**John Michell (1724-93): Father of Magnetometry?**

**by Jim Grozier**

## 1. Introduction

In 1910 Sir Edmund Whittaker said of Michell:

"The century which elapsed between the death of Newton and the scientific activity of Green [in other words, 1727-1828] was the darkest in the history of [Cambridge] University … In the entire period the only natural philosopher of distinction who lived and taught at Cambridge was Michell; and for some reason which at this distance of time it is difficult to understand fully, Michell's researches seem to have attracted little or no attention among his collegiate contemporaries and successors, who silently acquiesced when his discoveries were attributed to others, and allowed his name to perish entirely from Cambridge tradition" [Whittaker, p153]

Michell's scientific interests included what we now call seismology (in fact he has been called the father of that discipline), geology, and astronomy. He also built the apparatus with which his friend Henry Cavendish measured the gravitational force between two bodies, after Michell's death (the experiment became known as "the Cavendish Experiment" – just one example of the misattribution mentioned by Whittaker). He has acquired more recent significance through his postulation of what are now called black holes, having calculated that light could not escape from a star of the same density as the Sun and a radius 497 times larger [Michell 1783], the modern significance of this having been first pointed out by Simon Schaffer [1979].

However, in this essay I will concentrate on his work on magnetism. Michell has been described as the discoverer of the inverse-square law of the force between magnetic poles, thus placing the study of magnetism onto a quantitative footing, as magnetometry. I will try to find out what he did and didn't do, what he owed to his precursors and to the technological resources available, and what others owed to him.

## 2. Magnetometry before 1750

Whittaker refers to a "suggestion" of an inverse-square law between magnetic poles as far back as 1450, by Cardinal Nicholas of Cusa [Whittaker p56n]. I have not been able to obtain more information on this (the reference Whittaker gives is in Latin) but if it is true, de Cusa's law does not seem to have been generally adopted, since two centuries later we find Newton stating that "the power of magnetism … decreases not in the duplicate, but almost in the triplicate proportion of the distance, as nearly as I could judge from some rude observations" [Newton 1968, bk III, prop. VI, cor. 5] – in other words it is more like an inverse-cube law than an inverse-square. The problem here is the inability to distinguish between forces between magnets and those between *magnetic poles.*

In the modern magnetic paradigm there is no such thing as a magnetic pole, and the properties of magnets are seen as arising from the motion of electric charges inside them. However, according to the view that dominated from the early 19$^{th}$ century until perhaps the mid-20$^{th}$, magnets can indeed be described in terms of poles, but they always come in pairs. Thus it is, in a sense, more proper to consider forces between magnets than those between poles. But the former are complex, consisting generally of net forces (proportional to the inverse fourth power of the distance) and couples (inverse-cube), and also depending on the respective orientations of the magnets and their poles. The description of the force on a single pole is a good example of the sort of idealisation which characterises modern physics – not strictly true on its own, but able under certain conditions to predict a quantity within experimental uncertainty. Nineteenth-century theories of magnetism developed by Gauss and others incorporated an inverse-square law for poles, which was able to correctly predict the forces and couples exerted by magnets on one another. Thus, if de Cusa really did suggest an inverse-square law for poles, he and Newton could both have been right. Unfortunately we cannot tell, from Newton's terminology, whether "the power of magnetism" refers to magnets or poles.

Robert Palter, in his paper *Early Measurements of Magnetic Force*, tells us that it was Hooke who, in 1666, made the "rude observations" that Newton relied upon to deduce his inverse-cube relation, and that he did this with a lodestone under the pan of a balance and "presumably … a piece of iron placed on the pan" [Palter p545]. Thus we do not know the dimensions or orientation of the lodestone but it seems what he was measuring was the net force exerted by the lodestone on the iron, which can be regarded as an induced magnet due to the presence of the lodestone. In 1687, Edmond Halley observed the deflection of a needle in the presense of "a loadstone [*sic*] set at varying distance from the needle" [*ibid.*]. This configuration of course measures the couple. Halley however did not deduce a law of force.

Between the time of Halley and that of Michell there were eight further attempts to measure the magnetic force, by Taylor and Hauksbee (1712-21), Whiston (1719), Stephens (1722), Musschenbroek (1725-45), Desaguliers (1734), Helsham (1739), Calandrini (1742) and Martin (1747) [Fara p126]. Most of these used either the "balance" method of Hooke or the "deflection" method of Halley. Helsham, for instance, says "Let a loadstone be suspended at one end of a balance, and counterpoised by weights at the other; let a flat piece of iron be placed beneath it …" [Helsham p19]. He goes on to specify the weights needed to balance the pan for two different separations of the lodestone and iron; these distances are in the ratio 2:1, and the weights in the ratio 1:4, so he concludes an inverse-square law (but of course from a description of his apparatus we would expect an inverse-cube).

### 3. Michell's Properties of Magnets

Michell tells us in the introduction to his *Treatise of Artificial Magnets* (1750) that "I proposed at first to publish, with this Method of making Magnets, a Theory also of Magnetism, which I endeavoured to establish by Experiments … but finding that this would swell these sheets to too great a bulk, I chose to defer that part till some other opportunity", adding that the book was "principally … intended [for] Artificers and Seamen" [Michell 1750, p2]. Thus the bulk of the book consists of instructions for making artificial magnets. He does however list seven "Properties of Magnetical Bodies", but again appeals to "brevity" as a reason for failing to "give the proofs of them" [*ibid.* p16], once more deferring this to "some farther opportunity".

Invoking brevity also in the current document, I will summarise these seven properties in my own words:

Michell's "Properties of Magnetical Bodies"

*1. Every magnet contains two poles, called North and South; opposite poles of different magnets attract each other, while similar poles repel.*

*2. The attraction and repulsion between poles is not influenced by any body placed between them, except in the case of what we would nowadays call magnetisable materials, though here Michell says that the influence of such materials is only apparent.*

*3. The attractive and repulsive forces between poles at a given separation do not depend on direction.*

*4. The attractive and repulsive forces are equal (presumably here Michell means at the same separation).*

*5. The poles are located "at a little distance" from the ends of the magnet.*

*6. The inverse-square law: the attractive and repulsive forces decrease with the separation of the poles.*

*7. The last property, in Michell's words, says that "Magnets lift Iron, in an increased ratio of their Strength for touching, &c, and probably very nearly in a duplicate ratio". I will discuss the meaning of this below.*

The first property was not new; Whittaker says that it was "of course adopted from Gilbert", although in fact the analogy between the poles of a magnet and those of the earth – and the adoption of this word for magnets – goes back to Pierre de Maricourt in 1269, more than 300 years before the publication of Gilbert's *De Magnete* in 1600, as Whittaker himself points out [Whittaker p33-34].

The third property (which we would probably not subscribe to nowadays, since a completely isolated, and thus spherically symmetrical, pole is not now regarded as a realistic possibility) was a step forward – Michell tells us that "the want of knowing this property" had led his predecessors, including Gilbert, "into considerable mistakes". The fourth property was also new, and is described by Whittaker as "really a most important advance"; Michell says that the attraction and repulsion had previously been thought unequal, because of the failure to allow for a phenomenon nowadays known as magnetic induction – namely that the strength of a magnet may be influenced by the proximity of another magnet, and if two like poles are close "their power will be diminished", while conversely two unlike poles will mutually strengthen each other.

In a footnote to the sixth property, Michell suggests that Taylor and Musschenbroek would have found an inverse-square law if they had "made proper allowances" for the third and fourth properties. Palter's response to this is to point out that Michell did not explain how they could have taken account of these properties [Palter p550]. It is not clear to me how he could have done that. Indeed, the whole question of whether the force between poles "really" obeys an exact inverse-square law, with the induction element representing a perturbation of that law, takes us into philosophical territory, since it is not possible to eliminate the induced magnetism without also eliminating the magnet altogether. In modern textbooks the magnetic field in the presence of magnetisable media is considered to be the resultant of the applied field and an additional field due to magnetisation, but the distinction between these fields is made for mathematical convenience, and it is not clear that they can be separated in reality.

Should we say that the attraction and repulsion are "really" equal when actually they are not? It is a point of only historical interest, since the fundamental magnetic interactions are now seen as taking place between currents.

The position within the *Treatise* of this footnote is illuminating. It is a comment on the sixth property, which is the inverse-square law, and refers to several previous attempts to find the law. Taylor and Musschenbroek are singled out; both were unable to find a unique law, and it is possible that allowing for induction would have helped them. Since this effect increases with proximity between the poles, doing the experiment on like poles would have reduced the additional force at close range, and hence "flattened" the curve (suggesting a power of less than 2) and on unlike poles it would have enhanced the force at close range, making the curve steeper and perhaps suggesting an inverse cube. But since Patricia Fara, in "Sympathetic Attractions", tells us that the methods used by these two were "deflection" and "balance" [Fara p126], we know that neither was measuring the force between *poles* but rather between entire *magnets,* so whatever they could have done to allow for induction would not have led them to the result that we now regard as correct. Indeed, if we now look again at the exact wording of Michell's sixth property, we start to wonder whether he was altogether clear in his own mind about this. The wording is

*"The Attraction and Repulsion of Magnets decrease, as the Squares of the distances from the respective Poles increase"* [Michell p19].

This is rather imprecise: distances from the respective poles to what? Also, he does indeed seem to be talking about the forces *between magnets,* which we know is not an inverse-square force, rather than the forces between poles. So did Michell actually understand the full significance of his own emphasis on poles?

Indeed, even Fara does not seem to have understood it. She tabulates all the attempts at a magnetic force law, up to and including Michell's, but makes no attempt to distinguish between forces or couples between magnets and forces between poles [Fara p126]. She seems to see this sequence of experiments as inconclusive, and indeed, states that "from the middle of the century, English natural philosophers abandoned the search for a magnetic-force law" [*ibid.* p128]. This is true, strictly speaking, since Mayer and Lambert, whom Whittaker names as the inheritors of Michell's magnetic works, were both German; and Coulomb, who seems to have settled the issue in the 1780s, was of course French; yet these individuals were surely all aware of one another's work, so the reason for her singling out of "English" natural philosophers is not clear. The comment appears to describe a sort of collective decision; but there does not seem to have been a sufficient number of them to constitute an "English magnetic community" – just a few individuals.

Palter, whom she cites for some of the references in her table, is careful to point out that "the loadstones [*sic*] or magnets employed in these experiments were almost certainly "dipoles" … and the laws of force for dipoles are emphatically *not* inverse square laws" [Palter p547], and goes on to say that Michell's "recognition of the importance of measuring the force between poles … is surely praiseworthy" [*ibid* p550]. But Palter finds Michell's lack of data worrying, and is not prepared to attribute the discovery of the law to him, preferring to accord that honour to Mayer in 1760 [*ibid.* p548]; the implication here is that Palter thinks Michell did not actually verify the law experimentally, despite the fact that Michell himself claims to have done so. Furthermore, Palter does not even credit Michell with priority for the importance of poles, since he has his own candidate for that discovery, namely

J.L.Calandrini, who he says is the probable author of a long note to Newton's statement in the *Principia* previously referred to. This note, which Palter published, in a translation by himself and James Hynd, as an appendix to his paper, does indeed point out that "the magnet was composed of quite heterogeneous parts; some of these attracted, some repelled", and, later, that "each pole of the magnet shows equally a repulsive and an attractive force" [*ibid.* p553]. But nowhere in this appendix is there a reference to an inverse-square law of force between poles; indeed, the note describes a deflection experiment with a needle, and it is clear that what is being referred to when the author concludes from the experimental data, echoing Newton's own pronouncement, that "the magnetic force decreases in the ratio almost of the cube" [*ibid.* p558] is what we would nowadays call the couple acting on the needle as a whole.

As already mentioned, Palter is concerned about Michell's lack of a solution to the induction problem. If he believed, as his seventh property suggests, that the force between two identical poles depended on the square of the pole strength, he could have measured the forces for attraction and repulsion at equal distances, then taken the average of their square roots, thus eliminating the induced pole strength, as this would cancel. But perhaps that is clearer to someone possessing the benefit of a modern education in mathematical physics, than it would have been to an eighteenth century natural philosopher.

John Heilbron is another sceptic. He tells us that Michell "did not demonstrate how he deduced the law or that it saved the phenomena" [Heilbron p83]; and in his table of "Efforts to Obtain a Law of Magnetic Force 1710-85", which lists seven investigators (Taylor and Hauksbee counting as one), Michell is listed as having only "guessed" the inverse square law; clearly Heilbron does not believe he carried out the necessary experiments.

## 4. Theory of Magnetism

Since Michell did not honour his promise to publish, when "some other opportunity" arose, his theory of magnetism, we can say very little about it. As Russell McCormmach points out in a recent biography of Michell, a footnote to the third Property claims that it is "utterly inconsistent" with the "subtle fluid" theory of magnetism; McCormmach identifies such theories with the "Cartesian vortex" approach to mechanics. He also mentions a reference to Michell's "theory of molecular magnets" in connection with a discussion between Michell and Roger Boscovich in 1760 [McCormmach p66], and likens this to Coulomb's theory of molecular magnets, in which the "magnetic fluid" is trapped inside the molecules, and cannot pass from one molecule to another. A molecular theory, in which each molecule is thus a tiny magnet, explains why it is impossible to obtain an isolated pole.

## 5. Magnetometry after Michell

McCormmach describes Coulomb's experiments on the inverse-square law at the end of the $18^{th}$ century as "the experiments that eventually convinced the scientific world", which suggests that some doubts had remained following Michell's *Treatise* – probably because of the lack of experimental detail contained in it. This despite the corroborating evidence produced by Mayer and Lambert during the 1750s [McCormmach p66].

The law of force between magnetic poles was eventually re-formulated so as to include the pole strengths as well as the distance of separation. Although magnetism is no longer taught in terms of poles, we find this later form of the law in textbooks as recent as that of Fewkes and Yarwood, published in 1956, where it appears thus: [Fewkes & Yarwood, p1]:

$$F = A\frac{m_1 m_2}{d^2}$$

where $m_1$, $m_2$ are the pole strengths, $d$ is the separation and $A$ is a constant. McCormmach suggests that Michell's seventh property is as far as he got towards including the concept of pole strength, and postulates that the lack of an exact formulation of this "half" of the law "may be one reason he postponed publishing his experiments, intending to settle the point later" [McCormmach p65]. It does appear that there is an equivalence between the two, since the proximity of a magnetic pole to a piece of iron will induce in it an opposite pole of similar magnitude, and from Fewkes and Yarwood's formulation of the law we see that the force is then related to the product of these magnitudes, and hence to the square of the strength of the original pole – or, in the parlance of the day, to the pole strength "in a duplicate ratio". However, some doubts on this remain, since it is not clear how Michell quantified the "Strength for *touching, &c*" of a magnet, and hence we cannot be absolutely sure that his argument is not circular. And of course, the actual measurement of pole strength had to wait for the definition of the unit pole.

## 6. Michell and 18th Century Technology

Michell's *Treatise on Artificial Magnets* was aimed at seamen and instrument makers. It was primarily intended as a practical handbook on how to make magnets – in the age before Volta and Oersted, this meant using natural magnets or the earth's magnetic field to magnetise steel or iron bars (although McCormmach says the book was "probably read only by instrument-makers and natural philosophers who were interested in magnetism" [McCormmach p 61]). Luckily for our present purposes, it also included the "Properties of Magnetical Bodies".

In the *Treatise* Michell does not say how he came upon his quantitative laws of magnetism. We know that towards the end of his life he built a torsion balance with which he intended to determine the mass, and hence the density, of the earth (or equivalently, as we might describe it nowadays, to determine the gravitational constant) – an experiment which it was eventually left to his friend Henry Cavendish to perform, after Michell's death – but did he use a similar piece of equipment for his magnetic measurements in 1750? We know that Coulomb had a torsion balance in 1785; Whittaker says of Coulomb that the device was "independently invented by Michell and himself" [Whittaker p57]; Cavendish says that Michell informed him of his proposed use of the torsion balance in the gravity experiment "before the publication of any of Mr Coulomb's experiments" [Cavendish, quoted in McCormmach p283]. In a footnote, Whittaker concurs with this priority; he describes Michell as "the first inventor of the torsion balance" [Whittaker p56n].

But did he have a torsion balance in 1750? According to Palter, no: "One thing seems clear: Michell did not … use the torsion balance to measure magnetic force" [Palter p550n] – although he does acknowledge that two other authors, Pledge and Tricker, have claimed that he did. Hardin is also sceptical of Pledge's claims [Hardin p46].

Perhaps, in fact, he did not need a torsion balance to investigate the forces between magnets, since even the magnets he was able to make could clearly exert forces many times stronger than the gravitational force between the weights in the Cavendish experiment: there are references in the *Treatise* to magnets that can lift weights of several ounces. A long, thin magnet, pivoted about its centre, with a string attached at the far end holding a weight over a pulley, might thus suffice, and one might not need the thin, delicate wire by which the arm of the torsion balance was suspended.

Nevertheless – whether Michell built his first torsion balance in 1750 or 1780 – if Coulomb really did not know of Michell's design, the near-simultaneous appearance of the device in two different countries suggests that we look for some common precursor that enabled both to be built. Was it the availability of suitably fine wire? Or, perhaps (since the arm underwent damped oscillations) an improvement in timing devices? Alternatively, the technological breakthrough that enabled both men to carry out their magnetic experiments may have concerned the magnets rather than the balance. I have already referred to the need for long, thin magnets. McCormmach gives some hints that the crucial factor may have been the magnets; he says that Coulomb's success in convincing the sceptics was "primarily because of the narrow, two-foot long artificial magnetic needles he used in his torsion balance" [McCormmach p66]. Heilbron echoes this: he says that Coulomb used "long, thin artificial magnets with well-defined poles" [Heilbron p87], and that such magnets became available "soon after Calendrini found the law of dipoles" [*ibid.* p83] – in other words the early 1740s – which means that Michell would at least have had suitable magnets in 1750, if not suitable measuring apparatus.

Although Michell's interest appears to have been purely academic, two other magnet-makers, Gowin Knight and John Canton, saw the practice as potentially profitable. Michell acknowledges the work of Knight and also of his predecessor Servington Savery; though McCormmach notes that there is no reference to the work of Le Maire and Duhamel in France, who were also making magnets.

McCormmach tells us that there was no patent law in the mid-18th century, and consequently, competitors "did not reveal their methods" [*ibid.* p59]. Nevertheless, the *Treatise* contains full details of Michell's method. A dispute arose, however, following Canton's publication of his method of making magnets, which Michell clearly thought should have cited his own work. The dispute went on for decades, even after Canton's death, with Michell's and Canton's mutual friend, Joseph Priestley, caught in the middle [*ibid.* p68-71].

## 7. Conclusion

My opening quote from Whittaker portrayed Michell as an unjustly neglected and forgotten character. However in the course of my researches for this essay I have found that there are many valid criticisms of his work. Nowadays, a scientist whose career spanned several distinct disciplines – and Michell's included astronomy, seismology and geology as well as magnetometry and gravitation – would be regarded as either a rare polymath or, more likely, as a "jack of all trades and master of none". In the 18th century there was much less specialisation, and more room for polymaths; but even for a genius there is a limited amount of time, especially when one has to consider that the 18th century natural philosopher, unless of independent means, had to also earn a living, Michell's own profession being as a churchman. Indeed, his magnetic researches occupied only a short period at the beginning of his career; he soon moved on to geology and astronomy, and despite having promised to say more about magnetism when "some other opportunity" arose, he never did; even after building a torsion balance, he does not seem to have thought about using it to verify his theory of magnetism. A 1966 paper by Clyde Hardin concludes with the words: "thus none of

Michell's work was fully developed, promoted, and secured by him. It appears, then, that he was in great measure responsible for the unjust obscurity into which his reputation has fallen" [Hardin p47].

But, then, how much obscurity is the right amount? Our celebrity-obsessed culture tends to unjustly emphasise the rôles of superheroes such as Galileo, Newton, Darwin and Maxwell, at the expense of those on whose shoulders they stood. Recently there has been a drive towards the rehabilitation of scientists like Hooke who have suffered as a result of such hero-worship (sometimes exacerbated by the "heroes" themselves); but in truth, when we see that, in general, science proceeds in tiny steps, it is often difficult to know just who to honour for what. Did Michell discover the inverse-square law for magentic poles? If we don't feel we can give him full credit for that, what can we atrribute to him? Is it sufficient to say that he highlighted the problem of magnetic induction, and that he emphasised the importance of the poles (even if this was not always reflected in his terminology) and perhaps introduced the concept of pole strength, even though he was not quite able to measure it?

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