of our highest existing authority in these matters, as to the reality of the principle sought to be established. From private continental letters I also infer the necessity of the enquiry. It annihilates the objections contained in these letters, and thus establishes a scientific principle of the highest importance upon unquestionable foundations’. Tyndall also queried the view that his application ought to be more definite in the statement of objects in view, but that that was unreasonable since he was ‘working at the fringes of science’ where the outcomes and directions could not be predicted. He bridled at what he took to be slurs on his character, writing that if his record and character were not deemed sufficient he ‘would beg to withdraw from all participation in the government grant for the promotion of science’. During this period, on 15 December Tyndall read Riess’s reply to Faraday,<sup>318</sup> which he left with Francis on 17 December, and the correspondence ‘On the Action of Non-conducting Bodies in Electric Induction’ was published in *Philosophical Magazine* in January 1856.<sup>319</sup>

On 16 December Tyndall noted that Matteucci had written to Faraday and Grove about the experiments described in the Bakerian Lecture, denying their accuracy and being unable to obtain Tyndall’s results, but had now sent an ‘amenda honorable’.<sup>320</sup>

<sup>316</sup> Tyndall to Thomson, 27 December 1855, RI MS JT/1/TYP/5/1549.

<sup>317</sup> Tyndall to Grove, 5 December 1855, RI MS/Gr/3a/152.

<sup>318</sup> Tyndall, Journal, 15 December 1855.

<sup>319</sup> M. Faraday and P. Riess, ‘On the Action of Non-conducting Bodies in Electric Induction’, *Philosophical Magazine* (1856), 11, 1–17.

<sup>320</sup> Matteucci to Tyndall, 3 December 1855, RI MS JT/1/M/58.

retracting his remarks and asking him to pass them on to Faraday and Grove.<sup>321</sup> On 9 March 1856 he noted that Reich had been asked by Matteucci to repeat his experiments with his torsion balance, which he had done and corroborated them.<sup>322</sup>

On 20 December, after dinner at the Philosophical Club, Stokes read the introduction to his paper and he was asked by the President to explain the experiments himself, which he did to the apparent satisfaction of everyone.<sup>323</sup> The Fifth Memoir, entitled 'Further Researches on the Polarity of the Diamagnetic Force',<sup>324</sup> deals with criticisms, particularly from Matteucci and von Feilitzsch, that the previous experiments of Tyndall and Weber, which they claimed to show diamagnetic polarity, might instead be due to induced currents and should be repeated with insulators. Indeed von Feilitzsch did this and was unable to detect any effect. The paper was refereed by Joule<sup>325</sup> and Thomson.<sup>326</sup> Joule commented 'Besides verifying the result of Weber he has proved the production of diamagnetic polarity', thereby putting himself firmly in the non-Faraday camp. Thomson merely commented 'it is well suited to publication in the Transactions'. At this point, again, Tyndall's ability as an experimentalist showed itself. Using equipment designed by Weber he made a series of extremely sensitive experiments with copper, antimony and with insulators, using glass and six other materials, and found deflections to be permanent rather than temporary, which would be the case if there were a momentary induced current. In his terms this showed the polarity of a diamagnetic body as an insulator in addition to that of conductors. In addition he diplomatically noted that his equipment was sensitive enough to generate clear deflections, unlike the equipment earlier used by Faraday, who as a result had stated that he could 'find no experimental evidence to support the hypothetical view of diamagnetic polarity'. Tyndall ended the paper claiming that all objections to diamagnetic polarity had now fallen away, placing it 'among the most firmly established truths of science...The cause of science is more truly served, even by the denial of what may be a truth, than by the indolent acceptance of it on insufficient grounds. Such denials drive us to a deeper communion with Nature, and, as in the present instance, compel us through severe and laborious enquiry to strive after certainty, instead of resting satisfied, as we are prone to do, with mere probable conjecture'.

Tyndall's final and relatively short paper, the Sixth Memoir, 'On the Relation of Diamagnetic Polarity to Magnecrystallic Action',<sup>327</sup> followed the Fifth quite quickly and was in fact published in *Philosophical Magazine* in February 1856 before the Fifth, in September 1856, though the latter had been published earlier in *Philosophical Transactions*. In this paper, primarily addressing Faraday's statement that the magne-crystallic force is neither attraction nor repulsion, he gave a clear explanation of the complex effects of attraction, repulsion and the effect of the resulting moments, or couples, in explaining the direction of movement of spheres and bars of substances in different magnetic circumstances. In particular, he showed that a recession from the pole can be due to differential attraction and repulsion, i.e. to a 'polar' force, 'The most complicated effects of magne-crystallic action are thus reduced to mechanical problems of

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<sup>321</sup> Tyndall, Journal, 16 December 1855.

<sup>322</sup> Tyndall, Journal, 9 March 1856.

<sup>323</sup> Tyndall, Journal 20 December 1855.

<sup>324</sup> J. Tyndall (note 304).

<sup>325</sup> Joule to the Committee of Papers, 9 February 1856, RS RR/3/265.

<sup>326</sup> Thomson to Weld, 20 February 1856, RS RR/3/266.

<sup>327</sup> J. Tyndall (note 306).

extreme simplicity; and inasmuch as these actions are perfectly inexplicable except on the assumption of diamagnetic polarity, they add their evidence in favour of this polarity to that already furnished in abundance'. The memoir ends: 'The whole domain of magnetic-crystallic is thus transformed from a region of mechanical enigmas to one in which our knowledge is as clear and secure as it is regarding the most elementary phenomena of magnetic action'.

Throughout this time Tyndall demonstrated his skills as a systematic experimentalist which are more widely known through the subsequent work on radiant heat and spontaneous generation. His particular contribution to diamagnetism was to establish the physical facts unequivocally through experiment. His style was very much that of the systematist, meticulously controlling variables. In this he differed from Faraday, whose style might be described as dialogic; exploring and conversing with Nature. Only two experimental notebooks survive from this period and they are relatively sketchy and untidy compared to those of later years.<sup>328</sup> In this he follows the pattern of Faraday, whose recording likewise improved over time. Yet the papers themselves, and especially the later Memoirs, demonstrate the clarity and skill with which he prepared and pursued his investigations.

Airy wrote to Tyndall on 8 March, after Tyndall had sent him two papers (probably the Fifth and Sixth Memoirs), congratulating Tyndall on reducing diamagnetism to a 'mechanical and calculable' form, since 'It has been a matter of no small grief to me to find that till a comparatively late time, a totally different theory, a theory of extreme vagueness, has been advocated by the highest authority';<sup>329</sup> Airy here meaning Faraday's field theory. Airy had perhaps an over-exaggerated view of Tyndall's capability as a mathematician, writing in 1857 'You are so completely master in everything that relates to interference of undulations that I very much wish I could enlist you to thoroughly study the geometrical and algebraical theory of this phenomena of depolarization...Our physicists in general and our optical experimenters in particular (always excepting Stokes, the prince of mathematicians) have been such wretched mathematicians that these subjects are sealed to them: I wish greatly that you would enter into them'.<sup>330</sup>

Plücker was still agitating, writing to Wheatstone in French, decidedly unhappy at Tyndall's behaviour as he saw it; Wheatstone read part of the letter to Tyndall on 30 March.<sup>331</sup> Tyndall resolved not to respond unless 'he pushes too far'.<sup>332</sup> Plücker wrote to Faraday, after gap of over a year, on 24 March 1856<sup>333</sup> complaining that he had been misrepresented by Tyndall (in the Bakerian Lecture) on his understanding of the forces involved and had already made the point Tyndall was making in his 1849 paper,<sup>334</sup> and had now reported some new results in *Cosmos*.<sup>335</sup> He looked forward to publishing a definitive account of his work, which eventually appeared in 1858.<sup>336</sup> Plücker was elected

<sup>328</sup> RI MS JT3/45.

<sup>329</sup> Tyndall, Journal, 9 March 1856.

<sup>330</sup> Airy to Tyndall, 15 August 1857, MS.RGO.6/378:ff.515r-517r.

<sup>331</sup> Tyndall, Journal, 1 April 1856.

<sup>332</sup> Tyndall, Journal, 1 April 1856.

<sup>333</sup> Plücker to Faraday 24 March 1856 (Letter 3109 in F. A. J. L. James *The Correspondence of Michael Faraday, Volume 5, 1855–1860* (London, 2008).

<sup>334</sup> J. Plücker, 'Ueber die Fessel'sche Wellenmaschine, den neueren Boutigny'schen Versuch und das Ergebnis fortgesetzter Beobachtungen in Betreff des Verhaltens krystallisierten Substanzen gegen den Magnetismus', *Annalen der Physik und Chemie* (1849), 78, 421–31.

<sup>335</sup> J. Plücker, 'Action du magnetisme sur les axes des cristaux', *Cosmos* (1855), 7, 391–6.

<sup>336</sup> J. Plücker, 'On the Magnetic Induction of Crystals', *Philosophical Transactions of the Royal Society of London* (1858), 148, 543–87.

a foreign member of the Royal Society on 21 June,<sup>337</sup> particularly championed by Wheatstone,<sup>338</sup> who told Magnus in Paris<sup>339</sup> that he ‘became a member of the Royal Society only as a mathematician’.<sup>340</sup> Faraday replied in an emollient manner on 8 April<sup>341</sup> and Plücker’s eventual response on 2 January 1857 declared that he had no animosity towards Tyndall but intended to submit a paper to the Royal Society to make his position clear, though in the meantime he had met Tyndall, on 17 September 1856 in Vienna. Tyndall was introduced by Grailich. It was a cold meeting: ‘I was prepared to meet the man with a frank friendliness, but there was a sleek cold politeness in his glance which informed me that a similar feeling did not exist on his part. I stretched out my hand which he accepted, but so frigidly that the value of the acceptance was negative’.<sup>342</sup> Though he wrote to Hirst on 2 October ‘In Vienna I made many acquaintances and had every reason to be gratified by the cordial welcome and good treatment we received. I met Plücker there. He was polite and cold, and I reconciled myself to the fact. I saw him afterwards at Ettingshausen and I thought he seemed to relent as Ettingshausen and myself conversed together’.<sup>343</sup> Matteucci was also in touch, writing on 13 September that he had been ‘gathering all my experiments on the diamagnetism that I carried on for the last three years, almost without an interruption’.<sup>344</sup>

Plücker sent his paper to Faraday on 14 March 1857,<sup>345</sup> who sent it on to Miller, the Foreign Secretary at the Royal Society, with no endorsement.<sup>346</sup> The paper was refereed by Thomson<sup>347</sup> and Stokes<sup>348</sup> - Faraday declined to referee it claiming ‘it is mathematical in character and in that respect far beyond my powers of judgement’<sup>349</sup> - and approved for publication on 10 December 1857. Both referees saw the paper as over-elaborate, and both queried its reference to Poisson’s theory. Thomson commented that it was:

deserving of publication inasmuch as it shows the views regarding magnecrystallic action to which one of the chief investigators in this branch of science has been brought after much careful investigation...the theoretical part of the paper is not in my opinion of the same value as that in which the experimental illustrations and researches are described...all Plücker’s testings are *illustrations*, but not establishing anything previously certain.

Stokes suggested the Secretary should write to see if Plücker ‘which is not probable’ will volunteer to adopt the other method expressing the mathematical conclusions, ‘but it is what we cannot ask him to do’. Essentially both felt they had to publish the paper but that it added nothing new (Plücker was now a foreign member of the Royal Society). In an endnote to this paper, which is an extremely detailed and mathematical account of

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<sup>337</sup> RS MS EC/1855/17.

<sup>338</sup> Tyndall, Journal, 1 April 1856.

<sup>339</sup> Magnus to Tyndall, 20 June 1856, RI MS JT/1/M/19.

<sup>340</sup> Indeed the nomination states ‘distinguished for his investigations in geometry, and for his researches in various branches of physical science’. Tyndall did not sign the nomination paper.

<sup>341</sup> Faraday to Plücker, 8 April 1856 (Letter 3116 in F. A. J. L. James (note 333)).

<sup>342</sup> J. Tyndall, Journal, 17 September 1856.

<sup>343</sup> Tyndall to Hirst, 2 October 1856, RI MS JT/1/HTYP/470-471a.

<sup>344</sup> Matteucci to Tyndall 13 September 1856, RI MS JT/1/M/59.

<sup>345</sup> Plücker to Faraday, 14 March 1857 (Letter 3251 in F. A. J. L. James (note 333)).

<sup>346</sup> Faraday to Miller 1857, 23 March 1857 (Letter 3257 in F. A. J. L. James (note 333)).

<sup>347</sup> RS RR/3/222.

<sup>348</sup> RS RR/3/224.

<sup>349</sup> Faraday to Weld, 25 July 1857, RS RR/3/223.

Plücker's researches, it is surprising but illuminating that Plücker states that he did not know of Thomson's (by now well-established) theory when he wrote the paper.

Meanwhile Tyndall complained to Faraday of Plücker's behaviour in a letter of 24 March,<sup>350</sup> as did Plücker again of Tyndall in a letter of 7 July 1857.<sup>351</sup> The long-suffering Faraday replied to Plücker that only they could sort it out between them.<sup>352</sup> Tyndall was not the only one who sparred with Plücker; De la Rive wrote to Faraday on 10 May 1858 on a disputed matter of precedence 'it is not the first time that Mr. Plücker leaves something to be desired in his dealings with other savants; it is well known in Berlin and ask Tyndall and yourself',<sup>353</sup> although he immediately regretted saying it in a following letter.<sup>354</sup> Faraday replied that he was not surprised by the comment 'but scientific morality is not altogether satisfactory'.<sup>355</sup> However, in 1858 fences were mended as Hofmann, who had invited them both, brought them together in London: 'he laid aside his coldness and we talked together for a long time in a very friendly manner'.<sup>356</sup> After this, letters from Plücker show a different attitude to Tyndall; Plücker wrote Tyndall a very pleasant letter on 7 March 1859 in response to a letter from Tyndall of 14 February, congratulating him on his glacier work and giving news of his latest researches.<sup>357</sup> Tantalisingly, there is also an undated note in the Taylor & Francis archive, torn off from a piece of light blue writing paper, stating in Tyndall's handwriting 'Plücker has proved himself magnanimous!'

## 6. Tyndall's contribution to our understanding of magnetism, polarity, matter and force

Tyndall's work on diamagnetism came to an end in 1856; no more significant progress in his terms could be made with the mathematical and theoretical understanding of the time, until the application of vector theory. Starting around 1856 almost as Tyndall finished his work, it was James Clerk Maxwell who made the major theoretical contribution, developing Faraday's field theory and Thomson's initial modelling of it,<sup>358</sup> culminating in his great *Treatise* of 1873.

Tyndall had intended to collect and publish all his work on experimental physics in 1869, but an accident sustained by slipping over onto a block of granite above Bel Alp on 29 August prevented him from doing so, and required six weeks of recuperation with Lady Peel in Geneva.<sup>359</sup> Instead he offered his papers on diamagnetism, excluding his work on heat and other subjects, as being 'tolerably complete in themselves',<sup>360</sup> but with no explanation of why he had chosen them. His journal simply states 'While the experiments are going on in the laboratory I correct my memoirs. I have already gone

<sup>350</sup> Tyndall to Faraday, 24 March 1857 (Letter 3259 in F. A. J. L. James (note 333)).

<sup>351</sup> Plücker to Faraday, 7 July 1857 (Letter 3310 in F. A. J. L. James (note 333)).

<sup>352</sup> Faraday to Plücker, 11 July 1857 (Letter 3317 in F. A. J. L. James (note 333)).

<sup>353</sup> De la Rive to Faraday, 10 May 1858 (Letter 3435 in F. A. J. L. James (note 333)).

<sup>354</sup> De la Rive to Faraday, 19 May 1858 (Letter 3441 in F. A. J. L. James (note 333)).

<sup>355</sup> Faraday to De la Rive, 24 May 1858 (Letter 3445 in F. A. J. L. James (note 333)).

<sup>356</sup> J. Tyndall, *Journal*, 10 April 1858.

<sup>357</sup> Plücker to Tyndall, 7 March 1859, RI MS JT/1/P/128.

<sup>358</sup> J. C. Maxwell, 'On Faraday's lines of force', *Transactions of the Cambridge Philosophical Society* (1856), 10, 27–83.

<sup>359</sup> Tyndall to Hirst, 30 August 1869, RI MS JT/1/HTYP/553-555.

<sup>360</sup> J. Tyndall (note 81), xi.

through those on diamagnetism. For I wish to bring out a volume of them'.<sup>361</sup> This volume was published in 1870 as *Researches on Diamagnetism and Magnecrystallic Action*.<sup>362</sup> It is just possible that some initial impetus was given to this by Tyndall's publication in 1868 of 'Faraday as a Discoverer'. As he wrote to Helmholtz on 8 January:

I sent Tait the Memoir on Faraday, and he gave himself the trouble of reading it all through and of giving me his opinion upon it. At pages 24, 29, & 39 he refers to Thomson's researches and thinks that they ought to be dwelt upon. Now you are Thomson's intimate friend, and I am anxious to do all just honour to Thomson: would you point out the places where you think his labours might be referred to? ... I am anxious not only to do justice to Thomson, but to express in the most liberal manner my admiration of his intellect.<sup>363</sup>

In addition to the six main papers, or 'Memoirs' published between 1850 and 1856, Tyndall added new commentary in several places. At the end of the 'First Memoir' he noted that Plücker had approached the views expressed more closely in his paper of 1849 than previously recognised,<sup>364</sup> but this paper was unpublished until Tyndall had it published in Taylor's *Scientific Memoirs* in 1853, even though it still contained assertions which had been disproved. He gave more substantive commentary at the end of the 'Second Memoir' on Poisson's prediction of magne-crystallic action,<sup>365</sup> remarking that he believed his experiments were secure but he would like to 'review the molecular theory of the whole subject, and examine still further the remarkable variations of magnetic capacity produced by mechanical strains and pressures'.<sup>366</sup> Again, his emphasis on understanding underlying structure and mechanical effect is evident, and he referred to his conclusion that 'the state of the ether, or of the molecules, which produces great differences as regards calorific conduction, may produce no sensible difference as regards magnetic induction'.<sup>367</sup> This desire for a physical image is illustrated in a contemporary letter to Helmholtz 'I wish you or Clerk Maxwell, or somebody with the requisite force of imagination would give the world some physical image of an electric current. Without some such image there is a certain emptiness in that remarkable paper of Maxwell's on the Electromagnetic Field'.<sup>368</sup>

<sup>361</sup> Tyndall, Journal, 7 November 1868.

<sup>362</sup> J. Tyndall (note 81).

<sup>363</sup> Tyndall to Helmholtz, 13 January 1868, RI MS JT/1/T/485; this letter also talks about 'burying the hatchet' with Tait. In 1857 Tyndall had written to Maxwell about his mathematical treatment of Faraday's theory and implying that it was not the only way of looking at the phenomena: 'I never doubted the possibility of giving Faraday's notions a mathematical form, and you would probably be one of the last to deny the possibility of a totally different imagery by which the phenomena might be represented'. (Tyndall to Maxwell, 7 November 1857, CU S.Add.7655/II/13 and Add.7655/II/221).

<sup>364</sup> J. Tyndall (note 81), 37.

<sup>365</sup> J. Tyndall (note 81), 66–71.

<sup>366</sup> J. Tyndall (note 81), 68.

<sup>367</sup> J. Tyndall (note 81), 71.

<sup>368</sup> Tyndall to Helmholtz, 15 March 1870, RI MS JT/5/15b. This is presumably a reference to Maxwell's 1865 paper 'A Dynamical Theory of the Electromagnetic Field' (see note 391). Although Maxwell used physical analogies to guide his work, in particular the strange rotating molecular vortices with interposed electric particles, his eventual description was primarily mathematical. The evolution of Maxwell's ideas in electromagnetism from 1855 to 1873 is described by D. M. Siegel, "Maxwell's Contributions to Electricity and Magnetism", in *James Clerk Maxwell: Perspectives on his Life and Work*, edited by R. Flood, M. McCartney and A. Whitaker (Oxford: OUP, 2014). For the significance of Helmholtz's papers from 1870 onwards for placing Maxwell's ideas within the corpuscular approach of the Continentals see A. E. Woodruff, 'The Contributions of Hermann von Helmholtz to Electrodynamics', *ISIS* (1968), 59, 300–11.

Weber wrote to Tyndall on 18 March 1870, in a letter which encapsulates the different ways of visualising the phenomena:

I take the same interest as you in the beautiful and penetrating researches of Maxwell, and link it particularly to the *electrodynamic theory of light* that Maxwell has developed. The proof of a *medium*, through whose *molecular forces* the effects could be determined precisely, which electric currents and electric charges exert on each other at a distance, would be very interesting in itself. The assumption of such a medium which really acts like this I take as just as admissible as the assumption of forces acting at a distance, from which these effects have until now been determined. If indeed it were further shown that from the assumption of this medium the effects of light at a distance could also be determined at the same time, the alternative between the two assumptions would in my opinion be decided...As far as the medium itself is concerned, and the determination of the molecular forces effective within it, the agreement of the analytical expressions with the results of Faraday's experimental researches gives considerable confidence, even if we lack, as it appears to me, clear insight into the inner relationship between molecular forces and properties, which fundamentally is the case in general, where research into the inner molecular constitution of matter has led so far. I would like to think that the transfer of the laws of action at a distance to molecular interactions, as C. Neumann has attempted, could lead further...Through this I only want to express the interest which I take in Maxwell's researches, in the hope of agreeing with you in the most important aspects, and in particular in that the law of action at a distance, which has also been an object of my researches, would no more lose its significance in science than the theory of magnetism, if in the end it should be accepted that these forces acting at a distance, as well as magnetic fluids, are only ideal concepts, but in a wider view would be equivalent to the real ones'.<sup>369</sup>

Thomson wrote on 9 June 1870 to thank Tyndall for his 'beautiful volume of diamagnetism' and mentioning that he had had similar plans for more than two years for his own electrical papers, which might now appear before Christmas, asking 'I thought of including our magnetic correspondence and I presume you will have no objection that so much should be common to the two volumes'.<sup>370</sup> This work, including the correspondence, appeared in 1872 as *Reprints of papers on electrostatics and magnetism*.<sup>371</sup> Another hatchet seems truly to have been buried.

Tyndall produced a second edition of *Researches on Diamagnetism and Magnecrystalline Action* in 1888, in which he reprinted the six Memoirs but much less of the additional material; 7 items compared to 21 in the first edition, including removal of the items relating to correspondence with Thomson (by now Sir William, who would become Lord Kelvin just before Tyndall's death).<sup>372</sup> In a new preface to this edition, Tyndall gave no quarter either to Faraday ('his views were assuredly strange') or to

<sup>369</sup> Weber to Tyndall, 18 March 1870, R1 MS JT/1/W/17.

<sup>370</sup> Thomson to Tyndall, 9 June 1870, R1 MS JT/1/T/17.

<sup>371</sup> W. Thomson, *Reprints of papers on electrostatics and magnetism* (London: Macmillan, 1872).

<sup>372</sup> 1000 copies of the first edition were printed (RU MS 1393 A10, p195) and seem to have been sold by 1888. A further 1000 copies were printed in 1888 but 500 copies were 'wasted' in June 1904 and 150 in May 1910. 20 copies were delivered to Mrs Tyndall in 1930 (RU MS 1393 A13, p1678). Tyndall received £120 for the first edition. His more popular books were much more remunerative; *Heat a Mode of Motion* sold c16,000 copies in England, netting Tyndall around £2200 (RU MS 1393 A7, A10, A14).

Plücker ('His first striking generalisation, indeed, was corrected by himself; but his second statement of the law of magne-crystallic action was as faulty as the first. Pasteur truly describes the art of experiment as beset with difficulty and danger. Plücker, when he passed suddenly from mathematics to physics, was not sufficiently aware of this'). Both, by this time, had been dead for 20 years. So, towards the end of his life, and following all the developments of Thomson and Maxwell, Tyndall still saw the best interpretation of the phenomena of diamagnetism in his terms of polarity leading to attraction and repulsion of couples, rather than Faraday's field theory.

### 6.1 Polarity, matter and force

A significant point at issue between Tyndall, Faraday and others was the concept of diamagnetic polarity. This came down to a matter of deciding what was meant by polarity and can be resolved in one sense in terms of the geometry of magnetism, now best described in terms of vector algebra. This was not available to Tyndall when he did his work, though it is developed from the concept, introduced by William Hamilton in 1843 of quaternions, mathematical entities formed of a scalar and the three components of a vector, which he never attempted to master later and which Thomson much disliked. The controversy was linked to the more important question of whether diamagnetism is better represented in terms of 'action at a distance' between magnetic poles or in terms of a force field that fills all space.

Taking polarity first, it is not always clear what was meant by the term, and there were different understandings of it.<sup>373</sup> Even Faraday wrote at one point in late 1851 'I dare not venture to say that I recollect all I have read, or even all the conclusions I myself have at different times come to'.<sup>374</sup> It may appear that Faraday briefly flirted with the concept in his first 1846 paper, writing 'These two modes [magnetic and diamagnetic] are in the same general antithetical relation to each other as positive and negative in electricity, or as north and southness in polarity...'.<sup>375</sup> This was seized on by Tyndall, Plücker and others as evidence of Faraday's support for the concept,<sup>376</sup> yet earlier in the same paper Faraday had argued 'Here therefore we have magnetic repulsion without polarity, i.e. without reference to a particular pole of the magnet, for either pole will repel the substance, and both poles will repel it at once',<sup>377</sup> and this is the line he maintained. In electrostatics it is said that the forces of attraction or repulsion between two charges are polar; there is a straight line joining two charges or poles, about which there is cylindrical electrical symmetry. The OED defines polarity in this and similar contexts as 'The quality of exhibiting opposite or contrasted properties or powers', and cites as its first example that notable wordsmith William Whewell who, in 1840, gave the definition of 'opposite properties in opposite directions'. More pertinent to magnetism perhaps is the OED citation from Tyndall's *Notes on a course of seven lectures on electrical phenomena and theories*, 'Two opposite kinds of magnetism may be supposed to be concentrated at the

<sup>373</sup> I am grateful to Professor Sir John Rowlinson, for several ideas in this paragraph.

<sup>374</sup> M. Faraday (note 147), 49 (§3155).

<sup>375</sup> M. Faraday (note 3), 53 (§2419).

<sup>376</sup> Tyndall even wrote, in 1868, describing his own experiments 'the most complete antithesis was established between magnetism and diamagnetism. This antithesis embraced the concept of polarity, - the theory of reversed polarity, first propounded by Faraday, being proved to be true'. J. Tyndall, *Faraday as a Discoverer* (London: Longmans, 1868), 105.

<sup>377</sup> M. Faraday (note 3), 26 (§2274).



two ends. In this *doubleness* of the magnetic force consists what is called *magnetic polarity*.<sup>378</sup> Maxwell observed that the ‘opposition of properties in opposite directions constitutes the polarity of the element of space’.<sup>379</sup>

Tyndall believed he had established beyond doubt that diamagnetism was polar in his terms, but this cannot be disentangled from more fundamental concepts of matter, forces and fields.

Tyndall saw the structure of matter at the molecular level as critical to the mediation of force. Faraday, by contrast, saw force and the field as primary. In the ‘First Memoir’ in 1850 Tyndall had revealed his model of underlying structure, with plates of material alternating with unfilled spaces (‘expansion and contraction by heat and cold compel us to assume that the particles of matter do not in general touch each other’) through which the magnetic force might preferentially be directed. Indeed, ‘anything that affects the mechanical arrangement of the particles will affect...the line of elective polarity...’. So, at the molecular level substances are not in contact, and the channels between may differentially allow magnetic or other forces to be exerted. In Faraday’s terms, though, the lines of force represented something physically real, with continuous action understood in terms of forces filling space. Faraday explained the use of the term ‘contiguous’: ‘The word contiguous is perhaps not the best that might have been used here and elsewhere; for as particles do not touch each other it is not strictly correct...By contiguous particles I mean those which are next’.<sup>380</sup> Faraday built on the concept of an atom as a point with ‘an atmosphere of force grouped around it’.<sup>381</sup> In time the stress-field throughout space became fundamental; the field was not to be explained in terms of matter, matter was rather a particular modification of the field.<sup>382</sup>

Sugiyama describes Tyndall’s model of the constitution of materials and the importance of the aggregation of small parts into a mass with different proximity in different directions, therefore producing an ‘elective polarity’ of the mass; it was the molecular arrangement which was crucial. Thomson, by contrast, imagined small magnetic elements each of which had anisotropy to produce that in a whole mass.<sup>383</sup> For Tyndall, molecular interactions provide the causal links between macroscopic phenomena and underlying mechanisms; the idea of material molecularity enables him to make sense of his mental images.<sup>384</sup> The idea of molecular explanations is illustrated, at the time he was carrying out his work on diamagnetism, in his 1853 paper ‘On Molecular Influences. Part I. Transmission of Heat through Organic Structures’,<sup>385</sup> in which he suggests that differences between various categories of solids are due to

<sup>378</sup> J. Tyndall, *Notes on a course of seven lectures on electrical phenomena and theories* (London: Longmans, 1870), 6 (§31).

<sup>379</sup> J.C. Maxwell, *Treatise on Electricity and Magnetism* (OUP, 1873) §11; Harman’s edition of Maxwell’s Letters, vol. 1, 210–1.

<sup>380</sup> §1164n (December 1838) M. Faraday, *Experimental Researches in Electricity* (London: 1839), vol. 1, 362.

<sup>381</sup> M. Faraday, *Experimental Researches in Electricity* (London: 1844), vol. 2, p290, originally in ‘A speculation touching Electric Conduction and the Nature of Matter’, *Philosophical Magazine* (1844), 24, 136. A further discussion of the atomic-molecular model for the structure of matter contrasted with Faraday’s field approach is given in G. Boato and N. Moro (note 36).

<sup>382</sup> M. B. Hesse, *Forces and fields: the concept of action at a distance in the history of physics* (London: Nelson, 1961), 210.

<sup>383</sup> S. Sugiyama, ‘The significance of the particulate conception of matter in John Tyndall’s physical researches’, *Historia scientiarum* (1992), 2, 119–38.

<sup>384</sup> M. Yamalidou, ‘John Tyndall, the Rhetorician of Molecularity. Part One. Crossing the Boundary Towards the Invisible’, *Notes and Records of the Royal Society of London* (1999), 53, 231–42.

<sup>385</sup> J. Tyndall (note 166).

differences in their respective states of aggregation. Whatever the actual structures might be, their differences are posited to explain the differential transmission of heat or of magnetic forces in different directions related to underlying but unobservable structure; unobservable at least until the end of the 19<sup>th</sup> century. From the outset of his experiments on diamagnetism, using cubes, discs, thin bars and reconstituted materials, squeezed in particular directions, Tyndall was exploring the molecular constitution and arrangements of substances underlying their overall mass.<sup>386</sup> Indeed, Tyndall would have enjoyed a series of papers published by Oxley between 1914 and 1921 on ‘The Influence of Molecular Constitution and Temperature on Magnetic Susceptibility’, summarised in 1921,<sup>387</sup> which, through a model of molecules as complex diamagnets containing rotating electrons, thoroughly vindicated Tyndall’s ideas of the ‘line of elective polarity’ in relation to cleavage planes (with the direction of closest packing of molecules parallel to the principal cleavage), and supported his concept of reciprocal magnetic induction in quantitative terms, which Thomson had claimed was not possible.

A journal entry of Tyndall’s describing a conversation with Faraday in October 1854 is instructive:

He (Faraday) does not deny the polarity of diamagnetic bodies but could not accept the experiment of Weber’s as proving it... He did not coincide with the idea expressed in one passage of the memoir that force could not act upon force. He would not say that it could but he was not quite clear that it could not. I said that with me the conception of force necessitated the conception of matter. “Then would you call the ether matter?” he said. “Undoubtedly” I replied “as truly matter as the floor on which we stand, why one of the proofs of its existence is that it possesses the power of retarding a comet in its path.” He said he must think on the subject, but this remark showed what curious views he entertained as to the nature of matter and force.<sup>388</sup>

Faraday’s position on the ether, with respect to this argument, is discussed by Gooding.<sup>389</sup> Tyndall was a firm believer in the ether, seemingly throughout his life. In a note in 1870 he stressed how Faraday had connected the force of magnetism with the luminiferous ether (although it is doubtful if Faraday himself would have seen it like this), through his discovery of the rotation of polarised light by a magnet, and the importance of this understanding developed through the work of Thomson and Maxwell.<sup>390</sup>

Faraday by contrast had developed a field theory, which was put into mathematical expression by Thomson and Maxwell. Broadly speaking the physicists fell into two groups, those who thought that diamagnetism exhibited polarity and accepted ‘action at a distance’ as the origin of electric and magnetic effects, and those who did not accept polarity and chose field theory over ‘action at a distance’. There seems to be no necessary connection between ‘action at a distance’ and ‘polarity’ but there was natural affinity between the ideas. Plücker, Weber and von Feilitzsch were clearly in the first group of

<sup>386</sup> M. Yamalidou (note 384).

<sup>387</sup> A. E. Oxley, ‘Magnetism and Atomic Structure’, *Proceedings of the Royal Society of London* (1921), 98, 264–74.

<sup>388</sup> Tyndall, *Journal*, 31 October 1854. Later, on 19 January 1855, Tyndall noted ‘I think he deceives himself by attributing an objective existence to his mental images’.

<sup>389</sup> D. Gooding, ‘Faraday, Thomson, and the magnetic field’, *British Journal of the History of Science* (1980), 13, 91–120.

<sup>390</sup> J. Tyndall (note 81), 183.

physicists with Tyndall, as apparently was Airy from his letter to Tyndall of 8 March 1856. Airy, as an astronomer, could perhaps recognise a good action at a distance model, even if the distances involved in crystals were very small. Yet Tyndall hedged his bets to some extent, referring approvingly to Faraday's 'contiguous particles' in 1850 and was later effusive about Maxwell's approach in his 1865 paper, in which Maxwell endeavoured, through the use of an 'aetherial medium', 'to explain the action between distant bodies without assuming the existence of forces capable of acting directly at sensible distances'.<sup>391</sup> Faraday was not a believer in diamagnetic polarity or action at a distance, writing in 1849 'Finally, I am obliged to say that I can find no experimental evidence to support the hypothetical view of diamagnetic polarity'.<sup>392</sup> His lines of force he thought of as an entity that permeated all space. Thomson and later Maxwell<sup>393</sup> were in the second group of physicists with Faraday. Thomson exploited the analogies between fluid flow, heat flow and electricity. He often followed Fourier in supposing that all apparent action at a distance was in fact action between unspecified 'contiguous particles', a device invoked by those who did not accept 'action at a distance' but could not propose a better model, and indeed a device which Tyndall seemed to accept too. Maxwell explained his ideas in a Friday Evening Discourse at the Royal Institution on 21 February 1873,<sup>394</sup> pointing out to the action at a distance adherents that there is no such thing as complete contiguity; a space always intervenes between the bodies which act on each other; 'And as for those who introduce aetherial, or other media...without any direct evidence of their existence...or clear understanding of how the media do their work...the less these men talk about the philosophical scruples about admitting action at a distance the better'. Maxwell explained that for Faraday, unlike Cavendish, Coulomb and Poisson (who 'never doubted that the action took place at a distance') and for whom the mathematics of Poisson and Ampère was not accessible, lines of force have a continuous existence in space and time with a tension along the lines of force and pressure in all directions at right angles; so this is action at a distance like that of tension of ropes or pressure of rods, even in a vacuum. In this way we can 'resolve several kinds of action at a distance into actions between contiguous parts of a continuous substance'. Faraday, Thomson and Maxwell, unlike Tyndall, all had strong religious beliefs, and Gooding links the teleology and economy inherent in Faraday's interpretation to those beliefs.<sup>395</sup> In this discussion of polarity there are also resonances of the German tradition of *Naturphilosophie*, to which Tyndall was exposed, with its dialectical concept of polarity. In England the influential William Whewell, who had encouraged Faraday to coin words such as 'anode', 'cathode' and 'diamagnetic', was a particular proponent of the concept of polarity and was concerned that Faraday was moving away from it; he came to London

<sup>391</sup> J. C. Maxwell, 'A Dynamical Theory of the Electromagnetic Field', *Philosophical Transactions of the Royal Society of London* (1865), 155, 459–512.

<sup>392</sup> M. Faraday (note 75), 183 (§2693). See also D. Gooding, 'Final steps of field theory: Faraday's study of magnetic phenomena, 1845–1850', *Historical Studies in the Physical Sciences* (1981), 11, 231–75 (note 60).

<sup>393</sup> With some reservations, since Maxwell was noted also for his contribution to the kinetic theory of gases, a field that implicitly uses the concept of intermolecular forces acting at a distance. See his Friday Evening Discourse of 26 February 1863: J. C. Maxwell, 'On action at a distance', *Proceedings of the Royal Institution of Great Britain* (1873), 7, 44–54.

<sup>394</sup> J. C. Maxwell (note 393).

<sup>395</sup> D. Gooding, 'Empiricism in Practice: teleology, economy and observation in Faraday's Physics', *ISIS* (1982), 73, 46–67.

from Cambridge specifically to lecture at the Royal Institution on ‘The Idea of Polarity’ and to seek to place Faraday’s work in that context.<sup>396</sup>

After Tyndall’s experiments, it was not the facts that were in dispute but their interpretation. Faraday wrote to Matteucci on 2 November 1855 to say ‘I differ from Tyndall in phrases, but when I talk with him I do not find that we differ in facts. The phrase *polarity* in its present undefined state is a great mystifier’.<sup>397</sup> He continued ‘All Tyndall’s results are to me simple consequences of the tendency of paramagnetic bodies to go from weaker to stronger places of action, and of diamagnetic bodies to go from stronger to weaker places of action, combined with the true polarity or direction of the lines of force in the places of action’. Faraday saw magnetic conductivity as relative, with diamagnetics having a lower conductivity than space and magnetics a higher, an assumption on which Thomson’s first mathematical theory of diamagnetism was based.<sup>398</sup> So one could say that for Faraday, polarity lay in the field, charge being the polar strain of the medium, with properties relational not absolute, and for Tyndall it lay in the matter in the field, a property of material particles. For Faraday, ferromagnetics define the true polarity or direction of lines of force: other substances merely conduct this polarity.<sup>399</sup> In a note reflecting on this correspondence in 1870, Tyndall declared ‘I think it probable that as regards diamagnetic polarity, Faraday and myself were looking at two different things’,<sup>400</sup> Tyndall concentrating on ‘doubleness of action’ and Faraday on his lines of magnetic force, but to which he never gave a mechanical form that Tyndall needed and sought. Faraday also had the argument from the early results that whereas a magnet (polar) would always set in one sense in a magnetic field, a diamagnet could set either way round. Writing in 1896, Allen stated that ‘The difficulty Tyndall experienced in accepting Faraday’s views as to diamagnetism, is accounted for by the fact that he was thinking in terms of the fluid theory, while Faraday was considering the magnetic polarization in the diamagnetic substance’.<sup>401</sup> At the end of the ‘Third Memoir’ in 1851, contrasting the ‘magnetic fluids’ of Poisson with the ‘lines of force’ of Faraday, Tyndall claimed that Reich’s experiments, showing ‘that the matter evoked by one pole will *not* be repelled by an unlike pole, compels us to assume the existence of *two kinds* of matter, and this, if I understand the term aright, is polarity’.<sup>402</sup> This appears to be evidence for a belief of Tyndall in a type of two-fluid theory, but by the time he gave his Bakerian Lecture in early 1855 he was writing ‘whether we take the old hypothesis of imponderables or the new, and more philosophic one, of modes of motion’.<sup>403</sup> In April 1861, lecturing to primary school teachers at the South Kensington Museum, Tyndall was explicit that magnetic fluids should be regarded ‘as a symbol merely’,<sup>404</sup> in other words as an heuristic device. Later still, in

<sup>396</sup> See S. Schaffer, ‘The History and Geography of the Intellectual World: Whewell’s Politics of Language’ in *William Whewell: A Composite Portrait*, edited by M. Fisch and S. Schaffer (Oxford: 1991).

<sup>397</sup> He had made a similar statement in a paper of 20 December 1854 (note 269), 85, §3307.

<sup>398</sup> As Gooding as described, Faraday argued the space must conduct because it subdivides the class of material conductors into para- and diamagnetics. Empty space, the “zero” in Thomson’s formulation, must be analogous to matter in at least one respect, conductivity. Space must conduct lines without affecting them in any way. Polarity can exist in space as a property of the lines of force rather than a property of material particles. See D. Gooding, ‘Experiment and the Making of Meaning’ *Science and Philosophy* (Dordrecht: Kluwer, 1990), vol. 5, 267–8, 269.

<sup>399</sup> D. Gooding (note 60).

<sup>400</sup> J. Tyndall (note 81), 183.

<sup>401</sup> H. N. Allen, ‘The Graphical Representation of Magnetic Theories’, *The Physical Review* (1896), 3, 470–7.

<sup>402</sup> J. Tyndall (note 142).

<sup>403</sup> J. Tyndall (note 24).

<sup>404</sup> J. Tyndall, ‘Elementary Magnetism. A Lecture to Schoolmasters’, *Fragments of Science* (London: Longmans, 6<sup>th</sup> ed. 1879), 409.

1868, Tyndall wrote a revealing section in his book *Faraday as a Discoverer*, in which he used the idea of fluids as a ‘provisional conception’ to help visualise the phenomenon of electromagnetic induction.<sup>405</sup> This led on to a restatement of his belief in the ether as the medium through which the transformation took place. We can take this as significant since Tyndall had the excerpt published in *Researches on Diamagnetism and Magnecrystalline Action*,<sup>406</sup> making specific and enthusiastic reference to Maxwell’s paper of 1865.<sup>407</sup>

In the case of polarity the position was revealed when the phenomena were described more accurately in terms of vector analysis. The question of whether diamagnetism is or is not polar becomes a matter of words. It certainly has directional properties which can be described best in terms of axial or pseudovectors, and their products, but it differs from the simpler directional properties of a pair of electric charges. If the word ‘polarity’ is to be restricted to the reversal of effects by a change of orientation of 180 degrees, then diamagnetism is not polar. The differences of opinion in the period 1840 to 1880 can only really be resolved by the deeper understanding of the geometry of the interactions of electric and magnetic fields provided by the vector analysis of the 1880s onwards.<sup>408</sup>

The conflict over action at a distance came down to which view is more useful for handling the problem in hand. As early as 1850 Thomson had shown that Faraday’s lines of force could be reconciled with the inverse square law for the interaction between electric charges.<sup>409</sup> Today the Faraday-Maxwell force field is the weapon of choice in handling macroscopic problems of electrodynamics, but ‘action at a distance’ comes more naturally to the astronomers.

In a sense both Faraday and Tyndall were right – it was not a matter of either/or but a matter of convenience of interpretation and the ways in which they sought to understand the world. Their models were self-consistent and complementary ways of explaining and modelling the observed phenomena, the facts of which they agreed. Both could be expressed mathematically, although not by either Faraday or Tyndall, and it was only with the later use of vector theory that Tyndall’s could be treated in this way.

One can envisage a historical thought experiment in which Tyndall’s clarification of the facts of the phenomena took place at the time in 1848-1850 during which Plücker’s incorrect deductions led the case for the defence. Then there would have been a much stronger argument for the Ampère/Weber/Plücker/Tyndall approach at a time when Faraday was firming up his concepts. Had Tyndall also possessed a ‘Thomson’ to develop the mathematical modelling based on vectors, which Thomson disliked, the approaches would have been much more competitive. Indeed, although field theory holds explanatory and predictive sway today, many aspects of the Ampèrian approach remain, especially following the identification of the electron and its charge by J. J. Thompson in 1897. Diamagnetism is explained in current textbooks in terms of the induced magnetic

<sup>405</sup> J. Tyndall (note 376), 139–44

<sup>406</sup> J. Tyndall (note 81), 280–3.

<sup>407</sup> J. C. Maxwell (note 391).

<sup>408</sup> Paragraph largely taken from a private communication from Professor Sir John Rowlinson.

<sup>409</sup> Thomson absorbed his physics particularly from the Fourier/Fresnel/Cauchy school, avoiding hypotheses, rather than the Laplace/Poisson school which based observational physics on an underlying hypothetical molecular theory. Thomson’s definition in 1851 remains important: Any space at every point of which there is a finite magnetic force is called a ‘field of magnetic force’. Thomson ‘is attempting to formulate a definition of the magnetic field which would be acceptable to Faraday, to ether theory, to the positive tradition of Fourier, and even, to some extent, to the action at a distance tradition’. See ch. 7 of R. Flood, M. McCartney and A. Whitaker (Eds), *Kelvin. Life, Labours, and Legacy* (Oxford: OUP, 2008).

moment, opposing the external magnetic field, resulting from an electron with charge moving round an orbit, with its magnetic moment perpendicular to the orbital plane. As a property of matter, diamagnetism is shown by atoms with electrons having ‘paired’ spins, so there is no resulting magnetic moment (which also means that all substances are diamagnetic, and some may be paramagnetic or ferromagnetic as well). In this sense a corpuscular microphysics replaced the Maxwellian approach at the end of the 19<sup>th</sup> Century and became the ‘classical’ electromagnetism. There is a duality of modelling and interpretation here not dissimilar to that between the particle and wave interpretations for visualising quantum phenomena. Tyndall’s particle-based view remains valid, but Faraday’s field theory was more productive at the time.

Tyndall has been all but written out of the history of magnetism and it is time for a reassessment.

Soon after Tyndall’s death Lord Rayleigh gave a Friday Evening Discourse on his scientific achievements in which he ignored the magnetic work completely, moving straight from the water drop experiments to the glacier studies.<sup>410</sup> Oliver Lodge’s obituary, published in *Encyclopaedia Britannica* in 1902-3,<sup>411</sup> was little short of damning of his work in this area; ‘His early magnetic investigations, for instance (1850-1855) sadly lack the definiteness which was possible at their date’, remarking particularly that he ‘does not express it as a mathematician would’, a criticism that indeed could equally be applied to Faraday, if not more so.

William Bragg succeeded Dewar as Director of the Royal Institution Laboratory in 1923 and a few years later he did try to do justice to Tyndall’s work in this field, concentrating naturally on its crystallographic implications, in a Friday Evening Discourse on 21 January 1927.<sup>412</sup> Bragg very much took Faraday’s side: ‘When Faraday’s conceptions prevailed it became clear that Tyndall’s interpretation...must have been incorrect’. His collected account...never became a link in the chain of argument’, and he claimed that ultimately Tyndall’s experiments to show pressure produced proximity and proximity produced equivalence of magne-crystallic action must be held to have failed.

When Tyndall started his work on the diamagnetism of crystals it was an active field of research. This importance is reflected in Chrystal’s devotion to magnecrystallic action of three pages out of 57 in his treatment of magnetism in the 9<sup>th</sup> edition (1883) of *Encyclopaedia Britannica*,<sup>413</sup> and it is a fine summary. Chrystal comments ‘If we regard [Tyndall’s] theory merely as a way of representing the facts of observation...it is far inferior to the theory of Faraday and Thomson.... Regarded as an attempt to penetrate a little further into the relation between molecular structure and magnetic properties, it is of great interest and importance...’. The 11<sup>th</sup> (1911) was a completely new work in which *Magnetism* was treated at length by Bidwell. In a 32-page article he gave two short paragraphs to magnecrystallic phenomena, referencing Faraday, Thomson and Maxwell but not Tyndall, and concluding rather lamely: ‘The phenomena may therefore be exceedingly complicated’.<sup>414</sup>

<sup>410</sup> Lord Rayleigh (J. W. Strutt), ‘The scientific work of John Tyndall’, *Proceedings of the Royal Institution of Great Britain* (1894), 14, 216–24.

<sup>411</sup> O. Lodge, ‘Tyndall, John (1820–1893)’, *Encyclopaedia Britannica* (1903), vol. 10, 517–21.

<sup>412</sup> W. H. Bragg, ‘Tyndall’s experiments on magne-crystallic action’, *Proceedings of the Royal Institution of Great Britain* (1927), 25, 161–84.

<sup>413</sup> G. Chrystal, ‘Magnecrystallic Action’, *Encyclopaedia Britannica* (1883), vol. 15, 264–7.

<sup>414</sup> S. Bidwell, ‘Magnetism’, *Encyclopaedia Britannica* (1911), vol. 11, 321–53.

Essentially Tyndall cleared up what now appears as an almost forgotten footnote in the history of electricity and magnetism,<sup>415</sup> but it is in reality much more than a footnote and his experimental dissection of the complexities was masterful. As Gooding has observed, Faraday's theory, constructed between August 1848 and August 1850, could not have been developed without his further experimental study of the behaviour of crystalline bodies in the field.<sup>416</sup> Tyndall did what no-one else seemed capable of doing at the time; subduing the challenges of this weak phenomenon with all its geometric and structural complexity with complete consistency, on the basis of a model of diamagnetic polarity and the effect of magnetic forces acting in couples. In the process, Tyndall established his scientific reputation, gaining his Fellowship of the Royal Society and his position at the Royal Institution substantially on the basis of this work. In turn his ability to engage a Society audience, shown at the Royal Institution in his very first Discourse on this topic, led on to the introductions and building of relationships that took him into the Salons and set him on a public path. It all started with diamagnetism.

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<sup>415</sup> Tyndall's name is not even mentioned in the index to the 848 pages of J.D. Jackson's *Classical Electrodynamics* (2nd edition, 1975).

<sup>416</sup> D. Gooding (note 398), 259.