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Lodestone and earth: the study of magnetism and terrestrial magnetism in Great Britain, c 1750-1830

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Lodestone and earth: The study of magnetism and terrestrial magnetism in Great Britain, c. 1750-1830

by

Robinson McLaughry Yost

A dissertation submitted to the graduate faculty
in partial fulfillment of the requirements for the degree of
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Major Professor: David Ball Wilson

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CHAPTER 1:
INTRODUCTION
(c.1600-1750)

In 1683, English astronomer and natural philosopher Edmund Halley presented a paper to the Royal Society of London addressing what he called the "Magnetical System." Of this system Halley reflected:

> There are difficulties that occur that render the thing as yet not feasible, for first there are a great many observations requisite, which ought to be made at the same time; not at Sea, but ashore; with greater care and attention than the generality of Saylors apply. And besides it remains undetermined in what proportion the [magnetic] attractive power decreases, as you remove from the Pole of a Magnet; without which it were a vain attempt to go about to calculate. There is yet a further difficultie, which is the change of the variation . . . which shews, that it will require some Hundreds of years to establish a compleat doctrine of the Magnetical System.[1]

Nearly two and half centuries later, American geophysicist Louis Agricola Bauer delivered the 1913 "Halley Lecture" at Oxford University. In this lecture he remarked on the study of terrestrial magnetism:

> In spite of the accumulated facts of over three centuries, we are still unable to say definitely to what the Earth's magnetic field is really due. Perhaps we may not be able to solve the riddle until the physicist answers for us the questions: What is a magnet? What is magnetism, in general?[2]

Both men's statements emphasized the importance of accumulated facts over time and the enduring mysteries of the subject at hand, magnetism. Also, for Halley and Bauer, understanding terrestrial magnetism required an intimate acquaintance with magnetism.

In addition to these obvious similarities, however, the differences between Bauer and Halley are revealing as well. Published only four years before Isaac Newton's *Principia* (1687), Halley's paper appeared near the culmination of the "scientific revolution," a period of transformation in the institutions, goals, methods, and content of natural philosophy. Be that as it may, the science emerging from this "revolution" was not modern science. Hence, the "physics" of Halley's period was quite distinct from Bauer's "physics" more than two centuries later. These general differences regarding physics applied to their specific views on magnetism as well. While both men demonstrated considerable interest in magnetism, their motives and methods diverged greatly. Halley wrote of the "Magnetical system" and the "Saylor," while Bauer spoke of the "Earth's magnetic field" and the "physicist." While Halley's prime motivation for understanding magnetism was its navigational application, Bauer stressed its inherent scientific interest. Not surprisingly, the answers to "What is a magnet?" and "What is magnetism, in general?" differed dramatically as well.

By focusing on a portion of the two-century span between Halley and Bauer, this dissertation traces the shifting motives, methods, theories, and debates regarding the study of magnetism from 1750 to 1830. Placed firmly between the late seventeenth and early twentieth centuries, this eighty-year span witnessed transformations in the understanding of magnetism as well as changing views regarding terrestrial magnetic phenomena. Throughout the period, most investigators linked their controlled magnetic experiments with the phenomena of terrestrial magnetism. In other words, they assumed that both acted in an analogous manner. Stressing, but not limited to, the specific context of British science, this dissertation examines the study of magnetism and its links to terrestrial magnetic studies.

Before delving into these topics, a wider historiographical and historical context should be established for perspective. To this end, this chapter has three primary tasks. First, it presents an historiographical overview regarding several historians' of science attempts to categorize changes in the physical sciences from the seventeenth to the early nineteenth century. Second, the chapter discusses specific theories of magnetism and terrestrial magnetism from William Gilbert in the early seventeenth century through Halley's work about a century later. In this discussion, technical terms and historical background are introduced which will be utilized throughout the dissertation. Finally, the chapter briefly summarizes the content of the remaining chapters.

Mathematical, Experimental, and Natural Philosophical Traditions

In an article published in 1976, Thomas Kuhn argued that the history of the physical sciences could be regarded in terms of two distinct traditions, the mathematical and the experimental.[3] The mathematical tradition, contended Kuhn, encompassed the "classical sciences" many of which began in Greek antiquity (e. g., astronomy, statics, harmonics, and geometrical optics). Furthermore, this tradition included mathematically or geometrically idealized situations such as those as presented in works of Archimedes' *On Floating Bodies* or Ptolemy's *Almagest*. In contrast, Kuhn's experimental tradition referred to topics such as electricity and magnetism which receive infrequent attention until the early seventeenth century. In studying these phenomena, experimental evidence frequently took precedence over idealized mathematical results. Hence, Kuhn contended that this tradition embraced the "Baconian sciences," named after the inductive, empirical method espoused (but not practiced) by Francis Bacon.

Throughout the seventeenth century both traditions appealed to experiment, yet used it in distinct ways. Kuhn argued that those working primarily in the classical tradition like Galileo Galilei, Johannes Kepler, and Blaise Pascal utilized "thought experiments" often intended to confirm a conclusion known by non-experimental means.[4] For instance, although Galileo performed many experiments, he deemed some unnecessary because the power of his reasoning had ascertained the necessary outcome in advance. On the other hand, those working primarily within the experimental tradition including William Gilbert, Robert Boyle, and Robert Hooke often decried "thought experiments." Boyle, Kuhn pointed out, harshly criticized Pascal's book on hydrostatics due to its unrealizable experiments and impossible instruments.[5] Investigators in this tradition emphasized the use of instruments and often sought to generate new effects for the end purpose of constraining and controlling nature. They were again in a sense

"Baconian," because Bacon emphasized the promise of science for practical ends. As Kuhn concluded, the experimental tradition gave rise to relatively new sciences including the study of heat, electricity, and magnetism.

In 1986, Casper Hakfoort refined Kuhn's scheme by adding what he called the "natural philosophical" tradition.[6] Providing historical continuity, this tradition included theoretical and speculative portions of natural philosophy which were not encompassed by Kuhn's mathematical or experimental traditions. Hakfoort noted for instance that Aristotelian natural philosophy attempted to provide a complete account of nature closely linked to metaphysics. Of this he explained:

> [The Aristotelian] account was considered well established because it was based on metaphysical and empirical certainties. This *ideal* of a complete, certain, and partly a priori picture of the natural world did not die when the *contents* of Aristotelian natural philosophy were rejected in the Scientific Revolution.[7]

In the seventeenth century, René Descartes sought to replace Aristotelian natural philosophy with a quantitative, mathematical vision of the world. Nevertheless, similar to the Aristotelian system, the Cartesian system was ideally complete, certain, and partly *a priori*. As such, Descartes developed basic concepts and laws primarily from his "clear and distinct ideas" rather than from actual observation or experiment.

Though Aristotle allowed an important role to observation, neither he nor the Scholastic proponents of Aristotelianism had sought a quantitative, mathematical system of the world. Similarly, despite his famous dream of creating a mathematical and measurable view of the universe, the mechanical system Descartes eventually created was neither mathematical nor quantitative. As Hakfoort pointed out:

> [Descartes'] net result was a nonquantitative, visualisable, and complete explanation of the natural world in terms of a priori established concepts. So, what was new *in principle* in Descartes' approach did not *in fact* transform the epistemological claims and method of natural philosophy.[8]

Swirling Cartesian vortices of matter in motion were not readily quantified or mathematized. Nevertheless, Descartes' vision of a universe filled with matter in motion gained many adherents. The "mechanical philosophy," Cartesian or otherwise, appealed to many seventeenth-century natural philosophers. According to Hakfoort, attempts such as Descartes' to replace Aristotelian natural philosophy do not readily fit into Kuhn's mathematical or experimental traditions. Therefore, speculative, all-encompassing explanations belong within what Hakfoort called the natural philosophical tradition.

With these mathematical, experimental, and natural philosophical traditions in mind, the history of the physical sciences can become an analysis of their shifting relationships to one another over time. As examples, Hakfoort discussed Huygens' pulse theory of light and Newton's *Opticks* as illustrating three-way divisions between natural philosophical, mathematical, and experimental.[9] Using this tripartite division, Hakfoort further argued that these traditions are essential to understanding physical sciences in eighteenth-century Germany, particularly physical optics. While such an approach has its problems, it can nevertheless be used as an organizing principle for tracing historical developments in the physical sciences, including the study of magnetism.[10]

The Changing Meanings of "Physics"

A related approach, explored by John L. Heilbron, examines the multifarious classifications and divisions of natural knowledge over time. For instance, from the seventeenth to nineteenth centuries the shifting definition of the term "physics" reflected radically different relationships between the mathematical, natural philosophical, and experimental. From Greek antiquity through the seventeenth century, "physics" referred broadly to the study of all natural bodies, animate and inanimate. In this sense, traditional "physics" of the seventeenth century was inclusive, qualitative, and literary; the Aristotelian tradition of physics included both organic and inorganic realms. "Physics" in the seventeenth century recommended neither the use of experiment nor mathematics. As Aristotle argued, constraining nature through experimentation actually hid or distorted its true workings.[11]

On the other hand, the lesser-esteemed study of "mixed" or "applied" mathematics included quantified physical sciences and often those in the mathematical tradition such as observational astronomy, geometrical optics, mechanics, statics, and hydraulics. It also encompassed practically-oriented subjects such as geography, horology, fortification, surveying, and navigation. Thus, even though investigators including Copernicus, Galileo, and Kepler espoused the use of mathematics, introducing quantification and mathematization to seventeenth-century "physics" meant lowering its status in the established hierarchy of knowledge. Not surprisingly, most early Copernicans were mathematicians rather than natural philosophers.

By the early eighteenth century, though some continued using "physics" in its older, broader sense, many others narrowed the scope of "physics" and made it synonymous with "natural philosophy."[12] Of vital importance in this narrowing was the work of Isaac Newton. The full English title of Newton's magnum opus, *The Mathematical Principles of Natural Philosophy*, signaled the rising status of mathematical methods in natural philosophy. Indeed, Newton's work elevated several areas of mixed mathematics to the level of natural philosophy. In altering its scope and methods, Newton and others restricted the domain of natural philosophy or physics. As I. Bernard Cohen explained, Newton did not merely produce mathematical constructs for "saving the phenomena," instead he created what he considered to be "purely mathematical counterparts of simplified and idealized physical situations that could later be brought into relation with the conditions of reality as revealed by experiment and observation."[13] In this manner, Newton successfully combined the mathematical and experimental traditions. As we shall see, he also used elements of the speculative natural philosophical tradition, particularly in the Queries to the *Opticks*.

During the eighteenth century, natural philosophy became ever more restricted to particular areas of study. In addition to growing mathematical content, the increasing importance of instruments and experimental techniques contributed to the transformation of natural philosophy. Heilbron argued that the introduction of the demonstration experiment or demonstration lecture (utilizing air pumps, barometers, pendulums, lodestones, etc.) contributed to the narrowing of natural philosophy for at least three reasons. First, the biological sciences did not lend themselves readily to demonstration experiments. Second, the instrument trade, long established to meet the needs of "mixed" or "applied" mathematics, could easily furnish the professor of experimental philosophy or lecturer with apparatus. Third, during the 1730s and 1740s Newton's followers, particularly Dutchmen Willem Jakob 'sGravesande and Pieter van Musschenbroek, omitted biological and geological sciences, and almost all chemistry and meteorology from their popular, widely-read textbooks. Thus, concluded Heilbron, investigators used the demonstration-lecture to spread Newton's ideas and narrow the domain of natural philosophy. Therefore, by mid-century, "physics" (or *fisica*, *physique*, *physica*, *Naturlehre*) omitted most geological, biological, and chemical topics.[14] For many, "physics" and "natural philosophy" had become synonymous.

Although the Cartesian natural philosophical tradition survived, it drew increasing criticism from Newton and his disciples for its "system building" and for its use of unwarranted hypotheses. In the *Principia*, Newton clearly stated that general propositions were to be gathered by induction from the phenomena, not hypotheses. In 1709, curator of experiments at the Royal Society, Francis Hauksbee concurred:

> The learned World is now almost generally convinc'd that instead of amusing themselves with Vain Hypotheses, which seem to differ little from Romances, there's no other way to Improving Natural Philosophy but by Demonstrations and Conclusions founded upon Experiments judiciously and accurately made.[15]

Similarly, 'sGravesande's Newtonian textbook, *Mathematical Elements of Physicks*, (translated into English by John Keill) noted eleven years later:

> The Laws of Nature then are to be discovered in Physicks by the Phaenomena. And by Induction, they are to be accounted for general[ly]. As for the rest, we must reason Mathematically. He who seriously considers on what Foundation this Method of treating of Physicks depends, will easily find this is the only proper one, and that all Hypotheses are to be rejected.[16]

Alluding to Descartes' use of hypotheses, 'sGravesande reiterated in a later book that Newton's predecessor did "not think the Fiction of Hypotheses was entirely to be rejected out of Natural Philosophy."[17] Many English and Scottish Newtonians including Samuel Clarke, John Desaguliers, David Gregory, and John Freind agreed that true natural philosophy should reject hypotheses and embrace the fruitful wedding of experiment and mathematics.[18]

The ideals espoused by Newton and his followers often failed in the actual practice of natural philosophy. Despite its narrowing scope and changing methods, natural philosophy remained divided into two domains generally practiced by different groups of people. Historians of science have pointed out the persistence of these divisions, particularly regarding eighteenth-century physical sciences. Cohen, Kuhn, and others argued that natural philosophers tended to emphasize either mathematics or experimentation, but not both, in their work. In doing so, investigators stressed the methods of the *Principia* or the *Opticks* respectively.[19] At mid-century, mathematical subjects such as mechanics, hydrostatics, and planetary astronomy constituted the "physico-mathematical" sciences. These areas, dominated by mathematicians, concentrated on taking a single, simple generalization taken from experience and generalizing it mathematically. Subjects of lower status, dominated by experimenters, contained little, if any, mathematics. As such, the experimental branch of physics included the qualitative study of physical optics, heat, electricity, and magnetism. While eighteenth-century experimentalists frequently alluded to the desirability of quantitative data, they rarely went beyond simple numerical tables.[20] They did not integrate numbers or quantities into their qualitative theories. In fact, until the late eighteenth century, few natural philosophers successfully combined the mathematical and experimental traditions. As we shall see, despite changes in the early nineteenth century, "mathematical" and "experimental" physics remained more or less distinct.[21]

Newtonianism Triumphant?

In tracing developments of eighteenth-century natural philosophy, John Heilbron and others have questioned historiographies which take the triumph of Newtonianism as the primary guiding principle. They have done so for several reasons. First, using the label "Newtonian" requires broadening its meaning to such an extent that it is practically useless.[22] Although this does not imply that Newtonians did not exist, it does mean that the term "Newtonian" must be used in a qualified manner.

Not surprisingly, Newton's legacy inspired many individuals with differing educations, philosophies, goals, and methods. Some investigators, particularly French mathematicians, utilized the emerging calculus to extend the highly geometrical approach presented in the *Principia*. From their efforts emerged the rational mechanics of Jean d'Alembert and the celestial mechanics of Pierre-Louis Maupertuis, Alexis Clairaut and others. In contrast, experimental philosophers took their inspiration from the *Opticks*, virtually ignoring mathematics in their research. In the natural philosophical tradition, many investigators attempted to reduce phenomena to an all ecompassing system of particles and short-range forces of attraction and repulsion. In a similar manner, others took an interest in an active etherial medium as the common cause of light, gravity, electricity, heat, and other phenomena. Both mathematical and experimental approaches gained inspiration from Newton's own speculations. Also claiming to follow Newton, still others eschewed all hypotheses, contending that true causes remained unknown or unknowable. All of these approaches can be designated "Newtonian," but such a label must be carefully placed in context.[23]

A second major criticism launched against the eighteenth-century triumph of Newtonianism is that Newtonians, even when carefully defined, were not immune to non-Newtonian influences. Other philosophical traditions, originating on the continent, mixed and combined with Newtonianism. Elements of Cartesianism flourished as did the views of Leibnizians, Stahlians, and others. Illustrating the difficulties, various historians have called Leonhard Euler a Newtonian, a Cartesian, and a Leibnizian.[24] Daniel Bernoulli has been classified confusingly as an advocate of "Cartesian Leibnizean Newtonianism."[25] Euler, Pierre-Louis Maupertuis, and Johann Bernoulli accepted Newton's laws of mechanics and universal gravitation, yet rejected Newtonian optics.[26] In developing mechanics as a branch of mathematics, Jean d'Alembert drew upon both Newtonian and Cartesian traditions. "Pure Newtonianism", whatever it might have been, rarely if ever existed. Newtonianism usually became infused with original ideas as well. In this regard, major figures such as Euler, d'Alembert, and John Dalton are especially difficult to label "Newtonian" in a meaningful manner.[27]

A third critique of the triumph of Newtonianism is that too often historians make a general theory, a world-view, or a methodological principle the driving force for scientific change. Referring specifically to experimental physics, Heilbron argued that foundational or methodological concerns may often be too remote from actual experimental work to order it in useful ways. For instance, electrical experiments and instruments early in the eighteenth century cannot be meaningfully distinguished as "Newtonian" or "Cartesian." The same problem, noted Heilbron, extends to models and hypotheses explaining the phenomena:

> For although one recognizes that models incorporating vortices derived ultimately from Descartes, while those invoking springy spirits probably owed something to Newton, yet in practice all such qualitative models came to much the same thing, aether being to the one side what subtle matter was to the other.[28]

In like manner, Geoffrey Sutton argued that "explanations offered by French Cartesians and British Newtonians seem essentially interchangeable . . . [finding] a pair of paradigms that differentiates the two is a quixotic task at best."[29] In a related problem, Home remarked that applying the terms "Newtonian" or "Cartesian" to late eighteenth-century figures implies their preoccupation with the same concerns of earlier investigators. Believing this a faulty assumption, he concluded:

> Newtonianism was tempered by infusions from various Cartesian, Leibnizian and other sources . . . no longer did the practising scientists of the period feel constrained constantly to expound and justify their philosophical position . . . instead of worrying about past disputes, they looked forward to the resolution of a new and quite different set of scientific problems. We ought to follow their lead: that is, we ought to assess this later period on its own merits, rather than in terms of the intellectual concerns of a previous age.[30]

With these caveats in mind, historians of science have offered alternatives to the triumph of Newtonianism. Home, for instance, proposed to examine the actual practice of the scientists rather than stressing their prefatory methodological statements.[31] If this is done, the mathematical, experimental, and natural philosophical traditions emerge, frequently cutting across the barriers of English and Continental intellectual schools. The history of eighteenth-century physics becomes a complex story of different developing traditions rather than the simplistic triumph of a Newtonianism.

Similar to Home, Heilbron suggested a close examination of the shifting scope, methods, and definitions of physics during the eighteenth century. To illustrate these changes in a particular case, he divided the study of electricity into three periods. From 1700 to 1740, electricity became a distinct sub-species of experimental physics stemming from the work of Stephen Gray, Charles Dufay, and others. Experimenters associated with leading scientific academies performed most of the work, establishing the basic phenomena of electrostatics. In Heilbron's second period from 1740 to 1760, information increased and qualitative theories emerged with little or no mathematical content. French experimentalist Jean-Antoine Nollet, for instance, supposed the simultaneous influx and efflux of electric matter in his theory. In part due to Nollet's exciting demonstrations, electricity became the leading branch of experimental physics, commanding lengthy sections in natural philosophy treatises.[32] His theory, however, was eventually challenged by another qualitative theory developed by Benjamin Franklin. In the 1750s, Franklin explained that electric phenomena arose due to a subtle fluid which attracted ordinary matter, yet repulsed its own particles. Using the analogy of sponge holding water, he explained in 1753:

> As the spunge in its rarer state will *naturally* attract and absorb *more* water, and in its denser state will *naturally* attract and absorb *less* water; we may call the quantity it attracts and absorbs in either state, its *natural quantity*, the state being considered.[33]

As the sponge was to water, common matter was to the electric fluid. Hence, when electric fluid increased beyond a body's "natural quantity", the fluid spread across its surface, forming an electric "atmosphere."[34] Franklinian theory utilized electric atmospheres and the overabundance or deficiency of fluid to explain most electric phenomena qualitatively.[35]

As an Enlightenment climate of polite learning stimulated a popular curiosity in science, experimentalists like Nollet and Franklin were not the only ones interested in electricity. This broader curiosity in science, generally, and electricity, specifically, ranged from common men (and women) to university professors.[36] Although excitement and play were no doubt important motives, growing interest in electricity was not merely frivolous entertainment; electricity appeared to cause earthquakes and thunderbolts and to cure paralysis as well.[37]

In Heilbron's final period, from 1760-1790, qualitative theories and experimentation began to be replaced by mathematical formulation and precise measurement. Professors and academicians with mathematical training, including Franz Aepinus and Charles-Augustin Coulomb, dominated the subject.[38] As well, new instruments appeared, as did textbooks and specific monographs on electricity. By the 1790s, increasing numbers of natural philosophers attributed electricity as well as heat, light, and magnetism to the actions of distinct imponderable fluids.[39] As French experimental physics became increasingly mathematical and quantitative, British investigators adopted and adapted French mathematics and physics. Although the divisions between experimental and mathematical traditions persisted, most physicists at the beginning of the nineteenth century attempted to subject all phenomena to careful measurement and experiment.[40]

Despite the gradual transformation of experimental physics as a whole in the late eighteenth and early nineteenth centuries, most scholarly attention has focused on particular developments in the areas of electric, optical, and thermal phenomena. Electricity by itself, Heilbron noted in 1980, garnered over forty percent of the historical literature, as much as meteorology, optics, thermodynamics, pneumatics, and magnetism taken together.[41] By Heilbron's calculation, electricity, light, and heat accounted for sixty-five percent of the coverage of eighteenth-century experimental physics between 1927-1965 and fifty-nine percent between 1966-1977. In contrast, magnetism received only two and three percent respectively for these periods.[42] Books like Thomas Hankins' *Science and the Enlightenment* (1985) give scant coverage to the study of magnetism.[43]

Nevertheless, the study of magnetism, like the study of electricity, light, and heat, underwent important transformations between 1750 and 1830. Neither these changes nor their connections to contemporary theories of terrestrial magnetism, however, have been examined in the existing scholarship. Although historians of science have touched upon the development of the magnetic force law and imponderable fluid theories, they have paid little specific attention to study of magnetism, particularly in the British context before the research of Michael Faraday.[44]

In addition to filling in a gap in the existing scholarship, examining the study of magnetism from the mid-eighteenth century through the 1820s serves several general purposes. First, it clearly illustrates the difficulties of designating certain theories of magnetism "Newtonian." Second, it demonstrates in very general terms the shifting roles of the experimental, natural philosophical, and mathematical traditions. Third, it shows the links between the understanding of magnetism and the understanding of terrestrial magnetism. Fourth, it traces the continuing importance of continental influences on the development of British experimental physics. Fifth and finally, it examines broader changes in methodology and how these influenced the study of magnetism during the period. With these goals in mind, a closer examination of the context and content of magnetic studies before 1750 will set the stage for later chapters.

William Gilbert (1544-1603): Navigation, Magnetism, and Cosmology

From its earliest days, European interest in magnetism stressed navigational applications. Although known much earlier in China, the magnetic compass first appeared in Europe sometime during the twelfth century.[45] Over the next several centuries investigators began to recognize certain irregularities in the motions of the compass. Barring few exceptions, magnetism usually gained the attention of navigators, instrument makers and practitioners of mixed mathematics rather than natural philosophers. During the fifteenth century, Christopher Columbus, Sebastian Cabot, and other explorers noticed that the magnetic needle rarely pointed to the true geographic north, an observation called magnetic variation or declination. In the sixteenth century, investigators found that the needle tilted vertically with respect to a horizontal plane. The northern end of the needle, for instance, tilted or "dipped" in the northern hemisphere. This magnetic "dip" or inclination increased in higher latitudes and diminished in equatorial regions. In 1581, Robert Norman, a London instrument maker with wide acquaintance among ships' captains published *The New Attractive*.[46] Norman's book gave the first lengthy treatment of magnetic dip and described a special instrument, the dipping needle, for measuring it. Norman, however, made no attempt to explain the cause of dip.[47]

In contrast to Norman's book, William Gilbert's *De Magnete* (1600), with a preface by London navigational authority Edward Wright, sought to explain all magnetic and terrestrial magnetic phenomena. Gilbert, an English physician, unrelentingly attacked the methods and contents of most earlier magnetic studies. Prior to *De Magnete* most writers had favored celestial or localized terrestrial origins of global magnetic phenomena. Many argued that the compass needle indicated a guiding point in the heavens, usually the Pole Star. Others proposed huge magnetic islands or rocks far to the north which guided the motions of the compass needle; tales were even told of these rocks pulling nails from passing ships and sinking them.[48] While approving of the observations of his compatriots such as Edward Wright, Robert Norman, William Barlowe, and others connected with navigation, Gilbert dismissed most earlier theories as "figments and ravings." Such theories had been founded on reckless speculation rather than careful observation.[49]

Reflected in the full title, "A new philosophy of the magnet, magnetic bodies and the great magnet of the Earth," *De Magnete* shifted the emphasis from older notions to the magnetism of the entire earth. Railing against arguments founded solely upon speculation, authority, and books rather than reason, observation, and experiment, Gilbert exclaimed:

> why should I submit this noble and . . . this new and inadmissible philosophy to the judgment of men who have taken oath to follow the opinions of others, to the most senseless corrupters of the arts, to lettered clowns, grammatists, sophists, spouters, and the wrong-headed rabble, to be denounced, torn to tatters and heaped with contumely. To you alone, true philosophers . . . who *not only in books but in things themselves look for knowledge*, have I dedicated these foundations of magnetic science— a new style of philosophizing. But if any see fit not to agree with the opinions here expressed . . . *let them note the great multitude of experiments* and discoveries— these it is chiefly that cause of philosophy to flourish . . .[50]

Republished in England in 1628 and again in 1633, Gilbert's work was well-received through most of Europe for its experimental method and its magnetic discoveries. Over the next several centuries, references to Gilbert frequently cited him as the founder of experimental and magnetical philosophy.[51]

After dismissing the "fables and follies" of earlier investigators, Gilbert distinguished between the causes of magnetism and electricity. Proposing that electrical attractions exhibited by a piece of rubbed amber arose from the material emission of effluvia, he claimed that magnetic attractions did not share the same cause. Magnetic emanations, unlike material electric effluvia, could penetrate the densest bodies and magnetize needles without adding to their weight. Therefore, Gilbert concluded, "Electrical bodies [attract] by means of natural effluvia from humour; magnetic bodies by formal efficiencies or rather by primary native strength (*vigor*)."[52] Rejecting Aristotelian definitions of formal cause, he further explained, "This form is unique and peculiar: it is not what the Peripatetics call *causa formalis,*" nor was it the specific cause alchemists associated with mixtures or the propagator of generative bodies.[53] Asserting God had implanted this form, he remarked:

> it is the form of the prime and principal globes . . . the primary, radical, and astral form . . . Such form is in each globe— the sun, the moon, the stars— one; in earth also 'tis one, and it is that true magnetic potency which we call the primary energy. Hence the magnetic nature is proper to the earth and is implanted in all its real parts . . . There is in the earth a magnetic strength or energy (*vigor*) of its own . . . Thus we have to treat of the earth, which is a magnetic body, a loadstone.[54]

Identifying the magnetic form with an immaterial soul or earthly anima, Gilbert supposed each magnetic body, including the earth, to be surrounded by an orb of magnetic virtue (*orbis virtutis*) extending a certain distance in all directions. The extent of the orb depended on the purity of the magnet. Lodestone, iron and other magnetics within the surrounding orb became attracted to the body [see Figure 1].[55]

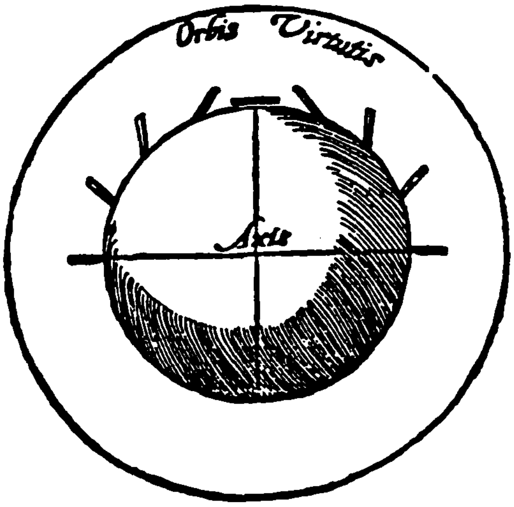

Figure 1. Gilbert's orb of virtue

Continuing his argument, Gilbert supposed that a suspended *terrella* perfectly represented the earth's magnetism as manifested by five magnetic properties: coition, verticity, declination, dip, and rotation. First, coition, the mutual attraction between magnetic bodies, occurred within the orb of virtue. Second, verticity was a magnet's ability to align itself in a fixed direction. Gilbert used these two properties to explain the earth's natural ability to turn on its axis and the stability of the axis. As evidence, he presented a multitude of experiments using small mounted compass needles or *versoria* moved around a spherical lodestones or *terrellae.* Analogous to mariners' observations with compass needles, his experiments with the *terrellae* indicated that the earth itself was a giant magnet. Therefore, magnetic substances within the terrestrial orb of magnetic virtue behaved analogously to substances within the orbs of ordinary, smaller magnets.

With *versoria* and *terrellae*, Gilbert duplicated all the phenomena known to navigators and mathematical practitioners including the third and fourth magnetic properties— declination and dip (which he called "variation" and "declination" respectively). Like the terrestrial globe, Gilbert's terrella had two poles and an equator. Further linking the earth and lodestone, natural magnets had been discovered throughout the earth's surface.[56] Gilbert also suggested that declination and dip might be mapped out for determining longitude and latitude.

Gilbert's cosmological arguments connected terrestrial magnetism with the rotation of the earth on its axis. Assuming the coincidence of the magnetic and the rotational poles, he explained:

> By the wonderful wisdom of the Creator, therefore, forces were implanted in the earth, forces primarily animate, to the end the globe might, with steadfastness, take direction, and that the poles might be opposite, so that on them, as the extremities of an axis, the movement of the diurnal rotation might be performed.[57]

Magnetic declination, however, indicated that the magnetic and geographic poles did not coincide. Bypassing this difficulty, Gilbert assumed that superficial irregularities of the terrestrial surface were the source of declination. Illustrating by analogy, he constructed a deformed *terrella* with indentations and raised portions representing the irregular distributions of sea and land. Appealing again to the *terrella*-earth analogy, Gilbert noted that the dip increased to a maximum at the earthly poles. Regardless of superficial irregularities, the magnetic poles and the geographic poles coincided.[58]

In the final book of *De Magnete*, Gilbert discussed the fifth magnetic property, rotation. In this book, he departed from the terrella-earth analogy by rejecting earlier speculations that a perfectly-aligned, spherical lodestone would rotate on its axis, "I omit as Petrus Peregrinus so stoutly affirms, that a terrella poised on its poles in the meridian moves circularly with a complete revolution in twenty-four hours. We have never chanced to see this: nay, we doubt if there is such movement." Lacking an analogous rotation of the *terrella*, he nevertheless asserted the rotation of the earth:

> The earth moves by its primary form and natural desire, for the conservation, perfecting, and beautifying of its parts, toward the more excellent things . . . The earth therefore rotates, and by a certain law of necessity, and by an energy that is innate . . . revolves in a circle toward the sun; through this motion it shares in

> the solar energies and influences . . . The sun (chief inciter of action in nature), as he causes the planets to advance in their courses, so, too, doth bring about the revolution of the globe . . .[59]

As we shall see, Gilbert's experimental conclusions regarding the earth's magnetism as well as his cosmological speculations regarding the earth's daily rotation were embraced, modified, and criticized by numerous seventeenth-century investigators.

The Reception of Gilbert's "Magnetical Philosophy"

Early in the seventeenth century, Gilbert's "magnetical philosophy" became a popular topic, particularly among Copernicans. Proponents including Simon Stevin, Johannes Kepler, and Galileo Galilei, borrowed and extended his arguments. Kepler, for instance, extended Gilbert's system by proposing magnetic forces emanating from the sun in the plane of planetary orbits. He explained the "body of the sun is circularly magnetic" and forms a circular "magnetic river" of immaterial emanations.[60] Later Kepler supposed that "every planetary body must be regarded as magnetic, or quasi-magnetic; in fact, I suggest a similarity, and do not assert an identity." Hence, each planet had two quasi-magnetic poles, one "friendly to the sun" and another "hostile."[61] Kepler believed that the cause of elliptical orbits lay in these magnetic properties of the sun and planets. In *Astronomia Nova* (1609) he explained, "the librational force is brought about by a magnetic force which is indeed innate and solitary, without any operation of a mind, but its description depends on the external solar body. The force, in fact, is defined as sun-seeking or as sun-fleeing."[62] Illustrating the importance of Gilbert, Kepler claimed in *Epitome of Copernican Astronomy* (1620) to have built his entire astronomy on Copernicus' system of the world, Tycho Brahe's observations, and "the Magnetical Philosophy of the Englishman William Gilbert."[63]

Though rejecting Kepler's elliptical orbits, Galileo accepted some of Gilbert's magnetic arguments in a *Dialogue Concerning the Two Chief World Systems* (1632). Toward the conclusion of the third day in the *Dialogue*, he wrote:

> Salviati: . . . if every minute particle of the [lodestone] have in it such a virtue, who will question but that the same more powerfully resides in this whole terrestrial globe, abounding in that magnetic matter, and which, haply, itself, as to its internal and primary substance, is nothing else but a huge mass of lodestone?
> Simplicio: Then you are one of those, it seems, who hold the magnetic philosophy of William Gilbert.
> Salviati: I am, for certain, and think that all those who have seriously read this book and tried his experiments will bear me company therein . . .[64]

Salviati and Sagredo then demonstrated to Simplicio, the Aristotelian character, that true elemental earth or lodestone, in contradiction to Aristotelian physics, had a circular motion. Furthermore, Salviati, Galileo's spokesman, endorsed Gilbert's argument for the magnetic stabilization of the Earth's axis.[65] Later, Salviati noted: "That which I could have desired in Gilbert is that he had been a somewhat better mathematician and particularly well grounded in geometry."[66] In this manner, Galileo, working in the mathematical tradition, criticized Gilbert, working in the experimental tradition.

Also in the experimental tradition (if in word, not deed), Francis Bacon, though not a Copernican, praised Gilbert's "many exquisite experiments" and lauded the compass as one of the most important discoveries of modern times. Despite his high praises, Bacon consistently condemned the magnetic philosophy. In *Novum Organum* (1620), he rejected the philosophy of Gilbert as well as the dogmas of alchemists. Constructing an entire system upon his favorite subject, Gilbert had, in Bacon's opinion, "become a magnet; that is, he has ascribed too many things to that force, and built a ship out of a shell."[67] In contrast to Gilbert, Bacon rejected the diurnal rotation of the earth and supposed a material cause for magnetism. Though frequently mentioning Gilbert, Bacon exhibited no deep knowledge of his work as did several of his fellow countrymen including Mark Ridley, William Barlowe, and Henry Gellibrand, all of whom worked on magnetism within the experimental tradition.[68]

In 1635, Gellibrand, professor of astronomy at Gresham College and an self-admitted Copernican, recognized that the declination in London had changed from past measurements.[69] This slow, gradual change, which he called the "variation of the variation" gave impetus to additional magnetic observations. Of this secular variation Gellibrand remarked:

> Thus hitherto (according to the Tenets of all our *Magneticall* Philosophers) we have supposed the variation of all particular places to continue one and the same: So that when a Seaman shall happily returne to a place where formerly he found the same variation, he may hence conclude, he is in the same former *Longitude.* For it is the Assertion of *Mr. Dr. Gilberts* . . . [that] the same place doth alwayes retaine the same variation . . . But most diligent magneticall observations have plainely offered violence to the same, and proved the contrary, namely that the variation is accompanied with a variation.[70]

Since Gilbert's theory did not allow compass needles to change direction with the passage of time, Gellibrand's "variation of the variation" clearly conflicted with the Gilbert's theory. The discovery of secular variation seemed to require that terrestrial magnetic poles become distinct from the geographical poles, thereby it cast doubts on the cosmological conclusions of the magnetical philosophy. Among other difficulties, secular variation did not mesh with Gilbert's explanation for diurnal rotation of the earth. As such, these factors contributed to the demise of Gilbertian cosmological arguments.

In the meantime, British mercantilism, overseas colonies, and trade wars continued, providing economic stimuli to the navigational application of magnetic studies. In their efforts, Gellibrand and other Gresham professors closely cooperated with members of the naval community such as John Wells, the Keeper of His Majesty's Naval Stores at Deptford. The national importance of navigation led many to propose magnetic solutions for determining latitude and longitude.[71] For instance, Henry Bond, a navigation teacher, put forth a scheme in 1648 which eventually appeared in *The Longitude Found* (1676).[72] Also with navigation in mind, the Royal Society of London established a Magnetics Committee in 1664 for studying and measuring magnetic variation. When the committee discovered instrumental errors too large to confirm

Bond's variation prediction, they assumed that unknown variables had altered the directive property of magnetic needles. President of the Royal Society, Sir Robert Moray, conjectured that perhaps different lodestones induced different directions or that the mechanical process of magnetization affected the needle's magnetism. During the second half of the seventeenth century, opinions akin to Moray's illustrated the growing acceptance of magnetism's mechanical origins and a concomitant rejection of Gilbert's immaterial magnetic souls.[73]

Magnetism and the Mechanical Philosophy

Although many continued using Gilbertian terms like "magnetic virtue", "magnetical vigor", and "intrinsic energy" to describe magnetic effects, numerous Englishmen after 1650 reduced natural phenomena, including magnetism, to matter and motion.[74] According to this mechanical-atomical philosophy, all phenomena were explicable in terms of the size, shape, number, and motion of particles of matter. As one of the leading advocates of mechanical philosophy, René Descartes sought to rescue magnetism from occult or animistic explanations such as Gilbert's.[75] Though he accepted that the earth contained or behaved as a giant magnet, Descartes vigorously rejected Gilbert's incorporeal emanations and orbs of magnetic virtue.[76] The Cartesian alternative utilized tiny screw-like particles which circulated through and around all magnets, including the earth, forming magnetic vortices. When iron filings were sprinkled onto a piece of paper placed over a magnet, their patterns indicated the underlying flow of these invisible particles. The minute particles of Descartes' first element which made up these vortices became grooved as they squeezed through gaps in clusters of his larger, spherical second element.[77] As the particles emerged, they naturally rotated and twisted, resembling either right-handed or left-handed headless cylindrical screws. Cartesian theory posited the two-way circulation of effluvia through appropriately threaded pores of iron. To explain all magnetic phenomena, Descartes depended exclusively on the motion of these threaded particles of matter. For its adherents, the theory persuasively described why magnets acted continuously without the loss of power or weight.[78]

Explaining terrestrial magnetism, Descartes' theory supposed one type of particles entered at the north pole, traveled through the terrestrial interior, and returned via the earth's atmosphere to its point of origin. Similarly, the oppositely-threaded particles entered at the south pole and circulated in the opposite direction [see Figure 2]. To accommodate the passage of particles, grooved or threaded channels ran along the entirety of the earth's interior. Magnetic needles aligned with the effluvial flow of particles circulating around and through the earth, giving them a general north-south direction. Magnetic attractions occurred when opposite poles faced each other and the particles circulated as if around a single magnet, eventually closing the gap between them. Similarly, Magnetic repulsions occurred when the threaded particles could not enter the channels made for them, thereby pushing objects apart.[79]

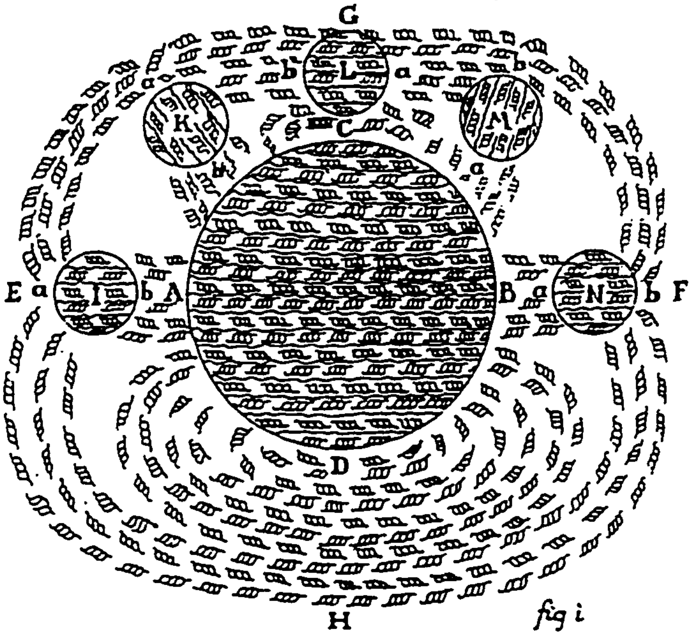

Figure 2. Descartes' magnetic vortices

Though the specific details of Descartes' magnetic theory were often rejected, the general concept of circulating effluvia became popular for several reasons. First, it plausibly explained why metals extracted from the earth became easily magnetized; these metals, it was supposed, simply had the correct kind of grooves. Second, it explained the irregularity of magnetic declination as originating from uneven deposits of iron and lodestone, thereby disturbing the symmetry of effluvial flow. Third, the "variation of variation" arose due to mankind's mining activities and numerous natural processes which altered the global distribution of ferrous materials. Finally, and most importantly, Cartesian theory appealed to an intellectual climate in which animistic forces and Aristotelian "substantial forms" were becoming anathema to many.[80]

From 1650 onward, many natural philosophers sought to elaborate what Robert Hooke called "the real, the mechanical, the experimental Philosophy."[81] Mechanical philosophers, whether in agreement with Descartes or not, believed that all phenomena could be explained in terms of matter and motion. Referring to the electrical hypotheses of Gassendi, Descartes and others, Robert Boyle explained that each had attempted "to solve the phaenomena in a mechanical way, without recurring to substantial forms, and inexplicable qualities."[82] Electricity, he noted, resulted from a material effluvium issuing from electrified bodies. In this and many other instances, British mechanical philosophers rejected Aristotelian explanations. Utilizing elements from both Cartesianism and revived Epicurean atomism, Boyle explained in 1660:

> There is yet another way to explicate the spring of air; namely, by supposing with that most ingenious gentleman, Monsieur *Des Cartes*, that the air is nothing but a congeries or heap of small and (for the most part) of flexible particles, of several sizes and of all kinds of figures, which are raised by the heat (especially that of the sun) into that fluid and subtle ethereal body that surrounds the earth; and by the restless agitation of that celestial matter, wherein those particles swim . . .[83]

Thereby, Boyle, among other mechanical philosophers, sought to explain magnetism by the shape, size, and motion of particles.

By the second half of the seventeenth century, mechanical explanations for magnetism had become fairly commonplace. French atomist Pierre Gassendi, for instance, proposed the continuous emission of hooked particles from the lodestone which become anchored in iron, thereby pulling the two together.[84] While Gassendi, Christiaan Huygens, and others on the continent advocated mechanical theories of magnetism, Henry More, John Wilkins, Walter Charleton, and Thomas Hobbes supported similar mechanical explanations in England.[85] Criticizing those who embraced Gilbert's argument for terrestrial rotation, Hobbes noted:

> As for them that suppose this may be done by magnetical virtue, or by incorporeal and immaterial species, they suppose no natural cause; nay, no cause at all. For there is no such thing as an incorporeal movent, and magnetical virtue is a thing altogether unknown; and whensoever it shall be known, *it will be found to be a motion of a body.*[86]

Furthermore, he explained that the lodestone's attractive power arose from "nothing else but some motion of the smallest particles thereof."[87]

Though wary of Aristotelian "forms" and "qualities", many English mechanical philosophers did not entirely reject action-at-a-distance. Astronomer John Wilkins, for example, kept alive the analogy between magnetic forces and those which governed celestial bodies.[88] An uneasy coexistence persisted between the aspects of the magnetical philosophy and aspects of the mechanical philosophy. As well, different elements of these traditions sometimes intermingled.[89] For instance, building upon Henry Power's Cartesian magnetic research, Boyle upheld the tenets of mechanical description in a qualified manner. In 1675, Boyle divided the general properties of bodies into several categories. Although his fourth category, "occult qualities," included electricity and magnetism, Boyle's *Experiments and Notes about the Mechanical Production of Magnetism*, published the following year, explained:

> Though the vertues of the *Loadstone* be none of the least famous of *Occult* Qualities, and are perhaps the most justly admired; yet I shall venture to offer something to make it probable, that some, even of these, may be introduced into bodies by the production of *Mechanical* changes in them.[90]

Hence, while generally favoring mechanical explanations, Boyle did not wholeheartedly embrace them when it came to magnetism.

While Boyle did not explain how corporeal effluvia produced magnetic effects, he did briefly describe the mechanical generation of several specific phenomena. Repeating some of Gilbert's experiments, Boyle argued that red-hot iron bar acquired magnetism when cooled in a north-south direction because it became "pervaded by the magnetical effluvia of the earth, which glide perpetually through the air from one pole to another, and by the passage of these steams [sic] it becomes endowed with a magnetical property, which some call polarity."[91] Admittedly, Boyle wrote, it might seem strange to attribute to "so gross and dull a body as the earth" the invisible power to communicate magnetism; in fact, he concluded, that we probably would not have dreamed of this "if our inquisitive Gilbert had not happily found out the magnetism of the terrestrial globe."[92] Hence, Boyle retained elements of both Gilbert and Descartes in his writings.

Robert Hooke, Curator of Experiments for the Royal Society, also gave equivocal support to effluvial explanations.[93] In the early 1670s, Hooke remarked of magnetical effluvia bending or inflecting themselves in different directions.[94] Magnetic power, he argued, came from the motions of "an Aethereal subtil Matter, which penetrates and pervades, and fills the Interstices of all Terrestrial Bodies."[95] While espousing the mechanical philosophy, Hooke simultaneously appealed to attractive forces. In *Lectures and Collections* (1678), he noted, without qualification, that the "attractive power" between the planets and the sun, was "as the Load-stone hath to Iron, and the Iron hath to the Load-stone."[96] In the very same work, however, he supposed, "all things in the Universe that become the objects of our senses are compounded of these two . . . namely, *Body*, and *Motion*."[97] Several years later, Hooke illustrated his growing doubts regarding the powers of magnetism and gravity:

> The causes of [Gravity and] Magnetical Attraction are so far remov'd beyond the reach of our Senses, that the greatest part of Philosophers who have indeavour'd

> to give us an information thereof, have rather made us more sensible of their and our own Ignorance and Inability to do anything therein, some making it Corporeal, some Spiritual; but whatever either of them mean either by Corpuscles of Magnetic Effluvia, or Atoms, or Magnetic Vertue, or Hylarchick Spirit, or Anima Mundi, when you come to inquire to the bottom of it you find, that neither they nor we know what is meant, and we do as good as say 'tis so, because it is so . . .[98]

As well, speculation and confusion continued regarding the mechanism of secular variation of terrestrial magnetism. In 1670, Boyle supposed that unknown internal changes were the cause, while Hooke suggested several years later that a magnetic pole rotated around the geographic pole once every 370 years.[99] In the 1680s and 1690s, astronomer Edmond Halley proposed the existence of four magnetic poles to explain the slow westward drift of variation. In 1683, Halley's first hypothesis conjectured the motion of magnetic poles, two in the northern hemisphere and two in the southern, on the terrestrial surface. Nine years later, he proposed a mechanism for the motion of the poles— an internal magnetic nucleus with two poles surrounded by a magnetic shell also with two poles. Thus, secular variation resulted from a slight difference in rotational periods for the inner kernel and the outer shell.[100] Despite these clever attempts to reduce secular variation to a physical mechanism, the myriad variables, complexities of effluvial magnetism, and scarcity of reliable terrestrial magnetic data tested the confidence of Hooke, Halley, and other investigators as well. In 1683, Halley listed a multitude of unknowns:

> a great many observations requisite . . . in what proportion the attractive power decreases . . . the change of variation . . . whether these Magnetical Poles move together with one motion, or with several; whether equally or unequally; whether circular of Libratory; if circular, about what center; if Libratory, after what manner; [these] are secrets as yet utterly unknown to Mankind; and are reserved for the Industry of future ages.[101]

He warned that investigators should be wary of accepting any hypothesis (including his own), no matter how plausible it might seem.[102]

Continuing the connections developed between Gresham professors and the Navy, the Admiralty in 1698 instructed Halley upon the wishes of King William III to "improve the knowledge of the Longitude and Variations of the Compasse" and to find *Terra Incognita* (the southern land mass whose existence had been postulated by ancient geographers).[103] After three Atlantic voyages (1698-1700), Halley failed to locate the great southern continent, yet he collected several hundred magnetic observations. These were used for a magnetic chart of the Atlantic (1700) showing lines of equal magnetic variation (later called isogonic lines). This chart and an extended world chart published in 1702 were intended to help solve the longitude problem. Of his magnetic chart and its uses, Halley wrote:

> A further Use is in many Cases to estimate the Longitude at Sea thereby; for where the Curves run nearly North and South, and are thick together . . . it gives a very good Indication of the Distance of the Land to Ships come from afar; for there the Variation alters a Degree to each two Degrees of Longitude nearly . . . it must be noted that there is a perpetual tho' slow Change in the Variation almost every where, which will make it necessary in time to alter the whole System.[104]

As we shall see in the next chapter, few were willing or able to make the necessary observations to periodically update Halley's magnetic charts.

Into the Eighteenth Century: Magnetism, Newton, and "Newtonianism"

Late in the seventeenth century the complexities of magnetic phenomena and its terrestrial manifestations led to the collapse of Gilbert's magnetic philosophy. Separate and distinct from Newton's force of gravity, magnetic phenomena lost their cosmological significance. Further hastening the decline of the magnetic philosophy, mechanical philosophers supposed that compass needles were subject to mechanical effects of heat, cold, and hammering, as well as irregular atmospheric and geological disturbances. Fraught with complexity and uncertainty, the study of magnetism became subsumed to the general study of mechanical effluvia.[105] Although the compass continued to guide mariners through rough seas, terrestrial magnetic irregularities and a lack of reliable data increasingly ruled out the application of magnetic measurements for determining longitude.

In the early eighteenth century the study of magnetism and its application to the longitude problem garnered less attention then previously. Despite continued, even frenzied, interest in the longitude problem (particularly fostered by the Longitude Act of 1714), eighteenth-century investigators, including Halley, gave less attention to magnetic solutions.[106] Magnetic longitude schemes persisted, but no longer with the frequency of those put forth in the seventeenth century.[107] Other solutions developed during the eighteenth century used astronomical observations or marine chronometers. These eventually proved more convenient and more accurate than magnetic charts. Until the nineteenth century, very few showed enthusiasm for collecting magnetic observations on a global scale. By then, the motivations for collecting magnetic data had changed.

Experimental research early in the century further complicated matters. Like the efforts of Hooke and Halley, eighteenth-century attempts to determine a magnetic force law led to uncertainty, even frustration. In the second edition of the *Principia* (1713) Newton noted that the power of the magnet diminished "not as the square but almost as the cube of the distance," yet neglected to explain how he had come to this conclusion.[108] Around the same time, Francis Hauksbee and Brook Taylor saw fit to confine their experimental results to tables of raw data.[109] In the 1720s, Taylor complained of the difficulties of locating the centers of magnetic power in his magnets and needles. He concluded that magnetism's power did not change according to any particular power of the distance.[110] Similarly, the Dutch experimentalist Pieter van Musschenbroek contended that there no constant law related magnetic force to distance.[111] Illustrative of the confusion and complexity, proposals for the law of magnetic force also included simple inverse, inverse to the 3/2 power, inverse square, inverse to the 5/2 power, and inverse to the fourth power.[112]

Complexity continued to confound terrestrial magnetic measurements as well. In 1724, London clockmaker George Graham noticed with a specially-made compass that magnetic variation fluctuated over the course of the day.[113] This diurnal variation of the magnetic needle and other irregular fluctuations merely added to the difficulties of explaining terrestrial magnetic phenomena. In the 1740s and 1750s, John Canton performed many experiments on diurnal variation. First, he placed a small magnet near a compass and noted the needle's deflections. Next, he covered the magnet with a brass container and poured hot water into the container. Heating the magnet, Canton noted, caused the nearby needle to fluctuate. Like Gilbert, Canton extended his observations to the entire earth so that, by analogy, he postulated that the sun heated different terrestrial regions during the day, thereby weakening the magnetic forces and deflecting the needle.[114] Using this analogy, Canton attempted to explain the small diurnal variations of the magnetic needle. These tiny, yet noticeable variations and other irregular fluctuations in magnetic measurements gained much greater attention in the nineteenth century.

Adding to early eighteenth-century confusion, Isaac Newton never gave a straightforward discussion of his theoretical views on the causes of magnetism. Without putting forth a detailed theory, he espoused conflicting, even contradictory messages on the subject.[115] Several times, Newton compared magnetism to gravity and other action-at-a-distance forces whose causes remained unknown. In the 31st Query to the *Opticks* (1730) he asked:

> Have not the small Particles of Bodies certain Powers, Virtues, or Forces, by which they act at a distance, not only upon the Rays of Light . . . but also upon one another for producing a great Part of the Phaenomena of Nature? For it's well known, that Bodies act one upon another by the Attractions of Gravity, Magnetism, and Electricity; and these Instances shew the Tenor and Course of Nature. . . . How these Attractions may be perform'd, I do not here consider.[116]

Nonetheless, Newton conjectured immediately following these remarks that what he called attraction "may be perform'd by impulse, or by some other means" unknown to him.[117] In fact, most evidence (published and unpublished) indicates that he, like many others, supported a Cartesian description of magnetic phenomena. For most of his career, Newton espoused circulating magnetic "streams" or "effluvia."[118]

The situation in the early eighteenth century aptly illustrated the difficulties of defining a "Newtonian" in a straightforward manner. With respect to magnetism early followers of Newton might take one of several paths which included accepting the circulating effluvial theory; claiming that magnetism's causes remained unknown; and endorsing an action-at-a-distance force like gravity. Newtonians Edmond Halley, George Graham, and Colin Maclaurin accepted effluvial theories, yet William Derham, Brook Taylor, and John Desaguliers, also dedicated Newtonians, avoided writing about the underlying causes of magnetism.[119] Rather than speculate about the underlying physical mechanisms, the latter investigators used terms such as "power", "attractive virtue", "polarity", and "magnetic virtue." Desaguliers, a popularizer of experimental natural philosophy, wrote in 1730:

> Whereas now our Principles are Four or Five at least, whose Cause we do not know, nor all the Laws of some of them, viz. Gravity, Attraction of Cohesion, Electricity, Magnetism, and Elasticity: we only know that there are such powers in Nature, & that they produce Effects which are the Subjects of our Contemplation.[120]

Other Newtonians such as William Whiston, John Keill, and Musschenbroek openly disapproved of corpuscular-mechanical theories. Keill, an opponent of the mechanical philosophy, commented in his natural philosophy lectures at Oxford in 1700 that:

> what some generally boast of, concerning Effluvia, a subtile Matter, Particles adapted to the Pores of the Loadstone, &c. does not in the least lead us to a clear and distinct Explication of these Operations: but notwithstanding all these things, the magnetick Virtues must be still reckoned amongst the occult Qualities.[121]

Appealing to experimental evidence, Musschenbroek argued into the 1740s that magnets did not act by circulating material effluvia.[122] As these and other examples illustrate, the label "Newtonian," even if chosen by the investigators themselves, did not presuppose one specific view with respect to magnetism. Hence, Newtonians explained magnetic phenomena in several ways and remained consistent with what Newton himself espoused.

Despite a lack of consensus among Newtonians, the Cartesian notion of a subtle magnetic effluvia circulating around and through magnets retained widespread acceptance in England and on the Continent. In 1696, German mathematician and philosopher Gottfried Wilhelm Leibniz, wrote of Descartes' theory:

> these particles twisted into folds seem quite unnecessary; it suffices that the openings are so adjusted to what passes through them that, after this passage has continued for some time, its return is prevented and is against the grain, so to speak. A contrary effort changes these folds, however, and reverses them.[123]

Despite Leibniz' objections, he and many others retained the basic elements of the circulating fluid theory. John Harris' *Lexicon Technicum* (1704) supported such a theory and the Gilbertian analogy as well. From all experiments, Harris concluded, "'tis plain (as Mr. Boyle concludes) That Magnetism doth much depend upon Mechanical Principles. As also, That there is such a Thing as the Magnetism of the Earth; or that there are Magnetical Particles, which continually are passing from Pole to Pole."[124] In an English translation of Cartesian Jacques Rohault's *A System of Natural Philosophy* (1723), Newtonian Samuel Clarke, who annotated much of the text with objections, did not object to Rohault's presentation of the Cartesian magnetic theory.[125] Five years later, Ephraim Chambers' popular *Cyclopaedia* (1728) commented, "The opinion that principally prevails among the moderns [on magnetism] is that of Des Cartes." The fifth (1741-43) and seventh (1752) editions of Chambers' *Cyclopaedia* reiterated this claim.[126] At mid-century, Britain's leading magnetic researcher Gowin Knight clearly embraced the effluvial theory; he wrote, "The magnetic Matter of a Loadstone moves in a Stream from one Pole to the other internally, and is then carried back in curve[d] Lines externally, till it arrives again at the Pole where it first entered, to be again admitted."[127] Though the theory was not Cartesian in the strict sense of following the details of Descartes' theory, it nonetheless retained the notion of a mechanical circulating effluvia traversing around and through all magnetic bodies. Variants of this general idea, for sake of convenience, be designated "Cartesian" throughout the remaining chapters.

In 1746, Leonhard Euler, Daniel and Jean Bernoulli (II), and Etienne-François DuTour split the prize offered by the Paris Academy of Sciences for the best paper on theory of magnetism. Though rejecting grooved particles and rifled channels, each paper proposed a qualitative, non-mathematical explanation appealing to a circulating subtle magnetic matter.[128] In the Cartesian tradition, able mathematicians such as Euler and the Bernoullis accepted that they were dealing with an irreducible, erratic phenomena not amenable to mathematical analysis.[129] Hence, the study of magnetism remained firmly within the experimental tradition. As we shall see, the dominance of "Cartesian" effluvial theories continued for most of the eighteenth century, even in "Newtonian" Britain.

Traditions in the Study of Magnetism

How does the British study of magnetism fit within the mathematical, experimental, and natural philosophical traditions? Before Gilbert's magnetical philosophy, navigators and instrument makers dominated the study of magnetism. Magnetic study was considered a part of navigational science under the domain of mixed mathematics. As navigators and instrument makers continued to study magnetism in the seventeenth century, *De Magnete* assisted in creating a new "Baconian science," which was soon taken up by mathematicians and some natural philosophers. Despite the involvement of mathematicians and magnetism's classification as part of mixed mathematics, the actual study of magnetism remained devoid of mathematical content. Efforts to quantify were limited to tables of numbers, either derived from experiments or from observations of magnetic dip or variation. As such, these studies remained squarely within the experimental tradition.

The rise of the mechanical philosophy in the mid-seventeenth century, however, placed the study of magnetism within the natural philosophical tradition as well. Descartes' screw-shaped particles, Gassendi's hook-shaped particles, and Boyle's magnetic corpuscles, although appealing to experimental evidence such as the patterns of iron filings, did not arise solely within the experimental tradition. Instead, magnetic effluvia arose from an a priori metaphysical assumption that all phenomena could be explained mechanically, in terms of matter and motion. Making simplistic mechanical interpretations of magnetism more complex, Boyle, Hooke, and Newton supposed action-at-a-distance forces while simultaneously espousing mechanical descriptions. Adding to the magnetic perplexities were numerous problems including the complete lack of a magnetic force law, the unpredictable effects of magnets being hammered, heated and cooled, and the unreliable, insufficient terrestrial magnetic data which grew ever more intransigent with the accumulation of measurements.

By 1750, circulating effluvia dominated theories of magnetism and terrestrial magnetism alike. Despite this dominance no consensus existed among British investigators during the eighteenth century. The study of magnetism, divided between the experimental and natural philosophical traditions, continued to be a mystery with tremedous philosophical and navigational potential. Nonetheless, magnetism was less studied than other areas of experimental physics, particularly electricity. It remained a riddle which even the incomparable Sir Isaac Newton had not satisfactorily reduced to a mathematical law. Despite numerous experimental attempts, the magnetic force law, if it existed at all, remained elusive. British attempts to resolve the mysterious natures of magnetism and of terrestrial magnetism are the focus of the remaining chapters.

The second chapter examines the impetus behind magnetic data collection beginning circa 1750. Although the close connections between the Royal Society, the Admiralty, and the Board of Longitude persisted, very few natural philosophers continued Halley's program of magnetic mapping. Most often, navigators such as James Cook collected data, while natural philosophers and instrument makers stayed at home to design, construct, and test a variety of magnetic and meteorological instruments. Throughout the voyages of the 1760s and 1770s, magnetic observations took a back seat to the more vital tasks of astronomical collecting and testing chronometers. Following the Napoleonic wars, magnetic collecting in Britain continued with new prominence as Arctic voyages resumed the search for the North-West Passage and the North Pole with renewed vigor. The division of labor continued, as scientific servicemen in the Royal Navy or Royal Artillery collected magnetic data in the frigid polar climate, while natural philosophers and mathematicians performed magnetic experiments at home. Tracing the collection of magnetic data up to 1835 reveals its shifting practical, scientific, and symbolic importance.

Returning to the theoretical scene of mid-eighteenth century, the third chapter closely examines the continuing division of magnetic studies between experimental and natural philosophical traditions. By the 1790s, some investigators slowly merged the mathematical with the experimental tradition. This happened for several reasons, including the acceptance of imponderable fluid theories which successfully quantified and mathematized the study of magnetism and other areas of experimental physics. Earlier in the century, speculations abounded, some even challenged the Gilbertian notion of a giant terrestrial magnet. Others conjectured that an electric fluid or solar rays were responsible for terrestrial magnetic effects. Finally the chapter discusses the influential magnetic and electric theories of German mathematical physicist Franz Aepinus. It examines how Aepinian theory diverged from circulating fluids and how it was initially ignored bu British experimenters.

The fourth chapter continues the story of the wedding of experimental and mathematical traditions for the period c. 1780 to 1820 by examining several key

transitional figures. The first, Tiberius Cavallo, accepted the one-fluid theory of Aepinus, while remaining in the experimental tradition. As the blending of experimental and mathematical traditions continued, the natural philosophical tradition took an important yet subsidiary role. Using Newton's ether or other basic unifying principles, this speculative tradition continued speaking of nature's intimate connections.
The second important transitional figure, John Robison, the professor of natural philosophy at the University of Edinburgh, was one of few British investigators who enthusiastically embraced Aepinian theory and scientific style. The development of Robison's scientific methodology, his influences, and his theory of magnetism are discussed in great detail. By bringing continental mathematics and physics to a wider British audience through encyclopedia articles and other writings, Robison and his successor at Edinburgh, John Playfair, helped alter the face of British experimental physics. In doing so, they brought closer together the use of mathematics and experiment.

As the fifth chapter demonstrates, other important factors in this transformation were the magnetic researches of Charles Augustin Coulomb and Laplacian scientists. This chapter discusses the development of Coulomb's magnetic theory, Laplacian science, and the growing impact of French physics in Britain during the early nineteenth century. Supporters of Aepinus' theory such as Thomas Young increasing questioned and rejected circulating effluvial theories. Despite their long-held popularity Cartesian theories yielded neither to mathematical analysis nor quantification, two desiderata of the emerging style of experimental physics. Though British investigators preferred Aepinian theory during the first decades of the century, the growing influence of Laplacian physics contributed to increasing approval for Coulombian theory by the early 1820s. The chapter suggests that Laplacian physics, particularly that of Jean Baptiste Biot, had certain similarities with the Scottish approach to physics. These similarities contributed to both personal connections and methodological affinities between Laplacian and British experimental physicists.

The final chapter examines how theories of magnetism dramatically changed in Britain from 1820 to 1835. The experimental tradition of magnetic research gained particular impetus from the discovery of electromagnetism in 1820. This discovery initiated a wave of experimental work and fostered further speculation about nature's unity and interconnectedness. The experimental, mathematical, and natural philosophical traditions all played roles in these developments. Meanwhile, continued research and new theories brought about a decline of Laplacian views. Affecting theories of terrestrial magnetism, Ampère, Arago, and others subsumed magnetic effects to the circulation of electric currents in all magnets, including the earth. Other researchers, including Humboldt, Oersted, and Hansteen supposed the effects of electricity, heat, chemical action, or rotation were intimately connected with magnetic and geomagnetic phenomena. This chapter examines various British responses to these experimental discoveries, while emphasizing their impact upon the understanding both magnetism and terrestrial magnetism.

This dissertation adds to the relatively small body of scholarship dealing with the history of magnetism and terrestrial magnetism. In particular, it emphasizes the shifting understanding of magnetism within the context of British experimental physics. Methodological concerns, personal links, changing instrumentation, navigational practices, and other factors such as educational context and nationalism are incorporated into the primary discussion of theoretical changes. This dissertation's main purpose is tracing the shifting theoretical understanding of magnetism and its relationship to changing theories of terrestrial magnetism.

Notes

[1] Edmond Halley, "A Theory of the Variation of the Magnetical Compass," *Philosophical Transactions of the Royal Society of London* (1683), 220.

[2] L. A. Bauer, "The Earth's Magnetism," *Bedrock*, 2 (1913), 291. Born in Cincinnati, Ohio, Bauer (1865-1932) earned a Ph.D. from the University of Berlin in 1895 for a thesis on the secular variation of the earth's magnetism. In 1902 Bauer was made the director of the Department of Terrestrial Magnetism at the newly founded Carnegie Institution of Washington. He also founded and edited the influential geophysical journal *Terrestrial Magnetism* from 1896 to 1927, renamed *Terrestrial magnetism and atmospheric electricity* in 1899. See Nathan Reingold, *DSB*, 1: 521-522.

[3] Thomas Kuhn, "Mathematical versus Experimental Traditions in the Development of Physical Science," *Journal of Interdisciplinary History*, 7 (1976), 1-31.

[4] Ibid., 12-13. For a brief summary and critique of Kuhn's argument, see H. Floris Cohen, *The Scientific Revolution, A Historiographical Inquiry* (Chicago: University of Chicago Press, 1994), 126-135.

[5] See Robert Boyle, "Hydrostatical Paradoxes, Made out by New Experiments," in A. Millar, ed., *The Works of the Honourable Robert Boyle* (London, 1744) 2: 414.

[6] Casper Hakfoort, *Optics in the age of Euler: Conceptions of the nature of light, 1700-1795* (Cambridge: Cambridge University Press, 1995), 180. Originally published in Dutch as *Optica in de eeuw van Euler: opvattingen over de natuur van het licht, 1700-1795* (Amsterdam: Rodopi 1986).

[7] Ibid., 180-181.

[8] Ibid.

[9] Hakfoort gives several examples of this division, for instance, in 1746, the Prussian Academy reorganized into three classes, "philosophie spéculative", "mathématiques", and "philosophie expérimentale." Ibid., 182. See also Casper Hakfoort, "Torn between three lovers: On the historiography of 18th-century optics," in *XVIIth International Congress of History of Science, University of California, Berkeley 31 July- 8 August 1985, Acts, vol. 1: Abstracts of papers presented in scientific sections* (Berkeley: 1985), section Pc.

[10] Geoffrey Cantor points out several difficulties with Hakfoort's historiography in Geoffrey Cantor, "Book review of Casper Hakfoort, *Optics in the Age of Euler. . .*" *British Journal for the History of Science*, 29 (1996), 236-238.

[11] John Heilbron, "Experimental natural philosophy, "The *ferment of knowledge: Studies in the Historiography of Eighteenth-Century Science*, edited by G. S. Rousseau and Roy Porter (Cambridge: Cambridge University Press, 1980), 362. See also Thomas Kuhn, "Mathematical versus Experimental Traditions in the Development of Physical Science," *Journal of Interdisciplinary History*, 7 (1976), 23-24.

[12] For the older definition of "physics" see John Quincy, *Lexicon physico-medicum . . .* (London: A. Bell, W. Taylor, and J. Osborn, 1719) [Landmarks of science microform, 1971], 344. Quincy defined Physick as "in general the Science of all material Beings, or whatsoever concerns the System of this visible World."

[13] I. Bernard Cohen, *The Newtonian revolution: With illustrations of the transformation of scientific ideas* (Cambridge: Cambridge University Press, 1980), 37-38.

[14] John Heilbron, "Experimental natural philosophy," *The ferment of knowledge: Studies in the Historiography of Eighteenth-Century Science*, edited by G. S. Rousseau and Roy Porter (Cambridge: Cambridge University Press, 1980), 357-365.

[15] Francis Hauksbee, *Physico-Mechanical Experiments on Various Subjects . . .* (London, 1709), preface. Quoted in Larry Stewart, *The Rise of Public Science, Rhetoric, Technology, and Natural Philosophy in Newtonian Britain, 1660-1750* (Cambridge: Cambridge University Press, 1992), 118.

[16] Willem Jacob 'sGravesande, *Mathematical Elements of Physicks, Prov'd by Experiments: Being an Introduction to Sir Isaac Newton's Philosophy*, translated, revised and corrected by John Keill (London: G. Strahan, 1720) [Landmarks of science microform, 1969], preface.

[17] Willem Jacob 'sGravesande, *An Explanation of the Newtonian Philosophy, in Lectures read to the Youth of the University of Leyden*, translated by Edmund Stone (London: W. Innys, 1741, Second Edition) [Landmarks of science microform, 1974], preface.

[18] See John Heilbron, *Elements of Early Modern Physics* (Berkeley: University of California Press, 1982), 1-9. For the broader social context of English experimental philosophy and the rise of popular Newtonianism, with particular emphasis on Desaguliers, see Larry Stewart, *The Rise of Public Science, Rhetoric, Technology, and Natural Philosophy in Newtonian Britain, 1660-1750* (Cambridge: Cambridge University Press, 1992).

[19] See I. Bernard Cohen, *Franklin and Newton An Inquiry into Speculative Newtonian Experimental Science and Franklin's Work on Electricity as an Example Thereof* (Philadelphia: The American Philosophical Society, 1956); A. Rupert Hall, "The scholar and the craftsman in the scientific revolution," in *Critical problems in the history of science*, edited by Marshall Clagett (Madison, 1962), 3-23; C. Stewart Gillmor, *Coulomb and the evolution of physics and engineering in eighteenth-century France* (Princeton, N. J.: Princeton University Press, 1971); Robert H. Silliman, "Fresnel and the Emergence of Physics as a Discipline," *Historical Studies in the Physical Sciences*, 4 (1974), 137-162; and Roderick W. Home, "Out of a Newtonian straitjacket: Alternative approaches to eighteenth-century physical science," in *Studies in the eighteenth century, IV: Papers presented at the fourth David Nichol Smith memorial seminar Canberra 1976* (Canberra, 1979), 235-249.

[20] Roderick W. Home, "Out of a Newtonian straitjacket: Alternative approaches to eighteenth-century physical science," in *Studies in the eighteenth century, IV: Papers presented at the fourth David Nichol Smith memorial seminar Canberra 1976* (Canberra, 1979), 245.

[21] See Maurice Crosland and Crosbie Smith, "The Transmission of Physics from France to Britain," *Historical Studies in the Physical Sciences*, 9 (1978), 1-62.

[22] John Heilbron, "Experimental natural philosophy, "The *ferment of knowledge: Studies in the Historiography of Eighteenth-Century Science*, edited by G. S. Rousseau and Roy Porter (Cambridge: Cambridge University Press, 1980), 360. See also Thomas L. Hankins, *Jean d'Alembert: Science and the Enlightenment* (Oxford, 1970), 4; Yehuda Elkana, "Newtonianism in the Eighteenth Century," *British Journal for the Philosophy of Science*, 22 (1971), 302-303; P. M. Heimann, "Newtonian Natural Philosophy and the Scientific Revolution," *History of Science*, 11 (1973), 1-7; and Robert E. Schofield, "An Evolutionary Taxonomy of Eighteenth-Century Newtonianism," *Studies in Eighteenth-Century Culture*, 7 (1978), 175. Heimann comments "Given the complexity of the Newtonian tradition and the varied and often contradictory nature of ideas apparently derived from the Newtonian corpus, great care is necessary if the term 'Newtonian' is not to be totally unilluminating and even misleading." Schofield similarly warns "We cannot identify, with any precision, a Newton doctrine; whatever Newtonian doctrines we can identify do not remain fixed, and however changing we permit our Newtonian doctrines to be, they are not of *general* influence."

[23] For an attempt to classify different "Newtonianisms" during the eighteenth century see Robert E. Schofield, "An Evolutionary Taxonomy of Eighteenth-Century Newtonianism," *Studies in Eighteenth-Century Culture*, 7 (1978), 175-192.

[24] Roderick W. Home, "Out of a Newtonian straitjacket: Alternative approaches to eighteenth-century physical science," in *Studies in the eighteenth century, IV: Papers presented at the fourth David Nichol Smith memorial seminar Canberra 1976* (Canberra, 1979), 243.

[25] Robert E. Schofield, "An Evolutionary Taxonomy of Eighteenth-Century Newtonianism," *Studies in Eighteenth-Century Culture*, 7 (1978), 184.

[26] Yehuda Elkana, "Newtonianism in the Eighteenth Century," *British Journal for the Philosophy of Science*, 22 (1971), 303.

[27] Home, "Out of a Newtonian straitjacket: Alternative approaches to eighteenth-century physical science," 243. See also Thomas L. Hankins, *Jean d'Alembert: Science and the Enlightenment* (Oxford: Oxford University Press, 1970), 3-10.

[28] Heilbron, "Experimental natural philosophy," 361.

[29] Geoffrey V. Sutton, *Science for a Polite Society, Gender, Culture, and the Demonstration of Enlightenment* (Boulder, CO: Westview Press, 1995), 12.

[30] Home, "Out of a Newtonian straitjacket: Alternative approaches to eighteenth-century physical science," 243.

[31] Ibid., 244.

[32] John Heilbron, *Elements of Early Modern Physics* (Berkeley: University of California Press, 1982), 10. See also R. W. Home, "The Notion of Experimental Physics in Early Eighteenth-Century France," in *Change and Progress in Modern Science*, edited by Joseph C. Pitt (Dordrecht: D. Reidel, 1985), 107-131.

[33] Benjamin Franklin to Peter Collinson, Letter XII. *Opinions and Conjectures, concerning the Properties and Effects of the Electrical Matter, arising from Experiments and Observations, made at Philadelphia*, (London: F. Newbery, 1774, 5th edition), in *Benjamin Franklin's Experiments*, prepared by I. Bernard Cohen (Cambridge, Mass., 1941), 273.

[34] Ibid., "Opinions and Conjectures, concerning the Properties and Effects of the electrical Matter . . .", 213. As Franklin explained in 1751: "Electrical matter differs from common matter in this, that the parts of the latter mutually attract, those of the former mutually repel each other. . . . But though the particles of electrical matter do repel each other, they are strongly attracted by all other matter. . . . Thus common matter is a kind of spunge to the electrical fluid."

[35] For the development of Franklin's theory and its reception, see R. W. Home, "Franklin's Electrical Atmospheres," *British Journal for the History of Science*, 6 (1972), 131-151.

[36] See Geoffrey V. Sutton, *Science for a Polite Society, Gender, Culture, and the Demonstration of Enlightenment* (Boulder, CO: Westview Press, 1995), 287-336.

[37] Heilbron, "Experimental natural philosophy," 369. On electricity and earthquakes see also Simon Schaffer, "Natural Philosophy and Public Spectacle in the Eighteenth Century," *History of Science*, 21 (1983), 15-21.

[38] Ibid.

[39] Robert H. Silliman, "Fresnel and the Emergence of Physics as a Discipline," *Historical Studies in the Physical Sciences*, 4 (1974), 137-143. See also Thomas L. Hankins, *Science and the Enlightenment* (Cambridge: Cambridge University Press, 1985), 10-13, 46-50 and P. M. Harman, *Energy, force, and matter: the conceptual development of nineteenth-century physics* (Cambridge: Cambridge University Press, 1982), 2-4, 12-21.

[40] Heilbron, "Experimental natural philosophy," 362.

[41] Ibid., 358. There are numerous books and articles dealing with the study of light, electricity, and heat from the eighteenth to nineteenth century. See the *Isis* cumulative bibliographies for specific examples.

[42] Ibid., 366-367.

[43] See Thomas L. Hankins, *Science and the Enlightenment* (Cambridge: Cambridge University Press, 1985), 50-80. Hankins spends the majority of a chapter on experimental physics discussing electricity, galvanism, and heat with only very brief mention of magnetism.

[44] See J. L. Heilbron, *Electricity in the 17th and 18th Centuries, a study of early modern physics* (Berkeley: University of California Press, 1979), 87-97; Roderick Weir Home, "Introduction," *Aepinus's essay on the theory of electricity and magnetism* (Princeton, N. J.: Princeton University Press, 1979); and Robert Palter, "Early Measurements of Magnetic Force," *Isis*, 63 (1972), 544-548.

[45] Julian A. Smith, "Precursors to Peregrinus: The early history of magnetism and the mariner's compass in Europe," *Journal of Medieval History*, 18 (1992), 21-74. Smith argues that Peregrinus did not originate many ideas commonly attributed to him, but coherently compiled ideas regarding magnetism in the thirteenth century. Given the lack of evidence of transmission from East to West, Smith treats the European development of the magnetic compass as independent from Chinese developments. For the ancient and medieval study of magnetism see also John B. Kramer, "The Early History of Magnetism," *Transactions of the Newcomen Society*, 14 (1933-34), 183-200; A. C. Mitchell, "Chapters in the History of Terrestrial Magnetism," *Terrestrial Magnetism and Atmospheric Electricity*, 37 (1932), 105-146, 42 (1937), 241-280, 44 (1939), 77-80. For the history of magnetism in China see Joseph Needham, *Science and civilization in China* (Cambridge: Cambridge University Press, 1962), vol. 4, part 1., 229-334 and vol. 4, part 3. (1971), 560-588.

[46] R. P. Multhauf and Gregory A. Good, *A Brief History of Geomagnetism and A Catalog of the Collections of the National Museum of American History* (Washington, D. C.: Smithsonian Institution Press, 1987), 4. Much of my general information on the history of geomagnetism is taken from this source.

[47] See J. A. Bennett, "The Mechanics' Philosophy and the Mechanical Philosophy," *History of Science*, 24 (1986), 12-28. Bennett considers the role of mechanics, particularly those working in navigational magnetism and connected with Gresham College, in the context of the developing mechanical philosophy of the seventeenth century.

[48] See Julian Smith, "Precursors to Peregrinus: The early history of magnetism and the mariner's compass in Europe," *Journal of Medieval History*, 18 (1992), 21-74 and Steven Pumfrey, "Geomagnetism: Theories of to 1800," 1995 [unpublished article].

[49] William Gilbert, *De Magnete* (New York: Dover, 1958) [reprint of P. Fleury Mottelay's translation published in 1893], Book 1, Chapter 1, 4-14.

[50] Ibid., author's preface, xlviii- xlix (my emphasis).

[51] Later writers dubbed Gilbert the "father of experimental philosophy", "father of the magnetic philosophy", "the Galileo of Magnetism," etc. See P. Fleury Mottelay, in William Gilbert, *De Magnete* (New York: Dover, 1958) [reprint of Mottelay's translation published in 1893], preface, v. and A. J. Snow, *Matter & Gravity in Newton's Physical Philosophy* (London: Humphrey Milford, 1926), 174, note 3.

[52] Gilbert, *De Magnete*, (New York: Dover, 1958) [reprint of Mottelay's translation published in 1893], Book 2, Chapter. 4, 105.

[53] Ibid.

[54] Ibid., 105-106.

[55] Sister Suzanne Kelly, "Gilbert, William," *DSB*, 5: 398.

[56] Ibid., 397. See also J. A. Bennett, "Cosmology and the Magnetical Philosophy, 1640-1680," *Journal for the History of Astronomy*, 12 (1981), 165-177.

[57] Gilbert, *De Magnete*, (New York: Dover, 1958) [reprint of Mottelay's translation published in 1893], Book 6, Chapter. 4, 328.

[58] See Ibid., Book 5, Ch. 2-3, 282-288. See also Gregory A. Good, "Follow the Needle: Seeking the Magnetic Poles," *Earth Sciences History*, 10 (1991), 156-157.

[59] Ibid., Book 6, Ch. 4, 332-333.

[60] Kepler to Maestlin (March 5, 1605), quoted in Job Kozhamthadam, S. J. *The Discovery of Kepler's Laws, the interaction of science, philosophy, and religion* (Notre Dame: The University of Notre Dame Press, 1994), 230.

[61] Kepler, quoted in Ibid., 232.

[62] Kepler, *Astronomia Nova* (1609), quoted in Ibid., 234. For discussion of the evolution of Kepler's ideas on these matters see, Job Kozhamthadam, S. J. *The Discovery of Kepler's Laws, the interaction of science, philosophy, and religion* (Notre Dame: The University of Notre Dame Press, 1994), 227-245 and Steven Pumfrey, "Magnetical philosophy and astronomy, 1600-1650," in *The General History of Astronomy, Vol. 2: Planetary Astronomy from the Renaissance to the Rise of Astrophysics, Part A: Tycho Brahe to Newton*, edited by René Taton and Curtis Wilson (Cambridge: Cambridge University Press, 1989), 48-49.

[63] Kepler quoted in M. Boas, *The scientific renaissance 1450-1630* (London, 1962), 301. For anti-Copernican and neo-Aristotelian challenges to Gilbert's theory, see Martha R. Baldwin, "Magnetism and the Anti-Copernican Polemic," *Journal for the History of Astronomy*, 16 (1985), 155-174, and Steven Pumfrey, "Neo-Aristotelianism and the magnetic philosophy," in *New Perspectives on Renaissance Thought*, edited by John Henry and Sarah Hutton (London: Duckworth, 1990), 177-189.

[64] Galileo Galilei, *Dialogue on the Great World Systems*, in the Salusbury Translation, revised and annotated by Giorgio de Santillana (Chicago: University of Chicago Press, 1953), 409. See Martha R. Baldwin, "Magnetism and the Anti-Copernican Polemic," *Journal for the History of Astronomy*, 16 (1985), 156.

[65] Ibid., 407-408. See also Steven Pumfrey, "Magnetical philosophy and astronomy, 1600-1650," in *The General History of Astronomy, Vol. 2: Planetary Astronomy from the Renaissance to the Rise of Astrophysics, Part A: Tycho Brahe to Newton.* edited by René Taton and Curtis Wilson (Cambridge: Cambridge University Press, 1989), 49-50.

[66] Ibid., 415. Galileo also performed experiments constructing magnets and capped lodestones the 1600s. See Annibale Fantoli, *Galileo, For Copernicanism and for the Church*, translation by George V. Coyne, S. J. (Studi Galileani, vol. 3, 1994), 80.

[67] Francis Bacon, *Historia gravis et levis* (1622) quoted by Sister Suzanne Kelly, "Gilbert's influence on Bacon: a revaluation," *Physis*, 5 (1963), 251.

[68] Sister Suzanne Kelly, "Gilbert's influence on Bacon: a revaluation," *Physis*, 5 (1963), 257. For examples of those more directly familiar with Gilbert's work see Mark Ridley, *A Short Treatise of Magnetical Bodies and Motions* (London: Nicholas Okes, 1613); Mark Ridley, *Magnetical Animadversions* (London: Nicholas Okes, 1617); William Barlowe, *Magneticall Advertisements: or, Divers pertinent observations, and approved experiments concerning the nature and properties of the load-stone . . .* (London: Edward Griffin, 1616); and William Barlowe, *A Breife Discovery of the Idle Animadversions of Mark Ridley* (London: Edward Griffin, 1618). Ridley and Barlowe agreed with Gilbert's conclusion that the earth was a giant lodestone, yet disagreed on his cosmological arguments, especially the earth's diurnal rotation. For Gilbert's influence see also Francis R. Johnson, *Astronomical Thought in Renaissance England, a study of the English Scientific Writings from 1500 to 1645* (Baltimore: Johns Hopkins Press, 1937), 231-247.

[69] Francis R. Johnson, "Gresham College: Precursor of the Royal Society," *Journal of the History of Ideas*, 1 (1940), 433-434. For a social constructionist' discussion of Gellibrand's discovery see Steven Pumfrey, "'O tempora, O magnes!' A sociological analysis of the discovery of secular magnetic variation in 1634," *British Journal of the History of Science*, 22 (1989), 181-214.

[70] Henry Gellibrand, *The Variation of the Magneticall Needle* (1635) quoted in Johnson, "Gresham College: Precursor of the Royal Society," *Journal of the History of Ideas*, 1 (1940), 433.

[71] F. R. Johnson, "Gresham College: Precursor of the Royal Society," *Journal of the History of Ideas*, 1 (1940), 430-432 and Pumfrey, "'O tempora, O magnes!' A sociological analysis of the discovery of secular magnetic variation in 1634," *British Journal of the History of Science*, 22 (1989), 189-190. See also Steven Pumfrey, "Magnetical philosophy and astronomy, 1600-1650," in *The General History of Astronomy, Vol. 2: Planetary Astronomy from the Renaissance to the Rise of Astrophysics, Part A: Tycho Brahe to Newton.* edited by René Taton and Curtis Wilson (Cambridge: Cambridge University Press, 1989), 53.

[72] Thomas Hobbes read *Longitude Found* and wrote a commentary criticizing Bond's theory. See Thomas Hobbes, Chapter IX "Of the Loadstone, and its Poles, and whether they show the longitude of places on the Earth," *Decameron Physiologium; or, Ten Dialogues of Natural Philosophy*, in *The English Works of Thomas Hobbes . . .* (London: John Bohn, 1839) [Second Reprint, Germany: Scientia Verlag Aalen, 1966] 7: 155-168.

[73] Steven Pumfrey, "Mechanizing Magnetism in Restoration England— the Decline of Magnetic Philosophy," *Annals of Science*, 44 (1987) 8, 17-20.

[74] John Henry, "The Scientific Revolution in England," in *The Scientific Revolution in National Context*. edited by Roy Porter and Mikulás Teich (Cambridge: Cambridge University Press, 1992), 184. See also J. A. Bennett, "Cosmology and Magnetical Philosophy, 1640-1680," *Journal of the History of Astronomy*, 12 (1981) 165-177. For a broader discussion of the mechanical philosophy, with particular emphasis on Robert Boyle's "corpuscular philosophy", see Marie Boas, "The Establishment of the Mechanical Philosophy," *Osiris*, 10 (1952), 412-541.

[75] William R. Shea, *The Magic of Numbers and Motion, The Scientific Career of René Descartes* (Science History Publications, U. S. A., 1991), 143, 302-305.

[76] René Descartes, *Principles of philosophy*, translated by Valentine Rodger Miller and Reese P. Miller (Dordrecht, Holland: Reidel, 1983), 242-244.

[77] All three Cartesian elements were composed of the same kind of matter. Descartes defined space as the extension of matter, thereby he rejected the existence of a void. The third element (corresponding to Earth) was of the largest particles composing the bulk of ordinary bodies. The second element (corresponding to Air), or *matière subtile*, was smaller and generally spherical. This subtle matter, later called the ether, filled in as much of the interstices as possible. The first, all-pervasive element (corresponding to Fire) was indefinite in shape and moved very swiftly. It guaranteed the plenum or continuity of matter by filling in all of the remaining pores.

[78] Steven Pumfrey, "Mechanizing Magnetism in Restoration England — the Decline of Magnetic Philosophy," *Annals of Science*, 44 (1987), 6.

[79] Descartes, *Principles of philosophy*, 256-257.

[80] For the reception of Descartes in England from the 1640s to the early eighteenth-century, see Sterling P. Lamprecht, "The Role of Descartes in Seventeenth-Century England," *Studies in the History of Ideas* (New York: Columbia University Press, 1935), III: 181-240.

[81] Robert Hooke, *Micrographia, or Some Physiological Descriptions of Minute Bodies . . .* (London: 1665), preface. Quoted in Steven Pumfrey, "Mechanizing Magnetism in Restoration England— the Decline of Magnetic Philosophy," *Annals of Science*, 44 (1987), 2.

[82] Robert Boyle. "Experiments and notes, about the mechanical origin or production of electricity," *The Works of the Honourable Robert Boyle* (London: Printed for A. Millar, 1744), 3: 647-648.

[83] Robert Boyle. *Spring and Weight of Air*, *Works*, I: 11-12. Quoted in Marie Boas, "The Establishment of the Mechanical Philosophy," *Osiris*, 10 (1952), 448-449.

[84] Piero E. Ariotti, "Benedetto Castelli's *Discourse on the loadstone* (1639-1640): the Origin of the Notion of Elementary Magnets Similarly Aligned," *Annals of Science*, 38 (1981), 128-130. Ariotti gives excellent summaries of Descartes' and Gassendi's mechanistic magnetic theories.

[85] See Steven Pumfrey, "Mechanizing Magnetism in Restoration England— the Decline of Magnetic Philosophy," *Annals of Science*, 44 (1987) 4, 10-11, 18. See also J. A. Bennett, "Magnetical Philosophy and Astronomy from Wilkins to Hooke," in *The General History of Astronomy, Vol. 2: Planetary Astronomy from the Renaissance to the Rise of Astrophysics, Part A: Tycho Brahe to Newton*. edited by René Taton and Curtis Wilson (Cambridge: Cambridge University Press, 1989), 222-230.

[86] Thomas Hobbes, *Elements of Philosophy*, Part IV, in *The English Works of Thomas Hobbes* . . . (London: John Bohn, 1839) [Second Reprint, Germany: Scientia Verlag Aalen, 1966], 1: 430 [my emphasis].

[87] Ibid., 527-528. Hobbes explained: "For, if in the loadstone there be supposed such reciprocal motion, or motion of the parts forwards and backwards, it will follow that the like motion will be propagated by the air to the iron, and consequently that there will be in all the parts of the iron the same reciprocations or motions forwards and backwards. And from hence also it will follow, that the intermediate air between the stone and the iron will, be little and little, be thrust away; and the air being thrust away, the bodies of the loadstone and the iron will necessarily come together."

[88] See J. A. Bennett, "Cosmology and the Magnetical Philosophy, 1640-1680," *Journal for the History of Astronomy*, 12 (1981), 165-177.

[89] Ibid., Bennett shows the coexisting tensions between the mechanical philosophy and the magnetical philosophy in the works of Christopher Wren, John Wallis, and Robert Hooke.

[90] Robert Boyle, *Experiments and Notes about the Mechanical Production of Magnetism* (London: E. Flesher, 1676) [Oxford: Old Ashmolean Reprints, 1927], 1. See also Boyle, *Mechanical Qualities* (1675), *Works* IV, 235.

[91] Robert Boyle, "Of the Systematical or Cosmical Qualities of Things," *The Works of the Honourable Robert Boyle* (London: Printed for A. Millar, 1744), 3: 84. Boyle's fellow corpuscularian, William Petty developed a theory of magnetism in which each atom acted as a small magnet. Petty, even more so than Boyle, retained occult qualities in his description. He also "supposed . . . that Atoms are also Male and Female, and the Active and Susceptive Principles of all things." See Marie Boas, "The Establishment of the Mechanical Philosophy," *Osiris*, 10 (1952), 481.

[92] Ibid.

[93] For more on Hooke's curatorship and magnetic research, see Stephen Pumfrey, "Ideas above his station: a social study of Hooke's Curatorship of Experiments," *History of Science*, 29 (1991), 15-44. See also M. Hunter and S. Schaffer (eds.). *Robert Hooke: New studies* (Woodbridge, Suffolk, 1990) and M. Hunter, *Establishing the New Science: the experience of the early Royal Society* (Woodbridge, Suffolk, 1989).

[94] Thomas Birch, *The History of the Royal Society for improving of natural knowledge* (London: Printed for A. Millar, 1757), 3: 124.

[95] Robert Hooke, *The Posthumous Works of Robert Hooke*, edited by Richard Waller (London: Printed by Sam. Smith and Benj. Walford, 1705), 192.

[96] Robert Hooke, *Lectures and Collections* (London: 1678) quoted in J. A. Bennett, "Cosmology and the Magnetical Philosophy, 1640-1680," *Journal for the History of Astronomy*, 12 (1981), 174.

[97] Ibid.

[98] Robert Hooke, *The Posthumous Works of Robert Hooke*, edited by Richard Waller (London: Printed by Sam. Smith and Benj. Walford, 1705), 483. Quoted in S. Pumfrey, "Mechanizing Magnetism in Restoration England— the Decline of Magnetic Philosophy," *Annals of Science*, 44 (1987), 21.

[99] Robert Boyle, *Tracts written by the Honourable Robert Boyle About the Cosmicall Qualities of Things . . .* (Oxford, 1670), 12 and Thomas Birch, *The History of the Royal Society for improving of natural knowledge* (London: Printed for A. Millar, 1757), 3: 131.

[100] See Edmond Halley, "A Theory of the Variation of the Magnetical Compass," *Philosophical Transactions of the Royal Society of London*, 12 (1683), 208-221 and "An Account of the cause of the Change of the Variation of the Magnetical Needle, with an Hypothesis of the Structure of the Internal parts of the Earth," *Philosophical Transactions of the Royal Society of London*, 21(1692), 563-578. See also Michael E. Evans, "Edmond Halley, Geophysicist," *Physics Today*, 41 (1988), 41-45, and N. Kollerstrom, "The Hollow World of Edmond Halley," *Journal for the History of Astronomy*, 23 (1992), 185-192.

[101] Halley, "A Theory of the Variation of the Magnetical Compass," *Philosophical Transactions of the Royal Society of London*, 12 (1683), 209.

[102] Ibid., 221.

[103] Norman J. W. Thrower, "Introduction," *The Three Voyages of Edmond Halley in the Paramore, 1698-1701* (London: Hakluyt Society, 1981), 29-35. Halley's instructions specifically said "in all the Course of your Voyage, you must be carefull to

omit no opportunity of Noteing the variation of the Compasse, of which you are to keep a Register in your lournall." See "Admiralty's Instructions to Halley (October 1698)," in *Correspondence and Papers of Edmond Halley*. . . arranged and edited by Eugene Fairfield MacPike (Oxford: Clarendon Press, 1932), 243.

[104] Edmond Halley, "Description to accompany Halley's Atlantic Chart . . . 1700," in *The Three Voyages of Edmond Halley in the Paramore, 1698-1701*, edited by Norman Thrower (London: Hakluyt Society, 1981), 365.

[105] Pumfrey, "Mechanizing Magnetism in Restoration England— the Decline of Magnetic Philosophy," *Annals of Science*, 44 (1987), 20.

[106] For more on Halley's voyages and charts see L. A. Bauer, "Magnetic Results of Halley's Expedition, 1698-1700," *Terrestrial Magnetism and Atmospheric Electricity*. 18 (March-December, 1913), 113-126 and D. W. Waters, "Captain Edmond Halley, F. R. S., Royal Navy, and the Practice of Navigation," in *Standing on the Shoulders of Giants: A Longer View of Newton and Halley*, edited by Norman Thrower (Berkeley: University of California Press, 1990), 171-202.

[107] See for example William Whiston, *The Longitude and Latitude found by the Inclinatory Dipping Needle* (London, 1721); Thomas Harding, "Observations on the Variation of the Needle," *Transactions of the Royal Irish Academy*, 4 (1790), 107-118; and P. R. Nugent, "A new Theory, pointing out the Situation of the Magnetic Poles, and a Method of discovering the Longitude," *Philosophical Magazine*, 5 (1800), 378-392.

[108] Isaac Newton, *Mathematical Principles of Natural Philosophy*, translated by Andrew Motte, revised by Florian Cajori (Berkeley: 1934), 414.

[109] See Francis Hauksbee, "An Account of Experiments concerning the Proportion of the Power of the Load-stone at different Distances," *Philosophical Transactions of the Royal Society of London*, 27 (1710-12), 506-511 and Brook Taylor, "An Account of an Experiment made . . . in order to discover the Law of Magnetical Attraction," *Philosophical Transactions of the Royal Society of London*, 29 (1714-16), 294-295.

[110] See Brook Taylor, "An Account of some Experiments relating to Magnetism," *Philosophical Transactions of the Royal Society of London*, 31 (1720-21), 204-208.

[111] For the search for the magnetic force law and its pitfalls, see Robert Palter, "Early Measurements of Magnetic Force," *Isis*, 63 (1972), 544-558; Roderick W. Home, "XVIII. Physical Principles and the Possibility of a Mathematical Science of Electricity and Magnetism," originally in *Siméon-Denis Poisson et la science de son temps*, edited by Michel Métivier, Pierre Costabel, and Pierre Dugac (Paris: École Polytechnique, 1981) in *Electricity and Experimental Physics in Eighteenth-Century Europe* (Great Britain: Variorum, Bookcraft Ltd., 1992), 151-166; J. L. Heilbron, *Electricity in the 17th and 18th Centuries, a study of early modern physics* (Berkeley: University of California Press, 1979), 87-97; and J. L. Heilbron, *Physics at the Royal Society during Newton's Presidency*. (University of California, Los Angeles: William Andrews Clark Memorial Library, 1983), 73-77.

[112] Robert Palter, "Early Measurements of Magnetic Force," *Isis*, 63 (1972), 550.

[113] R. P. Multhauf and Gregory A. Good, *A Brief History of Geomagnetism and A Catalog of the Collections of the National Museum of American History* (Washington, D. C.: Smithsonian Institution Press, 1987), 4. See George Graham, "An Account of Observations Made of Variation of the Horizontal Needle at London . . . 1722-23," *Philosophical Transactions of the Royal Society of London*, 33 (1724) 96-107.

[114] John Canton, "An attempt to account for the regular diurnal variation of the horizontal magnetic needle; and also for its irregular variation at the time of the aurora borealis," *Philosophical Transactions of the Royal Society of London*, 51 pt. 1 (1759) 398-399.

[115] See Roderick Home, "'Newtonianism' and the theory of the magnet," *History of Science*, 15 (1977), 252-266.

[116] Isaac Newton, *Opticks: or, a Treatise of the Reflections, Refractions, Inflections and Colours of Light* (Fourth Edition, London: William Innys, 1730) [Reprint, London: G. Bell & Sons, Ltd., 1931], 375-376.

[117] Ibid.

[118] R. W. Home, Introduction, *Aepinus's essay on the theory of electricity and magnetism* (Princeton, N. J.: Princeton University Press, 1979), 152-154. Home convincingly argues that if Newton's faith in circulating effluvia wavered it did so only once in early 1690s, a period in which Newton seemingly abandoned all attempts to explain certain phenomena mechanically.

[119] R. W. Home, "'Newtonianism' and the theory of the magnet," *History of Science* 15 (1977), 252-266. See Colin Maclaurin, *An Account of Sir Isaac Newton's Philosophical Discoveries* (London: A. Millar, J. Nourse, 1748) [Landmarks of science microform, 1970], 108-109. Maclaurin remarked: "The rays of light find a passage through a glass globe in all directions, which argues the great rarity of the globe, as well as the subtilty of the light. The same is to be said of the magnetic and electric *effluvia*, and of the subtile matter that pervades the pores of bodies with great freedom in chymical experiments."

[120] John Desaguliers to Hans Sloane, March 14, 1730/1. Royal Society MSS., Register Book (C), XV, 255-258. Quoted in Larry Stewart, *The Rise of Public Science, Rhetoric, Technology, and Natural Philosophy in Newtonian Britain, 1660-1750* (Cambridge: Cambridge University Press, 1992), 129.

[121] John Keill, *An Introduction to Natural Philosophy; or, Philosophical Lectures Read in the University of Oxford, anno. Dom. 1700* . . . (London: W. and J. Innys, and J. Osborn, 1720) [Landmarks of science microform, 1969], 100.

[122] Home, Introduction, *Aepinus's essay on the theory of electricity and magnetism* (Princeton, N. J.: Princeton University Press, 1979), 155-159.

[123] G. W. Leibniz to Gilles Filleau des Billettes, December 1696, in *Philosophical Papers and Letters*, a selection translated and edited, with an introduction by Leroy E. Loemker (Chicago: University of Chicago Press, 1956), 2: 773.

[124] John Harris, *Lexicon Technicum: or, an Universal English Dictionary of Arts and Sciences* (London: Printed for Dan. Brown, Tim. Goodwin, John Walthoe, etc . . ., 1704), vol. 1, art. "Magnet." The fourth edition (1725) included the same article. See John Harris, *Lexicon Technicum: or, an Universal English Dictionary of Arts and Sciences* (London: Printed for D. Browne, J. Walthoe, J. Knapton, etc . . ., 1725), vol. 1, art. "Magnet."

[125] See Jacques Rohault, *A System of Natural Philosophy* (London: James Knapton, 1723), a facsimile of the edition and translation by John and Samuel Clarke, with introduction by L. L. Laudan, [New York: Johnson Reprint Corporation], vol. 2: 163-187. Clarke's notes to Rohault's chapter on magnetism consisted of merely three unimportant glosses. See R. W. Home, "'Newtonianism' and the theory of the magnet." *History of Science*, 15 (1977), 260.

[126] Ephraim Chambers, *Cyclopaedia: or, an Universal Dictionary of Arts and Sciences* (London, 1741-1743, 5th edition), art. "Magnetism," n. p. Quoted in R. W. Home, Introduction, *Aepinus's essay on the theory of electricity and magnetism*, 158. See also Ephraim Chambers, *Cyclopaedia: or, an Universal Dictionary of Arts and Sciences* (London: Printed for W. Innys, J. & P. Knapton, etc . . ., 1752, 7th edition), art. "Magnetism," n. p.

[127] Gowin Knight, *Philosophical Transactions of the Royal Society of London* 44 (1748), 656-664. Quoted in R. W. Home, Introduction, *Aepinus's essay on the theory of electricity and magnetism*, 159.

[128] See R. W. Home, Introduction, *Aepinus's essay on the theory of electricity and magnetism* (Princeton, N. J.: Princeton University Press, 1979), 144-150. For citations to the original works see Ibid., notes 11 and 12, 145.

[129] R. W. Home, "XVIII. Physical Principles and the Possibility of a Mathematical Science of Electricity and Magnetism," originally in *Siméon-Denis Poisson et la science de son temps*, edited by Michel Métivier, Pierre Costabel, and Pierre Dugac (Paris: École Polytechnique, 1981) in *Electricity and Experimental Physics in Eighteenth-Century Europe* (Great Britain: Variorum, Bookcraft Ltd., 1992), 158.

CHAPTER 2:
GLOBAL MAGNETIC COLLECTING
(c.1750-1835)

Throughout the eighteenth century the British traveled the globe primarily in the interest of their colonies. First and foremost they sought to build and sustain the British Empire through naval and commercial might. In addition, world exploration allowed increased opportunities for enhancing royal and national prestige, expanding geographic knowledge, and collecting a wide range of scientific information. During the second half of the eighteenth century a broad-based "quantifying spirit" took root in Europe. Investigators in Britain and elsewhere enthusiastically assigned numbers to all types of natural phenomena and human activities. At the turn of the century, quantification and measurement affected such diverse areas as experimental physics, meteorology, geodesy, chemistry, forestry, and political economy.[1] Exemplifying the desire to quantify, British naval explorations of the late eighteenth century gathered, measured, and classified data of all kinds— astronomical, ethnological, meteorological, geographical, and natural historical.

In addition, this "quantifying spirit" or impulse to measure accompanied an urge to build better more precise instruments including magnetic instruments. Although few natural philosophers and instrument makers actually collected terrestrial magnetic data, many saw fit to design, construct, and test magnetic instruments— azimuth compasses, dipping needles, variation compasses, etc. Ultimately, most instrument designers and builders depended on navigators to provide them with global magnetic data. As they had since the sixteenth century, eighteenth-century mariners measured magnetic variation or declination to correct their courses. Ships' logbooks and journals routinely recorded variation along with latitude, longitude, temperature, wind, and weather conditions. The less frequently recorded magnetic dip had little practical utility. As such, variation dominated the magnetic data collected during the eighteenth century. A third magnetic component, magnetic intensity was not measured until the late eighteenth century in France and the early nineteenth century in Britain. Counting the vibrations or oscillations of a suspended magnetic needle within a given amount of time determined relative magnetic force or intensity at different geographic locations. Intensity, however, did not became important to British collecting efforts until the 1820s.

Regardless of continuing interest in magnetism during the eighteenth century, the nineteenth century witnessed a marked acceleration in the study of magnetism and terrestrial magnetism. For instance, the *Philosophical Transactions of the Royal Society of London* illustrated the growing interest in the study of magnetism and geomagnetism after 1820. Between 1781 and 1820 there were only a dozen papers published dealing with magnetic topics, yet from 1821 to 1830 there are nearly forty papers related to magnets and magnetism.[2] Several early nineteenth-century developments contributed to this reawakened activity. First, the recognition of ship magnetism led to a variety of considerations regarding navigation by compass and the collection of magnetic data. Second, the renewed search for the North-west passage after the Napoleonic wars gave impetus to the search for the north magnetic pole. It also led Arctic explorers to higher and higher latitudes where they observed the curious behavior of the compass. Third, the possible connections between magnetism and other forces of nature gave great impetus to the experimental study of magnetic effects, particularly after the discovery of electromagnetism in 1820. Though clearly reflected in navigational writings, this last development pertained primarily to experimental physics treated in the final chapter.

Although eighteenth-century British voyages gathered magnetic measurements, the study of magnetism generally received less attention than other areas. This general trend held true into the early nineteenth century. Within the context of global exploration and scientific collecting, this chapter examines the changing motives for gathering magnetic data. Particularly, it discusses the shifting reasons for measuring terrestrial magnetic phenomena from 1750 to 1835. The respective roles of navigators, natural philosophers, and instrument makers are also examined. As a rule, navigators gathered terrestrial magnetic data, instrument makers designed and constructed instruments, and natural philosophers analyzed and interpreted the amassed data. With few exceptions, this pattern held for most of the period.

Magnetic Collecting and Eighteenth-Century Voyages

Since the sixteenth century, navigators had recorded magnetic variation for correcting the course of their ships. Though this practice continued, the effort to measure variation and other scientific data gradually intensified in the eighteenth century. Changing motives and methods of collecting magnetic data become clear when the voyages of the late eighteenth century are examined, particularly those of Captain James Cook and Captain Constantine John Phipps. Cook, Phipps, and others carried with them the best available scientific instruments including magnetic apparatus. As well, trained astronomers accompanied many of these voyages. Both navigators and their astronomers had more scientific training than earlier navigators. Magnetism, nevertheless, gained less attention than other areas. Though the quest to determine longitude by magnetic means persisted, it had all but disappeared at the turn of the century.

Few eighteenth-century natural philosophers pursued Halley's plan of determining longitude by the periodic updating of variation charts. With little support, Newton's successor at Cambridge, William Whiston, sought to use maps of magnetic dip for determining both longitude and latitude.[3] Throughout the century, longitude schemes akin to Halley's and Whiston's failed to gain any large following. In the 1740s, Fellows of the Royal Society, William Mountaine and James Dodson, advertised requests for magnetic data which met with little interest. Wishing to update and extend the scope of Halley's charts, they explained their motives in 1755:

> The advantage that will arise by extending the variation lines over the land, as well as sea, will be the confirmation of those drawn over the waters; the continuation of which, from sea to sea, will be thereby conspicuous, and we shall be enabled to judge better of their nature, properties and causes; and, if the same can be extended over all the parts of the known world, the eye will be presented, at one view, with the different degrees of attraction, with which all the parts of this great magnet are endued, at the time when such lines are drawn.[4]

Seeking to benefit trade, navigation, and natural knowledge, Mountaine and Dodson presented a set of tables to the Royal Society of London in 1757. Compiled from approximately 50,000 observations in the log books and journals of the Royal Navy, the East India Company, the Hudson's Bay Company, and several individuals, these tables illustrated the distribution of magnetic variation for the years 1710, 1720, 1730, 1744, and 1756. In contrast to their stated goals, however, only the measurements of Royal Observatory astronomer, James Bradley, were land-based. Until the nineteenth century, extensive land-based magnetic observations remained a rarity in Britain.

While complaining of limited and deficient information, Mountaine and Dodson limited their efforts to compiling data "without attempting to introduce any hypothesis."[5] They supposed that periodic compilations of magnetic data might allow future philosophers to discover the rules of secular and diurnal variation. Though venturing no hypothesis for these rules, Mountaine and Dodson supposed earthly magnetism "influenced by various and different magnetic attractions, in all probability occasioned by the heterogeneous compositions in the great magnet, the *Earth.*"[6] Hence, they, like most others at mid-century, accepted the Gilbertian notion of giant terrestrial magnet.

Despite Mountaine and Dodson's more purely scientific objectives, the primary reason for understanding magnetic changes for them and others in the eighteenth century remained the improvement of navigational science. Practical application drove the majority of those interested in updating magnetic charts. For instance, in 1763, a ship's surgeon David Ross sent variation measurements to Mountaine and noted them "of great service to philosophy in general, but particularly to navigation, as in future ages they may serve as a basis, to found its theory upon."[7] In 1776, Mountaine sent a set of earlier observations to Astronomer Royal Nevil Maskelyne. He reiterated his motivations to Maskelyne:

> the discovery of that law [of secular variation] must greatly depend upon such comparisons made from multitudes of good observations taken at different periods, and those over the whole face of the terraqueous globe; but until that law is certainly known, charts can be constructed only from time to time from the latest observations.[8]

More than half a century later, Mountaine's vision for the regular global collection of magnetic data began to take shape. By then, the motives behind such massive efforts had shifted with navigational application taking subsidiary importance.

In contrast to Halley's efforts, most eighteenth-century natural philosophers left the measurement of terrestrial magnetic phenomena to experienced mariners. Natural philosophers and instrument makers stuck to the tasks of designing, constructing, and testing magnetic instruments. For instance, in the 1760s, the magnetic experimenter, Gowin Knight, designed an improved azimuth compass which stood until the early nineteenth century as the standard for Navy use.[9] Similarly, London instrument maker Edward Nairne designed the standard dipping needle. Nairne determined the dip for London in 1772, yet did not use his instrument on a regular basis.[10] Historian Patricia Fara characterized the division as an "unequal distribution of magnetic knowledge" between maritime practitioners, who desired sea-based practical navigational techniques and natural philosophers, who sought land-based theoretical knowledge of magnetism.[11] Though the practical and theoretical interests of mariners and natural philosophers overlapped, the participants tended to stress one aspect or the other in their work.

During the last quarter of the eighteenth century, instrument makers and natural philosophers continued designing magnetic instruments for navigational use. In 1776, Henry Cavendish tested Naime's dipping needle and several meteorological instruments at the Royal Society.[12] Though the Royal Society kept a meteorological journal with tables of magnetic variation and dip, such data, unlike temperature or atmospheric pressure, were not collected on a regular monthly basis.[13] Cavendish's concerns lay in testing the instrument's accuracy and describing possible sources of error, not in prolonged, systematic geomagnetic observation. Comparing indoor and outdoor readings of dip, he determined that errors arose from ironwork in the apartments of the Royal Society. Describing additional sources of error, he concluded that Nairne's dipping needle was at least as exact, if not more so, than any previous instrument. He also described a method for observing dip in which the instrument was rotated 180°, the observations repeated, and then the poles of the needle were reversed, and observations again repeated. Deferring to Cavendish as an authority, navigators often used this technique to determine the "true dip."[14]

While natural philosophers and instrument makers designed and tested instruments, officers in the British Royal Navy put them to use around the globe. A series of naval expeditions in the 1760s and 1770s allowed for observation and measurement on a grand scale. In spite of the increasingly scientific tone of these voyages, promoting British sea power, commerce, national pride, and geographic knowledge remained the traditional objectives.[15] Illustrating these concerns in 1764, Commodore John Byron's official instructions for a voyage to the Pacific noted:

> Whereas nothing can redound more to the honour of this nation, as a maritime power, to the dignity of the Crown of Great Britain, and to the advancement of the trade and navigation thereof; and whereas there is reason to believe that lands and islands of great extent, hitherto unvisited by any European power, may be found in . . . within latitudes convenient for navigation, and in climates adapted to the produce of commodities useful in commerce . . . his Majesty . . . has thought fit that it [a voyage] should now be undertaken.[16]

For a young King George III, expanding British dominion and exploring unknown portions of the globe, particularly the vast Pacific Ocean, remained of great importance.[17]

Geographical motives, among others, overshadowed the collection of magnetic data during eighteenth-century explorations.[18] Geography, astronomy, longitude, and natural history garnered the lion's share of attention.[19] Following the example of James Cook, Royal Navy officers frequently had the necessary skills for careful observing or learned them on the job. Indeed, Cook's predecessors had lacked the mathematical and astronomical training and the technical skills which he and later naval officers often possessed. In 1768, Cook received detailed instructions from the Board of Longitude drawn up by Astronomer Royal, Nevil Maskelyne. Not surprisingly, astronomical items related to longitude determination such as lunar observations and the eclipses of Jupiter's satellites dominated the list. Taking lower priority were magnetic measurements, appearing twelfth out of fifteen items in Maskelyne's instructions.[20]

Most of the essential data directly related to the determination of longitude. To assist the officers in these tasks, the Board of Longitude appointed astronomers to collect and measure astronomical data. Regarding the numerous lunar observations of appointed astronomer Charles Green, Cook commented:

> [Mr. Green] was Indefatigable in making and calculating these observations . . . by his Instructions several of the Petty officers can make and Calculate these observations almost as well as himself: it is only by such means that this method of finding Longitude at Sea can be put into universal practice.[21]

On Cook's first Pacific voyage (1768-1771) and most others of the late eighteenth century the accurate determination of longitude remained the primary navigational concern.

Earlier in the century, the quest for longitude had been stimulated when Parliament passed the Longitude Act of 1714. This legislation not only established the Board of Longitude, but also offered enormous monetary rewards for a reliable method of finding longitude— £10,000 within sixty miles, £15,000 within forty miles, and

£20,000 within thirty miles.[22] Throughout the eighteenth century, finding accurate methods of determining longitude retained a singular significance, particularly for a maritime nation such as Britain.

In addition to the quest for longitude, observing the transit of Venus held enormous scientific importance for Cook's initial voyage.[23] Indicative of the transit's importance, orders for its observation on the South Pacific island of Tahiti appeared first in Cook's official instructions from the Admiralty. After disappointing observations of the transit of 1761, astronomers hoped that observing the transit of 1769 (the last opportunity to observe such an event for over a century) from different positions would allow an accurate determination of the distance between the earth and sun.[24] Cook and Green, upon returning to England in 1771, reported transit observations in great detail to the Royal Society of London.[25] Though their reports also included tables of magnetic dip and variation, astronomy clearly held the greater importance.

Under the aegis of the Board of Longitude, Maskelyne's instructions for Cook's second Pacific voyage (1772-75) followed a similar pattern. Longitude and astronomical observations again dominated the list. As a perennial navigational problem, the determination of longitude saw the emergence of two practicable solutions in the late eighteenth century. The method of lunar distances, refined and advocated by Nevil Maskelyne, used the moon's motion against a background of stars as a time keeping device. This method depended upon numerous observations and corrections, intricate mathematical calculations, and accurate lunar tables. A simpler method relied on the development of an accurate marine chronometer.[26] By the early nineteenth century chronometers were commonly used for determining longitude. The method, in principle, was simple since it required comparing local time with Greenwich time kept by the chronometer. Each hour difference in time represented 15° difference in longitude as measured east or west from Greenwich. Replacing more complex and inaccurate methods, determining longitude by chronometer made possible safer, easier navigation. In fact, by the 1820s chronometers became widely available and Parliament abolished the Board of Longitude in 1828.

Illustrating longitude's importance in the eighteenth century, Maskelyne first ordered Cook's appointed astronomers, William Wales (1734-1798) and William Bayly, to thoroughly test a copy of the chronometer designed by carpenter turned clockmaker, John Harrison.[27] In addition, Maskelyne's instructions included numerous meteorological and astronomical observations and pendulum observations for determining the figure of the earth.[28] While the Royal Society and Board of Longitude provided magnetic apparatus made by Nairne, Adams, and other instrument makers, magnetic observations received only one line in the instructions to "observe, or assist at the Observations of the variation of the Compass; and observe the inclination of the Magnetic Dipping needle from time to time."[29] Maskelyne's phrasing again indicated the lesser importance of magnetism.

During all of his journeys, Cook exhibited the spirit of quantification and this spirit's connection to improved instrumentation. In 1773, one of Cook's journal entries made this link apparent:

> Such are the improvements Navigation has received from Astronomers of this Age, by the Valuable Table they have communicated to the Publick under the direction of the Board of Longitude contained in the Astronomical Ephemeris and the Tables for correcting the Apparent Distance of the Moon and a Star from the effects of Refraction and Parallax . . . Much Credet [sic] is also due to the Mathematical Instrument makers for the improvements and accuracy with which they make their Instruments, for without good Instruments the Tables would loose [sic] part of their use.[30]

However, though astronomical instrumentation gained great praise, magnetic instruments were frequently condemned. Finding the magnetic variation on a moving ship had never been a simple task. In *De Magnete,* William Gilbert had remarked, "Even expert navigators find it very difficult to observe the variation at sea on account of the ship's motions and her tossing in every direction, though they may employ the best instruments yet devised and in use."[31] Regardless of attempts to improve azimuth compasses and other magnetic instruments, similar complaints continued in the eighteenth century.

Finding magnetic variation required determining the altitude and bearing of a celestial object, usually the sun. In addition, it involved corrections for atmospheric refraction and quite a bit of mathematical calculation. Exacerbating matters, variation measurements depended on a variety of circumstances including the geographic location, the time of day, the placement of the compass on the ship, the same compass on another ship, and different compasses used on the same ship. Illustrating the difficulties, Cook remarked in 1774:

> Sence [sic] we have been a Mongest [sic] these Islands, we have found it difficult to determine the Variation with accuracy. Our Compasses have given from 8° to 12° the same Compass would vary so much on different days and even between the morning and evening of the same day, when the Ship's Change of Situation has been but very little.[32]

The article, "Variation," in the second edition of *Encyclopaedia Britannica* (1778-83) similarly lamented that because variation observations were "loose and inaccurate," it was impossible to represent them with precision.[33]

As a final difficulty, magnetic variation changed depending on the ship's head or direction of travel.[34] Though the cause of this phenomenon remained a mystery until the early nineteenth century, Cook's second voyage recorded numerous instances of it. Cook, for example, remarked in 1773, "this was not the first time we had made this observation, without being able to account for it."[35] Several months later, his second lieutenant, Charles Clerke, noted:

> AM We took several sets of Azimuths by Knight's and Gregory's Compasses. We've often observ'd 3° & sometimes 4° difference in the Angle of the Magnetic Azimuth by shifting the Tacks of the Ship, and taking the observations from the different sides— now this being a fine Morning, smooth Water, and just wind enough to veer her Head whichever way answers best our purpose; the following

> Obervations were made with two different Compass's [sic] to attain this Difference which I'm totally at a loss to account for.[36]

During Cook's final voyage (1776-80), Wales found that by placing the ship's head in the opposite direction, the variation differed from 3° to 6°, sometimes as much as 10°. He noted similar discrepancies when traveling up or down the English Channel. Baffled by this phenomenon, navigators and astronomers usually attributed it to imperfect instruments.

Finding the magnetic dip was even more difficult and frustrating than variation. Even on dry land, determining dip with any precision was a time-consuming and troublesome process. Measuring the dip at Hudson's Bay in 1775, Thomas Hutchins remarked, "I took particular care in placing the instrument in the magnetic meridian, and was near four hours before I got it right. The observations employed four hours more."[37] Despite improvements in the design and construction of dipping needles, they too suffered harsh criticisms. Practically impossible to use unless the ship was securely anchored or the instrument was on shore, dipping needles gained a reputation for being difficult to use and unreliable. In 1777, William Wales reported, "the dipping needle . . . we took ashore . . . was so much out of balance, and so difficult to get in [balance] again . . . [that] we did not get it perfectly adjusted before we went away, and of course were not able to get any observations of this kind at this time."[38]

Reiterating the difficulties of using the dipping needle, Wales' former assistant, William Bayly, noted in 1782, "when at sea the needle seldom rested quite steady, but vibrated one or more degrees each way."[39] To minimize these errors he took several precautions. Following Cavendish's technique, Bayly made ten observations with the dipping needle facing east and west alternately, then switched the needle's poles and repeated the observations. Through this commonly used technique, the mean of multiple observations determined the "true dip." Again paralleling Cavendish's work, Bayly removed the instrument as far as possible from iron to reduce errors.[40] Although

Wales, Bayly and others recognized the effects of nearby iron on their magnetic instruments, they failed to realize that compass deviations arising from changes of the ship's head also arose from shipboard iron. Investigators continued to fault the instruments. Indicating a general distrust of magnetic instruments, a commentator complained in 1800 that azimuth compasses and dipping needles, despite being the best money could buy were "totally inadequate to the correct and useful purposes of navigation, or indeed to any correctly useful purpose whatever."[41] These misgivings persisted in the nineteenth century.

Regardless of the difficulties of measuring magnetic data, Cook's explorations earned a reputation for their excellent charts, precise astronomical observations, and geographic discoveries. Geographic motives continued to play a major role in several British explorations during the late eighteenth century. Though the notion of a fertile, populous southern continent (i.e., *Terra Australis Incognita*) generally fell into disrepute following Cook's second voyage, other ideas, also originating with theoretical geographers, lingered throughout the century. Theories espousing the existence of a North-west passage from Atlantic to Pacific and an open polar sea gained many enthusiastic adherents.[42] In the mid-1770s, naturalist-lawyer Daines Barrington (1727-1800) appealed to geographical theories and navigators' stories of ice-free seas to bolster support for a trip to high northern latitudes.[43] Barrington embraced the ideas of Swiss geographer, Samuel Engels (1702-1784) who had argued in 1765 for the existence of an open polar sea (i. e., "une mer vaste et libre").[44] Prompted by Barrington's enthusiasm, the Royal Society of London proposed to the Admiralty a voyage to the North Pole. As a result, Captain Constantine John Phipps of the Royal Navy set out in April, 1773, "to try how far navigation was practicable towards the North Pole."[45]

Though Phipps' voyage primarily sought the extension of geographical knowledge, it also amassed natural historical, astronomical, and navigational data. Paralleling

Cook's earlier efforts, the expedition involved cooperation between the Board of Longitude, Admiralty, and Royal Society. As in earlier voyages, Maskelyne ordered appointed astronomer Israel Lyons (1739-1775) "to make nautical & astronomical observations & to perform other Services tending to the improvement of Geography and Navigation."[46] The expedition received an array of instruments designed by prominent instrument makers including, a sextant, telescope, chronometers, pendulum, hygrometer, barometer, manometer, and Nairne's dipping needle.[47] Similar provisioning of instruments continued in Arctic explorations of the nineteenth century.

A wall of impenetrable ice prevented Phipps in His Majesty's Ships (H.M.S.) *Racehorse* and *Carcass* from going much beyond Spitsbergen (east of Greenland). Nonetheless, the expedition continued to collect and measure. Phipps, however, complained that if scientific observations had been more than a secondary consideration, they might have been "more numerous and satisfactory."[48] While geography, natural history, and astronomy dominated the observational efforts, Phipps also took an interest in magnetism. Magnetic observations received the "most scrupulous attention" to remove accidental error.[49] Despite numerous precautions, Phipps remarked that his compasses, while adequate for navigating the ship, failed to give a degree of precision fine enough for testing or formulating a theory. The variation, he wrote,

> always an interesting object to navigators and philosophers, became peculiarly so in this voyage from the near approach to the Pole. Many of the theories that had been proposed on this subject, were to be brought to the test of observations made in high latitudes, by which alone their fallacy or utility could be discovered. They, of course, engaged much of my attention, and gave me the fullest opportunity of experiencing with regret, the many imperfections of what is called the azimuth compass.[50]

In spite of observing conditions deemed ideal, Phipps could not account for the irregular, often sudden changes in the variation in high latitudes. Like earlier investigators, he blamed the instruments.

Three years after Phipps' failure to reach the North Pole, the Admiralty proposed another voyage with comparable geographic and scientific motives. With encouragement from the Royal Society, the Admiralty ordered Cook in 1776 to find "a Northern Passage by sea from the Pacific to the Atlantic Ocean."[51] Exemplifying the quantifying spirit, the official instructions for Cook's final voyage included an immense list of things to measure and observe:

> as far as your time will allow, very carefully to observe the true situation of such places, both in latitude and longitude, the variation of the needle, bearings of headlands, height, direction and course of the tides and currents, depths of soundings . . . and also to survey, make charts, and take views of such bays, harbours and different parts of the coast, and to make such notations thereon as may be useful either to navigation or commerce . . . observe the nature of the soil and the produce thereof, the animals and fowls . . . the fishes . . . metals, minerals, or valuable stones, or any extraneous fossils . . . seeds of such trees, shrubs, plants, fruits and grains . . . [and] observe, the genius, temper, disposition and number of the natives and inhabitants . . .[52]

Aided by Lieutenant James King and astronomer William Bayly, Cook used the best available instruments for recording astronomical, oceanographic, and geophysical data. In their compiled observations published in 1782, the pages devoted to terrestrial magnetism indicated its relative importance. Bayly, Cook, and King included separate accounts related to astronomy (totaling 160 pages), chronometers (40 pages), meteorology (40 pages), magnetic variation (45 pages), and magnetic dip (15 pages).[53] As their accounts also show, variation measurements continued to overshadow those of dip. Variation was easier to measure aboard a moving ship. More importantly, variation had practical navigational utility and dip did not.

In 1778, Cook, like Phipps, encountered impenetrable ice and turned back at the appropriately named Icy Cape on the northwest coast of Alaska. Upon returning to the Sandwich Islands (i.e., Hawaiian Islands), Cook was murdered by islanders in early 1779, ending his illustrious career of exploration and careful observation. As we have seen, during the voyages of Cook and Phipps, three concerns guided the majority of measuring and collecting— geography, longitude, and natural history. Officers like Cook, King, and Phipps, and astronomers like Wales, Bayly, Green, and Lyons (and naturalists including Joseph Banks) gave their closest attention to mapping unknown lands, testing chronometers, calculating the longitude by astronomical methods, and collecting new plants and animals.

When compared with these activities, eighteenth-century investigators did not give high priority to the collection of magnetic measurements. Certainly navigators and astronomers diligently measured variation and dip, but these were given less attention than more important, less frustrating areas. Nevertheless, through careful observations, investigators recognized numerous changes in variation, including those depending on the direction of the ship's head. While often attributed to flawed instruments, the true source of these irregularities remained unknown. Because the instruments were often blamed, compass deviations stimulated a further drive to improve magnetic apparatus.

Cook's voyages set a high standard and a familiar pattern for future explorations of the French and the British.[54] Later British efforts commanded by naval officers stressed the prestige, power, and national honor of geographical objectives. As well, the links between Royal Society, Board of Longitude, and Admiralty continued shaping the scientific goals of later expeditions. Like their predecessors, nineteenth-century explorers received the best available scientific instruments for measuring a wide range of phenomena.[55] Also echoing Cook and Phipps, later expeditions showed an awareness of compass irregularities.

Early nineteenth-century investigators began to recognize that compass deviations originated from quantities of iron in and on the ship. Ship magnetism, also called "local attraction" or "compass deviation," affected the accuracy of all shipboard magnetic measurements. Hence, increasing amounts of iron in combination with increasingly sensitive magnetic instruments led to a wariness of all ship-based measurements. Eventually these developments stimulated strictly land-based magnetic observations performed far from the ship's disturbing influence. The next section examines the recognition of ship magnetism in greater depth and the effects it had upon magnetic collecting.

Iron in the Ships: Matthew Flinders and William Bain

Great Britain had relied on its sea power for several centuries, and investigators had long sought to save money and lives by improving navigation. Despite the solution of the longitude problem in the late eighteenth century, shipwrecks continued to be commonplace in the nineteenth century. Given the dangers of nocturnal navigation, gales, icebergs, storms, strong ocean currents, and human error, many factors possibly contributed to these shipwrecks. In 1804, *H. M. S. Apollo* and nearly forty other vessels ran aground on the northern coast of Portugal. An estimated three hundred sailors lost their lives from cold, hunger and drowning.[56] Eight years later the *H. M. S. Hero* ran aground near Texel Island off the Netherlands. Around the same time, the *H. M. S. Defiance* and *St. George* wrecked on the western coast of North Jutland (Denmark). On these three ships alone nearly two thousand suffered or died.[57] In 1831, the *H. M. S. Thetis* set sail from Rio de Janeiro. One day into the journey the ship met her fate:

> the first intimation they had of being near land, was the jib-boom striking against a high perpendicular cliff, when the bowsprit broke short off, the shock sending all three masts over the side and thus in a moment perished twenty-five valuable lives, and a fine vessel, with her cargo, worth nearly a quarter of a million sterling.[58]

Incidents of this kind remained commonplace. In fact, even as late as the period from 1852 to 1860, approximately one in every two hundred British ships ran against unseen obstacles or collided with other vessels. During this period, more than seven thousand people lost their lives.[59]

In addition to the many dangers contributing to these shipwrecks, another grew increasingly prominent during the nineteenth century. More iron fittings, iron cannon, iron equipment and, later, all-iron and steel hulls increased the potential for navigational errors. By 1850, iron was the principal building material in ship construction; after 1880, steel overtook iron.[60] Rising amounts ferruginous materials contributed to navigational errors and shipwrecks. This nineteenth-century problem had been largely absent or unnoticed in previous centuries, during an age of primarily wooden ships.

Though Cook, Wales, and others in the late eighteenth century noted deviations in variation arising from changes in the ship's direction or head, they attributed these alterations to imperfect instruments. While navigators since at least the sixteenth century realized that the close proximity of iron affected the compass, they failed recognize how this influence exerted itself. Neither did they understand the interactions between terrestrial magnetic forces and those exerted by iron masses surrounding the compass. Most assumed the simple attractive power of the north end of the needle towards iron (hence the term "local attraction"). For instance, in 1794, Captain Murdo Downie remarked, "I am convinced that the quantity and vicinity of iron in most ships have an effect in attracting the needle; for it is found by experience that the needle will not always point in the same direction when placed at different parts of the ship."[61] Though Downie and others recorded iron's disturbing effects, they failed to link compass deviations arising from changes in the ship's head with the presence of shipboard iron.[62] The research of Matthew Flinders, William Bain, and others in the early nineteenth century slowly altered the understanding of this phenomenon.

While held by French authorities on the Ile de France (Mauritius) in 1804, Captain Matthew Flinders (1774-1814) wrote a letter to Sir Joseph Banks, the President of the Royal Society. In this letter he reported several magnetic observations made during a survey of the southern coast of New Holland (Australia) in 1801-02.[63] As in earlier voyages, accurate mapping had stood as the primary goal of Flinders' expedition in H.M.S. *Investigator.* Mirroring previous efforts, the Admiralty instructed Flinders to measure numerous data including winds, tides, currents, latitude, longitude, and magnetic variation. Flinders and his appointed astronomer, John Crosley, brought with them a wide assortment of instruments, including chronometers provided by the Board of Longitude.

In his letter to Banks, Flinders reported observed changes in magnetic variation with alterations of the ship's head. While performing his surveying of the Australian coast, Flinders carefully took magnetic bearings of objects at sea and land. The consistent discrepancies of these measurements suggested shipboard iron as the possible cause. Of Wales' earlier efforts, Flinders later reflected, "it seems indeed extraordinary, that with the attention paid by Mr. Wales to [compass deviations], he should not have discovered, or suspected, that the attraction of the iron in the ship was the primary and general cause of the differences so frequently observed."[64] Echoing the complaints of his predecessors, Flinders noted:

> That the compasses, even in the Royal Navy and to this day, are the worst constructed instruments of any carried to sea, and often kept in a way to deteriorate, rather than to improve their magnetism, cannot be denied; but errors arising from the badness of compasses would not be reducible to regular laws as those were in the *Investigator*. . .[65]

Testing his idea that shipboard iron was the culprit, Flinders took magnetic bearings of a distant object with the ship's head pointed in different directions, and identical readings on shore with a theodolite. His careful comparisons of ship-based and land-based observations resulted in several general conclusions. First, Flinders recognized that the maximum deviations occurred with the ship's head pointed nearly east or west. Second, minimum deviations differences happened with the ship pointed nearly north or south, i.e., aligned with the magnetic meridian. Finally, differences in variation between the cardinal points of the compass took on intermediate values between minimum and maximum errors.

Additional observations suggested that compass deviations also depended upon the local dip. With various factors in mind, Flinders put forth an empirical generalization later known as Flinders' Rule. This rule stated, "the error produced at any direction of the ship's head, would be to the error at East or West, at the same dip; as the sine of the angle between the ship's head and the magnetic meridian, was to the sine of eight points, or radius."[66] Hence, changes in variation depended upon the direction of the ship and the amount of dip as well. Such a realization eventually lent new practical utility to dip measurements. Further complicating matters, Flinders found that the ship's distance from the line of no magnetic variation affected the amount of deviation.

Regarding the influence of shipboard iron, Flinders reported to Banks three general conclusions. First, the attractive power of different ferruginous bodies on the ship collected into something analogous to focal point or center of gravity. This center of magnetic attraction usually coincided with the location of the greatest quantity of iron on the ship. Second, the magnetic focal point had the same kind of attraction as the terrestrial magnetic pole of the hemisphere where the ship was located, the southern hemisphere in Flinders' case. As a result, compass deviations would be reversed in opposite hemispheres (i.e., north and south). Third, in ships of war, the attractive power of the focal point interfered with a compass placed in the binnacle. This last conclusion required procedures aimed at correcting or preventing navigational errors.

Flinders demonstrated a link between changes in the ship's head, iron in the ship, and terrestrial magnetic phenomena. In a way his predecessors had not, he explicitly connected compass deviations, shipboard iron, and dip. His conclusions brought with them a practical reason for observing magnetic dip as well as new procedures for collecting magnetic data. Because eighteenth-century magnetic charts and tables had been compiled without a knowledge of local attraction's effects, earlier magnetic measurements could not be considered reliable.

Generally refraining from theoretical discussion, Flinders viewed himself as a collector not an interpreter of terrestrial magnetic data.[67] He did not consider himself qualified to comment on theoretical issues. Cautioning that "constant employment upon practice" had not allowed him, "to become much acquainted with theories," Flinders wrote to Banks:

> I shall leave it to the learned on the subject of magnetism to compare the observations here given with those made by other in different parts of the earth, and to form from them an hypothesis that may embrace the whole of the phenomena: the opinion I have ventured to offer is merely the vague conjecture of one who does not profess to understand the subject.[68]

Regarding local attraction of land masses, he similarly noted in 1814:

> In some parts of this little discussion upon the attraction of land, I feel to have stepped out of my sphere; but if the hints thrown out should aid the philosopher in developing a system of magnetism applicable to the whole earth, or even be the means of stimulating inquiry, the digression will not have been useless.[69]

In these humble remarks Flinders illustrated a persistent division between navigators and natural philosophers. Though interested in aiding natural philosophers, his approach remained essentially practical and non-theoretical.

Towards the end of Flinders' internment on the Ile de France, he proposed future plans for

> making all the necessary experiments for ascertaining the magnetism of ships as far as can be useful to the accuracy of navigation; as also of making such as may enable me to determine the points on the surface of the Earth to which the needle of the compass is directed, and also the places of the poles within the earth which affect the dipping needle; what I have done here being only preparatory . . .[70]

After six and a half years of imprisonment by the French, Flinders returned to England and, in 1810, appealed to the Admiralty for a continuation of his experiments. In 1812, a series of observations made on five ships at Sheerness and Portsmouth confirmed that iron caused deviations in the way Flinders described. Nevertheless, many mariners continued to reject his explanation. Experienced seamen recognized compass deviations but, as Flinders pointed out, "the most general result of their observations seems to have been an opinion, that within some undefined and variable limits this instrument was radically imperfect."[71] After Flinders' death, similar judgments prevailed among many mariners.

Like Flinders, William Bain, a Master in the Royal Navy, hoped to convince navigators that shipboard iron contributed to their navigational woes. In 1817, Bain proclaimed that the subject was a "fact of much importance to navigation, and consequently to the general interests of the British nation."[72] He sought to direct nautical men to compass deviations arising from ship magnetism. Ignorance of local attraction, he argued, had occasioned many terrible losses and fatal accidents.

Bain pointed out that experiences during the Napoleonic wars had taught many navigators that setting a course opposite from that initially steered could lead to navigational misfortune. Given the ignorance of merchants, "neither skill, experience, nor fortitude" could guard against such errors. He argued that local attraction had contributed to a multitude of accidents in the English Channel, the St. Lawrence River, and elsewhere. Pointing to ship magnetism as the culprit, Bain reiterated the difficulties of convincing navigators otherwise. Unfortunately, most held fast to the notion that flawed instruments or unnoticed ocean currents were to blame.[73] Bain's work, like Flinders', illustrated a division between navigators and natural philosophers. He supposed that the uncertainties and doubts accompanying compass deviation could be removed by employing a few scientific men. Trained men of science, hired by the government, could immensely improve navigational science. Hence, Bain looked to scientists rather than navigators to solve the problem.

Again paralleling Flinders, Bain took an empirical and non-theoretical approach to his study of local attraction. He noted that "one single fact established by experience, is stronger and deserving of more credit than all the hypotheses founded on theory that can be brought together."[74] After slightly modifying Flinders' Rule, Bain offered several precautions for guarding against local attraction. First, the binnacle should be permanently fixed with copper, rather than iron, bolts and nails. Second, variation measurements with the azimuth compass should always be performed on the binnacle to ensure uniformity. Third, all ships of war and merchant ships loaded with cargo should, before setting sail, choose a distant fixed object and compare its magnetic bearings on and off ship with the azimuth compass. This procedure involved swinging the ship's head east and then west to determine maximum compass deviations. For future reference, these deviations should be recorded in the ship's log.[75] These precautions, argued Bain, would help reduce navigational errors arising from ship magnetism.

As we have seen, both Flinders and Bain sought to remedy the widespread ignorance of local attraction which persisted in the navigational community. Their main hope was to diminish the loss of lives and property attributable to compass deviations. To this end, both men determined empirical rules and practical techniques for calculating navigational errors arising from local attraction. Beyond a few general assumptions regarding terrestrial magnetism, their work remained empirical and lacked theoretical content. Regarding theoretical approaches to magnetism, both men deferred to the judgments of scientifically-trained men.

From the 1820s onward, interest in local attraction expanded because more iron (and later steel) was used in ship construction, more sensitive instruments were developed, and compass deviations became more of a hindrance. Throughout the nineteenth century, increasing amounts of iron were used for ballast, water tanks, cables, gun carriages, capstans, masts and other parts of ships.[76] Not surprisingly, the British government and private shipping firms took great interest in alleviating navigational and surveying errors caused by iron in the ships.[77] The most common methods of correcting these deviations involved carefully recording the compass deviations before the ship left port; comparing the readings of a compass located above deck with those of the steering compass; or the application of various compensation devices, including iron plates, iron bars, and magnets situated near the compass so as to counteract or determine the effects of local attraction. In-depth discussion of these methods has been examined in the existing scholarship.[78]

Beyond the intense interest in navigational error, the recognition of local attraction also fostered general interest in terrestrial magnetic phenomena. Attracted to a practical problem with important implications for both accurate navigation and map making, greater numbers of scientifically-trained men showed interest in the study of magnetism. In addition to local attraction, the renewed search for the North-west passage in 1818 fostered increased interest in terrestrial magnetism. Most theoretical estimates placed the north magnetic pole in the Arctic, yet there was little agreement on its precise location or its possible movements. Hence, Arctic exploration allowed the compilation in high latitudes of magnetic measurements against which theories could be tested or new explanations devised.

Therefore, arising from the recognition of local attraction and the renewal of Arctic exploration, magnetic collecting gained greater attention in the late 1810s, than it had in the previous century. Admittedly, both practical and scientific goals coexisted in the eighteenth and nineteenth centuries. Nevertheless, a greater scientific understanding of terrestrial magnetism became increasingly important in nineteenth-century efforts. In any case, the reasons for collecting magnetic data altered in both degree and kind from one century to the next. In the eighteenth century, relatively few natural philosophers stressed global magnetic measurements and those who did were driven by navigational purposes. By the 1820s, however, increasing numbers of scientifically-trained men showed interest in amassing, arranging, and interpreting terrestrial magnetic phenomena for non-navigational reasons. We will return to their work in the final chapter.

Napoleonic Interlude and the Revival of Arctic Exploration

The Napoleonic wars diverted the Royal Navy from exploration, resulting in decreased opportunities to classify, collect, and measure. During the early nineteenth century, one of the few British measurers of magnetic phenomena was George Gilpin. Gilpin, who served as an astronomical assistant on Cook's second voyage and afterwards as an assistant at the Royal Observatory (1776-81), recognized the lack of activity and called for increased magnetic observations.[79] In 1806, he noted as Clerk of the Royal Society that magnetic observations made for only limited periods were "not sufficient for minute purposes."[80] Similar Mountaine and Dodson fifty years earlier, Gilpin regretted that eighteenth-century travelers had not made more accurate, land-based observations with proper instruments in different parts of the world. Claiming that progress in terrestrial magnetic theory depended upon carefully registered, properly arranged observations with good instruments, he concluded, "It is hoped therefore, that in future attention to this subject will not be thought beneath those who may have it in their power essentially to promote an undertaking so interesting to the philosopher, and so valuable and useful to the maritime world."[81]

Illustrating a continuing lack of interest in land-based magnetic collecting was the private magnetic observatory of Colonel Mark Beaufoy at Bushey Heath, the only one of its kind in Britain. Between 1813 and 1822, Beaufoy collected land-based observations superior in accuracy and extent to most earlier British work.[82] Carefully recording dip and variation, as well as meteorological data, he complained in 1820 of the dearth of activity:

> The only [magnetic] observations which, I believe, have been published are those of the Royal Society, commenced by the late Mr. Gilpin, and continued by the present librarian; but notwithstanding the accuracy of the former, and the well-known scientific abilities of Mr. Lee, these observations being made in a room in which iron has been used to strengthen the ceiling (and not in the open air), it is doubtful whether the real variation can be truly ascertained.[83]

Indeed, Beaufoy knew of only two places where land-based magnetical observations were being made. Both Gilpin's and Beaufoy's remarks indicated a prevailing lack of interest with respect to land-based magnetic observations.

By the late 1810s, the renewed search for the North-west Passage fostered the global collection of terrestrial magnetic data, particularly in seeking the supposed location of the magnetic pole or poles. Arctic exploration raised the importance of magnetic measurement to a level unimagined in the days of Captain Cook. Since Elizabethan times England had sent expeditions in search of the North-west Passage.[84] Following the Revolutionary and Napoleonic wars, the Royal Navy sought new challenges and employment for its swelled ranks. By 1817, ninety percent of Britain's naval officers, six thousand in number, remained unemployed or under employed.[85] A possible avenue for their employment and promotion came in 1816-17, when whalers reported comparatively ice-free seas near Greenland. Remarking of a whaling voyage, experienced whaler, William Scoresby, Jr., noted "a remarkable diminution of the polar ice had taken place, in consequence of which I was able to penetrate in sight of the east coast of Greenland . . . A situation which for many years had been totally inaccessible."[86] Replying to an inquiry from Joseph Banks, Scoresby supposed that "the mystery attached to the existence of a north west passage might have been resolved" if his had been an expedition of discovery.[87] However, Greenland whalers such as Scoresby took an oath preventing them from such exploratory ventures.[88]

Excited by the renewed possibility of discovering the North-west passage, Banks wrote to the First Lord of the Admiralty, Lord Melville, about the unusually ice-free seas. Advancing arguments similar to Banks', the second secretary of the Admiralty, John Barrow, added the factor of Russian activity in the Arctic.[89] In October 1817, Barrow commented, "The Russians have for some time been strongly impressed with the idea of an open passage round America. . . . It would be somewhat mortifying if a naval power but of yesterday should complete the discovery in the nineteenth century, which was so happily commenced by Englishmen in the sixteenth."[90] Adding fuel to the fire, Barrington's arguments from the 1770s were republished in 1818.[91]

In response to pressure from the Royal Society and the Admiralty, Parliament's passage of the Longitude Act of 1818 gave immediate impetus for renewed British exploration. Amending the Acts of 1743 and 1776, this legislation supplemented the full reward of £ 20,000 with a graduated scale of awards for sailing further north or westward. Sailing to north latitudes of 83°, 85°, 87° and 88° earned £1000, £2000, £3000, and £4000 respectively. Similarly, £5000, £10,000, and £15,000 would be awarded for sailing west from Greenwich, 110°, 130°, and 150° within the Arctic Circle (i.e., 66 1/2° N).[92] These large rewards spurred naval officers to explore the frozen, inhospitable polar regions.

In addition to opportunities for professional advancement and pecuniary payoff, many argued that Arctic voyages would benefit numerous areas of science, including the study of magnetism. During the late eighteenth century, Greenland whalers and navigators including Phipps and Cook had described the unsteady movements of the compass, the increase of dip, and the large variations of high latitudes. Arising from these observations as well as theoretical considerations, natural philosophers supposed the existence of a northern magnetic pole, yet remained unsure of the pole's position and possible movements. For example, Euler placed the north magnetic pole at 75° north, 115° west longitude from Paris, while Buffon positioned it at 71° north, 100° west, and French observational astronomer Jerome Lalande put it at 77° 4' north, 86° west. In the 1790s, American John Churchman traveled to France, but failed to persuade the French government to fund a voyage to determine the magnetic pole's position. In 1802, Lalande lamented the lack of proper magnetic data for locating and calculating the motion of the magnetic pole.[93]

With the renewed possibilities of Arctic exploration following the Napoleonic wars, British investigators showed increasing enthusiasm for collecting scientific observations, including magnetic data, in far northern latitudes. In 1815, Scoresby read a paper to the Wernerian Natural History Society proposing a trip to the north pole. An outline of his forthcoming book on Greenland included sections describing the polar seas, ice, atmosphere, and fauna.[94] Scoresby's proposed appendix contained a series of meteorological and magnetic tables. Explaining the opportunities provided by Arctic research two years later, Mark Beaufoy contended that the collection of measurements of the depth, temperature, and salinity of the sea, as well as meteorological data "would contain much interesting and valuable information, and throw great light on the natural phenomena of these unexplored regions."[95] Of magnetism, he specifically noted that the

> extraordinary declination of the compass (peculiar to this part of the world) is so remarkable, that, were a vessel sent for no other purpose than of making magnetical observations, both time and money which might be bestowed on the expedition would be advantageously employed for the advancement of science.[96]

Beaufoy also conjectured that variation continued to increase in higher latitudes until the needle lost all its polarity. Though his proposal for an exclusively magnetic expedition did not come to fruition, Beaufoy's comments regarding unusual compass behavior in Arctic latitudes were frequently repeated by later investigators.

In addition to strange compass behavior, some speculated about the connections between terrestrial magnetism and other polar phenomena. Remarking on the recent disappearance of ice, a writer in the *Philosophical Magazine* noted in 1818:

> Whether there exists any combination of causes— whether the connexion is between the ice and the grand focal point of magnetic attraction, which some philosophers suppose to be situated in the earth, or whether it is between the ice, and electricity in the atmosphere, or the aurora borealis, or all these together, can as yet be only a matter of mere conjecture.[97]

Though such speculations did not gain much attention until the 1820s, Scoresby, Beaufoy, and others placed greater emphasis on magnetic observations than had investigators of the previous century.

In their quest to explore and measure, British advocates of Arctic research gained enthusiastic and powerful support from John Barrow of the Admiralty. In 1818, Barrow lamented the lost opportunities for observation during a recent voyage:

> The arctic regions are at this moment, from many circumstances, so peculiarly interesting, that we took up the present volume in the hope of meeting with some new or striking observations on the geography, hydrography, or meteorology of a part of the northern seas which of late years has not been much visited by men of nautical science; but we have been disappointed . . . In the 'Voyage to Hudson's Bay' there is literally nothing worth communicating to the public at large.[98]

Despite disappointments, Barrow asserted that forthcoming polar expeditions planned to collect much data "interesting and important to science," including the state of atmospheric electricity and its connection with magnetic inclination, declination, and intensity; these facts alone "would be worthy a voyage of discovery." Recognizing that the polar regions were the location of the north magnetic pole, Barrow noted that comparing polar and equatorial magnetic measurements might also lead to important results. Furthermore, he called for additional investigations of the temperature, depth, salinity, and specific gravity of seawater; velocity and direction of ocean currents; and pendulum experiments to determine the figure of the earth.[99]

While indicating the growing role of science, Barrow's comments also illustrated links between scientific achievement and national prestige. In *A Chronological History of Voyages into the Arctic Regions* (1818), Barrow explained that if the initial searches for the North-west passage should fail,

> from both [voyages] may at least by confidently expected much valuable information, and improvement in the hydrography and geography of the arctic regions; as well as many important and interesting observations on the atmospherical, magnetical, and electrical phenomena, which cannot fail to advance the science of meteorology; and lastly, many valuable collections of objects in natural history . . . Of the enterprize itself it may be truly characterized as one of the most liberal and disinterested that was ever undertaken, and every way worthy of a great, and prosperous and an enlightened nation; having for its primary object that of the advancement of science.[100]

Following the advice of Barrow and others, the Arctic voyagers' official instructions directed them to make measurements and observations of all kinds. Compared with similar eighteenth-century efforts, scientific concerns took an increasingly prominent role. Terrestrial magnetism clearly played a larger role in these efforts.

The Initial Voyages (1818): John Ross and David Buchan

Though a variety of geographic, national, economic, and scientific concerns contributed to the renewed British quest for the North-west passage and the North Pole, Arctic exploration directly stimulated magnetic collecting and interest in terrestrial magnetism as well. Receiving much scientific advice from the Royal Society and enthusiastic support from Barrow, the Royal Navy launched a series of Arctic expeditions. In 1818, two expeditions set sail with Captain John Ross seeking the North-west passage in *H. M. S. Alexander* and *Isabella*, and Captain David Buchan attempting to reach the North Pole in *H. M. S. Dorothea* and *Trent*.[101]

Reminiscent of eighteenth-century expeditions, British polar exploration gave renewed opportunity for collecting global magnetic measurements. The continuing partnership between the Royal Society and the Admiralty betrayed an increasing stress on magnetic collecting. For example, in November 1817, the Royal Society recommended to the Admiralty a voyage to the North Polar regions specifically to collect magnetic measurements.[102] Illustrating magnetism's importance, Ross' official instructions from the Admiralty noted:

> Amongst other objects of scientific inquiry, you will particularly direct your attention to the variation and inclination of the magnetic needle, and the intensity of the magnetic force; you will endeavour to ascertain how far the needle may be affected by the atmospherical electricity, and what effort may be produced on the electrometer and magnetic needle on the appearance of the Aurora.[103]

The Admiralty repeated these same instructions to several subsequent voyagers.

As in earlier efforts, Ross' expedition carried a great variety of instruments to measure wind, water depth, temperature, air pressure, humidity, atmospheric refraction, and magnetic phenomena.[104] The *Isabella* carried at least four dipping needles by different instrument makers, twelve compasses of various types, and numerous books containing the astronomical and magnetic data from previous voyages.[105] The appointed astronomer, Captain Edward Sabine (1788-1883) of the Royal Artillery, was told to assist Ross "in making such observations as may tend to the improvement of geography and navigation, and the advancement of science in general."[106] Sabine, a skilled observer educated at the Royal Military Academy, Woolwich, had been elected F. R. S. in 1818. He came highly recommended by Joseph Banks and the Council of the Royal Society. Sabine later became one of the major figures in the British study of geomagnetism in the 1830s and 1840s. We will return to his early magnetic research shortly.

Despite the growing prominence of the goal of magnetic data collection, Ross' remarks in *A Voyage of Discovery . . . Inquiring into the Probability of a North-West Passage* (1819) illustrate the continuing role of navigators as mere collectors of data. As he explained, "the following Article, on the Variation of the Compass and Deviation of the Magnetic Needle, is not offered as a contradiction or a confirmation to any theory which has been already adopted;—*the author has all along considered himself as a collector of facts only.*"[107] Reiterating practical concerns of earlier navigators, Ross remarked that the compass:

> should be rendered as unerring a guide as possible; and this can only be done by a certain universal and invariable mode of finding the true variation, at all times and places, and under all circumstances.
>
> This variation of the compass being one of the important objects of the Expedition under my command, it became my duty to examine the various reports and publications on the subject, and to endeavour to ascertain how far the different systems given to the Public are correct; and the rules for correcting the deviation of the variation to be depended on.[108]

Building upon the work of Flinders, an appendix to Ross' book carefully described experiments on the local attraction of the *Alexander* and *Isabella.*

Ross' second in command, William Edward Parry (1790-1855), also showed great enthusiasm for the study of magnetism. During the voyage with Ross and several later attempts to find the North-west passage, he assiduously collected magnetic data. Early in 1818, before Ross' ships had left from England, he wrote to his parents:

> The observations upon the magnet will form one of the most interesting objects of the expedition. A variety of compasses are prepared for us, and great expectations are formed of the results we are likely to obtain in high northern latitudes. The connection observed, in many instances, between magnetism and electricity, and between these and the Aurora Borealis, is very curious, and it is expected, that the observations we shall be enabled to make, may throw considerable light upon it. There are great speculations on foot, as to what effect may be anticipated upon our compasses, when we approach the Magnetic Pole.[109]

Parry's remarks show great enthusiasm for magnetic research unlike any of his eighteenth-century predecessors. During the voyage he excitedly remarked on unusually large variations and probable proximity of the magnetic pole. In a letter to Barrow, Parry exclaimed, "the Variation had increased to 89°!!— the Dip is 84° 25'. I suppose, therefore, that the data we send you officially will be sufficient for finding the *bearings and distance* of the Magnetic Pole at once."[110] Parry's other explorations will be examined later in this chapter.

Ross' 1818 voyage, like those of Cook and Phipps, failed to find a North-west passage. Quickly retreating from Lancaster's Sound due to a vast mountain range (he named the Croker Mountains) which apparently only he saw, Ross returned home to severe criticisms from Barrow and several officers, including Parry and Sabine.[111] Objecting to Ross' retreat, Sabine recalled his "very visible mortification at having come away from a place which I considered as the most interesting in the world for magnetic observations, and where my expectations had been raised to the highest pitch, without having had an opportunity of making them."[112] Furthermore, Sabine objected to Ross taking credit for certain magnetic observations. In one instance, Ross had refused to let Sabine take the dipping needle ashore because he did not wish to be detained by observations. Sabine also accused Ross of stealing magnetic measurements without giving him credit.[113]

Later in 1819, Ross responded to Sabine's accusations in a short essay.[114] Noting that Sabine did not have an exclusive right to publish observations made during the voyage, he explained that several officers, including himself, assisted in making observations and that they were frequently recorded by different persons. Of Sabine's charges of inaccuracy, Ross remarked, "with respect to the magnetic observations, I have only to observe, that although they may differ from Captain Sabine's, they are clear of the imputations bestowed upon them; and it will hereafter be made to appear, whether Captain Sabine's observations or mine are most likely to be incomplete, imperfect, or incorrect."[115] While this exchange reflected personal animosity between Ross and Sabine, it also hinted at the increasing importance attached to accurate magnetic observations.

Quickly reporting measurements of variation, dip, and intensity to the Royal Society, Sabine also published investigations on the effects of local attraction in the *Philosophical Transactions.*[116] Noting that the compasses on each ship disagreed with one another by 3° to 8°, Sabine sought to find the precise nature of these errors. He followed Flinders' procedure of fixing the location of the compass and swinging the ship to determine points of no compass deviation (or points of no error). Swinging the ship required steadying it on each point of the compass and recording magnetic bearings of a distant object. At the same time, a compass taken a sufficient distance from the ship (usually on the ice) insured that measurements were free from local attraction. Agreement between sets of bearings taken on and off the ship indicated the points of no error, while discrepancies illustrated errors arising from local attraction.[117]

Although Sabine viewed his work as confirmation and extension of Flinders', he stressed the need for the multiplication and repetition of observations. Regarding hourly changes in declination he asserted, "careful observations on the direction of the needle at different hours of the day, on all convenient occasions, might be more serviceable towards a more certain knowledge of its causes."[118] He also noted that the relationship which Flinders discovered between local attraction and dip had not taken into account the diminution of directive force as the dip increased. Such a diminution, Sabine believed, explained the sluggish movements of compasses in high latitudes. As had earlier magnetic collectors, Sabine generally avoided theoretical discussion in his writings. Suspicious of hypotheses throughout his career, he remained convinced that amassed observations would eventually lead to the true theory of terrestrial magnetism.[119] Local attraction hindered Sabine's empirical approach because in higher latitudes it "rendered observations on board ship of little or no value towards a knowledge of the true variation."[120] Hence, compass deviations due to ship magnetism resulted in both practical and theoretical considerations. Unlike the previous century, local attraction required investigators to go off their ships and collect magnetic data.

In general, extensive polar exploration helped to make magnetism an area of greater interest than in the eighteenth century. Ross, Sabine, Parry, and numerous other Arctic explorers were continually fascinated by the odd behavior of the compass, the effects of local attraction, and the possibility of locating the magnetic pole. For instance, Ross' assistant surgeon, Alexander Fisher, remarked that although the principal object of the voyage was to find the North-west passage, there were several others deemed important such as finding where the magnetic pole is situated and observing pendulum vibrations in high latitudes.[121] Since icebergs often afforded the opportunity to make observations free from local attraction, delays caused by ice blockage, he contended, were no longer wasted time.[122] Also in his account, Fisher discussed Parry's numerous experiments on local attraction.[123]

Like Ross' voyage in 1818, the North Pole expedition led by David Buchan was instructed to carefully observe magnetic variation, dip and intensity. Frederick Beechey, an officer serving with Buchan, later noted that:

> The peculiarity of the proposed route afforded opportunities of making some useful experiments on the elliptical figure of the earth; on magnetic phenomena; on the refraction of the atmosphere . . . and on the temperature and specific gravity of the sea at the surface, and at various depths; and on meteorological and other interesting phenomena.[124]

Like Sabine, the voyage's astronomer George Fisher of Cambridge University recorded observations of magnetic dip, variation, and intensity.[125] A journal from the voyage noted the sluggishness of the compasses and recorded, "we must now be crossing the Magnetic Pole fast, as the variation increases so much."[126] Similar compass behavior and magnetic readings were observed in many Arctic voyages.

Despite failing to find the North-west passage or reach the North Pole, the 1818 voyages of Ross and Buchan provided a template for additional assaults on the Arctic. During the 1820s and 1830s, attempts continued carrying large contingents of men heavily outfitted for Arctic travel. Though the allure of discovering the North-west passage continued to overshadow all other goals, scientific objectives gained more attention than previously. Admitting the primary objective, Parry conceded in 1821 that "the improvement of geography and navigation, as well as the general interests of science, were considered as of scarcely less importance."[127] In contrast to Phipps' complaint that scientific observations had received only secondary consideration, Parry's comments indicated their newly-elevated importance. Though astronomy, meteorology, and natural history continued to receive much attention, magnetism also gained great notice.

The Search Continues (1819-1829): W. E. Parry and John Franklin

Emerging from the controversy regarding Ross and the Croker Mountains, in 1819 Parry gained command of an expedition to find the North-west passage aboard *H. M. S. Hecla* and *Griper*. The Admiralty yet again instructed Parry and Sabine to pay particular attention to magnetic measurements as well as interactions between the magnetic needle, atmospherical electricity, and the aurora borealis.[128] Their official orders also called for magnetic data collection along the western shores of Baffin's Bay, near the supposed position of

> one of the great magnetic poles of the earth, as well as such other observations as you may have opportunities of making in Natural History, Geography, &c., in parts of the globe, &c., little known must prove most valuable and interesting to the science of our country; and *we, therefore, desire you to give your unremitting attention, and to call that of all the officers under your command, to these points, as being objects likely to prove of almost equal importance to the principal one* . . .[129]

Compared with instructions for earlier Arctic voyages, the Admiralty's instructions suggested a higher priority on science in general, and on magnetism in particular.

As they had during Ross' 1818 voyage, Parry, Sabine, and other officers meticulously collected magnetic data and recorded the effects of local attraction. Like many of his predecessors, Parry asserted his role as a collector of facts:

> The extent of my aim has been, to give a plain and faithful account of the facts which I collected, and the observations which were made by myself and others, in the course of the voyage; and these, as far as they go, may be relied on as scrupulously exact. It is for others, better qualified than ourselves, to make their deductions from those facts.[130]

In line with this ideal, Parry's account of the voyage included numerous tables of magnetic variation, dip, and intensity. He made no attempt to interpret the amassed data; that was not his job. Parry, with the help of Sabine and several others, made repeated, independent measurements, often with different instruments. Most observations were performed in portable observatories on ice or land, away from the ship's influence. Though Parry succeeded in removing the Croker Mountains from the map and pushing further west than any other expedition for many decades to follow, he nevertheless failed to discover the illusory passage.

While none of the attempts during the 1820s and 1830s achieved the prized goal of the fabled passage, they continued extending geographic knowledge and collecting scientific data. Referring to comparative measurements of magnetic intensity, Captain Sabine reported in 1825:

> M. de Humboldt's experiments, with a much fewer number made by M. Rossel . . . include it is believed, the whole of our experimental knowledge in regard to intensity, previously to the year 1818; when the determination of the British government to re-attempt the discovery of a North West Passage . . . opened a field of great interest for researches of every kind connected with magnetism, in countries to which the access had previously been extremely inconvenient.[131]

Such sentiments clearly linked the renewal of Arctic exploration with increased opportunities to study terrestrial magnetism.[132] In particular, unusual compass behavior, local attraction, and the possibility of locating the magnetic pole attracted the attention of many polar explorers and natural philosophers. Noting sluggish compass movements, Alexander Fisher, ship's surgeon on Parry's first voyage, reported that the directive power of terrestrial magnetism decreased upon approaching the magnetic pole causing the effects of local attraction to increase.[133] From this, he supposed the proximity of the magnetic pole. Similar remarks appeared in a set of anonymous letters written during Parry's first expedition of 1819-1820. Reiterating the difficulties of compass usage in high latitudes, these letters pointed out the probable proximity of the terrestrial magnetic pole. They also reiterated the necessity of measuring variation and dip away from the ship's influence.[134] Illustrating the persistent link between the study of magnetism and navigation, the writer asserted that "among the mysteries of nature by which men are environed, none is more interesting, because none is more essential to the navigator than the powers and the properties of the magnet."[135]

However, while the connection between magnetism and navigation remained intact, it was no longer the primary reason that natural philosophers showed interest in magnetism. With many of the earlier navigational problems solved, more purely scientific concerns took a larger role in the nineteenth century than in the previous century. In February 1819, for instance, Barrow reiterated the scientific benefits of Arctic exploration, particularly those related to magnetism:

> In the late Expedition for exploring a passage from the Atlantic to the Pacific Ocean, many observations were made of a nature highly interesting to Science,

> and, among others an extraordinary and unlooked for degree in the variation of the Magnetic Needle, from which it is more than probable that the direction of the Copper-Mine River . . . and the point where it discharges itself into the Northern Ocean are very erroneously marked down on the Charts.[136]

Stressing the defective geography of Samuel Hearne's earlier land expedition (1769-1772), Barrow desired that the Admiralty assign "an officer well skilled in astronomical and geographical science, and in the use of instruments" to command an overland voyage. The officer chosen for the trek across the North American wilderness was Captain John Franklin (1786-1847).

Franklin had gained much experience in observing and collecting. In 1801, when only fifteen years old, he began a long career of exploration aboard the *Investigator* commanded by Captain Flinders. After naval service in the Napoleonic wars, Franklin had been second-in-command during Buchan's failed attempt to reach the North Pole in 1818. A year later, for his first land expedition (1819-22), Franklin was instructed:

> You will also not neglect any opportunity of observing and noting down the dip and variation of the Magnetic needle, and the intensity of the Magnetic force, and you will take particular notice whether any, and what kind of degree or influence the Aurora Borealis may appear to exert on the magnetic needle. . . . The two Admiralty Midshipmen are to be employed in assisting you in all the observations above mentioned, and you will direct them to keep a register of them, and also accurate journals of all proceedings and occurrences.[137]

Despite great hardships during this voyage, Franklin and his men faithfully followed their instructions, amassing a multitude of observations including those of magnetic variation, dip, and intensity.[138] To obtain magnetic intensity or force, he counted the vibrations of a freely swinging dipping needle. However, despite ongoing attempts to improve magnetic instruments, navigators continued complaining. Franklin remarked that the instrument "was not of the best kind for making with accuracy such delicate observations, and our results may, perhaps, be considered as only approximations to the truth."[139]

Accompanying efforts to make better instruments, the quest continued for more complete and standardized magnetic measurements. Though less successful than his first voyage (which reached 110° W longitude and claimed the £ 5,000 prize), Parry's second (1821-23) and third (1824-25) expeditions in H. M. Ships *Hecla* and *Fury* amassed valuable scientific data. Reverend George Fisher replaced Sabine as the astronomer on the second voyage, but the official instructions repeated, verbatim, the advice regarding magnetic variation, dip, and intensity. Seeking standardization, the Admiralty wanted Fisher "to be particularly careful to keep an accurate register of all the observations that shall be made, precisely in the same forms, and according to the same arrangement, that were followed by Captain Sabine on the late voyage."[140]

Beyond the attempts to improve instrumentation and standardization, the scope and intensity of magnetic collecting changed as well. During Parry's third voyage, the work of Parry and Lieutenant Henry Foster, Fisher's replacement, clearly illustrated the growing complexity of Arctic magnetic research. Of observations made ashore during the winter of 1824, Parry noted, "The interest of these, especially of such as related to magnetism, increased so much as we proceeded, that the neighborhood of the observatory assumed, ere long, almost the appearance of a scattered village, the number of detached houses having various needles set up in them, soon amounting to seven or eight."[141] With the availability of more sensitive instruments, investigators became increasingly interested in small variations of magnetic variation and intensity. In addition to the standard magnetic observations, Parry and Foster took regular hourly observations with newly-introduced suspended needles. As well they performed magnetic experiments designed by Peter Barlow and Samuel Hunter Christie, mathematics professors at the Royal Military Academy, Woolwich.[142] The researches of Barlow and Christie will be examined in chapter six.

As previously mentioned, in the 1820s, Parry and other British began using French measuring techniques and instruments to determine relative magnetic intensity. Since the 1780s, French physicists including Jean Charles Borda and Charles Augustin

Coulomb had used silk-suspended rather than pin-supported needles for sensitive observations of magnetic intensity and diurnal variation. Recording the time it took for a set number of the needle's oscillations determined relative magnetic intensity. This technique, however, was not widely utilized by the British until the reawakened interest in magnetism which accompanied renewed Arctic exploration. With delicately suspended needles, Arctic explorers and other investigators could record smaller and more transient phenomena than their predecessors. Parry, for instance, noted a diurnal change in intensity which regularly increased from morning to afternoon and decreased from afternoon to morning. Of these changes, he speculated in 1826:

> It also appeared that the sun, and, as we had reason to believe, the relative position of the sun and moon, with reference to the magnetic sphere, had a considerable influence both on the intensity and diurnal variation, although the exact laws of this influence may still remain to be discovered.[143]

Repeating this suggestion in the *Philosophical Transactions*, Parry and Foster wrote, "when any extraordinary change, however, appeared to be going on, the needles were more closely watched; and every phenomenon, such as the aurora borealis, meteors, clouds, the kind and degree of light, the moon's position, and the temperature within and without, were at all times carefully noted."[144] Despite the search for connections between various phenomena characteristic of natural philosophers during the 1820s, Parry and Foster warned that such questions were "of great delicacy, and of intricate research, and will be best left to the investigations of those who are theoretically conversant with these subjects."[145] Hence, the division between navigators and natural philosophers persisted.

In addition to doing experimental research and collecting magnetic data, investigators continued to exhibit great interest in improved instrumentation. Early in 1821, Captain Henry Kater (1777-1835) devoted his Bakerian lecture to the optimum shape and kind of steel for making compass needles.[146] Recognizing the compass problems during Ross' first expedition, Kater wanted Parry's first expedition to have magnetic instruments which combined "as much power and sensibility as possible."[147] Indicating the impact of French experimental physics, he employed Coulomb's torsion balance, repeated some of Coulomb's experiments, and cited Jean-Baptiste Biot's improved method of magnetizing needles. Arising from his research, Kater produced what became the standard azimuth compass in the navy. Like Knight's azimuth compass in the eighteenth century, Kater's highly regarded instruments were widely used in the nineteenth century. Parry, for instance, explained after his first voyage, "it is therefore deserving of especial notice, that even in such extreme circumstances, Captain Kater's excellent compasses, when used on shore, and with patience and attention to frequent tapping, indicated the meridian with very tolerable precision."[148] In sum, Kater's research utilized French techniques and responded to the changing needs of Arctic exploration; thereby he produced instruments considered better than their predecessors.

Also desiring improved instrumentation, Sabine commented in 1822:

> the consequent advance which has been made in this branch of natural knowledge [magnetism], render it desirable, that a greater degree of accuracy should be obtained in all respects, in observing its various terrestrial phenomena . . .
>
> This remark applies especially to observations on the dip of the needle; the instruments in general use for this purpose have received little or no improvement during the last fifty years, and produce results which can only be considered as approximate.[149]

In 1825, Sabine's remarks further illustrated the increasing observational emphasis on dip and intensity, and their links to new instrumentation. These measurements, previously of less interest because they lacked direct importance to navigation, became the primary measurements for exploring terrestrial magnetism after the 1820s. Sabine attributed changes in magnetic intensity to either a fluctuation in the earth's magnetic intensity or the shifting positions of the terrestrial magnetic poles. Experiments indicated that geographical variations in intensity could not be represented by any function of the dip. Hence, magnetic intensity must be regarded "as an essential element of the computation, distinct from the dip, and necessary to be known by observation." In this, Sabine and others illustrated a growing interest in observing intensity as a distinct magnetic component.[150] The emphasis on observations which lacked direct relevance to navigation and also required different types of instrumentation demonstrated a growing curiosity in terrestrial magnetism for its own inherent scientific interest.

In addition to practical, theoretical, and instrumental concerns, national pride played a prominent role in global magnetic collecting. Remarking on Parry's first voyage, retired army colonel John Macdonald noted in 1822 that beyond the discovery of the North-west Passage bestowing "a new wreath to the naval crown of Great Britain," Parry's close approach to the location of the magnetic pole added to the "honour of our country."[151] Others agreed that locating the magnetic pole would increase British national and scientific prestige. A dozen years later, Commander James Clark Ross (1800-1862), John Ross' nephew, asserted magnetism to be an "eminently British" science. Reporting magnetic observations gathered during a privately-funded expedition led by his uncle (1829-33), Ross boasted in 1834:

> *Their is no other country in the world whose interests are so deeply connected with it [magnetism] as a maritime nation, or whose glory as such is so intimately associated with it, as Great Britain.* All the late discoveries and improvements are to be attributed to the perseverance of British science, and the encouragement and assistance of an enlightened and liberal Administration . . . enabling a few British seamen to plant the flag of their country upon the Northern Magnetic Pole of the earth.[152]

As with his uncle's earlier claims regarding the Croker Mountains, James Clark Ross' claims to have located the northern magnetic pole did not pass without controversy.[153] In addition to mounting a later expedition to find the south magnetic pole, J. C. Ross, along with Sabine, was a major instigator of the "Magnetic Crusade" of the 1830s.[154] In fact, Ross spent most of his time between 1834 and 1838 making a magnetic survey of Great Britain and Ireland by the order of the Admiralty. We will return to these developments in the final chapter.

In 1838, at a meeting of the British Association for the Advancement of Science, another officer in the Royal Navy expressed similar nationalistic sentiments about polar exploration. Claiming that all of Europe looked to Great Britain to solve the problem of terrestrial magnetism in the southern hemisphere, Captain Washington patriotically remarked:

> Under a deep and abiding conviction that our country's future glory is identified with the encouragement of British enterprise, and that she would lose her high national character by ceding to another this opportunity of completing the work first traced out by Cook, I could not refrain from recording my sentiments, and conclude with the ardent hope that through the exertions of the British Association our wishes may be realized, and that ere long the southern cross may shine over an expedition sailing to the Polar Seas . . . and that cross . . . will once again shine over 'the meteor flag of England,' proudly waving over Antarctic land, discovered by the zeal and intrepidity of British seamen.[155]

Such sentiments linked British nationalism to polar exploration and scientific achievement. Of numerous British scientific and geographic accomplishments during the 1820s and 1830s, those related to terrestrial magnetism took a more prominent position than they had in the preceding century.

Conclusion

The patriotic comments of Ross, Washington and others also strongly identified polar explorations with the extension of natural knowledge.[156] Since the days of Cook, these voyages had pursued the global collection of geographic, hydrographic, meteorological, zoological, and geomagnetic data. Among these observations, the previously tangential concern for magnetic collecting in the eighteenth century gained prominence in the nineteenth century. As has been shown, the recognition of local attraction and renewed Arctic exploration contributed to this shift.

The immense task of meticulously collecting magnetic data particularly suited the Royal Navy because they already possessed the necessary equipment to gain access to the frigid polar regions and the military discipline required to make repeated measurements under extremely harsh conditions. Similarly, the Royal Artillery possessed the order and organization necessary for later land-based surveys and the permanent geomagnetic observatories established in the 1830s.[157] First and foremost, collectors of magnetic data such as Ross, Sabine, Franklin, Parry, and Foster were scientific servicemen.[158] By and large, these militarily-employed investigators retained a role as collectors of facts, infrequently attempting to interpret the amassed data. Changes in instrumentation and technique indicated a continuing concern for precision, but also illustrated the importance of different kinds of measurements in the nineteenth century. In particular, magnetic dip and intensity garnered much more attention for their supposed theoretical value.

In addition to scientific servicemen, British natural philosophers and mathematicians became increasingly interested in terrestrial magnetism in the 1820s for a variety of reasons. The division of labor continued, with navy officers collecting magnetic observations, and natural philosophers and mathematicians trying to interpret the data within a theoretical framework. As well, men of science performed numerous experiments on isolated magnets and magnetic materials and compared their results with the data amassed from the entire earth. Comparing controlled experiments with terrestrial data resulted in speculations about the origins of magnetism and the laws of terrestrial magnetic change. The work of these physicists and mathematicians, however, will be discussed in the final chapter. Hence, with the general scope of magnetic collecting laid out, the next chapter examines British theories of magnetism beginning in the mid-eighteenth century and their relationship to terrestrial magnetic studies.

Notes

[1] For additional examples of the "quantifying spirit", see Tore Frängsmyr, J. L. Heilbron, and Robin E. Rider (eds.),*The Quantifying Spirit in the 18th Century,* (Berkeley: University of California Press, 1990).

[2] See *Index for the Philosophical Transactions of the Royal Society of London (1781-1820)*, 71-72; and *Index for the Philosophical Transactions (1821-1830)*, 34-36. See also the index for *Abstracts for the papers Royal Society of London*, 1 (1800-1814) and 2 (1815-1830).

[3] See William Whiston, *The Longitude and Latitude found by the Inclinatory or Dipping Needle* (London, 1721) and William Whiston, *Memoirs of the life and writings of Mr. William Whiston*, (London, 1749) [Landmarks of Science, microform, 1975], Part I: 296-297.

[4] William Mountaine and James Dodson, "An Attempt to point out, in a concise manner, the Advantages which will accrue from a periodic Review of the Variation of the magnetic Needle, through the known World," *Philosophical Transactions of the Royal Society of London*, 48, pt. 2 (1754), 877-878. Dodson (d. 1757), the mathematics teacher at the Royal Mathematical School, Christ's Hospital, is known chiefly for his work on logarithms. *An Account of the Methods used to describe Lines on Dr. Halley's Chart of the terraqueous Globe, showing the variation of the magnetic needle about the year 1756 in all the known seas* was published posthumously with William Mountaine in 1758. See [G. J. Gray], "Dodson, James," *DNB*, 15: 174-175.

[5] William Mountaine and James Dodson, "A Letter to the Right Honourable the Earl of Macclesfield, President, the Council, and Fellows, of the Royal Society, concerning the Variation of the Magnetic Needle; with a Sett of Tables annexed, which exhibit the Result of upwards of Fifty Thousand Observations . . ." *Philosophical Transactions of the Royal Society of London*, 50, pt. 1 (1757), 332.

[6] Ibid., 334.

[7] David Ross, "Extract of a Letter from Mr. David Ross, Surgeon of His Majesty's Ship the Montagu, to Mr. William Mountaine, F. R. S. relating to the Variation of the Magnetic Needle . . ." *Philosophical Transactions of the Royal Society of London*, 56 (1766), 218.

[8] "The Variation of the Compass; containing 1719 Observations to, in, and from, the East Indies, Guinea, West Indies, and Mediterranean, with the Latitudes and Longitudes at the Time of Observation . . . by Mr. Robert Douglass. Communicated by the Rev. Nevil Maskelyne, Astronomer Royal, F. R. S. with a Letter prefixed from William Mountaine, Esq. F. R. S. to Mr. Maskelyne," *Philosophical Transactions of the Royal Society of London*, 66 (1776), 19-20. Samuel Dunn claimed in 1778 to have discovered a new theory of variation which he explained in *The Theory and Practice of the Longitude at Sea* (London, 1778).

[9] Patricia Fara, *Magnetic England in the eighteenth century*. Ph.D. thesis, (University of London, Imperial College, 1993), Chapter 3. Fara gives the case of navigators' negative reactions to Knight's compasses as an example of the clash between

practical experience and theoretical knowledge. Different groups (mariners, inventors, and naval administrators) had different perceptions of how a compass's function, its purpose, and its accuracy. Fara discusses the perceptions within these groups.

[10] Edward Nairne, "Experiments on Two Dipping-Needles, which Dipping-Needles were made agreeable to a Plan of the Reverend Mr. Mitchell, F. R. S.," *Philosophical Transactions of the Royal Society of London*, 62 (1772), 476-480. Nairne (1726-1806) also determined the specific gravity of seawater, its freezing point, and performed various electrical experiments. He was an acquaintance of Joseph Priestley and elected F. R. S. in 1776. See "Nairne, Edward," *DNB*, 40: 25-26.

[11] Patricia Fara, *Magnetic England in the eighteenth century*. Ph.D. thesis, (University of London, Imperial College, 1993), 108-109.

[12] Henry Cavendish, "An Account of the Meteorological Instruments used at the Royal Society's House," *Philosophical Transactions of the Royal Society of London*, 66 (1776), 375-401.

[13] See, for instance, "Meteorological Journal kept at the house of the Royal Society, by order of the President and Council," *Philosophical Transactions of the Royal Society of London*, 66 (1776), 351-352.

[14] Henry Cavendish, "An Account of the Meteorological Instruments used at the Royal Society's House," *Philosophical Transactions of the Royal Society of London*, 66 (1776), 396-400. The method was meant to balance out or compensate for discrepancies between the needle's center of gravity and its center of magnetic power.

[15] Daniel A. Baugh, "Seapower and Science: the motives for Pacific Exploration," in Derek Howse (ed.), *Background to Discovery: Pacific Exploration from Dampier to Cook* (Berkeley: University of California Press, 1990), 15-42.

[16] Quoted in John Hawkesworth, *An Account of the Voyages Undertaken by the Order of His Present Majesty for making discoveries in the Southern Hemisphere . . .* vol. I (London: W. Strahan and T. Cadell, 1773), i-ii. James Cook and others received similar secret instructions from the Admiralty in the 1760s. See Daniel A. Baugh, "Seapower and Science: the motives for Pacific Exploration," in Derek Howse (ed.), *Background to Discovery: Pacific Exploration from Dampier to Cook* (Berkeley: University of California Press, 1990), 54-55.

[17] See Harry Woolf, *The Transits of Venus: a study of eighteenth-century science* (Princeton, NJ: Princeton University Press, 1959), 167.

[18] See "A Table of the Latitudes and the Longitudes West of London, with the Variation of the Needle . . . from Observations made on board his Majesty's Ship the *Dolphin* . . . in the Years 1766, 1777, 1768, under the command of Captain Samuel Wallis," 520-522 and "A Table of the Variation of the Compass as observed on board the *Swallow*, in her Voyage round the Globe, in the Years 1766, 1767, 1768, 1769," in John Hawkesworth, *An Account of the Voyages Undertaken by the Order of His Present Majesty for making discoveries in the Southern Hemisphere . . .* vol. I (London: W. Strahan and T. Cadell, 1773), 669-676.

[19] See William T. Stearn, "A Royal Society Appointment with Venus in 1769: The Voyage of Cook and Banks in the *Endeavour* in 1768-1771 and its Botanical Results," *Notes and Records of the Royal Society of London*, 24 (1969), 64-88.

[20] Extract from Council Minutes, 23 June 1768, from I. Kaye, "Captain James Cook and the Royal Society," *Notes and Records of the Royal Society of London*, 24 (1969), 13.

[21] James Cook, *The Journals of Captain James Cook. The Voyage of the Endeavour, 1768-1771.* edited by J. C. Beaglehole (Cambridge: Hakluyt Series, Extra Series No. XXXIV, 1955), 392.

[22] Derek Howse, "Navigation and Astronomy in the Voyages," in *Background to Discovery: Pacific Exploration from Dampier to Cook*, edited by Derek Howse (Berkeley: University of California Press, 1990), 168.

[23] "Instructions to Captain Cook for his three voyages," *Publications of the Navy Records Society*, 63 (1928), 343-346. For complete discussion of the transit of 1769 and the results of its observation, see Harry Woolf, *The Transits of Venus: a study of eighteenth-century science* (Princeton, NJ: Princeton University Press, 1959), 150-201.

[24] See Sir Richard Woolley, "Captain Cook and the Transit of Venus of 1769," *Notes and Records of the Royal Society of London*, 24 (1969), 19-32. The method derived from suggestions made by Edmond Halley earlier in the century.

[25] Charles Green and James Cook, "Observations made by appointment of the Royal Society, at King George's Island in the South Sea," *Philosophical Transactions of the Royal Society of London*, 61 (1771), 397-436. See also Lieutenant James Cook, "Variations of the Compass, as observed on board the *Endeavour* Bark, in a Voyage round the World," *Philosophical Transactions of the Royal Society of London*, 61 (1771), 422-432.

[26] For a concise discussion of techniques of determining longitude, see Derek Howse, "Navigation and Astronomy in the Voyages," in *Background to Discovery: Pacific Exploration from Dampier to Cook*, edited by Derek Howse (Berkeley: University of California Press, 1990), 160-176. For early eighteenth-century efforts at finding longitude see Larry Stewart, *The Rise of Public Science, Rhetoric, Technology, and Natural Philosophy in Newtonian Britain, 1660-1750* (Cambridge: Cambridge University Press, 1992), Chapter 6, "The Longitudinarians," 183- 211.

[27] For details on Harrison's numerous attempts to construct a reliable marine timekeeper and claim the longitude prize, see Dava Sobel, *Longitude, The True Story of a Lone Genius Who Solved the Greatest Scientific Problem of His Time* (New York: Penguin Books, 1995). Sobel argues that several astronomers (and particularly Maskelyne) thwarted Harrison's efforts because they preferred the lunar distances method. See also William J. H. Andrewes (ed.). *The Quest for Longitude: The Proceedings of the Longitude*

Symposium, Harvard University, Cambridge, Massachusetts, November 4-6, 1993. (Cambridge, Mass: Collection of Historical Scientific Instruments, Harvard University, 1996).

[28] Morton J. Rubin, "James Cook's scientific programme in the Southern Ocean, 1772-75." *Polar Record*, 21 (1982), 33-49.

[29] Extract from Council Minutes, 12 December, 1771, quoted in I. Kaye, "Captain James Cook and the Royal Society," *Notes and Records of the Royal Society of London*, 24 (1969), 17.

[30] James Cook, *The Journals of Captain James Cook*, edited by J. C. Beaglehole (Cambridge: Hakluyt Series, Extra Series No. XXXV, 1961), vol. II: 78-79.

[31] Gilbert, *De Magnete*, (New York: Dover, 1958) [reprint of Mottelay's translation published in 1893], Book IV, Chapter XIII, 266.

[32] Cook quoted in Charles H. Cotter, *Studies in maritime history*, Vol. 2: *A history of ship magnetism* (London: Mansell, 1977), D14.

[33] "Variation," *Encyclopaedia Britannica*, Second edition (Edinburgh: J. Balfour and Co., 1778-1783), v. 10 [pt. 1]: 8691-8692.

[34] See J. B. Hewson, *A History of the Practice of Navigation* (Glasgow: Brown, Son & Ferguson, Ltd., 1951), 57-58. Hewson summarizes Wales's account of differences in magnetic variation according to time, compass, compass location, ship location, and direction of ship.

[35] Captain James Cook quoted in Charles H. Cotter, *Studies in maritime history*, Vol. 2: *A history of ship magnetism* (London: Mansell, 1977), D13.

[36] Charles Clerke, *The Journals of Captain James Cook*, edited by J. C. Beaglehole (Cambridge: Hakluyt Series, Extra Series No. XXXV, 1961), vol. II: 104.

[37] Thomas Hutchins, "An Account of the Success of some attempts to freeze quicksilver, at Albany Fort, in Hudson's Bay, in the Year 1775," *Philosophical Transactions of the Royal Society of London*, 66 (1776), 179.

[38] William Wales, *The original astronomical observations, made in the course of a voyage towards the South Pole, and round the world, in His Majesty's ships the Resolution and Adventure, in the years 1772, 1773, 1774, and 1774, by William Wales and Mr. William Bayly* (London: Printed by W. and A. Strahan, 1777), 15.

[39] James Cook, William Bayly, and James King, *Astronomical Observations made in the course of a Voyage to the Northern Pacific Ocean by Capt. J. Cook, Lieut. J. King and W. Bayly . . .* (London: William Richardson, 1782) [Lost Cause Press, Louisville, 1969], in William Bayly, "Dips of the Magnetic Needle, observed aboard H. M. Sloops *Resolution* and *Discovery*," 303.

[40] Ibid.

[41] P. R. Nugent, "A new Theory, pointing out the Situation of the Magnetic Poles, and a Method of discovering the Longitude," *Philosophical Magazine,* 5 (1800), 381.

[42] See Ann Savours, "The British Admiralty and the Arctic, 1773-1876," in *Pôle Nord 1983: Histoire de sa conquête et problèmes contemporains de navigation maritime et aérienne.* edited by Jean Malaurie (Paris: Éditions du Centre National de la Recherche Scientifique, 1987), 153-154, and Trevor H. Levere, *Science and the Canadian Arctic: A Century of Exploration 1818-1918* (Cambridge: Cambridge University Press, 1993), 34-35.

[43] For Barrington's papers read to the Royal Society, see *The Possibility of Approaching the North Pole asserted by The Hon. D. Barrington* (New York: James Eastburn & Co., 1818).

[44] Samuel Engels, *Mémoires et observations géographiques et critiques sur la situation des pays septentrionaux d'Asie d'Amérique d'après les relations les plus récentes* (Lausanne, 1765), 262, quoted in Ann Savours, "'A Very Interesting Point in Geography': The 1773 Phipps Expedition Towards the North Pole," *Arctic*, 37 (1984), 403. One of Engels's arguments was that only fresh water could freeze. Hence, previous navigators had stayed too near the coastlines in far northern regions near the mouths of rivers where the water could freeze.

[45] Constantine John Phipps, *A Voyage Toward the North Pole Undertaken by His Majesty's Command, 1773* (London: J. Nourse, 1774), 10. For Phipps's complete instructions see Ann Savours, "'A Very Interesting Point in Geography': The 1773 Phipps Expedition Towards the North Pole," *Arctic*, 37 (1984), 408-409.

[46] Instructions to Lyons, by the Commissioners appointed by Acts of Parliament for the discovery of longitude at sea &c &c Appendix A quoted in Ann Savours, "'A Very Interesting Point in Geography': The 1773 Phipps Expedition Towards the North Pole," *Arctic*, 37 (1984), 423.

[47] Constantine John Phipps, *A Voyage Toward the North Pole Undertaken by His Majesty's Command, 1773* (London: J. Nourse, 1774), 119, 123, 139. With instructions by the Royal Society, Phipps received additional advice from prominent individuals including Jean D'Alembert and Joseph Banks. Ibid., 13.

[48] Ibid., 15.

[49] Ibid., 109.

[50] Ibid., 108.

[51] "Instructions to Captain Cook for his three voyages," *Publications of the Navy Records Society*, 63 (1928), 357. Also in 1776, Lieutenant Richard Pickersgill was instructed to explore Baffin's Bay so that information might be gathered for Lieutenant Walter Young sent in 1777 to meet Captain Cook coming from the west. Pickersgill's

instructions required surveying, charting, and noting observations useful to geography and navigation. He also collected observations of inclination and variation. John Barrow judged both voyages (of Pickersgill and Young) as failures. See Lieutenant Richard Pickersgill, "Track of His Majesty's Brig *Lion* from England to Davis's Streights and Labrador, with Observations for determining the Longitude by Sun and Moon and Error of Common Reckoning; also the Variation of the Compass and Dip of the Needle, as observed during the said Voyage in 1776," *Philosophical Transactions of the Royal Society of London*, 68 (1778), part 2, 1057-1063; Richard Pickersgill, *A concise account of voyages for the discovery of a north-west passage, undertaken for finding a new way to the East-Indies* . . . (London: printed for the proprietor, 1782) [New Haven, CT: Research Publications microfilm, Inc., 1975]; and John Barrow, *A Chronological History of Voyages into the Arctic Regions, Undertaken chiefly for the purpose of discovering a North-East, North-West or Polar passage between the Atlantic and Pacific* (London: John Murray, 1818) [Reprint, New York: Barnes & Noble, 1971], 320-328.

[52] Ibid., 361-362.

[53] See James Cook, William Bayly, and James King, *Astronomical Observations made in the course of a Voyage to the Northern Pacific Ocean by Capt. J. Cook, Lieut. J. King and W. Bayly* . . . (London: William Richardson, 1782) [Lost Cause Press, Louisville, 1969], v-vi. Other voyages continued to record magnetic variation and dip in journals and logbooks. Sometimes this data was published by the Royal Society. See for example Alexander Dalrymple, "A Journal of a Voyage to The East Indies, in the Ship *Grenville*, Capt. Burnet Abercrombie, 1775," *Philosophical Transactions of the Royal Society of London*, 68 (1778), part II: 389-416.

[54] For an example of the French response to Cook's achievements, see *The Journal of Jean-François de Galaup de la Pérouse, 1785-1788*, translated and edited by John Dunmore (London: the Hakluyt Society, 1994), vol I: cxi-cxii. Although De la Pérouse's prime objective was geographic discovery, the official instructions approved by Louis XVI ordered numerous observations related to astronomy, navigation, physics, and natural history. Their content closely resembled the instructions of Cook's voyages.

[55] Trevor H. Levere, *Science and the Canadian Arctic: A Century of Exploration 1818-1918* (Cambridge: Cambridge University Press, 1993), 33.

[56] William Scoresby, Jr., "Observations on the Deviation of the Compass; with Examples of its fatal influence in some melancholy and dreadful shipwrecks," read originally at the Royal Institution at Liverpool, Jan. 23, 1822, *Edinburgh New Philosophical Journal*, 14 (1833), 34.

[57] Ibid., 36.

[58] Peter Barlow, "On the Errors in the Course of Vessels, occasioned by Local Attraction; with some Remarks on the recent Loss of His Majesty's ship *Thetis*," *Philosophical Transactions of the Royal Society of London* (1831), 218-219.

[59] Roy M. MacLeod, "Science and Government in Victorian England: Lighthouse Illumination and the Board of Trade, 1866-1886," *Isis*, 60 (1969), 7.

[60] Charles H. Cotter, *Studies in maritime history*, Vol. 2: *A history of ship magnetism* (London: Mansell, 1977).

[61] As quoted by Peter Mark Roget, "Magnetism" in *The Library of Useful Knowledge: Natural Philosophy II* (London: Baldwin and Cradock, 1832), 61.

[62] Early in the seventeenth century Captain John Smith in his *Seaman's Grammar* warned against having iron nails in the compass box. The third edition of Edward Wright's *Errors in Navigation* (1657) noted "other errors (of the compass) may be eschewed, as that there be no iron near the compass in the time of observation." In 1684, Captain Samuel Sturmy wrote in *The Mariner's Magazine*, "The needle or wyers being Touched by the loadstone, are subject to be drawn aside by the guns in steerage, or by any iron near it, and liable to variation and do not show the true north and south, which ought continually to be observed by a good azimuth compass." Quoted in J. B. Hewson, *A History of the Practice of Navigation*, 58. See also "A Letter of Mr. De La Hire . . . concerning a new sort of Magnetical Compass, with several curious Magnetical Experiments," *Philosophical Transactions of the Royal Society*, 16 (1686-92), 344-351 and H. L. Hitchins and W. E. May, *From Lodestone to Gyro-Compass* (New York: Philosophical Library, 1953), 52.

[63] Matthew Flinders, "Concerning the Differences in the magnetic Needle, on Board the Investigator, arising from an Alteration in the Direction of the Ship's Head." *Philosophical Transactions of the Royal Society of London* (1805), 186-197. Flinders entered the Royal Navy in 1789. From 1791 to 1793, he served as a midshipman under William Bligh; he next joined *H. M. S. Reliance* under Captain John Hunter and sailed in the South Seas from 1795 to 1800. Largely through the influence of Joseph Banks, Flinders was given command of *H. M. S. Investigator* in 1801. The *Investigator* surveyed much of the coast of Australia. Returning to England in 1803, Flinders was unaware that England and France were at war and was detained by the French at the Ile de France until 1810. See also Charles H. Cotter, *Studies in maritime history*. Vol. 2: *A history of ship magnetism*. (London: Mansell, 1977), E3-E14, F1-F3; and James D. Mack, *Matthew Flinders, 1774-1814* (Melbourne: Nelson, 1966).

[64] Matthew Flinders, *A Voyage to Terra Australis; undertaken for the purpose of completing the discovery of that vast country, and prosecuted in the years 1801, 1802, and 1803, in His Majesty's Ship the Investigator* (London: G. and W. Nicol, 1814) [facsimile edition, Adelaide: Libraries Board of South Australia, 1966], II: 525.

[65] Ibid., 524.

[66] Matthew Flinders, *A Voyage to Terra Australis* (London: G. and W. Nicol, 1814) [facsimile edition, Adelaide: Libraries Board of South Australia, 1966], II: 514.

[67] Matthew Flinders, "Concerning the Differences in the magnetic Needle, on Board the Investigator, arising from an Alteration in the Direction of the Ship's Head," *Philosophical Transactions of the Royal Society of London* (1805), 196.

[68] Ibid., 194.

[69] Matthew Flinders, *A Voyage to Terra Australis* (London: G. and W. Nicol, 1814) [facsimile edition, Adelaide: Libraries Board of South Australia, 1966], II: 531.

[70] Matthew Flinders, *Notes upon the Magnetism of the Earth, and of Ships*, quoted in Charles H. Cotter, *Studies in maritime history.* Vol. 2: *A history of ship magnetism.* (London: Mansell, 1977), E9

[71] Matthew Flinders, *Voyage to Terra Australis*, (London, G. and W. Nicol, 1814) [facsimile edition, Adelaide: Libraries Board of South Australia, 1966], II: 522.

[72] William Bain, *An Essay on the Variation of the Compass showing how far it is influenced by a change in the direction of the ship's head* (Edinburgh: Printed for W. Blackwood and J. Murray, 1817) [Landmarks of science, microform, 1969].

[73] Ibid., 86.

[74] Ibid., 92.

[75] Ibid., 133-134.

[76] For more on iron used in shipbuilding see J. B. Hewson, *A History of the Practice of Navigation*, 60 and Charles H. Cotter, *Notes upon the Magnetism of the Earth, and of Ships*, quoted in Charles H. Cotter, *Studies in maritime history.* Vol. 2: *A history of ship magnetism.* (London: Mansell, 1977), D6-D10.

[77] Local attraction received particular attention at mid-century. See, for instance, Peter Barlow, "On the Errors in the Course of Vessels, occasioned by Local Attraction," *Philosophical Transactions of the Royal Society of London* (1831), 215-221; E. J. Johnson, *Practical illustrations of the necessity for ascertaining the deviations of the compass: with explanatory diagrams, and some account of the compass system now adopted in the Royal Navy.* London: J. D. Potter, 1852 (Second edition); Great Britain Hydrographic Office. *Practical Rules for ascertaining and applying the deviations of the compass caused by the iron in a ship* (London: Printed for the Hydrographic Office, 1855); and Liverpool Compass Committee, *First and second <and third> reports of the Liverpool Compass Committee to the Board of Trade*, 1855 & 1856 (London: Printed by G. E. Eyre and W. Spottiswoode, for H. M. Stationery Office, 1857-62).

[78] The history of ship magnetism has been dealt with in the existing scholarship, particularly by maritime historian, Charles H. Cotter. See Charles H. Cotter, "The Royal Society and the Deviation of the Compass," *Notes and Records of the Royal Society*, 31 (1977), 297- 310; Charles H. Cotter, *Studies in maritime history*, Vol. 2: *A history of ship magnetism* (London: Mansell, 1977); Charles H. Cotter, "The Early History of Ship Magnetism: The Airy-Scoresby Controversy," *Annals of Science*, 34 (1977), 589-599; Charles H. Cotter, "George Biddell Airy and his Mechanical Correction of the Magnetic Compass," *Annals of Science*, 33 (1976), 263-274; Charles H. Cotter, "Matthew Flinders and Ship Magnetism," *Journal of Navigation*, 29 (1976),123-134; and Charles H. Cotter, "Edward J. Johnson and the early history of ship magnetism," *Journal of Navigation*, 32 (1979), 415-421. See also Tom and

Cordelia Stamp, *William Scoresby, Arctic Scientist* (Whitby: Caedmon, 1976), 131-139; W. E. May, "Lord Kelvin and his compass," *Journal of Navigation*, 32 (1979), 122-134; Crosbie Smith and M. Norton Wise, *Energy and empire: a biographical study of Lord Kelvin* (Cambridge: Cambridge University Press, 1989), 754-798; and Alison Winter, "'Compasses All Awry': The Iron Ship and the Ambiguities of Cultural Authority in Victorian Britain," *Victorian Studies*, 38 (Autumn 1994), 69-98.

[79] Gilpin served as Clerk of the Royal Society from 1785 to 1809 and Secretary of the Board of Longitude from 1801 to 1809. See G. E. R. Deacon and Margaret Deacon, "Captain Cook as a Navigator," *Notes and Records of the Royal Society of London*, 24 (1969), 39. George Gilpin, "Observations on the Variation, and on the Dip of the magnetic Needle, made at the Apartments of the Royal Society," *Philosophical Transactions of the Royal Society of London* (1806), 385-419, and Mark Beaufoy, "Magnetical and Meteorological Observations," *Annals of Philosophy*, 10 (1817), 76, 156, 236, 316, 396, 470, and 11 (1818), 76, 156, 236, 316, 397, 470.

[80] George Gilpin, "Observations on the Variation, and on the Dip of the magnetic Needle, made at the Apartments of the Royal Society," *Philosophical Transactions of the Royal Society of London* (1806), 393. Using Coulomb's magnetic balance and an inclinometer, Cassini (IV) made daily observations at the Observatory. However, irregular vibrations marred the measurements sending Cassini to Coulomb for advice. Coulomb determined that electrical effects caused by the observer upon the silk suspension threads were the culprit. Although Cassini's *De la déclination et des variations de l'aiguille aimantée* (1791) established the relationship between the annual variation of declination and the position of the sun relative to the equinoxes and solstices, Cassini stopped work in the later 1790s as a Royalist political protest. See John Cawood, "Terrestrial Magnetism and the Development of International Collaboration in the Early Nineteenth Century," *Annals of Science*, 34 (1977), 555-557.

[81] Ibid., 397-398.

[82] [Agnes M. Clerke], "Beaufoy, Mark (1764-1827)," *DNB*, 4: 51. For details on Beaufoy's research into the ship resistance problem, see Thomas Wright, "Mark Beaufoy's Nautical and Hydraulic Experiments," *Mariner's Mirror*, 75 (1989), 313-327; and Mark Beaufoy, *Nautical & Hydraulic Experiments* (London, 1834).

[83] Colonel Mark Beaufoy, "On the retrograde Variation of the Compass," *Thomson's Annals of Philosophy*, 15 (1820), 339.

[84] The body of scholarship on the search for the North-west passage is enormous. For discussion of earlier attempts see Richard Vaughan, *The Arctic, A History* (London: Alan Sutton Publishing, 1994); L. P. Kirwan, *A History of Polar Exploration* (New York: W. W. Norton & Company, 1960); and Nellis M. Crouse, *The Search for the Northwest Passage* (New York: Columbia University Press, 1934). See also Pierre Berton, *The Arctic Grail: The quest for the North West Passage and the North Pole, 1818-1909* (New York: Viking, 1988), and Alan Edwin Day, *Search for the Northwest Passage: An Annotated Bibliography* (New York: Garland Publishing, Inc., 1986).

[85] Trevor H. Levere, *Science and the Canadian Arctic: A Century of Exploration 1818-1918* (Cambridge: Cambridge University Press, 1993), 37.

[86] William Scoresby quoted in Tom Stamp and Cordelia Stamp, *William Scoresby, Arctic Scientist* (Whitby: Caedmon, 1976), 64. Scoresby had written several years earlier about the possibilities of reaching the North Pole over the ice. See William Scoresby, "On the Greenland or Polar Ice," *Memoirs of the Wernerian Natural History Society*, 2 (1815), 261-338.

[87] Ibid., 66.

[88] See Daines Barrington, *The Possibility of Approaching the North Pole Asserted.* A new edition with an appendix, containing papers on the same subject, and on a northwest passage by Colonel Beaufoy (New York: James Eastburn & Co., 1818), 180.

[89] See B. M. Gough, "British-Russian rivalry and the search for the Northwest Passage in the early 19th Century," *Polar Record*, 23 (1986), 301-317.

[90] John Barrow, "Review Essay of *Narrative of a Voyage to Hudson's Bay, in His Majesty's Ship Rosamond*. . . by Lieut. Chappell, R. N." *The Quarterly Review*, 18 (1818): 219. See also John Barrow, *A Chronological History of Voyages into the Arctic Regions, Undertaken chiefly for the purpose of discovering a North-East, North-West or Polar passage between the Atlantic and Pacific* (London: John Murray, 1818) [Reprint New York: Barnes & Noble, 1971], 364-365.

[91] Daines Barrington, *The Possibility of Approaching the North Pole Asserted.* A new edition with an appendix, containing papers on the same subject, and on a northwest passage by Colonel [Mark] Beaufoy (New York: James Eastburn & Co., 1818).

[92] Nellis M. Crouse, *The Search for the Northwest Passage* (New York: Columbia University Press, 1934), 36-37. See also Alexander Wood, completed by Frank Oldham. *Thomas Young: Natural Philosopher, 1773-1829* (Cambridge: Cambridge University Press, 1954), 305-306 and Alfred Friendly, *Beaufort of the Admiralty: The Life of Sir Francis Beaufort, 1774-1857* (New York: Random House, 1977), 301.

[93] Jerome Lalande, "On the Northern Magnetic Pole of the Earth," *Philosophical Magazine*, 14 (1802-03), 249-251.

[94] William Scoresby, "On the Greenland or Polar Ice," *Memoirs of the Wernerian Natural History Society*, 2 (1815), 337-338.

[95] Mark Beaufoy, "On the North-West Passage; and the Insular Form of Greenland," *Annals of Philosophy*, 10 (1817), 427. See also Mark Beaufoy, Appendix in Daines Barrington, *The Possibility of Approaching the North Pole Asserted* (New York: James Eastburn & Co., 1818), 185.

[96] Ibid., 427.

[97] "On the Practicability of a direct Passage over the North Pole," *Philosophical Magazine*, 51 (1818), 295.

[98] John Barrow, "Review Essay of *Narrative of a Voyage to Hudson's Bay, in His Majesty's Ship Rosamond* . . . by Lieut. Chappell, R. N." *The Quarterly Review*, 18 (1818), 199.

[99] Ibid., 222-223.

[100] John Barrow, *A Chronological History of Voyages into the Arctic Regions, Undertaken chiefly for the purpose of discovering a North-East, North-West or Polar passage between the Atlantic and Pacific* (London: John Murray, 1818) [Reprint New York: Barnes & Noble, 1971], 378-379.

[101] Nellis M. Crouse, *The Search for the Northwest Passage* (New York: Columbia University Press, 1934), 41.

[102] John Cawood, "Terrestrial Magnetism and the Development of International Collaboration in the Early Nineteenth Century," *Annals of Science*, 34 (1977), 571. Hinting at the national rivalry with France, a French voyage set out in 1817 headed by Louis Freycinet, who was instructed to collect magnetic data. Cawood argues that this rivalry had political rather than scientific origins.

[103] John Ross, *A Voyage of Discovery, Made under the Orders of the Admiralty, in His Majesty's Ships Isabella and Alexander, for the Purpose of Exploring Baffin's Bay, and Inquiring into the Probability of a North-West Passage* (London: J. Murray, 1819), 10.

[104] Ibid.

[105] Ibid., Introduction, xvi-xix. Some of the books were by Wales and Bayly; Cook, King and Bayly; and Phipps. For biographical information on Sabine see Nathan Reingold, "Sabine, Edward (1788-1883)," *DSB*, 12: 49-53 and [R. H. V.], "Sabine, Edward," *DNB*, 51: 74- 78.

[106] Ibid., 10.

[107] Ibid., Introduction, iii (my emphasis).

[108] Ibid., Appendix, No. I., vii-viii.

[109] William Edward Parry, quoted in Rev. Edward Parry, *Memoirs of Rear-Admiral Sir. W. Edward Parry* . . . by his son (New York: Protestant Episcopal Society for the Promotion of Evangelical Knowledge, 1858), 73-74.

[110] William Edward Parry, quoted in Ann Parry, *Parry of the Arctic, the life story of Admiral Sir Edward Parry, 1790-1855* (London: Chatto & Windus, 1963), 37-38.

[111] For a brief account of the controversy see A. G. E. Jones, "Sir John Ross and Sir John Barrow," in *Polar Portraits, Collected Papers* (Caedmon of Whitby, 1992), 219-228.

[112] Edward Sabine, *Remarks on the Account of the late Voyage of Discovery to Baffin's Bay, published by Captain J. Ross, R. N.* (London: Richard and Arthur Taylor, 1819), 29. For Barrow's scathing review see John Barrow, "Review Essay of *A Voyage of Discovery, made under the order of the Admiralty, in His Majesty's Ships Isabella and Alexander . . .* by John Ross," *The Quarterly Review*, 21 (1819), 213-262.

[113] Ibid., 10-11.

[114] John Ross, *An Explanation of Captain Sabine's Remarks on the Late Voyage of Discovery to Baffin's Bay* (London: John Murray, 1819).

[115] Ibid., 12-13.

[116] Edward Sabine, "On Irregularities observed in the direction in the direction of the Compass Needles of H.M.S. *Isabella* and *Alexander* caused by the attraction of the iron contained in the Ships," *Philosophical Transactions of the Royal Society of London* (1819), 112-123. See also Edward Sabine, "Observations on the dip and variation of the magnetic needle, and on the intensity of the magnetic force, made during the late voyage in search of a north-west passage," *Philosophical Transactions of the Royal Society of London* (1819), 132-144.

[117] Ibid., 117-118. Sabine advised choosing an object sufficiently distant so that swinging the ship did not subtend a sensible parallax.

[118] Edward Sabine. "Observations on the dip and variation of the magnetic needle, and on the intensity of the magnetic force, made during the late voyage in search of a north-west passage," *Philosophical Transactions of the Royal Society of London* (1819), 142.

[119] Gregory A. Good, "Following the Needle: Seeking the Magnetic Poles," *Earth Sciences History*, 10 (1991), 162.

[120] Sabine, "Observations on the dip and variation of the magnetic needle, and on the intensity of the magnetic force, made during the late voyage in search of a north-west passage," *Philosophical Transactions of the Royal Society of London* (1819), 142.

[121] Alexander Fisher, *Journal of a Voyage of Discovery, to the Arctic Regions, Performed between the 4th of April and the 18th of November, 1818, in His Majesty's Ship Alexander* (London: Printed for Richard Phillips, 1821), iii.

[122] Ibid., 25.

[123] Ibid., 38-41, 49-50, 87-88.

[124] F. W. Beechey, *A Voyage of Discovery Towards the North Pole Performed in*

His Majesty's Ships Dorothea and Trent under the command of Captain David Buchan, R. N.; 1818 (London: Richard Bentley, 1843), 27-28.

[125] Ibid., Appendix, 337. Into the 1830s, Fisher collected and compiled global magnetic data. See George Fisher, "An account of the Magnetical experiments made on the Western Coast of Africa, 1830-31, by Commander Edward Belcher, of *H. M. S. Aetna*," *Philosophical Transactions of the Royal Society of London* (1832), 493-496; George Fisher, "Magnetical experiments made principally in the south part of Europe and Asia Minor, during the years 1827 to 1832," *Philosophical Transactions of the Royal Society of London* (1833), 237-252; see George Fisher, "Magnetical observations made in the W. Indies, on the N. coast of Brazil and N. America in 1834-37, by Capt. Sir E. Home," *Philosophical Transactions of the Royal Society of London* (1838), 343-349.

[126] "Extracts from a journal aboard the *Dorothy* and *Trent*," *London Times* (16 October, 1818), 3c.

[127] William Edward Parry, *Journal of a voyage for the discovery of a north-west passage from the Atlantic to the Pacific; performed in the years, 1819-20, in His Majesty's ships Hecla and Griper*, with an appendix containing the scientific and other observations (London: John Murray, 1821) [reprinted New York: Greenwood Press (1968)], vi.

[128] Ibid., xxv.

[129] Ibid., xxv-xxvi (my emphasis).

[130] Ibid., xv. Privately, Parry speculated constantly on magnetism, which he found the most interesting subject of the entire voyage. For instance, he conjectured that changes in magnetic variation might be linked to changes in weather or the state of the Greenland ice. See Levere, *Science and the Canadian Arctic: A Century of Exploration 1818-1918* (Cambridge: Cambridge University Press, 1993), 65-66.

[131] Edward Sabine, *An account of experiments to determine the figure of the earth, by means of the pendulum vibrating seconds in different latitudes: as well as on various other subjects of philosophical inquiry* (Printed at the expense of the Board of Longitude. London: John Murray, 1825), 463-464.

[132] See Edward Sabine, "The Force of Magnetism, compared with the Dip, Extracted from Captain Sabine's Appendix to Captain Parry's Journal," *Quarterly Journal of Science, Literature and the Arts*, 12 (1821), 374-378, and William Edward Parry, *Journal of a voyage for the discovery of a north-west passage from the Atlantic to the Pacific; performed in the years, 1819-20, in His Majesty's ships Hecla and Griper*, with an appendix containing the scientific and other observations (London: John Murray, 1821), cvii- cxxix.

[133] Alexander Fisher, *A Journal of a Voyage of Discovery to the Arctic Regions in H.M.S. Hecla and Griper in the years 1819 & 1820* (London: Longman, Hurst, Rees, Orme, and Brown, 1821), 54.

[134] *Letters written during the late Voyage of Discovery in the Western Arctic Sea* by an officer of the expedition (London: Printed for Sir Richard Phillips and Co., 1821), 12-13, 26-30, 57.

[135] Ibid., 29

[136] Letter from John Barrow to Henry Goulburn, 26 February 1819. in *Sir John Franklin's Journals and Correspondence: the First Arctic Land Expedition 1819-1822*, edited by Richard C. Davis (Toronto: The Champlain Society, 1995), 278. See also Samuel Hearne, *A Journey from Prince of Wales's Fort in Hudson's Bay to the Northern Ocean* . . . (Rutland, Vt.: Charles E. Tuttle Company, 1971), 64-65.

[137] Instructions from Lord Bathhurst to John Franklin, 29 April, 1819, quoted from *Sir John Franklin's Journals and Correspondence: the First Arctic Land Expedition 1819-1822*, edited with an introduction by Richard C. Davis (Toronto: The Champlain Society, 1995), 287.

[138] See General Orders from John Franklin to John Richardson, George Back, and Robert Hood, 22 June 1819, quoted in *Sir John Franklin's Journals and Correspondence: the First Arctic Land Expedition 1819-1822*, edited with an introduction by Richard C. Davis (Toronto: The Champlain Society, 1995), 297-299.

[139] John Franklin, *Narrative of a Journey to the Shores of the Polar Sea in the Years 1819, 20, 21, and 22* (London: John Murray, 1823), Appendix No. IV., 641.

[140] Official Instructions quoted in William Edward Parry, *Journal of a second voyage for the discovery of a north-west passage from the Atlantic to the Pacific;* performed in the years 1821-22-23, in His Majesty's Ships *Fury* and *Hecla* (London: J. Murray, 1824), xxvii.

[141] William Edward Parry, *Journal of a third voyage for the discovery of a north-west passage from the Atlantic to the Pacific;* performed in the years 1824-25 (Philadelphia: H. C. Carey and I. Lea, 1826), 63.

[142] See Henry Foster, "Observations on the diurnal changes in the position of the horizontal needle, under a reduced directive power, at Port Bowen, 1825," *Philosophical Transactions of the Royal Society of London* (1826), part 4: 129-176; Henry Foster, "A comparison of the diurnal changes of intensity in the dipping and horizontal needles, at Port Bowen," *Philosophical Transactions of the Royal Society of London* (1826) part 4: 177-187; and Henry Foster, "A comparison of the changes in magnetic intensity throughout the day in the dipping and horizontal needles, at Treurenburgh Bay in Spitsbergen," *Philosophical Transactions of the Royal Society of London* (1828), 303-311. John Franklin's second land expedition (1825-27) also performed experiments suggested by Barlow and Christie. See John Franklin, *Narrative of a Second Expedition to the Shores of the Polar Sea in the Years 1825, 1826, and 1827* (London: John Murray, 1828), Appendices V. and VI.

[143] William Edward Parry, *Journal of a third voyage for the discovery of a north-west passage from the Atlantic to the Pacific*; performed in the years 1824-25 (Philadelphia: H. C. Carey and I. Lea, 1826), 64.

[144] William Edward Parry and Henry Foster, "Magnetical Observations at Port Bowen, &c. A. D. 1824-25, comprehending observations on the diurnal variation and diurnal intensity of the horizontal needle," *Philosophical Transactions of the Royal Society of London*, 116 (1826), part 4: 75-76.

[145] Ibid., 78.

[146] Captain Henry Kater, "Bakerian Lecture: 'On the best kind of steel and form for a compass needle,'" *Philosophical Transactions of the Royal Society of London* (1821), 104-129. An officer in the army and treasurer of the Royal Society from 1827 to 1833, Kater (1777-1835) is best remembered for designing a reversible pendulum used to measure the local force of gravity. Primarily focusing on instrumentation and accurate measurement, Kater also designed instruments to measure magnetism, humidity and various astronomical phenomena. See Gordon Jones, "The Scientific Publications of Henry Kater," *Centaurus*, 11 (1965), 152-153.

[147] Ibid., 104.

[148] W. E. Parry, "Observations on the Variation," in *Journal of a voyage for the discovery of a north-west passage from the Atlantic to the Pacific; performed in the years, 1819-20, in His Majesty's ships Hecla and Griper*, with an appendix containing the scientific and other observations (London: John Murray, 1821) [reprinted New York: Greenwood Press (1968)], Appendix, cxvi.

[149] Edward Sabine, "The Bakerian Lecture. An Account of Experiments to determine the amount of the Dip of the Magnetic Needle in London, in August 1821; with Remarks on the Instruments which are usually employed in such determinations," *Philosophical Transactions of the Royal Society of London* (1822), 1.

[150] Edward Sabine, *An account of experiments to determine the figure of the earth, by means of the pendulum vibrating seconds in different latitudes: as well as on various other subjects of philosophical inquiry* (Printed at the expense of the Board of Longitude. London: John Murray, 1825), 498.

[151] John Macdonald, "On the North-West Magnetic Pole," *Gentleman's Magazine*, 92 (1822), part 2: 210-211. Macdonald (1759-1831) obtained an Indian cadetship in 1780 and was first posted to the Bombay infantry. Appointed to the Bengal engineers in 1783, Macdonald went to Sumatra where he remained until 1796. While in Sumatra he made numerous charts, maps, and observations on diurnal variation. Elected F. R. S. in 1800, Macdonald was a prolific writer on many different subjects including Asia, telegraphy, education, and music. He also translated several French treatises on infantry tactics. See "Macdonald, John," *DNB*, 35: 40-41. See also John Macdonald, "Observations of the diurnal Variation of the Magnetic Needle at Fort Marlborough, in the Island of Sumatra." *Philosophical Transactions of the Royal Society of London*, 86 (1796), 340-349 and "On the diurnal Variation of the Magnetic Needle in St. Helena." *Philosophical Transactions of the Royal Society of London*, 88 (1798), 397.

[152] James Clark Ross, "On the Position of the North Magnetic Pole." *Philosophical Transactions of the Royal Society of London* (1834), 51. See also "Commander Ross's Paper Relative to the North Magnetic Pole," *London Times* (26 December, 1834), 3e (my emphasis).

[153] For more on the actual discovery and controversy, see Ross, M. J. *Polar Pioneers John Ross and James Clark Ross* (Montreal & Kingston: McGill-Queen's University Press, 1994), 153-155; and Robert Huish, *The Last Voyage of Capt. Sir John Ross to the Arctic Regions; for the discovery of a north west passage; performed in the years 1829-30-31-32 and 33* (London: John Saunders, 1836), "On the Position of the North Magnetic Pole," supplement, 1-44.

[154] See G. E. Fogg, *A History of Antarctic Science* (Cambridge: Cambridge University Press, 1992), 74-93; John Cawood, "The Magnetic Crusade: Science and Politics in Early Victorian Britain," *Isis*, 70 (1979), 493-518; and Ann Savours, "Sir James Clark Ross, 1800-1862," *Geographical Journal*, 128 (1962), 325- 327.

[155] Captain Washington, "On the Recent Expeditions to the Antarctic Seas," Report on British Association Meeting, Section C.— Geology and Geography, *The Athenaeum* (1838), 631.

[156] See David Philip Miller, "The Revival of the Physical Sciences in Britain, 1815-1840," *Osiris*, 2 (2nd Series, 1986), 119-127.

[157] Trevor H. Levere, "Magnetic Instruments in the Canadian Arctic Expeditions of Franklin, Lefroy, and Nares," *Annals of Science*, 43 (1986), 58.

[158] See D. P. Miller, *The Royal Society of London, 1800-1835: A study in the cultural politics of scientific organization* (Ph.D. dissertation, University of Pennsylvania, 1981), 198-227. Miller divides investigators into three groups: mathematical practitioners, scientific servicemen, and members of the Cambridge network.

CHAPTER 3:
MAGNETISM:
MYSTERIOUS, IMPORTANT, CONFUSING
(c.1750-1790)

Despite the popularity of Cartesian theories of magnetism after 1750, little consensus existed among British investigators regarding the causes of magnetic phenomena. A well-read student of magnetism encountered general concord regarding basic magnetic phenomena, yet little agreement respecting their explanations or causes. This lack of theoretical unanimity frequently accompanied the portrayal of magnetic knowledge as uncertain or incomplete. Reinforcing these perceptions, as illustrated in the previous chapter, were huge gaps in terrestrial magnetic data, difficulties with the instruments, and the inherently complex and ever-changing nature of terrestrial magnetic phenomena. Whether investigators explained magnetism in terms of circulating effluvia, a material ether, forces acting at a distance, or supported none of these hypotheses, the subject remained enigmatic and confusing. Furthermore, because investigators often looked to Newton's writings to justify each of these possibilities, it is extremely difficult to label call them "Newtonian" without considerable qualification.

Just as magnetism's mysteriousness persisted, the experimental facts related to the subject remained relatively the same during the century. In 1730, Servington Savery reported to the *Philosophical Transactions* basic properties of magnetism including the attractive power of lodestones to iron and steel, the repulsion and attraction of magnetic poles, the communication of magnetism by contact or proximity, the destruction of magnetic power by heating, and the superior power of magnetic steel over iron. Furthermore, he recognized the directive and dipping properties of magnetized needles. Though the terrestrial magnetic poles were some distance from the geographical poles, Savery accepted the Gilbertian notion that earth contained a large magnet or lodestone. Almost twenty years later, London instrument maker Benjamin

Martin compiled a nearly identical list in his *Philosophia Britannica.*[1] Well into the eighteenth century, the experimental data remained fairly constant. Also, until late in the century, the study of magnetism in Britain was dominated by experimentalists with little mathematical training and gentlemanly speculators who dabbled in science.

In typical fashion, John Freke (1662-1744), an opthalmological surgeon at St. Bartholomew's Hospital, remarked around mid-century of the great benefits of magnetism's study to mankind, yet candidly admitted that "no satisfactory Account has yet been written of it."[2] The causes of magnetism and its global manifestations were unknown, hence Freke and many others referred to the subject as a puzzling subject of great practical worth. In contrast to universal gravitation, magnetism did not seem to obey a law of attraction with respect to distance. As well, the eighteenth-century rejection of Halley's theory accompanied the widespread acceptance of the notion that intricate, unobservable alterations within the earth generated magnetic variation. As a result, magnetic and terrestrial magnetic phenomena rarely went beyond the experimental tradition into the realm of mathematics. Investigators continued calling for empirical evidence to strengthen their speculations.

In 1761, English chaplain Temple Henry Croker remarked, "to this Day, [magnetism] remains but little understood, the least so of any general Law of Nature."[3] Croker sought to disprove the Cartesian system and Halley's four-pole hypothesis. His goal was to prove there was no "Central Loadstone, or a Magnetic Atmosphere . . . but that *Vis Magnetica* (whatever that Occult Power may be) acts, and directs the Needles, to the Horizon only."[4] Supposing that magnetized needles lost gravity from their south end, he asked, "Who can any longer doubt of Gravity being joined with Magnetism in the Motion of the Dipping Needle?" Magnetic attraction, therefore, acted only toward the horizon. If magnetism acted in Cartesian fashion as a "circumambient fluid," Croker argued that magnets should attract more according to their surface area rather than their mass.

This expectation, he explained, contradicted evidence showing that magnets attracted with the same power "edgeways" and "flatways."[5] Concluding that only further experiments would resolve magnetism's laws, Croker considered its cause an unknown "occult power."

Three years later, *The Complete Dictionary of Arts and Sciences* (1764), compiled by Croker and two others, gave a different, though still uncertain, description of magnetic phenomena. After listing the commonly-known magnetic properties, the entry "Magnet," reported inconclusive attempts like Musschenbroek's to find a law of magnetic attraction. Again rejecting Halley's shell-nucleus theory, the author conceded that irregular changes in declination and inclination were not subject to calculation. Such complex variations indicated unknown, hidden causes deep within the earth. Beyond these apprehensions, the author advocated an effluvial theory. Hence, every lodestone had two points or poles emitting magnetic virtue or effluvia. The patterns of iron filings sprinkled over a magnet illustrated the directions taken by these effluvia. Furthermore, the patterns demonstrated that the "magnetic virtue emitted from each pole, circulates to, and enters the other [pole]."[6]

Several years later, Irish poet and scientific dabbler Oliver Goldsmith similarly contended in *A Survey of Experimental Philosophy* that extremely fine magnetic effluvia pervaded even the hardest bodies. From patterns of iron filings spread over a magnet, Goldsmith inferred that the earth acted as one great magnet "sending forth effluvia in the same manner."[7] Iron and other ferruginous bodies acquired magnetism by lying in the direction of these effluvial currents. He rejected Halley's four-pole hypothesis, yet offered no alternative. Reminiscent of Descartes' analogy, Goldsmith noted that magnetic bodies generally assumed a polar direction just as timber in a stream floated lengthwise following the current. Admitting that no general law gave relationship between distance and magnetic power, Goldsmith concluded that every new magnetic experiment brought with it new wonders.[8]

Around the same time as Goldsmith, Englishman Richard Lovett (fl. 1750) of the Cathedral Church of Worcester offered a slightly different effluvial explanation in his *Philosophical Essays in Three Parts* (1766). Lovett supposed that an ether passed perpetually and swiftly through the upper regions of the earth. Magnetic virtue was communicated to magnets "by means of a subtile Matter constantly passing through them."[9] While supposing the identity of Newton's ether and electric fluid, Lovett contended that an even more subtle agent passed through vertical bars of iron, horizontal bars, and even glass as freely as if nothing impeded it. From this supposition, he concluded that the magnetic agent and the "pneuma" must be one and the same principle, although in different forms.[10] Like most discussions of magnetic phenomena of the time, Lovett's treatment remained brief, qualitative, and speculative.

In 1782, London mathematician and chemist William Nicholson (1753-1815) agreed that no law satisfactorily related distance to magnetic power. Unlike many other investigators, however, Nicholson rejected effluvial or etherial conjectures. Favoring unexplained forces acting at a distance, he declared that the physical causes of magnetic attractions and repulsions remained entirely unknown. Since the magnetic kernel and shell seemed the best explanation for changes in declination, Nicholson positively assessed Halley's four-pole hypothesis . Indeed, numerous geomagnetic observations confirmed the existence of more than two magnetic poles. Nicholson warned, however that these measurements had not continued long enough to allow "foundation for a good theory."[11]

The admitted lack of comprehension, applying to magnetic and terrestrial magnetic phenomena alike, persisted throughout the eighteenth century. Referring to magnetic dip and variation in 1774, William Mountaine of the Royal Society of London wrote, "as the true theory of this *arcanum* in nature is yet so little known, every thing that serves to illustrate it, deserves attention."[12] Acknowledging the capricious nature of magnetic variation, the Reverend Erasmus Middleton noted in 1778 that its cause remained "hitherto without any demonstrative discovery."[13] Six years later, London instrument maker George Adams asserted that no satisfactory hypotheses accounted for various magnetic properties.[14] In 1790, Thomas Harding of the Royal Irish Academy noted that the variation of the compass had not yet fallen to "any philosopher, notwithstanding the several hypotheses" put forth, while another remarked four years later that "no theory has as yet been established, or has proved to a conviction, what magnetism is."[15] In 1795, Charles Hutton, professor of mathematics at the Royal Military Academy, Woolwich, explained that in spite of many hypotheses put forth concerning the causes of magnetism, "nothing however has yet appeared that can be called a satisfactory solution of its phenomena."[16] Soon after, George Gregory, an Edinburgh-educated parish priest, described the cause of magnetism as "one of the undiscovered principles of natural philosophy," while John Imison, a watchmaker from Manchester, lamented that "very few additions have been made to the discoveries of the first enquirers upon the subject."[17] Into the nineteenth century, others reiterated sentiments reflecting magnetism's continuing enigmatic nature.[18]

Accompanying these admissions, investigators frequently championed the importance of magnetic knowledge to navigation and natural philosophy. Nicholson, for instance, supposed that future studies of magnetism would give clues revealing the causes of gravitational, cohesive, and electrical forces.[19] Harding reported that if magnetic laws were "universally known, the Longitude could be more readily ascertained by them, than by any other means."[20] Adams remarked in 1794 that the directive power of the magnetic needle was of the "greatest importance to mankind," allowing the mariner to traverse the oceans, thus uniting "the arts, the manufactures, and the knowledge of different countries, together."[21] He specifically praised the usefulness of the compass to England, a nation whose riches and power relied heavily on navigation.[22] In 1801, Edinburgh University professor of natural philosophy, John Robison asserted that the magnetism of the earth was of "very great importance, both to the philosopher and to society."[23] The study of magnetism, it was generally agreed, could reap great practical and scientific benefits.

Despite its acknowledged importance, coverage of magnetism remained brief in comparison to other areas of experimental physics, particularly electricity. Uneven treatments of electricity and magnetism were commonplace. The study of magnetism remained a small domain within the experimental philosophy. Goldsmith wrote twice as much on electricity. William Enfield, a teacher at the dissenting Warrington Academy, devoted three times as many pages to electricity as to magnetism.[24] At Cambridge, George Atwood's 1784 lectures covered electricity in eighteen pages with only three on magnetism.[25] Adam Walker, an itinerant lecturer in natural philosophy, spent nearly seventy pages on electricity, yet only twenty on magnetism in his textbook.[26] In like manner, George Adams, who wrote almost forty pages on magnetism, appended this to his three-hundred page *An essay on electricity* (1784).[27]

If there were fewer pages devoted to the study of magnetism, there were also fewer books and articles on the subject. The index of *Philosophical Transactions* articles published between 1710 and 1780 cited over 130 entries on electricity, yet only seventy related to magnets and magnetism.[28] While publishers printed numerous books devoted solely to the popular study of electricity, Tiberius Cavallo's *A Treatise on Magnetism* (1787) remained one of very few English books on magnetism published in the second half of the eighteenth century.[29] Despite the frequently professed practical and scientific importance of magnetism, the greater entertainment and possible utility offered by electrical demonstrations consistently won a larger following (among natural philosophers and spectators alike).[30] As Joseph Priestley noted in 1775, "electricity has one considerable advantage over most other branches of science, as it both furnishes matter of speculation for philosophers, and of entertainment for all persons promiscuously."[31] Indeed, eighteenth-century electrical research revealed many new, exciting effects frequently described as "wonderful."[32] In contrast, the mundane effects derived from magnetic experiments remained relatively constant for much of the century. Magnetism might be extremely important, but it did not grab one's attention.

The Persistence of Circulating Effluvia Theories

Despite the mysterious nature of magnetism and lack of theoretical consensus, effluvial explanations remained popular in the second half of the eighteenth century. Circulating fluid theories flourished on the Continent and in Britain. Relying on the impulsion of particles of magnetic effluvium, these theories remained qualitative and non-mathematical. During the 1760s and 1770s, Swiss natural philosopher and mathematician Leonhard Euler furnished support for circulating effluvia with the publication of the popular work, *Letters to a German Princess* (1768-1772, three volumes).[33] With a bit of historical irony, Euler, the most prolific mathematical physicist of the eighteenth century, continued espousing a non-mathematical theory of magnetism. In Cartesian fashion, he supposed an extremely subtle matter which circulated around all magnets forming a vortex. In like manner, the earth itself

> must be surrounded with a similar vortex, acting everywhere on magnetic needles, and making continual efforts to dispose them according to its own direction . . . this subtile matter is continually issuing at one of the magnetic poles of the earth, and after having performed a circuit round to the other pole, it there enters, and pervades the globe through and through to the opposite pole, where it again escapes.[34]

In contrast to his qualitative effluvial theory, Euler developed a mathematical theory of terrestrial magnetic variation. Like many others, Euler rejected Halley's theory of secular variation. However, he went further in demonstrating that two magnetic poles, if not diametrically opposed to each other, explained the magnetic data just as well, if not better than, four poles.[35] In 1757, Euler judged Halley's hypothesis:

> too daring at least for the present state of our knowledge, since the directive force, of two or more magnets acting at the same time on one needle, is all entirely unknown: and it would be no doubt better to first abandon this enterprise, than to found it on arbitrary hypotheses.[36]

In contrast to Halley, Euler did not propose a physical mechanism for the motions of the terrestrial magnetic poles. Instead, in the mathematical tradition, his theory gave an idealized mathematical treatment of secular magnetic variation. This approach sought to accurately explain the phenomena without worrying about the physical causes. Meanwhile, Euler's effluvial magnetic theory remained qualitative and non-mathematical.

Like Euler, most British investigators appealed to circulating fluid theories through the 1770s and 1780s. Effluvia remained a common way of explaining both magnetism and earthly magnetism. Hence, despite Euler's efforts to mathematize terrestrial magnetic variation, the British study of magnetism remained within the experimental tradition. Benjamin Martin explained in his "comprehensive system of the Newtonian Philosophy" published in 1771 that patterns of iron filings strewn over a magnet illustrated that magnetic effluvia emitted from one pole circulated and entered the other pole.[37] In 1774, William Hooper wrote in a popular treatment that effluvia passed from one pole to the other producing magnetic attraction.[38] Summarizing Euler's theory in 1784, George Adams claimed that most writers agreed with explanations relying on "corpuscles of a peculiar form and energy, which continually circulate around and through a magnet."[39] In like manner, terrestrial magnetism arose from "a vortex of the same kind" circulating around and through the earth. Similarly, the 1786 edition of Abraham Rees' *Cyclopaedia* included the statement from Ephraim Chambers' earlier editions:

> An opinion that has much prevailed among the moderns is that of DesCartes, maintained by Malebranche, Rohault, Regis, &c. and even admitted and confirmed by Mr. Boyle, &c. In this it is supposed, that there is continually flowing, from the poles of the world, a subtle, impalpable, and invisible matter, channelled or striated: which matter, circulating round the earth, in the planes of the meridians, re-enters at the pole . . . [40]

Professor of natural philosophy at the University of Glasgow John Anderson (1726-1796) wrote that magnetic power seemed "to move in a stream from one pole of a magnet to the other, internally; and to be then carried back in curve lines externally, till it arrives at the pole to be again admitted." Anderson attributed magnetic attraction to the "flux of the same stream" of magnetic matter through magnetic bodies and magnetic repulsion to the matter's "conflux and accumulation".[41] In 1788, Captain O'Brien Drury similarly reported to the Royal Irish Academy that a magnetic fluid circulated "continually around and through a magnet" as demonstrated by patterns of iron filings placed on glass over a magnet.[42] Flow-like patterns of iron filings continued to be the primary experimental evidence for circulating effluvia.

At the turn of the century, explanations relying on circulating magnetic effluvia enjoyed continued acceptance. For instance, the fourth edition of Imison's *School of Arts* (1796) explained that when rendering steel magnetic, it became necessary to dispose its pores such that they formed contiguous parallel tubes, capable of receiving the effluvia "so that the magnetic stream may enter with ease, and be made to circulate through it with the greatest force."[43] If a magnet were too short, then the fluid emerging from one pole would be "repelled and thrown back by the other acting parts of the magnet, and thus be carried too far from the pole into which it ought to enter," thereby hindering circulation.[44] In 1799, Adam Walker supposed a "subtil effluvium" flowing around and through the earth making it act as one great magnet.[45] Therefore, iron held within the terrestrial effluvial flow received the magnetic virtue from a rearrangement of its pores. A decade later, Walker reiterated that the direction of the earth's magnetic effluvium probably caused iron long remaining perpendicular (e.g., weather vanes, iron bars) to become magnetic.[46]

By the late eighteenth century, however, effluvial explanations met serious challenges. Advocates of circulating effluvia recognized its questionable status and often defended it through analogy. Such explicit defenses of effluvial theories suggest that they were being criticized by others. In 1799, Walker admitted that some might scoff at the "occult" qualities of Descartes' subtle matter, Euler's ether, or his own effluvium. He countered, however, that when imperceptible physical causes could be surmised from their effects, they could be legitimately compared to cases with sensible physical causes. Asserting the available evidence favored the existence of magnetic effluvia, Walker concluded that reasoning by analogy provided evidence "superior to any proof that can be brought, of ether being the cause of gravity, light, vision, etc."[47]

Using similar reasoning, George Adams defended the existence of invisible fluids including that of magnetism. Like Walker's, Adams' arguments applied generally to all substances known only indirectly. Heat, for example, an effect produced by an unseen fluid called "fire," escaped direct observation. As well, the motions and light created by the electrical fluid were "the only signs which give us notice of it's [sic] existence."[48] Asserting anology's vital role, Adams wrote, "it is *essential* in nature as soon as you consider physical objects, that to every *phenomena* [sic] there be a *cause*, and the only method of assigning a reasonable one, where they are not immediately discoverable, is *analogy*."[49] Like Walker, Adams argued that phenomena with hidden causes were analogous to those with observable causes. This, he noted, led naturally to the assumption of unobservable substances as physical causes. Known only indirectly through its effects, magnetism fit into Adams' categorization of substances understood by analogy.

Although Adams and Walker used similar terminology to explain magnetic phenomena, their speculations regarding terrestrial magnetism diverged considerably. Walker supposed that variations in earthly magnetism arose from heat weakening the attractive power of magnets and cold strengthening it. Thus, he argued that the frigid polar regions of the globe somehow were linked to the location of the magnetic poles. Walker also rejected Halley's four-pole theory because neither experimental nor analogical proof existed for it. Agreeing with Euler, he concluded that four poles gave no better account than two poles. Without elaborating, Walker appealed to the rotation of the earth or the action of subterranean fires to explain the winding lines of magnetic variation.[50] Such conjectures, he concluded, required additional observations to be confirmed or rejected. Though Walker generally endorsed the Gilbertian tradition that magnetism originated within the earth, Adams' speculations took quite a different direction.

Atmospheric Magnetism: An Alternative to Gilbert

Despite the widespread rejection of Halley's four-pole theory, eighteenth-century investigators reached little consensus on how terrestrial magnetic effects operated or why they altered over time. Did they emanate strictly from a giant magnet within the earth? Did they arise from the atmosphere? Was it perhaps a combination of earthly and atmospheric sources? These questions witnessed a variety of speculative answers. Adams and several others, for instance, challenged the notion that terrestrial magnetism originated within the earth. Theories relying upon atmospheric magnetism often conflicted directly with the Gilbertian tradition.

Notions that the earth's atmosphere, rather than its interior, caused magnetic effects stemmed from Cartesian theories and non-Cartesian theories as well. Using Franklinian electrical theory, which assumed the presence of electric atmospheres around positively electrified bodies, some investigators supposed the existence of analogous magnetic atmospheres surrounding all magnetized bodies, including the earth. Regarding terrestrial effects, neither Cartesian nor atmospheric alternatives required that the earth alone act as a large magnet. Hence, global magnetic phenomena might be strictly atmospheric, or a combination of atmospheric and terrestrial magnetism.

Several observations seemed to support the notion of atmospheric magnetism. In the context of researching various "airs," chemist Joseph Priestley briefly noted in 1779 that the "earth of iron" could be easily converted into air by chemical means. He remarked, "Should it be of this kind of earth that the bulk of atmospherical air in fact consists, it may perhaps help to account for the magnetism of the whole globe of the earth."[51] Shortly thereafter, the Reverend John Lyon proposed experiments to show that magnetic and electric atmospheres acted similarly. Blending the idea of magnetic atmospheres with the notion of circulating magnetic effluvia, Lyon's distinctions between these notions were not always clear.[52] In a different manner, Manchusian chemist John Dalton discussed atmospheric magnetism in *Meteorological observations and essays* (1793). Supposing that terrestrial magnetism resulted either from the united influence of natural magnets within the earth, or strictly from atmospheric influences, Dalton argued from observational evidence that magnetic beams high in the atmosphere governed the *aurora borealis*.[53] Despite the continuing dominance of circulating fluid theories, he cautioned that his "magnetic matter" was not the "magnetic effluvia" of most writers and cautioned, "My fluid of magnetic matter is . . . a substance possessed of the properties of magnetism, or, if these writers please, a substance capable of being acted upon by the magnetic *effluvia* and not the magnetic *effluvia* themselves."[54] Insisting on the hypothetical status of the effluvia, Dalton concluded that their existence had not yet been proven. Nonetheless, his work loaned support to the idea of magnetism in the earth's atmosphere.

Explicitly rejecting assumptions of the earth's magnetism, George Adams defended the existence of atmospheric magnetic fluids. Like many others, he rejected Halley's four-pole theory. Describing it as "laboured and unnatural," Adams explained that Halley's theory predicted regularities which conflicted with the actual observations. Though he did not specify what they were, Adams judged Euler's two-pole theory to have various imperfections as well.[55] Although resigning himself to speculation, he considered some conjectures more plausible than others. Of the many hypotheses put forth, he endorsed that of Swiss natural philosopher Pierre Prevost as undoubtedly the best.[56] As we shall see shortly, Adams' overarching speculations fit within the system building tradition. Therefore, while ignoring mathematics, Adams combined the experimental and natural philosophical traditions in his work.

Pierre Prevost (1751-1839) based his theory upon the mechanical system of his countryman and teacher, George-Louis Le Sage (1724-1803). Like many Continental natural philosophers, Le Sage explained gravitation without resorting to forces acting at a distance. In Cartesian fashion, he found it inconceivable that lumps of matter divined the presence of nearby matter and attracted it across empty space. Influenced by the ancient atomist Lucretius, Le Sage proposed the existence of "otherworldly particles" which were exempt from the law of gravity. Moving at very high velocities in all directions, these tiny atoms made up a mechanical gravitational fluid which explained gravity by movement and impulsion.[57] Starting with Le Sage's mechanical system, Prevost developed his theory of magnetism in *De l'origine des forces magnêtiques* (1788).[58]

Such a broad-ranging mechanical system appealed to Adams. In *Lectures on natural and experimental philosophy* (1794), he approvingly described the basic tenets of Prevost's magnetic theory. Departing from circulating effluvia, Prevost had offered a theory of magnetism relying on a very subtile, expansive fluid in and about the earth whose particles formed from a combination of two elements united by affinity. The different elements of this fluid, named A and B, combined with one other in groupings or "molecules" (i.e., AB or BA) more readily than they united with those of the same type (i.e., AA or BB). Hence, the theory relied on differing combinations of attractions to explain all magnetic phenomena, including repulsion.

However, in contrast to action at a distance theories, Prevost resorted to mechanical impulsion as the ultimate cause of magnetism. The fluids had an affinity for iron particles which acted, Adams explained, "only at contact, or when very nearly in contact."[59] In the presence of iron particles, the magnetic fluid decomposed into its two types. Using the consequent mechanical interactions of iron particles and the elements of fluid, Prevost qualitatively explained various phenomena.[60] In addition, he accounted for global magnetism by assuming each type of fluid had greater abundance in the northern and southern hemispheres. Modifying Prevost's account, Adams proposed that the fluid distributed itself solely in the atmosphere. Hence, earthly magnetism was, for Adams, a distinctly atmospheric phenomena.

Others put forth notions rejecting the Gilbertian analogy between magnet and earth. Differing from Prevost's mechanistic conjectures, English physician Edward Peart explained magnetic attractions and repulsions in *On the elementary principles of nature, and the simple laws by which they are governed* (1789).[61] Peart embraced active principles of attraction and repulsion rather than contact action or mechanical impulsion of particles. He described the magnetic fluid as "an atmosphere of active particles, surrounding the excited pole, and attracting, or drawing towards it" any particles of iron within a certain distance.[62] Because like poles repelled and unlike poles attracted, Peart reasoned that "the atmosphere surrounding the *north* pole, must be a fluid, *different* from that, inveloping [sic] the *south* pole, though similar in its attraction to iron."[63] Hence, the magnetic matter consisted of two distinct fluids. Peart explained that opposite magnetic poles "will penetrate each other, unite, and destroying each other's regular arrangement, will form *lines* of attracting particles, *drawing* their respective poles into *contact*."[64] Although different from Adams' theory, Peart supported atmospheric magnetism and rejected the standard Gilbertian view.

A Treatise on the Magnet (1798) by Jamaican sugar-planter Ralph Walker also supported the notion of atmospheric magnetism.[65] Supposing a fluid element pervaded the globe, perhaps the entire universe, Walker asserted that the existence of "a magnetic fluid in our atmosphere . . . can hardly be doubted."[66] Appealing to atmospheric magnetism, Walker explained that one of the "sorts of it [magnetism] is attracted by the northern hemisphere, and the other by the southern hemisphere." Also using terms such as "magnetic effluvia" and "magnetic vortices," he presented a mixture of Cartesian and Franklinian terms. Like many others, Walker admitted that no theory had been firmly established by observation and experiment. Such ideas, neither empirical nor mathematical, remained within a British speculative tradition.

The article "Magnetism" in the third edition of the *Encyclopaedia Britannica*, penned by Edinburgh-educated writer James Tytler (1747-1805), also reflected the speculative aspects of discussing magnetic phenomena. Tytler remarked that magnetism, like electricity, depended "on a cause so little subject to the investigation of our senses, that any regular and well-supported theory can as yet scarcely be expected."[67] In fact, magnetism remained even more problematic than electricity because no experiments rendered its fluid visible. Experiments made the effects of magnetism perceptible, but never its underlying cause. Despite this skeptical stance, Tytler willingly espoused a notion of terrestrial magnetism rejecting the Gilbertian tradition.

Regarding earthly magnetism, Tytler harshly criticized the notion of a giant terrestrial magnet and strongly favored its atmospheric origins.[68] One difficulty was that no experiment had demonstrated iron more powerfully attracted to the earth near the poles than at the equator, an effect seemingly predicted, he noted, within the Gilbertian framework. Other observations, noted Tytler, also discredited the idea of a magnetic earth. For instance, any of several theories including Halley's should have exhibited regularity with respect to terrestrial magnetic variation. Worldwide irregularities in the variation, Tytler claimed, overturned all such theories. Concluding his critique, he wrote:

> The poles of all the magnets, we know, are fixed and invariable; nor are we obliged to have recourse to magnets within magnets, or other uncouth suppositions, to account for their phenomena: *if the earth is a magnet, therefore, the magnetism it possesses must be of a kind so different from the property usually distinguished by that name, that we can in no respect determine them to be the same.*[69]

By rejecting the Gilbertian analogy between terrella and earth, Tytler abandoned the widely-held notion that terrestrial magnetism and ordinary magnetism originated from identical causes.

Like Adams and others, Tytler favored atmospheric explanations for terrestrial magnetism. Differing from Adams, however, he postulated a fluid flowing through the earth's interior. What was this mysterious fluid and how did it function? In the second edition of *Britannica* (1778-1783), Tytler explained:

> It is certain indeed, that both natural and artificial electricity will give polarity to needles, and even reverse their poles; but from this it may appear probable that the electric fluid is also the cause of magnetism, yet in what manner the fluid acts while producing the magnetical phenomena seems to be totally unknown.[70]

Continuing this line of thought in the third edition, he supposed the earth was "surrounded by a fluid whose motion" produced the magnetism of iron.[71] As the equatorial regions absorbed solar rays, these rays became subject to new laws of motion and acted as the electric fluid. This fluid, Tytler asserted, passed through the earth from the equator, emerged near the poles, rose high into the atmosphere, and finally, returned to the equator. Tytler was not alone in proposing such ideas, similar theories of streaming electric matter as the source of terrestrial magnetism had been put forth by the Italian experimental philosopher Giambatista Beccaria and French savant Comte de Buffon in the 1770s and 1780s.[72]

Continuing his argument, Tytler asserted that various natural phenomena supported the notion of a flowing electric fluid. Because the direction of earthly currents became increasingly perpendicular to the earth in high latitudes and nearly horizontal to the earth at the equator, the notion of a circulating fluid conformed with observations of increased magnetic dip at higher latitudes and diminished dip in equatorial regions. Hence the dipping needle behaved as if guided by currents of electric fluid. Although deeming his conjecture the closest approach to discovering the true causes of magnetic phenomena, Tytler admitted an inability to explain why the fluid influenced iron more than other metals or "why the direction of a current of electric matter . . . should cause such strong attractions as magnetical bodies are sometimes endowed with."[73] Resolving these problems, he believed, required additional experimental and observational data.

In the natural philosophical tradition, the role of Tytler's electric fluid was all encompassing, the ultimate source of all attractive and repulsive forces. His article, "Electricity" in the third edition of the *Britannica* (1788-1797) reiterated the claim that magnetic power depended "upon the secret operation of the electric fluid."[74] He agreed with the Comte de Tressan's *Essai sur le fluide electrique, considere comme agent universel* (1786) claim that all magnetic effects could be attributed to electricity —"the spirit and driving force of all magnetic phenomena."[75] Indeed, Tytler considered the electric fluid as the first principle of motion in the universe. Its actions, he supposed, guided the planets while giving stability and cohesion to the earth, terrestrial substances, and all bodies in the universe.[76] Echoing views nearly identical to Tytler's, several encyclopedia articles nearly twenty years later rejected the Gilbertian analogy as well. One, in fact, commented that many still believed the earth "neither *is*, nor *contains*, a magnet, but is surrounded by a fluid, whose motion is productive of magnetism in iron."[77]

Using arguments akin to Tytler's, Irish natural philosopher and bishop Matthew Young (1750-1800) argued against the Gilbertian view in *An Analysis of the Principles of Natural Philosophy* (1800). Like Tytler, he had difficulty in accepting earthly magnetism because iron did not weigh more near the poles than the equator. Discrepancies, Young noted, should be expected if proximity to the magnetic poles effected the strength of attraction. Similarly, a magnetic needle floating on water in a container should, but did not, move toward the north side of the vessel. Young also contended that extremely irregular observations of magnetic declination did not agree with the assumption of a giant terrestrial magnet. With this, he dismissed Halley's kernel-and-shell theory. Rejecting John Canton's notion that solar heat caused magnetic variations, Young noted that the fairly constant temperature in caves of even moderate depth contrasted with the great depth of the internal magnet supposedly heated by the sun.[78] Finally, because phenomena such as lightning and the aurora borealis affected the magnetic needle, he speculated that all global magnetic manifestations most likely stemmed from atmospheric causes.[79]

Adding to the speculative diversity, Erasmus Darwin's *The Temple of Nature* (1803) appealed to atmospheric magnetism, yet explained magnetism in a mechanical fashion different from Prevost's theory. Rejecting action-at-a-distance, Darwin remarked "nothing can act, where it does not exist, all distant attraction of the particles of bodies, as well as general gravitation, must be ascribed to some still finer ethereal fluid."[80] With this remark, he utilized several ethers in his speculative theories of electricity and magnetism. Appealing to the analogy between electricity and magnetism, Darwin wrote:

> Magnetism coincides with electricity in so many important points, that the existence of two magnetic ethers, as well as of two electric ones, becomes highly

> probable. We shall suppose, that in a common bar of iron or steel the two magnetic ethers exit intermixed or in their neutral state.[81]

The two magnetic ethers he named "arctic" and "antarctic." However, it was not these ethers which acted on particles of iron, but two additional ethers accompanying them. These secondary ethers, which Darwin called "masculine" and "feminine," formed atmospheres around magnetic objects including the earth.[82] In addition, these ethers accompanied electrical and gravitational effects. Such an all-encompassing view of combining ethers explaining different phenomena, appealed neither to the experimental nor the mathematical tradition.

Directly and indirectly, the speculations of Prevost, Adams, Peart, Tytler, Darwin, and others illustrate several themes of the late eighteenth century including: a departure from Cartesian circulating fluids, a challenge to the Gilbertian tradition, and a continuing lack of consensus regarding the causes of magnetism and terrestrial magnetism. In a broader sense they demonstrated the persistent bewilderment regarding the cause of attractions in general. Did attractions act at a distance or were they somehow mechanical? Some speculations such as Tytler's electric fluid or Darwin's secondary ethers attempted to link magnetism with other phenomena. Not the first attempts to unite nature's forces, conjectures of this kind frequently included magnetism. In this speculative vein, the study of magnetism kept its feet in both the experimental and natural philosophical traditions. So too it remained outside the domain of mathematical physics.

The Natural Philosophical Tradition: Nature's Forces Unified?

Long before the 1780s, investigators suggested connections between seemingly disparate natural phenomena. If nothing else, the principle of parsimony suggested that nature should operate in the simplest possible manner. In the natural philosophical tradition, Isaac Newton himself proposed in the second edition of the *Principia* (1713) a pervasive "subtle spirit" in all bodies responsible for numerous optical phenomena, as well as electrical attractions and repulsions. Several years later, in a new edition of the *Opticks* (1717), Newton reworked his ideas suggesting an all-pervasive "Aetherial Medium." This elastic, subtle ether filled not only ordinary, gross matter, but all of space as well. Newton suggested that the changing density of this ether was responsible for gravity, optical phenomena, and certain functions of the nervous system.[83]

Newton's speculations, however, did not imply the impulsive action of a mechanical ether. As he explained in the second edition of the *Principia* (1713), gravity operated "not according to the quantity of the surfaces of the particles upon which it acts (as mechanical causes are accustomed)."[84] Newton further noted in the 31st Query appended to the *Opticks* (1730): "Seeing therefore the variety of Motion which we find in the World is always decreasing, there is a necessity of conserving and recruiting it by active Principles, such as are the cause of Gravity"[85] Hence, Newton's ether embodied forces inexplicable in mechanical terms; it was part of his wider stress on immaterial "active principles," and ultimately evidence for the continual action of God. Newton regarded these principles as the causes of all natural forces, fundamental to the operations of nature. The ether was one of these active principles.[86]

In the hands of many eighteenth-century natural philosophers, particularly after the 1740s, Newton's suggestions took on a variety of manifestations and modifications. Seeking an overarching, unified view of nature, these speculations fell squarely within the natural philosophical tradition. As such, they infrequently appealed to experiment, and, even less often, to mathematics. Some natural philosophers supposed modifications in the density of an underlying ether, some appealed to a universal ether mixed with other substances, and some sought a few active principles at work. For example, in 1748, Gowin Knight published *An attempt to demonstrate, That all the Phœnomena in Nature may be explained by Two Simple Active Principles,* *Attraction and Repulsion*; a work whose title alone made clear Knight's intent. Connecting cohesive, gravitational, and magnetic attractions, he claimed that they were "one and the same."[87] Agreeing with Newton, Knight supposed that active principles produced and continued all the motions in the universe.

English physiologist David Hartley reached similar conclusions in *Observations on Man* (1749). Advocating Newton's universal ether, he noted:

> The Emission of odoriferous Particles, Light, magnetical and electrical Effluvia, may also be some Presumption in favour of the Existence of the Æther. Moreover, it is reasonable to expect, that it [i.e., the aether] should have a repulsive Force in respect of the Bodies which emit it; and for the same Reasons, its Particles may repel each other.[88]

After explaining the ether's additional properties including elasticity, compressibility, and vibratory motion, Hartley asserted that it explained a great variety of phenomena. Hoping to link these, he concluded:

> Each Part, Faculty, Principle, &c. when considered and pursued sufficiently, seems to extend itself into the Boundaries of the others, and, as it were, to inclose and comprehend them all. Thus Magnetism mixes itself with the Gravitation both of Bodies upon the Surface of the Earth, and with that of the Moon to the Earth: A polar Virtue of the same kind seems to have a principal Share in the Formation of Natural Bodies . . . Electricity may also extend . . . to small Distances, and join with the just mentioned polar Virtue, in making the Parts of Bodies cohere . . .[89]

Hence, Hartley linked magnetism, gravity, cohesion, and electricity, in addition to optical, chemical and sensory phenomena using his hypothetical ether.

Conjectures similar to Knight's and Hartley's continued throughout the eighteenth century, often with scant agreement in their specifics and little experimental evidence to back them up.[90] In 1771, London instrument maker and lecturer, Benjamin Martin, supposed that all attractions consisted of "a fine imperceptible particles or invisible effluvia, which proceed from every point in the surface of the attracting body."[91] Six years later, English linen draper and experimenter William Henly asked the Royal Society of London, "Upon the whole, is there not an high degree of probability in the supposition that light, fire, phlogiston, and electricity, are only different modifications of one and the same principle?"[92] Accordingly, he proposed that phlogiston was the quiescent state of this principle, the electric fluid its first active state, and fire its final state of violent agitation. John Lyon wrote in 1780, "we may reasonably conclude . . . the effluvia of both [electricity and magnetism] act from one principle."[93] Claiming attractive and repulsive properties in both electric and magnetic effluvia, Lyon's theory relied on streams of polar effluvia. He asserted the existence of a certain fluid within all substances and on the surface of all bodies termed variously: ether, phlogiston, inflammable substance, electric fluid, and elementary fire.[94]

As investigators put forth conjectures linking natural phenomena, there were also a variety of views with respect to the general causes of attraction and repulsion. In 1784, Irishman Felix O'Gallagher conflated Newton's ether, Dutch chemist Hermann Boerhaave's "elementary fire", and what he called the "elastic matter." Defining this matter, he wrote:

> Its essence consists in a double power of expansion and convergence; which it derives from the distinct essences of its components . . . One of its principles is the material cause of cohesion, the other the source of expansion and fluidity; and the exertions of both, when excited to action, produce the phaenomena of elasticity and tremour. This compound substance is the basis of all lively material powers and qualities.[95]

O'Gallagher noted at least four existing conceptions regarding the cause of attraction: 1) an imperceptible effluvium emitted from bodies; 2) an impelling force of some medium tending toward the sun, planets, etc.; 3) a law originally implanted upon all matter by God; 4) the immediate intervention of God. Of these various views, he conceded, "so great is the obscurity of [attraction's] physical principles, that it has not all this time been determined, whether the moving forces arise from an attracting effluvium or an impelling medium, whether they be mechanical or preternatural, material or spiritual."[96]

Despite O'Gallagher's cautious indecisiveness, investigators continued speculating about the ultimate causes of attraction and repulsion. From 1750 onward, increasing numbers considered active principles an inherent property of all matter. This view departed from Newton's notion of inert matter with all active principles originating from God. Not surprisingly, many general discussions of attraction and repulsion including magnetic phenomena as well.

Conjectures about magnetism often accompanied a unified view of nature put forth within the natural philosophical tradition. For instance, in 1789, country physician Edward Peart reduced magnetic effects to the actions of what he called the "earthy" and "acidifying" principles. The relations between a particle of each principle so strongly resembled the relations between "the two contrary poles of a magnet . . . that there can be no doubt, that both depend on the *same causes*, and that *those causes*, are the *two active principles*, *aether* and *phlogiston*."[97] According to Peart, all observed phenomena reduced to the actions of these underlying principles. Assuming that "aetherial" and "phlogistic" atmospheres evoked various effects depending on their level of excitement, he explained:

> in the *simpler* state of excitement, producing the *attraction* of *Gravity*: and when *more strongly* and peculiarly excited by the attraction of iron, producing the *attraction* of *magnetism*; it next follows, to consider the *third state* of their excitement . . . producing the *third* kind of *attraction*, that of *electricity*.[98]

Similar speculations continued in the early nineteenth century with little experimental evidence to support them. In 1802, T. Gale asserted that the "phenomena of gravitation and motion are founded in, and performed by the various states, effects and operations of [an] ethereal element, called fire, when expanded."[99]

Although linking phenomena in a variety of ways, such efforts remained unsubstantiated by systematic observation or experiment.[100] Not surprisingly, these speculations were not embraced with uniform enthusiasm. One critic, John Read, included Peart's attempt among numerous failures to identify the ether with electricity.[101] Dissatisfied with past magnetic hypotheses, George Adams remarked in 1784 that none had yet established "the links of the chain which connect [magnetism] with the other phenomena of the universe."[102] A decade later, however, he postulated that because the aurora borealis, zodiacal light, electricity and heat all affected the magnetic needle, there were grounds "for supposing that one or other of the elements of the magnetic fluid is furnished by the *solar rays*."[103] In 1799, Adam Walker similarly maintained that electricity, light and fire all originated from the sun.[104] Several years later, he supposed attraction and repulsion to be the "great acting principles in the universe." Walker considered light, fire, electricity, and phlogiston as modifications of the principle of repulsion.[105] Neither Adams nor Walker, however, developed their speculations in detail. Neither supported their ideas with experiment or mathematics. In this regard, such conjectures remained beyond the experimental and mathematical traditions.

The concept of active principles espoused by many investigators differed from Newton's conception in the *Opticks* (1717). In part, these differences arose from Newton's ambiguous discussion of the ether, but the differences also arose from the shifting place of divine action in scientific theories.[106] Scottish geologist James Hutton believed in nature's inherent active principles; gravitational matter acted by the principle of attraction, while matter emanating from the sun acted by the repulsive principle. Hutton wrote in 1794 that light, heat, and electricity appeared to be three different modifications of the same solar matter.[107] Two years later, Stephen Dickson, professor of medicine at Trinity College, Dublin, included fire, light, phlogiston, gravity, electricity and magnetism in his list of chemical principles. These principles, he remarked, "are causes of which the nature is not determined, and even the existence not known, except by inference that number of similar phenomena manifest in bodies subjected to our senses must be owing to the operation of an homogeneous cause."[108] Such unity could be attributed to the "addition or avolation of a peculiar substance, or in an alteration of the affinities or modes of motion of identical particles," yet this, Dickson concluded, could not yet be detemined. In both Hutton's and Dickson's cases they did not suppose as had Newton that God directly intervened to place active principles in nature.

Many put forth more limited conjectures, connecting only several rather than all phenomena. There were those, for instance, who linked magnetism to light, magnetism to heat, or magnetism to electricity. In 1809, Captain John Hamstead wrote, "There is such an apparent coincidence in many points between some of the properties of the magnetic fluid, the electric fluid, and fire, that we are led to infer they are only one and the same fundamental element."[109] The following year, Swiss-born geologist Jean André Deluc wrote, "it cannot be doubted, that *light* has, in various ways, a great share in the formation of *atmospheric fluids*, and thus probably of the *magnetic* [fluid]."[110] John Bywater suggested in 1813 that magnetic fluid and caloric were the same agent working under different circumstances.[111] Four years later, Barrister at Law, Charles Carpenter Bompass vaguely proposed that two ethereal fluids combined in different proportions to account for light, heat, electricity, magnetism, and chemical effects. Though several observations seemed to support such notions, no investigator, man of science or otherwise, bolstered speculations with solid experimental evidence or consistent theoretical foundations.[112]

More wary investigators hoped for the eventual discovery of general principles at some future date. John Murray, lecturer on Chemistry and Materia Medica at Edinburgh, noted in 1806 the suspected connections between light and heat, electricity and galvanism. Nonetheless rejecting the identity of light and caloric, he concluded "we may in the present state of knowledge, consider them as essentially distinct."[113] As we shall see in the next chapter, the Scottish natural philosophers John Robison and John Playfair tentatively suggested unifying principles in nature, yet ultimately favored explanations appealing to distinct imponderable fluids.

British investigators came into contact with numerous Continental speculations as well. For instance, in 1807 the *Philosophical Magazine* reported Pierre-Hyacinthe Azaïs' theory of electricity, galvanism and magnetism. As a candidate for a scientific prize offered by Napoleon, Azaïs elaborated a system of two electrical fluids which "formed, combined, and renewed incessantly" in the terrestrial atmosphere. Similar to Tytler's theory, these fluids proceeded "continually from the equator towards the poles, and from the poles towards the equator," producing by their movements and combinations "all the phenomena of electricity, galvanism, and magnetism."[114] Azaïs further asserted that the universe was "directed and tied together by one sole cause."[115] As we shall discuss in chapter six, such conjectures of nature's unity persisted in Britain and gained increasing popularity after 1820 when they were supported by experimental evidence.

A Plurality of Views: Continued Confusion

Given the numerous speculations on magnetism's links to other phenomena and the plurality of views surrounding studies of attraction and repulsion in general, it is not surprising that some remained indecisive about which magnetic hypothesis, if any, they supported. Did magnetic forces act at a distance or arise from the impulsion of circulating effluvia? Did the earth act as a giant magnet or was the atmosphere the primary generator of magnetic phenomena? Were there many principles in nature or a few underlying principles? How could magnetic phenomena be understood while their causes remained hidden? The attempt to address these questions affected the discussion of magnetism specifically, and attraction generally.

For example, Margaret Bryan's *Lectures on Natural Philosophy* (1806) remained unclear on whether two magnetic elements existed, or a single effluvium. Taking an ambivalent stance, she left it for her readers to decide. Bryan also claimed that the directive power of magnets resulted from the earth and its atmosphere.

Irregular changes in declination followed from the unequal diffusion of magnetic power between the earth and its atmosphere, and the effects of heat and cold. She concluded that these irregularities might preclude any perfect knowledge of the hidden causes of magnetism.[116]

In 1813, John Bywater remarked on the multitude of "wild and extravagant opinions" and "fanciful conjectures" put forth to account for magnetism.[117] Strongly objecting to notions of a great magnet and atmospheric magnetism, Bywater noted that the simple idea of the earth or its atmosphere causing magnetism failed to account for all the complex changes. The complicated systems of magnetic nucleus and nested shells, such as Halley's, were too hypothetical. Even imponderable fluid theories left him dissatisfied. In fact, so strange were magnetic effects and so incomprehensible the different theories accounting for them that Bywater concluded a secret magnetic principle lay unknown and unobserved.[118]

Whatever their doubts, however, Bryan and Bywater returned to descriptions relying on a mechanically-acting subtle magnetic matter. Bryan wrote of a magnetic "subtile effluvia" disseminated through the earth and atmosphere. Bywater similarly proposed that magnetic attraction depended on the external pressure of a "universal medium." Hence, all attractions originated from "the action of some highly elastic agent pressuring the particles of bodies."[119] Therefore, magnetic effects originated from the actions of some type of magnetic matter residing in both the earth and its atmosphere.

In contrast to Bryan's and Bywater's appeals to magnetic effluvia, other investigators rejected all hypotheses. They insisted that the causes of magnetism remained totally unknown; the existing theories simply did not rely enough on observation and experiment to be tenable. Rejecting magnetic effluvia, the Edinburgh-educated George Gregory conceded in 1796 that mankind remained "perfectly ignorant" of the causes of all magnetic phenomena. He disapproved of human imagination creating "invisible agents in order for the fabrication of plausible theories."[120] Similarly skeptical, William Nicholson remarked on the causes of magnetism in 1809:

> we know not of any hypothesis which strikes conviction in our minds, or which seems to convey any adequate idea of the origin, or *modus operandi*, of this wondrous influence. All we can treat of is the effect; also of the appearances which guide our practice, and of the manner in which the attractive power may be generated and increased.[121]

As discussed in the next section, however, not everyone embraced the circulating effluvia of Bywater and Bryan, the atmospheric magnetism of Tytler or Darwin, or the skeptical attitude of Gregory and Nicholson.

Franz Aepinus (1724-1802): Experiment and Mathematics United

In fact, by the end of the eighteenth century, new theories increasingly challenged the existing notions. Avoiding unifying ethers, these theories argued that magnetism and electricity resulted from the attractions and repulsions of distinct imponderable fluids. Such hypothetical fluids played a much different role than did the mechanistic effluvia of Cartesian theories. Developed by German-born natural philosopher Franz Ulrich Theodor Aepinus in the 1750s and 1760s, one influential theory also brought a new mathematical approach to the empirical understanding of electricity and magnetism. Aepinus' theory combined the mathematical and experimental traditions in a manner which most other eighteenth-century investigators had not. By century's end, the theory of Aepinus challenged the long-dominant effluvial theories in France and Britain. In doing so, it also questioned the existing notions regarding terrestrial magnetism.

Rejecting circulating effluvia, atmospheric magnetism, and unifying ethers, Aepinus introduced a degree of mathematical rigor which had not existed in earlier magnetic theories. Developed in the early 1760s, Aepinus' theories initially gained little attention or acceptance. As historian R. W. Home has argued, on one hand, such theories contained too much mathematics for experimental physicists, and even those not hostile toward mathematics found Aepinus' work incomprehensible, while on the other, those working in the mathematical tradition found Aepinus' mathematics too elementary to attract their interest.[122] However, by the 1780s Aepinian gained increased acceptance in France where experimental and mathematical traditions were coming closer together. By the 1790s, the theory increasingly influenced British studies as well.[123] Nonetheless, the initial reception of Aepinus' theory in Britain remained for the most part negative. In addition to the theory's mathematical aspects it included: 1) the use of the analogy between electricity and magnetism, and 2) forces of attraction and repulsion acting at a distance. These elements made Aepinus' theory quite different from its circulating fluid competitors.

Although similarities between electricity and magnetism had often been recognized, sharper dissimilarities took precedence in earlier work. Gilbert, for instance, had emphasized the distinct causes of electricity and magnetism; so had Pieter van Musschenbroek in the early eighteenth century. For instance, it was frequently pointed out that only iron could be magnetized, while all substances could be electrified. In addition, intervening matter such as glass, wood, or water did not interrupt magnetic effects as it did electrical phenomena. Despite acknowledging these and other differences, writers often presented electricity and magnetism in tandem because both phenomena involved analogous attractions and repulsions of separated bodies.

During the eighteenth century empirical connections between electricity and magnetism were frequently noted. When struck by lightning, ships' compasses often had their magnetism reversed or destroyed. Iron needles could also be strongly magnetized by artificial electric shocks.[124] The aurora borealis, believed by many to be electrical in origin, reportedly agitated the movements of magnetic needles as did electrified glass. However, of greatest importance to the development of Aepinus' theory, the crystalline mineral tourmaline exhibited many electrical phenomena analogous to those of a magnet.

Stimulated by the tourmaline-magnet analogy and Benjamin Franklin's one-fluid theory of electricity, Aepinus developed new theories of electricity and magnetism.[125]

Although others recognized the tourmaline's unusual behavior, Aepinus was the first to emphasize the analogy between it and the magnet.[126] Akin to magnetic poles, a heated piece of tourmaline exhibited electric polarities, plus and minus, in its opposing surfaces. Furthermore, when cut or fractured each new fragment of tourmaline had polarity analogous to that of a cut magnet. In the introduction to his major work on the subject, *Tentamen theoriae electricitatis et magnetismi* (1759), Aepinus remarked, "I was struck at the time . . . by the utmost similarity between this stone and a magnet. This is so obvious that I have no doubt that anyone who has read what I then wrote about the Tourmaline has thought of it without prompting."[127]

Appealing to the striking parallels between magnet and tourmaline, Aepinus developed a theory of electricity ridding Franklinian theory of electrical atmospheres. Though transferring his theory to the closely analogous magnetic phenomena, Aepinus insisted on the strict separation of electricity and magnetism. He wrote, "I in no way consider the magnetic and electric fluids as one and the same thing, as do those who toil to derive the phenomena of both electricity and magnetism, and many other things, from one single extremely subtle fluid, namely the aether."[128] Thereby, Aepinus' analogy rejected the unifying schemes prevalent in the British natural philosophical tradition.

Not surprisingly, Aepinus also rejected circulating magnetic effluvia and ambient atmospheres as mechanical explanations of terrestrial magnetism. For him, the earth's magnetism remained a mysterious phenomenon "for which we can recognize no efficient mechanical cause, and which must be derived from the immediate action of the creator of the world."[129] Though noncommittal regarding specific explanations for magnetic declination and inclination, Aepinus readily accepted the Gilbertian notion of a terrestrial magnetic core. Such a core, he supposed, underwent slow, continuous modifications, yet he dared not say whether variations of the core's shape, the differing distribution of magnetic matter in each hemisphere, or changing positions of the entire core caused these alterations. Too few observations had been collected to decide the matter. Whatever the cause, the directive force of compass needles and other magnets depended on "a magnetic force inherent in the core of the terrestrial globe."[130]

Aepinus reduced magnetic phenomena to the net effect of attractive and repulsive forces between static amounts of a hypothetical, extremely subtle magnetic fluid and ordinary matter. There were no atmospheres surrounding magnetized objects, nor did effluvia constantly circulate around and through all magnets. Therefore, movements of magnetic needles, patterns of iron filings, and all other magnetic phenomena reduced to unexplained forces acting at a distance. The magnetic fluid exerting these forces remained confined within ferruginous bodies. Within a piece of non-magnetic iron this fluid diffused evenly to reach an equilibrium state. This happened due to the fluid's self-repulsive nature and its attraction to iron particles. When a nearby magnet disturbed the equilibrium or "natural state", the iron became magnetic— one end overcharged with fluid, the other undercharged. With this scheme, Aepinus skillfully explained many magnetic phenomena in a mathematical and semi-quantitative fashion.

Analogy and Experiment: Aepinus' theory in Britain

As we have seen, the use of analogy and experiment played key roles in development of Aepinian theory. In fact, analogies between electricity, magnetism, and other phenomena were frequently discussed in the 1760s and 1770s. It appeared, however, that few in Britain had any intimate knowledge of Aepinus' work. The same year that the *Tentamen* appeared, a paper was published in the *Philosophical Transactions* by English electrician Benjamin Wilson (1721-1788) on experiments related to the tourmaline. Wilson mentioned Aepinus several times, yet seemed unaware of the differences between Aepinus' theory and Franklin's electrical theory:

> I have wondered that *Æpinus* did not take notice of some of the experiments which electrify glass either *plus* or *minus*, because the *Tourmalin* affordeth leading experiments towards it; and can ascribe it to no other cause than a favourable opinion he was willing to entertain of *Dr. Franklin's* hypothesis.[131]

In 1767, Joseph Priestley, drew upon an abridgment of Aepinus' *Essay* written by close friend Richard Price.[132] Summarizing the analogy, Priestley carefully enumerated nine similarities between electricity and magnetism, yet failed to directly discuss Aepinus' theory. He did not explain the connection between Aepinus' magnetic theory and Franklinian electric theory. Though mentioning Aepinus, Priestley, like Martin, gave little idea of the complete theory and its mathematical aspects.

Despite frequent comparisons of electricity and magnetism, investigators often reached conclusions distinct from Aepinus', presenting analogies to support their own speculations. In 1777, William Henly knew of Aepinus' analogy but only through reading Price's abridgment.[133] Using the tourmaline-magnet analogy, John Lyon remarked in 1780:

> the phaenomena of the tourmalin and the loadstone are in every respect the same. The pieces of each retain the same attractive and repulsive properties, as they did when united; and as the particles of the magnetic effluvia are allowed to have a polar virtue, I can see no reason why the electric particles many not possess the same properties.[134]

Appealing to Newton's second rule of philosophizing, he concluded that two different causes could not be assigned "where effects so nearly correspond."[135] Evidently unaware of Aepinus' arguments, Lyon asserted that magnetic and electric particles shared the same underlying cause.

Earlier in the chapter, we saw how Adams and Walker argued by analogy in favor of the existence of circulating magnetic effluvia. Appealing to the general usefulness of analogical reasoning in 1796, Irish chemist and mineralogist Richard Kirwan reached quite different conclusions. He explained to the Royal Irish Academy two methods for explaining natural phenomena. The first involved "discovering the conditions and circumstances" of the phenomena's production and the laws which governed them. The second method, however, "by far the most perfect and satisfactory," involved showing an "analogy, similarity or coincidence with some general fact with whose laws and existence we are already acquainted."[136] Kirwan claimed that electricity and magnetism had been partially explained with the first method, but not the second. Using analogical reasoning he compared magnetism with crystallization. Differences between these phenomena, Kirwan concluded, indicated more a "variety of degrees, in the same power, than any essential difference in the powers themselves."[137] Hence, magnetism was quite similar to the process of crystallization. Unconcerned with (or ignorant of) Aepinus' work, many investigators sought basic underlying principles possibly linking magnetism with other phenomena.

Others in the late eighteenth century knew of Aepinus' work, yet explicitly disagreed with him for various reasons. Some experimenters did not like or understand his methods, particularly his use of mathematics in a strictly experimental subject. For instance, Benjamin Wilson wrote to Aepinus:

> The introducing of algebra in experimental philosophy, is very much laid aside with us, as few people understand it; and those, who do, rather cho[o]se to avoid that close kind of attention; tho' I make no doubt but I dar[e] say you had a very good reason for making use of that method.[138]

With regard to electrical theory, George Adams also opposed the inclusion of mathematics, complaining in 1794 that Aepinus had "closed the door on all our researches into the nature and operations of this [electric] fluid."[139] Though he briefly described the one-fluid theory, Adams favored Euler's effluvial theory over Aepinus' mathematical approach. On similar grounds, Irishman George Miller rejected the electrical theory of Aepinus, calling it a "very elaborate scheme of mathematical reasoning." Miller, like many others, disapproved of Aepinus' assumption that particles of ordinary matter repelled one another when devoid of the electric fluid. He voiced the same objection with respect to the one-fluid magnetic theory. In the experimental tradition, Miller concluded that Aepinus' "system must therefore be considered, not as a physical solution agreeable to the known laws of natural operations, but merely as an ingenious exercise of mathematical ability."[140]

In addition to rejecting Aepinus' use of mathematics, confusion endured with respect to the differences between various magnetic theories. Though Prevost's theory differed distinctly from Aepinus', he applauded Aepinus in 1788, proclaiming that he had produced "in the theory of magnetism a revolution that can be compared to the one that Newton brought about in general physics."[141] Despite such high praise, Prevost favored a theory very different from Aepinus', noting that "in order to explain the phenomena, it is obligated to admit a fluid as generally diffused outside of the iron as mine . . . Whatever the cause of magnetism . . . one must suppose that some subtle fluid, universally diffused, intervenes in the phenomena."[142] Prevost's mechanically-acting atmospheres diverged greatly from Aepinus' internal fluid acting at a distance.

George Adams endorsed Prevost's atmospheric theory, yet rejected Aepinus' theory. Exacerbating an already confused situation, Adams also approved of Euler's theory. In doing so, he conflated Euler's circulating effluvia with Prevost's magnetic atmospheres. In the end, Adams supported theories which had a mechanical basis and which were non-mathematical. He and other experimentalists could not accept the new mathematically oriented theory of Aepinus which accepted forces acting at a distance. Among late eighteenth-century studies of magnetism, Adams' example illustrated not only the continuation of the experimental tradition, but also the widespread confusion and plurality of views regarding magnetism's causes.

During the 1790s, many continued to ignore or reject Aepinus' theories. For instance, Tytler's 1797 *Britannica* article, "Magnetism," assessed Aepinus' theory in a negative light. Tytler remarked that recent discoveries in electricity suggested that magnetic phenomena were caused by a fluid analogous to the electric, probably the same fluid.[143] While impressed by the "remarkable particulars" in which magnetism and electricity agreed, Tytler also found their differences remarkable. In contrast to electricity, magnetic power was permanent and did not affect the senses. Electricity resided on the surface, while magnetic virtue pervaded the entire substance. Finally, magnetic power did not lose its virtue when communicated to other objects as did electricity. Tytler further remarked that "the analogies betwixt magnetism and electricity are so great, that the hypothesis of a magnetic as well as of an electric fluid has now gained general credit."[144] Aepinus, he explained, had attempted to solve the problem of magnetism using just such a fluid.

Borrowing heavily from Cavallo's earlier discussion, Tytler accurately explained Aepinus' magnetic theory. Nonetheless he rejected it, particularly with regard to terrestrial magnetism. Because Tytler's explanation relied on a circulating, atmospheric electric fluid, Aepinus' theory of a internal magnetic fluid, distinct from the electric fluid, seemed "not to be tenable in any respect."[145] On the one hand, Tytler separated electric and magnetic fluids, while on the other he blurred the distinction. Such inconsistencies clearly illustrate the persistent confusion and mystery surrounding the investigation of magnetic phenomena.

While some rejected the one-fluid hypothesis, others simply remained unaware of Aepinus' work. In 1796, George Gregory upheld essential differences between electricity and magnetism.[146] Opposing Cartesian fluids, he did not wish to encumber his work with "grand systems." Gregory championed the distinct natures of electricity and magnetism, yet failed to mention either Aepinus or his theory.[147] Similarly, Irishman Matthew Young's *An Analysis of the Principles of Natural Philosophy* (1800) did not mention Aepinus. Like Gregory, he believed that electricity and magnetism did not interrupt each other's operations. Young remained dubious of proving the existence of the magnetic fluid, yet insisted that it "must be admitted; because we cannot conceive a body to act where it is not."[148] Hence, while accepting a magnetic fluid, Young's fluid differed from that of Aepinian theory. Rejecting action at a distance, his fluid, like the circulating effluvia, served a mechanical function. In contrast, Aepinus appealed to inexplicable forces acting at a distance.

Conclusion

Knowledge of Aepinian theory in Britain ranged from complete ignorance to indirect acquaintance to more direct familiarity. Regardless of whether investigators knew or had read his work, some clearly distinguished between electric and magnetic fluids while others continued to speculate about unifying principles (e.g., the ether). Although many supported circulating effluvial theories, Aepinus gained increasing support in both France and Britain from a new generation of physicists who challenged the older theories and experimental methods.[149] In the 1780s, a committee of the Paris Academy of Sciences led by Pierre Simon de Laplace judged Aepinus' *Tentamen* as creating "an epoch in the history of the sciences."[150] In like manner, Gaspard De la Rive read a paper in 1797 to Edinburgh's Royal Medical Society which began, "Aepinus has produced in the theory of Magnetism a Revolution, which may be compared to that which was operated by Newton in Physics."[151] By the early nineteenth century, more British investigators endorsed an internal imponderable fluid in their explanations of magnetic phenomena. This new theory combined quantitative experimental evidence with some mathematical elaboration. In doing so, it meshed with the emerging notions of experimental physics better than the qualitative, non-mathematical circulating fluid theories. Aepinian theory also rejected mechanical impulsions, an idea which was losing favor, and endorsed unexplained forces acting at a distance.

In general, embracing Aepinian theory meant renouncing the older Cartesian theories and atmospheric theories as well. His single imponderable fluid neither circulated outside of magnetic bodies nor formed magnetic atmospheres surrounding the magnetic poles. Aepinus' fluid remained confined inside all bodies, including the earth. From the 1780s onward, British advocates of the one-fluid theory also strengthened support for the Gilbertian hypothesis. In following Aepinus, they challenged speculations about the intimate connections between different phenomena, especially electricity and magnetism. Thereby, Aepinus and his followers downplayed the importance of unifying principles such as ether that remained prevalent in the British natural philosophical tradition. In contrast, the Aepinian approach to magnetism stressed applied mathematics based upon calculations which were verified by careful experiment. In this manner, Aepinus and others joined together elements of the experimental and mathematical traditions. However, despite more dramatic changes in French experimental physics, a handful of British investigators during the 1770s and 1780s sought to unite mathematics and experiment in the study of magnetism.

As this chapter has shown, the study of magnetism in Britain from the 1750s through the 1780s underwent several gradual changes. Beyond the frequently acknowledged importance of magnetism, it received less attention than other areas of experimental physics, especially electricity which saw tremendous growth in the eighteenth century. The persistently frustrating complexities of magnetic and terrestrial magnetic phenomena, including the lack of a force law, contributed to its relative lack of attention. At the turn of the century, experimentalists remained skeptical about attaining a predictive theory of terrestrial magnetism. Unobservable chemical and physical changes within the earth, among other variables, increased their doubts. As we shall see in the beginning of the next chapter, the work of several, including Italian-born experimental philosopher, Tiberius Cavallo, and Edinburgh natural philosophy professor, John Robison, illustrated the shifting state of British magnetic theory in the last quarter of the eighteenth century. In a broader sense, their work also demonstrated the changing nature of British experimental physics.

Notes

[1] Robert Palter, "Early Measurements of Magnetic Force," *Isis*, 63 (1972), 549-550. See also Servington Savery, "Magnetical Observations and Experiments," *Philosophical Transactions of the Royal Society of London*, 36 (1730), 295-340, and Benjamin Martin, *Philosophia Britannica* (London, 1747), 1: 35-38.

[2] John Freke, *A treatise on the nature and property of fire: in three essays* (London: Printed for W. Innys and J. Richardson, 1752) [Landmarks of science microform, 1970]. For more on Freke's researches see J. L. Heilbron, *Electricity in the 17th and 18th Centuries, a study of early modern physics* (Berkeley: University of California Press, 1979), 294-296.

[3] Temple Henry Croker, *Experimental magnetism* (London: J. Coote, 1761) [Landmarks of science, 1974], x. Croker (1730?-1790?) was elected to a scholarship at Trinity College, Cambridge in 1746, however he removed to Christ Church, Oxford where he graduated (B. A. 1750, M. A. 1760). He spent his career as a chaplain and miscellaneous writer. See [Thompson Cooper], "Croker, Temple Henry," *DNB*, 13: 132.

[4] Ibid., 15.

[5] Ibid., 59, 69. Although the cause of magnetism remained unknown, Croker explained in Aristotelian fashion that earth and water acted toward the nadir, air and fire acted toward the zenith, and magnetism acted toward the horizon. Hence, magnetism acted at right angles to the motions of the four Aristotelian elements without impeding their movements.

[6] "Magnet," *The complete dictionary of arts and sciences*, vol. II, (London: Printed for the authors, 1764) [Landmarks of science, 1969].

[7] Oliver Goldsmith, *A survey of experimental philosophy: considered in its present state of improvement* (London: T. Carnan and F. Newbery, 1776) [Landmarks of Science microform, 1968], 32. Goldsmith, better known as a poet, attended Trinity College, Dublin. He wrote on a wide variety of subjects including *A survey of experimental philosophy* written in 1765 and first published in 1776. See Leslie Stephen, "Goldsmith, Oliver," *DNB* 22: 87, 94.

[8] Ibid., 33.

[9] Richard Lovett, *Philosophical essays: in three parts* (Worcester: R. Lewis, 1766) [Landmarks of science II, 1979], 422.

[10] Ibid., 427.

[11] William Nicholson, *An introduction to natural philosophy* (London: Printed for J. Johnson, 1782) [Landmarks of Science microform, 1970], 327. Nicholson (1753-1815) was educated in North Yorkshire. He started a school of mathematics in London and published numerous works on practical chemistry. See Captain S. P. Oliver, "Nicholson, William," *DNB* 41: 28-30.

[12] William Mountaine, "Letter. . . from William Mountaine, Esq. F. R. S. to Mr. [Nevil] Maskelyne," *Philosophical Transactions of the Royal Society of London*, 66 (1776), 19.

[13] Rev. Erasmus Middleton, *The New and Complete Dictionary of Arts and Sciences or, An Universal System of Useful and Entertaining Knowledge, being a New Cyclopaedia* (London: Printed for Alex Hogg, 1778), n. p.

[14] George Adams, *An essay on electricity* (London: Printed for and sold by the Author, 1784) [Landmarks of science microform, 1968], 330.

[15] Thomas Harding, "Observations on the Variation of the Needle." *Transactions of the Royal Irish Academy* 4 (1790), 107 and Ralph Walker, *A treatise on magnetism: with a description and explanation of a meridional and azimuth compass, for ascertaining the quantity of variation, without any calculation whatever, at any time of the day* (London: Printed by R. Hindmarsh, 1794), 9.

[16] Charles Hutton, *A Mathematical and Philosophical Dictionary: containing an explanation of the terms, and an account of the several subjects, comprized under the heads mathematics, astronomy, and philosophy both natural and experimental* (London: Printed for J. Johnson, and G. G. and J. Robinson, 1795), 2: 73.

[17] George Gregory, *The economy of nature: explained and illustrated on the principles of modern philosophy* (London: Printed for J. Johnson, 1796) [Landmarks of science, microform, 1968], 60 and John Imison, *The School of Arts; or, an introduction to useful knowledge* (London: J. Murray and S. Highley, Fourth edition, 1796), 161. Gregory (1754-1808), although mostly self-educated, studied at the University of Edinburgh earning a D. D. in 1792. As a parish priest, most of his writings were on religious topics. See [Lloyd C. Sanders], "Gregory, George," *DNB*, 23: 97. John Imison (d. 1788) earned a living as a clock and watchmaker and also as a printer in Manchester. *The School of Arts* was first published in 1785; subsequent revised editions included those of 1796, 1807 and 1822. See [C. W. Sutton], "Imison, John," *DNB*, 28: 417.

[18] See Margaret Bryan, *Lectures on Natural Philosophy: the result of many years' practical experience of the facts elucidated* (London: Thomas Davison, 1806), 143; James Mitchell, *The elements of natural philosophy: illustrated throughout by experiments which may be performed without regular apparatus* (London: Printed for T. and J. Allman, 1819) [Landmarks of science microform, 1970], 270; and John Imison, *Elements of Science and Art: Being a familiar introduction to natural philosophy and chemistry*, a new edition, considerably enlarged by Thomas Webster (London: F. C. and J. Rivington, 1822), 412.

[19] William Nicholson, *An introduction to natural philosophy* (London: Printed for J. Johnson, 1782) [Landmarks of Science microform, 1970], 271.

[20] Thomas Harding, "Observations on the Variation of the Needle." *Transactions of the Royal Irish Academy*, 4 (1790), 117.

[21] George Adams, *Lectures on natural and experimental philosophy* (London: Printed by R. Hindmarsh, 1794) [Landmarks of science microform, 1968], 458. Adams, the younger (1750-1795), succeeded his father as mathematical instrument maker to King George III. He wrote a large number of elementary scientific works. See "Adams, George, the younger," *DNB*, 1: 97.

[22] Ibid., 435.

[23] John Robison, "Magnetism," *Encyclopaedia Britannica*, third edition, supplement, (1801) 2: 138.

[24] William Enfield, *Institutes of natural philosophy, theoretical and experimental* (London: printed for J. Johnson, 1785) [Landmarks of science microform, 1973]. Enfield (1741-1797), a clergyman who earned his LL. D. from the University of Edinburgh in 1774, wrote mostly on religious topics. A second edition of *Institutes of Natural Philosophy* appeared in 1799. See [C. W. Sutton], "Enfield, William," *DNB*, 17: 369-370.

[25] George Atwood, *An analysis of a course of lectures on the principles of natural philosophy: read in the University of Cambridge* (London: T. Cadell, 1784) [Landmarks of Science microform, 1968]. Atwood (1746-1807) finished third wrangler and first Smith's prizeman from Trinity College, Cambridge in 1769. He was elected Fellow of the Royal Society in 1776. See [Agnes M. Clerke], "Atwood, George," *DNB*, 2: 242 and Thomas Young, "No. LXXXVI., Life of Atwood," *Miscellaneous Works of the late Thomas Young. . .*(London: John Murray, 1855)[NY: Johnson Reprint Corp., 1972], vol. II: 617-623. John Imison wrote more about electricity than magnetism in a ratio of nearly six to one. John Imison, *The School of Arts; or, an introduction to useful knowledge* (London: J. Murray and S. Highley, Fourth edition, 1796).

[26] Adam Walker, *A system of familiar philosophy: in twelve lectures* (Edinburgh: Bell and Bradfute, 1799) [Landmarks of science microform, 1974]. Walker (1731?-1821), though mostly self-taught, earned his living as a traveling lecturer in natural philosophy. He later lectured in several public schools including Eton, Westminster and Winchester. See [E. Irving Carlyle], "Walker, Adam," *DNB*, 59: 42.

[27] George Adams, *An essay on electricity* (London: Printed for and sold by the Author, 1784) [Landmarks of science microform, 1968].

[28] Paul Henry Maty, *Index for the Philosophical Transactions of the Royal Society of London v. 1-70 (1710-1780)* (London: Printed for Lockyer Davis & Peter Elmsly, 1787), 159-166, 292-297.

[29] "Review of *A Treatise on Magnetism, in Theory and Practice, with Original Experiments* by Tiberius Cavallo, F. R. S." *The Critical Review: or, Annals of Literature*, series 1, 64 (August 1787): 101. See also Patricia Fara, *Magnetic England in the eighteenth century* (Ph.D. thesis, University of London, Imperial College, 1993), 24. I thank Dr. Gregory Good for loaning me his copy of this dissertation. See also Patricia Fara, *Sympathetic Attractions: Magnetic Practices, Beliefs, and Symbolism in Eighteenth-Century England* (Princeton, NJ: Princeton University Press, 1996).

[30] Patricia Fara, *Magnetic England in the eighteenth century* (Ph.D. thesis, University of London, Imperial College, 1993), 69. See also Thomas L. Hankins, *Science and the Enlightenment* (Cambridge: Cambridge University Press, 1985), 53-57.

[31] Joseph Priestley, *The History and Present State of Electricity, with original experiments*. third edition, 2 vol. (London: For C. Bathurst, T. Lowndes, etc., 1775) [Reprint: New York: Johnson Reprint Co., 1966], 2: 134.

[32] Marcello Pera, *The Ambiguous Frog: The Galvani-Volta Controversy on Animal Electricity*. translated by Jonathan Mandelbaum, originally published as *La rana ambigua*, Giulio Einaudi editore. Torino: 1986 (Princeton, NJ: Princeton University Press, 1992), 3-18.

[33] Roderick Weir Home, "Introduction," *Aepinus's essay on the theory of electricity and magnetism* (Princeton, N. J.: Princeton University Press, 1979), 204. See Leonhard Euler, *Letters of Euler on different subjects in physics and philosophy: addressed to a German princess*, Translated from French by Henry Hunter (London: Murray and Highley, 1802, 2d ed.).

[34] Leonhard Euler, *Letters of Euler on different subjects in Natural Philosophy addressed to a German Princess*, with notes and a life of Euler by David Brewster, (New York: Printed by J. & J. Harper, 1833), 2: 212-213.

[35] For Euler's critique of Halley and further mathematical elaboration of the two-pole theory see Leonhard Euler, "Recherches sur la declinaison de l'aiguille aimantée," *Histoire de l'Academie Royale des Sciences et Belles Lettres*, année MDCCLVII, (Berlin: Chez Haude et Spener, 1759), XIII: 175-251.

[36] Leonhard Euler, "Recherches sur la declinaison de l'aiguille aimantée," *Histoire de l'Academie Royale des Sciences et Belles Lettres*, année MDCCLVII, (Berlin: Chez Haude et Spener, 1759), XIII: 176. Euler wrote: "je conviens qu'une telle entreprise seroit trop hardie du moins pour l'état présent de nos connoissances, puisque la force directrice, dont deux ou plusieurs aimants agissent à la fois sur une aiguille, nous est encore tout à fair inconnue: & il vaudroit sans doute mieux d'abandonner d'abord cette enterprise, que de la fonder sur des hypotheses arbitraires."

[37] Benjamin Martin, *Philosophia Britannica: or, a new and comprehensive system of the Newtonian Philosophy, Astronomy, and Geography*, (London: Printed for W. Strahan, J. & F. Rivington. . ., 1771, Third Edition), 1: 44.

[38] William Hooper, *Rational recreations: in which the principles of numbers and natural philosophy are clearly and copiously elucidated* (London: Printed for L. Davis, 1774), 115. In 1784, I. Atkinson's lectures on experimental philosophy explained that a magnetic virtue was "emitted from two parts of the stone or magnet, in a very fine imperceptible effluvium." This was apparently different from the Cartesian circulating fluid theory. See I. Atkinson, *A compendium of a course of lectures on natural and experimental philosophy: viz., properties of matter and magnetism, chemistry, electricity , hydrostatics, fortification, hydraulics, mechanics, optics, astronomy, &c.:*

to which are added notes and demonstrations, poetical extracts, &c. (Kendall: James Ashburner, 1784), 13.

[39] George Adams, *An essay on electricity: in which the theory and practice of that useful science are illustrated by a variety of experiments, arranged in a methodical manner: to which is added an essay on magnetism* (London: 1784) [Landmarks of science microform, 1968], 332.

[40] "Magnetism," *Cyclopaedia: or, An universal dictionary of arts and sciences,* with the supplement, and modern improvements, incorporated in one alphabet, by Abraham Rees (London: Printed for J. F. & C. Rivington, 1786), n. p.

[41] John Anderson, *Institutes of physics,* fourth edition (Glasgow: Printed by Robert Chapman and Alexander Duncan, 1786), 230. I thank Dr. David B. Wilson for this source. Anderson (1726-1796) studied at Glasgow and was professor of natural philosophy there from 1757 until his death. See "Anderson, John," *DNB,* 1: 383-384.

[42] Captain O'Brien Drury, "Observations on the Magnetic Fluid," *Transactions of the Royal Irish Academy,* 2 (1788), 119.

[43] John Imison, *The School of Arts; or, an introduction to useful knowledge* (London: J. Murray and S. Highley, Fourth edition, 1796), 163-164.

[44] Ibid., 165.

[45] Adam Walker, *A system of familiar philosophy: in twelve lectures* (Edinburgh: Bell and Bradfute, 1799)[Landmarks of science microform, 1974], 43.

[46] Adam Walker, *Analysis of a Course of Lectures on Natural and Experimental Philosophy* (Lancaster: William Greer, 1809), 7.

[47] Adam Walker, *A system of familiar philosophy: in twelve lectures* (Edinburgh: Bell and Bradfute, 1799), 52.

[48] George Adams, *Lectures on natural and experimental philosophy* (London: Printed by R. Hindmarsh, 1794)[Landmarks of science microform, 1968], 300.

[49] Ibid.

[50] Nearly fifteen years later, geologist Robert Bakewell (1768-1843) also proposed an internal fire producing magnetic variations. Robert Bakewell, *An introduction to geology* (London: J. Harding, 1813), 324. See also Bert Hansen, "Bakewell, Robert," *DSB,* 1: 413.

[51] Joseph Priestley, *Experiments and observations relating to various branches of natural philosophy, with a continuation of the observations on air* (London: J. Johnson, 1779) [Millwood, N. Y. : Kraus Reprint Co., 1977], 225.

[52] John Lyon, *Farther proofs that glass is permeable by the electric effluvia: and that the electric particles are possessed of a polar virtue.* (London: Printed for the author, and J. Dodsley, 1781), 60-65.

[53] John Dalton, *Meteorological observations and essays* (London: Printed for W. Richardson, J. Phillips, and W. Pennington, 1793) [Landmarks of Science microform, 1973], 175, 63. See also Willem Hackmann, "Instrument and Reality: The Case of Terrestrial Magnetism and the Northern Lights," *Philosophy and Technology, Royal Institute of Philosophy Supplement*, 38 (1995), 29-52.

[54] Ibid., 187.

[55] George Adams, *Lectures on natural and experimental philosophy* (London: Printed by R. Hindmarsh, 1794), 463.

[56] Ibid., 469.

[57] J. B. Gough, "Lesage, George-Louis (1724-1803)," *Dictionary of Scientific Biography*, 8: 259. Though these particles were too small to move ordinary matter, two masses of ordinary matter blocked some of the particles coming from either direction along lines connecting their parts. From the resulting "gravitational shadow," the ensuing disequilibrium of forces impelled the two bodies together, thereby explaining the illusion of gravitational attraction. Le Sage explained his mechanism for gravitation in *Essai de chimie mécanique* (Rouen, 1758). See also George-Louis Le Sage, "The Newtonian Lucretius," read by M. Prevost at a meeting of the Berlin Academy in 1782, translated by C. G. Abbot from *Nouveaux Mémoires de L'Académie Royale des Sciences et Belles-Lettres* (Berlin, 1784), 404-427, in *Annual Report of the Board of Regents of the Smithsonian Institution* (1898), 141-160, and Pierre Prevost, "Notice de la Vie et des Ecrits de George Louis Le Sage de Geneve. . .," *The Edinburgh Review*, 10 (1807), 146-149.

[58] Pierre Prevost, *De L'Origine des Forces Magnétiques* (Geneva: Barde, Manget & Company, 1788).

[59] George Adams, *Lectures on natural and experimental philosophy* (1794), 469.

[60] For a more detailed account of Prevost's magnetic theory in the context of his other scientific ideas, see Burghard Weiss, *Zwischen Physikotheologie und Positivismus, Pierre Prevost (1751-1839) und die korpuskularkinetische Physik der Genfer Schule* (Frankfurt am Main: Verlag Peter Lang, 1988), 150-168.

[61] Chiefly known for his works on physical and chemical theory, Peart (1756?-1824) wrote on numerous scientific topics. He acutely criticized both Priestley and Lavoisier in his works and tried to explain all physical and chemical phenomena with four elements- aether, phlogiston, earth, and the acid principle. It appears that Peart had few converts to his theories. See [P. J. Hartog], "Peart, Edward," *DNB*, 44: 178.

[62] Edward Peart, *On the elementary principles of nature, and the simple laws by which they are governed* (Gainsborough: Printed by H. Mozley, 1789), 163.

[63] Ibid., 164-165.

[64] Ibid., 167.

[65] Walker (1749-1824) learned rudimentary mathematics and navigational skills in England before going to sea. Involved in the Jamaican trade from 1768 to 1774, Walker settled down as a Jamaican sugar planter in 1783. Continuing an interest in navigation, he worked on methods of improving compasses and finding longitude by magnetic measurements. See W. E. May, "The Gentleman of Jamaica," *Mariner's Mirror*, 73 (1987), 149-165.

[66] Ralph Walker, *A Treatise on the Magnet, or, Natural Loadstone, with tables of the variation of the magnetic needle, for all latitudes and longitudes* (London: Printed for the Author, 1798), 10.

[67] James Tytler, "Magnetism," *Encyclopaedia Britannica*, Third Edition, 10 (Edinburgh: A. Bell and C. Macfarquhar, 1797), 433.

[68] Tytler, who wrote about three-fourths of the *Britannica*'s second edition (1777-1784) and much of the third edition (1787-1797), attended the University of Edinburgh where he took medical courses. See [T. F. Henderson], "Tytler, James," *DNB*, 57: 452-453; Arthur Hughes, "Science in English Encyclopaedias, 1704-1875. Part I," *Annals of Science*, 7 (1951), 344; and Robert Chambers, "Tytler, James," *A Biographical Dictionary of Eminent Scotsmen* (Glasgow: Blackie & Son, 1835), 4: 365-368.

[69] James Tytler, "Magnetism," *Encyclopaedia Britannica*, 10 (Third Edition, 1797), 433 (my emphasis).

[70] James Tytler, "Magnetism," *Encyclopaedia Britannica*, v. 6, pt. 2 (Second Edition, 1778-1783), 4378.

[71] James Tytler, "Magnetism," *Encyclopaedia Britannica*, 10 (Third Edition, 1797), 436.

[72] See Giambatista Beccaria's *Dell' elettricita terrestre atmosferica a cielo sereno* (Turin, 1775) and Comte de Buffon's *Histoire naturelle des minéraux* (Paris, 1788). Beccaria (1716-1781), the professor of natural philosophy at the University of Turin and a strong advocate of Franklinian electric theory, claimed that a constant natural circulation of the electric fluid from north to south created terrestrial magnetic phenomena. See George John Singer, *Elements of Electricity and Electro-Chemistry* (London, 1814), 252; and John L. Heilbron, "Beccaria, Giambatista," *DSB*, 1: 546-548. For Buffon see R. W. Home, "Introduction," *Aepinus's essay on the theory of electricity and magnetism* (Princeton, N. J.: Princeton University Press, 1979), 220.

[73] James Tytler, "Magnetism," *Encyclopaedia Britannica*, Third Edition, 10 (Edinburgh: A. Bell and C. Macfarquhar,1797), 436.

[74] James Tytler, "Electricity," *Encyclopaedia Britannica*, Third Edition, 6 (Edinburgh: A. Bell and C. Macfarquhar, 1797), 537. Stephen J. Goldfarb attributes this article to Tytler. See Stephen Joel Goldfarb, *Main Currents of British Natural Philosophy, 1780-1820* (Ph. D. dissertation, Case Western Reserve University, 1973), 50-51. Maurice Crosland and Crosbie Smith incorrectly attribute Tytler's article to John Robison. Robison's "Electricity" appeared in the third edition's *Supplement* (1801). See Maurice Crosland and Crosbie Smith, "The Transmission of Physics from France to Britain," *Historical Studies in the Physical Sciences*, 9 (1978), 19.

[75] Louis Elisabeth Comte de Tressan, *Essai sur le fluide electrique, considere comme agent universel.* Paris: Buisson, 1786 [Landmarks of science microform, 1972], vol ii: 381.

[76] "Electricity," *Encyclopaedia Britannica* (Edinburgh: A. Bell and C. Macfarquhar, Third Edition, 1797), 6: 538. Tytler explained that a system of natural philosophy based upon this principle had been recently published by Count de Tressan. In this system, the fixed stars were the foci of action communicating electricity to their surrounding planets with electric atmospheres of differing extent.

[77] "Magnetism," *Encyclopaedia Perthensis* (Edinburgh: Printed by John Brown, Second Edition, 1816), 558. The same position was repeated in a very similar article, "Magnetism," *The Imperial Encyclopaedia; or, dictionary of the sciences and arts*, 363-371, compiled by William Moore Johnson. (London: J. & J. Cundee, 1812).

[78] Matthew Young, *An Analysis of the Principles of Natural Philosophy* (Dublin: The University Press, 1800) [Landmarks of Science microform, 1973], 442-443. Young (1750-1800), bishop of Clonfert, earned a B. A. and M. A. from Trinity College, Dublin. He wrote on a broad range of topics including acoustics, mathematics, theology and poetry. See [Richard Garnett], "Young, Matthew," *DNB*, 63: 384-385.

[79] Ibid., 448.

[80] Erasmus Darwin, *The temple of nature* (Facsimile of 1st ed., London: Johnson, 1803) [Menston, Scolar Press, 1973], 46.

[81] Ibid., 68.

[82] Ibid., 69.

[83] See R. W. Home, "Newton on Electricity and the Aether," I, 191-213, in *Electricity and Experimental Physics in Eighteenth-Century Europe* (Great Britain: Variorum, Bookcraft Ltd., 1992) and D. A. Arnold, "The Mécanique Physique of Simeon-Denis Poisson: The Evolution and Isolation in France of his Approach to Physical Theory (1800-1840)," *Archive for the History of Exact Sciences*, 28 (1983) Part I: 260-261.

[84] Isaac Newton, *Principia*, ed. Motte-Cajori (London, 1934), 546.

[85] Isaac Newton, *Opticks: or, a Treatise of the Reflections, Refractions, Inflections and Colours of Light* (Fourth Edition, London: William Innys, 1730) [Reprint London: G. Bell & Sons, Ltd., 1931], 399.

[86] P. M. Harman, "'Nature is a perpetual worker': Newton's aether and eighteenth-century natural philosophy," *Ambix*, 20 (1973), 3-10. See also J. E. McGuire, "Force, Active Principles and Newton's Invisible Realm," *Ambix*, 15 (1968), 154-208. McGuire shows the many ambiguities and alterations regarding the discussion of active principles in Newton's writings.

[87] Gowin Knight, *An attempt to demonstrate, that all the phoenomena in nature may be explained by two simple active principles, attraction and repulsion: wherein the attractions of cohesion, gravity, and magnetism, are shewn to be one and the same, and the phoenomena of the latter are more particularly explained* (London: 1748), title page. See pp. 66-95 for Knight's discussion of magnetism.

[88] David Hartley, *Observations on Man, His Frame, His Duty, and His Expectations* (London: S. Richardson, 1749) [Gainesville, FL: Scholars' Facsimiles & Reprints, 1966], 15.

[89] Ibid., 29. Hartley hoped that future investigations would "analyse all the Actions of Bodies upon each other, up to a few simple Principles, by making such Suppositions as the Phaenomena shall suggest, and then trying and modelling them by the Phaenomena. At least this is what one is led to hope, from the many simple and easy Solutions of very complex Problems, which have been produced within the last two Centuries." Ibid., 30.

[90] In 1762, William Jones (1726-1800) put forth an etherial medium whose alterations produced gravity, elasticity, cohesion, electricity, magnetism, heat, and illumination. How far changes occurred "from the different densities of the medium or the different magnitudes of its parts," he proposed as a subject for further investigation. See William Jones, *Essay on the first Principles of Natural Philosophy: wherein the Use of Natural Means, or second Causes in the Oeconomy of the Material World, is demonstrated from Reason, Experiments or various kinds, and the Testimony of Antiquity* (1762), 62. Quoted in Robert E. Schofield, *Mechanism and materialism; British natural philosophy in an age of reason* (Princeton, N. J.: Princeton University Press, 1969), 126-127.

[91] Benjamin Martin, *Philosophia Britannica: or, a new and comprehensive system of the Newtonian Philosophy, Astronomy, and Geography.* vol. I (London: Printed for W. Strahan, J. & F. Rivington. . ., 1771, Third Edition), 15. See also Bryan Higgins, *A Philosophical Essay Concerning Light* (London, 1776). Higgins, a lecturer and chemist, supposed that light, phlogiston, fire, and electricity were modifications of one repellent matter. See Harman, "'Nature is a perpetual worker': Newton's aether and eighteenth-century natural philosophy," *Ambix*, 20 (1973), 22-23.

[92] William Henly, "Experiments and Observations in Electricity," *Philosophical Transactions of the Royal Society of London*, 67 pt. 1 (1777), 135. See also Tiberius Cavallo, *A complete treatise of electricity in theory and practice: with original experiments* (London: Printed for E. and C. Dilly, 1777) [Landmarks of science microform, 1969], 115-116.

[93] John Lyon, *Experiments and observations made with a view to point out the errors of the present received theory of electricity* (London: J. Dodsley, 1780) [Landmarks of science microform, 1970], 70. Lyon (1734-1817), primarily an historian of Dover, also wrote several works on electricity including a commentary on Benjamin Franklin's one fluid theory in 1791. See [Gordon Goodwin], "Lyon, John," *DNB*, 34: 350.

[94] Ibid., 195.

[95] Felix O'Gallagher, *An Essay on the investigations of the First Principles of Nature. . .* vol I. (Dublin: Joseph Hill, 1784) [Landmarks of science microform, 1971], 306.

[96] Felix O'Gallagher, *An Essay on the investigations of the First Principles of Nature. . .* vol II. (Dublin: Joseph Hill, 1785) [Landmarks of science microform, 1971], 261.

[97] Edward Peart, *On the elementary principles of nature, and the simple laws by which they are governed* (Gainsborough: Printed by H. Mozley, 1789), 169.

[98] Ibid., 177.

[99] T. Gale, *Electricity, or, Ethereal fire considered* (Troy: Moffit & Lyon, 1802) [Landmarks of science microform, 1972], 22.

[100] See Stephen Joel Goldfarb, Chapter Three: "Amateur Etherialists," *Main Currents of British Natural Philosophy, 1780-1820* (Ph. D. dissertation, Case Western Reserve University, 1973). Goldfarb gives additional examples of this tradition including William Jones (1726-1800), Robert Young, Charles Carpenter Bompass, Richard Phillips (1767-1840), and Joseph Luckcock. Each of these men vaguely asserted the existence of an underlying primitive substance responsible for several natural phenomena. See also P. M. Harman, "'Nature is a perpetual worker': Newton's aether and eighteenth-century natural philosophy," *Ambix*, 20 (1973), 1-25.

[101] John Read, *A summary view of the spontaneous electricity of the earth and atmosphere* (London: Elmsley, Richardson, Hookham & Carpenter, Deighton, 1793) [Landmarks of science microform, 1972], 102. Others specifically mentioned by Read in this failure included William Whiston (1667-1752) and Benjamin Hoadly (1706-1757).

[102] George Adams, *An essay on electricity: in which the theory and practice of that useful science are illustrated by a variety of experiments, arranged in a methodical*

manner: to which is added an essay on magnetism (London: 1784) [Landmarks of science microform, 1968], 329.

[103] George Adams, *Lectures on natural and experimental philosophy* (1794), 470.

[104] Adam Walker, *A system of familiar philosophy: in twelve lectures* (Edinburgh: Bell and Bradfute, 1799) [Landmarks of science microform, 1974], 56.

[105] Adam Walker, *A system of familiar philosophy,* rev. ed., 2 vols., (London, 1801) I: 1. For discussion of Walker's active principles, see P. M. Harman, "'Nature is a perpetual worker': Newton's aether and eighteenth-century natural philosophy," *Ambix,* 20 (1973), 23.

[106] For full discussion see P. M. Harman, "Ether and Imponderables," in *Conceptions of Ether: Studies in the History of Ether Theories, 1740-1900,* edited by G. N. Cantor and M. J. S. Hodge (Cambridge: Cambridge University Press, 1981), 61-83.

[107] James Hutton, *Dissertations on Different Subjects in Natural Philosophy* (Edinburgh, 1792), 505. For more on Hutton see Harman, "'Nature is a perpetual worker': Newton's aether and eighteenth-century natural philosophy," *Ambix,* 20 (1973), 17-22.

[108] Stephen Dickson, *An Essay on Chemical Nomenclature* (Dublin, 1796), 64-65.

[109] Captain John Hamstead, *A philosophical enquiry into the properties and laws of magnetism* (London: G. Kearsley, 1809).

[110] J. A. De Luc, "On the Electric Column," *A Journal of Natural Philosophy, Chemistry, and the Arts,* 27 (1810) 268. Deluc (1727-1817) moved from Geneva to England in 1773. As a well-known geologist and meteorologist, he was warmly received and elected a fellow of the Royal Society. Soon afterwards he was appointed reader to Queen Charlotte, consort of George III. See Robert P. Beckinsale, "Deluc, Jean André," *DSB,* 4: 27-29; and [W. Jerome Harrison], "Deluc, Jean André," *DNB,* 14: 328- 329.

[111] John Bywater, *An essay on light and vision. . . to which are added some original remarks and experiments on the magnetic phenomena, intimately connected with practical navigation* (Liverpool: E. Smith, 1813) [Landmarks of science microform, 1968], ii.

[112] Charles Carpenter Bompass, *An Essay on the nature of Heat, Light, and Electricity* (London: T. & G. Underwood, 1817) [Landmarks of science microform, 1968], 191, 257-259. Bompass was a Barrister at Law at Bristol. For a discussion of inconsistencies in Bompass' system and others proposed by non-scientists, see Stephen Joel Goldfarb, *Main Currents of British Natural Philosophy, 1780-1820* (Ph. D. dissertation, Case Western Reserve University, 1973), 59-72.

[113] John Murray, *A System of Chemistry* (Edinburgh, 1806), vol. 1, notes 120.

[114] Pierre-Hyacinthe Azaïs, "Théorie générale de l'Electricité, du Galvanisme, et du Magnetisme," *Philosophical Magazine*, 28 (1807), 182-183. Elaborating his unified cosmological system, Azaïs (1766-1845) wrote *Système universel* which appeared in eight volumes between 1800 and 1812. See L. de Lacger, "Azaïs (Pierre-Hyacinthe)," *Dictionnaire de biographie Française*, 4: 972.

[115] Ibid., 184.

[116] Margaret Bryan, *Lectures on Natural Philosophy: the result of many years' practical experience of the facts elucidated* (London: Thomas Davison, 1806), 157-158.

[117] John Bywater, *An essay on light and vision. . . to which are added some original remarks and experiments on the magnetic phenomena, intimately connected with practical navigation* (Liverpool: E. Smith, 1813) [Landmarks of Science microform, 1968], 63.

[118] Ibid., 68.

[119] Ibid., 88.

[120] George Gregory, *The economy of nature: explained and illustrated on the principles of modern philosophy* (London: Printed for J. Johnson, 1796) [Landmarks of science, microform, 1968], 44.

[121] William Nicholson, "Magnetism," *The British encyclopaedia; or, Dictionary of arts and sciences* (London: Longman, Hurst, Rees, and Orme, 1809), vol. IV: n. p.

[122] R. W. Home, "Out of a Newtonian straitjacket: Alternative approaches to eighteenth-century physical science." in *Studies in the eighteenth century, IV: Papers presented at the fourth David Nichol Smith memorial seminar Canberra 1976*, (Canberra, 1979), 244-248.

[123] See R. W. Home, "Aepinus and the British Electricians: The Dissemination of a Scientific Theory," *Isis* 63 (1972), 190-204 and R. W. Home, "Aepinus, the Tourmaline Crystal, and the Theory of Electricity and Magnetism," *Isis*, 67 (1976), 21-30.

[124] See Anon., "That the Electricity of Glass disturbs the Mariner's Compass, and also nice Balances," *Philosophical Transactions of the Royal Society of London*, 44 (1746), 242-245; and Benjamin Franklin, *Experiments and Observations on Electricity, made at Philadelphia in America. To which are added Letters and Papers on Philosophical Subjects* (1750), 91-94.

[125] On Franklin's theory see I. B. Cohen, *Franklin and Newton, An Inquiry into Speculative Newtonian Experimental Science and Franklin's Work on Electricity as an Example Thereof* (Philadelphia: The American Philosophical Society, 1956), 537-543; R. W. Home, "Franklin's Electrical Atmospheres," *British Journal for the History of*

Science, 6 (1982), 131-151; and John Heilbron, *Electricity in the 17th and 18th Centuries, a study of early modern physics* (Berkeley: University of California Press, 1979), 324-343.

[126] See Benjamin Wilson, "Experiments on the Tourmalin," *Philosophical Transactions of the Royal Society of London*, 51, part 1 (1759), 308- 339. Wilson briefly mentioned Aepinus's work on the tourmaline and the fact that Aepinus accepted Franklin's electrical hypothesis. He said nothing of the one-fluid magnetic theory or of the differences between Aepinian and Franklinian theories.

[127] F. U. T. Aepinus, *Aepinus's essay on the theory of electricity and magnetism*, translation by P. J. Connor (Princeton, N. J.: Princeton University Press, 1979), 238.

[128] Ibid., 243.

[129] Ibid., 399.

[130] Ibid., 400-401.

[131] Benjamin Wilson, "Experiments on the Tourmalin," *Philosophical Transactions of the Royal Society of London*, 51, part 1 (1759), 314.

[132] Joseph Priestley, *The history and present state of electricity, with original experiments* (London: Printed for J. Dodsley, 1767), 430. Price (1723-1791), a nonconformist minister, was primarily known for his political and moral writings. He was also intimate friends with Benjamin Franklin, from whom he probably learned of Aepinus's theories. See "Price, Richard," *DNB*, 46: 334-337.

[133] William Henly, "Experiments and Observations in Electricity," *Philosophical Transactions of the Royal Society of London*, 67 pt. 1 (1777), 135.

[134] John Lyon, *Experiments and observations made with a view to point out the errors of the present received theory of electricity* (London: J. Dodsley, 1780) [Landmarks of science microform, 1970], 90.

[135] Ibid. Newton's second rule of philosophizing was that to the same effects must be, as far as possible, assigned the same causes.

[136] Richard Kirwan, "Thoughts on Magnetism, read March 19, 1796," *Transactions of the Royal Irish Academy* 6 (1797), 177. Kirwan (1733? - 1812) wrote on a wide variety of subjects including geology, mineralogy, meteorology, and applications of science to industry. See E. L. Scott, "Kirwan, Richard," *DSB*, 7: 387-390.

[137] Ibid., 180.

[138] Benjamin Wilson to F. U. T. Aepinus, n. d. (draft; British Library, Add. MS. 30094, f. 91) quoted in Roderick Home, "Out of a Newtonian Straitjacket: Alternative Approaches to Eighteenth-Century Physical Science," *Electricity and Experimental*

Physics in Eighteenth-Century Europe (Great Britain: Variorum, Bookcraft Ltd., 1992), 247.

[139] George Adams, *Lectures on natural and experimental philosophy* (London: Printed by R. Hindmarsh, 1794) [Landmarks of science microform, 1968], 304.

[140] Rev. George Miller, "Observations on the Theory of Electric Attraction and Repulsion," *A Journal of Natural Philosophy, Chemistry, and the Arts*, edited by William Nicholson. 2 (1801), 461. He also discussed Cavallo's and Jean André de Luc's (1727-1817) reservations about the electrical theory of Aepinus. Miller (1764-1848) was educated at Trinity College, Dublin earning B.A. (1784), M. A. (1789), and D.D. (1799) degrees there. He published *Elements of Natural Philosophy* in 1799, which appeared in a second edition in 1820. See [J. M. Rigg], "Miller, George," *DNB*, 37: 406-408.

[141] Pierre Prevost, *De l'origine des forces magnétiques* (Geneva: Barde, Manget & Co., 1788), preface, v. Prevost wrote, "M. Aepinus a produit dans la théorie du magnétisme une révolution qu'on peut comparer à celle que Newton a opérée dans la physique générale."

[142] Ibid., 155.

[143] James Tytler, "Magnetism," *Encyclopaedia Britannica*, third edition, (Edinburgh, 1797), 10: 433. Though Tytler gained a medical education at the University of Edinburgh, he failed as a surgeon and apothecary. Acquiring huge debts throughout his life, he was obliged to flee to America in 1792 due to a contentious political pamphlet. Tytler edited, compiled and wrote much of the second edition of the *Britannica* (1777-1784). Many of his articles were included in the third edition (1787-1797) as well. See Paul Kruse, *The Story of the Encyclopaedia Britannica, 1768-1943*. (Ph.D. Dissertation, University of Chicago, 1958), 60-65.

[144] Ibid., 434.

[145] Ibid., 436.

[146] George Gregory, *The economy of nature: explained and illustrated on the principles of modern philosophy* (London: Printed for J. Johnson, 1796) [Landmarks of science, microform, 1968], 59.

[147] Ibid., 61.

[148] Matthew Young, *An Analysis of the Principles of Natural Philosophy* (Dublin: The University Press, 1800) [Landmarks of Science microform, 1973], 448-450.

[149] See R. W. Home, "Introduction," *Aepinus's essay on the theory of electricity and magnetism* (Princeton, N. J.: Princeton University Press, 1979), 213-224.

[150] Extract from the *Registres* of the Academy, published in R. J. Haüy, *Exposition raisonée de la théorie de l'électricité et du magnétsime, d'apres les principes de M. Aepinus* (Paris: Chez la Veuve Desaint, 1787), xxix.

[151] Gaspard de la Rive, "Observations on the theory of electric attractions and repulsions," *Royal Medical Society, Edinburgh: Dissertations,* 35 (1797-98), 249-266. Quoted in Roderick Weir Home, "Introduction," *Aepinus's essay on the theory of electricity and magnetism* (Princeton, N. J.: Princeton University Press, 1979), 223.

CHAPTER 4:
PONDERING THE IMPONDERABLE
(c.1775-1815)

During the last quarter of the eighteenth century, imponderable fluid theories such as Aepinus' challenged the dominance of circulating fluid theories. Emphasizing unexplained forces of attraction and repulsion, Aepinus' approach initially attracted the attention of several British natural philosophers including Italian-born Tiberius Cavallo, Englishman Henry Cavendish, and Scotsman John Robison. Because Aepinian theory brought quantification and mathematics to the realms of magnetism and electricity, it emulated the model science of Newtonian mechanics. Nonetheless, Cavallo and others remained within the experimental tradition using little if any mathematics in their work. In contrast, Cavendish and Robison, used both their experimental and mathematical talents to develop gradual support for Aepinian theory in Britain. Unlike experimentalists such as Cavallo, Cavendish and Robison were well-versed in mathematics. Though their work combined the mathematical and experimental traditions, nevertheless, it gained scant attention among experimental physicists until the turn of the century. Cavendish, in fact, was a recluse and published little of his magnetic research.[1] Robison's teaching, research, and publications, on the other hand, had more immediate impact on developments in British experimental physics.

As previously noted, Cavallo's presentation of Aepinus' theory remained within the experimental tradition, qualitative and non-mathematical. In contrast, Cavendish's and Robison's research indicated that British magnetic investigations, and experimental physics in general, were slowly moving in a different direction. Rejecting circulating effluvia and atmospheric theories, Cavendish, Robison, and increasing numbers of early nineteenth-century physicists incorporated mathematics with experiment into their investigations of magnetism. These investigators pursued experimental physics in a different manner than the gentlemanly speculators and experimenters discussed in the previous chapter. With more mathematical training, the emerging group of physicists also tended to take experimental physics, and science in general, not as a fashionable hobby, but as a serious lifetime pursuit.[2] Along with this greater seriousness of purpose, increasing numbers in Great Britain, as well as France, Germany, and Switzerland showed growing concern for clarifying the philosophical presuppositions of science.[3] Many Scottish physicists and philosophers, for example, reflected these trends in their teaching, research, and writings. In fact, several historians of science have tried to demonstrate that the Scottish methodological tradition had a significant impact on the style of British physics in the nineteenth century.[4]

With these broader developments in mind, this chapter first examines Cavallo's experimental approach to Aepinus' theory and his methodological position regarding hypotheses. Turning to Robison's education, career, and the evolution of his ideas on magnetism, the similarities and differences between Cavallo and Robison are discussed. Robison's attention to scientific methodology and epistemology betrayed the influence of Scottish education and affinities with foreign scientists as well. With these in mind, the possible sources of Robison's methodological outlook and magnetic theories are proposed. This chapter also illustrates how Robison's colleague and successor, John Playfair, shared similar methodological concerns and views on magnetic theory. Finally, the chapter briefly turns to the subjects of the next chapter— the development of magnetic theory in France and its growing influence in Britain.

Tiberius Cavallo (1749-1809): Magnetism and Experiment

After completing studies at the University of Naples, Tiberio Cavallo settled in England at age twenty two as a merchant in 1771. Encouraged by English physicist William Henly, Cavallo's interests turned from commerce to experimental science. Presenting the results of numerous electrical experiments during the mid-1770s, he became a member of the Royal Society of London in 1779. His research included a wide range of experimental studies in electricity, magnetism, acoustics, chemistry and medicine.[5] Though Cavallo did not include much mathematics in his work, he did expose accurate, qualitative versions of Aepinus' theories to a wider English audience than earlier investigators.

In *A complete treatise on electricity in theory and practice* (1777), Cavallo showed his initial support for ideas similar to those of Aepinus. For instance, he advocated Franklinian theory as the most probable electrical hypothesis. As well, Cavallo questioned the notion that the electric fluid and the ethereal fluid were one and the same. While Newton's extremely subtle and elastic ether repelled particles of matter, the electric fluid attracted matter. Rejecting the ether, Cavallo called it a mere "hypothetical entity," with essence, properties, and existence "absolutely unknown."[6] His support of Franklinian electric theory, his rejection of the ether, and his desire to keep the electric fluid distinct from other fluids made Cavallo predisposed toward Aepinus' interpretation of the analogy between electricity and magnetism.

Reporting his research to the Royal Society of London, Cavallo's Bakerian Lecture of 1786 discussed the effects of heating and hammering magnets, and immersing magnets, iron, and steel in various acids. Cavallo found that iron and steel effervesced when immersed in certain acids and that this effervescent action, within certain limits, increased the attraction of iron and steel toward a magnet.[7] After reporting the experimental results, Cavallo applied them to terrestrial magnetic variations.

Cavallo attempted to base his theory of magnetic variation on experimental results. Though many had tried to explain the causes of terrestrial magnetic variation, none had been successful. No previous hypothesis had been, in his view, founded upon "evident principles." Of various hypotheses put forth, he wrote:

> The supposition of a large magnet being inclosed within the body of the earth, and of its relatively moving with respect to the outward shell or crust; the supposition of their being four moveable magnetic poles within the earth; the hypothesis of a magnetic power partly within and partly without the surface of

> the earth; together with several other hypotheses . . . *are not only unwarranted by actual experiments, but do neither seem analogous to the other operations of nature.*[8]

In the experimental tradition, he argued that Canton's explanation of diurnal variation derived from "properties actually proved by experiments," and extended Canton's theory of heating and cooling to general variations as well. Given the experimentally verified causes of magnetic change including heating, cooling, hammering, and chemical action, Cavallo concluded that all ferruginous bodies within the earth were effected by these same natural actions. Hence, terrestrial magnetic variations arose from the irregular heating and cooling of the earth; volcanoes and earthquakes decomposing, altering, and moving ferruginous substances; deep internal chemical reactions; and the mysterious effects of the aurora borealis. Thus, Cavallo concluded, "The magnetic needle, therefore, being necessarily affected by those causes, it seems unnecessary to have recourse to other hypothetical causes which are not established on actual experience."[9]

In *A Treatise on Magnetism, in theory and practice* (1787), Cavallo gave a comprehensive discussion of his empirical findings. In this treatise, he also put forth the first complete, yet qualitative, English discussion of Aepinus' magnetic hypothesis. As had many others, he began by noting that the cause of magnetism had "eluded the most accurate researches of very able philosophers."[10] In addition, Cavallo's discussion exhibited concern for methodological matters, particularly the legitimate roles of hypothesis and analogy.

Prefacing his discussion of magnetic theory, Cavallo made a point to distinguish between facts and hypotheses. Facts, or pieces of empirically-acquired evidence, came with great labor and rewarded the researcher's hard work. Hypotheses, on the other hand, were the offspring of the imagination, showed weakness of understanding, and misled those who followed them blindly. Cavallo nevertheless believed that hypotheses could serve important functions in natural philosophy. At the very least, they generated more facts while promoting further experimentation. More significantly, a probable hypothesis, upon closer experimental examination, often became more evident, while a false hypothesis became more absurd. Therefore, after collecting a number of facts, Cavallo contended that it was often useful to propose a tentative hypothesis. Despite their possible utility, he cautioned against becoming too fond of any particular hypothesis "even when it seems to have the greatest degree of probability."[11] Experiment and observation always took precedence over hypothesis, not the other way around.

In such statements, Cavallo and others in the late eighteenth century moved away from a literal interpretation of Newton's *hypotheses non fingo.* Their willingness to openly entertain hypotheses, albeit cautiously, differed greatly from earlier strictures against all hypotheses. For instance, Benjamin Martin had written in 1769:

> The Philosophers of the present Age hold [hypotheses] in vile Esteem, and will hardly admit the Name in their Writings; they think that which depends upon bare Hypothesis and Conjecture, unworthy of the name of Philosophy; and therefore have framed new and more effectual Methods for philosophical Enquiries.[12]

Cavallo's methodological position and the similar approach of other late eighteenth-century natural philosophers diverged from this literal anti-hypothetical stance. In great detail, Scottish Common-Sense philosophers modified and carefully qualified Newton's strictures against hypotheses. As we shall see, they discussed Newtonian methodology to a degree rarely seen earlier in the century.

Examining magnetic hypotheses in his *Treatise on Magnetism*, Cavallo clearly exhibited his dislike of speculative theories. He wrote, "Human imagination, ever ready to supply the deficiency of real knowledge, has offered an abundance of hypotheses; but their insufficiency to explain the various phenomena of magnetism, renders them most improbable, and often evidently absurd."[13] Rejecting theories which proposed perpetually circulating fluids and ferruginous bodies full of valves, Cavallo regarded Aepinus' hypothesis as the most plausible.[14]

Though Cavallo sympathized with Aepinus' work, his admiration remained tempered by a reluctance to accept Aepinus' explanation of why two undercharged magnetic poles repelled each other. This required, he said, either that the matter of ferruginous bodies become self-repulsive when deprived of its magnetic fluid or that the undercharged extremities merely appeared to repel each other "because either of them attracts the opposite overcharged extremities." Both possibilities, he concluded, were "embarrassed with difficulties."[15]

Regardless of these problems, Cavallo favored the general tenets of Aepinus' theory while clearly rejecting Cartesian theories. Patterns of iron filings laid over a magnet, he argued, did not prove the circulation of an external fluid. If effluvia did indeed act on the filings, they all would be driven towards one pole. Instead, he argued, each sliver of iron becoming a tiny magnet arising from its proximity to the larger magnet, resulting in the familiar patterns. Cavallo explained, "Now, when there are many particles of iron near the magnet, those which touch its surface are rendered magnetic; consequently they attract other particles; and these being also rendered magnetic, attract others, and so on, forming strings of small magnets."[16]

Also in general agreement with Aepinus on terrestrial magnetism, Cavallo believed that the earth acted as a magnet. So many observations illustrated this fact that there could "hardly be a philosopher sceptic enough to doubt of its truth."[17] Support for the earth's magnetism, he explained, arose from abundant evidence including the vast masses of magnetic substances excavated from nearly every part of the earth. Arguing that the phenomena of the compass and dipping needle were exactly imitated by a *terrella*, Cavallo wrote that the only exception, i.e., that the earth's magnetic poles did not attract iron like normal magnets, simply awaited more accurate experiments near the poles. Of several theories explaining the changing positions of the earth's magnetic poles, none could reliably predict temporal or geographic variations. Furthermore, the charts of Halley and others illustrated that declination must be a "matter of conjecture or guess" in many places.[18] Supposed regularities fell victim to so many exceptions that charts of variation were of little practical use.

Though supporting the earth's magnetism, Cavallo diverged from Aepinus in rejecting the notion that the earth contained a single magnetic core. Echoing earlier sentiments expressed in his Bakerian lecture, he explained, "the magnetism of the earth arises from the magnetism of all the magnetic substances therein contained, and intermixed with other bodies; that the magnetic poles of the earth may be considered as the centres of the polarities of all the particular aggregates of the magnetic substances."[19] Hence, the magnetic poles changed position as the powers of magnetic material increased or diminished. Assuming four magnetic poles moving on the earth's surface, he examined the possible location of these poles. These poles were not the same as the poles of Halley's model, rather they represented the points of aggregate magnetic action. Citing the work of Edinburgh-trained physician, John Lorimer, Cavallo enumerated four distinct possibilities or cases for the poles' positions:

> Case I: magnetic poles coincide with true poles (Gilbert's position)
>
> Case II: magnetic poles are in same meridian and in opposite parallels
>
> Case III: magnetic poles are in opposite meridians and in opposite parallels
>
> Case IV: magnetic poles in neither the same nor the opposite meridians or parallels.[20]

Observations suggested that the final option was the closest to the truth. Terrestrial magnetism did not divide the earth into two equal magnetic hemispheres, as the other cases indicated. In Case IV, the hypothesized lines of declination formed curves variously inclined to both the equator and the meridian, thus it most closely resembled earlier charting efforts [see Figure 3].

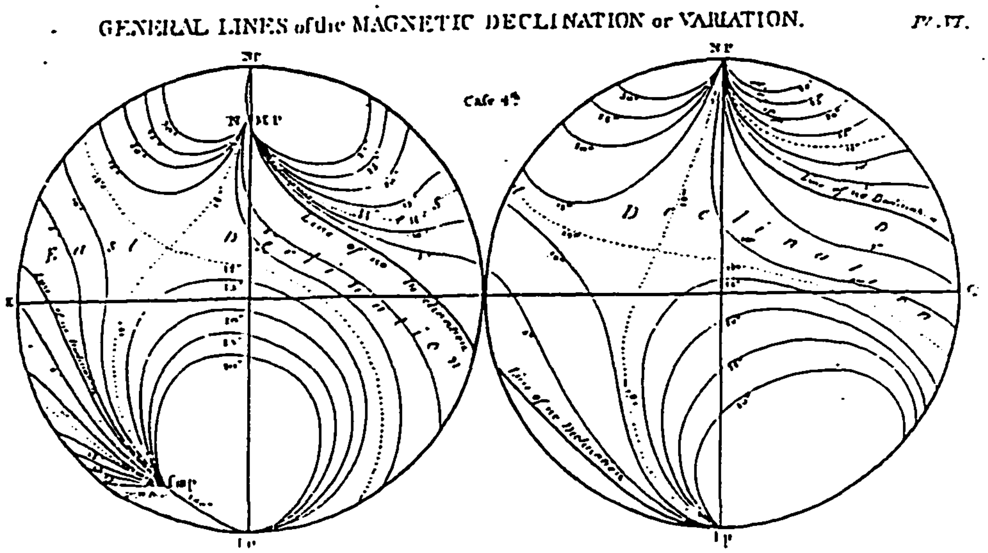

Figure 3. Lorimer's predicted lines of magnetic variation

The inherent irregularity of global magnetic observations dovetailed nicely with Cavallo's skeptical position regarding terrestrial magnetism. Hence, he contended that the myriad factors contributing to changes in terrestrial magnetic phenomena made it impossible to formulate a useful, predictive theory of terrestrial magnetic variations.

Cavallo also discussed the physical location of the poles asserting they lay some depth beneath the surface. This at least held true with respect to the southern magnetic pole as the southern hemisphere contained vast amounts of water incapable of magnetism. Therefore, the magnetic pole was probably at the ocean's bottom. Agreeing with Gilbert, Cavallo considered the irregular distribution of land and sea with respect to magnetic variations. Large bodies of land drew the needle toward them, while oceans did not. Once again he skeptically concluded that it was "impossible to form a useful theory upon it [declination]."[21]

After pointing out the dearth of English works on magnetism, an anonymous reviewer of *A Treatise on Magnetism* called Cavallo's summary of Aepinus "judicious and well connected." The writer further described the one-fluid theory as "very ingenious" and "in a great degree true." The reviewer also agreed that terrestrial magnetism arose "from the conjoined influence of all the magnetic bodies in it."[22] Such a positive response hinted at a slowly growing awareness of Aepinus' ideas. Although Cavallo's treatment remained non-mathematical and largely qualitative, it nonetheless helped make known Aepinus' magnetic theory.

Two later editions of Cavallo's *Treatise* also exposed English readers to the one-fluid theory. With little change in their contents, the later editions continued to include a highly experimental, qualitative discussion of Aepinus' hypothesis. Cavallo commented in 1795 that the supplementary materials did not occasion any alteration of the first edition.[23] In the supplement, he included letters written by Lorimer who agreed with him in many theoretical matters. Like Cavallo, Lorimer claimed that the collective magnetism of the earth arose from "the magnetism of all the ferruginous bodies therein contained" and that the two magnetic poles should therefore be considered as "the centres of the powers of those magnetic substances."[24] In a letter, he encouraged Cavallo to compare the magnetic observations made during different voyages so that a new chart might be published. This would be valuable to navigators and, Lorimer claimed, "might also furnish a material step towards the investigation of this curious and interesting subject."[25] Echoing Cavallo, Lorimer took an extremely cautious stance regarding the use of hypotheses. In *A concise essay on magnetism* published in 1800, he sought to avoid merely hypothetical suppositions, "for we do not pretend to explain the causes of magnetism on any theory, however plausible."[26]

In 1803, Cavallo reiterated his views on magnetism in *The elements of natural or experimental philosophy*. In the third section of this treatise he examined properties of hypothetical substances, such as the imponderable fluids of heat, light, electricity, and magnetism. The existence of these substances, he concluded, had not been satisfactorily proved.[27] Of magnetic theories, Cavallo noted:

> Human ingenuity has contrived abundance of hypotheses in explanation of the wonderful phenomena of magnetism; but the insufficiency of most of them

> renders it useless to state them in this work, excepting however one which was proposed by Aepinus, and which is similar to the Franklinian theory of Electricity.[28]

With respect to terrestrial magnetism, vast masses of iron, found almost everywhere in various states, proved "beyond a doubt that the earth is a vast but irregular magnet, and that its magnetism arises from the magnetism of all the ferruginous bodies that are contained in it."[29] Arising from a plethora of hidden causes the magnetic poles or "collected powers" of all magnetic substances shifted frequently and unpredictably. Such irregular shifts resulted in yearly, daily, and hourly changes in magnetic declination. Again, Cavallo concluded that it was "not and perhaps never will be in our power to determine what part of the effect is due to each of those causes, or what is the precise result of the whole."[30]

While Cavallo did not believe the existence of imponderable fluids had been experimentally verified, he nonetheless favored the hypothesis of imponderables over speculations about nature's unity. Following Aepinus in admitting the close analogy between the electricity and magnetism, he claimed they were "two different powers of nature, which are quite distinct from each other."[31] While some proposed that light, heat, electricity, and magnetism were "the effects of a single fluid; respecting the nature of which, however, various opinions are entertained," Cavallo did not pursue such speculations.[32] Instead, he listed light, caloric, electric fluid, and magnetic fluid as the first four elementary substances. For Cavallo, the failed conjectures of the past had demonstrated

> the necessity of substituting experiments and strict mathematical reasoning to the suggestions of the imagination. . . . The progress of experimental investigations, and the mathematical mode of reasoning, are both slow and laborious; but they are safe and productive of true and useful knowledge; nor has the human being any other means of feeling his way through the dark labyrinth of Nature.[33]

While advocating the use of both experiment and mathematical reasoning, Cavallo remained, first and foremost, an experimentalist.

Though not everyone partook of Cavallo's cautious views, his experimental work and publications had widespread influence on discussions of magnetism in Britain and elsewhere. For example, Cavallo experimentally demonstrated that hammered brass could be made magnetic and that chemical reactions could affect a nearby compass needle. His often-cited *Treatise* appeared not only in two English editions, but was translated into German as well.[34] Numerous encyclopedia articles discussed Cavallo's empirical research. In Abraham Rees' *Cyclopaedia*, completed in 1819, Cavallo's article on magnetism continued to support the Aepinian hypothesis.[35]

Another investigator who embraced the hypothesis of Aepinus was Scottish natural philosopher, John Robison. By skillfully combining the experimental and mathematical traditions, Robison, unlike Cavallo, hinted at the new direction which the study of electricity and magnetism (and British experimental physics in general) would take in the early years of the nineteenth century.

John Robison (1739-1805): Background and Lecture Notes

Robison, the son of a prosperous Glaswegian merchant, entered the University of Glasgow in 1750 and received his M. A. in 1756. While at the university, he studied moral philosophy under famed political economist Adam Smith and mathematics with geometer Robert Simson.[36] At Glasgow, Robison also became acquainted with chemist Joseph Black and mathematical instrument-maker James Watt. His interests turned from clerical work to science, particularly mathematics and mechanics. As he later recalled:

> I had, from my earliest youth, a great relish for the natural sciences, and particularly for Mathematical and Mechanical Philosophy. I was eager to be acquainted with the practice of Astronomical observations, and my wishes were much encouraged by the celebrated Dr. [Robert] Simpson [sic] Professor of Geometry, Dr. [Robert] Dick professor of Nat[ura]l Phil[osoph]y, and Dr. [James] Moor Professor of Greek, gentlemen eminent for their mathematical abilities.[37]

In 1759, Robison left Glasgow and served as a midshipman in the navy for the next four years. During this period he performed surveys of the Canadian coast and privately tutored Admiral Charles Knowles' son in mathematics and navigational science. While serving in North America, he also remarked on the effect which the aurora borealis had upon compass needles, a phenomenon not commonly recognized at the time.[38] Robison's continuing interest in magnetism fit well with his combined navigational and scientific education.

Due to a bout with scurvy, Robison returned home in 1762 intent on resuming the study of theology. However, after recovering from his disorder at Knowles' home, he accompanied Knowles' son on a trip to Portugal, before returning once again to the Admiral's residence. Through the patronage of Knowles, the Board of Longitude appointed Robison to test John Harrison's chronometer during a trip to Jamaica.[39] After this trip, he returned to the University of Glasgow in 1766 to serve as chemistry lecturer, replacing Black.[40] Renewing his ties with Black and Watt, Robison also became friendly with ornithologist Alexander Wilson and moral philosopher Thomas Reid during his four years teaching chemistry at Glasgow.[41]

Leaving Glasgow again in 1770, Robison served as private secretary to Admiral Knowles in St. Petersburg, Russia where Knowles served as the president of the Russian Board of Admiralty. Two years later, he became a mathematics professor and the inspector-general for the corps of marine cadets at Russian naval base of Kronstadt.[42] During his stay in Russia, Robison met Aepinus and became familiar with his experiments on the tourmaline.[43] Upon returning to Scotland in 1774, he took the chair of natural philosophy at University of Edinburgh, a position he retained until his death in 1805. Undated lecture notes, probably written in the late 1770s and early 1780s, recount the evolution of his ideas.[44]

In particular, Robison's lecture notes recall his early notions on magnetic phenomena. As a student at Glasgow (1750-56), Robison considered that each iron filing arranged in the familiar patterns around a magnet became magnetic itself. Of this time, he noted:

> This opinion concerning the arrangement of iron filings first occurred to me in 1755 while studying Natural Philosophy under Dr [Robert] Dick of Glasgow— in 1756, I read a dissertation on the subject in an academical society . . . and at that time I formed the theory which I am now delivering— I long thought it peculiar to myself— in 1771, I got acquainted with Mr Æpinus of Petersburgh— and found that he had published in 1759 a similar theory, from which he had deduced a most ingenious conjecture as to the cause of all the magnetic phenomena— this I shall soon lay before you.[45]

Hence, Robison had not read the *Tentamen* in 1771, when he first met Aepinus and became aware of his book. As Robison's ideas developed, he consistently rejected Cartesian effluvial theories. Of his tenure as chemistry lecturer at Glasgow (1766-70), he reflected:

> I gave an account of the usual mechanical theories [of magnetism] and the insurmountable objections which may be made to them, and took that opportunity of showing that the arrangement of Iron filings, and all the phenomena of magnetism are consequences of the general fact that a piece of iron became magnetic by mere juxtaposition to a Magnet.[46]

Circulating fluid theories contradicted the established laws of motion. Robison, like Aepinus, persistently rejected contact action by impulsion and embraced forces acting at a distance.

Unpublished lecture notes, however, clearly illustrate Robison's initial ignorance of Aepinus' theory. Reading about the electrical theory in a *History of Electricity* (1767), he remembered Priestley's account as so "indistinct" and "uninteresting" that when he later had an opportunity to converse with Aepinus on the subject, he "avoided it, as being likely to lead us into disagreeable discussion for I had always been an enemy to invisible fluids." Despite this wariness, Robison enthusiastically approved of Aepinus' results regarding the tourmaline. He remarked that the resemblance of electric and magnetic phenomena was "very striking," and that no use was made of the fluid mentioned by Priestley.[47] Until his years teaching at Edinburgh had begun, it seems probable that Robison had neither read nor received a copy of Aepinus' *Tentamen*.[48]

Upon returning from Russia in 1774, Robison sought information regarding electricity for his university lectures. He recalled:

> In the month of February 1775, I met with a paper . . . by the Hon. Henry Cavendish, where he endeavoured to give a theory of Electricity. [Cavendish] says that Aepinus' notion of the subject was nearly the same as his. On reading the paper I was struck with the ingenuity and the beauty of the theory of Aepinus which I now know that Dr. Priestley had never understood . . . I could not avoid adopting it, and recommending it in my first course of lectures.[49]

Writing in late 1771, Cavendish had explained that Aepinus' electrical hypothesis was nearly the same as his own independently developed hypothesis:

> Aepinus, in his *Tentamen* . . . has made use of the same, or nearly the same hypothesis that I have; and the conclusions he draws from it, agree nearly with mine, as far as he goes. However, I have carried the theory much farther than he had done, and have considered the subject in a different, and, I flatter myself, in a more accurate manner.[50]

Impressed with Cavendish's approach, Robison embraced the one-fluid electrical theory which merged quantitative experimental results with mathematics. Though recognizing the importance of Aepinus' contribution, it seems that Robison initially had difficulties in obtaining a copy of the *Tentamen*. After reading Cavendish's "beautiful" theory, he remarked: "I have ever since [sought] to procure the Work of Aepinus, but without success. For I longed to see the use which he would probably make of the analogy between electric and magnetic phenomena. Not being able to procure the book, I then [set] about pursuing the analogy myself."[51] Pursuing the analogy between electricity and magnetism independently, Robison eventually obtained a copy of the *Tentamen*. He recalled in his notes:

> I afterward procured Aepinus' Work from abroad and had the satisfaction to find that I had been very successful in my investigation, which, indeed, was not difficult, seeing that the familiar path in the Electric phenomena had been so clearly marked out by Mr. Cavendish. Let us now see how this hypothetical fluid

> will produce a set of phenomena similar to the observed magnetical phenomena.[52]

Therefore, Robison, the "enemy to invisible fluids", explicitly recognized the hypothetical status of Aepinus' fluid. Later in this chapter, we will examine Robison's ambivalent stance toward hypotheses.

Robison's theory paralleled Aepinus' except in one important respect, the formulation of a magnetic force law. Although Aepinus had not been able to determine any constant law relating magnetic power to distance, Robison's experiments and readings led him to conclude that the force of attraction (F) between magnetic fluid particles of iron obeyed an inverse-square relationship according to distance (D) between the particles (i.e., $F \propto 1/D^2$). Bolstering support for his views, he mentioned a "most ingenious" memoir by German Johann Heinrich Lambert which reached the same conclusion .[53] In later work, Robison remained characteristically cautious about asserting the certainty of the inverse-square law.

All magnetic phenomena, Robison proposed, were the same as those which would result from the actions of a hypothetical fluid with three distinct properties. First, the fluid's particles repelled each other with a force inversely proportional to the square of the distance between them. Second, these particles attracted the particles of iron, also according to an inverse-square relationship. Third, the fluid moved with great difficulty among the particles of iron, steel, and lodestone, yet with perfect ease through all other solid bodies. He added that the fluid moved with very great difficulty through common air. Because iron attracted the particles of magnetic fluid, ferruginous materials contained it in great quantities, uniformly diffused throughout their substance. Appealing to unexplained attractions and repulsions, Robison commented:

> If the quantity [of fluid] is so great that the repulsion between its parts is greater than its attraction for the iron, some will flow out and if it is less, some will enter, till the equilibrium is restored, this quantity and this uniform distribution I call the natural quantity and the natural state.[54]

When fluid accumulated in one end of an iron bar, that end became "overcharged" and the other end "undercharged." Hence, in a manner similar to Aepinus, Robison described magnetic phenomena utilizing movements of the magnetic fluid and forces of attraction and repulsion. With the exception of the inverse-square relationship, Robison's assumptions about the magnetic fluid paralleled those of Aepinus.

In addition to his espousal of the one-fluid theory, Robison showed admiration, even reverence, for Gilbert's work. On *De Magnete* he remarked, "It is an excellent piece of investigation and may be looked on as the first fruits of the true philosophy, or the logic of induction."[55] In agreement with the Gilbertian analogy, Robison considered natural magnets as a consequence of a great magnetic nucleus within the earth.[56] He also believed, like many others, that uncovering the mysteries of magnetism had potential benefits. Hence, magnetism had strong claims for further investigation. Robison remarked that the lodestone's relevance to the "noble art of Navigation should recommend [its study] to every man, surely to every Briton." The discovery of the laws of magnetism would also allow further "access to many other mysteries of Nature."[57]

As we have seen, Robison, independently developed notions similar to Aepinus' with respect to magnetism. Cavendish's 1771 paper peaked his interest in Aepinus' *Tentamen.* Like Aepinus and Cavendish, Robison sought to fuse mathematics with experiment. Although these investigators' understanding of electricity and magnetism was mathematical, most British experimental philosophers of the 1760s and 1770s were not trained in mathematics and ignored its applications. In contrast, Robison was known for his knowledge of mathematics, particularly continental mathematics. Attesting to his mathematical prowess, Scottish historian and statesman James Mackintosh called Robison "one of the greatest mathematical philosophers of his age."[58]

In fact, Robison's extensive knowledge of mathematics contributed to the unpopularity of his Edinburgh lectures. In 1815, mathematician John Playfair noted that Robison's extensive mathematical knowledge included considerable familiarity with the work of foreign mathematicians, a rarity for late eighteenth-century Britain. Of Robison's lectures he remarked, "To understand his lectures completely, was, on account of the rapidity, and the uniform flow of his discourse, not a very easy task, even for men tolerably familiar with the subject. On this account, his lectures were less popular than might have been expected."[59] Similarly, Thomas Young, a former student of Robison's, noted, "His lectures were considered by most of his pupils as somewhat too difficult to be followed."[60]

By the early nineteenth century, however, more experimental physicists embraced both experiment and mathematics in their magnetic investigations. Robison's publications, particularly his encyclopedia articles, played a part in this shift in British magnetic studies. This changing situation indicated wider changes in British experimental physics as various topics of study, previously the domain of experiment alone, appealed increasingly to mathematics as well. As a transitional figure, Robison illustrated the shift from the experimental physics of the eighteenth century and a burgeoning interest in continental ideas.

Robison: *Encyclopaedia Britannica* (1797-1801)

Robison had a greater impact than Cavendish on British studies of electricity and magnetism. Unlike Cavendish, Robison was not a recluse; he held teaching positions at Glasgow and Edinburgh, and published much more frequently than Cavendish. At the turn of the century, Robison exposed English readers to the work of Aepinus and other continental natural philosophers in a series of widely-read *Encyclopaedia Britannica* articles. The third edition, which appeared between 1788 and 1797, contained several articles written by Robison. A two-volume supplement, appearing in 1801, included Robison's "Astronomy", "Boscovich", "Dynamics", "Electricity", "Impulsion", "Mechanics", and "Magnetism."[61] Robison also wrote for the *Britannica* on numerous engineering-related topics including "Pumps", "Resistance", "Steam Engine", "Steelyard", "Waterworks", "Arch", "Machinery" and, "Watchwork." At a time when continental mathematics remained scarce in British universities, Robison introduced it and as well as other topics to a wider British audience.

Despite the possibly limited influence of Robison's lectures suggested by Playfair's and Young's remarks, his *Encyclopaedia Britannica* articles introduced the ideas of Aepinus and other continental physicists to a wide audience.[62] Attesting to the importance of these articles, Young commented that they exhibited a "more complete view of the modern improvements in physical science than had ever before seen in the possession of the British public."[63] In 1835, Cambridge polymath William Whewell reported that the Aepinian theory was hardly known in England, "except by name, till the late Prof. Robison gave a view of it, at considerable length, in the article ELECTRICITY in the *Encyclopaedia Britannica*."[64] In 1842, David Brewster more broadly reflected:

> The state of physical science was at a low ebb in England previous to the writings of Robison. The labours of continental philosophers were but little known even to those who occupied the chairs in our universities; and those who had obtained some knowledge of them could impart it to their pupils only. The general student and the ingenious artisan drew their information from its ancient springs, while the finest researches lay concealed in foreign languages, or were confined to a few philosophers. . . . How fortunate, then, was it that the *Encyclopaedia Britannica* held out an ample remuneration for this laborious enterprise, and induced so accomplished a person as Robison to transfer to its pages the noblest researches of modern science![65]

One of the reasons for the general British avoidance or ignorance of continental physical sciences dealt with the different mathematical styles involved. Following Newton and earlier figures, British mathematicians and natural philosophers generally preferred geometrical methods over the algebra and symbolic analysis so highly developed on the continent. This British preference for geometry remained strong, particularly in eighteenth-century Scottish universities. Robison, for instance, saw fit to use continental mathematics, yet remained convinced of the greater level of certainty which the sensory foundations of geometry allowed and its resulting pedagogical effectiveness.[66] In this, Robison exhibited views similar to many eighteenth-century Scottish mathematicians, including his teacher at Glasgow, Robert Simson. In the 1797 *Britannica*, he wrote of Simson's geometry:

> Perspicuity and elegance are more attainable, and more discernible, in pure geometry, than in any other parts of the science of measure . . . [Simson] preferred the ancient method of studying pure geometry, and even felt a dislike to the Cartesian method of substituting symbols for operations of the mind, and still more was he disgusted with the substitution of symbols for the very objects of discussion, for lines, surfaces, solids, and their affections.[67]

Simson, for instance, preferred to convert "the algebraic formula into an analogous geometrical theorem" when solving problems where only quantity was considered.

Though in general agreement with Simson's geometrical preference, Robison believed that Simson had taken an extreme position. In some instances, Simson elevated his geometrical bias to the level of idolatry. In contrast to his teacher, Robison cautiously approved of algebraic methods:

> And there is no denying, that if general unsophisticated taste alone is to be consulted, Dr. Simson was in the right, for though it is must also be acknowledged, that the reasoning in algebra is as strict as in the purest geometry of Euclid or Apollonius, the expert analyst has *little perception* of it as he goes on, and his final equation is not felt by himself as the result of ratiocination, any more that if he had obtained it by Pascal's arithmetical mill. This does not in the least diminish our admiration of the algebraic analysis; for its almost boundless grasp, its rapid and certain procedure, and the delicate metaphysics and great address which may be displayed in conducting it.[68]

Despite his warnings, Robison pointed out that Simson actually knew much about modern symbolic analysis and algebra. In fact at Glasgow, he taught analysis in his upper level lectures with Robison as a pupil. Simson pointed out in these classes the "proper province [of analysis] . . . and in what cases it might be applied with safety and advantage even to questions of pure geometry."[69] While Robison continued to prefer geometry for teaching purposes, he eagerly endorsed continental analysis for furthering areas of natural philosophical inquiry. In 1804, for instance, he commented:

> It is from experience of my own studies that I am induced to prefer this method [geometry]; I am fully aware, however, that its advantages are restricted to mere elementary instruction, and that no very great progress will be made in the

> more recondite parts of physical astronomy without emphasizing the algebraic along with the geometrical analysis.[70]

This view applied not only to physical astronomy, but to areas of experimental physics as well, including electricity and magnetism.

In his *Britannica* articles on magnetism, Robison developed the ideas put forth earlier in the Edinburgh lecture notes. Like the notes, Robison's "Variation of the compass" (1797) supported both Aepinus' theory and Gilbert's analogy.[71] Asserting that Gilbert had performed more magnetic experiments than all before him, Robison claimed that Gilbert's theory "of the magnetical phenomena is now completely confirmed."[72] With this high praise, Robison turned to the proposition upon which he claimed Gilbert had founded his theory— the principle of magnetic induction. Given a bar magnet (NS), a small non-magnetized piece of soft iron (*ns*) supported by a point (*c*) would arrange itself in the position [See Figure 4.].

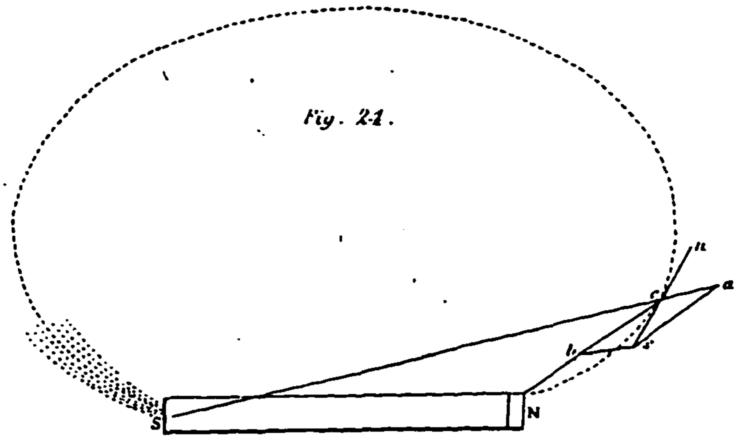

Figure 4. Robison's geometrical magnetic curves

Although only temporarily magnetic, *ns* acted like a magnetic needle of the same size and shape. When turned 180°, *ns* would remain in its newly acquired position, reversing polarity. Furthermore, with a paper held over *ns* with fine iron filings strewn upon it, the filings "arrange[d] themselves into curves issuing from one of its ends and terminating at the other," as if held over a real compass needle. Arguing along the same lines as Cavallo, Robison explained:

> Each fragment becomes a momentary magnet, and arranges itself in the true magnetic direction; and when so arranged, attracts the adjoining fragments, and cooperates with the forces, which also arrange them. We throw this out to the ingenious mechanician as the foundation of a *complete* theory of the magnetical phenomena.[73]

Each iron filing, almost infinitely small when compared with NS, it would take a position tangent to the curve NcS. Robison concluded by extending the inductive principle to the entire earth. In this manner the globe induced magnetism on iron ores in the same way as a lodestone induced magnetism in iron filings. Thus a compass needle arranged itself in every part of the world in the magnetic direction, i.e., tangent to one of the magnetic curves. The giant terrestrial magnet and all smaller magnets remained closely linked by analogy.

Regarding magnetic variation, Robison took a skeptical position like Cavallo's. After reviewing the theories of Halley, Euler and others, he concluded that "for our own part, we have little hopes of this problem ever being subjected to accurate calculation."[74] Differing from Cavallo, however, Robison combined Halley's notion of an internal nucleus with Euler's theory of two magnetic poles. The regular motion of a magnet within the earth produced very irregular motions of the compass needle. Irregularities arose due to localized, magnetic masses on or near the earth's surface. The existence of large deposits of iron ore and extensive outlays of magnetic rocks (e.g., the island of Elba, the island of Cannay west of Scotland, and the Faeroe Islands in the North Sea) further supported this notion. Conceding ignorance of regions below the thin terrestrial crust, he concluded [see Figure 5.]:

> when we see appearances which tally so remarkably with what would be the effects of great masses of magnetical bodies, modifying the general and regularly progressive action of a primitive magnet, whose existence and motion is inconsistent with nothing that we know of this globe, this manner of accounting for the observed change in variation has all the probability that we can desire.[75]

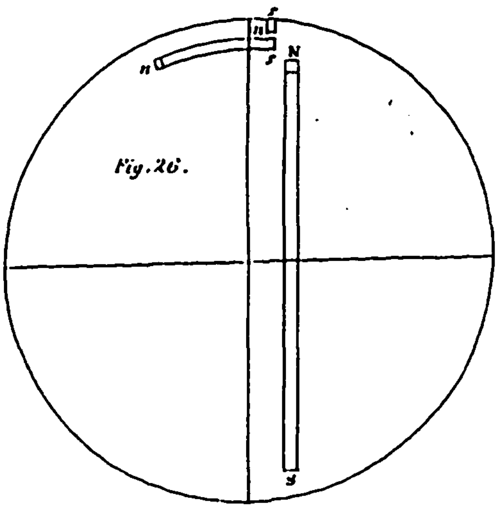

Figure 5. Robison's theory of terrestrial magnetic variation

In this manner, Robison retained the Gilbertian notion of a primitive terrestrial magnet and supported the theory of two magnetic poles moving on or near the surface. He also concluded, as had Cavallo, that irregular magnetic variations could never be accurately calculated because of the continual alterations of ferruginous material within the earth.

Discussing the causes of diurnal variation, Robison criticized the theories of John Canton and Aepinus. Criticizing Canton's conjecture that the solar heat caused diurnal variation, Robison thought it improbable that the sun's heat penetrated deeply enough to modify the primitive terrestrial magnet and create daily fluctuations. Aepinus' alternative theory, although better than Canton's, also failed to convince. Aepinus, explained Robison, supposed "that the sun acts on the earth as a magnet acts on a piece of soft iron, and in the morning propels the [magnetic] fluid in the north-west parts. The needle directs itself to this constipated fluid, and therefore it points to the eastward of the magnetic north in the afternoon."[76] Robison warned that such tiny changes in the fluid's position did not adequately explain the great diversity of diurnal variation at different locations. Claiming that the diurnal variation could not be ascribed to changes in the magnetism of the "primitive terrestrial magnet," Robison concluded that local ferruginous masses were the source of diurnal variation.[77]

In 1801, the supplement to the third edition of *Encyclopaedia Britannica* included Robison's articles, "Electricity" and "Magnetism." These articles illustrated Robison's conviction in Aepinus' theories. In "Electricity," Robison called Aepinus' *Tentamen*, "unquestionably one of the most ingenious and brilliant performances of this century."[78] Surprised that Aepinus remained little known in Britain, he again cited the "very slight and almost unintelligible account" of Joseph Priestley from 1767. He remarked that, "seeing so much algebraic notation in every page, and being at that time a novice in mathematical learning, [Priestley] contented himself with a few scattered paragraphs, which were free of those embarrassments, and thus could only get a very imperfect notion of the system."[79] More mathematically versed than the older experimentalist, Robison gave a complete treatment of what he considered Aepinus' important contributions.[80]

Robison's discussion of magnetism began with a series of questions challenging the Cartesian effluvial theories. If fluids streamed from the poles, how did such streams begin? What impelled these streams to move the way they did? Why did they move in curves and return to the magnet? Directly disputing the internal mechanisms of the theories of Descartes, Euler and others, he remarked:

> As to the explanations, or descriptions, of the canals and their dock gates, opening in one direction, and shutting in the other, constructions that are changed in an instant in a bar of iron, by changing the position of the magnet, *we only wonder that men, who have a reputation to lose, should ever hazard such crude and unmechanical dreams before the public eye.* The mind of man cannot conceive the possibility of their formation . . .[81]

Echoing earlier critiques, Robison concluded that the production, maintenance, and operation of magnetic vortices could not be reconciled with the known laws of impulsion. Widening his criticisms in the article "Impulsion," he claimed that every attempt to explain gravitation, magnetism, electricity, or any apparent action at a distance "by the impulsions of an unseen fluid, are futile in the greatest degree."[82] To understand these phenomena Robison consistently appealed to attractive and repulsive forces.

In "Magnetism" (1801), Robison credited Aepinus with the first "really philosophical attempt to explain all these mysteries."[83] Recounting Aepinus' use of analogy, he wrote:

> Dr. Franklin's theory of the Leyden phial which led [Aepinus] to think that the faculty of producing the electrical phenomena depended on the deficiency as well as the redundancy of this fluid, combined with the phenomena of induced electricity, suggested to Aepinus a very perspicuous method of stating the analogy of the tourmaline and the magnet . . .
>
> Reflecting more deeply on these things, Mr. Aepinus came by degrees to perceive the perfect similarity between all the phenomena of electricity by position and those of magnetism; and this led him to account for them in the same manner.[84]

In his explication of Aepinian theory, Robison issued several caveats regarding the use of hypotheses. Differing from earlier treatments, Robison's discussion illustrated a growing trend among early nineteenth-century natural philosophers to more closely examine scientific methodology, in general, and Newtonian methodology, in particular.[85]

Robison: Common-Sense Philosophy and the Use of Hypotheses

Robison's warnings about the use of hypotheses, as we shall see, had much in common with those of the Common-Sense philosopher, Thomas Reid (1710-1796). Building upon the work of John Locke and David Hume, Reid remained wary of the natural philosopher's quest for causes. Science, he argued, should stress the formulation of descriptive laws not explanatory principles.[86] He wrote in 1780 "natural philosophers may search after the cause of a law of nature; but this means no more than searching for a more general law, which includes that particular law, and perhaps many others under it."[87] Championing Francis Bacon's inductive method and Isaac Newton's maxim *hypotheses non fingo*, Reid took a strong anti-hypothetical position.[88] In *Essays on the Intellectual Powers of Man* (1785), he remarked that scientific discoveries had always relied upon observations and experiments,

> or by conclusions drawn by strict reasoning from observations and experiments, and such discoveries have always tended to refute, but not to confirm, the theories and hypotheses which ingenious men have invented.
>
> As this is a fact confirmed by the history of philosophy in all past ages, it ought to have taught man, long ago, to treat with contempt *hypotheses in every branch of philosophy, and to despair of ever advancing real knowledge in that way.*[89]

Hence, the surest route to natural knowledge emerged from careful experiment and observation, not hypothetical reasoning. Reid considered the goal of natural philosophy as collecting particular facts, "by just induction, the laws that are general, and from these the more general, as far as we can go."[90] In many ways, Robison's writings hinted at the influence of Reid, particularly in his penchant for induction, the classification of facts, and the search for general principles.

Most Scottish thinkers followed Reid in shunning the natural philosopher's search for true causes. Unlike Reid, however, Robison and others were less adamant about the outright rejection of hypotheses. Contrary to Newton's *hypotheses non fingo*, one of Reid's opponents, English physician David Hartley, argued that hypotheses played a vital role in scientific development. Commenting on Newton's ether in 1749, Hartley noted that "any Hypothesis that has so much Plausibility, as to explain a considerable Number of Facts, helps us to digest these Facts in proper Order, to bring new ones to Light, and to make *Experimenta Crucis* for the sake of future Inquirers."[91] Parting company with Reid's position, several Common-Sense philosophers agreed with Hartley. They contended that adopting plausible hypotheses could simplify scientific concepts by relating diverse phenomena to one general class. Furthermore, in the absence of complete inductive information, hypothesis or analogy might be the sole available guides to further inquiry.[92] Hence, the strict inductivism espoused by Reid was not fruitful.

Reid's pupil, Dugald Stewart, the moral philosophy professor at Edinburgh from 1785 to 1820, reserved an interpretive, creative role for the scientist, not merely a passive, receptive one. Countering Reid's anti-hypothetical position, Stewart argued for the cautious use of hypotheses. When comparing hypothesis with experiment, "the cautious inquirer is gradually led, either to correct [the hypothesis] in such a manner as to reconcile it with facts, or finally abandon it as an unfounded conjecture."[93] Stewart contended that judicious hypothetical reasoning led to important discoveries. The Copernican system and Newton's theory of gravitation, for instance, had both began as hypotheses. The utility of hypotheses, he noted, applied to failures as well as successes: "Indeed it has probably been in this way that most discoveries have been made; for although a knowledge of facts must be prior to the formation of a legitimate theory, yet a hypothetical theory is generally the best guide to the knowledge of connected and of useful facts."[94]

Thomas Brown taught moral philosophy at Edinburgh in Stewart's stead from 1810 to 1820. He too supported the usefulness of hypothetical reasoning. Without a guiding hypothesis, Brown noted, the number of possible experiments and observations was virtually infinite. He contended, "To make experiments at random, is not to philosophize; it becomes philosophy, only when the experiments are made with a certain view; and to make them, with any particular view, is to suppose the presence of something, the operation of which they will tend to either prove or disprove."[95] During the first stages of inquiry, hypotheses, far from being "inconsistent with sound philosophy, may be said to be essential to it."[96] He and many other early nineteenth-century thinkers openly urged the cautious use of hypotheses founded upon experiment and observation. Summarizing, Brown concluded:

> we should use hypotheses to suggest and direct inquiry, not to terminate or supersede it; and that, in theorizing . . . we should not form any general proposition, till after as wide an induction as it is possible for us to make . . .

> [we] should never content ourselves, in any new circumstances, with the mere probability, however high, which this application of [a general law] affords; while it is possible for us to verify, or disprove it, by actual experiment.[97]

Robison, along with many nineteenth-century British physicists, embraced positions similar to those of Reid, Stewart, and Brown, emerging from the school of Common-Sense philosophy.[98]

Both philosophers and scientists contributed to the development of Robison's methodological and epistemological positions. Influenced by natural and moral philosophers, Robison's methodology falls squarely within the cautious, inductivist Scottish tradition. Although Robison was acquainted with Adam Smith and Thomas Reid at Glasgow, his friendship with Joseph Black had perhaps a more immediate impact on his methodological proclivities. As editor of Black's chemical lectures, Robison recalled in 1803:

> [Black] pressed on me the necessity of improving in mathematical knowledge, and gave me Newton's *Opticks* to read, advising me to make that book the model of all my studies, and to reject, even without examination, every hypothetical explanation, as a mere waste of time and ingenuity.[99]

Crediting this advice with any abilities he possessed for scientific achievement, Robison concluded that Black had set him on a path which, "I fear I should never have chosen for myself."[100]

Like Reid and Black, Robison often took a vehemently anti-hypothetical stance, particularly with regard to invisible effluvia acting by impulsion. In keeping with Black's eschewal of hypothetical entities, Robison, as we have seen, called himself an "enemy to invisible fluids" in his lecture notes.[101] In 1797, the lengthy revision of the *Britannica* article "Optics," reiterated his distaste for such fluids. Explaining that many philosophers had resorted to aethers and other fluid atmospheres to avoid the difficulties of action at a distance, Robison noted:

> We now see that this is only putting the difficulty a step further off. We may here add, that in all these attempts the very thing is supposed which the philosophers wish to avoid. These aethers have been fitted for their tasks by

> supposing them of variable densities. It is quite easy to show, that such a variation in density cannot be conceived without supposing the particles to act on particles not in contact with them, and to a distance as great as that to which the change of density extends. . . . To get rid of one action at a distance, therefore, we introduce millions.[102]

Criticizing Newton for his speculative ether, Robison wondered how such an eminent figure had deviated from the "path of logical investigation" and transgressed "*all* the rules of philosophizing which he had prescribed to himself and others." He further cautioned of Newton's mistake: "Let this slip, this mark of frail mortality, put us on our guard, lest we also be seduced by the specious offers of explanation which are held out to us by means of invisible atmospheres of every kind."[103] Similarly, Robison warned in the article "Philosophy," co-written with *Britannica* editor George Gleig, that many modern philosophers were "not yet cured of the disease of hypothetical systematizing." In fact, many writings retained ethers, nervous fluids, animal spirits, vortices, vibrations, and other invisible agents. These attempts, Robison concluded, could all be shown either "unintelligible, fruitless, or false."[104]

Given such general strictures against invisible fluids, Robison, not surprisingly, approached the use of electric and magnetic fluids with tremendous caution. In 1801, he wrote of the electric fluid, "[we] cannot be too cautious on what grounds we admit invisible agents to perform the operations of Nature."[105] Disparaging magnetic effluvia in his "Magnetism" article, he recommended that Gowin Knight's 1748 essay should be read "by all those who have recourse without scruple to the agency of invisible fluids, when they are tired of patient thinking."[106] After noting that Aepinus' magnetic fluid was unobservable, Robison warned that the one-fluid hypothesis gave "no extension of knowledge; for it can have no greater extension than the phenomena on which it is founded." In other words, Aepinus' hypothesis, founded upon the phenomena themselves, was incapable of predicting new phenomena. It could not, "without risk of error, be applied to an untried case, of a kind dissimilar in its nature" to the phenomena upon which it was founded.[107] Continuing, he asserted that his explanation of magnetic phenomena remained independent of "the hypothesis of Aepinus, or any hypothesis whatever."[108] Based upon these comments, Robison seemingly rejected Aepinus due to his distaste for invisible fluids and the one-fluid hypothesis' lack of predictive value. This interpretation, however, distorts and oversimplifies Robison's stance on Aepinian theory.

Agreeing with Reid, Robison argued that natural philosophy did not seek to discover causes, but to find descriptive laws by induction. In the 1797 *Britannica* article, "Philosophy," he warned of the dangers of assuming cause and effect relationships. Concurring with the anti-hypothetical stance of Reid and Black, Robison remarked:

> All hypotheses therefore must be banished from philosophical discussion as frivolous and useless, administering to vanity alone. As the explanation of any appearance is nothing but the pointing out the general fact, of which this is a particular instance, a hypothesis can give no explanation: knowing nothing of cause and effect but the conjunction of two events, we see nothing of causation where one of the events is hypothetical. Although all the legitimate consequences of a hypothetical principle should be perfectly similar to the phenomenon, it is extremely dangerous to assume this principle as the real cause.[109]

Therefore, rather than causes, the philosopher should seek to discover physical laws which described the phenomena. To do this the natural philosopher must carefully and accurately describe the events of nature and group them into classes:

> By gradually throwing out more circumstances of resemblance, he renders his classes more extensive; and, by carefully marking those circumstances in which the resemblance is observed, he characterizes all the different classes; and by a comparison of these which each other . . . he distributes his classes according to their generality and subordination; thus exhausting the whole assemblage, and leaving nothing unarranged but accidental varieties.[110]

However, diverging from the anti-hypothetical strictures of Newton, Reid, and Black, Robison saw a useful, important role for hypotheses in science. In the very same article, he noted that hypotheses could be admitted into experimental philosophy, not as explanations, but as "conjectures serving to direct our line of experiments."[111]

Impressed with the writings Aepinus, Cavendish, and Boscovich, Robison also exhibited affinities with their methodological pronouncements. In 1759, Aepinus had cautioned:

> I am fully aware that it cannot be certainly concluded from the agreement of an hypothesis with the phenomena that we have reached the true cause. Although the theory I propound here satisfies the majority of magnetic phenomena, I prefer to proceed more modestly than confidently, and to put forward my proposition as probable rather than as certain.[112]

Although wary of hypotheses, Aepinus, like Robison, allowed them a position in his science. Insisting that hypotheses must be founded upon experimental evidence, Aepinus described his own theoretical insight (i.e., the analogy between electricity and magnetism) as an "hypothesis."[113]

Similarly, Robison admired the work of Henry Cavendish and R. J. Boscovich, and was perhaps influenced by their acceptance of hypotheses. For instance, Cavendish wrote in his paper on electrical theory of 1771:

> The method I propose to follow is, first, to lay down the hypothesis; next, to examine by strict mathematical reasoning, or at least, as strict reasoning as the nature of the subject will admit of . . . and lastly, to examine how far these consequences agree with such experiments as have yet been made on this subject.[114]

After reading Cavendish's paper four years later, Robison followed much the same method regarding his hypothetical magnetic fluid. Respecting the use of hypotheses, his position also resembled that of R. J. Boscovich. In 1760, Boscovich remarked:

> In some instances, observations and experiments at once reveal to us all that we wish to know. In other cases, we avail ourselves of the aid of hypotheses; —by which word, however, is to be understood, not fictions altogether arbitrary, but suppositions conformable to experience or to analogy. By means of these, we are enabled to supply the defects of our data, and to conjecture or divine the path to truth; always ready to abandon our hypothesis, when found to involve consequences inconsistent with fact. And, indeed, in most cases, I consider this to be the method best adapted to physics.[115]

Hence, Robison's cautious acceptance of hypotheses arose from his Scottish educational experiences and his readings of non-Scottish scientists as well.

Robison: Magnetism and Methodology

Robison's intellectual debts and methodological proclivities clearly emerged in his writings, particularly in discussions of magnetic theory. Recognizing that the immediate causes of magnetic, electrical, and optical phenomena were unobservable, he remarked at the beginning of his notes on magnetism:

> In this case all that we can do is to class the phenomena in the most distinct manner according to their generality. . . . We may take it for granted that those which are most general are the nearest allied to the general cause— But further, by [this] means we get a theory of all the more particular phenomena . . .[116]

Once this process had reached the greatest generality, "we shall be able with great probability or even certainty to assign the cause even of this general fact, that is to point out the law of nature in which it is included."[117] In the case of magnetism, the "most general facts . . . of which all the rest are particular cases" were 1) attraction and 2) polarity. Later in his notes, Robison remarked, "I do not . . . presume to say that what I have now to offer points out the cause of these phenomena. All I have pretended to is to point out a very simple and perspicuous manner of arranging and generating all the appearances."[118] Thus, Robison saw the physicist's job as classifying facts into increasingly general categories which would eventually encompass all the empirically-generated particulars.

In addition to stressing induction, Robison, like Aepinus, Boscovich and the Common-Sense philosophers, reserved a place for the legitimate use of hypotheses while remaining mindful of their careless or casual acceptance. Despite misgivings, he considered that Aepinus' one-fluid hypothesis greatly aided the imagination in conceiving magnetic phenomena. He remarked that, regardless of the fate of Aepinus' electrical theory, Aepinus deserved praise for "his classification of the facts, and his precise determination of the mechanical phenomena to be expected from any proposed situation and condition."[119] Robison took the same position with respect to attractive and repulsive forces. Magnetic action at a distance remained a useful yet ultimately inexplicable concept:

> We offer no explanation of this attraction, more than of the attraction of gravity. There is nothing contrary to the laws of human intellect, nothing inconsistent with the rules of reasoning, in saying, that things are so constituted, that when particles are together, they separate, although we are ignorant of the immediate cause of their separation.[120]

In contrast to action-at-a-distance explanations, effluvial theories appealed to the impulsion of particles. These, argued Robison, were "illogical" and "absurd" because the fluid traveling around the magnet required another fluid to impel it. This fluid required another fluid conducting its motion "and so on without end."[121] Echoing his lecture notes, Robison asserted in 1801 that distinguishable facts, attained by observation, could be reduced to a series of increasingly general classes of facts. Hence, the modification of a particular fact allowed it to fit within a class of more general facts. For example, magnetic forces illustrated a particular case of the general facts of attraction and repulsion.

This type of reasoning led Robison to seek out broad general principles in nature. Agreeing with Reid and other Scots, he believed that science aimed to reduce knowledge to a few general laws. Newtonian gravitational theory gave a perfect example of this type of reduction. While separating phenomena according to distinct imponderable fluids, Robison nevertheless remained open to the possibilities of future unification. He speculated that electricity and magnetism might be related by some unknown powers. More broadly, he commented:

> There is no doubt now among naturalists about the mechanical connexion of the phenomena of nature; and all are agreed that the chemical actions of the particles of matter are perfectly like in kind to the action of gravitating bodies; that all these phenomena are the effects of forces like those which we call attractions and repulsions, and which we observe in magnets and electrified bodies; that light is refracted by forces of the same kind.[122]

Recommending to his readers Aepinus' *Tentamen*, he discussed the analogy between electricity and magnetism. In this, the close analogy between magnetic phenomena and those of induced electricity became apparent, particularly regarding attractions and repulsions. Paradoxically, Robison agreed with Aepinus that the electric and magnetic fluid were "*totally different*, although their mechanical actions are so like that there is hardly a phenomenon in the one which has not an exact counterpart in the other."[123] Like his statements regarding hypotheses, Robison's statements about the unification and distinctness of natural powers often seemed at cross purposes. One the one hand, Robison hoped for general laws or principles. On the other, he treated imponderable fluids as distinct, unconnected entities and remained extremely wary of hasty hypothetical reasoning. As such, Robison's work illustrated broader tensions within Scottish natural philosophy.

Published posthumously, in 1822, in the revision of his 1801 magnetism article Robison seemed more convinced of the usefulness, even the possible truth, of Aepinus' magnetic hypothesis. An extensive comparison of the one-fluid hypothesis with a vast number of experimental facts tallied "precisely with the induction of magnetism."[124] Recounting the total agreement of hypothesis with observation, he assuredly noted, "The coincidence is indeed so complete, that it seems hardly possible to refuse granting that nature operates in this or some very similar manner."[125] Although the existence of a magnetic fluid had still never been proven, the great amount of indirect evidence in its favor suggested to the prudent Robison its probable reality.

John Playfair (1748-1819): Robison's Successor

At the conclusion of "Magnetism" (1801), Robison enlisted the aid of Edinburgh professor of mathematics John Playfair. In a brief mathematical appendix, Playfair elaborated the geometry of the magnetic curves proposed by Robison. The appendix not only demonstrated the growing presence of mathematics in the treatment of magnetic phenomena, but also the Scottish predilection for geometric expression. An excerpt hints at Playfair's geometrical approach:

> If the magnetical force be inversely as the square of the distance, that is, if $m = 2$ $\cos\varphi + \cos\psi$ is equal to a constant quantity. Hence if, beside the points A and B any other point be given in the curve, the whole may by described. For instance, let the point E (Plate IV. fig. 22) be given in the curve, and in the line DE which bisects AB at right angles. Describe from the centre A a circle through E, viz. QER; then AD being the cosine of DAE to the radius AE . . .[126]

Beyond his fondness for geometry, Playfair, Robison's successor in the natural philosophy chair at Edinburgh (1805-1819), also lectured on continental mathematics and experimental physics. With this in mind, he more than likely included Aepinus' theories of electricity and magnetism in his natural philosophy lectures.

In addition to elements of instructional continuity, both Robison and Playfair espoused similarly Scottish views on mathematics, induction, causation, and hypotheses. Educated at the University of St. Andrews, Playfair moved to Edinburgh in 1769 where he continued his studies privately. From 1785 to 1805, Playfair taught mathematics at the University of Edinburgh, part of that time spent as an assistant to former natural and moral philosophy professor, Adam Ferguson (1723-1816). Playfair had close professional connections with Robison, Ferguson, and Dugald Stewart, who taught moral philosophy at Edinburgh from 1785 to 1820.[127]

In the Scottish tradition, Playfair's mathematical work indicated his geometrical proclivities. Praising the inventive and elegant genius of ancient Greek geometry, he wrote of Euclid:

> The elementary truths of that science were connected by Euclid into one great chain . . . the whole digested into such admirable order, and explained with such clearness and precision, that no similar work of superior excellence has appeared, even in the present advanced state of mathematical science.[128]

Playfair, however, did not idolize the use of geometry. Visiting Perthshire in 1774, he witnessed gravitational experiments by English astronomer Nevil Maskelyne. Though the two became close friends, Playfair later complained:

> [Maskelyne] is very much attached to the study of geometry, and I am not sure that he is very deeply versed in the late discoveries of the foreign algebraists. Indeed, this seems to be somewhat the case with all the English mathematicians:

> they despise their brethren on the continent, and think that everything great in science must be for ever confined to the country that produced Sir Isaac Newton.[129]

Remarking on the development of differential equations, Playfair lamented the continuing gap between the efforts of continental mathematicians and those in Britain. British admiration for Newton, combined with a dislike of his rivals (e. g., Leibniz), Playfair noted, had produced and perpetuated this gap in mathematical development. Diversity of notation between the continent and Britain merely exacerbated the divisions. Arising from Britain's perceived lag, Playfair embraced continental analytical mathematics. For instance, his popular edition of Euclid's *Elements*, first published in 1795, used algebraic signs rather than words to more clearly and compactly present proportions.[130] In 1816, Playfair lauded analytical mathematics as "the most philosophical and refined art which men have yet employed for the expression of their thoughts."[131]

Playfair's interest in continental mathematics was closely linked to his interest in continental physics. He considered Britain's lagging stature in physics intimately connected with its stunted mathematical development. With respect to techniques of integrating differential equations, Playfair remarked:

> In this, our countrymen has fallen considerably behind . . . and the distance between them and their brethren on the Continent continued to increase, just in proportion to the number and importance of questions, physical and mathematical, which were found to depend on these integrations.[132]

His concern for mathematical methods, both geometrical and analytical, coalesced with his views on scientific method as well.

Like his fellow Scots, Playfair praised careful induction based upon experiment and observation. In *Outlines of Natural Philosophy* (1814) he explained, "It is from induction that all certain and accurate knowledge of the laws of nature is derived."[133] A few years later, Playfair discussed the Baconian inductive method extensively in his *Dissertation: Exhibiting a General View of the Progress of Mathematical and Physical*

Science, since the Revival of Letters in Europe (1816). As an exemplar of Bacon's method, Playfair held Gilbert's *De Magnete* in high esteem, remarking that Gilbert had carried on experimental philosophy "with more correctness, and more enlarged views," than any of his predecessors.[134]

Nonetheless, Playfair believed that diverging from a strict Baconian route had led to many of science's greatest successes. Referring to Newton as an example, he stressed the importance of mathematical methods and quantification. Furthermore, Playfair criticized Bacon's method, arguing that Bacon had not given sufficient importance to the quantification of physical quantities. He also complained that Bacon had not encouraged the application of geometry. Playfair contended that conclusions deduced by mathematical reasoning were often as certain as inductively-reached principles. Noting the frequent necessity of geometrical methods in completing the inductive process, he explained:

> it appears, that, after experiment has done its utmost, geometry must be applied before the business of induction can be completed. This can only happen when the experiments afford accurate measures of the quantities concerned . . . and this advantage of admitting generalization with so much certainty is one of their properties, of which it does not appear that even Bacon himself was aware.[135]

Playfair's appeals to combine mathematical reasoning and experimental induction paralleled those of other Scottish scientists, particularly Robison.

Again in the Scottish tradition, Playfair took a cautious stance to the search for general principles and the use of hypotheses. Though the existence of unifying principle connecting the actions of impulsion, cohesion, elasticity, chemical affinity, crystallization, heat, light, magnetism, electricity, galvanism, and gravitation seemed "highly probable," Playfair concluded that its discovery was "an honour reserved for a future age."[136] Like his immediate predecessor, he too rejected Newton's ether: "Notwithstanding the highest respect for the author of these conjectures, I cannot find any thing like a satisfactory explanation of gravity in the existence of this elastic ether."[137] Nevertheless, while seeking to unite mathematics and experiment in interpreting nature, Playfair did cautiously entertain certain hypotheses.

One of the hypotheses of which Playfair tentatively approved was that of imponderable fluids. In his view the hierarchy of natural philosophy included mechanics, astronomy, optics, and the laws of "unknown substances, if, indeed, they may be called substances, —Heat, Electricity, and Magnetism."[138] These last three subjects supposed substances which agreed in several particular instances; each could permeate all other substances and each received motion, without taking away any from the body which communicated the motion. Playfair concluded that heat, magnetism, and electricity might be denominated impalpable substances. If they had "any gravity, it cannot be appreciated." Similar to Robison, he cautioned, "We know, indeed, nothing of them but as powers, transferrable from one body to another; and it is in consequence of this last circumstance alone that they are entitled to the name of substances."[139] More importantly, these substances allowed the reduction of their respective phenomena to several general facts. Although a degree of mystery hung over such imponderables, Playfair explained:

> light, electricity, magnetism, elasticity, gravity, are all in the same circumstances; and the only advance that philosophy has made toward the discovery of the essences of these qualities or substances is, by exploding some theories, rather than by establishing any, —so true Bacon's maxim, that the first steps in philosophy necessarily consist in negative propositions. *Besides this, in all the above instances the laws of action have been ascertained; the phenomena have been reduced to a few general facts,* and in some cases, as in that of gravity, to one only; and for ought that yet appears, this is the highest point which our science is destined to reach.[140]

In this reductive effort for general facts and laws, Playfair pursued goals similar to other Scottish men of science. In the Scottish tradition, he also rejected the natural philosopher's quest for true causes. Of this, he noted, "Appearances ought to be described in terms which involve no opinion with respect to their causes."[141]

In all of these ways, Playfair's views paralleled Robison's. Each utilized continental mathematics in their research, yet often preferred geometrical methods in their teaching. Both men praised Baconian induction from experiments, bolstered by the application of mathematical methods and quantification. Both favored the cautious formulation of descriptive laws, while rejecting the search of ultimate principles or causes. Finally, both men cautiously accepted useful hypotheses such as imponderables which reduced phenomena to a few general facts. In sum, both Robison and Playfair fit within the Scottish methodological tradition.

Given these similarities and the personal connection between the two, Playfair's support for Aepinus' theories is not surprising. Echoing Robison's earlier judgments, Playfair, in 1824, called the *Tentamen*, "the first systematic and successful attempt to apply mathematical reasoning to the subjects of electricity and magnetism."[142] Emphasizing the use of analogy in his own work, Playfair noted that Aepinus had first seen "the affinity between electricity and magnetism, in its fullest extent, and perceived the light that these two mutually cast on one another."[143] Though critical of several points, he concluded that most of Aepinian theory could be easily accommodated to the supposition of two elastic fluids. In this, Playfair hinted at the influence of another continental theory, the two-fluid theory developed by French mathematical physicist, Charles Augustin Coulomb. Coulomb's magnetic research and its impact is the subject of the next chapter.

Conclusion

Despite many unsolved mysteries regarding terrestrial magnetism, the study of magnetism by the 1810s was becoming more successful at combining quantitative experimental results with mathematical calculations. While terrestrial magnetism continued to confound, the basic laws of magnetic phenomena were being reduced to the motions of either one or two hypothetical magnetic fluids which could be measured indirectly and treated mathematically. Regardless of specific theories, by the late 1810s, the widely accepted approach to magnetism as well as other areas of experimental physics involved a combination of measurement, mathematics, and experiment. A generation earlier, this had not been the case as most experimentalists (e.g., Wilson, Adams, and Cavallo) ignored mathematics and advanced qualitative theories. Scottish physicists (e.g., Robison and Playfair) and French physicists (e.g., Coulomb and Biot) played significant roles in initiating this transformation in the understanding of magnetism. French magnetic theory, within the context of broad changes in French physics, is the starting point for the next chapter.

Notes

[1] See Charles Chree, "Cavendish's Magnetic Work," in *The Scientific Papers of the Honourable Henry Cavendish, F. R. S.* , edited by Sir Edward Thorpe (Cambridge: Cambridge University Press, 1921), volume II, Chemical and Dynamical: 438-492.

[2] In a similar manner, Robert Kargon categorized different groups interested in science in Victorian Manchester. Kargon considers that the gentleman-amateur who pursued science as polite learning or a fashionable pursuit became increasingly challenged by the devotee who took science to be a serious intense pursuit, but did not make a living at it. See Robert Kargon, *Science in Victorian Manchester: Enterprise and Expertise*, 1977.

[3] Richard Olson, *Scottish Philosophy and British Physics, 1750-1880*, (Princeton: Princeton University Press, 1975), 8. See also Laurens Laudan, "Theories of Scientific Method from Plato to Mach," *History of Science*, 7 (1968), 24-32.

[4] See Richard Olson, *Scottish Philosophy and British Physics, 1750-1880*, (Princeton: Princeton University Press, 1975); Crosbie Smith, "'Mechanical Philosophy' and the Emergence of Physics in Britain: 1800-1850," *Annals of Science*, 33 (1976), 3-29; L. L. Laudan, "Thomas Reid and the Newtonian Turn of British Methodological Thought," in *The Methodological Heritage of Newton*, edited Robert E. Butts and John W. Davis (Toronto: Toronto University Press, 1970), 103-131; G. N. Cantor, "Henry Brougham and the Scottish Methodological Tradition," *Studies in the History and Philosophy of Science*, 2 (1971), 69-89; and G. N. Cantor, "The Reception of the Wave Theory of Light in Britain: A Case Study Illustrating the Role of Methodology in Scientific Debate," *Historical Studies in the Physical Sciences*, 6 (1975), 109-132.

[5] See Thomas Young, "Cavallo, (Tiberius)," *Supplement to the Fourth, Fifth, and Sixth Editions of the Encyclopaedia Britannica* (Edinburgh: Printed for Archibald Constable and Company, 1824), 2: 663-666 and John L. Heilbron, "Cavallo, Tiberius (1749-1809)," *DSB*, 3:153-154.

[6] Tiberius Cavallo, *A complete treatise of electricity in theory and practice: with original experiments* (London: Printed for E. and C. Dilly, 1777) [Landmarks of science microform, 1969], 117-118.

[7] Tiberius Cavallo, Bakerian Lecture: "Magnetical Experiments and Observations," *Philosophical Transactions of the Royal Society of London*, 77 (1787), 17-21. See also Tiberius Cavallo, Bakerian Lecture: "Magnetical Experiments and Observations," *Philosophical Transactions of the Royal Society of London*, 76 (1786), 62-80.

[8] Ibid., 22-23 (my emphasis).

[9] Ibid., 24.

[10] Tiberius Cavallo, *A treatise on magnetism, in theory and practice: with original experiments* (London: Printed for the Author, 1787), 105.

[11] Ibid., 106.

[12] Benjamin Martin, *Philosophical Grammar*, Seventh edition (London, 1769), 19, quoted in L. L. Laudan, "Thomas Reid and the Newtonian Turn of British Methodological Thought," in *The Methodological Heritage of Newton*, edited Robert E. Butts and John W. Davis (Toronto: Toronto University Press, 1970), 117.

[13] Tiberius Cavallo, *A treatise on magnetism, in theory and practice: with original experiments* (London: Printed for the Author, 1787), 132-133.

[14] Ibid.

[15] Ibid., 136-137.

[16] Ibid., 193-195.

[17] Ibid., 107-108.

[18] Ibid., 114.

[19] Ibid.

[20] Ibid., 117-123. See also John Lorimer, *A concise essay on magnetism : with an account of the declination and inclination of the magnetic needle, and an attempt to ascertain the cause of the variation thereof* (London: Printed by J. Dillon, 1800, second edition).

[21] Ibid., 126.

[22] "Review of *A Treatise on Magnetism, in Theory and Practice, with Original Experiments by* Tiberius Cavallo, F. R. S." *The Critical Review: or, Annals of Literature*, series 1, 64 (August 1787), 102-104.

[23] Tiberius Cavallo, *A treatise on magnetism, in theory and practice: with original experiments*, the second edition, with a supplement (London: Printed for the author and sold by C. Dilly . . ., 1795), preface to supplement.

[24] John Lorimer, quoted from Tiberius Cavallo, *A treatise on magnetism, in theory and practice: with original experiments*, third edition, with a supplement (London: Printed for J. Dillon, 1800), 273.

[25] Ibid, 262.

[26] John Lorimer, *A concise essay on magnetism: with an account of the declination and inclination of the magnetic needle, and an attempt to ascertain the cause of the variation thereof*, The second edition, with corrections (London: Printed by J. Dillon . . ., 1800), 51.

[27] Tiberius Cavallo, *The elements of natural or experimental philosophy*, (London: printed by L. Hansard for T. Cadell and W. Davies, 1803) [Landmarks of science microform, 1968], vol. I: 2-3.

[28] Ibid., part III: section 220.

[29] Ibid., vol III: 555. Cavallo repeated the suggestion that irregularities in magnetic declination arose from the effects of heat, cold, decomposition, mixture, volcanoes, earthquakes and other mechanical derangements which changed the constitution of magnetic masses within the earth.

[30] Ibid., 556.

[31] Tiberius Cavallo, *A treatise on magnetism, in theory and practice: with original experiments* (London: Printed for the Author, 1787), 127.

[32] Tiberius Cavallo, *The elements of natural or experimental philosophy* (London: printed by L. Hansard for T. Cadell and W. Davies, 1803) [Landmarks of science microform, 1968], 16-17.

[33] Tiberius Cavallo, *The elements of natural or experimental philosophy*, (London: printed by L. Hansard for T. Cadell and W. Davies, 1803) [Landmarks of science microform, 1968], preface, vol. I: vi-vii.

[34] A German translation appeared in 1788. See Tiberius Cavallo, *Theoretische und praktische Abhandlung der Lehre vom Magnet: mit eignen Versuchen* (Leipzig: Im Schwickertschen Verlage, 1788).

[35] Tiberius Cavallo, "Magnetism," *The Cyclopaedia; or, Universal Dictionary of Arts, Sciences, and Literature* (London: Longman, Hurst, Rees, Orme, and Brown, 1819), XXII: n. p. Cavallo mentioned the magnetic work of Tobias Mayer, Johann Lambert, Charles Augustin Coulomb, and John Robison, yet only remarked that their inquiries had met with great theoretical and experimental success.

[36] Thomas Young, "No. LXXXIV. Life of Robison," *Miscellaneous Works of the late Thomas Young . . .* (London: John Murray, 1855) [NY: Johnson Reprint Corp., 1972], vol II: 505. Simson (1687-1768) served as mathematics professor at Glasgow from 1711 to 1761. He was acquainted with Edmond Halley and, like Halley, showed a predilection for ancient Greek geometry. Simson published on conic sections and editions of Apollonius and Euclid. He probably passed on his love of geometry and aversion to algebra to many of his students. See Robert Chambers, "Simson, (Dr.) Robert," *A Biographical Dictionary of Eminent Scotsmen*, (Glasgow: Blackie & Son, 1835), IV: 239-243.

[37] Robison to Watt, written in 1796, in Eric Robinson and Douglas McKie (eds.), *Partners in Science: Letters of James Watt and Joseph Black* (Cambridge, Mass.: Harvard University Press, 1970), 256-257.

[38] Thomas Young, "No. LXXXIV. Life of Robison," *Miscellaneous Works of the late Thomas Young . . .* (London: John Murray, 1855) [NY: Johnson Reprint Corp., 1972], vol II: 507.

[39] Robert Chambers, "Robison, (Dr.) John," *Biographical Dictionary of Eminent Scotsmen*, (Glasgow: Blackie & Son, 1835), IV: 161.

[40] Harold Dorn, "Robison, John," *DSB*, 11: 495.

[41] Thomas Young, "No. LXXXIV. Life of Robison," *Miscellaneous Works of the late Thomas Young . . .* (London: John Murray, 1855) [NY: Johnson Reprint Corp., 1972], vol II: 508. Young noted: "It was from this time that he dated his serious application to his studies; he became extremely intimate with Dr. Reid and Dr. Alexander Wilson . . ."

[42] Ibid., 509-510. See also Chambers, "Robison, (Dr.) John," *Biographical Dictionary of Eminent Scotsmen*, (Glasgow: Blackie & Son, 1835), IV: 162.

[43] For Robison's personal connection with Aepinus, see R. W. Home, "Introduction," *Aepinus's essay on the theory of electricity and magnetism* (Princeton, N. J.: Princeton University Press, 1979), 223; Robison to James Watt, December 1796, in Eric Robinson and Douglas McKie (eds.), *Partners in Science: Letters of James Watt and Joseph Black* (Cambridge, Mass.: Harvard University Press, 1970), 248; and J. B. Morrell, "The University of Edinburgh in the Late Eighteenth Century: Its Scientific Eminence and Academic Structure," *Isis*, 62 (1971), 161. At this time Robison perhaps read F. U. T. Aepinus, "Mémoire concernant quelques nouvelles expériences électriques remarqualbes," *Histoire de l'Académie Royale des Sciences et Belle Lettres à Berlin*, 1756 (published 1758), 12: 101-121 or F. U. T. Aepinus, *Recueil de differents memoires sur la tourmaline* (St. Petersburg, 1762). It does not appear that he read the *Tentamen* until returning to Scotland.

[44] Based on Robison's handwriting, Richard Olson dates Robison's lecture notes on natural philosophy (EUL Dc. 7.29) from the late 1770s or early 1780s. I make the assumption that Robison's notes on magnetism (EUL Dc. 7.32) are from about the same period. See Richard Olson, "The Reception of Boscovich's Ideas in Scotland," *Isis*, 60 (1969), note 17, 94.

[45] John Robison, Lecture notes on magnetism, Section G, Edinburgh University Library, Dc.7.32., n. d. I thank Dr. David B. Wilson for this source.

[46] Ibid., Section O.

[47] John Robison, Lecture notes on magnetism, Section G, Edinburgh University Library, Dc.7.32., n. d.

[48] In contrast to the chronology presented here, Home claims that during Robison's stay in Russia (1769-1773), Aepinus presented him with a copy of the *Tentamen*. However, Robison's lecture notes (1774-1805) indicate that he did not yet have Aepinus's work until after returning to Edinburgh. He perhaps had an earlier discussion of the tourmaline, but not the *Tentamen*. See Home, Introduction, *Aepinus's*

essay on the theory of electricity and magnetism (Princeton, N. J.: Princeton University Press, 1979), 222 and note 134.

[49] John Robison, Lecture notes on magnetism, Section P, Edinburgh University Library, Dc.7.32., n. d. See also Henry Cavendish, "An Attempt to explain some of the principal Phaenomena of Electricity, be Means of an elastic Fluid." *Philosophical Transactions of the Royal Society of London*, 61 (1771), 584-677.

[50] Henry Cavendish, "An Attempt to explain some of the principal Phaenomena of Electricity, be Means of an elastic Fluid," *Philosophical Transactions of the Royal Society of London*, 61 (1771), 584. Home suggests that Cavendish had a copy of the *Tentamen* in the mid-1760s but did not read it until later due to his aversion to reading in Latin. Home also demonstrates that Aepinus's theory was better known in Britain than Cavendish's work suggests. See R. W. Home, "Aepinus and the British Electricians: The Dissemination of a Scientific Theory," *Isis*, 63 (1972), 190-204.

[51] John Robison, Lecture notes on magnetism, Section P, Edinburgh University Library, Dc.7.32., n. d.

[52] Ibid., Section Q.

[53] Ibid., Section B. See Johann Heinrich Lambert, *Mémoires des Academie de Sciences Berlin*, 22 (1766), 22-48, 49-77. For a brief summary of Lambert's magnetic research, see R. W. Home, Introduction, *Aepinus's essay on the theory of electricity and magnetism* (Princeton, N. J.: Princeton University Press, 1979), 208-209.

[54] John Robison, Lecture notes on magnetism, "Theory," Edinburgh University Library, Dc.7.32., n. d.

[55] Ibid., Section A.

[56] Ibid., Section L.

[57] Ibid., Section S.

[58] James Mackintosh, *Memoirs of the Life of the Right Honourable Sir James Mackintosh*, edited by his son, Robert James Mackintosh (Second Edition, London: Edward Moxon, 1836) I: 22. Mackintosh (1765-1832) attended King's College, Aberdeen before studying law at Edinburgh in 1784.

[59] John Playfair, "Biographical Account of the late John Robison, LL. D. F. R. S. Edin. and Professor of Natural Philosophy in the University of Edinburgh," *Transactions of the Royal Society of Edinburgh*, 7 (1815), 514.

[60] Thomas Young, "No. LXXXIV. Life of Robison," *Miscellaneous Works of the late Thomas Young . . .* (London: John Murray, 1855) [NY: Johnson Reprint Corp., 1972], vol II: 510. Originally published in the *Supplement to the Fourth, Fifth, and Sixth*

Editions of the Encyclopaedia Britannica (Edinburgh: Printed for Archibald Constable and Company, 1824).

[61] Robison's contributions to the third edition also included: "Optics", "Philosophy" (jointly with editor George Gleig), "Physics", "Pneumatics", "Precession", "Projectiles", "Rotation", "Seamanship", "Sound", "Specific Gravity", "Telescope", and "Variation." For a complete list, see Harold Dorn, "Robison, John," *DSB* 11: 497.

[62] The Britannica's third edition was quite popular selling at least 10,000 copies. See Frank A. Kafker, "William Smellie's edition of the *Encyclopaedia Britannica*," in *Notable encyclopedias of the late eighteenth century: eleven successors of the Encyclopédie*, edited by Frank A. Kafker, *Studies on Voltaire and the Eighteenth Century*, 315. (Oxford: The Voltaire Foundation, 1994), 180.

[63] Thomas Young, quoted in David Brewster, "Essay Review of *The Encyclopaedia Britannica; or Dictionary of Arts, Sciences, and General Literature* (Seventh Edition. Edited by Macvey Napier. Edinburgh, 1842)" *The Quarterly Review*, 70 (1842), 47. The third edition, which appeared in installments from 1788 to 1797, established the *Britannica* as the premier British eighteenth-century encyclopedia. See Frank A. Kafker, "The Achievement of Andrew Bell and Colin MacFarquhar as the first publishers of the *Encyclopaedia Britannica*," *British Journal for Eighteenth-Century Studies*, 18 (Autumn 1995), 139-152.

[64] William Whewell, "Report on the Recent Progress and Present Condition of the Mathematical Theories of Electricity, Magnetism, and Heat," *Report of the Fifth Meeting of the British Association for the Advancement of Science* (1835), 7.

[65] David Brewster, "Essay Review of *The Encyclopaedia Britannica; or Dictionary of Arts, Sciences, and General Literature* (Seventh Edition. Edited by Macvey Napier. Edinburgh, 1842)," *The Quarterly Review*, 70 (1842), 47.

[66] For a broader discussion of Scottish mathematics and its interactions with Common-Sense philosophy, see Richard Olson, "Scottish Philosophy and Mathematics 1750-1830," *Journal of the History of Ideas*, 32 (1971), 29-44.

[67] John Robison, "Simson (Dr. Robert)," *Encyclopaedia Britannica* (Third Edition, 1797), 17: 504.

[68] Ibid.

[69] Ibid., 507.

[70] John Robison, *Elements of Mechanical Philosophy, being the substance of a course of lectures on that science, volume first, including dynamics and astronomy* (Edinburgh: Printed for Archibald Constable & Co . . ., 1804), 272-273.

[71] John Robison, "Variation of the Compass," *Encyclopaedia Britannica* 18 (Third Edition, 1797), 619-625.

[72] Ibid., 621.

[73] Ibid.

[74] Ibid., 623.

[75] Ibid., 623-624.

[76] John Robison, "Variation of the Compass," *Encyclopaedia Britannica* 18 (Third Edition, 1797), 624.

[77] Ibid., 624-625.

[78] John Robison, "Electricity," *Supplement to the Encyclopaedia or Dictionary or Arts, Sciences, and Miscellaneous Literature* 1: (Philadelphia: Printed by Budd and Bartram, 1803), 643.

[79] Ibid., 644.

[80] An anonymous review called Robison's treatment of magnetism "full and comprehensive . . . the quintessence of all that has been written on the subject." See "Review of Supplement to the Third Edition of the *Encyclopaedia Britannica*," *The Critical Review; or, Annals of Literature* 35 (1802), 384.

[81] John Robison, "Magnetism," *Encyclopaedia Britannica* 2: (Third Edition, supplement, 1801), 144 (my emphasis).

[82] John Robison, "Impulsion," *Supplement to the Encyclopaedia or Dictionary or Arts, Sciences, and Miscellaneous Literature* (Philadelphia: Printed by Budd and Bartram, 1803), 2: 237-238.

[83] John Robison, "Magnetism," *Encyclopaedia Britannica* 2: (Third Edition, supplement, 1801), 145.

[84] Ibid.

[85] See Laurens Lauden, "Theories of Scientific Method from Plato to Mach," *History of Science* 7 (1968), 1-63 and L. L. Laudan, "Thomas Reid and the Newtonian Turn of British Methodological Thought," in *The Methodological Heritage of Newton* edited by Robert E. Butts and John W. Davis (Toronto: Toronto University Press, 1970), 103-131.

[86] Richard Olson, *Scottish Philosophy and British Physics 1750-1880, A Study in the Foundations of the Victorian Scientific Style* (Princeton, New Jersey: Princeton University Press, 1975), 94-95.

[87] Reid to Lord Kames, 16 December, 1780, in *Works of Thomas Reid, D.D.*, edited by William Hamilton (Edinburgh, 1863), I: 57-58.

[88] See Paul Wood, "Reid on Hypotheses and the Ether; a Reassessment," in *The Philosophy of Thomas Reid.* edited by Melvin Dalgarno and Eric Matthews (Dordrecht: Kluwer Academic Publishers, 1989), 433-446. Wood discusses the evolution of Reid's attitudes towards hypotheses and the ether. He also explores Reid's ambivalent attitude toward hypothetical reasoning.

[89] Thomas Reid, *Essays on the Intellectual Powers of Man* (1785) quoted in L. L. Laudan, "Thomas Reid and the Newtonian Turn of British Methodological Thought," from *The Methodological Heritage of Newton* edited by Robert E. Butts and John W. Davis (Toronto: Toronto University Press, 1970), 108. Countering Hume's skepticism, Reid argued that ideas relied upon data gathered through instinctive or common sense experience. As Common-Sense philosopher James Beattie explained: "To believe our senses, is, therefore, according to the law of nature; and we are prompted to this belief, not by reason, but by instinct, or common sense. I am as certain, that at present I am in a house, and not in the open air; that I see by the light of the sun, not by the light of a candle; that I feel the ground hard under my feet; and that I lean against a real material table, —as I can be of the truth of any geometrical axiom or of any demonstrated conclusion . . ." James Beattie, *An Essay on the Nature and Immutability of Truth; in opposition to Sophistry and Scepticism* (Edinburgh: A. Kincaid & J. Bell, 1770), 62-63. See also L. L. Laudan, "Thomas Reid and the Newtonian Turn of British Methodological Thought," in *The Methodological Heritage of Newton* edited by Robert E. Butts and John W. Davis (Toronto: Toronto University Press, 1970), 103-131 and S. A. Grave, *The Scottish Philosophy of Common Sense* (Westport, CT: Greenwood Press, Publishers, 1960), 11-44.

[90] Letter from Reid to Kames dated 16 December, 1780. *Works of Thomas Reid, D.D.*, ed. W. Hamilton (Edinburgh, 1863), I: 58. Quoted in Geoffrey Cantor, "Henry Brougham and the Scottish Methodological Tradition," *Studies in the History and Philosophy of Science* 2 (1971), 75. Similarly, Robison's moral philosophy professor Adam Smith favored the search for generalities: "Where [the mind] can observe but one single quality, that is common to a great variety of otherwise widely different objects, that single circumstance will be sufficient for it to connect them all together, to reduce them to one common class, and to call them by one general name."

[91] David Hartley, *Observations on Man, His Frame, His Duty, and His Expectations* (London: S. Richardson, 1749) [Gainesville, FL: Scholars' Facsimiles & Reprints, 1966], 16. Hartley (1705-1757) is known for his physiological speculations and the doctrine of associationism.

[92] Geoffrey Cantor, "Henry Brougham and the Scottish Methodological Tradition." *Studies in the History and Philosophy of Science* 2 (1971), 73-75.

[93] Dugald Stewart, *Elements of the Philosophy of the Human Mind, The Collected Works of Dugald Stewart*, edited by William Hamilton (Edinburgh: Thomas Constable and Co., 1858), 3: 301-302. Stewart (1753-1828) was educated at Edinburgh where he succeeded his father, Matthew Stewart (1717-1785), as mathematics professor in 1775, teaching jointly with John Playfair. In 1785, he became professor of moral

philosophy, a position he held until retiring in 1820. Stewart's major work, *Elements of the Philosophy of the Human Mind*, appeared in three volumes (1792, 1814, 1827).

[94] Ibid., 301.

[95] Thomas Brown, *Lectures on the Philosophy of the Human Mind* (19th edition, London: William Tegg & Co., 1858), Lecture VIII., 47. Brown (1778-1820) attended the University of Edinburgh at the turn of the century. From 1797 to 1800, he was one of the leading members of an informal group called the Academy of Physics which aspired to "the investigation of nature, the laws by which her phenomena are regulated, and the history of opinions concerning these laws." Brown had personal and professional ties with many Scottish scientists and philosophers. See David Welsh, *Memoir of the Author*, from Thomas Brown, *Lectures on the Philosophy of the Human Mind* (19th ed., London: William Tegg & Co., 1858), v-xxxi.

[96] Ibid.

[97] Ibid., 50.

[98] See Richard Olson, *Scottish Philosophy and British Physics 1750-1880, A Study in the Foundations of the Victorian Scientific Style* (Princeton, New Jersey: Princeton University Press, 1975), 96-98.

[99] Joseph Black, *Lectures on the elements of chemistry delivered at the University of Edinburgh*, edited with a preface by John Robison (Edinburgh: Mundell and Son, 1803), vol. I, Preface, vii.

[100] Ibid., viii.

[101] John Robison, Lecture notes on magnetism, Section G, Edinburgh University Library, Dc.7.32., n. d.

[102] John Robison, "Optics," *Encyclopaedia Britannica*. Edinburgh: A. Bell and C. Macfarquhar, 1797 (Third Edition), 13: 263-264. Robison also opposed the aether because it had been appropriated by French atheists. He explained in 1804: "It is somewhat amusing to remark how the authority of Sir Isaac Newton has been eagerly catched at by the atheistical sophists to support their abject doctrines . . . M. Diderot worked into a better shape the slovenly performance of Robinet . . . and affected to deduce all his vibrations and vibratiuncles from the elastic aether of Sir Isaac Newton, dressing up the scheme with mathematical theorems and corollaries. And thus, Newton, one of the most pious of mankind, was set at the head of the atheistical sect." John Robison, *Elements of mechanical philosophy*, being the substance of a course of lectures on that science, volume first, including dynamics and astronomy (Edinburgh: Printed for Archibald Constable & Co . . ., 1804), 693-694.

[103] Ibid., 264. For similar reasons, Robison rejected Newton's theory of reflection. He explained: "It is ever with extreme reluctance that we venture to call in question the doctrines of Newton; but to his theory of reflection there is this insuperable objection, that it explains nothing, unless the *cause* of the fits of more easy reflection

and transmission be held as legitimate, namely, that *they are produced by the undulations of another elastic fluid, incomparably more subtile than light,* acting upon it in the way of impulse . . . to admit this theory of them [the fits] would be to transgress every rule of philosophising . . ." John Robison, "Optics," *Encyclopaedia Britannica.* Edinburgh: A. Bell and C. Macfarquhar, 1797 (Third Edition), 13: 307.

[104] John Robison and George Gleig, "Philosophy," *Encyclopaedia Britannica.* Third Edition (Edinburgh: A. Bell and C. Macfarquhar, 1797), 14: 594. Gleig (1753-1840) became the new editor of the last six volumes of the third edition when Colin Macfarquhar died in 1793. He also was the sole editor of the supplement published in 1801. Gleig induced Robison to revise the article "Optics" and write a series of natural philosophy articles. He was also assisted by John Playfair in writing "Mathematics" for the supplement. Scottish-educated like Robison, Gleig distinguished himself at King's College, Aberdeen in mathematics as well as moral and physical sciences. His contributions included "Theology", "Instinct", "Metaphysics", "Moral Philosophy", and "Nature." See Paul Kruse, *The Story of the Encyclopaedia Britannica, 1768-1943* (Ph.D. Dissertation, University of Chicago, 1958), 68-72.

[105] John Robison, "Electricity," *Supplement to the Encyclopaedia or Dictionary or Arts, Sciences, and Miscellaneous Literature* (Philadelphia: Printed by Budd and Bartram, 1803), 1: 696. Thomas Dobson's American edition of the *Britannica* included the same scientific articles by Robison as its British counterpart.

[106] John Robison, "Magnetism," *Encyclopaedia Britannica* (Third Edition, supplement, 1801), 2: 154.

[107] Ibid., 153. For further discussion of Robison's views on hypotheses, see Crosbie Smith, "'Mechanical Philosophy' and the Emergence of Physics in Britain: 1800-1850," *Annals of Science*, 33 (1976), 7-11.

[108] Ibid.

[109] John Robison and George Gleig, "Philosophy," *Encyclopaedia Britannica,* Third edition (Edinburgh: A. Bell and C. Macfarquhar, 1797),14: 587.

[110] Ibid.

[111] Ibid., 595.

[112] Franz Aepinus, *Aepinus's essay on the theory of electricity and magnetism.* translated by P. J. Connor (Princeton, N. J.: Princeton University Press, 1979), 239.

[113] R. W. Home, Introduction, *Aepinus's essay on the theory of electricity and magnetism* (Princeton, N. J.: Princeton University Press, 1979), 112-113. Like Newton, Aepinus believed that nature probably produced "analogous phenomena by analogous means." Franz Aepinus, *Aepinus's essay on the theory of electricity and magnetism.* translated by P. J. Connor (Princeton, N. J.: Princeton University Press, 1979), 239.

[114] Henry Cavendish, "An Attempt to explain some of the principal Phaenomena of Electricity, be Means of an elastic Fluid," *Philosophical Transactions of the Royal Society of London*, 61 (1771), 584.

[115] R. J. Boscovich, *De Solis ac Lunæ Defectibus* (London: 1760) quoted in Dugald Stewart, *Elements of the Philosophy of the Human Mind, The Collected Works of Dugald Stewart*, edited by William Hamilton (Edinburgh: Thomas Constable and Co., 1858) 3: 305. For an examination of the Boscovich-Robison connection, see Richard Olson, "The Reception of Boscovich's Ideas in Scotland," *Isis*, 60 (1969), 91-103.

[116] John Robison, Lecture notes on magnetism, Section A, Edinburgh University Library, Dc.7.32., n. d.

[117] Ibid.

[118] Ibid., Section S.

[119] John Robison, "Electricity," *Supplement to the Encyclopaedia or Dictionary or Arts, Sciences, and Miscellaneous Literature* (Philadelphia: Printed by Budd and Bartram, 1803) 1: 702.

[120] John Robison, "Magnetism," *Encyclopaedia Britannica* 2: (Third Edition, supplement, 1801), 153.

[121] Ibid.

[122] John Robison, *A system of mechanical philosophy*, with notes by David Brewster (Edinburgh: J. Murray, 1822), III: 481.

[123] John Robison, "Magnetism," *Encyclopaedia Britannica* 2: (Third Edition, supplement, 1801): 153-154 (my emphasis).

[124] John Robison, *A system of mechanical philosophy*, with notes by David Brewster, (Edinburgh: J. Murray, 1822), IV: 322, 323-330.

[125] Ibid., 330.

[126] Ibid., 352.

[127] See Robert Chambers, "Playfair, John," *A Biographical Dictionary of Eminent Scotsmen*, (Glasgow: Blackie & Son, 1835), IV: 106-112. In 1785, Stewart exchanged the mathematics professorship with Ferguson. He was taught mathematics at Edinburgh by his father, Matthew Stewart, before holding the mathematics chair from 1775 to 1785.

[128] John Playfair, *Dissertation Second: Exhibiting a General View of the Progress of Mathematical and Physical Science, since the revival of letters in Europe* (Edinburgh, 1835) reprinted in *Dissertations on the Progress of Knowledge* (New York: Arno Press, 1975), part I: 3.

[129] John Playfair quoted in Robert Chambers, "Playfair, John," *A Biographical Dictionary of Eminent Scotsmen*, (Glasgow: Blackie & Son, 1835), IV: 108. Playfair was elected F. R. S. in 1807. He is perhaps best remembered for popularizing James Hutton's vulcanist geological theories in *Illustrations of the Huttonian Theory of the Earth* (1802).

[130] Robert Chambers, "Playfair, John," *A Biographical Dictionary of Eminent Scotsmen*, (Glasgow: Blackie & Son, 1835), IV: 109. Of Playfair's algebraic notation, Chambers noted, "The plan has been repeatedly practised since that period, and "Playfair's *Euclid*" is a book well known to the boys in most mathematical schools . . ."

[131] John Playfair, *Dissertation Third*, 441, quoted in Richard Olson, "Scottish Philosophy and Mathematics: 1750-1830," *Journal of the History of Ideas*, 32 (1971), 32. Playfair was one of very few Scottish mathematicians who divorced mathematics from sensory experience and justified the use of imaginary quantities. See Richard Olson, "Scottish Philosophy and Mathematics: 1750-1830," *Journal of the History of Ideas*, 32 (1971), 32, 40.

[132] John Playfair, *Dissertation Second: Exhibiting a General View of the Progress of Mathematical and Physical Science, since the Revival of Letters in Europe.* (Edinburgh, 1835) reprinted in *Dissertations on the Progress of Knowledge* (New York: Arno Press, 1975), part II: 26.

[133] John Playfair, *Outlines of Natural Philosophy*, being heads of lectures delivered in the University of Edinburgh (Edinburgh: Neill & Co., 1814), 2.

[134] John Playfair, *The Works of John Playfair*, vol. II. *Dissertation; Exhibiting a General View of the Progress of Mathematical and Physical Science, since the Revival of Letters in Europe* (Edinburgh: Archibald Constable & Co., 1822) [Landmarks of science microform, 1971], 71.

[135] Ibid., 127.

[136] Ibid., 340-341.

[137] John Playfair, *Dissertation Second: Exhibiting a General View of the Progress of Mathematical and Physical Science, since the Revival of Letters in Europe.* (Edinburgh, 1835) reprinted in *Dissertations on the Progress of Knowledge* (New York: Arno Press, 1975), part II: 83.

[138] Playfair, *The Works of John Playfair*, vol. II. *Dissertation; Exhibiting a General View of the Progress of Mathematical and Physical Science, since the Revival of Letters in Europe* (Edinburgh: Archibald Constable & Co., 1822) [Landmarks of science microform, 1971], 6.

[139] Ibid., 7.

[140] Ibid., 131-132 (my emphasis).

[141] Ibid., 87.

[142] John Playfair, "Aepinus, (Francis Ulrich Theodore)," *Supplement to the Fourth, Fifth, and Sixth Editions of the Encyclopaedia Britannica* (Edinburgh: Printed for Archibald Constable and Company, 1824), 1: 63.

[143] Ibid., 64.

CHAPTER 5:
FRENCH PHYSICS IN GREAT BRITAIN (c.1800-1820)

Between the publication of Robison's 1801 "Magnetism" article and his death in 1805, he composed a revision for the fourth edition of *Encyclopaedia Britannica*. This version, however, did not appear until 1822, when David Brewster compiled and edited a collection of Robison's writings.[1] In addition to growing convictions regarding the reality of the magnetic fluid, the revision illustrated Robison's increasing awareness of continental ideas. His discussion of the works of J. H. Van Swinden, R. J. Haüy, C. A. Coulomb and other investigators made this evident. As well, Robison's *Elements of Mechanical Philosophy* (1804) illustrated his admiration for certain aspects of *Systême du Monde* (1796) by eminent French natural philosopher, Pierre Simon de Laplace.[2]

In the early nineteenth century, the emerging theories of numerous continental physicists, particularly those from France, won Robison's attention as well as many others in Britain. Although British scientists neither quickly nor completely embraced these new ideas, continental theories, particularly French mathematics and physics, fueled the fusion of mathematical and experimental traditions which Robison, Cavendish, and a very few others had begun in the 1770s.

This generalization held true not only in mechanics and astronomy, but areas of experimental physics, including electricity and magnetism. In the late eighteenth and early nineteenth centuries experimental physics flourished in France. Hence, as Robison worked in Edinburgh, experimental physicist Charles Augustin Coulomb approached electricity and magnetism in a similar fashion in Paris. As a forerunner of Laplacian science, Coulomb played a major role in bringing mathematics and experiment together. Emphasizing precision measurement and the application of analytical mathematics to experiment, he and the disciples of Laplace made significant changes in both the content and method of French experimental physics.

With this wider context in mind, this chapter initially examines French magnetic and electrical research, particularly the magnetic theories of Coulomb and several Laplacian physicists. The chapter also suggests affinities in content and methodology between Scottish and French experimental physics. These similarities, it is argued, contributed to the demise of circulating fluid theories and to the wider British acceptance of imponderable fluid theories. There were, of course, differences affecting the reception of French physics, and these are also examined. Finally, the chapter examines the reception of Laplacian magnetic and electric theory in Britain. Following Robison's lead, many early nineteenth-century British physicists favored Aepinus' one-fluid theory. In contrast, Coulomb's theory remained initially little known or accepted usually due to British ignorance, misinformation, or methodological dissonance. However, by the late 1820s, the two-fluid magnetic theory espoused by Haüy, Biot, and Poisson also gained a growing audience in Britain.

Charles Augustin Coulomb (1736-1806): The French Robison?

As their dates indicate, Coulomb (1736-1806) and Robison (1739-1805) were contemporaries. The similarities, however, do not end with this chronological coincidence. Both men took great interest in experimental physics and in engineering problems as well. In addition to similar research interests, both sought descriptive quantitative laws through a combination of precise experimentation and mathematical analysis. Similar as well was their tentative use of hypothetical imponderable fluids. During the last quarter of the eighteenth century, both Robison and Coulomb independently embraced Aepinian theories with great enthusiasm. Beyond these similarities, however, there were important differences. For one, Coulomb went further than Robison in altering Aepinus' magnetic theory.

As Robison taught Aepinus' semi-mathematized theories in Scotland, in France Coulomb modified those same theories in several significant ways. Utilizing new experimental techniques, precision apparatus, and improved artificial magnets, Coulomb investigated and eventually established the long-sought force laws for magnetism and electricity. Such findings allowed further quantification, while transforming electrostatics and magneto-statics into rigorously deductive mathematical sciences. Establishing the quantitative force law which now bears his name, Coulomb gave the largely experimental and qualitative sciences of electricity and magnetism a solid mathematical foundation.[3] Furthermore, Coulomb developed an experimental physics that extended the inverse-square relationship to encompass magnetism and electricity. In this, some saw a triumph of Newtonian science.

Coulomb's engineering education at the École du Génie at Mézières prepared him in mathematics as well as experimental physics (*physique expérimentale*). Upon graduating in 1761, Coulomb had become both a competent mathematician and a skilled experimenter. He also gained important friendships with physicist Jean Charles Borda and mathematician Charles Bossut. Spending over three decades as a military engineer at various posts and as an engineering consultant in Paris, Coulomb's experimental physics was strongly influenced by his engineering background. His persistent preoccupation with accurate measurement and precise instrumentation betrayed this influence. Not surprisingly, these concerns also affected his approach to experimental physics.

Coulomb's initial foray into magnetism, "Investigations of the Best Method of Making Magnetic Needles," (1777) hinted at his life-long concern with precision instrumentation.[4] In this memoir he, like Aepinus and Robison, rejected circulating fluid theories. Coulomb's paper appeared in an intellectual climate long dominated by Cartesian effluvialist ideas. In the 1750s, the leading French investigators of magnetism, Duhamel and Antheaulme, accepted circulation theories without question. French experimentalist Jean-Antoine Nollet wrote in 1764:

> Although savants have embraced various opinions on the causes of magnetism . . . they have always agreed on one point for . . . the basis of their systems; there is scarcely one amongst them who does not admit, around each natural or artificial magnet, a subtle and invisible fluid which circulates from one pole to the other. . . . This supposition is entirely probable, and can scarcely be denied in view of the following experiment [i.e., patterns of iron filings around a magnet].[5]

Indeed, until the publication of Coulomb's award-winning memoir, few French physicists paid any attention to Aepinus' alternative.

Seeking to replace them with a more precise, quantitative description, Coulomb attacked the circulating fluid theories. He argued, with a formal mathematical proof, that reducing magnetic effects to the pressure of Cartesian vortices or etherial currents was mechanically untenable. Rejecting contact action theories, Coulomb stressed, above all else, the mathematical analysis of forces between particles of an imponderable magnetic fluid.[6] Like Aepinus and Robison, he did not attempt to reduce these attractions and repulsions to impulsive action, instead he left them unexplained, acting at a distance.

Although Coulomb wrote little about terrestrial magnetism, his speculations closely paralleled those of Aepinus. Devoting a small section of his 1777 paper to the diurnal variation of magnetic declination, Coulomb questioned Canton's explanation relying on solar heat. If the sun's heat continually weakened terrestrial magnetism, Coulomb contended that the earth's magnetism would have been destroyed long ago.[7] Offering an alternative, he explained, "If it is not the heat of the sun which produces diurnal variation: [and] if in the meantime that effect is due to this star, it must be that the sun acts on the terrestrial globe, like one magnet acts on another magnet."[8] Like Aepinus' explanation, the sun's magnetism redistributed the terrestrial magnetic fluid, causing daily variations. Solar action upon the earth, for Coulomb, was analogous to the positive pole of a magnet driving away the magnetic fluid in the point of a steel knife.

Diffused across the earth's surface, the magnetic fluid acted on the ends of magnetic needles by attractions and repulsions depending on the density of fluid in each point of the earth. He also appealed to varying distributions of magnetic fluid to explain irregular geographic changes in magnetic declination. In these notions, Coulomb agreed with Aepinus.

Coulomb: Modifying Aepinian Theory

Although greatly impressed by Aepinus' *Tentamen*, Coulomb openly criticized and modified his theory. Initially, he noted that the one-fluid theory, and atmospheric two-fluid theory proposed by Franeker professor Anton Brugmans in 1765, contradicted certain observations of magnetic phenomena.[9] For instance, neither theory satisfactorily explained why the fragments of a cut magnet each became new magnets. How could this happen if the fluid or fluids moved to the extremities of the original magnet? No one had ever produced or observed a magnet with one pole.

In his early work, Coulomb explained this difficulty within the context of Aepinian theory. Following Aepinus, he assumed that a small amount of fluid escaped when a magnet was cut or broken in two. Nevertheless, he remained dissatisfied with this conjecture, suggesting that "each point of a magnet or of a magnetized bar can be regarded as the pole of a tiny magnet."[10] Like Robison, Coulomb also had an aversion to Aepinus' assumption that the particles of ordinary matter repulsed each other. This blatantly contradicted Newton's law of universal gravitation. Coulomb's concerns regarding fragmented magnets and self-repulsive ordinary matter eventually led him to significantly modify Aepinian theory.

First, however, and perhaps most importantly, he experimentally established the long-sought law of magnetic force. In 1784, Coulomb wanted to demonstrate that "the attractive and repulsive force of the magnetic fluid is exactly [as]. . . the inverse square of the distances of the magnetic molecules."[11] Using two distinct methods (i.e., one with an oscillating magnetic needle at differing distances from the pole of a long thin magnet, the other utilizing the newly invented torsion balance) he convincingly demonstrated that magnetic attractions and repulsions diminished as the inverse square of the distance.[12] This finding allowed the further quantification of Aepinian magnetic theory because one could now calculate the exact forces involved.

In addition to the inverse-square law, Coulomb altered Aepinus' theory by adding another fluid. Departing from earlier two-fluid theories such as Brugmans', Coulomb's two magnetic fluids remained confined within all magnetic materials and did not form external atmospheres. From 1780 onward, Coulomb's distaste for Aepinus' notion that particles of ordinary matter repelled each other led him to consistently favor the two-fluid theory. Accepting the existence of two distinct fluids, often called "boreal" and "austral", bypassed the need for supposing a repulsive force among the particles of ordinary matter. Admitting the mathematical indistinguishability of the one- and two-fluid alternatives, Coulomb nevertheless consistently preferred the latter. He, however, did not dogmatically defend the physical existence of two fluids.[13] An anonymous summary of Coulomb's work in the *Journal de Physique* (1794) explained that he did not regard the existence of two magnetic fluids as demonstrated.[14] Although Robison accepted Aepinian theory with reservations, he, like Coulomb, cautiously used the one-fluid hypothesis without insisting that it reflected physical reality. Therefore, both Coulomb and Robison accepted hypothetical invisible fluids primarily for their usefulness in explaining the phenomena.

In 1789, Coulomb returned to difficulties posed by a magnet cut into two or more pieces. Explaining why the fragments became new magnets, he made his third major modification of Aepinus' theory. His solution lay in assuming microscopic rather than macroscopic movements of the two magnetic fluids:

> I believe that one could reconcile the result of the experiments with calculation by making some changes in the hypotheses; here is one which appears able to

> explain all the magnetic phenomena of which the preceding experiments have given precise measurements. It consists in supposing in M. Æpinus' system that the magnetic fluid is contained in each molecule or integral part of the magnet or the steel; that the fluid can be transported from one extremity to the other of this molecule, giving each molecule two poles; but this fluid may not pass from one molecule to another.[15]

Molecular confinement of the magnetic fluid avoided the need for movement of fluids along the magnet's entire length, thereby explaining the previously puzzling phenomena. No fluid need escape as in Aepinus' explanation.

Hence, from the 1770s to 1790s, Coulomb went beyond his cautious Scottish contemporary in extending Aepinian ideas. Though both Coulomb and Robison fused the experimental and mathematical in their research, Coulomb's work altered the course of magnetic investigations in several significant ways. First, he quantified Aepinus' theory by determining the magnetic force law. Although the inverse-square law was not immediately accepted by all, Coulomb eventually succeeded in this where many others, including Robison, had failed.[16] Second, he used precise experimental apparatus (e.g., torsion balance) in addition to long, thin magnets with the poles concentrated near the ends. Such magnets allowed for the isolation of forces needed to determine the force law and further quantify the theory. Third, he modified Aepinian magnetic theory to include two magnetic fluids confined inside each particle or "molecule" of the magnetized body. These latter two changes obviated the need for Aepinus' troublesome self-repulsive ordinary matter while plausibly solving the cut magnet problem.

A combination of Coulomb's research and broader scientific changes led many French physicists to espouse Aepinus' one-fluid theory or a modified version of it. Owing much to the force of Coulomb's arguments, French physicists increasingly recognized the merits of Aepinus' work. By the late 1780s, many hailed Aepinus as the founder of the new mathematical approach to electricity and magnetism.[17] Many French physicists accepted and further refined Coulomb's two-fluid theory. Their "Laplacian" views arose, not merely from adhering to Coulomb, but from wider changes in French scientific education and methodological outlook.

Laplacian Physics (c. 1800-1815)

"Laplacian physics," a style of physics that flourished in France during the Napoleonic era (c.1799-1815), sought to explain phenomena on all scales in terms of central attractive and repulsive forces between ordinary matter and half a dozen or so imponderable fluids.[18] Building upon an experimental tradition tracing its origins to the *Opticks*, the followers of Laplace restated many of Newton's suggestions in highly mathematical form and addressed outstanding problems, particularly in areas of experimental physics which previously had remained qualitative. Significantly, most Laplacians either studied or taught at the École Polytechnique, established in 1794, where mathematics was the principal subject taught.[19] Laplacians, well versed in mathematics, followed a research program laid out by Laplace, their leader. In 1809, Laplace clearly stated this program:

> In general, all the attractive and repulsive forces in nature can be reduced, ultimately, to forces of this kind exerted by one molecule on another. Thus, in my *Theory of Capillary Action*, I have shown that the attractions and repulsions between small objects floating on a liquid . . . depend on intermolecular attractions which are negligible except at insensible distances. Similarly an attempt has been made to reduce the phenomena of electricity and magnetism to intermolecular action.[20]

In the Laplacian view, ordinary matter interacted with a variety of imponderable fluids. Each imponderable fluid, such as the fluid of heat (i.e., caloric), consisted of mutually repulsive particles and, in some cases, an opposite fluid which attracted its counterpart (e.g., boreal and austral magnetic fluids). Using powerful mathematical analysis and Laplace's program of short-range forces, Laplacian scientists transformed sciences including chemistry, optics, and many areas of experimental physics, including magnetism.

Laplacians preferred mathematical and quantitative methods in developing the theory of imponderables. Like many late eighteenth-century Scottish mathematicians, they utilized both continental analysis and the older geometrical methods. However, when choosing between the two approaches, Laplace explained in *Exposition du système du monde* (1796):

> Geometrical synthesis has the advantage of never allowing us to lose sight of its goal . . .; whereas algebraic analysis quickly allows us to forget the principal goal in the form of abstract combinations . . . [however] No other language lends itself so elegantly . . . to the long train of interconnected expressions, all flowing from one fundamental equation. Analysis also offers the advantage of always leading us to the simplest methods. One need only make a judicious selection of unknowns using the proper methods and give the results the form most easily reducible to . . . numerical calculation.[21]

Therefore, Laplace and his followers preferred analysis over geometry as the superior form of mathematical expression.

As had Robison and Coulomb, many Laplacians took a cautious attitude toward the existence of hypothetical invisible fluids. Writing on experimental physics in 1809, Jean-Baptiste Biot noted:

> In order to explain [these phenomena], physicists have imagined certain elastic fluids endowed with attractive and repulsive properties, and capable of penetrating all bodies or only some of them. . . . By means of these suppositions one can, to a certain point, represent the majority of phenomena . . .; but there still remain many of them which lend themselves with difficulty to these explanations, and others which escape them entirely.
>
> Consequently the true physicists admit the consideration of these fluids solely as a convenient hypothesis, to which they are very careful not to attach ideas of reality, and which they are ready to modify or to abandon entirely as soon as the facts show themselves to the contrary.[22]

Thus, both Scottish and French experimental physicists considered that the "convenient hypotheses" of imponderable fluids should be readily abandoned upon the discovery of new, contradictory experimental evidence. Despite their differences in mathematical approach, affinities between the content and methodology of French and Scottish science manifested themselves in scientific connections between France and Scotland.

In line with Coulomb's urge to quantify, Laplacians also stressed the use and improvement of precision instruments. Many displayed a natural affinity for the precise, quantitative, and mathematical theories of Aepinus and Coulomb.[23] For instance, French crystallographer René Just Haüy published a book favorable to Aepinian ideas in 1787 and, five years later, gave a glowing commentary on one of Coulomb's memoirs.[24] In *Traité de Minéralogie* (1801), he asserted that Coulombian theories of electricity and magnetism attained a perfection not seen in earlier efforts. The new theory of magnetism excluded vague, careless explanations appealing to atmospheres and magnetic effluvia. It replaced them with forces whose laws had been demonstrated by observation and rigorous mathematical demonstration.[25] In 1803, Haüy again endorsed Coulomb's theory in an official textbook used in the French *Lycées* .[26] A favorable review of his text commented that the theory left "nothing to wish for regarding the declination and inclination of the magnetic needle."[27] Coulomb's theories also gained support from other leading Laplacians, including Jean-Baptiste Biot and Siméon Denis Poisson. Before turning to their work, we return to parallel theoretical developments across the English Channel.

Thomas Young (1773-1829) and the Demise of Circulating Fluids

As support for quantitative, imponderable fluid theories grew in Britain, the Cartesian circulating fluid theories continued to fall out of favor. Such theories no longer meshed with the emerging style of experimental physics based upon careful experiment, precise instrumentation, and powerful mathematical analysis. Neither did the older theories fit with Scottish and French physicists' acceptance of action at a distance or their views on scientific method. As interest in Laplacian mathematics and physics expanded in the early nineteenth century, new ideas altered the face of British experimental physics.

Many investigators, French and British alike, realized that experimental physics had not yet reached the status of the model science, mechanics. A hierarchy of physical sciences persisted with the more mathematical rational mechanics and gravitational astronomy at the top, and the more empirical areas of experimental physics at the bottom. Acknowledging the gulf between mechanics and experimental physics, an anonymous reviewer of Thomas Young's natural philosophy textbook noted in 1807:

> The doctrines of pure mechanics rest upon principles, the truth of which has been impressed upon the mind so forcibly by constant and uniform experience of our lives, that we regard them as a species of axioms or self-evident truths . . . There remains still an immense mass of interesting phenomena, to which the rules of calculation and the art of analysis are still less applicable. On these therefore we are necessitated to content ourselves with simple description, or the adoption of hypotheses as nearly coincident with the phaenomena as imperfect and inadequate data will admit.[28]

This "immense mass of interesting phenomena" included cohesion, heat, electricity and magnetism. While mechanics remained at the zenith of physical sciences, it was generally believed that elevating the status of experimental physics required making it more mathematical and amassing more adequate empirical data with precise instruments. With these new criteria, the hypothesis adopted by many British physicists to explain electricity and magnetism was that of Franz Aepinus.

Meanwhile, though the details of terrestrial magnetism remained mysterious, most believed that its improvement as well lay in mathematics, quantification, and instrumentation. Embracing the Aepinian theory often accompanied accepting that the earth acted as, or contained, a large magnet. In contrast, the older circulating fluid and atmospheric theories could often coincide with the rejection of the Gilbertian notion. Furthermore, Cartesian theories explained atmospheric magnetic phenomena as the result of effluvia flowing through iron and other magnetic bodies. In contrast, Aepinian theory accepted that such phenomena arose from forces exerted at a distance, without contact action or impulsion. Hence, an important connection existed between action at a distance, internal magnetic fluid, and acceptance of the Gilbertian notion on the one hand,

and mechanical impulsion, external circulating effluvia, and rejection of the Gilbertian notion on the other. In other words, these sets of ideas frequently accompanied one another.

The change from circulating effluvia to Aepinus' internal magnetic fluid becomes apparent when early nineteenth-century works are compared with earlier writings on magnetism. For instance, while John Imison's *School of Arts* (1796) supported the hypothesis of circulating magnetic effluvia, a revised edition of this work, *Elements of Art and Science* (1803), portrayed a distinctly different picture. The revision supposed that iron filings sprinkled over a magnet formed their familiar patterns, not from the flow of effluvia, but due to each iron filing becoming a tiny magnet. With respect to terrestrial magnetism, the author argued, not for effluvial circulation, but for the distribution of large masses of iron dispersed throughout the globe. Indeed, the earth acted as "an immense magnet . . . [whose] magnetism arises from the magnetism of the ferruginous bodies contained in it."[29] While magnetism's actual causes lay undiscovered, Imison's revised text considered Aepinus' hypothesis the most ingenious.[30]

Similarly, English savant Thomas Young rejected circulating fluid theories, while supporting Aepinian theory and the Gilbertian view. Exposed to Scottish natural philosophy as a student, Young had studied under Robison, Playfair and others while attending the University of Edinburgh in 1794-1795.[31] From 1799 to 1802, Young went beyond the caution of his professors, attempting to reduce the multitude of subtle elastic fluids to one general principle. He remarked, "it is not improbable that they [electrical phenomena] may depend on some modification of the actions of the medium which appears to be concerned in the effects of light, heat, cohesion and repulsion."[32] After 1802, however, Young abandoned such speculations and favored distinct fluids for electricity and magnetism. Although diverging from Scottish natural philosophers in his advocacy of the undulatory theory of light and the luminiferous ether, Young's discussion of magnetism hints at the continuing influence of the Scottish tradition.[33]

A brief discussion of magnetism in *A Course of Lectures on Natural Philosophy* (1807) illustrated Young's Scottish-influenced positions.[34] Some magnetic theories, he noted, were too complicated or poorly supported by the evidence. The doctrine of circulating streams of magnetic fluid, he claimed, had been "universally abandoned." In its place, Young acknowledged that all observed magnetic effects could be explained by the redistribution of a single magnetic fluid. He also argued that magnetism induced in each filing yielded the familiar patterns of iron filings strewn over a magnet. Stressing the analogy between electricity and magnetism, Young noted that the strong similarities between them required their placement near each other in any classification of natural philosophy. Nevertheless, he cautioned that there was no immediate connection between the two. Noting that Aepinus had laid a foundation for Coulomb and others, Young preferred Aepinus' one-fluid hypothesis. Though mentioning Coulomb, he did not discuss the merits or defects of the two-fluid hypothesis.

In typical Scottish fashion, Young remained wary of the unbridled use of hypotheses, yet admitted their legitimate role, particularly with regard to the study of magnetism. Like Robison and other Scots, he remarked that hypotheses were of "great utility in assisting us to generalize, and to retain in memory, a number of particular facts which would otherwise be insulated."[35] Again in agreement with Robison, Young hypothesized that the accumulation and deficiency of fluid in the great terrestrial magnet determined the position of the magnetic poles. Furthermore, he supposed that these two magnetic poles or "centers of force" were considerably diffused on the earth's surface. Rejecting Halley's kernel-and-shell theory on the grounds that it predicted far too regular and uniform changes, Young supposed that variations in terrestrial magnetism depended on complex alterations in the earth's intricate internal structure. Like

Robison, Young considered that influences such as the aurora borealis, volcanic eruptions, and other physical and chemical changes made it impossible to calculate the true position of the magnetic needle either temporally or geographically. He concluded, "in subjects so little understood as the theory of magnetism, we are obliged to admit some paradoxical propositions, which are only surprising on account of the imperfect state of our knowledge."[36] Young cautiously accepted Aepinus' hypothesis and recognized the complex, inexplicable nature of terrestrial magnetic variation. In both instances, his ideas emulated Robison's.

Like Robison and Young, other British physicists preferred Aepinian theory over circulating effluvia and endorsed the Gilbertian position.[37] In this way, theories of magnetism and terrestrial magnetism remained linked together. Rejecting circulating fluids while affirming the notion of a large terrestrial magnet, the fourth edition of *Britannica* (1810) optimistically noted that the Gilbertian view, if established, allowed "a complete explanation of *all the phenomena of magnetism.*"[38] Two years later, Scottish chemist Thomas Thomson proclaimed Robison's 1801 "Magnetism" article the best treatise available on the subject, thereby implicitly embracing Aepinian theory and the Gilbertian view.[39] Pointing out the "absurdity" of the circulation of magnetic fluid, the magnetism article in the *Encyclopaedia Londinensis* (1815) accepted the inductive principle resulting in the familiar patterns of iron filings.[40] The article further noted that the movements of the compass needle and dipping needle were "exactly imitated by a common magnet, or a terrella."[41] One year later, an English review of a French scientific textbook noted that Franklin's "beautiful and simple theory" of electricity was generally established in England, but in France the two-fluid explanation was favored. The review further explained that mineralogist F. S. Beudant, the author of the text, recognized the "greater simplicity" and "nearer accordance with facts" of the one-fluid theory, yet nevertheless explained all the phenomena with the two fluid hypothesis, "for what appears the very insufficient reason of its general prevalence."[42] The reviewer claimed that such questionable reasoning also extended to the Coulombian theory of magnetism.

Numerous investigators demonstrated the widespread English support for one-fluid theories. For instance, Bristol barrister at law Charles Carpenter Bompass claimed in 1817 that the opinion of magnetic phenomena "most generally entertained" was that of a peculiar fluid, different from the electric fluid, but with properties very analogous to it.[43] One year later, John Millington's lecture at the Royal Institution of London illustrated the waning support for Cartesian effluvia. For Millington, Robison's principle of magnetic induction seemed "much more rational and correct" than others. He too noted that the earth's magnetism had "long been placed beyond all doubt."[44] Although refraining from using the word "fluid", by substituting terms like "power", "principle", "influence", and "virtue," Millington preferred Aepinus' one "principle" over Coulomb's two.[45] Finally, he lamented the dearth of knowledge regarding a subject so important to a maritime nation like Britain. As we shall see in the next section, with the broad acceptance of Aepinus' theories and the changing style of experimental physics, Coulomb's theories began appearing in Britain as well.

Coulombian Theory in Britain (c. 1800-1820)

As with Aepinian theory, Coulomb's ideas did not gain quick recognition or easy acceptance in Britain. Nevertheless, growing numbers of early nineteenth-century British physicists adopted and adapted Coulomb's ideas and those of other French physicists as well.[46] These theories, however were not always endorsed wholeheartedly or understood in the way which Coulomb had originally intended. Many times they were criticized and adapted to the particular British context.

When Coulomb's work first appeared in English scientific journals at the turn of the century it tended to stress practical matters. In 1798, a paper describing Coulomb's methods of making strong artificial magnets appeared in William Nicholson's *Journal of Natural Philosophy, Chemistry and the Arts.*[47] Several years later, an article in the *Philosophical Magazine* summarized one of Coulomb's papers on methods of magnetizing steel bars. Almost all magnetic phenomena, noted the article, could be subjected to calculation, "if we suppose in steel two magnetic fluids, in each of which the moleculae repel each in the inverse ratio of the squares of the distances, and attract in the same ratio the particles of the other fluid."[48] Regardless of whether the fluids moved to the extremities of a piece of steel or merely moved within each molecule of the steel, Coulomb's calculations yielded the same results. The article explained that Coulomb had chosen the "molecular" position.

Although other British investigators at the turn of the century briefly mentioned Coulomb's experimental work, many remained unawares or simply ignorant.[49] Some failed to mention Coulomb at all, while others mentioned his name but not his theory. In early nineteenth-century Britain, Aepinian theory continued to be more widely known and accepted. For instance, John Lorimer's essay of 1800 discussed Aepinus not Coulomb.[50] In 1807, George Gregory wrote that the law of diminution of magnetic attraction remained unknown. Evidently unaware of Coulomb's work, he explained that from the subject's difficulty or insufficiently accurate experiments the question remained undecided.[51] This situation changed, albeit slowly, as the works of several Laplacians entered the picture.

In 1807, Olinthus Gregory, a Scottish-educated mathematics instructor at the Royal Military Academy, Woolwich, translated R. J. Haüy's 1803 textbook into English. He lauded Haüy's goal of being "better able to place Physics in the situation it ought to occupy," by giving attention to comparatively recent branches such as magnetism, electricity, galvanism, and crystallography.[52] Initially hoping to write a textbook himself, Gregory judged Haüy's effort far superior to anything he might have produced.

Gregory's translation exposed English readers to Laplacian experimental physics, including Haüy's discussion of magnetism and terrestrial magnetism.[53]

In Laplacian fashion, Haüy criticized Cartesian circulating fluid theories and applauded precision and calculation. Effluvialist Charles-François Dufay, he noted, presented a "machine of his own invention instead of the mechanism of nature." In contrast, Aepinus was the first to use "simple powers subjected to calculation." Haüy asserted that the precise experiments of Coulomb left no doubt that magnetic forces obeyed the inverse square law, thereby improving Aepinian theory.[54] Agreeing with the notion that magnetic fluids were confined in each iron molecule, Haüy explained that when a magnet was cut, "the effect of the whole assimilates itself to that of the component parts; and thus . . . there is no longer any thing extra-ordinary in the phenomena produced by those bodies [cut magnets] which may be termed the *polypi of the mineral kingdom*."[55] His Laplacian discussion centered on quantifying the forces of attraction and repulsion between the austral and boreal magnetic fluids.

While introducing the two-fluid theory to English readers, Gregory's translation of Haüy also exposed them to a lengthy discussion of terrestrial magnetism. Devoting more than thirty pages to the subject, Haüy began by clearly distinguishing between magnetic and electric phenomena. Contrasting the differences between earthly electricity and magnetism, he noted that electricity, a transient natural phenomenon, occurred rapidly in localized and variable circumstances, while magnetism acted universally with slow and gradual variations. Electricity and magnetism, despite their superficial similarities, were explicable in terms of distinct imponderable fluids.

Haüy also embraced the Gilbertian position. Pointing to the complex changes in magnetic declination and inclination, he argued that the terrestrial magnet acted as an irregularly-acting magnet.[56] After excluding vortical explanations, Haüy asserted that two alternatives existed. The first option supposed abundant magnetic mines located at the poles; irregular, ever-altering distribution of magnetic matter deep within these mines explained magnetic variations. The second option, entertained by Halley, Aepinus and others, considered a large globular magnet forming the nucleus of the terrestrial globe as the principal source of magnetism. For Haüy, the hypothesis of an individual magnetic nucleus had "the air of having been invented by naturalists rather to support their own theories, than to give a fair representation of nature."[57] Like Cavallo and several others, he favored the aggregate magnetic action of all moleculae composing the earth. Therefore, the magnetic centers of action, or poles, continually changed because of the irregular distribution of magnetic fluids in all parts.

Although Haüy's views on terrestrial magnetism generally agreed with the ideas of Cavallo, Robison and Young, most British investigators during the first decade of the century favored what they considered the greater simplicity of Aepinus' theories over Coulomb's two-fluid alternative. In a footnote to Haüy's text, Olinthus Gregory pointed out that many English preferred the single-fluid hypothesis of electricity. He protested that Haüy had given a "more slight and superficial" presentation of Aepinus' hypothesis than it deserved.[58] Even when aware of Coulomb's work, many British investigators favored the one-fluid alternative.

The article, "Magnetism," in the fourth edition of the *Encyclopaedia Britannica* (1810) showed a growing though still incomplete awareness of Coulomb's research. In agreement with Robison, the article argued that magnetic attractions could be caused neither "by impulsion, nor by the action of any other fluid." The analogy between electricity and magnetism naturally led to a magnetic fluid made up of two components. Accordingly, the "hypothesis of two magnetic fluids had long been a favourite on the continent, where it has been chiefly supported by Coulomb and Haüy." Agreeing that Coulomb's experiments strengthened the inverse square law of magnetic force, the article credited Robison with the actual discovery of the law.[59] Nevertheless, the experiments and observations of Coulomb entitled him to the "highest respect" and a sketch of his theory of magnetism.[60] The *Britannica*'s sketch of Coulomb's magnetic theory derived neither from Coulomb nor Haüy. Following the contents of an anonymous French abstract published in 1794, the article did not recognize the molecular confinement of the fluids.[61] When a body became magnetized, the fluids separated— "one of the fluids N, retiring towards one extremity, and the other fluid S to the other extremity of the magnetized body."[62]

In 1813 and 1814, brief English excerpts of memoirs by Laplacian physicist Siméon Denis Poisson exposed readers to mathematical refinements of Coulomb's electrical theory.[63] Although Poisson claimed that the hypothesis most generally received ascribed electrical phenomena to two fluids, an English biographical sketch of Coulomb from 1818 claimed that the doctrine of two electric fluids, at least in Britain, "had been almost unanimously renounced for the more simple doctrine" of a single fluid.[64] The author explained that until the impossibility of an explanation using a single fluid had been demonstrated, two fluids were merely a "gratuitous assumption." Such a position agreed with Robison, who wrote to James Watt fifteen years earlier, "I am unwilling to admit two electricities, since the redundance and deficiency of one does as well, and, I think, agrees better with the phenomena of electric attraction and repulsion."[65] As well, the anonymous author criticized Coulomb for assuming "imaginary data," concluding that Coulomb's electrical and magnetic researches focused more on, "the establishment or elucidation of his hypotheses than to the development of any new facts; so that, although he devoted so much of his attention to these departments, he has produced in either of them very little of what can properly be considered as discoveries."[66]

Such responses indicated the cautious acceptance of the one-fluid hypothesis and the continuing force of Scottish-British caution and inductivism. In an extreme version of this inductivist tendency, Richard Phillips noted in 1820 that there was no such thing as an electrical fluid. Calling for facts rather than theories and false analogies founded upon erroneous theories, he noted that "the philosophical electrician talks flippantly of his fluids and his fires— his negatives and his positives— his charges, surcharges, and discharges— his saturations and non-saturations— his attractions and repulsions— and other conjurations— and believes that he can bottle up this fluid *sui generis.*"[67] Similarly, a revision of Imison's *Elements of Science and Art* (1822) remarked that the cause of magnetism was "entirely unknown to us, nor has any thing farther than mere hypotheses been advanced."[68]

Blanket condemnations of hypotheses, however, came to be the exception rather than the rule as British investigators grew aware of the usefulness of the two-fluid hypothesis through the writings of Coulomb, Haüy, Poisson, and other Laplacians. For example, in 1820, Charles Bonnycastle, a mathematician at the Royal Military Academy, Woolwich, sought to add stability to the Coulombian hypothesis by showing "how ready an explanation it affords of many magnetic phenomena."[69] Important to Bonnycastle's and others' increased recognition of the two-fluid hypothesis was the research of a leading Laplacian, Jean-Baptiste Biot.

Jean Baptiste Biot (1774-1862): Mathematics and Measurement

Indicating the growing, perhaps intimidating influence of French science, an anonymous English reviewer of the *Mémoires de Physique et de Chimie, de la Société d'Arcueil*, reported in 1810, "the humiliating confessions of our national inferiority as mathematicians have not escaped the vigilance of our hereditary rivals on the continent."[70] Many of the early nineteenth-century French rivals of British scientists met every fortnight near Paris at the home and laboratory of chemist Claude Louis Berthollet to discuss and perform philosophical experiments. As the followers of Berthollet and Laplace, members of the Society of Arcueil including Étienne Malus, Joseph Louis Gay-Lussac, François Arago, Siméon Denis Poisson, and Jean Baptiste Biot stressed the Laplacian program of quantifying imponderable fluids.

The introduction of Laplacian physics into Britain furthered ongoing changes in physics which emphasized measurable, mathematical theories of electricity, magnetism, and other previously qualitative areas. Attesting to the growing French presence, Jean-Baptiste Biot's work, like Coulomb's, began appearing in English publications. As his ideas on magnetism entered British experimental physics, so too did the work of other Laplacians. Beginning with the two-fluid theory, Biot developed the first mathematical model for terrestrial magnetism. Before developing this model, he made or utilized many careful measurements of terrestrial magnetic change.

In 1804, an account of an "aërostatic voyage" by balloon appeared in the *Philosophical Magazine*.[71] Napoleon's Minister of the Interior, Jean-Antoine Chaptal, obtained government funding for a voyage undertaken by Biot and Gay-Lussac in late August of that year. The voyage primarily sought to determine whether the earth's magnetism experienced any appreciable diminution with increasing altitude. Illustrating a Laplacian fondness for measurement, Biot and Gay-Lussac's balloon came equipped with various magnetic instruments, barometers, thermometers, electrometers, hygrometers, an exhausted glass balloon for collecting air, and metallic disks for repeating some of Volta's experiments.[72]

Earlier investigators argued that magnetism vanished entirely as one ascended from the earth. If true, this fact needed to be confirmed and measured. It had particular implications for the causes of terrestrial magnetism. Using Coulomb's method of determining magnetic intensity, Biot and Gay-Lussac counted the oscillations of a magnetic needle suspended from a fine silk thread. More rapid oscillations of the needle about the magnetic meridian indicated a greater relative magnetic force. They took with them a less sensitive variation compass and dipping needle for observing the magnetic meridian and changes in inclination. To minimize local disturbances, the car of the balloon contained no iron and iron tools were kept in a basket suspended below their working area.[73]

During the ascent, Biot and Gay-Lussac determined that the balloon's slow rotary motion prevented them from aligning the variation instrument with the magnetic meridian. They soon recognized, however, that this rotary motion gradually decreased and reversed direction. At the point of reversal the car became stationary, allowing them brief periods in which to determine the magnetic intensity. Their measurements, they claimed, established that, "the magnetic property experiences no appreciable diminution from the surface of the earth to a height of 4,000 metres. Its action in these limits is constantly manifested by the same effects and according to the same laws."[74] They proposed that neither dip nor declination altered noticeably with increasing altitude. In their account Biot and Gay-Lussac did not espouse a theory of terrestrial magnetism, this was not the case in Biot's later collaboration with Prussian-born natural philosopher Alexander von Humboldt.

Alexander von Humboldt (1769-1859): More Measurement!

Humboldt's interest in the earth's magnetism dated from 1796. While serving as a mining inspector in the Fichtel Mountains, he noticed an intense magnetic anomaly caused by an outcropping of serpentine, a type of mineral. Commenting on this discovery in a letter, Humboldt set the tone for his later work, "Let us pursue the path of observation; let us collect indubitable facts. By this method the theories of natural philosophies will be established on solid and durable foundations."[75] Following this advice, he continued collecting observations of all kinds on a global scale.

Humboldt, however, wanted to go beyond simple collecting. Before leaving Europe for the New World in 1799, he expressed ambitious intentions:

> I will collect flora and fauna; I will investigate the heat, elasticity, and magnetic and electrical charge of the atmosphere, and chemically analyze it; I will determine latitudes and longitudes, and measure mountains. But all this is not the aim of my voyage. *My sole true object is to investigate the confluence and interweaving of all physical forces*, and the influence of dead nature on the animate animal and plant creation.[76]

Humboldt's search for the "confluence and interweaving of all physical forces" placed principal importance on careful, systematic measurement rather than mere collecting. Following his passion, he learned techniques of magnetic measurement from Coulomb himself and Jean Charles Borda, another Frenchman with a penchant for precise instrumentation.[77]

From 1799 to 1804, Humboldt explored much of South and Central America, amassing a multitude of plants, animals, and minerals as well as geological, meteorological, geographic and magnetic information.[78] Lacking the Laplacians' stress on mathematical analysis, he nonetheless shared their fondness for quantifying scientific data and seeking general laws. Upon returning to Europe in 1804, Humboldt settled in Paris and became a regular at the Arcueil meetings. Thereafter, he maintained close contacts with, Biot, Arago, Gay-Lussac, and other leading French scientists.[79]

Similar to the speculative tradition established in Britain, Humboldt went beyond the program of his Laplacian friends by seeking links between all terrestrial powers. In such a grand quest, the measurement of phenomena including magnetic intensity, diurnal variation, temperature, humidity, atmospheric electrical charge, and barometric pressure took paramount importance. Measuring, however, was not enough, as Humboldt complained to the Berlin Academy in 1806:

> Little has been done by traveling naturalists for the physical description of the earth, or rather for the physics of the globe (*physique du monde*), because almost all of them are concerned exclusively with the descriptive sciences and with collecting, and have neglected to track the great and constant laws of nature manifested in the rapid flux of phenomena . . .[80]

Humboldt's stress on precision instrumentation, meticulous measurement, and careful arrangement of global data, and the determination of general laws from that data had a significant impact on the study of global physics or *physique du monde*. This particular confluence of emphases has been called "Humboldtian science" by historian Susan Faye Cannon. Prevalent in Great Britain during the 1820s and 1830s, the Humboldtian style of science frequently accompanied a "cosmical" search for the interweaving of all earthly or "telluric" forces.[81] As we shall see in the final chapter, due to Humboldt's approach and several other factors, the Laplacian program witnessed its eventual demise. However, in the years before 1820, Laplacian science continued to dominate.

Humboldt and Biot: Measurement and Mathematics (1804)

After returning to Paris, Humboldt provided Biot with the magnetic data he had collected during his South American trip. This resulted in a paper read by Biot in 1804 to the mathematical and physical section of the French National Institute.[82] Quickly translated into English the following year, Biot's introduction simultaneously acknowledged the importance and uncertainty of his subject:

> An inquiry into the laws of terrestrial magnetism is no doubt one of the most important questions that philosophers can propose. The observations already made on this subject have discovered phaenomena so curious, that one cannot help endeavouring to solve the difficulties they present; but notwithstanding the efforts hitherto employed, it must be confessed that we are absolutely unacquainted with the causes of them.[83]

He blamed the poor observations on poor instruments; too little time had passed since Coulomb had rendered measurements "completely exact." Humboldt's observations, Biot explained, for the first time allowed "a series of correct facts on the variation of the magnetic forces in the northern part of the globe, and in some points of its southern part." Biot categorized terrestrial magnetism by three measurements: declination, inclination and intensity. From his balloon expedition with Gay-Lussac, he concluded that these magnetic elements acted not only on the whole surface of the globe, but beyond the earth's surface as well.

Humboldt's observations and Biot's mathematical theory entailed two innovations important for future terrestrial magnetic investigations.[84] For one, they included the first comparative measurement of magnetic intensity. Noting that intensities increased from the equator to the poles, Biot grouped Humboldt's measurements into four zones of approximately equal intensity. Because inclination varied much less than declination, Biot reasoned that the former would more easily reduce to a mathematical law. His change of emphasis, away from declination to inclination, would be followed by later investigators. Using Humboldt's data, Biot determined the position of the magnetic equator— the great circle joining points of zero inclination. With this he introduced his second innovation, a mathematical model for calculating inclinations at any point on the globe. Biot's model assumed the presence of two magnetic poles, "boreal" and "austral." These poles lay on a magnetic axis at equal distances from its midpoint located near the center of the earth. In addition, each terrestrial pole exerted inverse square forces on the ends of any magnetized needle.

In Biot's equations, a constant parameter (K) represented the poles' distance from the center of the earth. Next, he altered K's value and compared the theoretical calculations with Humboldt's inclination data. As K approached zero, the difference between theory and observation diminished, therefore, Biot concluded that the two poles were located near the center of the earth: "The most proper supposition would be to make K null, or so small that it would be needless to pay attention to it; which amounts to the same thing as to consider the two centres of action placed, as we may say, in the same molecula."[85] Although Biot's model worked relatively well for calculating inclinations, it failed to accurately predict geographic distributions of magnetic intensity or declination. Similar to his cautious stance on imponderable fluids, Biot made clear that his hypothesis should be considered not as "any thing real, but only as a mathematical abstraction useful to connect the results, and proper to ascertain in future whether any changes exist."[86] Because the causes of declination and intensity remained unknown, he claimed that anyone who successfully reduced all three magnetic measurements to a general principle would make "one of the greatest discoveries ever."[87]

While Biot attempted to extend the theory to cover all three magnetic components, he soon realized that his model did not coincide with the irregular observations. This became particularly evident after Humboldt and Gay-Lussac returned with precise magnetic measurements of dip and intensity from travels in France, Italy, Germany, and Switzerland during 1805-1806.[88] Though Biot planned an entire book devoted to terrestrial magnetism, he never published another paper on the subject.[89] Biot's model, despite its weaknesses, gained attention in Britain, particularly among mathematicians and Scottish-trained investigators.

Biot's Theory in Britain: Mathematics and Scottish Physics

Not surprisingly, Biot's theory of terrestrial magnetism initially attracted British investigators with mathematical competence. It also appealed to those who were wary of hypotheses, yet cognizant of their usefulness. As such, Biot's approach meshed in particular ways with the cautious Scottish methodological tradition. In his 1807 translation of Haüy's *Traité élémentaire de physique*, Scottish mathematician Olinthus Gregory summarized the contents of Biot's joint memoir with Humboldt in a footnote. Immediately, he noted that Biot "did not pretend to consider the hypothesis as any thing real, but solely as a mathematical abstraction useful in connecting the results."[90] Peter Barlow, like Gregory a mathematician at the Royal Military Academy, cited Biot's paper in 1814.[91] Although failing to explain its contents in any detail, Barlow apparently had read Gregory's translation of Haüy. We shall return to Barlow's magnetic researches in the final chapter.

In 1817, yet another mathematician with connections to the Royal Military Academy, Thomas Simpson Evans, treated Biot's ideas in greater depth. Evans, the master of mathematics at Christ's Hospital in London, translated the section on terrestrial magnetism in Biot's *Traité de Physique expérimentale et Mathématique* (1816).[92] Noting that the study of terrestrial magnetism had declined in Britain because of the "uncertainty attending most of its conclusions," Evans chided those who did not believe the earth's magnetism reducible to calculable laws. In the past, he noted, the complexity of lunar motions, the tides, and the orbits of comets had also defied mathematical analysis. Nevertheless, the perseverance of mathematicians had removed the obstacles and eventually solved these problems.

With these successes in mind, Evans put forth an inductive method for solving the mysteries of terrestrial magnetism:

> Empiric modes are first applied to explaining and computing the several motions; then by investigating, comparing, and gradually approximating to the observations, we come at length to causes which rest on established principles, and ultimately every apparent anomaly is accounted for, by a reasonable and satisfactory theory.[93]

Little could be done towards terrestrial magnetic theory without amassing many more observations. Urging astronomers, travelers, ships' captains, and others to publish their measurements, Evans asserted, "it is only by discussing a series of them [observations], made in a great number of places, and continued for a long period of time, that we can expect to arrive at a complete knowledge of the laws of magnetic attraction over the whole surface of the earth."[94] Indeed, he entreated commanders in the Royal Navy, the East India Company, and other public and private companies to collect dip, variation, and intensity as often as the weather permitted. Such faith that observations would eventually yield empirical laws of magnetic change contrasted sharply with the skepticism of Cavallo, Robison, and Young. As we shall see, Evans' optimism spread to other investigators during the 1820s and afterwards.

Beyond Biot's theory of terrestrial magnetism, however, broader theoretical and methodological affinities existed between Laplacian and British (and particularly

Scottish) experimental physics. For instance, Haüy's work of 1787 had similarities to the Scottish methodological positions. Like Robison, Haüy argued that legitimate theories depended on a few "facts" which pointed out a unifying structural basis for what would otherwise be a random collection of data. Also similar to the Scottish position, Haüy tentatively admitted hypotheses in certain cases, such as the Coulombian hypotheses of electricity and magnetism, without insisting on their physical reality.[95] These similarities illustrate the changing state of British experimental physics in the early nineteenth century.

Though parallels existed between Scottish and Laplacian approaches, there were differences as well, particularly regarding the proper use of mathematics. In 1818, an anonymous review of Biot's *Traité de Physique* (perhaps by Thomas Thomson) simultaneously praised and criticized the work's highly mathematical nature. Appreciative of Biot's approach, the reviewer remarked, "No one will deny the propriety of introducing mathematics into all departments of natural philosophy." Hence, in contrast to half a century earlier, mathematics had become an essential part of British experimental physics. Believing, however, in Scottish fashion that Biot had used too much algebraic notation, the reviewer decided that only "the sparing and cautious introduction of mathematical expressions into general physics" favored to the progress of knowledge.[96]

Further illustrating the Scottish recognition of Laplacian physics were French connections with the Royal Society of Edinburgh. During the first two decades of the nineteenth century, Arago, Biot, Gay-Lussac, Haüy, and Humboldt were elected honorary members of the Royal Society of Edinburgh (and, soon after, the Royal Society of London).[97] As well, several articles on experimental physics written by Laplacians appeared in Scottish encyclopedias. Published in Edinburgh between 1815 and 1824, the *Supplement to the Fourth, Fifth, and Sixth Editions of the Encyclopaedia Britannica* included Biot's "Electricity", "Galvanism" and "Pendulum" as well as Arago's "Double Refraction" and "Polarization."[98] In 1819, Biot's article "Magnetism" appeared in volume XIII of David Brewster's *Edinburgh Encyclopaedia.*[99] The inclusion of Frenchmen in British scientific societies and encyclopedias hints at the wider approval and dissemination of Laplacian ideas, particularly among Scottish natural philosophers.

Biot's 1819 *Britannica* article, "Electricity," sought to update the theoretical sections of earlier editions. He asserted that previous electrical theory was "founded on suppositions more or less doubtful; on ingenious but contracted views of the subject; and rather on empirical relations among the phenomena than on calculations rigorously mathematical."[100] Stressing quantification and mathematics, Biot wrote that in examining the interacting electric fluids, "We must endeavour, above all, to find which [laws], being susceptible of a precise and numerical value, admit of greater rigor in their verification . . . these deductions cannot be obtained but by very profound calculations, which require all the resources of [mathematical] analysis."[101] Since the development of Aepinian one-fluid electric theory, many phenomena, noted Biot, were "more accurately, and more precisely fixed, and many have been limited by exact measurements. . . . In fine, we know them by numbers, and it is in number that theory must now represent them."[102] Such statements made clear the great importance of quantification and mathematization to the Laplacian approach.

Just as Robison judged the Aepinian electric theory, Biot concluded that the Coulombian hypothesis of two fluids reproduced "exactly and numerically all the phenomena."[103] His prime concern was the descriptive value of the hypothesis. Biot allowed Coulombian hypothesis of electricity because it quantitatively embraced all or most of the phenomena, not because it necessarily reflected physical reality. This stress on developing phenomenological laws was initially true of his acceptance of other imponderable fluids as well. Such an instrumentalist stance was analogous to ideas propounded by Robison and other Scottish natural philosophers regarding hypotheses.

By emphasizing mathematics, quantification, and the utility of hypothesis, Biot approached magnetism in the same manner as electricity. In the 1819 article, "Magnetism," published in the *Edinburgh Encyclopaedia*, Biot's use of hypothesis paralleled that of the Scottish methodological tradition.[104] While confessing ignorance about the true causes of magnetism, Biot noted from the beginning, "It is necessary only, in order to proceed philosophically, to attribute to this unknown principle only the properties and qualities which are indicated, or rather rendered necessary, by the phenomena which it produces." He claimed that observations of the lodestone's attraction and repulsion warranted distinguishing between two kinds of magnetism. These two types differed, "if not in their physical essence, at least in the external and apparent mode of their action."[105] Though Biot favored Coulombian theory, his methodological leanings regarding the utility of hypotheses and status of imponderable fluids were quite similar to Robison's.

Reflecting on the nature of the two imponderable magnetic principles, Biot recognized that speculations about the physical nature of the underlying principles were unnecessary. In this realization, he paralleled a typical Scottish position once again. For instance, in 1824, Biot remarked of basic magnetic phenomena:

> What is the nature of the principle which produces these phenomena? We do not know. But whatever it might be we will define it, for the sake of conciseness, by the name of *magnetism*; it is thus that one calls *electricity* the unknown principle of the electrical phenomena, and *caloric* the no less unknown principle of heat.[106]

Nonetheless, due to Poisson's rigorous elaboration of Coulombian electrical theory in 1812, and the intimate analogy between the laws of the magnetic and electric principles, Biot supposed there was the "strongest possibility that the electrical principles are really fluids . . . [and] there is the same probability that the two magnetic principles have also a similar constitution." The cautious Biot and Robison traveled similar paths, initially disregarding the existence of imponderable fluids. After tentatively adopting fluid hypotheses, however, each man later became assured that the fluid or fluids, more or less, reflected underlying physical reality. In 1819, Biot concluded, "We are now arrived at that point to which we are permitted to penetrate in the study of nature; since, by the observation of the phenomena, we have been conducted to their laws, and from these laws to the forces by which they are produced."[107] Both Robison and Biot stressed the importance of empirical evidence, mathematical analysis, the cautious use of hypotheses, and the formulation of increasingly general laws. In the 1810s and 1820s, other British experimental physicists emphasized these elements as well, thereby continuing to combine Laplacian and Scottish ideas with their own.[108]

Conclusion

British encyclopaedia articles made apparent the shifting ideas regarding the theories and approaches to the studies of magnetism and terrestrial magnetism. By the late 1810s, internal imponderable fluids to a great degree replaced external circulating fluids; quantitative descriptions supplanted qualitative ones; and mathematical analysis was lauded over non-mathematical approaches. Despite the many remaining mysteries, theories of the internal causes of terrestrial magnetism overshadowed external effluvial and atmospheric theories. In general, most investigators contended that hypotheses could be profitable if based upon experimental facts and used cautiously.

The articles related to magnetism in Abraham Rees' *Cyclopaedia* hinted at several of these changes. Though all volumes indicated 1819, they appeared incremently in different years.[109] The *Cyclopaedia* included "Magnetism" (1812) by Cavallo; while other volumes contained several anonymous articles (perhaps by Cavallo also) including "Dipping" (1808), "Declination" (1808), and "Variation" (1817). In 1808, the author of "Declination" reported that the earth behaved as a vast magnet with all the properties of common magnets. Regarding the causes of declination itself, the article briefly explained the theories of Halley, Aepinus, and Biot all of which depended upon some type of internal magnetic nucleus.[110] The author cautioned that the notion of a moveable or immovable internal magnet seemed warranted neither by analogy nor by the coincidence of theory with observation. Mirroring Cavallo, the author argued that the true causes of the continuing alterations of the needle were the varying concurrence of heat, cold, electricity, decomposition, and derangement of materials in the earth. Hence, he concluded that magnetic variations must "be derived from these adequate causes, without recurring to suppositions purely chimerical."[111]

Also in the same volume the article "Dipping" agreed that the earth behaved as a very irregular magnet due to ferruginous parts unevenly distributed throughout the globe. Consequently, philosophers had not determined the precise positions of the earth's magnetic poles. Again paralleling Cavallo's position, gradual and uncertain variations arose "from the irregular heating and cooling, from the formation and decomposition of the different internal parts of the earth, and perhaps from other causes."[112] Optimistically, the author hoped that the dipping needle would be the principal instrument for completing a magnetic theory of the earth. In this light, it recommended the construction of accurate, inexpensive instruments and numerous observations in every part of the globe. Exhibiting French influence, "Variation" appeared in 1817. Referring readers to works which advocated Coulomb's two-fluid hypothesis, the author cited Haüy's *Traité élémentaire de physique* and Biot's *Traité de Physique.*[113] Acknowledging the complexities involved, the article concluded that all theoretical attempts to fix the exact positions of the curves of no variation must prove "entirely abortive."[114]

By 1820, most British embraced the one-fluid theory of Aepinus while fewer espoused Coulomb's two-fluid hypothesis as elaborated by Haüy, Biot, and Poisson.

Despite similarities between electricity and magnetism, the two subjects, for most investigators, retained their distinct natures. Regarding the earth's magnetism, many agreed that the terrestrial globe acted as a giant, irregular magnet. Though it was widely held that two magnetic poles or centers of force moved slowly across the earth's surface, the causes for secular variation remained a mystery. Diurnal variation, and the fluctuations accompanying the aurora borealis also remained inexplicable.

Though the unification of experimental physics did not reach fruition in the early nineteenth century, a merging of another sort did emerge. While it cannot be concluded that the roles of mathematics, experiment, and measurement in experimental physics changed uniformly from discipline to discipline or country to country, by 1820 magnetism and experimental physics as a whole had become more like mechanics. More extensive use of mathematics, improved experimental design, and precise instrumentation all contributed to make experimental physics, including magnetism, more like mechanics.[115] The theories of Aepinus and Coulomb had to determine *what* was important to measure and *how* to measure it. Hence, as scientific styles changed, precise measurement emerged as an important goal. As we have seen in the last two chapters, discussion also increased regarding the definitions and roles of hypothesis, analogy, and theory. In many of these changes the Scottish methodological tradition took the lead, eventually greatly influencing British experimental physics. The literal interpretation of Newton's *hypotheses non fingo* was no longer widely endorsed.

During the early years of the nineteenth century, the study of terrestrial magnetism remained closely connected with the general study of magnetism. In part due to the strength of the Gilbertian analogy, investigators continued considering terrestrial and controlled experimental phenomena as stemming from identical causes. Even those who did not believe the earth contained single magnetic nucleus accepted that the earth acted like a large magnet due to aggregate action of its parts. However, the years after

1820 witnessed increased speculations about the nature of magnetism which would transform both the studies of magnetism and terrestrial magnetism. It is this period that is examined in the sixth and final chapter.

Notes

[1] Because the copyright for the 1801 supplemental articles had changed hands, Robison's revised article on magnetism did not appear in the fourth edition. See Arthur Hughes, "Science in English Encyclopaedias, 1704-1875. Part I," *Annals of Science*, 7 (1951), 345.

[2] John Robison, *Elements of mechanical philosophy, being the substance of a course of lectures on that science, volume first, including dynamics and astronomy* (Edinburgh: Printed for Archibald Constable & Co . . ., 1804), 682-692. Robison called Laplace's work a "beautiful Synopsis of the Newtonian Philosophy," yet took issue with Laplace's materialistic and atheistic conclusions. In contrast to Laplace's view that gravity was an inherent quality of all matter, Robison considered that universal gravitation illustrated the wisdom and purposefulness of God.

[3] R. W. Home, "Introduction," *Aepinus's essay on the theory of electricity and magnetism* (Princeton, N. J.: Princeton University Press, 1979), 219. See also Eugene Frankel, "J. B. Biot and the Mathematization of Experimental Physics in Napoleonic France," *Historical Studies in the Physical Sciences*, 8 (1977), 39.

[4] See C. A. Coulomb, "Recherches sur la meilleure manière de fabriquer les Aiguilles Aimantées," *Memoires de mathématique et de physique presentes, Académie des Sciences, Paris*, 9 (1780), 167-264.

[5] J.-A. Nollet, *Leçons de physique expérimentale*, vol. 6 (Paris, 1764), quoted in R. W. Home, "Introduction," *Aepinus's essay on the theory of electricity and magnetism* (Princeton, N. J.: Princeton University Press, 1979), 215-216.

[6] R. W. Home,"Introduction," *Aepinus's essay on the theory of electricity and magnetism* (Princeton, N. J.: Princeton University Press, 1979), 214-215. See also C. Stewart Gillmor, *Coulomb and the evolution of physics and engineering in eighteenth-century France* (Princeton, N.J.: Princeton University Press, 1971), 177; and James R. Hofmann, *André-Marie Ampère* (Oxford, UK: Blackwell, 1995), 45-48.

[7] C. A. Coulomb, "Recherches sur la meilleure manière de fabriquer les Aiguilles Aimantées," *Memoires de mathématique et de physique presentes, Académie des Sciences, Paris*, 9 (1780) 262.

[8] Ibid. Coulomb wrote: "Si ce n'est pas la chaleur du Soleil qui produit les variations diurnes: si cependant cet effect est dû à cet Astre, il faut que le Soleil agisse sur le Globe terrestre, comme un aimant agit sur un autre aimant."

[9] Anton Brugmans (1732-1789) presented his two-fluid theory in *Tentamina philosophica de materia magnetica ejusque actione in ferrum et magnetem* (Franeker: Excudit G. Colon, 1765). Writing in apparent ignorance of Aepinus's work, Brugmans was, like Aepinus, one of the first to abandon the circulation theory of magnetism. He developed a theory analogous to, but more developed than Scot Robert Symer's (c.1701-1763) two-fluid theory of electricity. Brugmans supposed that iron was normally saturated with a neutral combination of two types of elastic fluids. Magnetization occurred when these fluids moved to opposite ends of an iron bar. The fluids' high elasticity caused its accumulations to spread into the surrounding air, thereby creating

atmospheres of one fluid or the other around magnetic poles. Rejecting the circulation of these two fluids, Brugmans explained magnetic phenomena by the interactions of the magnetic atmospheres. Home, "Introduction," *Aepinus's essay on the theory of electricity and magnetism* (Princeton, N. J.: Princeton University Press, 1979), 209. For Symer's theory, see J. L. Heilbron, "Robert Symer and the two electricities," *Isis*, 67 (1976), 7-20.

[10] Charles Augustin Coulomb, "Recherches sur la meilleure manière de fabriquer les aiguilles aimantées," (Paris, 1777), art. 23. Quoted in Gillmor, *Coulomb and the evolution of physics and engineering in eighteenth-century France* (Princeton, N.J.: Princeton University Press, 1971), 181.

[11] Charles Augustin Coulomb, "Second Mémoire sur L'Électricité et Le Magnétisme," *Mémoires de l'Academie des sciences de l'Institut de France* (Paris, 1785), 611. Earlier in the century, others including John Michell, Johann Tobias Mayer, John Robison, and Johann Lambert had stated the inverse-square relationship for magnetic forces. These efforts, however, remained either speculative or little known. Of these, Coulomb's research was the most influential in the widespread acceptance of this relationship.

[12] For a discussion of Coulomb's methods, see John Heilbron, "Weighing Imponderables and Other Quantitative Science around 1800," *Historical Studies in the Physical and Biological Sciences*, Supplement to Vol. 24, Part 1. (Berkeley: University of California Press, 1993), 67-72.

[13] Home,"Introduction," *Aepinus's essay on the theory of electricity and magnetism* (Princeton, N. J.: Princeton University Press, 1979), 219.

[14] "Exposition succinte, de quelques notions élémentaires sur l'Electricité & le Magnétisme, pour servir d'introduction à la théorie de Coulomb, relative à ces deux sciences," *Journal de Physique, de Chimie and D'Histoire Naturelle.* 45 (1794), 456. The French reads, "Coulomb ne regarde point l'existence des deux fluides magnétiques comme démontrée."

[15] Charles Augustin Coulomb, "Septième Mémoire sur L'Électricité et Le Magnétisme," *Mémoires de l'Academie des sciences de l'Institut de France* (Paris, 1789), 481. Translated in Gillmor, *Coulomb and the evolution of physics and engineering in eighteenth-century France* (Princeton, N.J.: Princeton University Press, 1971), 217. See also Charles-Augustin Coulomb, Mémoire sur L'Électricité et Le Magnétisme," *Journal de Physique, de Chimie and D'Histoire Naturelle*, 43 (1793), 272.

[16] In his latest article (1822), composed between 1801 and 1805, Robison remained unsure of the magnetic force law. Regarding Aepinus's explanation of fractured magnets, he supposed the law of action might be in a higher ratio than the squares of the distances. Therefore, the inverse-square relationship was not absolutely certain. See John Robison, *A system of mechanical philosophy*, with notes by David Brewster, (Edinburgh: J. Murray, 1822), IV: 340.

[17] Home,"Introduction," *Aepinus's essay on the theory of electricity and magnetism* (Princeton, N. J.: Princeton University Press, 1979), 222.

[18] See Robert Fox, "The Rise and Fall of Laplacian Physics," *Historical Studies in the Physical Sciences* 4 (1974), 89-136. Heilbron calls this program the "Standard Model", see J. L. Heilbron, *Weighing Imponderables and Other Quantitative Science around 1800, Historical Studies in the Physical and Biological Sciences*, Supplement to Vol. 24, Part 1. (Berkeley: University of California Press, 1993), 5-31; and John Heilbron, "Introductory Essay," in *The Quantifying Spirit in the 18th Century*, edited by Tore Frängsmyr, J. L. Heilbron, and Robin E. Rider (Berkeley: University of California Press, 1990), 4-8.

[19] See Ivor Grattan-Guinness, "Does History of Science Treat of the History of Science? The Case of Mathematics," *History of Science*, 28 (1990), 160. Grattan-Guinness gives a brief summary of developments in French science from 1800-1840. See also Ivor Grattan-Guinness, "Mathematical Physics in France, 1800-1840: Knowledge, Activity, and Historiography," in *Mathematical Perspectives: Essays on Mathematics and Its Historical Development*, edited by Joseph W. Dauben (New York: Academic Press, 1981), 95-138; and James R. Hofmann, *André-Marie Ampère* (Oxford, UK: Blackwell, 1995), 103-105.

[20] Pierre Simon Laplace, "Mémoire sur les mouvemens de la lumière dans les milieux diaphanes," *Mémoires de la Classe des Sciences Mathématiques et Physiques de l'Institut de France* 10 (1809), 329. Quoted in Robert Fox, "The Rise and Fall of Laplacian Physics," *Historical Studies in the Physical Sciences*, 4 (1974), 100 [note 35].

[21] Pierre Simon Laplace, *Exposition du système du monde* (Paris: Cercle Social, an IV [1796]), 2: 289-290, quoted in Roger Hahn, "The Laplacean View of Calculation," in*The Quantifying Spirit in the 18th Century* (Berkeley: University of California Press, 1990), 378.

[22] J. B. Biot, "Sur l'esprit du système," *Mercur de France*, 36 (1809): 247-252, quoted in Ivor Grattan-Guinness, *Convolutions in French Mathematics, 1800-1840* (Basel: Birkhäuser, 1990), vol. 1: 516.

[23] See Robert Fox, "Laplacian Physics," in *Companion to the History of Modern Science* (London and New York: Routledge, 1990), 278-280; and Robert Fox, "The Rise and Fall of Laplacian Physics," *Historical Studies in the Physical Sciences*, 4 (1974), 91-109.

[24] See René Just Haüy, *Exposition raisonnée de la theorie de l'electricité et du magnétisme, d'apres les principes de m. AEpinus* (Paris: Chez la veuve Desaint, 1787) and René Just Haüy, "Exposition raisonnée du Septième Mémoire de M. Coulomb, sur le Magnétisme," *Annales de Chimie*; 12 (1792), 27- 45.

[25] René Just Haüy, *Traité de Minéralogie* (Paris: Chez Louis, 1801), 4: 10-12.

[26] Gillmor, *Coulomb and the evolution of physics and engineering in eighteenth-century France* (Princeton, N.J.: Princeton University Press, 1971), 218. See Haüy's *Traité élémentaire de physique*, ouvrage destine pour l'enseignement dans les lycees

nationaux (Paris: Imprimerie de Delance et Lesueur, an 12, 1803) [Landmarks of science microform, 1974]. This text went through two later editions in 1806 and 1821.

[27] Bouillon-Lagrange, Edme Jean Baptiste, "Review of Haüy's *Traité élémentaire de physique*," *Annales de Chimie*, 48 (1803), 221. The French reads, "Cette théorie ne laisse rien a desirer à l'égard de la déclinaison et de l'inclinaison de l'aiguille aimantée."

[28] "Review of *A Course of Lectures on Natural Philosophy, and the Mechanical Arts*. by Thomas Young," *The Critical Review; or, Annals of Literature*, third series, 12 (Sept. 1807), 11.

[29] John Imison, *Elements of Science and Art: Being a familiar introduction to natural philosophy and chemistry* (London: J. Harding, J. Murray, . . . 1803), vol. 1: 594.

[30] Ibid., 597. See also John Imison, *Elements of Science and Art: Being a familiar introduction to natural philosophy and chemistry*, a new edition, considerably enlarged, and adapted to the improved State of Science, by Thomas Webster (London: F. C. and J. Rivington, et. al., 1822), vol. I: 396-412. Webster's update is out of date regarding magnetism, he does not mention Coulomb's work or any recent research. Only briefly mentioning Aepinus' theory, Webster commented: "The cause of magnetism is entirely unknown to us, nor has any thing farther than mere hypotheses been advanced to account for this, as well as every other species of attraction." Ibid., 412.

[31] See Crosbie Smith, "'Mechanical Philosophy' and the Emergence of Physics in Britain: 1800- 1850," *Annals of Science*, 33 (1976), 18-20. Smith argues that Robison's system of mechanical philosophy had a significant impact on Young's approach to natural philosophy. For additional information on Young's career see "Memoir of the Life of Thomas Young, M. D., F. R. S." *Edinburgh Journal of Science*, series 2, 6 (1832), 191-207; and Edgar W. Morse, "Young, Thomas," *DSB*, 14: 562-572.

[32] Thomas Young, *A Syllabus of a Course of Lectures on Natural and Experimental Philosophy* (London: 1802), Art. 493. Quoted in Geoffrey Cantor, "The Changing Role of Young's Ether," *British Journal for the History of Science*, 5 (1970), 44.

[33] For Young's largely methodological conflict with Henry Brougham over the wave theory of light, see G. N. Cantor, "Henry Brougham and the Scottish Methodological Tradition," *Studies in the History and Philosophy of Science*, 2 (1971), 69-89.

[34] Thomas Young, *A course of lectures on natural philosophy and the mechanical arts* (London: 1807) [New York: Johnson Reprint Corp., 1971], 1: 685-695.

[35] Ibid., 686.

[36] Ibid., 695.

[37] See Thomas Young, "Coulomb, (Charles Augustin)," *Supplement to the Fourth, Fifth, and Sixth Editions of the Encyclopaedia Britannica* (Edinburgh: Printed for Archibald Constable and Company, 1824), 3: 417.

[38] "Magnetism," *Encyclopaedia Britannica*, (Edinburgh: A. Bell, Fourth Edition, 1810), 380 (my emphasis).

[39] Thomas Thomson, *History of the Royal Society, from its institution to the end of the eighteenth century* (London: Printed for R. Baldwin, 1812), 459. Thomson (1773-1852) studied at the University of St. Andrews before attending the University of Edinburgh in the 1790s. Earning an M. D. in 1799, he also acted between 1796 and 1800 as assistant editor for the Supplement to the third edition of *Encyclopaedia Britannica*, contributing lengthy articles on chemistry and mineralogy. From 1813 to 1820 Thomson edited the *Annals of Philosophy*. He also lectured in chemistry at the University of Glasgow from 1817 until his death. See J. B. Morrell, "Thomson, Thomas," *DSB*, 13: 372-374.

[40] "Magnetism," *Encyclopaedia Londinensis, or Universal dictionary of the arts, sciences, literature*, compiled by J. Wilkes (London: Printed by J. Adlard, 1815), 14: 113.

[41] Ibid., 110.

[42] "Review of a Work entitled, "Essai d'un Cours élémentaire et général des Sciences physiques. Par F. S. Beudant, Sous-Directeur de Cabinet de Minéralogie du Roi, Professor de Physique dans l'Université Royale . . ." *Quarterly Journal of Science and the Arts*, 1(London: John Murray, 1816), 101-102.

[43] Charles Carpenter Bompass, *An Essay on the nature of Heat, Light, and Electricity* (London: T. & G. Underwood, 1817) [Landmarks of science microform, 1968], 245.

[44] "Report on Mr. Millington's Lectures, delivered in the Royal Institution during the Session of 1818," *Quarterly Journal of Science, Literature and Art*, 6 (1819), 75. Millington (1779-1868) began lecturing at the Royal Institution in 1815 and became professor of mechanics two years later. Until 1829, he gave lectures on natural philosophy, mechanics and astronomy at the Royal Institution. Millington, one of the original members of the Astronomical Society of London, later taught natural philosophy and chemistry at the College of William and Mary in Williamsburg, Virginia. See [R. B. Prosser], "Millington, John," *DNB*, 37: 441-442.

[45] Ibid., 79-82.

[46] For a general discussion of the French-British transmission, see Maurice Crosland and Crosbie Smith, "The Transmission of Physics from France to Britain," *Historical Studies in the Physical Sciences*, 9 (1978), 1-62. For mathematics in particular, see Joan L. Richards, "Rigor and Clarity: Foundations of Mathematics in France and England, 1800-1840," *Science in Context*, 4 (1991), 297-319.

[47] "The Method of making strong Artificial Magnets," *A Journal of Natural Philosophy, Chemistry, and the Arts*, edited by William Nicholson, 2 (1798), 80-83

[48] "Extract from a Memoir on the Degree of Magnetism acquired by Bars of Steel of different Thickness, and on some Results in regard to the Magnetic Needle," *Philosophical Magazine*, 11 (1801-02), 183.

[49] See Matthew Young, *An Analysis of the Principles of Natural Philosophy* (Dublin: The University Press, 1800) [Landmarks of science microform, 1973], 433. See also "Note on Mr. Coulomb's Experiments on Magnetism," *Journal of the Royal Institution of Great Britain*, 1 (1802), 134-135, 217; and "Experiments to prove that all Bodies, whatever may be their Nature, are obedient to the Action of Magnetism, and that this Action is sufficiently powerful to admit of being measured," *Journal of Natural Philosophy, Chemistry, and the Arts*, edited by William Nicholson, series 2, 2 (1802), 143-144.

[50] John Lorimer, *A concise essay on magnetism* (London: Printed by J. Dillon, 1800), 11.

[51] George Gregory, *A Dictionary of Arts and Sciences* (London: Richard Phillips, 1807), 94.

[52] René Just Haüy, *An elementary treatise on natural philosophy*, translated from the French by Olinthus Gregory (London: George Kearsley, 1807), translator's preface, vi-vii. An edition of Gregory's popular science textbook published in 1799 did not include a discussion of magnetism. See Olinthus Gregory, *Lessons Astronomical and Philosophical, for the Amusement and Instruction of British Youth*, second edition, much enlarged and improved (London: Bye and Law, 1799).

[53] René Just Haüy, *An elementary treatise on natural philosophy*, translated from the French by Olinthus Gregory (London: George Kearsley, 1807), 59. Gregory (1774-1841) succeeded Charles Hutton as Professor of Mathematics at Woolwich in 1807 and continued lecturing until 1838. With an A.M. (1805) and LL.D. (1808) from the University of Aberdeen, Gregory published books on many topics including mathematics, mechanics, astronomy, steam engines, and theology. He also helped Dr. John Mason Good compile *The Pantologia*, a twelve volume dictionary of arts and sciences (1802-13). In fact, most of the *Pantologia's* article on magnetism came directly from Gregory's translation of Haüy. See "Magnetism," *The Pantologia, A new encyclopaedia comprehending a complete series of essays, treatises & systems alphabetically arranged* (London: Kearsley, 1813), n. p.. For more on Gregory see W. Johnson, "The Woolwich Professors of Mathematics, 1741-1900," *Journal of Mechanical Working Technology*, 18 (1989), 165-167.

[54] R. J. Haüy, *An elementary treatise on natural philosophy*, translated from the French by Olinthus Gregory (London: George Kearsley, 1807), 60.

[55] Ibid., 90-91.

[56] Ibid., 111.

[57] Ibid., 117.

[58] R. J. Haüy, *An elementary treatise on natural philosophy*, translated from the French by Olinthus Gregory (London: George Kearsley, 1807), note (h) by Gregory, 382.

[59] "Magnetism," *Encyclopaedia Britannica*, (Edinburgh: A. Bell, Fourth Edition, 1810), 385.

[60] Ibid., 394.

[61] See "Exposition succinte, de quelques notions élémentaires sur l'Electricité & le Magnétisme, pour servir d'introduction à la théorie de Coulomb, relative à ces deux sciences," *Journal de Physique, de Chimie and D'Histoire Naturelle*, 45 (1794), 448-456. The summary reads as if translated directly from the anonymous French author point for point. For instance, "2. The particles of each of these two fluids are mutually repulsive of each other; that is, the particles of the fluid N mutually repel each other, and the particles of the fluid S repel each other." Compare with the French, "Première Proposition. Les molécules de chacun de ces deux fluides se repoussent entr'elles. Les molécules du fluide N se repoussent entr'elles. Les molécules du fluide S se repoussent entr'elles." And later, "11. A magnetic needle being broken in any place, each of its parts is found to have two poles." Compare with the French, "X. Proposition. Une aiguille magnétique étant brisée dans un endroit quelconque, chacune des parties se trouve avoir ses deux poles."

[62] See also Andrew Horn, "On Magnetism," letter to Dr. [Thomas] Thomson, *Annals of Philosophy*, series 1, 9 (1817), 139. Following the 1810 *Britannica* article, Horn explained that two fluids, austral and boreal, accumulated at the extremities of a magnetic bar. Unaware of Coulomb's notion of the molecular confinement of the fluids, the difficulty of a cut magnet remained "a serious objection to the theory of Coulomb."

[63] For more on Poisson's electrical research and the internal politics of French science see, R. W. Home, "Poisson's Memoirs on Electricity: Academic Politics and a New Style in Physics," *British Journal for the History of Science*, 16 (1983), 239-259.

[64] Siméon-Denis Poisson, "Memoir on the Distribution of Electricity on the Surface of Conductors," *Annals of Philosophy*, series 1, 1 (1813), 152. See also Siméon-Denis Poisson, "Second Memoir on the Distribution of Electricity on the Surface of Conductors," *Annals of Philosophy*, series 1, 3 (1814) 391-392. "Biographical Sketch of Charles Augustin Coulomb," *Annals of Philosophy*, series 1, 12 (1818) 83.

[65] Robison to Watt, Dec. 20, 1803, in Eric Robinson and Douglas McKie (eds.), *Partners in Science: Letters of James Watt and Joseph Black* (Cambridge, Mass.: Harvard University Press, 1970), 384.

[66] Ibid., 84.

[67] Sir Richard Phillips, "Electricity and Galvanism explained on the mechanical Theory of Matter and Motion," *Philosophical Magazine*, 56 (1820), 195.

[68] John Imison, *Elements of Science and Art: Being a familiar introduction to natural philosophy and chemistry*, a new edition, considerably enlarged by Thomas Webster (London: Printed for F. C. and J. Rivington, 1822), vol. 1: 412.

[69] Charles Bonnycastle, "On the Distribution of the Magnetic Fluids in Masses of Iron: : and on the Deviations which they produce in Compasses placed within their Influence," *Philosophical Magazine*, 55 (1820), 446.

[70] "Review of *Mémoires de Physique et de Chimie, de la Société d'Arcueil*, vol. I. Paris, 1807, and Vol. II. 1809," *The Quarterly Review*, 3 (1810), 464. Over the next several decades British scientists including Charles Babbage and David Brewster complained that their national science had declined in comparison to French achievements. These "declinist" arguments played an important role in the formation of the British Association for the Advancement of Science (1830/31). See A. D. Orange, "The Origin of the British Association for the Advancement of Science," *British Journal for the History of Science*, 6 (1972), 152-176.

[71] Jean-Baptiste Biot and L. J. Gay-Lussac, "Account of an Aërostatic Voyage performed by Messrs. Guy-Lussac and Biot," *Philosophical Magazine*, 19 (1804), 371-379. Translated from J. B. Biot and L. J. Gay-Lussac, "Relation d'un voyage aérostatic," *Journal de Physique, de Chimie and D'Histoire Naturelle*, 59 (1804), 314-320.

[72] Ibid., 372.

[73] Ibid.

[74] Ibid., 375. Biot and Gay-Lussac spoke of a future voyage which would rise to 6,000 meters making similar measurements, and in fact Gay-Lussac made a solo ascent in September, 1804 to reach a height of 7,000 meters. See L. J. Gay-Lussac, "Relation d'un voyage aérostatic," *Journal de Physique, de Chimie and D'Histoire Naturelle*, 59 (1804), 454-462; L. J. Gay-Lussac, "Account of an Aerostatic Voyage, made by M. Gay-Lussac," *Journal of Natural Philosophy, Chemistry, and the Arts*, series 2, 10 (1805) 278-288; and M. P. Crosland, "Biot, Jean Baptiste," *DSB*, 2: 134-135.

[75] Alexander von Humboldt, "A Letter from M. de Humboldt to M. Pictet, on the Magnetic Polarity of a Mountain of Serpentine," *A Journal of Natural Philosophy, Chemistry, and the Arts*, 1 (1797), 100.

[76] Alexander von Humboldt quoted in Michael Dettelbach, "Global physics and aesthetic empire: Humboldt's physical portrait of the tropics," in *Visions of Empire: Voyages, botany, and representations of nature*, edited by David Philip Miller and Peter Hanns Reill (Cambridge: Cambridge University Press, 1996), 266 (my emphasis).

[77] Eugene Frankel, "J. B. Biot and the Mathematization of Experimental Physics in Napoleonic France," *Historical Studies in the Physical Sciences*, 8 (1977), 53.

[78] Alexander von Humboldt, "Extract of a Letter from M. von Humboldt to Lalande," *Philosophical Magazine* 11 (1801-1802) 355- 361. See also Peter Bowler, *The Norton History of The Environmental Sciences* (New York: W. W. Norton & Company, 1992), 206-207; and S. R. C. Malin and D. R. Barraclough, "Humboldt and the Earth's magnetic field," *Quarterly Journal of the Royal Astronomical Society*, 32 (1991), 279-281.

[79] For more on Humboldt's Parisian experience see L. Kellner, *Alexander von Humboldt* (London: Oxford University Press, 1963), 66-88. See also L. Kellner, "Alexander von Humboldt and the organization of international collaboration in geophysical research," *Contemporary Physics*, 1 (1959), 35-48.

[80] Alexander von Humboldt, "Beobachtungen über das Gesertz der Wärmeabnahme in den höhern Regionen der Atmosphäre, und über die untern Gränzen des ewigen Schnees," *Annalen der Physik*, 24 (1806), 2-3, quoted in Michael Dettelbach, "Global physics and aesthetic empire: Humboldt's physical portrait of the tropics," in *Visions of Empire: Voyages, botany, and representations of nature*, edited by David Philip Miller and Peter Hanns Reill (Cambridge: Cambridge University Press, 1996), 260.

[81] See Susan Faye Cannon, *Science in Culture: The early Victorian period* (New York: Science History Publications, 1978), 73-110.

[82] Alexander von Humboldt and Jean-Baptiste Biot, "Sur Les Variations du magnétisme terrestre a différentes latitudes," *Journal de Physique, de Chimie and D'Histoire Naturelle* (1804), 429-450.

[83] Alexander von Humboldt and Jean-Baptiste Biot, "On the Variation of the Terrestrial Magnetism in different Latitudes," *Philosophical Magazine*, 22 (1805), 248.

[84] Eugene Frankel, "J. B. Biot and the Mathematization of Experimental Physics in Napoleonic France," *Historical Studies in the Physical Sciences*, 8 (1977), 54.

[85] Alexander von Humboldt and Jean-Baptiste Biot, "On the Variation of the Terrestrial Magnetism in different Latitudes," *Philosophical Magazine*, 22 (1805), 301. Biot's model was similar to that developed earlier by German astronomer Johann Tobias Mayer (1723-1762) in 1760. Mayer too conjectured an infinitely small magnet located near the earth's center and supposed the force varied inversely as the cube of the distance. In any case, Mayer's long unpublished writings gained infrequent notice in Britain and elsewhere. See also Gregory A. Good, "Follow the Needle: Seeking the Magnetic Poles," *Earth Sciences History*, 10 (1991), 160; and Eric Forbes, *The unpublished Writings of Tobias Mayer*, vol. III. *The Theory of the Magnet and its application to terrestrial magnetism* (Göttingen: Vandenhoeck & Ruprecht, 1972).

[86] Alexander von Humboldt and Jean-Baptiste Biot, "On the Variation of the Terrestrial Magnetism in different Latitudes," *Philosophical Magazine*, 22 (1805), 308.

[87] Ibid.

[88] "Scientific News. French National Institute," *Journal of Natural Philosophy, Chemistry, and the Arts*, series 2, 17 (1807), 369-371. See also Alexander von Humboldt and L. J. Gay-Lussac, "Sur l'intensité et l'inclinaison des forces magnétiques, faites en France, en Suisse, en Italie et en Allemagne," *Mémoires de physique et de chimie, de la Société d'Arcueil*, 1 (1807), 1-22 and "Abstract of Observations on the Intensity and Inclination of the Magnetic Force, made in France, Switzerland, Italy, and Germany by Messieurs Humboldt and Gay-Lussac," *Edinburgh Review*, 15 (1809), 143-146.

[89] Eugene Frankel, "J. B. Biot and the Mathematization of Experimental Physics in Napoleonic France," *Historical Studies in the Physical Sciences*, 8 (1977), 56.

[90] Olinthus Gregory, Footnote (n) in René Just Haüy, *An elementary treatise on natural philosophy*, translated from the French by Olinthus Gregory (London: George Kearsley, 1807), 122.

[91] Peter Barlow, *A New Mathematical and Philosophical Dictionary* (London: Printed by Whittingham and Rowland, 1814), art. variation or declination of the magnetic needle.

[92] Thomas Simpson Evans, "On Terrestrial Magnetism, with a translation of J. B. Biot, 'On the Laws of Terrestrial Magnetism in different Latitudes,'" *Philosophical Magazine*, 49 (1817), 3-16, 95-103. See also J. B. Biot, "Lois du Magnétisme terrestre à différentes latitudes," *Traité de Physique expérimentale et Mathématique* (Paris: Chez Deterville, 1816), 3: 127-145. Evans (1777-1818) the eldest son of Rev. Lewis Evans, mathematical master at the Royal Military Academy. After working at the Royal Observatory, Greenwich from 1800-1805 as an assistant to Nevil Maskelyne, Evans taught mathematics at the Royal Military Academy (1802-1810) and the mathematical school at New Charlton before moving in 1813 to Christ's Hospital, London. See N. S. Kiernan, "Thomas Simpson Evans, 1777-1818," *Journal of British Astronomical Association*, 88 (1978), 365-370 and [Gordon Goodwin], "Evans, Thomas Simpson," *DNB*, 18: 75.

[93] Ibid., 4.

[94] Ibid., 6.

[95] R. J. Haüy, *Exposition raisonée de la théorie de l'électricité et du magnétsime*, d'apres les principes de M. Aepinus (Paris: Chez la Veuve Desaint, 1787), viii-ix. See also James R. Hofmann, *André-Marie Ampère* (Oxford, UK: Blackwell, 1995), 109-110.

[96] "Analyses of Books: *Traité de Physique expérimentale et Mathématique* appeared in the *Annals of Philosophy*," *Annals of Philosophy*, 11 (1818), 59.

[97] *Transactions of the Royal Society of Edinburgh*, 7 (1815), 542-546. Biot, Humboldt, and Gay-Lussac were also elected Foreign Members of the Royal Society of London in 1815. Arago soon followed in 1818.

[98] Paul Kruse, *The Story of the Encyclopaedia Britannica, 1768-1943* (Ph.D. Dissertation, University of Chicago, 1958), 127-139. Arago's articles were translated by Thomas Young, Arago's treatment of polarization also included notes by Young.

[99] J. B. Biot, "Magnetism," *The Edinburgh Encyclopaedia*, conducted by David Brewster (Edinburgh: Printed for William Blackwood, 1830), 13: 246-278. This volume was originally published in 1819.

[100] J. B. Biot, "Electricity," *Supplement to the Fourth, Fifth, and Sixth Editions of the Encyclopaedia Britannica* (Edinburgh: Printed for Archibald Constable and Company, 1824), 4: 76. Volume four of the *Supplement* originally appeared in 1819-20.

[101] Ibid., 81.

[102] Ibid., 93.

[103] Ibid., 92.

[104] J. B. Biot, "Magnetism," *The Edinburgh Encyclopaedia*, conducted by David Brewster (Edinburgh: Printed for William Blackwood, 1830), 13: 246-278. Sections of this article had appeared previously in J. B. Biot, *Precis élémentaire de physique expérimentale*. Paris: Deterville, 1824 (Third Edition) tome II; and its English translation, John Farrar, *Elements of Electricity, Magnetism, and Electro-Magnetism, embracing the late discoveries and improvements* (Cambridge, New England: Printed by Hillard and Metcalf, 1826), 193-304.

[105] Ibid., 246.

[106] J.-B. Biot, *Precis élémentaire de physique expérimentale*, third edition (Paris: Deterville, 1824), ii, 2.

[107] J. B. Biot, "Magnetism," *The Edinburgh Encyclopaedia*, conducted by David Brewster (Edinburgh: Printed for William Blackwood, 1830), 13: 277.

[108] Influences from France to Britain were not uni-directional. Both Continental and British investigators had an impact on Biot. His concluding list of the principal sources on magnetism included Aepinus and Coulomb as well as Gilbert, Cavallo, and Robison. In the Gilbertian tradition, he believed that the earth influenced magnetic needles "exactly like a true magnet," owing to deposits of magnetic substances and iron. Furthermore, the earth's magnetic action produced dip and polarity in "precisely the same manner" as those brought about by any other lodestone. Ibid., 278.

[109] "Notices respecting New Books. *The Cyclopaedia; or, Universal Dictionary of Arts, Sciences, and Literature* by Abraham Rees," *Philosophical Magazine*, 56 (1820), 218.

[110] "Declination," *The Cyclopaedia*, 11 (1819), n. p.

[111] Ibid., n. p.

[112] "Dipping, in Magnetics," *The Cyclopaedia*, 11 (1819), n. p.

[113] "Variation, in Geography, Navigation, etc.," *The Cyclopaedia*, 36 (1819), n. p.

[114] Ibid.

[115] Robert H. Silliman, "Fresnel and the Emergence of Physics as a Discipline," *Historical Studies in the Physical Sciences*, 4 (1974), 137-143.

CHAPTER 6:
CONCLUSION:
NATURE'S FORCES UNITED?
(c.1800-1835)

The initial third of the nineteenth century witnessed considerable changes in the scope and intensity of magnetic research and geomagnetic measurement in Great Britain. As discussed in chapter two, the discovery of local attraction and renewed attempts to find a Northwest Passage specifically contributed to these latter changes. Arctic expeditions allowed the Royal Navy to perform experiments with respect to local attraction, collect unusual magnetic data, and search for the earth's north magnetic pole. Accompanying these efforts, investigators designed, constructed, and utilized increasingly sensitive instrumentation to determine secular, seasonal, and diurnal magnetic variations. Newly-designed instruments also recorded violent, irregular oscillations of the needle called "magnetic storms." Furthermore, investigators often looked for interactions between magnetism and other geophysical phenomena, including the aurora borealis.

In the 1820s, collecting magnetic data in Britain grew in scope and became more standardized than in 1800. In contrast to the early decades of the century, many investigators after 1820 deemed the precise measurement of three magnetic elements or components — variation, dip, and intensity — essential for improving the theoretical understanding of earthly magnetism. However, despite the improved instruments and the widened scope, the study of terrestrial magnetism retained unanswered questions. Why did the earth act like a giant magnet? Did it have two poles or four poles, and how did they move? By what mechanisms did terrestrial magnetism vary geographically and temporally? Could these changes be predicted? While continuing to push for accumulated global observations, investigators of the 1820s and later appealed to newly emerging experimental results to speculate about answers to such questions.

Hence from 1820 onward, a surge in experimentation became a third major factor affecting the understanding of magnetism and terrestrial magnetism as well. In 1820, the discovery of electromagnetism by Danish natural philosopher, Hans Christian Oersted (1777-1851), sparked a European-wide torrent of experimental activity. In Britain, a count of articles related to magnets or magnetism in the *Philosophical Transactions of the Royal Society of London* illustrates the post-1820 explosion:

1781-1820 (vol. LXXI to vol. CX)	14 articles[1]
1821-1830 (vol. CXI to vol. CXX)	39 articles[2]

Hence, in the *Philosophical Transactions* alone, nearly three times as many entries appeared in the 1820s as had during the previous four decades. Furthermore, the focus of the articles shifted. From 1781 to 1820, ten of the fourteen articles dealt with either measurements of variation and dip (e. g., Gilpin, Sabine) or the study of local attraction (e. g., Flinders, Scoresby). In contrast, the 1820s had nine entries dealing with magnetic observations (e. g., Parry, Foster, Sabine) while most of the remaining thirty stressed experiments rather than observations (e. g., Davy, Barlow, Christie).

In addition to these quantitative and qualitative changes, the wave of magnetic-related experimentation transformed theoretical views on the subject. Investigators adapted the existing theories or developed new ones to account for the wealth of emerging experimental effects. In doing so, many appealed to connections between various phenomena. Although unclear about how these links operated, many physicists continued to speculate on the matter. With these things in mind, this chapter discusses the new experimentation, the theories that accompanied it, and the application of these ideas to terrestrial magnetism.

Imponderable Fluids Questioned

As discussed in the previous chapter, distinct imponderable fluids, by the late 1810s, became the most accepted way of accurately describing magnetism, electricity, and many other phenomena. Whether such imponderables actually existed or not, they allowed mathematical, quantitative descriptions of the phenomena. Therefore, with the ongoing transformation of experimental physics, such theories had desirable characteristics which the circulating fluid theories lacked. Though widely advocated in both Britain and France, the endorsement of magnetic and electric fluid theories also saw clear divisions along national lines. Following Robison's lead, many British physicists, even into the 1830s, endorsed some form of the one-fluid hypothesis. Across the English Channel, however, most French physicists embraced the Coulombian two-fluid hypothesis.

While one-fluid theories dominated in Britain, the growing influence of French science and the success of Poisson's two-fluid electric theory (1812) garnered support for Coulombian theories as well. In British scientific journals and encyclopedias, and in English translations of French works, the ideas of Haüy, Biot, and Poisson presented Coulombian electrostatics and magnetostatics to a wider English audience.[3] Regardless of their preference for one or two fluids, investigators endorsed Coulomb's inverse-square law as a triumph of Newtonian physics. By quantifying magnetism and electricity in a manner analogous to universal gravitation, Coulombian theories brought these sciences closer to the model science of mechanics.

In spite of the success of imponderable fluid theories, they did not go unquestioned or unchallenged. First, acceptance differed for particular imponderables. For example, while the magnetic fluid or fluids were not observed, the electric fluid or fluids could be seen transferring from one body to another. Hence, some argued that the electric fluid was better established than the magnetic fluid. Second, some investigators considered imponderables only as useful hypotheses, contending that they did not reflect physical reality. In 1809, Biot clearly stated that French physicists considered fluids "merely as convenient hypotheses to which they take care not to attach any ideas of reality, and which they are ready to modify or abandon completely as soon as the facts are shown contrary to them."[4]

As more facts appeared, Biot and others spoke of certain imponderables as if they really existed. Despite this growing confidence, however, other investigators continued questioning the ontological status of imponderable fluids. Did they exist? Were they necessary? As early as 1791, Scottish mathematician John Leslie answered both these questions negatively. Seeing no advantage in admitting the electric fluid's existence, Leslie remarked, "what was presumed to be the electric fluid, is only air endued with certain properties. The existence of such a fluid is therefore illusory; it is unnecessary, and inconsistent even with mechanical principles."[5] Similarly criticizing the magnetic fluid, he noted that the gratuitous properties ascribed to it were "more complex than the facts which they are intended to explain."[6] While Leslie remained in the minority in his outright rejection of imponderables, he and others (e. g., Count Rumford and Richard Phillips) preferred reducing many phenomena of experimental physics to particular conditions, states, or movements of ordinary ponderable matter.[7]

Another objection to imponderable fluids was that they trespassed upon the principle of simplicity. Writing in 1809, English chemist Humphry Davy complained:

> Vulgar idea- like that of the peasant, every thing done by a spring; so every thing must be done by a fluid. The ether was the ancient fluid; then there was a phlogistic fluid; we have had the magnetic fluid, the vitreous fluid, the resinous fluid; and within the last few years there has been a fluid of sounds; and, in a book, which I lately received from France . . . all the phenomena of nature are explained by gravic fluid.[8]

The plethora of imponderables perturbed Davy and others who stressed explanations founded upon nature's simplicity. Perhaps, if seemingly disparate phenomena depended on simpler underlying principles or powers, the number of imponderables might be reduced or even be eliminated entirely from the domain of experimental physics. Or as Leslie and others suggested, maybe the phenomena were reducible to the motions of ordinary matter, foregoing the need for special imponderable entities.

As we have seen, many eighteenth-century British natural philosophers, Newton included, speculated about unifying principles (e. g., the ether) encompassing a great diversity of natural phenomena. Many agreed that these notions required strong empirical foundations and even the most cautious expected future discoveries to bear fruit. As discussed in the third chapter, eighteenth-century British investigators embraced notions of nature's simplicity, pointing out suggestive analogies between different phenomena (e.g., electricity and magnetism, heat and light). As well, many appealed to empirical links between phenomena (e. g., lightning's effects on magnetic needles, electric discharges producing heat and light, the effects of heat upon magnets). Despite these conjectures, however, other arguments supported the distinctness of certain phenomena, particularly electricity and magnetism. For this reason, imponderable fluid theories retained their high degree of acceptance in Britain and elsewhere. More importantly, imponderables were too successful at accurately describing the phenomena to simply abandon them.

Galvanism, the Voltaic Pile, and Electrochemistry

At the turn of the century, several developments, which later influenced magnetic research and magnetic theories, led to a transformation in the study of electricity. While studying animal irritability in the 1780s, Italian anatomist Luigi Galvani (1737-1798) revealed another manifestation of electricity.[9] Noting the convulsion of a frog's leg when touched with his dissecting scalpel, he believed that a form of "animal electricity" residing in the tissues of the freshly-killed frog had been discovered. Continuing research on this "galvanic" effect, Galvani realized that two metals (e. g., zinc and copper) completed a circuit with the interposed frogs' muscles and nerves. He argued from further experiments that the electricity originated not from the metals, but from the movement of electric fluid within the frog itself.

As numerous European investigators pursued galvanic research, they debated the origins of galvanism and whether the galvanic and electric fluids were the same. Contrary to Galvani's theory of animal electricity, Italian physicist Alessandro Volta (1745-1827) concluded in 1800 that animal electricity resided not in the frog's tissues, but in the different metals in contact with one another.[10] Therefore, the frog's leg acted only as a sensitive detector of electricity, not its source. Volta's research excited widespread experimentation with ever-more powerful "Voltaic piles" or batteries. It also led Volta to the contact theory of the pile, supposing that merely a series of dissimilar metals separated by conducting material (e. g., moistened cardboard) generated electricity.

Though Volta, Biot, and many others explained galvanism in terms of a rapid series of electrostatic discharges, experimenters manipulated the continuous current of electricity in ways not possible with the Leyden jar and other earlier apparatus.[11] The constant current of the Voltaic pile elicited new experimental effects. For instance, in the spring of 1800, William Nicholson and Anthony Carlisle (1768-1840) at the Royal Society of London decomposed water using electricity generated by a powerful pile. Developing a more efficient pile utilizing a single metal and acid, Humphry Davy supposed, in opposition to Volta's contact theory, that chemical changes in the pile were the cause of electrical effects.[12] Such investigations gave rise to the chemical theory of the pile and the new field of electrochemistry.[13]

While performing electrochemical research, Davy and others supposed close links between animal electricity, static discharges, and voltaic electricity.[14] For many, these phenomena hinted at modifications of a single underlying power. In 1801, Volta asserted the identity of the galvanic and electric fluids, while English chemist William Hyde Wollaston (1766-1828) concurred that electricity and galvanism were essentially the same, and confirmed "an opinion that has already been advanced by others, that all the differences discoverable in the effects of the latter, may be owing to its being less intense, but produced in much larger quantity."[15] Edinburgh-educated physician John Bostock wrote the following year that "there can scarcely remain any reasonable doubt of its [galvanism's] perfect identity with the electric fluid."[16] In *Elements of Galvanism* (1804), Dublin lecturer C. H. Wilkinson concluded that most philosophers "considered the galvanic fluid as identical with the electric fluid."[17] Indicating the importance of this subject, the class of physical and mathematical sciences of French Institute offered as a first prize subject in 1805:

> The electric and galvanic fluids offer so many points of analogy, and so great a number of different effects, that many philosophers believe them to be identical, and many others make them two distinct fluids:
>
> *New experiments are required which shall decide, in a definitive manner, on their identity or diversity.*[18]

Although many readily accepted the identity of galvanism and electricity, the debates continued. Not until the 1830s did Michael Faraday firmly established the equivalence of these and several other kinds of electricity.[19]

In addition to the possible sameness of electric and galvanic fluids, investigators continued grappling with the mysterious links between electricity and magnetism. While many treated these phenomena as distinct, in the years after 1820, the distinctions between magnetism, electricity, and other areas of experimental physics became less definite. New evidence and theories challenged the dominance of distinct imponderable fluids. Some reduced the numbers of fluids or modified their actions, while others rejected imponderables outright. As this chapter demonstrates, the experimentation of the 1820s and 1830s altered the understanding of magnetism and helped transform the understanding of global magnetism as well.

Ritter and Oersted: Natural Philosophical joins Experimental

While nineteenth-century British investigators continued entertaining unifying speculations, similar notions were enthusiastically put forth on the Continent. In particular, early in the century, ideas stemming from the Romantic German school of *Naturphilosophie* gathered a small, yet receptive, British audience.[20] Though such ideas meshed well with the British speculative tradition, they lacked enough empirical evidence to convince most British investigators. In 1802, a letter published in the *Journal of Natural Philosophy, Chemistry, and the Arts* suggested that German philosophers and chemists identified the electric, galvanic, and magnetic fluids as one and the same.[21] Two years later, the same journal reported the research of German physicist Johann Wilhelm Ritter (1776-1810) in an abstract written by Ritter's friend and colleague, Danish natural philosopher Hans Christian Oersted (1777-1851).[22]

Influenced by Immanuel Kant's philosophy of science as well as *Naturphilosophie*, Ritter, Oersted, and many other German and Scandinavian natural philosophers believed in the ultimate interrelatedness of natural processes and the polarity of all forces.[23] German *Naturphilosophie*, or simply "nature philosophy", followed in the natural philosophical tradition as an all encompassing, partly *a priori*, explanation of nature. The leading *Naturphilosoph*, Friedrich Schelling (1775-1854), asserted in 1799:

> There is no question but that much in the science of Nature may be known comparatively *à priori*; as, for example, in the theory of the phenomena of electricity, magnetism, and even light. There is such a simple law recurring in every phenomenon that the results of every experiment may be told beforehand; here my knowing follows immediately from a known law, without the intervention of any particular experience. But whence then does the law itself come to me? The assertion is, that all phenomena are correlated in one absolute and necessary law, from which they can all be deduced; in short, that in natural science all that we know, we know absolutely *à priori* . . . By this deduction of all natural phenomena from an absolute hypothesis, our knowing is changed into a construction of Nature itself, that is, into a science of Nature *à priori*.[24]

Although Ritter, Oersted, and other *Naturphilosophen* rejected Schelling's strictly *a priori* "science of Nature," the idea that natural phenomena manifested themselves as different forms of basic underlying powers remained a dominant driving force in their experimental research.[25] Oersted, for example, speculated in 1803:

> The constituent principles of heat which play their role in the alkalis and acids, in electricity, and in light are also the principles of magnetism, and thus we have the unity of all forces which, working on each other, govern the whole cosmic system, and the former physical sciences thus combine into one united physics . . . Our physics would thus be no longer a collection of fragments on motion, on heat, on air, on light, on electricity, on magnetism, and who knows what else, but we would include the whole universe in one system.[26]

For Oersted, Ritter, and others the speculative and experimental joined together in the search for this grand underlying system.

Seeking for hidden relationships which necessarily existed, Ritter persistently compared electric, magnetic, galvanic, and chemical effects in his experimental work. Several English reports published in 1803 and 1804 discussed his comparisons of galvanism with static electricity.[27] Ritter's comparisons extended to geophysical forces, for instance, in 1806, the *Philosophical Magazine* included an extract of a letter in which Ritter asserted that the earth was a "Voltaic column of enormous size."[28] Expanding on Ritter's notion, the fourth edition (1801-1810) of the *Encyclopaedia Britannica* explained:

> the earth considered as a magnet, may be taken as an equivalent to an immense pile of Volta, of which the poles are on one side sufficiently closed by the waters of the ocean. And the action of this pile must produce, and has produced the greatest chemical changes in the materials of the earth. . . . The foregoing experiments appear to prove that magnetism has some effect in producing chemical changes.[29]

The article noted that Ritter also illustrated a close analogy between magnetism and "that modification of electricity which we call galvanism."[30] In numerous experiments he reported galvanic palpitations in frogs when uniting a magnetized iron wire with a non-magnetic wire. Ritter further claimed that a louis d'or and a gold needle became magnetized when placed in a galvanic circuit, and that certain arrangements of magnetic wires generated galvanic phenomena. Although these results were contentious, summaries of Ritter's research continued appearing in British encyclopedia articles including the *Encyclopaedia Londinensis* (1815) and in the fifth (1811-1817) and sixth (1819-1823) editions of the *Encyclopaedia Britannica*.[31]

Though recognized, Ritter's work never received much serious attention in Britain. Humphry Davy wrote of Ritter in 1820, "His ideas are so obscure that it is often difficult to understand them."[32] Peter Barlow similarly recalled in 1824 that Ritter's experiments "were never much regarded."[33] In 1826, Davy again recalled the "wild views" and "inexact experiments" of Ritter, while a year later, Secretary of the Royal Society of London, Peter Mark Roget, noted that Ritter's research had "attracted little attention, and certainly threw but little light upon the connexion between electricity and magnetism."[34] Also in 1827, Cambridge professor of chemistry James Cumming recalled Ritter's writing style "so obscure, and he abounds so much in hypothesis, that few, if any of his experiments were repeated either in France or England; and his inferences from them . . . were long disregarded."[35] In any case, an early death in 1810 prevented Ritter from continuing his highly speculative research.

Further attesting to the difficulties of Ritter's work, Oersted remarked in his autobiography of his friend's submission to a 1802 French Institute prize competition:

> [Ritter] wrote a paper on the subject [i. e., a voltaic storage device] in his customary obscure style, and requested Oersted to translate it. Word for word, it was impossible. He rewrote it entirely as a French dissertation, which Ritter later declared he understood better than his own. No one received the prize that year, since the French Institute believed Ritter's experiments were not any more significant than many older ones.[36]

Before becoming professor extraordinary at the University of Copenhagen in 1806, Oersted acted as Ritter's spokesman in Paris (1802-1804), reporting Ritter's various experiments. Much to his embarrassment, however, many of Ritter's results could not be reproduced by Parisian experimenters.[37] Recognizing this, Oersted cautioned in 1804:

> These facts [i. e., Ritter's] . . . are neither numerous enough, nor sufficiently conclusive, to compose a complete theory. A series of experiments, exhibiting the magnetic needle in all its relations to electricity, at present better known by means of the [Voltaic] pile, would undoubtedly throw much light on a subject heretofore so obscure.[38]

Hoping to succeed where Ritter had failed, Oersted continued speculating along the lines which Ritter had so enthusiastically set out, yet became increasingly wary regarding experimental claims. In 1806, Oersted proposed that electricity, heat, light, and magnetism propagated through a series of rapid "undulatory" expansions and contractions.[39] Unsupported by new experimental facts, however, Oersted's speculations garnered little attention from a physics community still engrossed by the voltaic pile and dominated by French ideas.

An undaunted Oersted continued his quest for a synthesis of forces. Further sparking his interest in 1807, the prize question in physics at the Royal Academy of Sciences of Berlin asked, "Has electricity any direct influence upon the greater or less force of magnetism?— And this influence being proved by experiment, what are the modifications experienced from it by the magnetic force?"[40] Two years later, Oersted proposed a broader question for a similar competition at the Royal Danish Society of Sciences, "What is the connection between the variation and inclination of the magnetic needle and physical forces, both in their usual, mild modes of action such as wind, atmospheric electricity, northern lights, etc. and in their unusual, more violent modes of action, lightning, earthquakes, hurricanes, etc.?"[41]

Eager to answer these questions regarding magnetism and terrestrial magnetism, yet retreating from the extremes of Schelling's *a priori* "science of Nature," Oersted's qualitative and non-mathematical experimental approach remained at odds with the dominant Laplacian style.[42] Also diverging from the Laplacian program, Oersted rejected distinct imponderable fluids in favor of the general propagation of forces. He once again supposed in 1812 that magnetism, electricity, and other phenomena emerged from some common source.[43] Though recognizing that galvanic, magnetic, and electric phenomena were different, Oersted conjectured the differences resulted merely from differing degrees of tension. Further supposing that chemical, thermal, and optical phenomena arose from fundamental forces of positive and negative electricity existing in all matter, he proposed that the "undulatory propagation" of electricity arose from the incessant establishment and destruction of the equilibrium of forces.[44] Therefore, electric fluids did not discharge through the wire as imponderable fluids. Uncertain of the details, Oersted cautioned:

> when we speak of primitive or fundamental forces, we only intend to designate the most simple activity our experiences can give us an idea of. Thus, not wanting to become engaged in metaphysical discussions, we will not make any decisions as to whether these forces are distributed within the various molecules of bodies . . . or whether these forces are spread in space without being fixed at such points.[45]

Although Oersted eventually chose the latter of these two options (i. e., forces spread in space), the precise nature of such undulations remained unclear to British and French physicists who tried to represent Oersted's idea by a material ether.[46] Undeterred by the fact that electrical bodies acted upon magnetic bodies as if "not animated by any particular force whatsoever," Oersted optimistically concluded in 1813, "since the present state of physics has not yet furnished facts sufficient for that, we shall show at least that this involves merely a difficulty, not a fact absolutely contrary to the identity of the electrical and magnetic forces."[47] While Oersted's search continued, his work received little serious attention in Britain before 1820, the British speculative tradition seeking to unify or link phenomena persisted.

Electricity and Magnetism: Nature's Unity in Britain (before 1820)

Though galvanic and electrochemical research renewed hopes for discovering unity, determining the nature of relationships between phenomena required solid empirical support and theoretical foundations. Particularly vexing were the long-supposed connections between electricity and magnetism. Eighteenth-century investigators noted that changes in temperature affected magnets. As well, lightning or a static electric discharge strongly magnetized iron needles, and sometimes reversed or destroyed the polarity of previously magnetized needles. Furthermore, John Dalton, among others, noted that the beams of the aurora borealis, believed to be an electric phenomena, always paralleled the magnetic meridian. Of these connections, English chemist James Cumming remarked in 1821, "These two facts seemed to prove an intimate connexion between Magnetism and Electricity, and when afterwards a similar connexion was observed between Electricity and Galvanism, it was an obvious inference that these powers might possibly be identical."[48] Indeed, the same inference had given impetus to Oersted's research. Nevertheless, though Oersted and others placed magnets within galvanic circuits and magnetic needles atop voltaic piles, they recorded no interactions until Oersted's successful experiment of 1820.

Despite the failures to convincingly link electricity and magnetism, the allure of incorporating all natural powers into a single description persisted. As with earlier speculations, however, these views were usually qualitative and not mathematically developed. Conjectures based on an all pervading ether attempted to explain the appearance of heat and light that sometimes accompanied galvanic experiments. For instance, repeating earlier suggestions, Adam Walker contended in 1809 that electricity, heat, and light were modifications of a single ethereal fluid inherent in all matter.[49] Because the conducting wire of a galvanic device often emitted heat, others speculated that the discharging fluids within the wire freed up caloric. Advocates of the undulatory theory of light, such as Thomas Young, supposed that electric currents set up vibrations in a caloric ether, thereby resulting in glowing high-resistance wires which gave off heat as well as light.[50]

Many proceeded with more caution, expecting future research to vindicate their suspicions of underlying unity. One year later, Aberdeen natural and moral philosopher Robert Eden Scott supposed more generally, "Many important properties of matter remain, no doubt, yet to be ascertained; and many of those properties which we at present consider as distinct from each other, may, by future investigations, be reduced to a single class, and shewn to be different modifications of some simple quality."[51] Asserting that unifying conjectures had been made by "every philosopher of any consequence," Matthew Allan, an English chemistry lecturer, explained in 1818:

> To say that I conceive that attraction in general, gravitation, chemical affinity, electricity, galvanism, magnetism, caloric, and light, arise from one power regulated by one law,—that their diversified effects are merely modifications which circumstances and substance produce on its actions— is easy. But to support this opinion by a clear explanation and exposition of facts, by pointing out what those circumstances are, and in what way they operate, is a matter of some difficulty. *I conceive it however to be a work which will tend to give us clear and simple views of each part of science, and of the whole combined, as one undivided, sublime and majestic fabric of nature.*[52]

Similarly, Aberdeen alumnus and scientific writer James Mitchell reflected in 1819 that connections between magnetism, electricity, galvanism, light, heat, and chemical action were still far from being understood. Nonetheless, Mitchell believed that such links existed, concluding that the current state of research afforded ample room for future discoveries.[53] Hence, while open to the possibility that links would be revealed, the available evidence did not convince characteristically cautious investigators. Within the British inductive tradition, the proof remained to be seen in future empirical pudding. In the meantime, the "matter of some difficulty" which Allan had pointed out persisted.

Beyond hopeful speculations and vague conjectures, many British physicists accepted the distinct origins of electricity and magnetism. In 1807, Thomas Young concluded that "there is no reason to imagine any immediate connexion between magnetism and electricity."[54] In 1819, Biot's article in the *Edinburgh Encyclopaedia* noted that, despite the perfect analogy between electricity and magnetism, "the independence which exists between their [i. e., magnetic principles] actions does not allow us to suppose them to be of the same nature as electricity."[55] That same year, in a review of Oersted's *Recherches sur l'Identité des Forces Chimiques et Electriques*

(1813), Scottish chemist Thomas Thomson reiterated that electric bodies acted upon magnetic bodies "as if they were not endowed with any peculiar force."[56] For example, a magnetic needle charged with electricity showed no signs that the two phenomena influenced or modified one another. The skeptical Thomson concluded that Oersted, unable "to deny the existence of this difficulty, endeavours to elude by pointing out some phenomena in electricity itself of somewhat an analogous nature."[57]

Using similar arguments, the article "Magnetism" appearing successively in the fourth, fifth, and sixth editions of *Encyclopaedia Britannica* regarded electricity and magnetism as distinct. In the slightly revised sixth edition (1819-1823), the anonymous author repeated several well-known facts demonstrating these forces' separate origins.[58] First, all bodies could be electrified, while magnetism acted perceptibly only in iron and its compounds. Second, unlike a magnetic body with two poles, an electric body could be entirely positively or negatively charged. Finally, while electrical phenomena often exhibited incredibly rapid motions, magnetic phenomena showed nothing of the sort. Because differences outweighed superficial similarities, the author judged magnetism and electricity to have separate causes. Therefore, magnetizing an iron bar by passing electricity through it, indicated the mechanical effect of the shock upon the iron's internal constitution, not the identity of magnetism and electricity. Heat's effects on magnetism could be described in like manner. Arguments of this sort convinced many British physicists who continued writing in terms of distinct magnetic and electric fluids. Regardless of the strong analogy between electricity and magnetism, and seeming empirical connections, they remained separated by experimental evidence and well-developed, highly successful theories. These sharp distinctions, as we shall see, began getting murky during the 1820s.

Even though Oersted's "undulatory propagation" attracted few British proponents, the discovery of electromagnetism in 1820 had an immense impact on

British and Continental experimental physics. Reflecting over two decades later, the chair of natural philosophy and astronomy at London University, Dionysius Lardner (1793-1859), noted:

> This discovery being made known caused unqualified astonishment through Europe; the more especially, as all the attempts made before to trace the relation between the electric current and the magnet had been unavailing. The enthusiasm which had been lighted up by the great discovery of Volta twenty years before, and which time had moderated, was relumined, and the experimental resources of every cabinet and laboratory were brought to bear on the pursuit of the consequences of this new relation between sciences so long suspected of closer ties.[59]

As we shall see, investigators from across Europe reacted in various ways to the newly-discovered phenomena. Some incorporated electromagnetism into the Laplacian program, while others reduced all magnetic phenomena, including electromagnetism, to electricity. Though many British physicists followed these two approaches, some disavowed both. Whether awaiting more experimental evidence or venturing their own hypotheses, they transformed their understandings of magnetism.

In order to place the impact of Oersted's discovery in a broader context, several related approaches to the study of magnetism, and more particularly, terrestrial magnetism, must be addressed before turning to electromagnetism. Prussian natural philosopher Alexander von Humboldt and Norwegian mathematician Christopher Hansteen, like Ritter and Oersted, sought the interrelatedness of natural forces. In conjunction with intense experimentation of the 1820s, their ideas influenced British studies of magnetism and terrestrial magnetism. Similar to the British speculative tradition, Humboldt and Hansteen believed in the interconnectedness of nature. Non surprisingly, these ideas appealed to those investigators in Britain who sought unifying principles.

Alexander von Humboldt: The Cosmical Approach

Early in the century, Alexander von Humboldt supported the search for relationships between natural phenomena. In the late 1790s, Ritter, then a student at Jena, reviewed Humboldt's early galvanic research upon Humboldt's request. Suitably impressed with Ritter's comments, the wealthy Humboldt became one of Ritter's first patrons, thereby directly encouraging his quest for nature's connections.[60] Similar to Ritter and Oersted, Humboldt explained in 1806, "to have general views, to conceive the connection of all phenomena — a connection we call nature — it is necessary first to discern the parts, and then to reunite them, organically, under the same point of view."[61] Emphasizing geophysical phenomena called "telluric" or earth-originating forces, Humboldt supposed that they also interacted with celestial forces emanating from the moon, the sun, and the planets. Stating his grand vision in 1807:

> In the great chains of causes and effects, no material, no activity, can be considered in isolation. The equilibrium that reigns amidst the perturbations of apparently conflicting elements derives from the free play of dynamic forces; and a complete overview of nature, the final object of all physical studies, can only be achieved by attending to every force, every process of formation.[62]

Although Humboldt worked with Biot, Gay-Lussac, and other prominent Laplacians, his quest to elaborate the "cooperation of forces" had more in common with the conjectures of Ritter and Oersted.

Humboldt's "cosmical" approach to geophysical and celestial phenomena often accompanied the practice of "Humboldtian" science. Humboldtians contended that widespread, accurate measurement of phenomena would yield quantitative, descriptive laws, and perhaps the underlying connections as well. As a growing force in the 1820s and 1830s, Humboldtians amassed a multitude of oceanographic, meteorological, and geophysical measurements carefully recorded in iso-maps, tables, charts, and graphs. In fact, the 1820s, for the first time, witnessed the widespread use of graphs for displaying scientific data.[63] In addition, Humboldtian scientists sought to improve the accuracy of their measurements by constructing better instruments. In all their efforts, they wished, not primarily to collect and quantify as did late eighteenth-century investigators, but to reveal the underlying relationships between disparate phenomena. As well, Humboldtians hoped to discern general mathematical laws from their assiduously collected data.[64]

Accustomed to worldwide scientific collection since the eighteenth century, many British investigators, whether directly or indirectly inspired by Humboldt's views, naturally approached geophysical phenomena in a Humboldtian-like manner. With new experimental results, some contended that precise, global measurements would reveal the relationships between geophysical forces such as terrestrial magnetism, atmospheric electricity, and the aurora borealis. Such an approach might possibly unveil the cosmical links between terrestrial and celestial forces.[65] Following Oersted's discovery, investigations seeking ties between magnetism and other phenomena became commonplace in Britain and on the Continent. As a result, these experiments enlivened the search for a Humboldtian-style synthesis of geophysical forces.

Although earlier British investigators called for increased observational efforts, their general pessimism contrasted sharply with the optimistic Humboldtian-cosmical approach. Earlier investigators such as Cavallo, Robison, and Thomas Young contended that establishing a predictive theory of terrestrial magnetism was virtually impossible. They argued that too many factors precluded isolating the individual effects of each one. In contrast, Humboldtians believed that persistent, accurate global observations would reveal the laws, perhaps even the true causes, of terrestrial magnetic phenomena and its intertwining with other phenomena.

Owing in part to renewed observational opportunities offered by Arctic exploration, investigators of terrestrial magnetism during the 1820s and 1830s expressed hopes not prevalent in their late eighteenth and early nineteenth-century counterparts. Colonel John Macdonald aptly illustrated this optimism. Proclaiming the importance of determining the location of the northwest magnetic pole, Macdonald proposed in 1820 that "a series of accurate observations on meridians, in many distant situations, are requisite to remove serious objections lying against the best-imagined of these conjectural theories."[66] Expressing excitement over Arctic expeditions, he claimed that such voyages were undertaken for several reasons including enabling the advance of certain sciences, particularly magnetism.[67] These pursuits, Macdonald contended, required "experiments of a delicate description to be made, and observations of an accurate nature to be taken, in opposite, and unfrequented paths of the world." Again praising renewed Arctic exploration, Macdonald wrote in 1821:

> If no other advantage arose from the present Voyages that the recent discovery of a North-west Magnetic Pole, that alone is so valuable to Science in establishing, in process of time, a sure theory of the Magnetic Variation . . . that the best thanks of the country are due to the Admiralty for the efficient manner in which these Voyages have been directed.[68]

With further sustained efforts, he asserted that the "complete establishment" of a theory of magnetic variation would soon be possible. Experimental results, in combination with more intense magnetic data collection, suggested that a true understanding of terrestrial magnetism was within reach.

Throughout the 1820s, a growing sense emerged that the study of terrestrial magnetism had improved, yet required additional work. In 1820, Scottish physicist David Brewster proclaimed the present a "more auspicious period" for the study of magnetism. Justifying this optimism, he noted:

> So far as regards the nature of magnetic attraction and repulsion, a few of its laws appear to be pointed out with tolerable accuracy; the art of experimenting had received fresh improvements; and attempts at least have been made to bring its results under the dominion of mathematical analysis.[69]

Brewster's sentiments make evident the stress he and others placed on new experimental techniques, mathematical analysis, and the formulation of quantifiable laws. In 1829, Edinburgh natural philosophy professor John Leslie similarly remarked of advances in the field:

> Magnetism has also, within these few years, been advancing to maturity. The various circumstances which affect the declination and depression of the Needle are at length ascertained with some degree of precision. Empirical laws have hence been framed, that seem to indicate the changes of magnetic influence which are going forward in different parts of the surface of the earth. But the connecting principles, which would harmonize the whole, remain still unknown.[70]

Peter Barlow related four years later that, "the great progress which has been made within a few years in establishing the laws of magnetic action . . . leads to a rational expectation that the still unknown and mysterious laws of terrestrial variation may by perseverance be likewise elicited."[71] In 1834, Commander James Clark Ross reiterated that "several of the laws of magnetism have of late years been gradually developed."[72] British investigators, Humboldtian or not, believed that with enough data, appropriately arranged and interpreted, the inner workings of earthly magnetism were surely within reach. Like Humboldt and many others, Norwegian natural philosopher and mathematician, Christopher Hansteen, held this view as well.

Christopher Hansteen: Reviving the Four-Pole Theory

In 1818, Thomas Thomson announced in the *Annals of Philosophy* a forthcoming publication by Norwegian Christopher Hansteen (1784-1873). Though he had not yet seen Hansteen's book, Thomson asserted its importance to the long-neglected British study of terrestrial magnetism:

> The very little progress which the theory of magnetism has yet made, and the little knowledge of the laws of the variation of the compass which has yet been acquired, are known to all of my readers. This is probably the cause why magnetism has of late years been so much neglected in this country. I am induced, partly on this account, and partly in consequence of the great importance of the subject, to call the attention of literary men to a treatise on magnetism to be published about this time by M. Hansteen, Professor of Practical Mathematics in the Norwegian University of Christiana.[73]

In 1810, a competition proposed by the Danish Royal Academic Society sparked Hansteen's interest in the subject by asking, "Is it possible to explain the magnetic uniqueness of the earth by one magnetic axis only, or is one forced to suppose several?"[74] Using much of the magnetic declination data accumulated since 1600, Hansteen's prize-winning thesis of 1812 concluded that two magnetic axes represented the observations better than Euler's single-axis theory. Hansteen's subsequent research, including an expedition to find a magnetic pole in Siberia (1828-30), focused on elaborating and refining this four-pole hypothesis.[75]

Published in 1819, Hansteen's extensive magnetic atlas, *Untersuchungen über den Magnetismus der Erde*, contributed to the growing European curiosity in terrestrial magnetism.[76] Hansteen's book, which analyzed the geomagnetic data of the past two hundred years, made him well known among European scientists. Several eminent investigators, including Humboldt and Oersted, soon took notice of his work.[77] Illustrating his fondness for the cosmical approach, Oersted explained in 1821:

> The daily course of the light of the sun round the earth produces warmth, evaporation, and chemical agency, from the east to the west. From this also proceeds an alternation of the destruction and renovation of electrical equilibrium, and the effect of it must be similar to that of a galvanic circle applied round the earth. . . . The length of the circle or electrical belt is that of the periphery of the earth. . . . The width of this belt varies every day, since the diameter of the circle around the poles of the earth, during night or day, changes continually during several revolutions.[78]

In support of his theory, Oersted reported that the two northern magnetic poles supposed by Hansteen were "under the same meridian as the celebrated Humboldt . . . places the greatest concavity, that is to say, the greatest polar distance, from his isothermal line of 0°."[79]

Endorsing the four-pole theory again in 1827, Oersted remarked that Hansteen's theory was only a mathematical representation of the phenomena, not a physical one. Nevertheless, he hoped that Hansteen's views would "recommend themselves to farther investigation, as they would, if proved, have the great advantage of showing an intimate connexion between an extensive series of phenomena upon the earth and those of the universe."[80] Despite Hansteen's revival of the four-pole theory, the two-pole and four-pole theories continued to coexist. Not surprisingly, locating the poles' positions and collecting more precise magnetic data became desiderata for deciding between the two theories. Hansteen, Oersted, and others agreed that determining the locations and movements of each magnetic pole would lead to a better theory of magnetic variation.

As a student at the University of Copenhagen, Hansteen had learned from his professor, Oersted, and others who asserted the connectedness of natural powers.[81] Though his 1826 description of electromagnetism appealed to an elastic fluid , it nevertheless emulated Oersted's conflict of electricities:

> In the complete voltaic circuit, the conductor is traversed in the opposite direction by the opposite electricities. Every plus elementary particle strives to combine with a minus one; thus united in pairs, they neutralize each other, and their electric power disappears. But in the neutral state they perhaps appear as elastic fluid *elementary magnets* [elastisch flüssige *Elementarmagnete*], which so surround the surface of the polar wire that all north poles are turned on one side, and all south poles on another; and the axis of every elementary magnet is the tangent of the circular section of the conducting wire.[82]

Like Oersted and Humboldt, Hansteen also demonstrated a continuing fondness for the cosmical approach to terrestrial magnetism. In 1819, he remarked:

> By the stifled voice of the magnetic needle, the earth proclaims the movements of her interior; and could we rightly interpret the flaming page of the polar light, it would not be less instructive for us. The connection of meteorology with the aurora borealis, and, consequently, with the magnetic forces, is perfectly clear: the similarity between Humboldt's isothermal lines and the lines of the same magnetic dip, is equally remarkable.[83]

Arguing for celestial influences, Hansteen numerically linked the rotational periods of the earth's four moving magnetic poles with the distances of sun and moon, and the precession of the equinoxes. As well, he supposed the existence of magnetic interactions between the sun and the planets, and between the planets and their satellites.[84] Desiring a deeper understanding of the earth's magnetism, Hansteen wished that the governments of Britain, France, and Russia would combine their observational efforts.

Working with freshly amassed data, he hoped that in a short time, "the hitherto inexplicable magnetic appearances of the earth, might be submitted to as sure a calculation as the movements of the heavenly bodies."[85]

With subsequent experimental discoveries of the 1820s, Hansteen, like many British investigators, supposed that secular variations in terrestrial magnetism arose from electrochemical and thermoelectric forces within the earth. Gaining support from the views of investigators like Oersted, Humboldt, and Hansteen, the topics dominating British terrestrial magnetic research during the 1820s and 1830s indicated an endorsement of the cosmical view. While investigators continued to wonder how earthly magnetic forces originated, they also wanted to understand how did the sun, moon, and other celestial bodies affected terrestrial magnetism. Finally, they sought out the interactions between the aurora borealis, atmospheric electricity, heat, and terrestrial magnetic phenomena.[86] In attempting to answer these questions, the interweavings of numerous geophysical phenomena had become a serious consideration.

As the cosmical approach came to the fore in the 1820s and 1830s, Hansteen's work in terrestrial magnetism influenced the work of several British investigators (e. g., Sabine, Brewster). More generally, whether siding with the two-pole theory or the four-pole theory, navigators and natural philosophers of the 1820s stressed the possibility of determining general terrestrial magnetic laws. Thus, as the pessimism of earlier decades receded, optimistic British investigators believed that continued efforts would yield general laws.

However, prior to the 1820s, ideas like Hansteen's, Humboldt's, and Oersted's gained only small favor in Britain. Even though the search for "a complete overview of nature" complemented the British speculative tradition, it conflicted with traditional British inductivism and admonitions against unwarranted hypotheses. As well, the cosmical approach of Oersted, Humboldt, and Hansteen clashed with the widely-accepted notion of distinct imponderables. In these theories, the distinctions between magnetic, electric, and other fluids were clearly drawn. For these reasons, divisions persisted between those wished to separate natural phenomena and those who joined them together. As we shall see, speculations arising from the discovery of electromagnetism and other experiments challenged the orthodox Laplacian understanding of the relationship between magnetism and electricity.

Oersted: The Discovery of Electromagnetism

On July 21, 1820, Hans Christian Oersted announced in a brief Latin publication that a conducting wire attached to a voltaic battery deflected the normal orientation of a nearby suspended bar magnet.[87] Although primarily emphasizing *how* he had observed the effect, Oersted also offered a physical explanation called the "electric conflict." Clearly distinct from Laplacian theory, Oersted's theory claimed that this mechanism acted in the space surrounding the wire:

> It is sufficiently evident from the preceding facts that the electric conflict is not confined to the conductor, but dispersed pretty widely in the circumjacent space. From the preceding facts we may likewise infer that this conflict performs circles. . . . Besides, a motion in circles, joined with a progressive motion, according to the length of the conductor, ought to form a conchoidal or spiral line.[88]

As the conflict of electricities traveled in opposite directions in the space around the conducting wire, the positive electricity repelled the south pole and attracted the north pole of the compass needle, while negative electricity, traveling in the other direction, had the opposite effect. Oersted also supposed that heat and light arose from this conflict of electricities.[89]

The theory of the "electric conflict" gained few British or French adherents. In fact, in early 1822, Michael Faraday confessed that he had little to say about Oersted's theory "for I must confess I do not quite understand it."[90] Nonetheless, Oersted's long-awaited experimental discovery gave immediate impetus to the study of electricity and magnetism.[91] Within months of its announcement, investigators from all over Europe verified and extended Oersted's results. Some distinguished Oersted's discovery as the greatest in physical science since that of the voltaic pile. Biot reported that as soon as it was known in France, England, and Germany, it had "excited the most lively sensation among men of science."[92] Initially doubtful himself, Arago repeated Oersted's experiment for a skeptical French audience in early September, 1820.[93] A month earlier, an English journal reported the discovery and commented that such effects seemed "to indicate laws of magnetism entirely unknown hitherto."[94] As well, Oersted's announcement was soon translated into English.[95] In November, Davy presciently remarked that electromagnetism opened "a new field of enquiry, into which many experimenters will undoubtedly enter."[96] Praising Oersted, another commentator hopefully remarked, "It is a great fact, which will perhaps be connected with others already known, or hereafter discovered, and which will multiply the relations between the magnetic, electric, calorific, and luciferous forces."[97] In early 1821, English chemist William T. Brande asserted that no discovery had, for a long time, "so strongly excited the attention of the philosophic world, as that of the magnetic phenomena belonging to the Voltaic apparatus."[98]

While arousing great excitement across Europe, Oersted's experiment and theory, like his earlier speculations, diverged from orthodox Laplacian science in several significant ways. First, the electromagnetic action seemed to be a stunning exception to the accepted independence of magnetic and electric phenomena. Second, its action did not appear to be centrally-acting like the short-range attractive and repulsive forces of Laplacian theory. Instead, the suspended magnet arranged itself across the direction of the electric current in the conducting wire— an effect variously described as "transverse", "tangential", or "circular." Third, Oersted's conflict of electricities did not flow within the wire and across its surface. Instead, he supposed an undulatory propagation moving in opposing helical patterns through the space around the conducting wire. Such a view obviously departed from Laplacian imponderable fluids. Finally, Oersted's vague physical conjecture neither quantified the phenomena nor put it in a mathematical form. In fact, he showed no interest in reducing phenomena to mathematically formulated force laws. With respect to electromagnetic phenomena, these factors led Oersted and others to question and eventually reject orthodox Laplacian explanations.

Biot and Ampère: Coulombian Theory versus Electrodynamics

Not surprisingly, some responded to Oersted's experiment by defending and adapting the Coulombian two-fluid theory. Performing electromagnetic experiments in the fall of 1820, J. B. Biot and Félix Savart sought to quantify and mathematically analyze the forces involved. With help from Laplace, they experimentally deduced that the forces exerted on a magnetic pole by an element of electric current followed the inverse square law.[99] In Laplacian fashion, Biot explained in 1824,

> Thus, when an indefinite connecting wire, animated by voltaic current, acts on an element of austral or boreal magnetism situated at a certain distance FA or FB from its centre, the resultant of the actions which it exerts is perpendicular to the shortest distance between the molecule and the wire.[100]

Ultimately, Biot reduced the electromagnetic effect to forces between particles of magnetic fluid in the wire and the magnet. Seemingly violating the Laplacian stipulation that electric and magnetic fluids did not interact, he assumed that the conducting wire somehow generated "molecular magnetism." The mathematical force law took precedence over the actual physical mechanism.

In the months and years following Oersted's discovery, others questioned Laplacian descriptions of electromagnetism. Of particular importance were the researches of French chemist and physicist André-Marie Ampère (1775-1836). Ampère's early familiarity with Oersted's chemical work, outsider status in the

Parisian scientific community, and lack of commitment to the Laplacian program fueled his excitement for investigating electromagnetism.[101] As one in a group of French anti-Laplacians, including Augustin Fresnel, Pierre Louis Dulong, and François Arago, he shared their view that connections between various phenomena could be explained in terms of mechanical vibrations in an all-pervading ether.[102] In fact, as early as 1801 Ampère had rejected action-at-a-distance phenomena (excepting gravity).[103]

During the closing months of 1820, Ampère skillfully reproduced a multitude of magnetic effects using arrangements of electric currents. After verifying and measuring the effect on September 18, he showed a week later that electric wires bent into planar spirals attracted and repelled one another like the poles of ordinary bar magnets. In early October, Ampère demonstrated that parallel conducting wires attracted when the currents ran in the same direction, yet repelled when the currents ran in opposite directions.[104] These phenomena, he noted, were distinct from ordinary electric attractions and repulsions. Several weeks later, Ampère illustrated that a suspended conducting loop aligned itself in a plane perpendicular to the magnetic meridian. In early November, Ampère used a helix wrapped around an axial current to replicate the action of one bar magnet upon another.[105]

Citing these and other experiments, Ampère proposed his theory of "electrodynamics." In opposition to the notion of distinct imponderables, he concluded that all magnetic phenomena reduced to "admitting that a magnet is only an assemblage of electrical currents . . . which move in planes perpendicular to the line which joins the two poles of the magnet."[106] Lending further support to Ampère's claim that all magnetism reduced to electric currents, Arago showed in late September 1820, that current-carrying wires attracted iron filings in the same manner as ordinary magnets. Around the same time, he successfully magnetized pieces of steel placed within spiral conducting wires. Further strengthening the supposed identity of galvanic and static electricity, Arago announced in early November that he had used static or common static electricity to produce similar effects.[107]

By September 1820, Ampère had also extended his electrodynamic theory to earthly magnetism. Rejecting previous explanations, including Biot's theory of terrestrial magnetism, he remarked in 1821:

> One of the principal consequences of the theory founded upon this identity [of electricity and magnetism] is, that the directing action of the earth does not emanate either from the polar regions, or from the center of the globe, as had been successively supposed, but that it proceeds especially from the equatorial zone, where heat and light act with the most intensity. I think that this determination of the regions of the earth, where the cause of the directing action resides, will interest natural philosophers, who endeavour to represent, by general formulas, the amounts of the declinations and inclinations of the magnetic needle from the poles to the equator.[108]

By appealing to his experimental results, Ampère asserted that electric currents ran east to west through the earth, in planes at right angles to the dipping needle. While these currents gave rise to terrestrial magnetism, the actions of heat and light upon the currents gave rise to terrestrial magnetic variations.

As Ampère's ideas regarding magnetic theory diverged even further from Coulombian theory, his disagreements with Biot, Poisson, and other Laplacians intensified. In January 1821, Augustin Fresnel convinced Ampère that molecular electric currents confined to each magnetic element were a more reasonable hypothesis than larger currents about the magnet's axis.[109] Clearly frustrated with the Laplacians, Ampère complained the following month to a friend, "It is amusing to observe the efforts that certain minds make to try to reconcile the new facts with the gratuitous hypothesis of two magnetic fluids distinct from electrical fluids merely because they have become accustomed to that idea."[110]

As the divisions widened, Biot became more open in his opposition to Ampère's electrodynamic theory. In 1824, he leveled the devastating charge that the molecular electric currents of Ampère's theory resembled Cartesian vortices. In Biot's judgment, the theory made a multitude of

> complicated suppositions; for he [Ampère] is under the necessity of considering all the mutual actions of magnetic bodies in general, as produced by voltaic currents circulating about the metallic particles which compose them, in a manner greatly resembling the vortices of Descartes. . . . I have thought proper to give the observations of M. Ampère, without adopting his explanation.[111]

For Biot, electromagnetic and magnetic effects must be analyzed in terms of forces between magnetic fluid particles. Ampère electrodynamics simply failed to do this.[112] Defending himself from Biot's attack, Ampère insisted on guidance from Newtonian rather than Cartesian principles:

> I have reduced the phenomenon observed by M. Oerstedt [sic] . . . to forces acting along a straight line joining the two particles between which the actions are exerted; and if I have established that same arrangement, or the same movement of electricity, which exists in the conductor is present also round the particles of the magnets, *it is certainly not to explain their action by impulsion as with a vortex, but to calculate, according to my formula, the resultant forces acting between particles of a magnet and those of a conductor*, or of another magnet . . .[113]

Adding additional support, he noted that his theory agreed with the magnetic laws which Coulomb and Biot had deduced from their experiments.

Meanwhile, Biot and other Laplacians continued defending the Coulombian theory. The same year that Biot accused Ampère with Cartesianism, Poisson questioned the notion that magnetism could be reduced to electricity. Was magnetism a particular fluid, found only in bodies susceptible of its influence, or merely a modification of the electric fluid? Though Poisson concluded that this question could not be decided with the available evidence, he treated magnetism in Laplacian fashion as entirely distinct from electricity.[114] Alluding to the discoveries of Ampère and others, Poisson simply remarked that the identity of the fluids was not necessarily proven by "the important facts, which have lately been discovered, relating to their connection."[115]

Using mathematical analyses similar to those used in the electrical memoir of 1812, Poisson developed equations expressing the laws of induced magnetism within bodies in addition to the attractions and repulsions these magnetic bodies exerted upon one another. Beginning with Coulomb's theory, he supposed that the boreal and austral fluids were confined by some unknown force to the interior of small "magnetic elements" within the magnetic material. In the magnetized state, the two fluids became polarized within each magnetic element, forming a thin layer of "free fluid" on the surface of each element. Finally, Poisson calculated the distributions of magnetic fluid and the resulting magnetic forces.[116] Like Biot, Poisson developed mathematical laws agreeing with experimental results, thereby skirting the issue of interacting magnetic and electric fluids. Though Poisson's mathematical analysis brought the Laplacian magnetostatics to its zenith, additional experimentation and speculation continued to alter the understanding of magnetism in France and Britain as well. While some embraced mathematics in their understanding of magnetism, others preferred a more strictly experimental approach.

Wollaston, Davy, and Faraday: The Reaction of English Chemists

Despite the growing enthusiasm for magnetic research, British investigators in the 1820s gave a mixed reception to French and other continental theories. In particular, discord persisted with respect to explanations of electromagnetic phenomena. While some treated magnetic and electric fluids as distinct entities, others deemed Ampère's theory more plausible.[117] Some British investigators cautiously endorsed new ideas, while others declared allegiance to no particular hypothesis. Repeating and extending the experiments performed by Oersted, Ampère, Arago, Biot, and others, British physicists assimilated continental research and ideas. Sparking interest in the relationships between magnetism and other phenomena, the flood of experimentation also promoted study of terrestrial magnetism and its supposed connections with other geophysical phenomena.

While renowned English chemist William Hyde Wollaston did not publish a statement of his ideas, the editor of the *Quarterly Journal of Science, Literature and the Arts*, in 1821 briefly expressed Wollaston's response to recent discoveries.[118] Unlike Ampère's electrical currents within the magnet, Wollaston supposed spiral currents passing around the conducting wire in much the same fashion as Oersted's electric conflict. Thus, when Wollaston's "vertiginous electricity" moved along two parallel wires in the same direction, the wires attracted each other. Conversely, when the helical currents propagated along the wires in opposite directions, they repulsed one another. Such ideas led Wollaston, Oersted, and many others away from the standard fluid theories and Ampère's electrodynamics. Because these forces did not act centrally, they did not fit with orthodox views regarding electric or magnetic attractions and repulsions.[119] As Wollaston and others realized, such difficulties required additional research.

We have seen from electrochemical research that Humphry Davy willingly entertained limited speculations regarding nature's interrelatedness.[120] In 1812, he had noted, "Electrical effects are exhibited by the same bodies, when acting as masses, which produce chemical phaenomena when acting by their particles; it is not therefore improbable, that the primary cause of both may be the same."[121] Reacting to Oersted's discovery of 1820, Davy performed experiments (independently from Arago) in which he attracted iron filings to a conducting wire and magnetized steel needles placed on or near the wire. From additional experiments, he concluded that concentrated electricity passed through space generated magnetism. Since these experiments, among others, generated many questions, Davy asked:

> Is electricity a subtile elastic fluid? or are electrical effects merely the exhibition of the attractive powers of the particles of bodies? Are heat and light elements of electricity, or merely the effects of its action? Is magnetism identical with electricity, or an independent agent, put into motion or activity by electricity?[122]

Though considering these queries of great importance, Davy prudently judged the present data insufficient to decide such "abstruse and difficult parts of corpuscular philosophy." Uncertain of electrodynamic theory, he wrote to Ampère in May 1821, "I wish you may be able to furnish some *direct* proof of the existence of electrical currents in the magnet."[123]

Despite apprehensions, Davy cautiously entertained connections between electricity, magnetism, and other phenomena. These experimental speculations applied to geophysical phenomena as well. In a paper from 1821, he noted that terrestrial magnetism might possibly arise from electricity. Terrestrial magnetic variations, Davy ventured, arose from changes in "the electrical currents of the earth, in consequence of its motions, internal chemical changes, or its relations to solar heat." Supposing the aurora's electrical origins, he further suggested "that if strong electrical currents be supposed to follow the apparent course of the sun, the magnetism of the earth ought to be such as it is found to be."[124] Therefore, Davy considered terrestrial magnetic variation dependent upon electrical, chemical, and solar interactions. As well, he suggested that magnetism might have the same cause as electricity. In 1822, Davy remarked in a letter to Wollaston that the "intimate connection, if not the identity," of magnetism and electricity had been partially demonstrated.[125]

Only three years before his death, Davy distanced himself from broad conjectures in the 1826 Bakerian Lecture on electrochemistry.[126] Defending experiment against the wild speculations of Newton and *Naturphilosophie*, he remarked:

> The queries of Newton at the end of his "Optics" contain more grand and speculative views that might be brought to bear upon this question [i.e., electrochemical theory] than any found in the works of modern electricians; *but it is very unjust to the experimentalists who, by the laborious application of new instruments, have discovered novel facts and analogies*, to refer them to any such suppositions as, "that all attractions, chemical, electrical, magnetic, and gravitative, may depend upon the same cause;" or to still looser expressions, in which different words are used and applied to the same ideas, and in which all the phenomena of nature are supposed to depend on the Dynamic system, or the equilibrium and opposition of antagonist powers.[127]

Unlike proponents of unifying ether theories or *Naturphilosophie*, a grand synthetic view of nature did not propel Davy's research. Contending that present philosophical systems were "exceedingly imperfect," he retreated from the idea that chemical and electrical phenomena had an identical cause:

> I never attached much importance to this hypothesis; but having formed it after a copious induction of facts, and having gained immediately by the application of it a number of practical results . . . and having developed it in an elementary work, I have never criticised or examined the manner in which different authors have adopted or explained it, —contented, if in the hands of others it assisted the arrangements of chemistry or mineralogy, or became an instrument of discovery.[128]

In the experimental tradition, Davy focused on formulating hypotheses from careful induction, eliciting new effects and practical applications, and cautiously endorsing limited connections between different phenomena. Also echoing Scottish methodology, Davy contended that hypotheses helped arrange the experimental facts and gave impetus to future discoveries.

In the experimental tradition, Davy's successor at the Royal Institution of London, Michael Faraday remained wary of grand unifying systems as well. Corresponding with Ampère and other continental physicists, Faraday, even more than Davy, cautiously considered hypotheses and speculations.[129] After carefully repeating many of the electromagnetic experiments of Oersted, Ampère, and others, he wrote in September 1821 to Swiss physicist Charles-Gaspard De la Rive of Ampère's theory:

> theory makes up the great part of what M. Ampere has published and *theory in a great many points unsupported by experiments when they ought to have been adduced* [.] At the same time M. Ampere's experiments are excellent & his theory ingenious and for myself I had thought very little about it before your letter came simply because *being naturally sceptical on philosophical theories I thought there was a great want of experimental evidence.*[130]

Also in late 1821, the first two installments of Faraday's anonymous three-part historical sketch of electromagnetism appeared in the *Annals of Philosophy.*[131] Omitting theoretical discussion, Faraday's sketch related the experiments of Oersted, Ampère, Arago, and many others. The second installment made evident Faraday's preference for experiment over theory: "I am desirous . . . to mention the *facts* as they were discovered than the theories attached to them: in the first place, because they are of the most importance; and in the second, because there is no danger of attributing the theories to any but those from whom they originate."[132]

Early in 1822, the third installment briefly discussed theoretical views. After considering the various theories of electromagnetism, Faraday concluded that Ampère's notions were "the most extensive and precise" and "tested by the application of facts and calculation very far beyond any of the rest."[133] Regardless of this praise, however, Faraday had misgivings about embracing Ampère's theory wholeheartedly. Ampère, for instance, described two electricities running in opposite directions, yet frequently wrote as if electricity flowed in one direction. While Faraday recognized unidirectional flow as a convenient simplification, he complained that Ampère explained neither how the double current moved through the wire nor how this current produced magnetism.[134] Of the electrodynamic theory, he asked:

> Currents of electricity, according to the theory, were essentially necessary to the production of magnetic phenomena, but where are the currents in a common magnet? It was a bold thought to say they actually existed in it, but M. Ampere has ventured the idea, and has so arranged them, theoretically, as to account for very many magnetic phenomena.[135]

Faraday concluded his sketch by reiterating doubts regarding the assumed existence of two distinct electric fluids, and the reduction of magnetism to electric currents.[136]

Like Oersted, Wollaston, and others, Faraday quickly realized that the action of the conducting wire upon the magnetic needle was not centrally acting. In fact, he recognized that the wire attempted to make the needle's poles move around it in a circle in a manner akin to Wollaston's "vertiginous electricity." While making him well-known in the European scientific community, Faraday's experiments beginning in September 1821 demonstrated that the end of a suspended current-carrying wire rotated about a magnetic pole and, conversely, a suspended magnetic pole rotated around a conducting wire.[137] He had clearly illustrated the "vertiginous" action proposed by Wollaston.

While the ever-skeptical Faraday required additional proof of the supposed electrical currents in magnets, Ampère subsequently contended that this "rotatory effect" strengthened his theory. Faraday, however, intended not "to adopt any theory of the cause of magnetism, nor to oppose any." He remained unconvinced by Ampère's tiny electric currents.[138] If magnetism did depend upon such currents, then why did not ordinary magnets produce electrical effects such as the decomposition of water? Faraday insisted that if electricity produced magnetism, then magnetism should generate electricity. He devoted much of his research toward this end.[139]

Much of Faraday's caution emanated from his distrust of mathematical analysis and his ability to analyze experimental evidence. Unlike Ampère and the Laplacians, the close coincidence of mathematical calculation with experimental measurement was not Faraday's primary goal. For him, physical mechanisms (e. g., two electric fluids, tiny electric currents) required not mathematical expediency, but solid empirical evidence. Doubting the electrodynamic theory, he wrote to Ampère in February 1822:

> I regret that my deficiency in mathematical knowledge makes me dull in comprehending these subjects. I am naturally sceptical in the matter of theories and therefore you must not be angry with me for not admitting the one you have advanced immediately[.] Its ingenuity and applications are astonishing and exact, but I cannot comprehend how the currents are produced and particularly if they be supposed to exist round each atom or particle and I wait further proofs of their existence before I finally admit them.[140]

Later that year, Faraday similarly commented to Gaspard De la Rive:

> Its [Ampère's theory] beauty I admire, but I have been unwilling to admit it into my own mind to a rank with those theories in other branches of physical science which are accompanied continually by experimental proofs because though it accords pretty well with most if not all the phenomena yet there are many parts in it that seem to me to be mere assumption. . . . I have really been ashamed sometimes of my difficulty in receiving evidence urged forward in support of opinions on electro magnetism but when *I confess my want of mathematical knowledge and see mathematicians themselves differing about the validity of the arguments used it will serve as my apology for waiting for experiment.*[141]

Since even the mathematicians could not agree on electromagnetism, Faraday believed it best to rely on experimental evidence.

Hence, Faraday's non-mathematical, experimental approach to electromagnetism remained at odds with Ampère's electrodynamics and Biot's Coulombian theory. Indeed, Faraday's scientific style owed more to English experimental chemistry than to French mathematical physics. Continuing to support Wollaston's mechanism, Faraday noted in a lecture from 1827, "My rotatory apparatus is a striking illustration of the vertiginous nature of the power in the wire."[142] In many of his later researches, Faraday explicitly sought the interconvertibility of forces.[143] Thus, the "rotary effect" illustrated chemical powers converted into dynamic form in the electric conducting wire which, in turn, produced the magnet's circular motions. Faraday rejected Ampère's theory because it lacked sufficient experimental proof and ignored the conversion of forces.[144] In September 1821, Faraday stated:

> I have no doubt that electricity puts the circles of helices into the same states as those circles are in that may be conceived in the bar magnet but I am not certain that this state is directly dependent on the electricity, or that it cannot be produced by other agencies and therefore until the presence of Electrical currents be proved in the magnet by other than magnetical effects I shall remain in doubt about Ampere's theory.[145]

Rather than follow Ampère's reductionism, he ultimately sought to determine how chemical, electric, magnetic, and other powers converted from one to the other. With this goal in mind, Faraday's later research generated electric effects with magnets in 1831 and illustrated a connection between magnetism and light in 1845. In 1833, after discussing the definite chemical action of electricity, Faraday confidently remarked on electricity's magnetic action, "I have no doubt that the success which has attended the development of the chemical effects is not more than would accompany an investigation of the magnetic phenomena."[146]

During the three decades following the discovery of electromagnetism, Faraday's keen experimental and theoretical insights led him to develop the "field theory." This major conceptual development in the history of physics has been treated extensively in the existing scholarship and is beyond the scope of this dissertation.[147] Though many investigators enthusiastically embraced Faraday's experimental results in the 1820s and 1830s, few initially embraced his qualitative theories of the "electro-tonic state", "lines of force", or the "electric field." Until mathematical physicists such as William Thomson and James Clerk Maxwell applied mathematical analysis to Faraday's ideas in the 1850s and 1860s, his theories gained little acceptance.

Seebeck, Cumming, and Traill: Thermoelectricity

In the early 1820s, continued experimentation bolstered the plausibility of the supposed bonds between magnetism, electricity, and other phenomena. In 1822, German experimenter Thomas Seebeck stimulated ongoing research with the discovery of thermoelectricity. Given a circuit of two metals (e. g., copper and antimony) with two junctions, Seebeck discovered that heating the junctions to different temperatures caused the deflection of a nearby compass needle. Not at first recognizing the generation of an electric current in the metals, later called the thermoelectric or Seebeck effect, Seebeck termed this phenomenon "thermo-magnetism." Reporting this discovery in 1823, an English account explained:

> These currents can be discovered only by the magnetic needle, on which they exercise a very perceptible influence. Henceforth we must distinguish this new class of electric currents by a significant denomination; as such, the expression *thermo-electric circuits*, or perhaps *therm-electric*, are proposed.[148]

This discovery, like that of electromagnetism, was quickly repeated and extended by others. For instance, Dutch physicist, Gerard Moll, demonstrated that a circuit of one metal (rather than two) and an acid (rather than heat) also generated thermoelectric effects.[149]

Cambridge professor of chemistry, James Cumming (1777-1861) showed particular interest in thermoelectricity and its relationship to electromagnetism. In

1821, before Seebeck's discovery, Cumming had repeated the experiments of Ampère and Arago. While considering the analogy between galvanic magnetism and the magnetism of ordinary magnets, he did not equate these two forces or reduce them to electric currents.[150] Two years later, Cumming demonstrated that thermoelectricity generated rotary movements akin to those demonstrated by Faraday.[151] He classified the thermoelectric properties of numerous metals in experiments performed throughout 1823.[152] From his research Cumming concluded that the sole condition for eliciting electromagnetic effects was the "juxtaposition of two particles of the same metal, at different temperatures." Hence, he explicitly considered links between heat, electricity, and magnetism in his explanations.

Like other proponents of the cosmical approach, Cumming hoped that new experimental evidence would improve the understanding of the causes and laws of terrestrial magnetism. In a paper read to the Cambridge Philosophical Society in 1823, Cumming described additional experiments on the development of electromagnetism by heat. From these, he offered the following possibility:

> Magnetism . . . it appears is excited by the unequal distribution of heat amongst metallic, and possibly amongst other bodies. Is it improbable that the diurnal variation of the needle, which follows the course of the sun, and therefore seems to depend upon heat, may result from the metals and other substances which compose the surface of the earth, being unequally heated, and consequently suffering a change in their magnetic influence?[153]

Four years later, Cumming reflected that since various magnetic effects arose from heat, light, chemical action, electricity, and rotation, it seemed highly probable that terrestrial magnetism, "may be the result of electro-dynamic currents originating in these agents."[154] Hence, Cumming believed that earthly magnetism arose from the interactions of terrestrial electrical currents with other phenomena. In proposing such ideas, he directly applied the newfound wealth of experimental evidence to terrestrial magnetic theory.

Similar to Cumming, Edinburgh University professor of medical jurisprudence, Thomas Stewart Traill, attempted to link phenomena as well as their geophysical manifestations. In 1822, Traill delivered a paper with William Scoresby to the Royal Society of Edinburgh on a series of electromagnetic experiments. The authors hopefully remarked that the discoveries of Oersted, Ampère, Arago, and others would "throw a clearer light on the mysterious nature of Galvanism and Magnetism."[155] Responding to Seebeck's discovery in 1824, Traill remarked of the striking similarities between electromagnetism and thermomagnetism. From additional experiments, he concluded that all phenomena tended to show "that a thermo-electric apparatus becomes a real magnet."[156] Always careful not to embrace a particular hypothesis such as Ampère's, Traill nonetheless believed that conjectures could prove useful.

Thereby, after experimentally linking galvanism, magnetism, and heat, Traill applied his results to terrestrial magnetism. In line with the cosmical approach, he proposed that the disturbance of the earth's temperature equilibrium by continual action of solar rays "on its intertropical regions, and of the polar ices, must convert the earth into a vast thermo-magnetic apparatus."[157] In addition, he asserted that changes in magnetic declination could be most satisfactorily explained upon thermomagnetic principles. Traill considered:

> the existence of two poles of greatest cold in either hemisphere, established by a comparison of actual registers of temperature, as generalised in the isothermal lines of Humboldt . . . the migration of the isothermal poles has been strongly insisted on by Dr. Brewster, as the chief cause of our improved climate. Dr. Traill maintains the same argument, and endeavours to shew, that the accumulations and disruptions of the Greenland ice, and the coldness of ancient Europe, would appear . . . to have a remarkable connection with magnetic phenomena.[158]

Therefore, he concluded that terrestrial magnetic and temperature variations were intimately linked.

While some, including Faraday, awaited future evidence to determine the precise nature of the intimate ties between magnetism and other phenomena, other British investigators, including Davy, Cumming, Traill, considered these links as viable explanations supported by a wealth of experimental evidence. Following the cosmical approach, they often applied these ideas to the operations of the entire earth. Although most no longer followed the Gilbertian earth-magnet analogy, they nonetheless did as Gilbert had done by applying experimental evidence they could manipulate to geophysical forces which they could not. Not surprisingly, this use of the experimental evidence often affected speculations regarding the causes of terrestrial magnetism. For instance, as seen in comments by Cumming, Traill, and others, the discovery of thermoelectricity had a direct impact on terrestrial magnetic theories. Other new experimental discoveries were also incorporated into these speculations. Clearly illustrating the acceptance of the cosmical view, the *Edinburgh Philosophical Journal* of 1826 entertained the magnetic hypothesis of a German chemist named Büchner, saying that however bold it might appear that "nothing should be absolutely rejected."[159] A year earlier, Büchner had explained his bold hypothesis:

> It cannot be refused to admit, that light, caloric, electricity and magnetism, are in a certain mutual relation of causality: the question is merely, what is this relation? The following hypothesis appears to me the most simple and most natural.
>
> The planets receive from the sun light and electricity in the neutral state; they decompose these principles, and reproduce, in their turn, caloric, and the two polarised electric principles. . . . Then caloric itself undergoes a modification, which is still enigmatical to us, in virtue of which it is transformed into magnetism. . . . In the present hypothesis, magnetism would not emanate from the earth only, but also from all bodies in the universe that are illuminated by the sun.[160]

He contended that several experiments gave proof of magnetic emanations, therefore the earth itself could be considered as "nothing else than a great thermo-magnetic apparatus." Other experiments would be directly applied to terrestrial magnetic theory as well.

Herschel and Babbage: Arago's Wheel and the Magnetism of Rotation

In 1824, François Arago performed another experiment which excited British investigators. Arago discovered that a copper disc, when rotated beneath a suspended magnetic needle, initially caused the needle to deflect in the direction of the rotation. Moreover, if rotated quickly enough, the needle rotated quickly along with the disc.[161] Magnetism of rotation caused excitement in Britain as others independently demonstrated a similar effect with solid spheres and disks of iron or repeated and extended Arago's experiments.[162]

While Ampère and Biot quickly interpreted "magnetism of rotation" as vindicating their theories and refuting their opponents', English experimenters interpreted the new phenomena in several ways.[163] Cambridge-educated mathematicians and physicists Charles Babbage and John Herschel altered Arago's experiment by suspending the copper disk (and disks of other materials as well) directly above a rotating magnet.[164] In the disk, they explained, a succession of points became magnetic by induction due to the magnet rotating beneath them. This phenomena, "obviously *induced* by the action of the magnetic bar, compass needle, etc.," on the "molecules" in the disk, however, took time, it was not instantaneous.[165] Using the phenomena's temporal dimension, Babbage and Herschel explained why the disk rotated:

> The points over which in succession it [the disk] becomes vertical, not *instantly* receiving all the magnetism of which they are susceptible, will not have reached their maximum of polarity at the precise moment of nearest appulse. . . . In like manner, the points which have attained their maximum of polarity, being left behind by the magnet, will by degrees lose their magnetism. . . . There will thus arise an oblique action between the pole of the magnet and the opposite pole of the plate so lagging behind it; and were the plate free to move in its own plane, the resolved portion of this action parallel to its surface, would continually urge it in the direction of the magnet's motion.[166]

The "oblique action" between these magnetic poles meant that the faster the magnet rotated, the faster the plate rotated. In addition they discovered that radial slits cut in the disk greatly weakened the rotational effect or the "magnetic susceptibility" or the disk; hence, the more slits, the more difficult it became to make the disk rotate. Furthermore, soldering these slits with tin restored some of disk's magnetic susceptibility, allowing it to rotate.

Though endorsing Ampère's elegant mathematical theory, Babbage and Herschel could not explain why this type of induced magnetism required relative motion of the disk or the magnet.[167] Warning against hasty generalization, they remarked:

> Whoever has considered the progress of our knowledge respecting the magnetic virtue, which, first supposed to belong only to iron and its compounds, was at length reluctantly conceded to nickel and cobalt . . . and now extended, apparently with an extraordinary range of degrees of intensity to all the metals— will hardly be inclined to stop short here, but will readily admit, at least the probability, of all bodies in nature participating in it more or less. Yet if the electro-dynamical theory of magnetism be well founded, it is difficult to conceive how that internal circulation of electricity . . . can be excited or maintained in non-conducting bodies.[168]

Babbage and Herschel left the problem of why induction required the motion of the disk or the magnet unanswered.[169] While they did not apply these experimental results to terrestrial magnetic theory, others freely used experimental evidence to make conjectures that fit within the cosmical approach.

Barlow and Christie: Magnetic Research at the Royal Military Academy

As discussed briefly in the previous chapter, several teachers at the Royal Military Academy, Woolwich, pursued both practical and theoretical magnetic research. In particular, the mathematicians Peter Barlow and Samuel Hunter Christie (1784-1865) conducted experiments on local attraction, diurnal variation, and the relationships between magnetism, electricity, light, heat, and rotation. Barlow's practically-oriented research, though less mathematically sophisticated than the theories of Biot, Poisson, and Ampère, nonetheless appealed to mathematics much more than the work of chemists such as Wollaston, Davy, Faraday, and Cumming. During the 1820s, Barlow, like many other British investigators, rejected Coulombian magnetic theory and the Gilbertian notion of the earth as a giant magnet.

Though lacking formal education, Barlow in 1801 successfully completed a competitive examination for the post of assistant mathematical master at the Royal Military Academy. While the majority of Barlow's experimental work focused on remedying local attraction, his research clearly illustrated the re-awakened interest in magnetism and terrestrial magnetism during the 1820s. In 1824, Barlow became a Fellow of the Royal Society of London and the following year he won the Copley Medal for his studies of magnetism and navigational improvements.[170]

In his earliest publications on magnetism, Barlow supported the Gilbertian view which described the earth as a giant magnet, giving its magnetic properties to all ordinary magnets by induction. In support of this Gilbertian view, he remarked in 1814, "almost all the phenomena, which may be exhibited with a usual magnet, may be also exhibited with the earth."[171] Hence, the terrestrial magnet made other objects magnetic and this magnetism remained distinct from electricity. As Barlow incorporated the new experimentation, these views changed.

Beginning in the late 1810s, Barlow performed a multitude of magnetic experiments utilizing the iron shot and machinery of the Royal Military Academy's nearby arsenal, foundry, and dockyard. In 1819, an anonymous account of Barlow's researches contended that William Bain's treatise and the observations of Sabine and Ross in the Arctic regions had "turned the attention of men of science to the deviation produced by the action of the ship upon the needle of the compass."[172] In Barlow's case, this was certainly true as local attraction had indeed drawn him to magnetic research.

Thus, with ship magnetism foremost on his research agenda, Barlow performed an extensive series of experiments to determine the mathematical laws of compass deviations in the presence of iron masses. With a pulley system, Barlow lowered and raised suspended iron balls of different sizes into a table with a round hole in its center while systematically moving a compass on the table around an iron ball situated at different heights above and below the plane of the table. From this procedure he determined the existence of a "plane of no attraction" or set of points where the needle exhibited no deflection from the magnetic meridian (i. e., it behaved as if the iron were not attracting it, hence the name). In *An Essay on Magnetic Attractions* (1820), Barlow summarized his research on a correcting plate devised to correct for the errors arising from local attraction. This highly practical work contained little discussion of theory.

In 1820, Barlow, like Faraday, remained reluctant to commit to any particular magnetic hypothesis; he intended "no hypothesis explanatory of the law of action which may be conceived to have place between the iron and compass." While admitting great respect for the two-fluid magnetic theory of Coulomb and Biot, Barlow complained that it led:

> to such a complicated analysis as to render it wholly useless as a practical theory; and if I were to add that I have some doubts of the accuracy of the deductions on which it is founded, I should be supported by the opinion of some other writers . . . Dr. [Thomas] Young, for example[173]

Echoing Young and the Scottish methodological tradition, Barlow viewed specific hypotheses merely as "convenient vehicle[s] of illustration, for uniting under one general head a number of facts which would be otherwise detached and insulated."[174] Concerning the causes of both magnetism and terrestrial magnetism, he contended that the jury was still out. Hypotheses might be useful, but they should always be used cautiously and based upon careful induction from empirical data.

Though Barlow did not discuss electromagnetism in the 1820 *Essay*, this changed as he incorporated the discoveries of Oersted, Ampère, and others into the second edition of 1823 and later publications as well. Unsatisfied with Ampère's theory, Barlow proposed that electromagnetic effects could be explained by a simple principle:

> that every particle of the galvanic fluid in the conducting wire acts on every particle of the magnetic fluid in a magnetized needle, with a force varying inversely as the square of the distance; but that the action of the particles of the fluid in the wire is neither to attract nor to repel either poles of a magnetic particle, but a tangential force.[175]

Contending that this simple principle conformed with many electromagnetic phenomena, he intended it only as a useful hypothesis:

> I pretend not to illustrate the mechanical principles by which such an action can be produced; I propose only to show, that if such a force be admitted, all the results obtained from the reciprocal action of a galvanic wire and a magnetized needle may not only be explained, but computed, and that the results agree numerically with experiments.[176]

Recognizing the difficulties of explaining his tangential force by mechanical principles, Barlow asserted the prime importance of its agreement with facts. He noted that it must be conceded that "the simple power of attraction is equally difficult to conceive, and that we admit it, not from having any idea of the *modus operandi*, but because we find that it leads to results that are consistent with actual observations."[177] Although Barlow's hypothesis differed from Coulombian theory or electrodynamics, his intent and method had more in common with the French mathematical physicists than the English chemists.

As more experimental evidence emerged, Barlow hypothesized about the connections between magnetic experiments and terrestrial magnetic theory. For instance, in an 1825 letter to John Herschel regarding magnetism of rotation, he explained:

> I think there are strong reasons for assuming, that the magnetism of the earth is of that kind which we call induced magnetism; but at present we have no knowledge of the inductive principle, and are therefore unable to judge, how far the earth's rotation may be influential in producing those discrepancies from the general laws which are known to exist. . . . I beg however to be understood as advancing nothing in this letter, beyond the mere experimental fact above state.[178]

As his research continued, Barlow became less cautious, eventually proposing the sun as the "inductive principle." Furthermore, he endorsed Ampère's idea that terrestrial magnetism reduced to electrical currents.[179] In fact by 1831, Barlow stated that all terrestrial magnetic phenomena probably arose from electricity, and that magnetism as a distinct quality had "no real existence."[180] To demonstrate this, he constructed a wooden globe and distributed galvanic currents across its surface. The globe, he noted, should "exhibit, while under electrical induction, all the magnetic phenomena of the earth."[181] For this purpose, latitudinal grooves were cut in the globe and within the grooves nearly ninety feet of copper wire were laid. After attaching a battery to the ends of the wire, Barlow remarked:

> the whole surface of the globe was put into a state of transient magnetic induction; and consequently . . . a neutralized [from the earth's magnetism] needle freely suspended above such a globe, would . . . take different angles of inclination according to its situation between the equator and either pole.[182]

Thereby, he claimed to have generated "all the phenomena of terrestrial magnetism, without the aid of any body usually called magnetic."[183] Appealing to the thermoelectric effect, Barlow further supposed that replacing all the copper wire with bi-metallic strips might represent all the same phenomena "by the application of heat only."[184] Finally, he suggested the sun as the source of differential heating of metals within the earth's crust. In this manner, Barlow directly applied experimental discoveries to his speculations regarding terrestrial magnetism.

From around 1820, one of Barlow's colleagues at Woolwich, Samuel Hunter Christie (1784-1865) also considered the ramifications of experimental discoveries for terrestrial magnetic theory. Like Barlow, Christie diverged from the English chemists in his application of mathematics to experimental magnetic research. In fact, in 1805, he graduated from Trinity College, Cambridge, as second wrangler and first Smith's prizeman (shared with Thomas Turton).[185] After becoming a mathematical instructor at the Royal Military Academy the following year, Christie read his first paper on magnetism to the Cambridge Philosophical Society in May, 1820.[186] Like Barlow, he attempted to find the mathematical laws by which masses of iron influenced magnetic needles. Reflecting the coincidence of his early views with Ampère's theory, Christie noted in 1821, "the hypothesis which I previously advanced, accords perfectly with the theory to which Ampere has been gradually led by his experiments."[187] Also that year, he noticed a magnetism of rotation in iron similar to that of Arago's copper wheel. Upon further research, Christie supposed in 1825 that the earth's magnetic polarity might be attributed to its rotation. Like Barlow, he speculated that terrestrial magnetism might ultimately derive from the sun. Christie explained:

> Since it appears from all the observations . . . that the direction of the magnetic polarity, which iron acquires by *rotation about an axis* . . . has always reference to the direction of the terrestrial magnetic forces, we must infer that this magnetism is communicated to it from the earth . . . if from these experiments we might be led to attribute the magnetic polarity of the earth to its rotation, we must at the same time suppose a source from which the magnetic influence is derived. Is it not then possible that the sun may be the centre of such influence, as well as the source of light and heat?[188]

In this manner, the rotating earth and the other planets received magnetism from solar influences. If this argument were correct, further experiments and observations on the magnetism of rotation, Christie asserted, might indicate "the cause of the situations of the earth's magnetic poles, and of their progressive movements or oscillations."[189]

In addition to his work on the magnetism of rotation, Christie performed many experiments on the effects of heat and light upon magnetic needle in which he conjectured about terrestrial magnetic phenomena. In another paper from 1825, he supposed that diurnal variation in direction and intensity was caused by the heat of the sun.[190] A year later, in a paper entitled "On the magnetic influence in the solar rays," Christie concluded that there was an effect which the rays had upon magnetic needles independent of any effect produced by heat.[191] Indeed, several Italians in the 1810s claimed to magnetize needles by exposing them to the violet rays of the spectrum.[192] Though Davy and Playfair witnessed these experiments firsthand and Mary Somerville reported in 1826 successful magnetization of needles by the violet ray, these results were eventually discredited.[193] In spite of this, they show the willingness of many British investigators to entertain connections between magnetic and optical phenomena.

Again in 1827, Christie proposed a theory of diurnal variation which drew largely from experimental discoveries of the 1820s. Reading about Seebeck's discovery of thermoelectricity and subsequent work by Cumming confirmed Christie's opinion that "temperature must have a considerable effect in producing some of the phenomena of terrestrial magnetism."[194] Furthermore, he supposed that these effects were modified by solar rays and by rotation. Hence it appeared probable to Christie that the "disturbance of the equilibrium of temperature arises from the inequality of the conducting powers of the atmosphere, and of the land or water, with which it is everywhere in contact" or alternatively, different conducting powers of land and water with various terrestrial strata.[195] To test these ideas on a small scale he proposed an experiment involving the heating of a hollow copper sphere filled with bismuth. While the copper represented the atmosphere, the bismuth was analogous to the earth, each substance in contact with different conducting powers. Unable to correctly heat different points of this copper sphere, Christie nonetheless supposed that certain experiments displayed phenomena akin to daily variation.[196] He concluded that if the apparatus were larger with copper of uniform thickness and contact of the metals perfect throughout, then much light would be shed on the phenomena of terrestrial magnetism. Therefore, more than three centuries after Gilbert, Christie similarly embraced a direct analogy between his apparatus and the earth.

In numerous instances, Christie and Barlow linked specific experiments with general theoretical speculations about the earth's magnetism. Although they no longer applied the Gilbertian notion of a giant terrestrial magnet, both men maintained a direct analogy between the study of magnetism and the study of terrestrial magnetism. Therefore, as with most earlier investigators, an improved experimental understanding of magnetism, for Christie, Barlow, and others in the 1820s, meant an increased understanding of the earth's magnetism as well. The study of one remained closely linked the study of the other. As well, mathematics played an increasingly important role in attempts to understand these phenomena. Though their theories became increasingly less acceptable, Aepinus', Coulomb's, and Poisson's laws of magnetostatics, and Ampère's electrodynamic laws retained solid support. By the late 1820s, Faraday's relative ignorance of mathematics was becoming more the exception than the rule among investigators of magnetism.[197]

Roget, Leslie, Brewster: The Scottish Tradition

Making evident the changes in the study of magnetism during the 1820s were several men educated in the Scottish tradition. While some exhibited characteristic Scottish caution, others diverged from the views of Robison, Playfair, and other Scottish natural philosophers. Whether agreeing with the newly emerging theories or not, Scottish-educated investigators reacted in ways clearly indicating the ongoing transformation of magnetic experimentation and theory. While some remained characteristically cautious, others more willingly embraced the new ideas.

One figure who analyzed the experimentation of the 1820s was London-born Peter Mark Roget (later of thesaurus fame). Roget (1779-1869) studied mathematics on his own before attending Edinburgh University in 1793 at the age of fourteen. Among his many interests at Edinburgh, he attended chemistry lectures of Joseph Black and befriended the moral philosophy professor Dugald Stewart.[198] Taking his M. D. in 1798 at the age of nineteen, Roget, as we shall see, was strongly influenced by the Scottish methodological tradition. Nearly thirty years later, Roget's Scottish training can be seen in his analysis of electromagnetic phenomena.

In 1827, Roget wrote an essay review of the recent works by Ampère (1822) and Barlow (1824). While admitting that new scientific fields had been opened by galvanism and Oersted's discovery, he showed particular admiration for Ampère who seemed "to have constructed the master-key which is adapted to open every compartment of this intricate science, and procure us a clear and consistent view of the whole."[199] Roget complained that Ampère's work had not received enough attention in England. After giving a brief historical overview of the relationship between electricity and magnetism, he turned to Barlow's hypothesis of tangential action between particles of galvanic and electric fluid. This conjecture, in addition to Wollaston's "vertiginous motion," and Faraday's rotary experiments led to the general fact of "transverse rotatory motion in the magnetic and electric fluids, when acting freely on each other."[200] Roget concluded that all the phenomena were immediate and necessary consequences of this fundamental principle.

However, upon turning to Ampère's research, Roget objected to the transverse or rotatory explanations of Barlow, Wollaston, and Faraday. In the Scottish methodological tradition, he argued that electrodynamics assumed the attractive and repulsive actions of the electric currents themselves as "the primitive and fundamental fact, to which . . . all other facts of the sciences both of electro-magnetism and of magnetism itself must be reduced."[201] Also advocating Ampère's theory of terrestrial magnetism, Roget noted that, all magnetic bodies, and the globe of the earth among the number, derive their magnetic properties from currents of electricity continually circulating among the parts of which they are composed, and having, with respect to the axes of these bodies, one uniform direction of revolution."[202]

Therefore, Roget supposed that the electrodynamic hypothesis conformed perfectly with all magnetic and terrestrial magnetic phenomena. In fact, "every experiment that has been tried . . . has served but to confirm the correctness of Ampère's views of the theory of magnetism."[203] Concluding his review, Roget asserted that electrodynamics explained many facts of magnetic induction much more readily than any other theory. In the Scottish methodological tradition, the important aspects of hypotheses for Roget were that they conformed readily with all the observations and reduced them to a simple general fact.

Several years later, Roget continued defending Ampère's electrodynamics from the objections of Faraday and others in the *The Library of Useful Knowledge* (1832) in which he wrote lengthy surveys of galvanism, electricity, magnetism, and electromagnetism.[204] Using Barlow's electrified globe as evidence of the correctness of Ampère's theory, he wrote that if the magnetic needle used in the demonstration were replaced by an electro-dynamic cylinder, then "all phenomena of terrestrial magnetism might be exhibited, without the intervention of magnetism, by means of electricity alone."[205] Roget proposed that the origin of terrestrial electric currents could be traced to the action of solar rays upon the earth, specifically in the equatorial regions. This supposition gained support from the discovery of thermoelectricity.

Continuing in the Scottish methodological tradition, while incorporating new discoveries, Roget defended the notion that all magnetic phenomena reduced to the action of electric currents. This hypothesis, he remarked, satisfied every condition required of a true theory. First, it gave a "complete explanation of all the phenomena, even in their minutest details." Second, it united "the character of simplicity in principle, and comprehensiveness in its applications." Third, the hypothesis suggested new experiments which "led to the discovery of new facts." Fourth, and finally, it presented "greater facility of mathematical investigation, and for the comparison of analytical formulae thence obtained, with the results of experiments."[206] Similar criteria had been used by Robison, Playfair, and Thomas Young in their acceptance of Aepinian theory. Also within the Scottish tradition, Roget hoped for a future synthesis. Thus, when electrodynamics became firmly established electricity and magnetism would become "merely branches of a single and more extended and comprehensive science." After discussing the experiments connecting magnetism with heat and light (e. g., those of Seebeck and Christie), Roget asserted strong grounds for believing

> that there subsists some mutual connexion, or rather an intimate relation and affinity, between the several imponderable agents, namely, *Heat, Light, Electricity*, and *Magnetism*, which pervade in so mysterious a manner all the realms of space, and which exert so powerful an influence over all the phenomena of the universe.[207]

From this statement, he clearly accepted a cosmical view of geophysical forces.

While he agreed with Roget that electromagnetic discoveries placed the connection between magnetism and electricity beyond all doubt, John Leslie, the natural philosophy professor at Edinburgh, took a more cautious stance toward hypotheses and broad cosmical speculations. In 1829, he hopefully concluded that "every thing seems to betoken our near approach to some grand and pervading discovery."[208] Nevertheless, alarmed by facile conjectures linking magnetism, electricity, heat, and other natural powers, he cautioned:

> Patient induction, though much commended, has very few followers at present; and the passion for hypotheses appears to have again obtained ascendancy in the learned world. Vague and fanciful images are but too often substituted for close reasoning. The more popular branches of physics have absolutely grown rank with metaphorical expression.[209]

Leslie reiterated his earlier criticisms of imponderable fluids as well. In 1835, he supposed that imponderables, while ingenious devices, merely shifted the difficulties:

> We must imagine the constitution of the unknown fluid, while the properties of the magnet itself are obvious to the senses. Sound logic, therefore, dissuades us from indulging in dreams hardly more instructive than the occult qualities of the Schoolmen. *The true business of the philosopher though not flattering to his vanity, is merely to ascertain, arrange, and condense the leading facts.*[210]

Therefore, in the Scottish tradition, he sought to reduce the study of magnetism to arranging and classifying general facts.

In contrast to Leslie's hypercritical attitude, David Brewster (1781-1868), like Roget, entertained hypothetical and cosmical speculations, yet remained within the Scottish methodological tradition. In 1794 he entered Edinburgh University studying natural philosophy and mathematics with Robison and Playfair. Brewster also was influenced by Professor of Logic and Metaphysics, James Finlayson who, like Robison and Playfair, believed and taught that unequivocal, immutable truths could be known about physical reality. Finlayson proposed that the careful use of sense experience and logic would lead to these truths.[211] Physics required the use of mathematics and the cautious induction from specific experiment to increasingly general facts.

While Brewster's main interest was experimental optics, he also wrote at length about magnetism and terrestrial magnetism. In 1820, a paper read to the Royal Society of Edinburgh betrayed his affinity for the cosmical approach.[212] Discussing isothermal lines mapped out by Humboldt, Brewster drew tentative connections between the poles of the greatest cold and the earth's magnetic poles. He deemed the imperfect analogy between isothermal and magnetic centers as "too important to be passed over without notice."[213] Brewster cautiously remarked that solutions required more observations upon which to draw inductive conclusions. Though tempered by Scottish prudence, he nevertheless entertained aspects of the cosmical approach and Humboldtian science.

Aptly demonstrating broader changes in the British outlook, Brewster wrote a favorable commentary on Hansteen's book in 1820. Explaining that magnetism should be studied not merely for practical applications, he noted that:

> the science itself, besides the immediate and valuable application of its discoveries to the purposes of navigation, promises to develope so many curious relations, and to throw so much light over the secrets of electricity, and the other chemical or mechanical powers of Nature, *as to demand investigation, though it were but for its own sake.*[214]

Magnetism, he lamented, had not been investigated with "the rapidity or success which might have been expected."[215]

In Scottish fashion Brewster praised Halley's kernel and shell hypothesis for its "simple and beautiful expression for a very complicated class of appearances." Although the hypothesis did not embrace all phenomena, Halley had shown "the track by which the solution of them was to be obtained."[216] The theory, however, erred in supposing an interior nucleus, yet was "perfectly correct" in assuming the existence of four poles or two magnetic axes. Like other Scots, Brewster admitted the utility of cautiously-adopted hypotheses. Comparing Halley's theory to Ptolemaic epicycles, he judged it of "no small use to possess an hypothesis, which shall connect by any plausible principle, the phenomena of so complicated and vast a department of knowledge."[217] Such sentiments paralleled those of Robison, Playfair, and other Scottish physicists.

Why had progress in the study of magnetism been so slow? Brewster explained that the extreme difficulty of finding decisive experiments had "retarded and misled all inquiries." Listing the many barriers to understanding terrestrial magnetism, he included observational errors, the small number of observations, and the scattered way in which they were gathered from voyages. All of these, noted Brewster, had contributed to "perpetuate the obscurity that still conceals this department of science."[218]

In addition, seamen, claimed Brewster, now paid more attention to the movements of the compass. The discoveries of Flinders and others had "introduced a degree of correctness hitherto unknown in such inquiries." Removing the "fulfilment of Mr. Hansteen's anticipations to a very distant epoch," Brewster cautioned that our knowledge of magnetism was "very far from such a consummation."[219]

Brewster's caution and emphasis on observations fit well within the tradition of Scottish natural philosophy. Although praising Hansteen's theory, which claimed to be an empirical result, he judged Hansteen's more speculative conclusions ought not to contradict the "established properties of natural magnets." In this, Brewster argued that understanding earthly magnetic phenomena first required accurate observations of magnets "as we have it in our power to submit to experiments."[220] For instance, he credited Coulomb for first establishing "with considerable certainty" that magnetic attraction or repulsion always acted inversely as the square of the distance. Others asserted the inverse square law earlier, yet their arguments relied upon rather "vague deductions, than on decisive facts."[221]

Although displaying tentative approval for Hansteen's four-pole theory, Brewster's commentary made apparent many difficulties. Merely stating the known facts regarding artificial magnets (e. g., the inverse square law) gave little assistance in examining the great terrestrial magnet. In fact, knowledge of artificial magnets shed little light on terrestrial magnetic phenomena. Spelling out the difficulties, Brewster wrote:

> while so many obstacles continue to retard the progress of both, great part of the subject must remain enveloped in obscurity. At first view, indeed, the results present nothing but the most perplexing intricacy. The magnetic intensity varying at different times, and at different places in the same time; the lines of equal dip, and the lines of equal variation, arranged in such complex forms, and changing their position with inconstant rapidity, at one time to the east, at another to the west, appear to indicate the agency of forces so numerous and so entangled, as to set our power of estimating them for ever at defiance.[222]

Differing from earlier figures such as Robison and Young, Brewster optimistically believed that regularities could be discovered as "certain leading principles arise dimly above the crowd of minute appearances." Despite lacking a comprehensive, intelligible theory, three centuries of work had at least been arranged ready for "fresh augmentation and corrrection."[223]

In part, Brewster credited Hansteen's theory for revived hopes. Testing the theory required a continued accumulation of observations which separated "the correct from the erroneous, and the ambiguous from the decisive."[224] Since Hansteen's calculations coincided fairly well with the observations, Brewster believed that

> the general hypothesis appears to represent the phenomena with a degree of fidelity hardly to be expected, when the simplicity of the former is contrasted with the intricacy of the latter. A far stricter and more extensive scrutiny will, of course, be required to establish the theory on firm foundations . . .[225]

Brewster agreed with Hansteen that the operating principle behind terrestrial magnetic alterations lay not with a moving magnetic nucleus, but in something exterior to the earth. In line with the cosmical view, Hansteen had attributed magnetic variations to the sun and moon. Brewster himself remained unconvinced by Hansteen's speculations, concluding that "beyond the mere elements, the whole science is involved in conjecture."

Brewster awaited future observations to resolve the uncertainties. For instance, though tempered by Scottish caution, he asked in 1823:

> Whether or not the magnetic, or galvanic, or chemical poles of the globe . . . may have their operations accompanied with the production of cold. . . . Or whether the great metallic mass which crosses the globe, and on which its magnetic phenomena have been supposed to depend, may not occasion a greater radiation of heat from those points where it developes its magnetic influence?—are a few points, which we may attempt to discuss, when the progress of science has accumulated a greater number of facts, and made us better acquainted with the superficial condition, as well as the internal organization, of the globe.[226]

In 1837, Brewster published a lengthy treatise on magnetism which reiterated his earlier views. Believing that there had been rapid progress in this area of physics, Brewster again showed his preferences for Hansteen's theory, the Humboldtian approach, and cosmical connections between a variety of natural phenomena. The study of terrestrial magnetism had been much advanced, he noted, by British and French expeditions, Hansteen's journey to Siberia, and Peter Barlow's 1833 chart of magnetic curves.[227] He further deemed Hansteen one of the "most zealous and successful cultivators of magnetical science."[228]

Reiterating earlier connections, Brewster asserted that it was "placed beyond a doubt, that the phenomena of temperature and magnetism are closely connected, and that the latter are powerfully influenced by the former."[229] Regarding experimental discoveries made during the 1820s, however, various questions remained unanswered. First, Brewster asked if terrestrial magnetic phenomena had an electric origin; "that is, is the magnetism developed by electro-magnetic or thermo-magnetic causes?"[230] Second, are they owing to magnetic metals diffused throughout the earth gaining induced magnetism from some exterior cause?

Addressing the first question, Brewster claimed that the electromagnetic hypothesis had been ably supported by Peter Barlow's wired wooden globe. Also supporting this hypothesis was Seebeck's discovery of thermoelectricity. With characteristic caution, Brewster concluded that if it could be shown that solar heat developed magnetism in the earth,

> the great difficulty would be removed; but until this is done, we are disposed to lean to the old enough though not yet exploded notion, that terrestrial magnetism is the effect of magnetic or ferruginous materials, which are disseminated through the mass of the earth, or throughout it atmosphere.[231]

Answering the second question, Brewster concluded that iron and magnetic metals were not regularly diffused as to produce terrestrial magnetic phenomena. Because magnetic intensity did not diminish over the deepest parts of the ocean, during balloon ascents, or at the tops of the highest mountains, Brewster argued the improbability that magnetic phenomena were produced either "by ferruginous matter *near the surface, or far removed from it.*"[232] Hence, as Dalton had presumed, some ferruginous matter probably existed in the atmosphere. Rejecting a regular metallic nucleus or arrangement of metallic strata in the earth's crust, he contended that magnetic materials within the earth only exercised "a disturbing force in rendering irregular the action of some more general cause."

Where did the cause of terrestrial magnetism originate? Brewster supposed, "we are limited to the Sun, to which all the magnetic phenomena have a distinct reference; but whether it acts by its heat or by its light, or by specific rays, or influences of a magnetic nature, must be left to future inquiry."[233] In summary, Brewster noted that terrestrial magnetism resided wholly in the earth's atmosphere and:

> The magnetism which directs the needle is induced upon the magnetic matter in the atmosphere, like that of an iron sphere, by some exterior cause, although it is very probable small local effects may be produced by ferruginous matter within the earth, and near its surface; but the only effect of these will be to produce small irregularities in the intensity of the magnetism of the needle, and in its direction.[234]

Such views clearly went against the Gilbertian notion of the earth as a giant magnet. In addition, Brewster's discussion of isothermals, electromagnetism, thermoelectricity, and other experimental discoveries show his willingness to speculate about the connections between geophysical.

Conclusion: Imponderable Fluids and Nature's Unity?

Though still accepted as useful hypotheses, the distinct imponderable fluids of the Laplacian program became less and less accepted by the late 1820s and early 1830s. Critiques of imponderables became more frequent following the experimental discoveries of the 1820s. The Gilbertian view also declined in popularity as investigators applied new experimental evidence to develop alternatives. Some chose to reduce magnetism to electricity, while others sought a much grander synthesis of forces. Although the understanding of magnetism and terrestrial magnetism had changed dramatically since the late eighteenth century, many phenomena remained unexplained and much discord persisted between different investigators. Despite a general agreement regarding certain magnetic and electromagnetic laws and connections between different phenomena, theoretical consensus did not emerge.

Following Ampère, many agreed that all magnetism was really due to electricity. For example, in 1827, Edward Turner, an extramural lecturer in chemistry at the University of Edinburgh, willingly reduced magnetic effects to the actions of the electric fluid. After studying medicine at Edinburgh from 1816 to 1819, Turner toured the Continent in the early 1820s, studying chemistry and physics in Paris and Göttingen.[235] His popular textbook entitled *Elements of Chemistry* (1827), explained that "the phenomena of galvanism, and probably of magnetism are produced by it [electric fluid]; and it exerts such an influence over chemical changes, as to have given plausibility to the notion that it is the cause of them."[236] Hence, galvanic, magnetic, and chemical phenomena, for Turner, all depended upon the motions of the electric fluid.

Similarly, in *A Preliminary Discourse on the Study of Natural Philosophy* (1830), John Herschel endorsed Ampère's electrodynamics. Characterizing the blending of electricity and magnetism as the "most satisfactory result which the experimental sciences have ever yet attained," he noted that magnetic polarity, attraction, and repulsion had been resolved into one general fact, i. e., that two currents of electricity, moving in the same direction repel, and in opposite directions attract, each other. In addition, Ampère's theory gave Herschel hope that magnetism would "be at length completely merged, as far as the consideration of the acting causes goes, in the more general one of electricity."[237]

In contrast to Roget, however, Herschel did not enthusiastically embrace electrodynamics because the tiny electric currents in each molecule were judged too complex and artificial. Nevertheless, echoing the Scottish methodological tradition, Herschel supposed that if this or a more complicated supposition enabled in "a general point of view a great number of particular facts," making them part of a system and allowing the prediction of new facts, "we would ask, why should it *not* be granted?"[238] It was important for a theory to "represent all the facts, and include all the laws, to which observation and induction lead." Mirroring the Scottish methodological tradition, Herschel asserted that an hypothesis served as "a scaffold for the erection of general laws."[239] Blind or bigoted adherence to hypotheses was the "bane of all philosophy." The safest course was to

> rise by inductions carried among laws, as among facts, from law to law, perceiving, as we go on, how laws which we have looked upon as unconnected become particular cases, either one of the other, or all of one still more general, and, at length, blend altogether in the point of view from which we learn to regard them.[240]

Such an admission for Herschel, however, did not mean treating magnetism, as had Ampère, as merely electrical in origin. Rather "we must proceed as if that origin were totally unknown, and, at least up to a certain point . . . conduct our enquiries into the subject on the same inductive principles as if this branch of physics were absolutely independent of all others."[241] As they had in the Scottish methodological tradition, cautious induction, provisional use of hypotheses, and a desire to find general unifying structure fused together in Herschel's views .

The same year as Herschel's book, Thomas Thomson's *An Outline of the Sciences of Heat and Electricity* (1830) put forth a more cautious interpretation of the relationship between electricity and magnetism. In his discussion of electromagnetism, Thomson remarked "the connexion between electricity and magnetism is so close, that the same theory slightly modified must apply to both."[242] Nonetheless, he retained the assumption of two distinct magnetic fluids: "This hypothesis, with certain suppositions respecting conducting and non-conducting bodies, would enable us to explain all the phenomena of magnetism with considerable success."[243] Despite his doubts, Thomson nevertheless admired Ampère's theory and experimental results:

> even if we should feel disposed to reject the theory, as too bold for the present state of knowledge, the multiplicity of new and important facts which Ampère's papers contain, and the ingenuity and plausibility with which he supports his views, must always give them a distinguished place in the Annals of Electro-Magnetism.[244]

More daring than Herschel or Thomson, Reverend William Whewell of Cambridge University noted in 1834 that magnetism had "so close a connexion with galvanism, that they may be said to be almost different aspects of the same agent."[245] Noting that phenomena produced with magnets could be imitated by coiled galvanic wires, Whewell also appealed to experiments which indicated the generation of electric currents in metalliferous veins in the earth:

> Hence we have undoubtedly streams of galvanic influence moving along in the earth. Whether or not such causes as these produce the directive power of the magnetic needle, we cannot here pretend to decide; they can hardly fail to affect it. The Aurora Borealis too, probably an electrical phenomenon, is said, under particular circumstances, to agitate the magnetic needle.[246]

Despite his caution, he supposed that terrestrial magnetism ultimately derived from the same cause by which electricity operated.

As an advocate of the undulatory theory of light, Whewell affirmed the existence of the luminiferous ether. Uncertain, however, of the relationship between the ether and other imponderable fluids, he wrote:

> whether heat, electricity, galvanism, magnetism, be fluids; or effects or modifications of fluids; and whether such fluids or *ethers* be the same with the luminiferous ether, or with each other; are questions of which all or most appear to be at present undecided, and it would be presumptuous and premature here to take one side or the other.[247]

Regardless of uncertainties, Whewell was convinced of the ether's existence and contended that this mere fact extended "our views of the structure of the universe" and the "power by which it is arranged."[248]

Several years later in a *History of the Inductive Sciences* (1837), Whewell expressed more definite views on the imponderables. Of the role of magnetic fluids, he explained:

> For though the hypothesis accounted, to a remarkable degree of exactness, with large classes of the phenomena, the presence of a material fluid was not indicated by facts of a different kind, such as the spark, the discharge from points, the shock, and its mechanical effects. Thus the belief of a peculiar magnetic fluid or fluids was not forced upon men's minds; and the doctrine above stated was probably entertained by most of its adherents, chiefly as a means of expressing the laws of phenomena in their elementary form.[249]

Whewell further explained that, after the discoveries pointing out the close connection between electricity and magnetism, "no philosopher would dream of assuming electric fluids and magnetic fluids as distinct material agents."[250]

Indeed, Whewell's comment became increasingly true as the acceptance of imponderable fluids faded into the past and the notions of reducing one force to another, and the language of combining and converting these forces from one to the other came to dominate discussions of magnetic and electric theory. In 1834, English chemist William Prout (1785-1850) noted that whether electricity and magnetism were different forms of the same "energies" or whether they were distinct, "it is sufficient for our present purpose to know that *they are inseparably associated with one* *another* . . . and are always present at least in, if they be not the immediate cause of, all molecular actions among ponderable bodies."[251] These ideas applied to terrestrial magnetic theories as more and more geomagnetic data were systematically collected and analyzed. These global efforts eventually led to the "magnetic crusade" in Victorian Britain, international collaboration, and the establishment of a worldwide network of permanent geomagnetic observatories.[252] In the meantime, mathematicians and physicists continued altering their understanding of magnetism (e. g., James Clerk Maxell) and of terrestrial magnetism (e. g., Karl Friedrich Gauss) into the late nineteenth century. As in the past, the one subject remained intimately tied to the other. Investigators continued to grapple with the difficulties of linking magnetism, electricity, light, heat, rotation, and other phenomena in controlled experiments and on a global scale as well. Despite several transformations in the studies of magnetism and terrestrial magnetism since William Gilbert, the subjects retained many of their mysteries.

Notes

[1] *A Continuation to the Alphabetical Index of the Matter Contained in the Philosophical Transactions of the Royal Society of London* (London: W. Bulmer and W. Nicol, 1821), 71-72. This number also illustrates a relative lull in British magnetic research compared with the earlier period. For instance, between 1710 and 1780 the *Philosophical Transactions* cited about seventy articles related to magnets and magnetism. See Paul Henry Maty, *Index for the Philosophical Transactions of the Royal Society of London v. 1-70 (1710-1780)* (London: Printed for Lockyer Davis & Peter Elmsly, 1787), 292-297.

[2] *A Continuation to the Alphabetical Index of the Matter Contained in the Philosophical Transactions of the The Royal Society of London* (London: Richard Taylor, 1833), 34-36.

[3] For Biot's views on magnetism see John Farrar, *Elements of Electricity, Magnetism, and Electro-magnetism, embracing the late discoveries and improvements* (Cambridge, New England: Printed by Hilliard and Metcalf, 1826), 193-304. Farrar's work, with the exception of notes, consisted of translated portions of Biot's *Précis Elémentaire de Physique*, third edition, (Paris, 1824). For Poisson's magnetic work in English journals see "Extract of a Memoir on the Theory of Magnetism." *Quarterly Journal of Science, Literature and the Arts*, 17 (1824), 317-334; "Memoir on the Theory of Magnetism," *Edinburgh Journal of Science*, 1 (1824), 356-358; "Second Memoir on the Theory of Magnetism. By Mr. Poisson," *Quarterly Journal of Science, Literature and the Arts*, 19 (1825), 122-131; and "Abstract of a Memoir on the Theory of Magnetism in Motion," *Edinburgh Journal of Science*, 5 (1826), 328-330.

[4] J. B. Biot, "Sur l'esprit du système," *Mercur de France*, 36 (1809), 113, quoted in James R. Hofmann, *André-Marie Ampère* (Oxford, UK: Blackwell, 1995), 112.

[5] John Leslie, "Observations on Electrical Theories," *The Edinburgh Philosophical Journal*, 11 (1824), 19. This paper was originally written in 1791. For further discussion of Leslie's paper and its adherence to Common Sense philosophical principles see, Richard Olson, *Scottish Philosophy and British Physics* (Princeton, New Jersey: Princeton University Press, 1975), 195-210.

[6] Ibid., 18. Like his predecessors, Robison and Playfair, Leslie preferred geometry over continental analysis for pedagogical purposes. In a geometry textbook of 1809, Leslie explained: "It is a matter of deep regret, that Algebra, or the Modern Analysis, from the mechanical facility of its operations, has contributed, especially on the Continent, to vitiate the taste and purity so conspicuous in the ancient method of demonstration. The study of geometrical analysis appears admirably fitted to improve the intellect, by training the habits of precision, arrangement, and close application." John Leslie, *Elements of Geometry, Geometrical Analysis, and Plane Trigonometry* (Edinburgh: James Ballantyne and Co., 1809), preface, viii-ix.

[7] In 1787, Count Rumford's (1753-1814) experiments suggested that heat was a mode of motion, not a special substance called caloric. His idea, however, did not overturn the valuable caloric theory. See Thomas L. Hankins, *Science and the Enlightenment* (Cambridge: Cambridge University Press, 1985), 77-80. Richard

Phillips (1767-1840) suggested similar ideas, but extended them to many other phenomena. See Richard Phillips, "Electricity and Galvanism explained on the mechanical Theory of Matter and Motion," *Philosophical Magazine*, 56 (1820), 195-200, and Richard Phillips, *Twelve essays on the proximate causes of the material phenomena of the universe: with illustrative notes* (London: Printed by J. and C. Adlard ...., 1821). On Phillips' life and career, see [Thomas Seccombe], "Phillips, Richard" *DNB*, 45: 210-211.

[8] Humphry Davy, *The collected works of Sir Humphry Davy, Bart.*, edited by J. Davy in nine volumes (London: 1839-40) viii: 348.

[9] For full discussion of Galvani's research and the scientific debates regarding animal electricity, see Marcello Pera, *The Ambiguous Frog: The Galvani-Volta Controversy on Animal Electricity*. translated by Jonathan Mandelbaum. originally published as *La rana ambigua*, Giulio Einaudi editore. Torino: 1986 (Princeton, NJ: Princeton University Press, 1992).

[10] Theodore M. Brown, "The Electric Current in Early Ninteenth-Century French Physics," *Historical Studies in the Physical Sciences*, 1 (1969), 62-65. See also Thomas L. Hankins, *Science and the Enlightenment* (Cambridge: Cambridge University Press, 1985), 71-72.

[11] For a discussion of Biot's influential "electrostatic theory" of the Voltaic pile, see Ibid., 69-76. Volta's results quickly reached England in a letter he wrote in 1800 to the President of the Royal Society, Joseph Banks. See also John L. Heilbron, "Volta's Path to the Battery," in *Proceedings of the Symposium on Selected Topics in the History of Electrochemistry*, edited by George Dubpernell and J. H. Westbrook (Princeton, NJ: The Electrochemical Society, Inc., 1978), 39-65.

[12] Humphry Davy, "Experiments on Galvanic Electricity." Journal of Natural Philosophy, Chemistry and the Arts, 4 (1800), 275. See also Humphry Davy, *Elements of chemical philosophy* (London: Printed for J. Johnson, 1812)[Landmarks of Science microform, 1969], 177; David Knight, *Humphry Davy: Science & Power* (Oxford: Blackwell, 1992), 39-41; and R. A. R. Tricker, *Early Electrodynamics: The First Law of Circulation*. Selected Readings in Physics, general editor, D. Ter Haar (Oxford: Permagon Press, 1965), 8-9.

[13] See Colin A. Russell, "The Electrochemical Theory of Sir Humphry Davy, Part I: The Voltaic Pile and Electrolysis," *Annals of Science*, 15 (1959), 1-13, and Colin A. Russell, "The Electrochemical Theory of Sir Humphry Davy, Part II: Electrical Interpretation of Chemistry," *Annals of Science*, 15 (1959), 15-25.

[14] David Knight, *Humphry Davy: Science & Power* (Oxford: Blackwell, 1992), 59.

[15] A. Volta, "Letter of Professor Volta to J. C. Delametherie, on the Galvanic Phenomena," *Journal of Natural Philosophy, Chemistry, and the Arts*, edited by William Nicholson, new series 1 (1802), 135 and William Hyde Wollaston, "Experiments on the chemical Production and Agency of Electricity," *Philosophical Transactions of the Royal Society of London* (1801), 434.

[16] John Bostock, "On the Theory of Galvanism," *Journal of Natural Philosophy, Chemistry, and the Arts*, new series 3 (1802), 69. Bostock (1773-1846) attended Joseph Priestley's chemical lectures in 1792 before attending medical school at Edinburgh with the likes of Thomas Young, Thomas Thomson, and Alexandre Marcet. After earning an M. D. in 1798, he practiced in Liverpool before moving to London in 1817 to pursue science full-time. See Arnold Thackray, "Bostock, John," DSB 2: 335-336 and [Norman Moore], "Bostock, John," *DNB* 5: 422-423

[17] C. H. Wilkinson, *Elements of Galvanism, in theory and practice* (London: John Murray, 1804) vol. I: 254-255. See also C. H. Wilkinson, "Letter from C. Wilkinson, Esq. on Galvanism and Electricity," *Journal of Natural Philosophy, Chemistry, and the Arts*, new series 9 (1804), 175-177, and C. H. Wilkinson, "Galvanic Illustrations and Remarks," *Journal of Natural Philosophy, Chemistry, and the Arts*, new series 10 (1805), 56-58.

[18] "Scientific News, Class of Physical and Mathematical Sciences," *Journal of Natural Philosophy, Chemistry, and the Arts*, new series 10 (1805), 302.

[19] See Geoffrey Cantor, David Gooding, and Frank A. J. L. James, *Michael Faraday* (New Jersey: Humanities Press, 1996), 57-58.

[20] See L. Pearce Williams, *Michael Faraday: a biography* (New York: Basic Books, 1965). With regard to the influence of *Naturphilosophie*, Williams argues for the connection between Samuel Taylor Coleridge, Humphry Davy, and Michael Faraday.

[21] "Extract of a Letter from Brunn, in Moravia, dated January 3, 1802," *Journal of Natural Philosophy, Chemistry, and the Arts*. edited by William Nicholson, series 2, 1 (1802): 234.

[22] H. C. Oersted, "Experiments on Magnetism; by Mr. Ritter, of Jena," *Journal of Natural Philosophy, Chemistry, and the Arts*, edited by William Nicholson. new series 8 (1804), 184-186.

[23] Timothy Shanahan, "Kant, Naturphilosophie, and Oersted's Discovery of Electromagnetism: A Reassessment," *Studies in History and Philosophy of Science*, 20 (1989), 287-305. See also Robert J. McRae, "Ritter, Johann Wilhelm," *DSB*, 11: 473-475 and, Walter D. Wetzels, "Johann Wilhelm Ritter: Romantic physics in Germany," in *Romanticism and the Sciences*, (Cambridge: Cambridge University Press, 1990), 199-212. For Oersted's career see, L. Pearce Williams, "Oersted, Hans Christian," *DSB*, 10: 182-186. Strongly influenced by Kantian philosophy, Oersted (1777-1851) studied astronomy, physics, mathematics, chemistry, and pharmacy at the University of Copenhagen, receiving his pharmaceutical degree in 1797. On the heels of Volta's discovery of current electricity in 1800, he gathered knowledge about electrochemistry while visiting Berlin, Göttingen, and Weimar. During his travels he met Ritter at Göttingen, and listened to lectures on *Naturphilosophie* in Berlin and Jena.

[24] Friedrich W. J. Schelling, translated by Tom Davidson in "Introduction to the Outlines of a System of Natural Philosophy; or On the Idea of a Speculative Physics and the Internal Organization of a System of this Science," *The Journal of Speculative*

Philosophy, 1 (1867), 196. See also Robert C. Stauffer, "Speculation and Experiment in the Background of Oersted's Discovery of Electromagnetism," *Isis*, 48 (1957), 35-36.

[25] See Dan Ch. Christensen, "The Oersted-Ritter Partnership and the Birth of Romantic Natural Philosophy," *Annals of Science*, 52 (1995), 153-185.

[26] H. C. Oersted, quoted in Robert C. Stauffer, "Speculation and Experiment in the Background of Oersted's Discovery of Electromagnetism." *Isis*, 48 (1957), 38.

[27] "Additional Experiments of Mr. Ritter, of Jena, on Galvanic Phenomena," *Journal of Natural Philosophy, Chemistry, and the Arts*, new series 6 (1803), 221-223. See also, "Abstract of a Memoir on Galvanism, sent to the National Institute by Mr. Ritter, of Jena," *Journal of Natural Philosophy, Chemistry, and the Arts*, new series 7 (1804), 288-291; H. C. Oersted, "Experiments with the Electric Pile, by Mr. Ritter, of Jena," *Journal of Natural Philosophy, Chemistry, and the Arts*, new series 8 (1804), 176-180. In 1801, Ritter (1776-1810) announced that a two-part needle made of zinc and silver oriented itself like a compass needle. Later, he imagined the existence of subterranean electricity analogous to terrestrial magnetism. Claiming he had discovered the underlying general principle which governed living and non-living phenomena, he called it "siderism."

[28] "Extract of a Letter to Professor Pictet, from a Correspondent at Munich, upon some galvanico-magnetic Experiments recently made by M. Ritter," *Philosophical Magazine*, 25 (1806), 369.

[29] "Magnetism," *Encyclopaedia Britannica*, Fourth Edition (Edinburgh: A. Bell, 1810), 397.

[30] Ibid., 396.

[31] See "Magnetism," *Encyclopaedia Londinensis, or Universal dictionary of the arts, sciences, literature*, compiled by J. Wilkes (London: Printed by J. Adlard, 1815), 14: 128. This account is taken almost directly from the *Britannica* article from the fourth edition. As well, the fifth and sixth editions were essentially reprints of the fourth edition with minor additions and corrections. See Paul Kruse, *The Story of the Encyclopaedia Britannica, 1768-1943* (Ph.D. Dissertation, University of Chicago, 1958), 128-135.

[32] H. Davy, quoted by Michael Faraday, "Historical Sketch of Electro-magnetism," *Annals of Philosophy*, 18 (1821), 290.

[33] Peter Barlow, *An Essay on Magnetic Attractions and on the laws of Terrestrial and Electro Magnetism*. second edition (London: Printed for J. Mawman, Ludgate Street, 1824), 222.

[34] Humphry Davy, "The Bakerian Lecture. On the relations of electrical and chemical changes," *Philosophical Transactions of the Royal Society of London*, 116

(1826) part III: 385 and P. M. Roget, "Electro-Magnetism," *The Quarterly Review*, 35 (1827), 245.

[35] James Cumming, in J. F. Demonferrand, *A Manual of Electro Dynamics*, translated from French with notes and additions by James Cumming (Cambridge: J. Smith, 1827), preface, iv-v.

[36] H. C. Oersted, quoted in Robert C. Stauffer, "Speculation and Experiment in the Background of Oersted's Discovery of Electromagnetism." *Isis*, 48 (1957), 41-42.

[37] Robert C. Stauffer, "Speculation and Experiment in the Background of Oersted's Discovery of Electromagnetism." *Isis*, 48 (1957), 41-42. See also L. Pearce Williams, "Oersted, Hans Christian," *DSB*, 10: 183 and "Lettre de M. Hachette, professeur à l'Ecole Polytechnique," *Annales de Chimie et de Physique*, 65 (1808), 211-215.

[38] H. C. Oersted, "Experiments on Magnetism; by Mr. [Johann] Ritter, of Jena." *Journal of Natural Philosophy, Chemistry, and the Arts*, edited by William Nicholson. new series, 8 (1804), 184.

[39] For a summary of Oersted's proposal see J. L. Heilbron, "The electrical field before Faraday," in *Conceptions of ether: studies in the history of ether theories, 1740-1900.* edited by G. N. Cantor and M. J. S. Hodge (Cambridge: Cambridge University Press, 1981), 198-199. See also Kenneth L. Caneva, "Ampère, the Etherians, and the Oersted Connexion," *The British Journal for the History of Science*, 13 (1980), 128 and H. C. Oersted, "Sur la propagation de l'électricité," *Journal de physique, de chimie, d'histoire naturelle et des arts*, 62 (1806), 369-375.

[40] "Royal Academy of Sciences of Berlin," *Philosophical Magazine*, 27 (1807), 90-91.

[41] H. C. Oersted, quoted in Robert C. Stauffer, "Speculation and Experiment in the Background of Oersted's Discovery of Electromagnetism," *Isis*, 48 (1957), 38.

[42] L. Pearce Williams, "Oersted, Hans Christian," *DSB*, 10: 183. In 1800, Oersted's speculative chemical ideas had been dismissed by most Parisian chemists.

[43] James R. Hofmann, *André-Marie Ampère* (Oxford, UK: Blackwell, 1995), 215. Oersted's work, *Ansicht der chemischen Naturgesetze, durch die neueren Entdekungen gewonnen* (Berlin, 1812) was published in French, much revised, the following year as *Recherches sur l'Identité des Forces Chimiques et Electriques*, translated by Marcel de Serres (Paris: J. G. Dentu, 1813).

[44] Ibid.

[45] H. C. Oersted, *Recherches sur l'Identité des Forces Chimiques et Electriques*, translated by Marcel de Serres (Paris: J. G. Dentu, 1813), 130. Quoted in Ibid., 216.

[46] See Kenneth L. Caneva, "Ampère, the Etherians, and the Oersted Connexion." *The British Journal for the History of Science*, 13 (1980), 128 and J. L. Heilbron, "The electrical field before Faraday," in *Conceptions of ether: studies in the history of ether theories, 1740-1900.* edited by G. N. Cantor and M. J. S. Hodge (Cambridge: Cambridge University Press, 1981), 199.

[47] H. C. Oersted, *Recherches sur l'Identité des Forces Chimiques et Electriques* (Paris, 1813) quoted by Robert C. Stauffer, "Speculation and Experiment in the Background of Oersted's Discovery of Electromagnetism," *Isis*, 48 (1957), 39.

[48] James Cumming, "On the Connexion of Galvanism and Magnetism," *Transactions of the Cambridge Philosophical Society*, 1(1822), 270.

[49] Adam Walker, *Analysis of a Course of Lectures on Natural and Experimental Philosophy* (Lancaster: William Greer, 1809), 36-38.

[50] See John L. Heilbron, "The electrical field before Faraday." *Conceptions of ether: studies in the history of ether theories, 1740-1900* (Cambridge: Cambridge University Press, 1981), 197-197, notes 47-49.

[51] Robert Eden Scott, *Inquiry into the limits and peculiar objects of physical and metaphysical science, tending principally to illustrate the nature of causation; and the opinions of philosophers, ancient and modern, concerning that relation* (London, 1810), 222. In 1785, Scott (1770-1811) graduated M. A. at the University and King's College, Aberdeen. Three years later he was appointed regent and soon after held co-professoriate in the chair of natural philosophy along with Greek, mathematics, and moral philosophy. He was the sole moral philosophy professor from 1800 until his death in 1811. See J. M. Rigg, *DNB*, 51: 66.

[52] Matthew Allan, "On Chemical Philosophy," *Philosophical Magazine*, 51 (Jan. - June, 1818), 339 (my emphasis).

[53] James Mitchell, *The elements of natural philosophy: illustrated throughout by experiments which may be performed without regular apparatus* (London: Printed for T. and J. Allman, 1819) [Landmarks of science microform, 1970], 272. See [Agnes M. Clerke], "Mitchell, James," *DNB*, 38: 69. The Scottish-educated Mitchell (1786?-1844) moved to London in 1805, becoming a member of the Geological Society of London. He published works on geology, astronomy, and chemistry.

[54] Thomas Young, *A course of lectures on natural philosophy and the mechanical arts* (London: 1807) [New York: Johnson Reprint Corp., 1971], 1: 694.

[55] J. B. Biot, "Magnetism," *The Edinburgh Encyclopaedia*, conducted by David Brewster (Edinburgh: Printed for William Blackwood, 1830), 13: 277.

[56] Thomas Thomson, "Analyses of Books: *Recherches sur l'Identité des Forces Chimiques et Electriques*, par M. H. C. Œrsted . . . Traduit de l'Allemand par M. Marcel de Serres,"*Annals of Philosophy*, 14 (1819), 48.

[57] Ibid.

[58] "Magnetism," *Encyclopaedia Britannica*, Sixth Edition (Edinburgh, Printed for Archibald Constable and Company, 1823), 12: 396. Differences between electricity and magnetism were also noted in "Magnetism." *The Cyclopaedia; or, Universal Dictionary of Arts, Sciences, and Literature*, v. XXII (London: Longman, Hurst, Rees, Orme, and Brown, 1819), n. p.

[59] Dionysius Lardner, *A Manual of Electricity, Magnetism, and Meteorology*, vol. I. (London: Longman, Brown, Green & Longmans, 1841), 205-206.

[60] George Dubpernell, "Von Humboldt's Contributions to Electrochemistry," in *Proceedings of the Symposium on Selected Topics in the History of Electrochemistry*, edited by George Dubpernell and J. H. Westbrook (Princeton, NJ: The Electrochemical Society, Inc., 1978), 66-67.

[61] Alexander von Humboldt quoted in Michael Dettelbach, "Global physics and aesthetic empire: Humboldt's physical portrait of the tropics," in *Visions of Empire: Voyages, botany, and representations of nature*. David Philip Miller and Peter Hanns Reill (eds.), (Cambridge: Cambridge University Press, 1996), 266-267.

[62] Alexander von Humboldt, *Essay on the Geography of Plants with a Physical Portrait of the Tropics* (1807), quoted in Michael Dettelbach, "Global physics and aesthetic empire: Humboldt's physical portrait of the tropics," in *Visions of Empire: Voyages, botany, and representations of nature*. edited by David Philip Miller and Peter Hanns Reill (Cambridge: Cambridge University Press, 1996), 270-271.

[63] See Laura Tilling, "Early experimental graphs," *British Journal for the History of Science*, 8 (1975), 193-213.

[64] Susan Faye Cannon, *Science in Culture: The early Victorian period* (New York: Science History Publications, 1978), 104-105.

[65] John Cawood, "The Magnetic Crusade: Science and Politics in Early Victorian Britain," *Isis*, 70 (1979), 497.

[66] John Macdonald, "Miscellaneous Correspondence," *Gentleman's Magazine*, 90, pt. 2 (1820), 484. See also *Philosophical Magazine*, 57 (1821), 90.

[67] John Macdonald, "On the North-west Magnetic Pole," *Gentleman's Magazine*, 91, pt. 1 (1821), 67-68.

[68] John Macdonald, "On the Discovery of a North-west magnetic Pole," *Gentleman's Magazine*, 91, pt. 2 (1821), 36-37. See also *Philosophical Magazine*, 58 (1821), 99.

[69] David Brewster, "Remarks on Professor Hansteen's *Inquiries concerning the Magnetism of the Earth*," *Edinburgh Philosophical Journal*, 3 (1820), 126.

[70] John Leslie, *Elements of Natural Philosophy*, vol. I. including Mechanics and Hydrostatics, second edition, corrected and enlarged (Edinburgh: Oliver & Boyd, 1829), lvi.

[71] Peter Barlow, "On the present Situation of the Magnetic Lines of equal variation, and their Changes on the Terrestrial Surface," *Philosophical Transactions of the Royal Society of London* (1833), 667.

[72] James Clark Ross, "On the Position of the North Magnetic Pole," *Philosophical Transactions of the Royal Society of London* (1834), 47.

[73] Thomas Thomson, "Scientific Intelligence: V. Magnetism," *Annals of Philosophy*, 12 (1818), 389.

[74] Asgeir Brekke and Alv Egeland, "Christopher Hansteen (1784-1873): A Pioneer in the Study of Terrestrial Magnetism," *Eos*, 67 (1986), 186. See also Kurt Møller Pedersen, "Hansteen, Christopher," *DSB*, 6: 106-107.

[75] For further discussion of Hansteen's four-pole theory see, Gregory A. Good, "Follow the Needle: Seeking the Magnetic Poles," *Earth Sciences History*, 10 (1991), 161-162.

[76] See Christopher Hansteen, *Untersuchungen über den Magnetismus der Erde*, übersetzt von P. Treschow Hanson (Christiana: Jacob Lehmann und Chr. Gröndahl, 1819).

[77] Asgeir Brekke and Alv Egeland, "Christopher Hansteen (1784-1873): A Pioneer in the Study of Terrestrial Magnetism," *Eos*, 67 (1986), 186.

[78] H. C. Oersted, "On Electro-magnetism," *Annals of Philosophy*, 18 (1821), 333-334.

[79] Ibid., 335.

[80] H. C. Oersted, "Thermo-Electricity," *Edinburgh Encyclopaedia*, conducted by David Brewster (First American edition, Philadelphia: Joseph and Edward Parker, 1832), 17: 731.

[81] Hansteen corresponded regularly with Oersted. See M. C. Harding (ed.) *Correspondance de H. C. Örsted avec divers savants*, 1 (Copenhagen, 1920), 77-251. See also Kurt Møller Pedersen, "Hansteen, Christopher," *DSB*, 6: 107.

[82] Christopher Hansteen, quoted in Francis Watkins, *A popular sketch of electro-magnetism, or electro-dynamics* (London: Printed for J. Taylor and Watkins and Hill, 1828)[Landmarks of science microform, 1979], 68. See also Christopher Hansteen, "Zusätze und Berichtigungen zu den Bemerkungen über Polarlichter und Polarnebel von Chr. Hansteen, im Jahrbuche der Chemie und Physik, für 1826," *Jahrbuche der Chemie und Physik*, 18 (1826), 366. Hansteen further asserted that a "cylindrical atmosphere

of neutralized molecules" combined in pairs surrounded the current-carrying wire with each pair contained a north and south magnetic pole.

[83] Christopher Hansteen, quoted in David Brewster, "Remarks on Professor Hansteen's *Inquiries concerning the Magnetism of the Earth,*" *Edinburgh Philosophical Journal*, 3 (1820), 127.

[84] Of these proposals, Hansteen explained: "Each of the planets might thus give rise to a particular magnetic axis in the sun; but as their orbits make only small angles with the sun's equator and each other, those magnetic axes would, perhaps, on the whole, correspond with the several rotary axes. Such planets as have no moons would, on this principle, have but one magnetic axes; the rest would, in all cases, have one axis more than they have moons; if those different axes, by reason of the small angles which the orbits of their several moons form with each other, did not combine into a single axis." Ibid., 123.

[85] Ibid., 127.

[86] John Cawood, "Terrestrial Magnetism and the Development of International Collaboration in the Early Nineteenth Century," *Annals of Science*, 34 (1977), 582.

[87] See Timothy Shanahan, "Kant, Naturphilosophie, and Oersted's Discovery of Electromagnetism: A Reassessment," *Studies in History and Philosophy of Science*, 20 (1989): 302-305.

[88] H. C. Oersted, "Experiments on the Effect of a Current of Electricity on the Magnetic Needle," *Annals of Philosophy*, 16 (1820), 275-276. See also H. C. Oersted translated from the French publication in *Bibliothèque Universelle*, (1820) 14: 279-280. Oersted wrote: "It appears, according to the reported facts, that the electric conflict is not restricted to the conducting wire, but that it has a rather extended sphere of activity around it. . . . Furthermore, it seems that the circular movement, combined with the progressive movement in the direction of the length of the conjunctive wire, should form a mode of action which is exerted as a helix around this wire as an axis." Quoted in James R. Hofmann, *André-Marie Ampère* (Oxford, UK: Blackwell, 1995), 231.

[89] Ibid., 276.

[90] [Michael Faraday], "Historical Sketch of Electro-magnetism," *Annals of Philosophy*, 19 (1822), 107.

[91] See H. A. M. Snelders, "Oersted's discovery of electromagnetism," in *Romanticism and the Sciences*, (Cambridge: Cambridge University Press, 1990), 228-240.

[92] Jean-Baptiste Biot, "On the Magnetism impressed on Metals by Electricity in Motion," read at the public Sitting of the Academy of Sciences, *Quarterly Journal of Science, Literature and Art*, 11 (1821), 284.

[93] James R. Hofmann, *André-Marie Ampère* (Oxford, UK: Blackwell, 1995), 231. Arago was also skeptical until he repeated Oersted's experiment. He commented "people rejected them [the electromagnetic phenomena]. . . . Everyone decided it was impossible." See Kenneth L. Caneva, "Ampère, the Etherians, and the Oersted Connexion," *The British Journal for the History of Science*, 13 (1980), 124, notes 25 and 26.

[94] "Magnetical Electricity," *Literary Gazette or Journal of Belles Lettres, Arts, Politics, etc.*, no. 186 (August 12, 1820), 523.

[95] See H. C. Oersted, "Experiments on the Effect of a Current of Electricity on the Magnetic Needle," *Annals of Philosophy*, 16 (1820), 273-276 and H. C. Oersted, "Galvanism and Magnetism," *Literary Gazette or Journal of Belles Lettres, Arts, Politics, etc.*, no. 196 (October 21, 1820), 682-684 and no. 198 (November 4, 1820), 714-715.

[96] Humphry Davy, "On the magnetic phenomena produced by Electricity," *Philosophical Transactions of the Royal Society of London* (1821), 8.

[97] "Galvanism and Magnetism," *Literary Gazette or Journal of Belles Lettres, Arts, Politics, etc.*, no. 198 (November 4, 1820), 715.

[98] [William T. Brande], "On the Connexion of Electric and Magnetic Phenomena," *Quarterly Journal of Science, Literature and the Arts*, 10 (1821), 361.

[99] J. B. Biot and F. Savart, "Note sur le magnétisme de la pile de Volta," *Annales de Chimie et de Physique*, 15 (1820), 222-223. English translation by O. M. Blunn, "Note on the magnetism of Volta's pile," in R. A. R. Tricker, *Early Electrodynamics: The First Law of Circulation* (Oxford: Permagon Press, 1965), 118-119.

[100] J. B. Biot, *Precis élémentaire de physique expérimentale*, third edition (Paris: Deterville, 1824), 707-723. English translation by O. M. Blunn in R. A. R. Tricker, *Early Electrodynamics: The First Law of Circulation* (Oxford: Permagon Press, 1965), 126. For further analysis of the Biot-Savart law see James R. Hofmann, *André-Marie Ampère* (Oxford, UK: Blackwell, 1995), 231-235.

[101] See Kenneth L. Caneva, "Ampère, the Etherians, and the Oersted Connexion." *The British Journal for the History of Science*, 13 (1980), 121-124.

[102] Ibid., 128. Caneva calls this group of anti-Laplacians the "Etherians." They also accepted anti-Laplacian theories such as the wave theory of light and the atomic theory of matter. Ampère, for instance, wrote to Faraday in 1825: "Everything that has been done since the work of Dr. Young on light and the discovery of M. Ørsted is preparation for a new era. . . . Explanations deduced from the effects produced by the motions of imponderable fluids will gradually replace those now accepted. . . . I believe that we must look to the motions of fluids distributed in space for the explanation of general effects." Quoted in J. L. Heilbron, "The electrical field before Faraday," *Conceptions of ether: studies in the history of ether theories, 1740-1900* (Cambridge: Cambridge University Press, 1981), 203.

[103] James R. Hofmann, *André-Marie Ampère* (Oxford, UK: Blackwell, 1995), 53-56. Ampère proposed a theory of electricity and magnetism which integrated three mechanically-acting elastic fluids (i. e., electric, magnetic, and caloric). His attempt to reduce electricity and magnetism to the mechanics of elastic fluids supposed each molecule was surrounded by a thin atmosphere of caloric with which additional electric or magnetic fluid could be mixed. The action between these fluids and bodies depended solely upon the action of each molecule upon those surrounding it.

[104] See A.-M. Ampère, "Notes, by M. Ampère, of the Communications which he made to the Academy of Sciences," *Philosophical Magazine*, 57 (1821), 47-49.

[105] For a complete summary of Ampère's discoveries and an in-depth analysis of the development of his electrodynamic theory, see James R. Hofmann, *André-Marie Ampère* (Oxford, UK: Blackwell, 1995), 236-350. Hofmann also lists Ampère's French publications on electrodynamics in his bibliography, see Ibid., 385-391. For the sequence of Ampère's discoveries see also L. Pearce Williams, "What were Ampère's Earliest Discoveries in Electrodynamics?" *Isis*, 74 (1983), 492-508.

[106] A.-M. Ampère, translated in "Account of the Discoveries of M. Oersted, respecting the Connection between Magnetism and Galvanism, and the subsequent Researches of Sir Humphry Davy, Bart. M. Ampère, and M. Biot," *Edinburgh Philosophical Journal*, 4 (1821), 173. See also "Description and Use of the Apparatus employed by M. Ampère in his Electro-Magnetic researches," *Edinburgh Philosophical Journal*, 4 (1821), 406-416.

[107] [Michael Faraday], "Historical Sketch of Electro-magnetism," *Annals of Philosophy*, 18 (1821), 276-280. See also "Account of the Discoveries of M. Oersted, respecting the Connection between Magnetism and Galvanism, and the subsequent Researches of Sir Humphry Davy, M. Ampère, and M. Biot," *Edinburgh Philosophical Journal*, 4 (1821), 167-175; and Charles Hatchett, "On the Electro-Magnetic Experiments of MM. Oersted and Ampère,"*Philosophical Magazine*, 57 (1821), 42, 48. Davy independently performed experiments similar to Arago's. See Humphry Davy, "On the magnetic phenomena produced by Electricity," *Philosophical Transactions of the Royal Society of London* (1821), 7-19.

[108] A.-M. Ampère, quoted in "Notice of the Electro-magnetic Experiments of Messrs. Ampère and Arago, read in the Public Sitting of the Royal Academy of Sciences, at Paris," *Literary Gazette or Journal of Belles Lettres, Arts, Politics, etc.*, no. 238 (August 11, 1821), 509.

[109] For an analysis of why and how Ampère modified his theory, see James R. Hofmann, *André-Marie Ampère* (Oxford, UK: Blackwell, 1995), 282-290.

[110] Ampère to Roux-Bordier, *Correspondance du Grand Ampère*, edited by L. de Launay (Paris: Gauthier Villars), 2: 566. Quoted in James R. Hofmann, *André-Marie Ampère* (Oxford, UK: Blackwell, 1995), 275.

[111] J. B. Biot, *Precis élémentaire de physique expérimentale*, third edition (Paris: Deterville, 1824), 2: 771-772. Quoted from English translation by John

Farrar, *Elements of Electricity, Magnetism, and Electro-Magnetism, embracing the late discoveries and improvements* (Cambridge, New England: Printed by Hillard and Metcalf, 1826), 362-363.

[112] See James R. Hofmann, *André-Marie Ampère* (Oxford, UK: Blackwell, 1995), 281-282.

[113] A.-M. Ampère, "Mémoire sur la théorie mathématique de phénomènes électro-dynamiques uniquement déduite de l'expérience . . ." *Mémoires de l'Académie Royale des Sciences de l'Institut de France*, 6 (1823) [1827]: 175-387. Quoted from translation by O. M. Blunn, in R. A. R. Tricker, *Early Electrodynamics: The First Law of Circulation* (Oxford: Permagon Press, 1965), 157 (my emphasis).

[114] See S. D. Poisson, "Mémoire sur la théorie du magnétisme," *Mémoires de L'Académie Royale des Sciences de L'Institut de France*, second series, 5 (1821-22) [1826]: 247-338 and S. D. Poisson, "Second mémoire sur la théorie du magnétisme," *Mémoires de L'Académie Royale des Sciences de L'Institut de France*, second series, 5 (1821-22) [1826]: 488-533. The memoirs were read in 1824, published in 1826, and appeared in the confusingly-dated 1821-22 *Mémoires*.

[115] S. D. Poisson, "Extract of a Memoir on the Theory of Magnetism," *Quarterly Journal of Science, Literature and the Arts*, 17 (1824), 319-320.

[116] For more discussion of Poisson's magnetic theory, see James R. Hofmann, *André-Marie Ampère* (Oxford, UK: Blackwell, 1995), 324-327.

[117] For Poisson's magnetic theory in English journals see, S. D. Poisson, "Memoir on the Theory of Magnetism," *Edinburgh Journal of Science*, 1 (1824), 356-358; "Second Memoir on the Theory of Magnetism. By Mr. Poisson," *Quarterly Journal of Science, Literature and the Arts*,19 (1825), 122-131; and "Abstract of a Memoir on the Theory of Magnetism in Motion," *Edinburgh Journal of Science*, 5 (1826), 328-330.

[118] [Brande, William T.] "On the Connexion of Electric and Magnetic Phenomena," *Quarterly Journal of Science, Literature and the Arts*, 10 (1821), 361-364. For Wollaston's views see also "Connexion between Electricity and Magnetism," *American Journal of Science*, 3 (1821), 386-389.

[119] L. Pearce Williams, *Michael Faraday, a biography* (New York: Basic Books, 1965), 152-153.

[120] David Knight, *Humphry Davy: Science & Power* (Oxford: Blackwell, 1992), 57-59. Knight discusses the influence of the *Naturphilosophe* Friedrich Schelling on the outlook of Davy, Samuel T. Coleridge, and William Wordsworth.

[121] Humphry Davy, *Elements of chemical philosophy* (London: Printed for J. Johnson, 1812) [Landmarks of Science microform, 1969], 164.

[122] Humphry Davy, "On the Electrical phenomena exhibited in vacuo," *Philosophical Transactions of the Royal Society of London* (1822), 64.

[123] Humphry Davy to A.-M. Ampère, 26 May 1821, in S. Ross, "The search for electromagnetic induction, 1820-1831," *Notes and Records of the Royal Society of London*, 20 (1965), 197.

[124] Humphry Davy, "On the magnetic phenomena produced by Electricity," *Philosophical Transactions of the Royal Society of London* (1821), 17.

[125] Humphry Davy, "On the Magnetic Phenomena produced by Electricity, in a Letter from Sir H. Davy to W. H. Wollaston," *Quarterly Journal of Science, Literature and Arts*, 12 (1822), 126.

[126] Humphry Davy, "The Bakerian Lecture. On the relations of electrical and chemical changes," *Philosophical Transactions of the Royal Society of London*, 116 (1826) part III: 383-422.

[127] Ibid., 385-386 (my emphasis).

[128] Ibid., 390.

[129] For a brief summary of Ampère's connections with Davy, Faraday, and several other British scientists, see K. R. and D. L. Gardiner, "André-Marie Ampère and his English acquaintances,"*British Journal for the History of Science*, 2 (1965), 235-245. See also letters between Ampère and Faraday, in *The Correspondence of Michael Faraday*, volume 1, 1811-December 1831, edited by Frank A. J. L. James (London: Institution of Electrical Engineers, 1991).

[130] Michael Faraday to Charles-Gaspard De la Rive, 12 September 1821, *The Correspondence of Michael Faraday*, volume 1, edited by Frank A. J. L. James (London: Institution of Electrical Engineers, 1991), letter 148, 222 (my emphasis).

[131] [Michael Faraday], "Historical Sketch of Electro-magnetism," *Annals of Philosophy*, 18 (1821), 195-200, 274-290 and 19 (1822), 107-117.

[132] [Michael Faraday], "Historical Sketch of Electro-magnetism," *Annals of Philosophy*, 18 (1821), 274.

[133] [Michael Faraday], "Historical Sketch of Electro-magnetism," *Annals of Philosophy*, 19 (1822) 111.

[134] Ibid., 113.

[135] Ibid., 114.

[136] Ibid., 117.

[137] Michael Faraday, "Description of an Electro-magnetical Apparatus for the Exhibition of Rotary Motion," *Quarterly Journal of Science, Literature and Arts*, 12 (1822), 283-285. See also Friedrich Steinle, "Looking for a 'Simple Case': Faraday and Electromagnetic Rotation," *History of Science*, 33 (1995): 179-202. Building upon arguments by David Gooding, Steinle discusses Faraday's contexualizing of the rotary phenomena within a theoretical framework.

[138] Michael Faraday, "On some new Electro-Magnetical Motions, and on the Theory of Magnetism," *Quarterly Journal of Science, Literature and Arts*, 12 (1822), 94.

[139] Faraday was not successful in this quest to generate electricity from magnetism until his discovery of electromagnetic induction in 1831. See L. Pearce Williams, *Michael Faraday, a biography* (New York: Basic Books, 1965), 175-190, and Sydney Ross, "The search for electromagnetic induction, 1820-1831," *Notes and Records of the Royal Society of London*, 20 (1965), 184-219.

[140] Michael Faraday to André-Marie Ampère, 2 February 1822, *The Correspondence of Michael Faraday*, volume 1, edited by Frank A. J. L. James (London: Institution of Electrical Engineers, 1991), letter 165, 252.

[141] Michael Faraday to G. de la Rive, 9 October 1822, *The Selected Correspondence of Michael Faraday*, volume 1, 1812-1848, edited by L. Pearce Williams (Cambridge: Cambridge University Press, 1971), letter 60, 138 (my emphasis).

[142] Michael Faraday, "A course of lectures on the philosophy and practice of chemical manipulation," (1827) MS no. 13 at the Royal Institution, 54. Quoted in J. L. Heilbron, "The electrical field before Faraday." *Conceptions of ether: studies in the history of ether theories, 1740-1900* (Cambridge: Cambridge University Press, 1981), 212, note 91.

[143] Geoffrey Cantor, David Gooding, and Frank A. J. L. James, published originally as *Faraday*, London: Macmillan, 1991. *Michael Faraday* (New Jersey: Humanities Press, 1996), 49-52.

[144] See L. Pearce Williams, *Michael Faraday, a biography* (New York: Basic Books, 1965), Chapter 4.

[145] Michael Faraday to Charles-Gaspard De la Rive, 12 September 1821, *The Correspondence of Michael Faraday*, volume 1, edited by Frank A. J. L. James (London: Institution of Electrical Engineers, 1991), letter 148, 223.

[146] Michael Faraday, "Experimental Researches in Electricity. —Seventh Series," *The Annals of Electricity, Magnetism, and Chemistry; and the Guardian of Experimental Science*, 1 (1836-1837), 366-367.

[147] On Faraday's development of field theory see, Ibid.; L. Pearce Williams, *Michael Faraday, a biography* (New York: Basic Books, 1965); David Gooding and Frank

A. J. L. James (eds.), *Faraday rediscovered: essays on the life and work of Michael Faraday, 1791-1867* (New York: Stockton Press, 1985); and David Gooding, "Final steps to the field theory: Faraday's study of magnetic phenomena, 1845-1850," *Historical Studies in the Physical Sciences*, 11(1981), 231-275. For precursors to the field concept, see John L. Heilbron, "The electrical field before Faraday," *Conceptions of ether: studies in the history of ether theories, 1740-1900* (Cambridge: Cambridge University Press, 1981), 187-213 and David Gooding, "'Magnetic Curves' and the Magnetic Field: Experimentation and Representation in the History of a Theory." in *The Uses of Experiment: Studies in the natural sciences* (Cambridge: Cambridge University Press, 1989).

[148] "Electro-Magnetism. New Experiments by M. Seebeck on Electro-Magnetic Action," *Quarterly Journal of Science, Literature, and the Arts*, 15 (1823), 374. See also "Electro-magnetic Experiment." *Annals of Philosophy*, 20 (1822), 318.

[149] Gerard Moll (1785-1838) studied at the University of Amsterdam and received a Ph. D. in 1809. After studying in Paris, Moll returned to Holland in 1812, becoming the director of the observatory at Utrecht. In 1815, he became professor of physics at Utrecht. Moll held both positions until his death in 1838. See "Moll, Gerard," *DSB*, 9: 459-460.

[150] James Cumming, "On the Application of Magnetism as a Measure of Electricity," *Philosophical Magazine*, 60 (1822), 257.

[151] "Thermo-electric Rotation, by Professor Cumming," *Quarterly Journal of Science, Literature, and the Arts*, 16 (1823), 372-373. See also "Thermo-Electric Rotation.— Mr. Marsh, of Woolwich," *Quarterly Journal of Science, Literature, and the Arts*, 16 (1823), 373-374.

[152] See James Cumming, "On the Development of Electromagnetism by Heat," *Annals of Philosophy*, 21 (1823), 427-429; James Cumming, "A List of Substances arranged according to their Thermoelectric Relations, with a Description of Instruments for exhibiting Rotation by Thermoelectricity," *Annals of Philosophy*, 22 (1823), 177-180; and James Cumming, "On some Anomalous Appearances occurring in the Thermoelectric Series," *Annals of Philosophy*, 22 (1823), 321-323.

[153] James Cumming, "On the Developement of Electro-Magnetism by Heat." read April 28, 1823. *Transactions of the Cambridge Philosophical Society*, 2, part I (1827), 64.

[154] James Cumming, in J. F. Demonferrand, *A Manual of Electro Dynamics*, translated from French with notes and additions by James Cumming (Cambridge: J. Smith, 1827), 232.

[155] Thomas Stewart Traill and William Scoresby, "Electro-Magnetic Experiments and Observations," *Transactions of the Royal Society of Edinburgh*, 9 (1822), 465.

[156] Thomas Stewart Traill, "On Thermo-Magnetism," *Edinburgh Philosophical Journal*, 11 (1824), 261.

[157] Ibid., 262.

[158] Ibid., 262-263. Pursuing magnetic studies into the 1830s, Traill continued to carefully record magnetic intensity. See Thomas Stewart Traill, "Experiments on the Intensity of Terrestrial Magnetism, at Liverpool and Manchester, with Hansteen's Needles," *B.A.A.S. Report* (1831-32), 557 and "Experiments on the Intensity of Terrestrial Magnetism, at Liverpool and Manchester," *Proceedings of the Royal Society of Edinburgh*, 1 (1832), 40-41.

[159] "Hypothesis regarding Magnetism, by Dr. Büchner," *Edinburgh Philosophical Journal*, 14 (1826), 236.

[160] Büchner quoted in "Hypothesis regarding Magnetism, by Dr. Büchner," *Edinburgh Philosophical Journal*, 14 (1826), 237. Büchner's hypothesis, excerpted from his *Elements of Chemistry*, originally appeared in *Archiv fur die gesammte Naturlehre*, 1825, no. 12.

[161] François Arago, "Note concernant les phénomènes magnétiques auxquels le mouvement donne naissance," *Annales de Chimie*, series 2, 32 (1826), 213 ff. See also "M. Arago's magnetic experiments," *Dublin Philosophical Journal*, 1 (1825), 457, and "Notice of the recent Researches of M. Arago, on the Influence of Bodies reckoned not magnetic, on the motions of the Magnetic Needle," *Edinburgh Journal of Science*, 5 (1826), 325-328.

[162] Peter Barlow, "On the temporary magnetic effect induced in iron bodies by rotation," *Philosophical Transactions of the Royal Society of London* (1825), 317-327; Peter Barlow, "Illustrations of some Facts connected with the Developement of Magnetism by Rotation," *Edinburgh Journal of Science*, 5 (1826), 214-218; Samuel Hunter Christie, "On the Magnetism of Iron arising from its rotation," *Philosophical Transactions of the Royal Society of London* (1825), 347-417; Samuel Hunter Christie, "On the magnetism developed in copper and other substances during rotation," *Philosophical Transactions of the Royal Society of London* (1825), 497-509; David Brewster, "Notice of Mr. Christie's Discoveries respecting the Effect of Rotation on the Magnetic Forces," *Edinburgh Journal of Science*, 3 (1825), 135-137; David Brewster, "A Popular Summary of the Experiments of Messrs Barlow, Christie, Babbage, and Herschel, on the Magnetism of Iron and other Metals, as exhibited by Rotation," *Edinburgh Journal of Science*, 4 (1826), 13-19.

[163] L. Pearce Williams, *Michael Faraday, a biography* (New York: Basic Books, 1965), 170.

[164] Charles Babbage and J. F. W. Herschel. "Account of the Repetition of M. Arago's Experiments on the Magnetism manifested by various Substances during the Act of Rotation," *Philosophical Transactions of the Royal Society of London*, 115 (1825), 489-490.

[165] Ibid., 471.

[166] Ibid., 487-488.

[167] L. Pearce Williams, *Michael Faraday, a biography* (New York: Basic Books, 1965), 171.

[168] Charles Babbage and J. F. W. Herschel. "Account of the Repetition of M. Arago's Experiments on the Magnetism manifested by various Substances during the Act of Rotation," *Philosophical Transactions of the Royal Society of London*, 115 (1825), 484.

[169] In 1826, Babbage reported experiments on magnetism and electricity induced by rotation. Reiterating the time factor involved, he wrote, "The essential circumstance in producing the rotation of the suspended magnet is, that the substance revolving below it shall acquire and lose its magnetism in a finite time, and not instantly." Additional experiments extended this principle to rotations arising from electricity. See Charles Babbage, "On electrical and magnetic rotations," *Philosophical Transactions of the Royal Society of London*, 116 (1826), 497-503.

[170] H. I. Sharlin,"Peter Barlow," *DSB*, 1: 459-460.

[171] Peter Barlow, *A New Mathematical and Philosophical Dictionary* (London: Printed by Whittingham and Rowland, 1814), art. "Magnetism."

[172] "Account of some important discoveries in Magnetism, recently made by P. Barlow," *Edinburgh Philosophical Journal*, 1 (1819), 344.

[173] Peter Barlow, quoted in "Analyses of Books: *An Essay on Magnetic Attractions*, particularly as respects the Deviation of the Compass on Ship-Board, occasioned by the local Influence of the Guns, &c.," *Annals of Philosophy*, 16 (1820), 299. See also Thomas Young, *A course of lectures on natural philosophy and the mechanical arts*, (London, 1807) [New York: Johnson Reprint Corp., 1971], vol. i. 685.

[174] Peter Barlow, quoted in "Analyses of Books: *An Essay on Magnetic Attractions*, particularly as respects the Deviation of the Compass on Ship-Board, occasioned by the local Influence of the Guns, &c.," *Annals of Philosophy*, 16 (1820), 300.

[175] Peter Barlow, *An Essay on Magnetic Attractions and on the laws of Terrestrial and Electro Magnetism*, second edition (London: Printed for J. Mawman, Ludgate Street, 1824), 233. The 1824 edition is simply a reprint of the 1823 edition. See Peter Barlow, *An Essay on Magnetic Attractions and on the laws of Terrestrial and Electro Magnetism*, second edition (London: Printed for J. Mawman, Ludgate Street, 1823).

[176] Ibid., 233-234.

[177] Ibid., 256.

[178] Peter Barlow, "On the temporary magnetic effect induced in iron bodies by rotation," *Philosophical Transactions of the Royal Society of London* (1825), 326.

[179] Peter Barlow, *An Essay on Magnetic Attractions and on the laws of Terrestrial and Electro Magnetism*, second edition (London: Printed for J. Mawman, Ludgate Street, 1823).

[180] See Peter Barlow, "On the probable Electric Origin of all the Phenomena of Terrestrial Magnetism; with an illustrative Experiment," *Philosophical Transactions of the Royal Society of London* (1831), 104.

[181] Ibid., 104.

[182] Ibid., 105.

[183] Ibid., 106.

[184] Ibid., 107-108. Barlow wrote that if "the development of terrestrial magnetic phenomena be due to the transmission of caloric and inequality of temperature, we ought to expect the same kind of irregularities in this action as . . . exist in the temperature and climate of places situated geographically the same."

[185] Edgar W. Morse, "Christie, Samuel Hunter," *DSB*, 3: 259-261. Christie entered Trinity College, Cambridge, as a sizar in 1800. In 1805, he took his bachelor's degree. One year later, Christie took a position as mathematical assistant at the Royal Military Academy at Woolwich. Christie was elected a fellow of the Royal Society in 1826 and served as its secretary from 1837 to 1854. During the same period, he also was a professor of mathematics at Woolwich.

[186] S. H. Christie, "On the Laws according to which Masses of Iron influence Magnetic Needles," read in May 1820, *Transactions of the Cambridge Philosophical Society* (1822),147-174.

[187] S. H. Christie, "Observations on Magnetic Observations," *Edinburgh Philosophical Journal*, 5 (1821), 301.

[188] Samuel Hunter Christie, "On the Magnetism of Iron arising from its rotation," *Philosophical Transactions of the Royal Society of London* (1825), 411-412.

[189] Ibid., 412.

[190] S. H. Christie, "On the effects of temperature on the intensity of magnetic forces; and on the diurnal variation of the terrestrial magnetic intensity." *Philosophical Transactions of the Royal Society of London*, 115 (1825), 1-65.

[191] S. H. Christie, "On the magnetic influence in the solar rays." *Philosophical Transactions of the Royal Society of London*, 116 (1826), 230.

[192] David Brewster, "Account of the Experiments of Morichini, Ridolfi, Firmas, and Gibbs, on the Influence of Light in the Development of Magnetism," *Edinburgh Philosophical Journal*, 1 (1819), 239-243. See also Colonel George Gibbs, "On the Connexion between Magnetism and Light," *American Journal of Science*, edited by Benjamin Silliman, 1 (1818), 89-90; John Murray, "On Aphlogistic Phaenomena and the Magnetism of Violet Light," *Philosophical Magazine*, 53 (1819), 268-271.

[193] See Ibid. See also Mary Somerville, "On the magnetizing power of the more refrangible solar rays," *Philosophical Transactions of the Royal Society of London*, 116 (1826), 132-139.

[194] S. H. Christie, "Theory of the Diurnal Variation of the Magnetic Needle, illustrated by experiments," *Philosophical Transactions of the Royal Society of London*, 117 (1827), 308.

[195] Ibid., 310.

[196] Ibid., 351-352.

[197] See for example Thomas Stephen Davies, "Geometrical Investigations concerning the Phenomena of Terrestrial Magnetism," *Philosophical Transactions of the Royal Society of London* (1835), 221-248; and Thomas Stephen Davies, "Geometrical Investigations concerning the Phenomena of Terrestrial Magnetism. Second Series: On the number of points at which a magnetic needle can take a position vertical to the Earth's surface," *Philosophical Transactions of the Royal Society of London* (1836), 75-106. Following in the tradition of Christie and Barlow, Davies was a mathematical instructor at the Royal Military Academy, Woolwich.

[198] "Peter Mark Roget, M.D." *Proceedings of the Royal Society of London*, vol. XVIII (1870): xxviii-xl. Roget spent much of his early career as a physician and lecturer on anatomy and physiology. He was also involved in the beginnings of the public health movement of utilitarian Jeremy Bentham. From 1833 to 1836, Roget served as the first Fullerian professor of physiology at the Royal Institution of London. Succeeding John Herschel in 1827, he served as the secretary of the Royal Society of London until retiring in 1849. From 1827 to 1849 he also edited the Society's Proceedings. See also [Surgeon-Captain W. W. Webb], "Roget, Peter Mark," *DNB*, 49: 149-151.

[199] P. M. Roget, "Electro-Magnetism," *The Quarterly Review*, 35 (1827), 237.

[200] Ibid., 249.

[201] Ibid., 251.

[202] Ibid.

[203] Ibid., 266.

[204] P. M. Roget, *The Library of Useful Knowledge, Natural Philosophy II.* (London: Baldwin and Cradock, 1832).

[205] P. M. Roget, "Magnetism," *The Library of Useful Knowledge, Natural Philosophy II.* (London: Baldwin and Cradock, 1832), 92.

[206] Ibid.

[207] Ibid., 99.

[208] John Leslie, *Elements of Natural Philosophy*, vol. I. including Mechanics and Hydrostatics, second edition, corrected and enlarged (Edinburgh: Oliver & Boyd, 1829), lvi-lvii.

[209] Ibid., lvii.

[210] John Leslie, "Dissertation Fourth; exhibiting a general view of the progress of mathematical and physical science chiefly during the eighteenth century," in *Dissertations on the history of metaphysical and ethical, and of mathematical and physical science*, by Dugald Stewart, Sir James Mackintosh, John Playfair, and John Leslie (Edinburgh: Adam and Charles Black, 1835), 627 (my emphasis).

[211] Edgar W. Morse, *Natural Philosophy, Hypotheses and Impiety: Sir David Brewster confronts the Undulatory Theory of Light* (Ph.D. thesis, University of California, Berkeley, 1972), 36-39.

[212] David Brewster, "Observations on the Mean Temperature of the Globe," *Transactions of the Royal Society of Edinburgh*, 9 (1823), 201-225.

[213] Ibid., 223.

[214] David Brewster, "Remarks on Professor Hansteen's *Inquiries concerning the Magnetism of the Earth*," *Edinburgh Philosophical Journal*, 3 (1820), 126-127 (my emphasis).

[215] Ibid., 124.

[216] Ibid., 125.

[217] Ibid., 128.

[218] Ibid., 126.

[219] Ibid., 129.

[220] Ibid.

[221] Ibid., 132-133.

[222] David Brewster, "Remarks on Professor Hansteen's 'Inquiries concerning the Magnetism of the Earth,'" *Edinburgh Philosophical Journal*, 4 (1820-21), 114.

[223] Ibid., 114-115.

[224] Ibid., 115.

[225] Ibid., 117.

[226] David Brewster, "Observations on the Mean Temperature of the Globe," *Transactions of the Royal Society of Edinburgh*, 9 (1823), 225.

[227] David Brewster, *A treatise on magnetism: forming the article under that head in the seventh edition of the Encyclopaedia Britannica* (Edinburgh: Adam and Charles Black, 1838), iv. See also Peter Barlow, "On the present Situation of the Magnetic Lines of equal variation, and their Changes on the Terrestrial Surface," *Philosophical Transactions of the Royal Society of London* (1833), 667-673.

[228] Ibid., 16-27.

[229] Ibid., 267.

[230] Ibid., 268.

[231] Ibid., 271.

[232] Ibid., 272.

[233] Ibid., 275.

[234] Ibid., 279.

[235] W. H. Brock, "Turner, Edward," *DSB*, 13: 499-500.

[236] Edward Turner, *Elements of chemistry: including the recent discoveries and doctrines of the science* (Edinburgh: W. Tait; London: C. Tait, 1827) [Landmarks of science microform, 1972], 71. Turner (1796-1837) studied medicine at Edinburgh from 1816 to 1819. From 1820 to 1823, he studied chemistry and physics in Europe, hearing lectures by Gay-Lussac, Pelletier, and Robiquet in Paris and learning from Friedrich von Stromeyer in Göttingen. In 1823, he returned to Edinburgh becoming an important extramural lecturer in chemistry and the chemical editor of Brewster's *Edinburgh Journal of Science*. Four years later, Turner became professor of chemistry at the new University of London. See W. H. Brock, "Turner, Edward," *DSB*, 13: 499-500.

[237] John F. W. Herschel, *A preliminary discourse on the study of natural philosophy.* A facsimile of the 1830 edition [New York: Johnson Reprint Corp., 1966], 324.

[238] Ibid., 203.

[239] Ibid., 204.

[240] Ibid.

[241] Ibid., 324.

[242] Thomas Thomson, *An Outline of the Sciences of Heat and Electricity* (London: Baldwin & Cradock, 1830), 559.

[243] Ibid.

[244] Ibid., 569.

[245] William Whewell, *Astronomy and General Physics Considered with Reference to Natural Theology* (London: William Pickering, 1834), 113-114.

[246] Ibid., 114. See also Robert Were Fox, "On the Variable Intensity of Terrestrial Magnetism, and the Influence of the Aurora Borealis upon it," *Philosophical Transactions of the Royal Society of London* (1831), 199-207.

[247] Ibid., 140.

[248] Ibid.

[249] William Whewell, *History of Inductive Sciences, from the earliest to the present times* (London: John W. Parker, 1837) vol. 3: 60.

[250] Ibid., 61-62.

[251] William Prout, *Chemistry, meteorology, and the function of digestion, considered with reference to natural theology* (London: William Pickering, 1834), 43 (my emphasis).

[252] See John Cawood, "Terrestrial Magnetism and the Development of International Collaboration in the Early Nineteenth Century," *Annals of Science*, 34 (1977), 551-587. For specific events in Britain from about 1835 to 1860, see John Cawood, "The Magnetic Crusade: Science and Politics in Early Victorian Britain," *Isis*, 70 (1979), 493-518; Jack Morrell and Arnold Thackray, *Gentlemen of Science: Early years of the British Association for the Advancement of Science.* (Oxford: Clarendon Press, 1981), 354-370, 523-531; and David P. Miller, *The Royal Society of London, 1800-1835: A study in the cultural politics of scientific organization* (Ph.D. dissertation, University of Pennsylvania, 1981), 198-227.

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