The pursuit of magnetic shadows: the formal-empirical dipole field of early-modern geomagnetism – A.R.T. Jonkers

…observations of skylfull pylotts is the onlye waye to bring it in rule; for it passeth the reach of naturall philosophy. – Michael Gabriel, 1576 (Collinson 1867, p.30)

Abstract

The tension between empirical data and formal theory pervades the entire history of geomagnetism, from the Middle Ages up to the present day. This paper explores its early-modern history (1500-1800), using a hybrid approach: it applies a methodological framework used in modern geophysics to interpret early-modern developments, exploring to what extent formal conjectures shaped observation and vice versa. A range of pertinent case studies supports classification of this entire period as proto-scientific, characterised by the initial formation of theories being largely disconnected from observational constraints, and their subsequent evolution being advanced primarily by their empirical falsification, and not necessarily associated with the introduction of an alternative. The few exceptional instances of purely data-driven discovery were essentially due to an improved signal-to-noise ratio.

Understanding the geomagnetic field is tough. Generated inside a hot, mostly liquid iron core roughly the size of Mars, its internal workings remain shielded from direct scrutiny by some three thousand kilometres of impenetrable mantle rock. What we are left with is the pursuit of shadows, the heavily attenuated magnetic outlines of the distant fluid motions at the top of the source region. Physics suggests that this complex system dynamically balances Coriolis force, Lorentz force and buoyancy, locally affected by diverse boundary effects and heterogeneities in pressure, temperature, chemistry and magnetic, thermal, and viscous diffusion. But the extreme conditions in the core prevent adequate representation of any Earth-like magnetohydrodynamo, in either laboratory or numerical simulations, at least for the foreseeable future (decades). Presently, extremely crude simplifications (hyperviscosity, hyperdiffusivity) are routinely imposed to produce results at all.[1]

Part of the problem's intractability is due to its broad extent across scales. The geodynamo's nonlinear interactions span many orders of magnitude in time and space; for example, small changes in flow may cause large changes in magnetic field, and vice versa (Zhang 1999; Zhang and Gubbins 2000). Although deterministically unpredictable on a timescale of a few years, this internal ocean exhibits systemic memory spanning hundreds of millions of years, captured in the crustal palimpsest on which we dwell (Carbone et al. 2006; Jonkers 2003b, 2007). Geomagnetism has always been a difficult problem, and despite five centuries of dogged investigation, empirical insights are still at a premium. A recent survey of geodynamo modelling (Dormy et al. 2000, p.86) identified a paltry seven long-term averaged observations, plus three qualitative features, as useful guidelines. These hardly provide clear direction for future work. Thus theory remains poorly constrained by measurements, and most observed field behaviour is left unexplained.

This tension between empirical data and formal theory pervades the entire history of the geomagnetic discipline. One way to envisage it is as a *couple*, two equal but opposite forces whose lines of action do not coincide. A dipole field of science can thus be imagined that bears analogies to our understanding of the Earth's magnetic field: a simple premise that, upon closer inspection, reveals significant non-dipole parts, unpredictability, and a long, intricate history. This paper explores this perspective in a novel, hybrid fashion: by using a methodological framework borrowed from current geophysics called inverse theory, with which to analyse early-modern geomagnetism in action.

This type of conceptual anachronism (the use of interpretative categories that are alien to the studied period; Jardine 2003, p.127), is all too easily mistaken for pernicious presentism, so some clarification is in order. Firstly, the chosen framework does not contain any tenets or definitions derived from, or specific to, modern geomagnetism or any other science. Instead, it purely describes and classifies relationships between observables, theoretical constructs, and the uncertainties affecting both. It therefore constitutes a most appropriate, well-defined conceptual context in which to study these aspects. And as Hull (1979, p.5, 8, 15) has argued, free historical inquiry should use all evidence and tools presently available in reconstructing the past, even if the studied agents did not possess them (or applied them explicitly). The alternative, a total ban on appeal to such knowledge, could easily lead to historiographical paralysis; some discernment is required (Jardine 2003, p.134-135).

Secondly, writing history invariably involves translation for a contemporary audience (Hull 1979, p.8, 15), and the focus here is, moreover, on the historical identity of various ideas, practices, and works, which have a significance that is not limited to what was, or could have been, originally assigned (Jardine 2000, p.252, 265). Thirdly, neither the interpretative framework used, nor any current understanding of geomagnetism is ahistorically attributed to the early-modern period or its agents. Far from a presentist *Hineininterpretierung*, the aim is to dissect historical case studies in their original theatre, but using the sharpest methodological scalpels presently available, through which a fundamental, persistent imbalance will be laid bare.

Terms and Conditions

Modern geophysics tends to be mathematically rigorous in its modus operandi, and nowhere is this made more explicit than in the definitions of, and interactions between, empirical data and the theoretical constructs that describe and explain them. These methodological concepts are not bound to any particular discipline, time, or place; rather, they describe general attributes of any quantified representation of observable reality, such as: the direction of inference; to what extent a model or theory depends on data; whether a problem is under-, equi-, or overdetermined (or ill-posed); how data resolution and sources of error affect the interpretation; and the trade-off between model complexity and misfit (Menke 1964; Parker 1994; Langel 1987, pp.346-366). Once mastered, these notions can also be used productively in the history of science, identifying trends and watersheds at a conceptual level that remains inaccessible to a traditional history of events.

A generic example will serve to illustrate the most relevant aspects studied hereafter. Consider two towns, A and B, linked by railway. Every day, one train travels from A to B while another one makes the opposite trip. It is a long journey, and given that the trains leave around the same time and tend to move at roughly similar speed, they will likely meet somewhere along the line. The central question is: Where?

One could describe this system with two equations (the model), incorporating average speed and departure times of both trains (the model parameters) as well as several implicit a priori assumptions. Yet regardless of what specific values we choose, we can always calculate when and where, if at all, the trains will meet. This is called the *forward problem*: we feed initial conditions and parameters to our formal engine, crank the handle, and data predictions (and/or new parameter values for the next time step) come out. An example in modern geomagnetism is the *field model*, a numerical dynamo simulation. Due to the multiple nonlinear relationships ruling the relevant physics, we never know in advance what it will do, but once it does it, we know absolutely, completely, and as precisely as required.

Now we turn the problem inside out: assume we only know some, or perhaps none, of these input values in advance, and instead we have collected various reports of train sightings at different points along the track, from which we have to reconstruct what happened. This is called the *inverse problem*, and it is usually much harder to solve than its forward twin. A good geomagnetic example is the *field map*, a global, continuous spatial representation of one of its quantified properties, based on a limited number of irregularly distributed, discrete, error-prone measurements. In this sense, an empirical field map is the opposite of a formal field model.

Whether we can still answer the train intersection question now depends on numerous factors. Crucial is the balance between observations and unknowns, yielding three scenarios. If we have more data than unknowns, the problem is overdetermined, and we can find a range of *non-unique* solutions, e.g., a smooth one that minimises the observational error based on statistics, or a complicated one that interprets all data as error-free. Secondly, if data and unknowns are equal in number, the problem is equidetermined, and may at best have one, unique solution. This means that under certain conditions there is exactly enough information to solve for the unknowns. Thirdly, if we had gathered fewer data than unknowns, the problem is underdetermined, and the only way our theoretical engine will function at all is if we supply the missing parts ourselves, not as synthetic data but as additional a priori assumptions.

Furthermore, note the complicated effects of error, and the distinction made between signal and noise; new questions demand to be addressed. How precisely do we know where each witness saw which train? How accurate was their sense of time? How good is their memory, and our map? Are the reports approximately evenly-spaced along the entire route, or are all witnesses clustered in a single hamlet (spatial resolution)? Can we interpolate reliably between our collected measurements, or are we forced to extrapolate from a limited survey into the wild blue yonder? More fundamentally, can we be sure that our working approximations, for instance, of regular speed, are valid simplifications? Or should we add new parameters to describe the more complex, but more realistic situation of trains encountering stations, rickety bridges, or even a cow on the tracks? How much irregularity is signal, and how much is noise? Lastly, given many trials on as many days, is our final overall estimate more determined by the formal or the empirical, and does this balance change over time, and if so, how and when and why?

It will be clear from the above that proper interpretation of the role of observables in a scientific discipline depends on the type of problem defined (forward or inverse), a priori assumptions, unknowns, data resolution in time and space, awareness and accommodation of error, signal-to-noise ratio, and how theory and measurement can affect one another. These concepts represent a valuable toolkit when analysing early-modern northwest European geomagnetism, here applied in turn to the power of theory, data processing issues, and the power of data. Inevitably, such a thematic approach jumps hither and thither through time; for chronological treatments, consult Benjamin 1895, Fleury Mottelay 1922, Daujat 1945, Still 1946, Balmer 1956, Malin 1987, Good 1991 and Jonkers 2003a.

Table 1. The four phases of early-modern geomagnetic hypotheses

Phase: dipole	Tilt	Dynamics	Disjointed	Parameters
1: axial	—	—	—	0
2: tilted	+	—	—	2 (4, 6…)
3: precessing	+	+	—	5 (10, 15…)
4: disjointed	+	+	+	10 (20, 30…)

Note: parameters multiply with each added dipole; Source: Jonkers 2003a, p.37

Vision versus Verisimilitude

The early-modern history of geomagnetism divides conceptually into four phases of increasing complexity (Table 1). Underlying all is the mistaken belief that a magnetised needle would everywhere respect the distant global magnetic pole(s) directly, rather than following the local *flux* of the *field* (two 19th-century concepts). Given a small number of such attractive points, the resulting postulated global pattern was thought by many to allow the determination of longitude, an unsolved practical problem in oceanic navigation for much of this period (Andrewes 1996). It was primarily seafaring that exposed ever more of Earth's peculiar magnetic features, prompting formal representations to follow suit. But how and to what extent did the empirical actually shape the formation of new ideas?

The medieval notion of the magnetic poles maintaining perfect alignment with the celestial poles gradually gave way in the 16th century to the idea that the dipole held a fixed stance at some angle to the planet's rotational axis. Furthermore, where and when the field's change over time, or *secular variation*, was recognised, new parameters were introduced to describe the dipole's postulated slow precession around the geographic poles. Additional irregularity could furthermore be accommodated by:

a) increasing the number of magnetic poles,
b) relinquishing the constraint of antipodality (phase four), and/or
c) introducing various local disturbing agents.

These formal choices coexisted for much of the studied period, entertained by some and rejected by others, largely based on the same available corpus of published observations. Theoretical choices could thus be founded non-empirically, or supported through highly selective reliance on data. This section will review some examples.

Magnetic declination is the horizontal angle between true and magnetic north. In the 16th century, this difference was quite small near the Azores, the Canaries and the Cape Verdes, but could easily exceed twenty degrees elsewhere, especially in high latitudes. This was more than enough to worry those who relied on the magnetic compass to safely cross oceans and chart treacherous new coastlines. Iberian navigators therefore started to measure this variable discrepancy along their routes, compensated for it, and recorded the values associated with landmarks and ports for future reference in their sailing directions.[2] Speculation regarding the causes ranged widely: some blamed differences in the loadstones used to magnetise their compass needles, or the latter themselves mutating, whereas others discerned divine design, seemingly indicating Nature's preferred prime meridian (where compasses pointed true) from which to reckon longitude (Jonkers 2005).

Spanish cosmographer and examiner of masters Pedro de Medina would have none of it, however. In the third chapter of his 1545 navigational textbook *Arte de Navegar* (translated into French, English, Dutch, and Italian in ensuing decades) he utterly rejected the existence of declination on the grounds that a) compass needles all behave the same way, b) regardless of geographical location, and c) the notion of many magnetic poles is 'a verye greate errour' (Medina 1545, transl. Frampton 1581, p.67v). Empirical support for this categorical denial was, however, entirely absent. Ironically, Medina's sixth chapter describes in detail how to use a gnomon to trace a meridian line, to establish whether a compass was functioning properly. While in reality local declination was thus measured, de Medina would interpret the difference as a technical defect in the instrument.

A mere two years later, Flemish cartographer Gerard Mercator expressed a rather different view. In a letter to his patron Perrenot de Granvelle he elaborated the first of several attempts to determine the exact coordinates of the Arctic magnetic pole through the mathematical technique of spherical crossbearing. Taking the registered needle orientation at Gdansk (14º northeasting) and Walcheren (9º northeasting) to orient two great circles, one through each of these places, he proceeded to calculate their intersection. In modern terms, this is an equidetermined inverse problem: two observations are used to quantify two unknown parameter values: the latitude and longitude of a tilted dipole (the latter itself being an a priori assumption).

In hindsight, one can conclude that Mercator's formal approach was not forced by paucity of evidence, but represented a conscious choice. Because twice did he repeat the exercise in later years, completely disregarding the earlier-obtained values and producing two novel polar locations, adopting 16º44' northeasting at Regensburg in both attempts, but using either the Azores or the Cape Verdes (where declination was supposedly zero) as the other vertex of his spherical triangle. Remarkably, both interpretations are visualised side by side in an inset on his famous world map of 1569, with one 'polus magnetis respectu insularii Capitis Viridis,' the other 'polus magnetis respectu Corui insule.'[3]

These parallel hypotheses, based on different datasets, externalised the discomfort of conflicting observations. The declination measurements and their coordinates were treated as if completely accurate; in the absence of statistical techniques to arrive at a single solution that minimised error, no attempt was made to use all available estimates to delineate the region most likely containing the magnetic pole. Instead, the number of theories simply multiplied in step with the data, leaving the final choice to the beholder. Contrast this with Robert Hooke's 1684 critique of a number of similar schemes, stating 'by comparing several observations together it is found that this theory will not hold.' (Waller 1705, p.483) By this time, the bar of acceptance had clearly been raised.

Rather than ignoring the possibility of uncertainty, it could alternatively be exploited to paper over the cracks of messy reality, allowing the a priori imposition of a more regular magnetic system than what Nature would allow. This is what Flemish preacher and teacher of navigation Petrus Plancius did in the 1590s. By this time, accumulated evidence from roteiros recognised four regions around the globe where the compass purportedly pointed true, spawning the idea of two tilted dipoles inclined along different longitudes. In their simplest arrangement, these were separated by precisely 90º in longitude, creating two declination-free, or *agonic* great circles that crossed at right angles, evenly quartering the globe in alternating zones of northeasting and northwesting. Plancius opted instead for one sector of 60º and three of 100º each (travelling eastwards from the agonic prime meridian over Corvo) with the magnetic poles placed on the Arctic Circle.[4] This suggests an implicit cosmic connection with the poles of the ecliptic, foreshadowing other nearby polar placements as propounded in the early 17th century by Guillaume de Nautonier, Jean Tarde, and Nicolas le Bon, and in the 1730s by Guillaume le Vasseur and Emanuel Swedenborg.[5]

But how well did Plancius's formal concept mirror empirical reality? The answer presented here is based on time-dependent field map *gufm1* (Jackson et al. 2000; Jonkers et al. 2002). This geophysical reconstruction of the field and its evolution over the period 1590-1990 is founded on the world's largest compilation of historical magnetic measurements. It can provide snapshots for any specified time within the covered interval, of any field component, for any latitude and longitude (or large areas, or the entire globe), either on the surface, or at any depth from the crust down to the top of the outer core. Moreover, it can yield an impression of the field's irregular secular change; see the animation of surface isogonics (i.e., isolines of declination) provided online.

[FIGURE 1: PLANCIUS]

The Plancius hypothesis is visualised at the top of Figure 1; a reconstruction of the real geomagnetic field at the time is provided at bottom. Note that most of Plancius's Atlantic (the most heavily traversed ocean) and Asian Pacific declination exhibits the wrong sign, not to mention severe discrepancies in magnitude nearly everywhere else. The confrontation of the formal and the empirical is quite striking here, given the author's claim that it was based on the 43 data points he published with it (1598), of which none was located in the Pacific hemisphere. With two exceptions (England and Natal), all differences between expected (based on his mathematical technique) and "observed" declination (based on the table) nevertheless remained far below 1º. This was an improbably accurate result, achievable only by treating geomagnetism as a forward problem. Closer inspection of the coordinates reveals harder evidence of tampering; for example, a compass measurement at Bantam (Java) was placed 18º54' east of its true longitude, yielding a much improved model misfit. This difference exceeded cartographical uncertainty for that region at the time, sparking a long-running controversy with another maritime expert, Jan Huyghen van Linschoten. Some other identifiable Asian locations likewise differed substantially in longitude: Goa by 14º, Cochin by 15º, Canton by 16º.[6]

Yet it was not empirical concerns that spawned two revisions of the Plancius scheme by polymath Simon Stevin, but purely mathematical ones. Two tilted dipoles cannot possibly account for Plancius's asymmetrical arrangement of agonic meridians; it is a physical and mathematical contradictio in terminis. Stevin's 1599 revision therefore re-interpreted the first three meridians as part of great circles (creating the first-ever magnetic sextupole proposition), equally unsupported by Pacific data, but at least internally consistent. In Stevin's second, 1608 revision, all references to Pacific agonics were dropped, once again without new data there having become available.

[FIGURE 2: BRIGGS DIP-LATITUDE]

Lack of data was even more painfully evident in the study of magnetic inclination (or dip), first recognised by instrument maker Georg Hartmann in 1544

(Hellmann 1898, p.64), and quantified ca. 1580 by compass maker Robert Norman in London. Based on this single measurement, professor Henry Briggs at Gresham College cast a global postulate of an axial dipole into a numerical dip-latitude table, deemed of practical benefit at sea, enabling latitude to be determined with an inclinometer when cloudy weather obscured celestial bodies. As Figure 2 makes clear, the surmised relationship was not only mathematically regular(ised), but also seriously underdetermined, requiring a priori fixing of both termini to arrive at the desired curve. In the ensuing half century, about a dozen individuals (Wright, Blundeville, de Nautonier, Ridley, and Kircher among them) republished this table or presented their own version; none provided significant empirical support. The inconsistency between these attempts was heavily criticised by debunker of superstitions Thomas Browne (1646, p.62; Courtillot and Le Mouël 2007, Fig. 2a/b).

One inescapable conclusion drawn from the London dip measurement was that the source of the Earth's magnetism was to be sought inside the planet, rather than on or above the crust. As Norman wrote: 'This straight lyne must be imagined to proceede from the center of the needle into the globe of the Earth' (Norman 1581, in Hellmann 1898, p.96). Lucasian professor of mathematics William Whiston reiterated this over a century later: 'The true tendency of the north or south end of every magnetick needle is not at all towards that place in the horizon whither the horizontal needle points, but towards another directly under it, in the same vertical' (Whiston 1721, p.3).

Less agreement existed among natural philosophers regarding the actual constitution of the deep Earth, a realm beyond measurement until the early 20th-century advent of damped seismometry. Physician Mark Ridley, a contemporary of Norman, surmised for example that 'the magnetical globe of the Earth's inward substance consisteth neither of sollid loadstone, nor of iron-like mine or clay or suchlike materials, but of a magneticall substance unknowne unto us' (Ridley 1613, p.154). Jesuit polymath Athanasius Kircher imagined quasi-organic magnetic fibres transporting magnetic force from pole to pole through a fiery, cavernous interior (Kircher 1654, pp.340-346; Kircher 1682, p.130). Astronomer Edmond Halley in 1692, and Whiston in 1721, contemplated a solid kernel and crust separated by a (gaseous or liquid, possibly luminous) fluid medium, in which unknown lifeforms might live, according to Whiston 'either on the inner surface of the upper Earth, or outward surface of the central loadstone, or else in the very fluid itself also'. Other spectacular, but equally speculative ideas concerning the inner Earth included a huge spherical central fire, a molten core inside a solid crust, and an internal magnetic kernel '*whose mountains may attract somew.t stronger than its other parts*' as Royal Society Fellow Servington Savery contemplated in 1732.[7]

A literally different way into this problem was offered by laboratory experiments as promulgated by physician William Gilbert. His idea of interpreting a *terrella*, a small, spherically polished lodestone, as a valid proxy for planet Earth far exceeds mere analogy. Rooted in Neoplatonic animism, it constitutes the establishment of mimesis in 17th-century natural philosophical practice, that is, direct imitation rather than semiotic representation: the Earth is a great magnet, and the terrella is a small "child-Earth," exhibiting the same characteristics. Small dip needles, when moved from pole to pole along a meridian on such a little orb, displayed the reassuringly predictable pattern reproduced in Figure 2. An explanation of declination, on the other hand, inspired the more drastic action of carving out a large gap in the lodestone to represent an ocean, with the protruding edges representing continental landmasses. A tiny needle positioned close to an edge would be deflected toward it, whereas in the middle, "out at sea," it would point north without deviation.[8]

Large amounts of crustal magnetic matter on land were thus thought to affect a compass at sea, whereas the deep water and small islands would not. This erroneous notion was judged possibly true ('it may be so') as late as 1689, in a navigation manual by cartographer John Seller (Seller 1672, pp.137-138; Seller 1689, p.149-151), in spite of poignant contemporary criticism. For example, in 1603 Gilbert's main rival, de Nautonier, and others since, argued that any manually-made indent in a terrella's surface would proportionally be vastly deeper than a real ocean (Pumfrey 1989, p.197). Moreover, nowhere in his *De Magnete* did Gilbert provide convincing real-Earth empirical support for his explanation. Historical field map *gufm1* does provide a glimpse of which candidate regions could (and which could not) have been considered. The arrows in the bottom panel of Figure 1 represent the local declination sign (i.e., northeasting or northwesting) along those coasts that could have yielded confirmation (the north Atlantic and around Africa). Nevertheless, declination throughout other significant regions traversed by English ships, such as south American and southeast Asian waters, would have rapidly falsified the entire conceit.

The imagined continental needle deflections had serious consequences for geomagnetic longitude-solutions based on postulated symmetrical field line arrangements (such as Stevin's sextupole), as Gilbert wrote in his fourth book, chapter nine:

> …variation is in divers ways ever uncertain, both because of latitude and longitude and because of approach to great masses of land, also because of the altitude of dominant terrestrial elevation; but it does not follow the rule of any meridian (...) Hence the bounds of variation are not properly defined by great meridian circles… (Gilbert 1600, transl. Fleury Mottelay 1958, pp.251-254; Roller 1959, p.158)

All was not lost, however; information of import to navigators might still be extracted. A hotly debated maritime topic at the time was whether an ice-free Arctic route to the Spice Islands might exist, and if so, which way this gateway lay (eastward through the Kara Sea, or westward past Newfoundland). Based on his theory of continental attraction and his (mistaken) impression that declination was less extreme in the seas north of Russia than north of Canada, Gilbert advocated the northeast passage as most promising. The extent to which this argument may have misled explorers of the Arctic over the next three decades remains unclear.

[FIGURE 3: CRUQUIUS]

Such practical conclusions deriving from theoretical assumptions were not confined to the early 17th century. Two employees of the Dutch East-India Company (or V.O.C.) likewise preferred vision over verisimilitude. The first of these, Nicolaas Cruquius, multi-talented surveyor, cartographer, engineer, and Fellow of the Royal Society, also acted as examiner of masters of the Delft chamber of the VOC (in 1725-1739). In this capacity he had access to all navigational logbooks of East-Indiamen sailing from that city. Furthermore, his private notebooks contain scattered sequences of secular variation at a number of locations the world over. Given this evidence of his obvious awareness of the field's change over time, it remains a mystery why he published in tabular form the static geomagnetic longitude solution visualised at the top of Figure 3. (Cruquius 1738; Engelen and Geurts 1985, pp.iv-v, 15, 18, 20, 27-28, 151). As in Plancius's scheme, vast portions of the globe, including the Atlantic traverse to and from the East-Indies, were dangerously misjudged, and apparently unburdened by empirical concerns (compare Figure 3 bottom). Unlike Plancius's case, no evidence of practical implementation has come to light.

Three decades later, examiner of masters Meindert Semeyns of the VOC's Enkhuizen chamber developed a sophisticated triple nested dipole scheme in which a magnetic kernel, intermediary shell and crust, all revolving with different angular momentum, gave rise to a fiendishly complicated declination pattern at the Earth's surface. The strength of the former navigator's convictions is evident not just in his publications, but also in the deliberations with his peers regarding the revision of the VOC's official sailing directions (1766-68). The great majority of edits concerned (as always) changed values of declination at various way stations, and in almost all instances, Semeyns stubbornly opposed (based on predictions from his magnetic system) the value agreed upon by all others (based on the many recently returned logbooks they had perused). Fortunately for the mariners, none of the synthetic values eventually made it into the new draft (Semeyns 1762; ARA, Dutch state archives The Hague, 1.02.04/8481).

Much larger audiences than professional committees were exposed to theoretical geomagnetic musings through the publication of some isogonic charts. The most famous, data-founded ones will be treated later. But smooth isolines can equally represent preconceptions without (much) empirical underpinning. Even the great populariser of isogonics, Edmond Halley, several years before his Atlantic scientific surveys but shortly after releasing his disjointed kernel-shell hypothesis of 1692 'shewed the map of the south pole wherein he had drawn the several variations, exhibiting at one view the several tracts wherein the variations of the magnetical needle are regularly east & west' (Bodleian Rigaud mss 37 *Extracts Royal Society Journal*, f.74, 31 Jan 1695). French engineer Frezier followed in 1717 with a chart on which nearly all isogonics appeared as smooth ellipses centred on 60º S, 40º W (Frezier 1718, p.1; van Bemmelen 1899, p.54). Mathematical practitioner Samuel Dunn freely admitted that the (never disclosed) principles of a regular geomagnetic theory had also played an important part in laying down the isolines in his 1775 magnetic chart of Atlantic and Indian Ocean (Dunn 1775, pp.8-18; Dunn 1788, p.18) Three years thereafter, French longitude finder Le Monnier illustrated his theoretical disjointed dipole with isogonics, magnetic poles and magnetic equator on a double-hemisphere, equal-angle projection (Arch.Nat.Paris (ANP) MAR G 99, f.86, 93).

Apart from the aforementioned terrella experiments, legitimate within their own ambit but not necessarily transferable to the Earth, and these primarily theory-driven efforts in magnetic thematic mapping, another category of so-called impossible devices and thought experiments (Kuhn 1981) in early-modern geomagnetism also clearly lacks empirical foundation. The most pervasive one was doubtless the rotating terrella: a perpetuum mobile, unerring timepiece, and longitude solution all in one. The first description dates back to Petrus Peregrinus's 1269 *Epistola de Magnete* (ch.10). Postulated bonds of magnetic sympathy would link the revolving skies overhead with a suspended terrella stationed on the motionless globe. If properly aligned with the magnetic celestial poles, it would therefore, like the stellar outermost sphere of the Ptolemean universe, complete one full turn in exactly 24 hours.[9] The idea was championed again over three hundred years later by William Gilbert to support the notion of Earth's diurnal rotation being a magnetic effect. Furthermore, it was submitted to the VOC as a chronometer solution to the longitude problem in 1641 by Georg Konigh, reputedly based on a German prototype by Johan Stocken. The magnetic clock was also put forward by Franciscus Linus (Francis Line) of Liège.[10]

Other magnetic devices unlikely to pass consumer panel judgement include the magnetic telegraph described in Kircher's *Magnes* (two distant compasses rotating in sympathy over cards graduated with the alphabet), various descriptions submitted to the English Board of Longitude claiming contraptions able to show latitude, longitude and declination at a glance; Servington Savery's design for an instrument to measure magnetic kernel topography from declination at the Earth's surface, and various true-pointing magnetic compasses (i.e., unaffected by declination) such as, for example, advertised by natural philosopher De la Hire in 1687 (a single circular steel ring) and by engineer Jacques le Maire in 1732 (using three concentric magnetised rings).[11] Predictably, the few inventions that did make it to the testing stage never failed to disappoint.

The Anvil of Experience

The bewildering variety of theoretical constructs that hallmarks early-modern geomagnetism supports a classification of proto-science; fundamental tenets were still heavily disputed, argumentation was infused with metaphysical reasoning (Neoplatonic sympathy, Gilbertian animism, teleological geocentrism), and speculation was driven by untested (often untestable) deductions and analogies. Perhaps the most facile explanation for this disciplinary immaturity is a perceived lack of large, top-quality data sets: poor instruments, cartography, record-keeping, observation protocol, and information processing afterwards can all be blamed and shamed. Historical reality, however, is infinitely more interesting, albeit less easily generalised. Surprisingly, despite the listed handicaps, many observers of magnetic declination attained a high measurement accuracy; observational error was reduced further with elementary statistics from the 1580s; 18th-century datasets could contain tens of thousands of values, spanning years to decades (incidentally, only one order of magnitude less than *gufm1*, with 365,694 observations spanning four centuries; Jackson et al. 2000). Clearly, a closer look at empirical geomagnetic data is warranted, both in their interactions with formal theory (in the next section), and its processing proper, in particular with regard to resolution, signal versus noise and extrapolation.

It is easy to appreciate the need for more and better data, regardless of the state of the discipline. Even in today's satellite-monitored world, many scientific papers in geomagnetism still contain an almost formulaic exhortation to that effect. This is not just a rhetorical shield against future criticism, but a legitimate perception borne out of research that reveals (a little more of) the extent of our gaps in knowledge. The early-modern era was no different in that respect. When Simon Stevin revised the Plancius hypothesis (originally based on 43 measurement sites), he stressed the preliminary nature of the postulates, liable to be altered when new, more reliable data, either in terms of declination, latitude, or longitude, would become available (Stevin 1608, p.165). Nearly a century on, Halley considered the exact determination of the movements ruling his disjointed quadrupole (still based on a mere 47 locations) to be 'reserved for the industry of future ages,' stating:

> 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 that [sic] the generality of saylors apply. (Halley 1683, pp.220-221)

Nine years later, when he had reassigned the four poles pair-wise to kernel and crust, he kept open the possibility of additional magnetic shells inside the Earth, and again he wielded the crutch of empirical paucity, especially in the Pacific:

> But if it shall in future ages be observed otherwise; we must then conclude there are more of these internal spheres, and more magnetical poles than four, which at present we have not a sufficient number of observations to determine, and particularly in that vast Mar del Zur, which occupies so great a part of the whole surface of the earth.[12]

This perception highlights not just the flexibility of proto-scientific hypotheses, but also the benefits of limited data support, in particular for dynamic interpretations. When mathematical practitioner Henry Bond predicted in 1639 that declination at London would reach zero in 1657, he was taking a gamble, but it paid off handsomely. After its corroboration, he remained in the limelight for two decades, publishing tracts, partaking in the Royal Society's annual declination measurement, and being consulted by scholars and even royalty.[13] When postulated polar precession takes centuries to millennia to complete one revolution, it becomes nigh impossible for contemporary critics to compile counter-evidence of sufficient temporal scope for incontestable falsification.

One could ahistorically condemn as unfalsifiable, and thus unscientific, the ideas of Bond, Williams and Savery (dipole precession period ca. 600 years), of Phillippes, Hooke and Harrison (ca.370), and of Halley (700), Whiston (1,920), Swedenborg (386 and 1,080), Semeyns (1,080 and 2,273), Lovett (506) and Churchman (426 to 5459) (Jonkers 2003a). However, a historiographically more productive stance would recognise the underdetermined temporal dimension as an inherent trait of proto-scientific Earth sciences. The commensurate inability by proponents to corroborate, and opponents to challenge hypotheses immediately upon presentation may very well have facilitated the development of new concepts, ideas that might have received short shrift if launched within the bounds of a rigidly-defined paradigm. Lack of empirical constraints can foster scientific growth and creativity.

Besides data sparsity, a second processing aspect concerns accuracy: how much of a registered value was coming from the deep Earth, and how much was due to physical limitations of the instrument, insufficient care by the operator, nearby sources of magnetic deviation (iron-containing clothing accessories, ship architecture, weaponry and armour, volcanic rocks), or more transient disturbances (e.g., solar or electrical effects)? The key (a priori) question here is where the demarcation was drawn between signal and noise, a decision that directly affects the way data are treated once obtained. Recall, for example, de Medina's rejection of the very existence of signal, and Plancius's choice to adjust the longitudes in his data table, the weakest of his three model parameters, which suffered from the largest error margins. More structural is the profound split in 17th-century geomagnetism between England (producing a steady trickle of precessing dipole schemes) and continental western Europe (where Cartesian influences stressed unpredictability).

Descartes (in his 1644 *Principia*) had followed Gilbert in perceiving the whole Earth as a large magnet, but with many circulating vortices of magnetic particles never reaching the surface, instead traversing deep-seated metallic ore bodies from pole to pole (the reason why the planet was proportionally weaker than a similar-sized lodestone). The distribution of surface declination he deemed primarily the result of crustal heterogeneity; its recently-discovered change over time he attributed to the slow generation and deterioration of iron mines (Marcorini 1988, vol.1, p.189; Daujat 1945, pp.298-302, 308, 311; Benjamin 1895, pp.357-361; Still 1946, pp.114-115, 164-165.). A decade later, Kircher (1654) identified corrosive salts, metallic humours, and subterranean fire as potential destructive agents there. Some of the derivative works produced by Cartesian followers (e.g., Rohault, d'Alencé, le Grand, Bayle, Fabri) elaborated further. Jesuit scholar Honoré Fabri in his 1670 *Physique*, for instance, suggested that magnetic corpuscules exited the crust on their poleward journey, which exposed them to irregular variations in atmospheric circulation and air density.[14] Earthquakes provided another possible factor, mentioned by Kircher and echoed in the 18th century by French naturalist Le Clerc, count of Buffon (1788), and by Royal Society Fellows William Mountaine (1757) and Tiberius Cavallo (1800).[15] Since these physical causes had unpredictable local effects (noise) that largely obfuscated the deep-Earth signal, continental natural philosophy stressed the unfeasibility of data reduction to a simple rule, as well as longitude solutions dependent thereupon.

Nonetheless, this judgement did not prevent the very practical pursuit of accuracy, especially in the maritime realm. From the mid-17th century onward, shipboard observational practice often incorporated multiple readings of declination per day, weather permitting. Subsequently, either an average was computed or (more commonly) the median taken, reducing the standard deviation in some cases to below half a degree (Jonkers 2003a). Similar zeal was expressed in the Dutch VOC with regard to technical improvements that would reduce observational error. Aside from several technological innovations in standard-issue compasses throughout the studied period, one particular event in 1654 stands out. Two highly-regarded maritime experts (C. Lastman and I. Blaeu) then decided to perform a trial to statistically compare the handiwork of two compass makers (two sets of six traditional compasses with a lozenge-shaped needle) with six novel devices that bore two straight parallel needles under the card (ARA 1.04.02/4928). The latter type's much improved directivity was evident in their reduced range relative to a fixed reference; whereas the two traditional sets varied by 5.33 and 3 degrees respectively, the six parallel-needle versions differed by a mere 0.75 degrees.[16] The year thereafter, the improved design became standard issue on Dutch Eastindiamen, and would remain on board until well into the 1710s.[17]

Another textbook example of signal-versus-noise awareness is the discovery of secular variation in London. William Borough's ensemble of eight measurements at Limehouse set the stage in 1580, yielding an average of ca. 11º19' northeasting, neatly in-between two earlier measurements of 11º15' and 11º30'. Forty-two years later, however, Gresham professor of astronomy Henry Gunter was startled to find a mere 6º13' at Deptford, two miles from Borough's site. So he assembled a party of observers (reducing the chance of observer bias or error), and took two extra-large instruments (allowing more precise and mutually consistent readings) back to the original Limehouse location (eliminating geographical differences) where the average of another eight readings (replicating Borough's protocol) again yielded a much lower value of 5º57', confirming the Deptford reading. Crucially, this diminution was over an order of magnitude larger than instrumental inaccuracy could explain at that time.

The only reason why Gunter did not announce the discovery of geomagnetic inconstancy right there and then was because one aspect of ceteris paribus remained unconfirmed; Gunter could not be sure that Borough, with possibly poorer instruments, had not made a mistake or otherwise produced an error. Eleven years later, instrument maker John Marr and Gunter's successor Henry Gellibrand rectified this by taking Gunter's instrument back to the Deptford location to take five morning and six afternoon measurements (accounting for time of day effects), resulting in a combined average of 4º05'. Gellibrand cum suis confirmed this value the following year in Kent (4º01'), using Gunter's needle and four other large instruments to obtain an average of thirteen observations.[18] This time around, all known potential sources of error had been recognised, cross-checked, and accounted for, exposing a clear geomagnetic signal along the new parameter axis of time.

This discovery forced a re-evaluation of all previously accumulated data, including the realisation that undated observations were worthless. In France, intendant Pierre Petit de Monluc was initially reluctant to accept the finding, based on his unfortunate selection of historical testing data from different places that later turned out to have all been acquired ca. 1630. A second set concentrated solely on Parisian records up to 1660, indeed long enough to find incontrovertible evidence of inconstancy (ANP MAR 2JJ 59 *Delisle papers*, bk.15, nos.3-4; Alexandrescu et al. 1996; Pumfrey 1989, pp.188-189; Balmer 1956, p.175). Petit thus re-interpreted small differences as signal where earlier he had discarded them as noise; familiarity breeds content.

Once a trend has been established, the remaining observational scatter also invites a redefinition of the bounds of acceptance. Although the notion of standard deviation did not yet exist, the concept of data *outliers* was quickly incorporated. A good 17th-century example is the Royal Society's Magnetics Committee's annual attempt to verify Bond's predictions. The latter mostly stayed within one degree of observation, but in 1664 the measurements proved inaccurate, ranging between 1º northeasting and northwesting. One historian has interpreted this 'worthless failure' as heralding the imminent collapse of English magnetic philosophy (Pumfrey 1987, pp.8, 17-20). This seems an untenable proposition given that throughout the next decade the Committee continued the verification process to find excellent agreement with Bond's predictions (e.g., a mere three arc minutes difference in 1665). Moreover, a royal committee of investigation was assembled in 1674, which pleaded a year later to king Charles II for financial support to continue research into Bond's hypothesis. In other words, the 1664 anomaly was recognised as being just that, an exception to be omitted from further consideration (Brit.Mus.London Add. 4393/4 *Pell correspondence*, f.40-46v; *Phil.Trans.* 3, 1668, no.40, p.790; Taylor 1954, pp.90-112).

Table 2. Sustained geomagnetic time series obtained at a fixed location

Observer	**Place**	**Time span**	**Measurements**
Graham	London	1722-23	1,000+
Van Musschenbroeck	Leyden	1729-31	daily
Celsius & Hiorter	Uppsala	1740-47	20,000
Canton	London	1756-57	4,000+
De la Cépède	Paris	1778-79, 6 months	3 obs/day
Cassini (IV)	Paris	1783-89	daily
Van Swinden	Franeker	1780s-90s, 10 yrs	hourly
Von Humboldt	Berlin	1806-07	6,000
Arago	Paris	1820-35	50,000

Source: Jonkers 2000

From the early 18th century, a series of increasingly intensive observation protocols ensued with special, more accurate instruments. The greater diligence, consistency and regularity (from daily to hourly, maintained for up to ten years, see Table 2) again shifted the signal-to-noise ratio further in favour of the former. A number of discoveries followed. In 1722, clockmaker George Graham, investigating a source of compass error initially attributed to pivotal friction, eventually recognised that diurnal geomagnetic variation affected the 12-inch needles of his dedicated *declinatorium*. Inventor of the Leyden jar Pieter Van Musschenbroeck combined daily observations of weather (air pressure, humidity, rain, wind) with declination and inclination (1729-31) to investigate whether seasonal geomagnetic inconstancies were correlated with meteorological changes, eventually drawing a negative conclusion. Subsequently, seasonal patterns were classified by astronomer Jean-Jacques Cassini (1780s) into four 3-4 month periods.

Furthermore, after circumstantial evidence by Halley (1716) and W. Derham (1728), two series of parallel magnetic readings made in the 1740s by Graham in London and Anders Celsius and Olof Hiorter in Uppsala confirmed diurnal declination outliers to be linked with the aurora borealis. This conclusion was subsequently confirmed by P. Wargentin and J. C. Wilcke in Stockholm, and arrived at independently by John Canton in 1759 (based on 6-10 readings per day). The latter classified 29 of his 603 observation days as irregular outliers, and correctly interpreted seasonal variations as a solar effect.[19] Hence, this century harboured another shift in empirical processing; instead of focussing on a single signal while discarding the rest as noise, the residuals (of higher temporal resolution) were subjected to further scrutiny to reveal additional (smaller or occasional) signal of different origin.

The last attribute of data processing treated here is data extrapolation, the extension of perceived pattern beyond what was empirically observed, based on a priori formal assumptions. In early-modern geomagnetism, it came in two flavours: spatial and temporal. The former kind is ubiquitous in 16th-century postulated tilted dipoles, in two forms. Firstly, given observed zero declination at some latitude and longitude, this needle behaviour was deemed to hold meridionally (i.e., for all latitudes on that longitude), and often also everywhere on the antimeridian (180º east of it). Secondly, a regular global distribution of declination was inferred from geographically patchy, confined measurements. The oldest example is João de Lisboa’s 1508 hypothesis, which took the empirically attested decreasing northeasting on sailings from Portugal to the Atlantic archipelagoes along the parallel of 38º north to extrapolate not just an agonic great circle, but also a symmetrical declination distribution elsewhere that peaked midway between the two agonic meridians (45º northeasting in Asia, northwesting in the Pacific). Similar conclusions were reached over the next hundred years by Faleiro, Guillen, de Santa Cruz, Rotz, Cortès, de Oviedo, Menendez de Avilés, de Bessard, de Vaulx and de Fonseca (Jonkers 2003a).

By this time, geomagnetic data were increasingly plotted. On his 1576 discovery voyage to find the Northwest Passage, Martin Frobisher marked magnetic observations with tiny arrows on a chart prepared by Borough. In France, Jean Guérard de Dieppe situated his compass data on a Mercator projection of the Atlantic, while in Spain Diego Ramirez de Arellano illustrated his gathered declination data on a map accompanying his printed description of a voyage to Magellan Straits (1620). Robert Dudley's sea atlas *Arcano del Mare* (1646-47) featured 127 charts on which local needle behaviour at sea was inscribed. Even as late as 1788, Buffon included seven ‘cartes magnetiques’ with plotted declinations and inclinations.[20]

[FIGURE 4: HALLEY 1700]

The next conceptual leap, to connect all points of equal value with an unbroken isoline, heralded the birth of magnetic thematic mapping, replacing scattered point values by closed curves (Robinson 1982; Hellmann 1895, pp.5-6, 10). As discussed in the previous section, isogonics could easily be based (predominantly or uniquely) on theory. By contrast, the oldest extant printed isogonic chart of empirical lineage is Halley’s 1701 Atlantic chart for the year 1700, derived foremost from about 150 points he obtained on his two oceanic magnetic surveys (1698-1700, Thrower 1981, pp.56-58, 61). Following criticism by French hydrographer Guillaume Delisle (Bib.Nat.Paris, Nouv.Acq.Fr. 10764, f.17-18v), Halley attributed inaccuracies in depicted declination to extrapolation, stating: 'tis from the accounts of others, and the analogy of the whole, that in such cases I was forc'd to supply what was wanting.' (Halley 1715, p.166-167). Halley's 1702 *Sea chart of the whole world* additionally drew isogonics in the Indian Ocean, based on compiled logbook data from other voyages, but as before, the continents were left blank, as was the entire Pacific; contrast this with Plancius and Stevin a century earlier. Other examples of isogonic extrapolation (or interpolation) across continents can be found on Van Ewyk's 1752 double polar projection, and (dotted only) on Rennell's map of Africa of 1799.[21]

Figure 4 represents *gufm1*'s isogonic reconstruction for 1700, matching Halley's first magnetic chart in spatial bounds and projection. It is included here to disprove recent claims (Fara 1996, pp.93, 108-109, 113) that the astronomer imposed preconceived regularity (i.e., his disjointed quadrupole) on his painstakingly collected observations. Given the known spatial resolution of Halley's surveys (Thrower 1981, p.48), direct comparison of this figure with his best-known isogonic chart yields no significant differences whatsoever. Applying Occam's Razor, the only formal theory Halley can be accused of imposing is the basic mathematics to compute grid cell averages from which isolines were normally constructed (spatial reduction). The same analysis and conclusion can be employed to defend Halley's successors, Mountaine and Dodson (who twice produced a revision, for 1744 and 1756) against similarly unfounded allegations (Fara 1996, pp.108, 112). Figure 5 (top) depicts isogonics based on the tabulated grid (covering oceans only) the two mathematicians published in *Phil.Trans.* (Mountaine and Dodson 1757, pp.335-348), with *gufm1*'s reconstruction below it for comparison. Once again, the minor discrepancies are trivial, confirming the empirical nature of this effort, which involved the reduction of some fifty thousand observations.

[FIGURE 5: DECL1756]

Nevertheless, in temporal respect some empirical isogonic charts did extrapolate beyond reason and experience. In the description accompanying the Atlantic chart, Halley prognosticated regular change for several locations (Thrower 1981, pp.59-60, 368-370). Forty years thereafter, the first attempt to update Halley's world chart by teacher of mathematics Charles Leadbetter ended in failure due to the latter's reliance on linear extrapolation, by several decades, of past local secular change (Mountaine and Dodson 1755, pp.7-8; Taylor 1956, p.241; Taylor 1966, pp.28, 132-133). Yet the most egregious attempt was by hydrographer Jacques-Nicolas Bellin, who republished Mountaine and Dodson's 1756 world chart in his own 1765 *Petit Atlas Maritime*. Leaving all isogonics untouched, and assuming a global increase in northwesting of 9-10 arc minutes per annum, he advised his readers to simply add 1.5º of declination to the copied values (ANP MAP 6JJ/30 no.1; Dulague 1775, p.184; Franco 1947 p.63; Marguet 1917, p.66). Unlike the earlier-encountered untestable propositions of exceedingly slow-moving poles, this gross simplification was already far removed from observable reality at the time of publication (Langel 1987, p.457; see also the online isogonic animation).

Data Dynamics Deconstructed

In 1581, compass maker Robert Norman emphasised that a freely suspended magnetised needle will orient itself to "respect" the Earth's north and south magnetic poles, but is not physically drawn towards either (Hellmann 1898, pp.87, 96-100). Likewise is science directed by the separate forces of the formal and the empirical, without being completely ruled by one or the other. The tension between the two is most apparent at their point of interaction, more specifically, where observables were used constructively, to build and adjust, or destructively, to reject, geomagnetic theory.

Surveying the previous two sections, several examples of positive data application in theory formation can be recalled. Portuguese and Spanish navigators often founded their tilted dipoles on their own experiences traversing the oceans. Norman concluded from his vertically inclined needles that the source was to be sought inside the Earth. The discovery and subsequent confirmation of secular variation opened a new dimension of inquiry and interpretation, eagerly explored and exploited by those entertaining dynamic dipole schemes. When Halley observed an aurora over London in 1716, he inferred a geomagnetic effect from the luminous arch being highest in the magnetic meridian, and striae roughly aligning with local dip (Halley 1716, pp.406-408; Brigss 1967, pp.491-492) From the 1740s, aurorae occurring simultaneously with large-amplitude, erratic diurnal variation offered further support. In 1788, Buffon deduced a quadrupole from a (recently observed) third agonic in the Pacific (Clerc 1788, pp.69-70). To these achievements can be added the work of Alexander von Humboldt around the turn of the 19th century (Hellmann 1895, pp.13-14). He used his collected measurements of relative magnetic intensity (by displacing a needle some fixed distance from its magnetic orientation, and then counting the number of swings in ten minutes, a technique pioneered by Whiston) to deduce a law of regularly decreasing magnetic force from pole to magnetic equator.

Data-driven adjustment of existing theory, although much rarer, can also be broadly categorised as benign. Halley's exploits furnish two examples. His 1683 hypothesis had located four poles in terms of latitude and longitude, but made no mention of their depth. In the published introduction of his 1692 tract, he argued the need for theoretical revision by identifying two empirical constraints he had previously overlooked: a) no lodestone he had ever heard of had more than two poles, and b) these poles never shifted position within the stone by themselves. Halley's solution, as related, was to assign one dipole to a kernel, the other to a crustal shell, and both adhering to Newtonian dynamics.[22]

At that time, Halley imagined these two entities as separated by a <u>non</u>-magnetic fluid intermedium (whether gaseous or liquid remained unspecified, although water was invoked as analogy elsewhere in the text). Interestingly, the astronomer briefly contemplated magnetic fluids, but rejected the notion because they had never been observed: '…the solid parts of the Earth are not to be granted permeable by any other than fluid substances, of which we know none that are any ways magnetical' (Halley 1683, p.567). It is this aspect that Halley revised upon sighting the aurora in 1716 and associating it with geomagnetism. For in his report on the phenomenon in *Phil.Trans.* later that same year, he reasoned: a) subtle magnetic effluvia consist of 'atoms [that] freely permeate the pores of the most solid bodies;' b) '...this subtile matter (...) may now and then (...) be capable of producing a small degree of light;' and c) 'parts of this lucid substance may, on very rare and extraordinary occasions, transude through and penetrate the cortex of our Earth.'[23]

Table 3. Churchman's three disjointed dipole hypotheses (1787-1794)

Publication	Pole (year)	Colatitude	Longitude	Direction	Period in years
1787-89	North 1779	13º56'	274º48'	anticlockwise	464
	South 1777	18º	140º	unknown	unknown
1790	North 1777	13º56'	269º02'	anticlockwise	426
	South 1777	18º	140º	clockwise	5,459
1794	North 1794	30º55'	225º	anticlockwise	1,096
	South 1793	25º14'	158º50'	clockwise	2,289

Note: colatitude is arc distance from nearest geographic pole; longitudes are reckoned east relative to Greenwich; Jonkers 2003a, p.123

Another example of theoretical adjustments made in the face of new empirical evidence is the work of Philadelphia surveyor John Churchman; Table 3 lists the quantified parameters of his three hypotheses. According to the extensive correspondence in the Board of Longitude's archive (Univ.Lib.Cambridge, RGO 14/42 no.5, 14/11 no.11) and the descriptions accompanying the four editions of his *Magnetic Atlas* (1790, 1794, 1800, 1804), Churchman had tested his 1787 hypothesis by first sketching on a globe the isogonics as produced by his disjointed dipole in 1777, and then comparing these with the published magnetic observations of captain Cook, purportedly yielding good agreement. At this stage, the precessional period and direction of the southern pole was apparently still un(der)determined. This omission was amended in the second hypothesis, which also made slight adjustments to the north pole's period (and consequently its longitude). In this case, Churchman claimed, 'recourse has been had to actual observations of the magnetic variation, made at different times, in both hemispheres, at several places' (Churchman 1790, p.105). However, the only declination readings mentioned were Philadelphia in 1790, London in 1657 and Van Diemen's Land in 1642 (Tasman) and 1777 (Cook); if this constituted the full extent of the empirical foundation, then the above assertion suggests rather more than was delivered. The last hypothesis (of 1794) performed better in this respect; not only did it incorporate a series of observations Churchman had himself obtained while travelling along the north American east coast in 1793 (addressing spatial resolution), he moreover included a table of twenty observed and predicted declination values at London (1622-1794) that evinced a good fit of less than half a degree on average (addressing temporal resolution). Note that nearly all parameters of both poles had by then undergone substantial revision. (Churchman 1794, pp.35-45, 49).

Other instances of theory formation or alteration supported by "observation" are more suspect, however, sowing doubt rather than reaping confidence. The freely-suspended rotating terrella clock (presented in various capacities by Peregrinus, Gilbert, Linus, Stocken, and Konigh) is a notorious exemplar. In hindsight obviously a rhetorical thought experiment, its intention was eagerly perverted by Jesuit scholars on a quest to advance geocentrism (Baldwin 1985). By upholding the sympathetic link of the magnetic orb with the Terran sphere and actually performing the experiment, the defenders of papal authority could hoist the desired negative conclusion as ultimate proof of a stable Earth at the centre of creation (Grandamy 1645, preface, pp.73, 81, 83). The planet was instead thought to be prevented from rotating, either diurnally or annually, by the restraining magnetic 'virtus sistiva' (Zucchi 1649, p.186), a divinely bestowed force to maintain constant axial tilt, enabling the harmonious reception of celestial influxes to spawn procreation and health on the blessed Earth (Schott 1659, p.252). It was of course debatable to what degree the imparted teleological, metaphysical, and religious baggage heaped onto this experiment actually followed from direct observation.

More question marks can be placed next to claims of successful testing at sea of geomagnetic longitude solutions, usually by the (hardly unbiased) proponents themselves (e.g., Lisboa, Cabot, Nautonier, Fonseca, Bruno, Feuillée, and Walker; Jonkers 2000). Another peculiar shift is evident in Henry Bond's efforts, which for decades had focussed on magnetic declination. By the mid-1670s, as his case was in danger of receding into obscurity, Bond finally published the triumphant *The Longitude Found* (Bond 1676), in which the main empirical support stemmed from inclination, that is, 97 predictions all over the globe for 1676. Of these, a meagre four (in India, Virginia, south Africa and Magellan's Strait) were presented as agreeing satisfactorily with recent observation. Bond's stated rationale for his change of heart was that the horizontal magnetic orientation was a 'forc'd motion, and not natural' (Bond 1676, p.9, citing Norman), which fails to quench a nagging suspicion that opportunism may also have played a part. Would gathered declinations have raised the nasty spectre of discrepancy over the enterprise, whereas predictions of a fairly uncommon measurement, in faraway places (some with questionable longitude), for the very year of publication, would be increasingly hard to refute in years to come?

Peter Blackborrow certainly thought so. In his riposte *The Longitude Not Found* two years later, he attacked Bond's 'airy imagination' (the postulated magnetic atmospheric sphere), his 'false suppositions' (the dipole's undemonstrated tilt of 8º30' of arc) and the 'impossible conclusions' reached (the gradually revolving global distribution of inclination). Beating Bond at his own game, he produced an alternative table of 93 recently observed inclinations, which consistently and convincingly undermined Bond's imposed regular pattern. Furthermore, it deserves mention that Blackborrow cannot be accused of smearing an opponent to advance his own longitude solution, as a) he had none, and b) his aim was to demonstrate the futility of all such pursuits, stating: '…the longitude is not, nor cannot be found, by the magnetical inclinatory needle' (Blackborrow 1678, title, pp.ii-v, 45-46, 61-77).

An equally strongly-worded reaction, published in 1611 by professor of mathematics Dounot de Barleduc, concerned de Nautonier's tilted dipole scheme of 1603, which embodied some twenty-four thousand gridded predictions. Again the criticism was multi-pronged, piercing a priori assumptions (the extent of dipole tilt), calculation errors, and data manipulation through selection of favourable evidence while discarding everything else (de Nautonier had even pilloried specific observations because they disagreed with his theory). And as in Bond's case, the empirical sledgehammer was merciless:

> Mais les obseruations sont tant differentes, qu'il est impossible de les rapporter souz une seule regle. (…) Les vrays principes de ceste doctrine mecometrique sont les obseruations des declinaisons de l'aiguille: & cest par icelles qu'il faut regler ceste science. (Dounot 1611, pp.1, 4, 8-9)

Edward Wright, Jacques Grandamy, Georges Fournier, and Robert Hooke equally condemned the scheme as groundless; none appeared driven by motives other than to prove the French nobleman wrong.[24]

These are but two examples; roughly one hundred geomagnetic hypotheses were unleashed in early-modern times, of which over three quarters were subsequently refuted in print. In the overwhelming majority of cases, empirically attested irregularity was either the sole foundation, or the most important rationale for rejection. Data could be laboriously compiled from navigational manuscripts, gleaned from scientific publications, received through correspondence, or even observed in one's own backyard (e.g., Slikker 1703, pp.48-54). Yet regardless of source, irrational reality refused to wear spatial or spatio-temporal straightjackets for long. Instances are too numerous to expound here: Norman debunking Cortés; Wright undermining all suppositions of regularity in general; Gilbert rejecting Stevin's sextupole; the failure of Plancius's magnetic longitudes upon naval testing at sea in 1611; VOC officials using secular variation as sole argument to reject a time-invariant longitude solution submitted by Grisly in 1647; Halley recalling specific observations to falsify Gilbert, Descartes, Bond, and Kircher in 1683; hydrographer Delisle doing the same to Halley in 1710 to emphasise the inconstancy of secular acceleration; Parisian academics Cassini and Maraldi using Halley's 1702 isogonic chart (interpreted empirically) to refute the geomagnetic longitude solution submitted in 1731 by de la Croix; Riccioli (1672), Fournier (1676), Millet Dechales (1677), Slikker (1703), Valois (1735), Struick (1768), Erzey (1777), Lorimer (1794), and Cavallo (1800) arguing that observed irregular secular variation made it 'impossible to form a useful theory upon it;' the list goes on and on.[25]

Some specific conjectures invited more versatile refutations. Chief among these was the legendary magnetic mountain, a giant lodestone mound often situated in the high Arctic, believed by some to guide all compasses, accused by others of capturing or even destroying nearby ships that bore iron.[26] Aside from Norman's riposte that inclined needles indicated a deeper source, and André Thevet's classification of such lodestone landmarks as 'une pure fable' in 1586 (BNP mss Fr. 15452, f.34v), Gilbert employed empirical argument: 'For if it were correct, in different place on land and sea the variation point would in geometrical ratio change to east or to west, whereas in reality the arc of variation changes in different ways erratically' (Gilbert 1600, transl. Fleury Mottelay 1958, p.231). Oxford Fellow Nathaniel Carpenter likewise judged it 'a meere coniecture without ground (...) Moreover the disproportion in the degrees of variation in places of equall distance, will easily correct this errour...' (Carpenter 1635, p.61). From the mid-1630s, secular variation provided another lethal attack. Most thorough was Thomas Browne in 1646, pointing out the absence of any visual evidence, the observed effect of crustal deviation being very localised (using Elba as example), and the southern hemisphere requiring a second magnetic mountain of similar strength (equally unobserved). His most ingenious argument, however, was that compasses displayed increased variability in high latitudes, whereas a powerful magnetic source nearby would have caused stronger directivity instead (Browne 1646, pp.70-71).

Equally varied was the opposition against Gilbert's magnetic Earth. Observed compass needles close to, but directed away from, continents were for example put forward by navigators Baffin (1616) and Reael (Waters 1958, p.282; Reael 1651, pp.78-81). Kircher also used collected measurements in his confutation of Gilbert, but additionally mined a philosophically richer vein in recognising that a planet-sized lodestone would have attracted iron far stronger than experience taught (Kircher 1681, pp.251-257; Baldwin 1985, p.159). Naval lieutenant Edward Harrison followed suit, considering the notion of a terrella as proxy for Earth 'a weak and ridiculous opinion' (Harrison 1696, pp.41-42). Jesuit reactions to its supposed diurnal rotation have already passed review; outside of geocentrist circles this idea was also discarded by Kepler, Galilei and Petit (Daujat 1945, pp.164, 178; Petit 1667, pp.529-530).

The recipients of these assorted outpourings of disagreement, if still breathing, tended to react either by vehement rebuttal, stolid regurgitation of earlier claims, or stoic indifference. Extremely few are the remarkable individuals that, in the face of empirical evidence of irregularity, had the courage to admit that Earth's magnetism proved more complex than they had imagined. Cosmographer Alonso de Santa Cruz was one of them; when the tilted dipole he devised in the late 1530s failed to match the first-ever geomagnetic survey compiled en route to the Indies (by João de Castro, in

1539-42), he wrote: 'the whole idea of thinking that the longitude might be learned (…) by means of the variation that the sailing-compass made, or that it produces them proportionally, left me.' (Santa Cruz post-1542, transl. Bankston 1992, p.20) Two and a half centuries later, the earlier-encountered John Churchman was equally brave. For many years he had produced charts, globes, memoranda, petitions, three disjointed dipole hypotheses and four editions of his *Magnetic Atlas*. But in 1804 he consulted a chart of Baffin's Bay made by Aaron Arrowsmith, which carried recent magnetic measurements there. Following 'mature deliberation' concerning his two magnetic poles, he eventually concluded: 'from a multitude of observations it appears that two alone are not sufficient' (RGO 14/42 no.5, f.138).

Table 4. Class attributes of early-modern geomagnetic hypotheses, per century

Class	**Attribute**	**16th century**	**17th century**	**18th century**
Data (empirical)	*Set Size*	order: 10^1	order: 10^2	order: 10^4
	Plotting	local values	global values	global isolines
	Extrapolation	Spatial	spatial; temporal	temporal
	Error	large; unquantified; underestimated; poor cartography	parallel needles, statistical reduction, improved cartography	standardised instruments and measurement practice, excellent cartography
Theory (formal)	*A Priori Assumptions*	direct ferromagnetic polar attraction; antipodality; meridional agonics; fixed in time	terrella mimesis, dynamics (SV), circular precession, vortices, fibres, kernel & shell	kernel & shell(s); disjointed dipoles; double/single vortex; solid kernel rejected
	Complexity	Low (2 QP), large misfit	medium (5 QP), moderate misfit	high (10 QP), smaller misfit
	Uniqueness	underdetermined, equidetermined	spatially overdet., temporally underdet.	spatially overdet., temporally underdet.

Note: SV = secular variation; QP = number of quantified parameters per postulated dipole

Historiography, like geomagnetism, is a fundamentally underdetermined inverse problem. It seems likely that there will always be more unknowns than we can solve for in both disciplines. Nevertheless, when studying the tension between the formal and the empirical, the methodological toolkit borrowed from modern inverse theory has at least proved productive in separating some signal from noise in early-modern geomagnetism. Table 4 invokes the main concepts one final time in the somewhat Procrustean effort of assigning class attributes per century. Among its features are the massive increase in geomagnetic data in the 18th century, as well as improvements in instruments and measurement, and the advent of isoline representations. In terms of observational error, though, the main breakthrough was the 17th-century introduction of simple statistical procedures, coupled in the formal arena with the transition from under- and equidetermined problems to (spatially) overdetermined ones. Yet despite these various discontinuities, the overall complexity of constructs can be seen to rise fairly steadily with time.

In surveying the interactions of theory with data, a few exceptional events fit the traditional mould of theoretical evolution through empirical discovery, notably the time series that revealed secular variation, diurnal variation and the geomagnetic nature of the aurora. In each of these cases, a clear link can be established with changes in measurement protocol that improved the signal-to-noise ratio. But these watersheds are highly atypical. If analysis of geomagnetic theory formation endorses one impression, it is that the most important driver of new conjectures was theory itself; empirical support was initially absent or weak, and subservient at best, often very limited in (spatial and/or temporal) scope, easily extrapolated far beyond experience, and sometimes sought as support after the fact. It is no accident that the transition regarding the direction of inference, from forward to inverse problem, runs parallel to a second transition, from causal postulates involving the inaccessible deep Earth towards predominantly descriptive hypotheses of geomagnetic features witnessed at the surface. The latter would culminate by the 1830s in Carl Friedrich Gauss's mathematical rendition of Earth's magnetism in terms of superposed spherical harmonics (Malin 1987, pp.45-46; Langel 1987, pp.250-259, 285-289).

This is not to say that geomagnetic data were not important in early-modern geomagnetism; quite the opposite is true. When examining the supplanting of axial dipoles with tilted ones, or antipodal poles with disjointed ones, or dipoles with multipoles, each time the overwhelming pressure of discordant empirical data was key. Moreover, a surprisingly large number of critics of particular interpretations did not take up the gauntlet in order to advance their own rivalling scheme, but merely to refute perceived oversimplification. An early-modern geomagnetic theory generally fell, not by virtue of being bested by a more elegant, empirically better founded alternative, but as soon as counter-evidence had acquired sufficient mass to crush its tentative tenets. In other words, empirical data constitute a crucial driver of change, but mainly by exposing the shortcomings of existing formal constructs, far less so as initial building blocks for new hypotheses. This overriding empirical emphasis on falsification represents the fundamental imbalance in the formal-empirical dipole field of geomagnetic proto-science.

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Figures

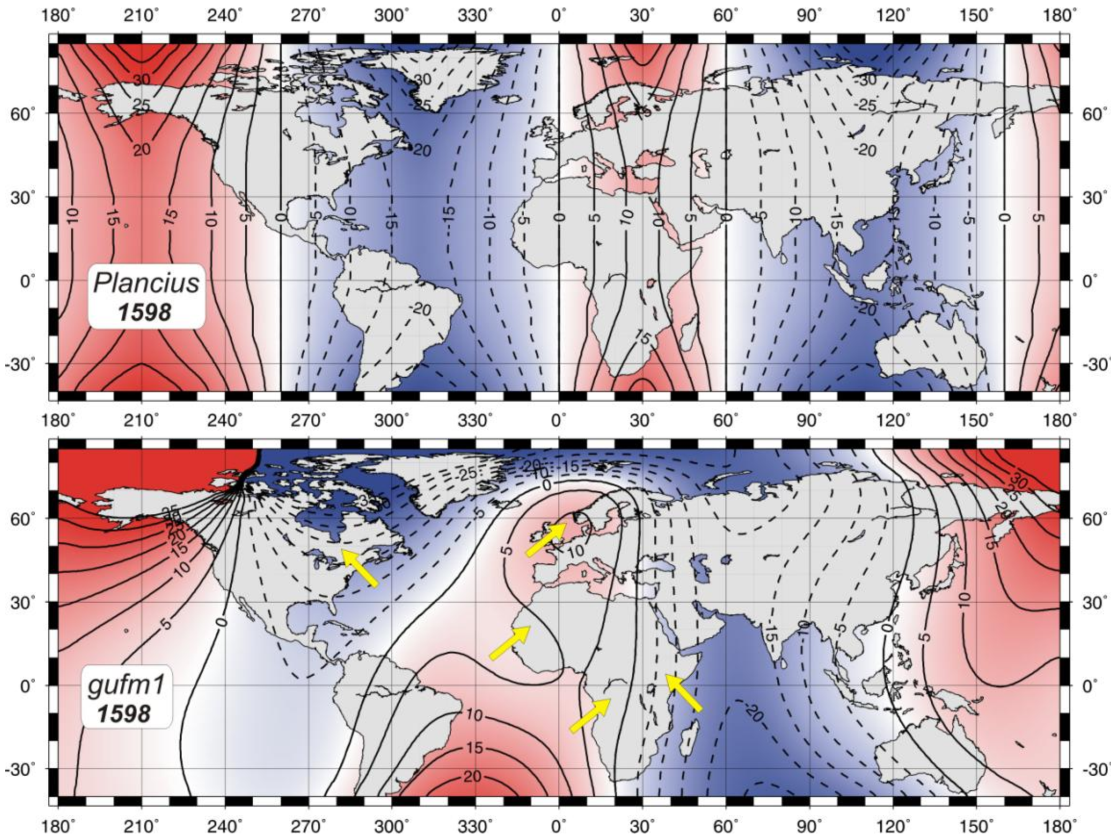

Figure 1. The Plancius hypothesis (top) versus reconstructed global magnetic declination in 1598 (bottom) based on historical field map *gufm1* (Jackson et al. 2000). Isogonics (lines of equal declination) are drawn at 5º interval, solid for northeasting (positive values; online: red), dashed for northwesting (negative; online: blue); absolute declinations above 35º are omitted for clarity; darker areas signify more intense declination. The arrows at bottom indicate regions where reigning declination would have supported William Gilbert's contemporary postulate of continental magnetic attraction. Cylindrical equidistant projection.

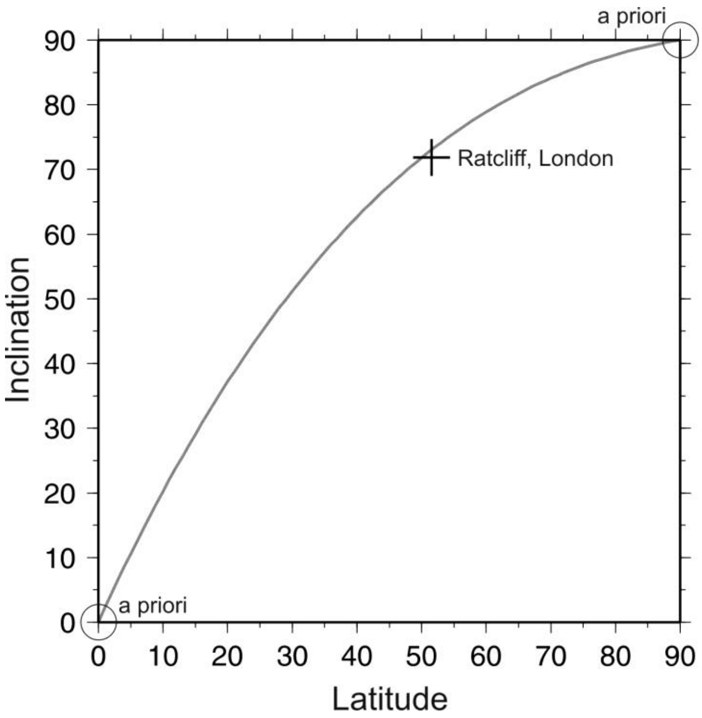

Figure 2. Magnetic inclination versus latitude as tabulated by Henry Briggs in 1598, based on a single observation at London by Robert Norman ca.1580; both end points are *a priori* assumptions. The relationship was deemed to hold regardless of longitude.

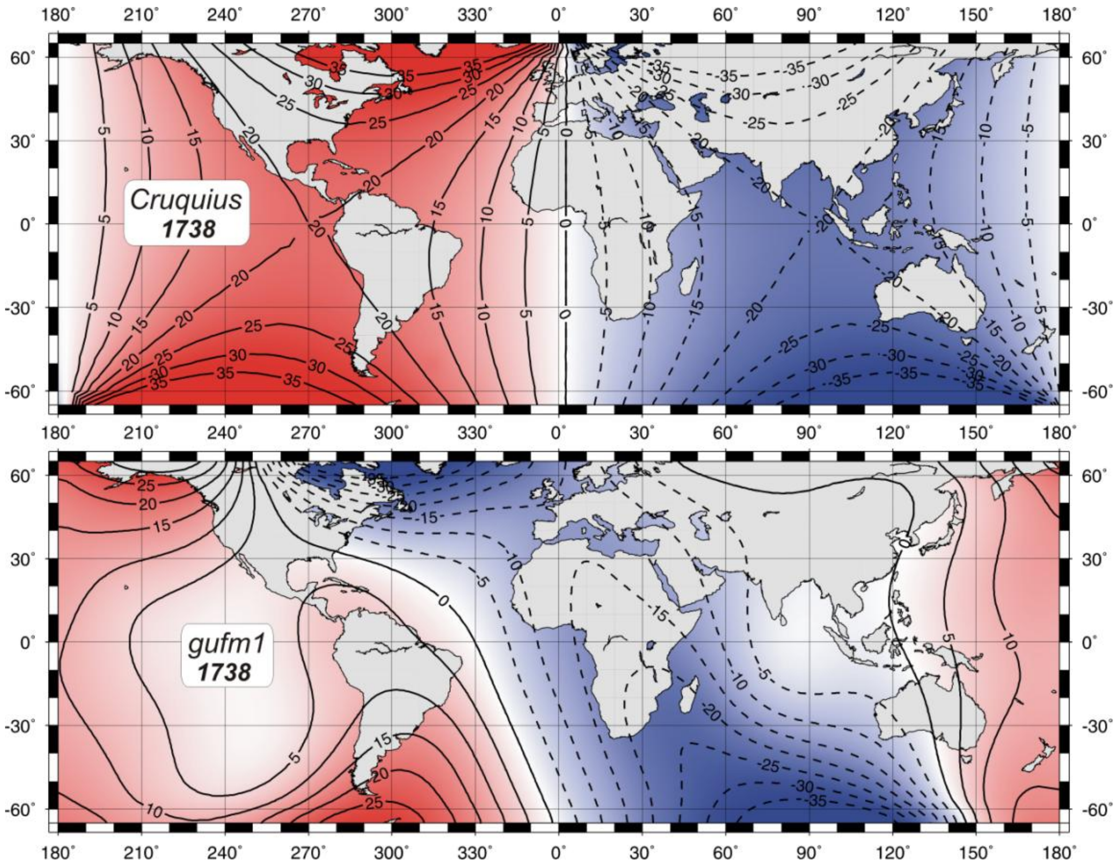

Figure 3. The Cruquius hypothesis (top) versus reconstructed global magnetic declination in 1738 (bottom) based on historical field map *gufm1*. Legend as in Figure 1; cylindrical equidistant projection. Serious discrepancies abound.

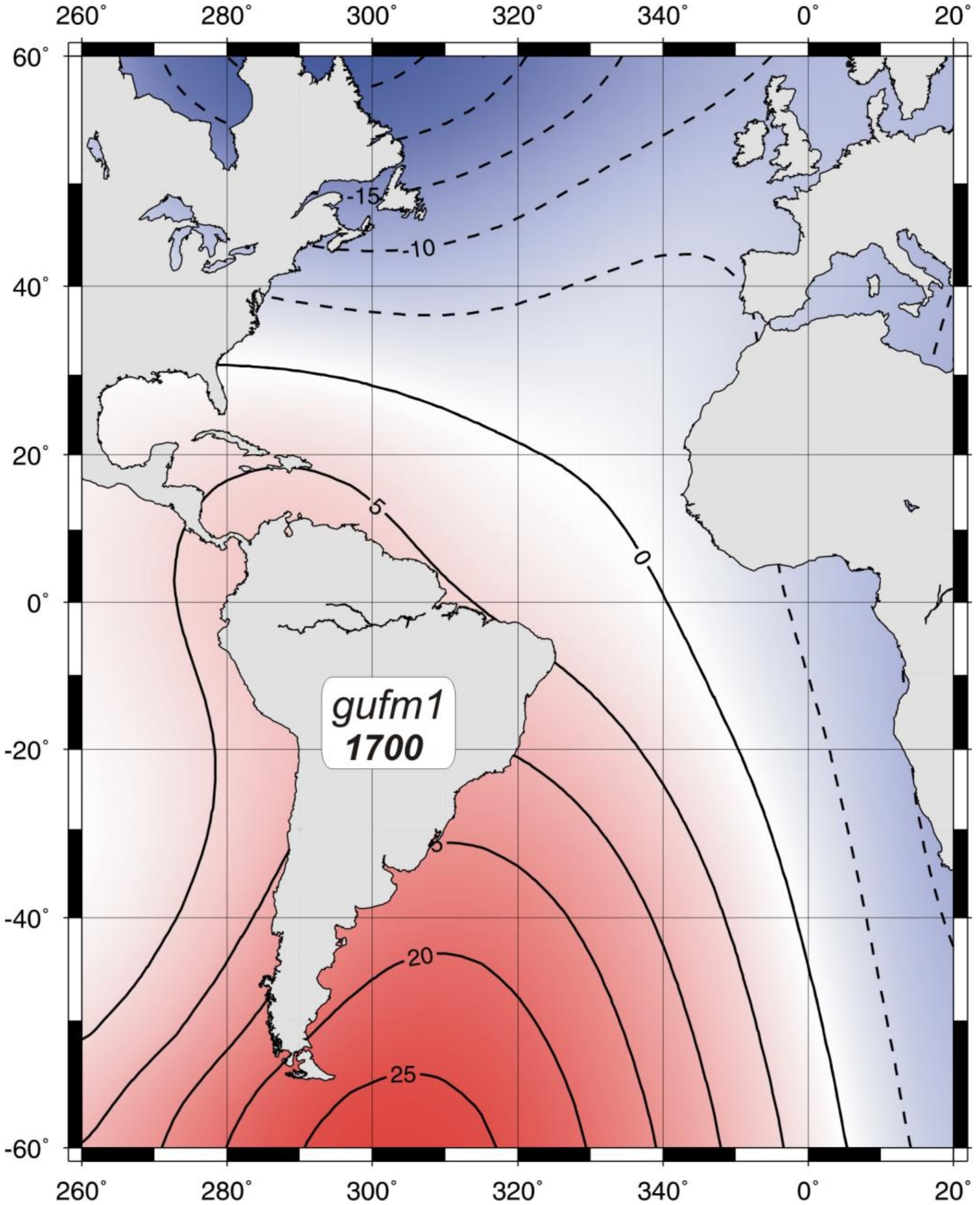

Figure 4. Reconstructed Atlantic magnetic declination in 1700, based on historical field map *gufm1*. Legend as in Figure 1; Mercator projection. Comparison with Halley's isogonic chart for the same year yields no significant differences (within the error bounds of the spatial distribution of his sample), supporting a methodology based purely on the reduction of empirical data.

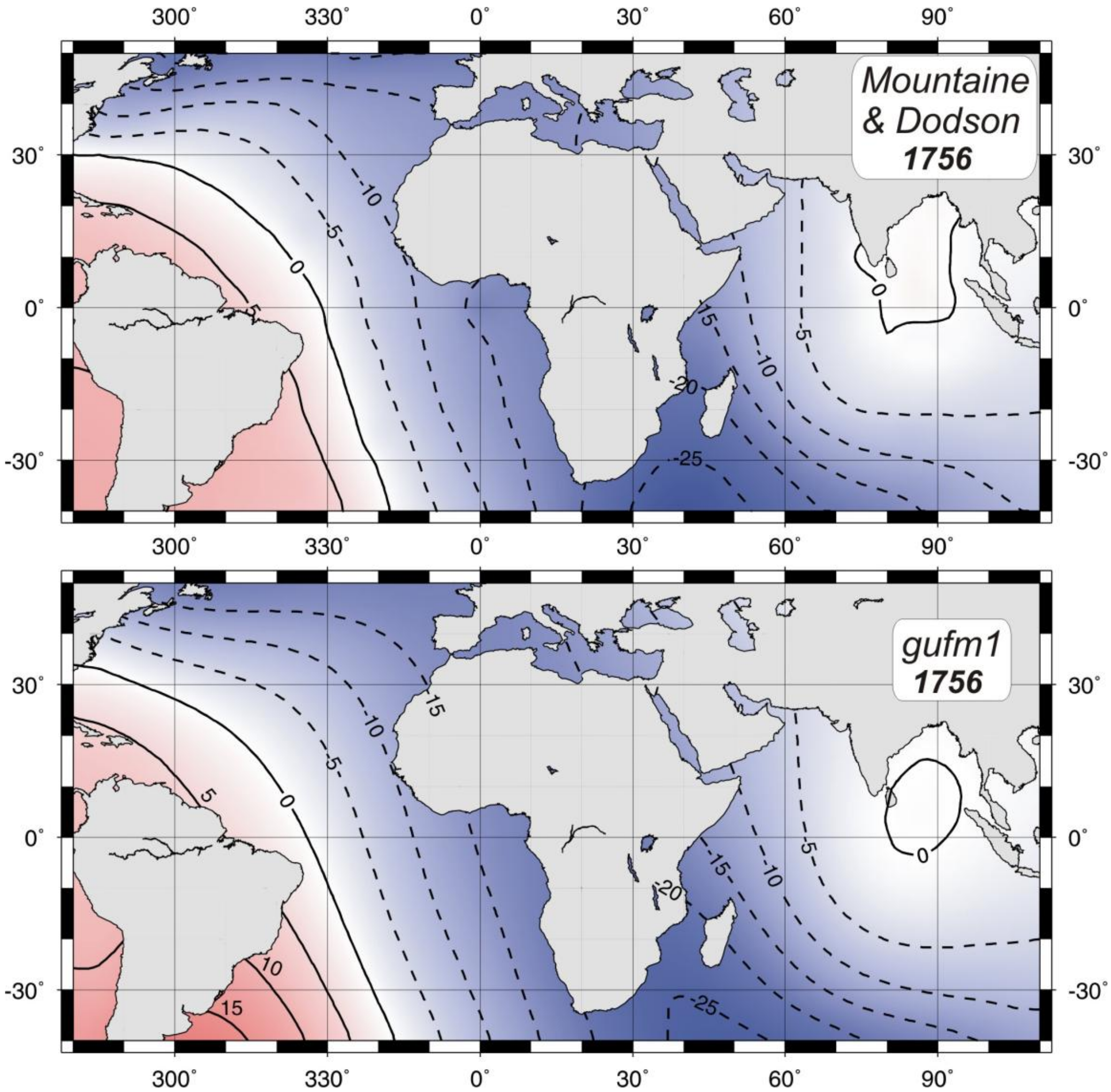

Figure 5. Isogonic rendition (top) of the tabulated gridded declination in 1756, as reduced and published by Mountaine & Dodson (1757), versus contemporary reconstructed magnetic declination in Atlantic and Indian Ocean (bottom) based on historical field map *gufm1*. Legend as in Figure 1; cylindrical equidistant projection. As in Figure 4, no significant differences are apparent, suggesting an empirical approach.

Notes

[1] Gubbins 2001; Glatzmaier et al. 1999; Sakuraba and Kono 1999; Dormy et al. 2000; Jones et al. 1995; Hollerbach 2003; Glatzmaier 2002

[2] Lisboa 1514; Nordenskiold 1897; Morais e Sousa 1924; Boxer 1934; Albuquerque 1970; Warnsinck 1939; Godinho 1993; Pos 1998

[3] Smet 1962, pp.131-137; Hellmann 1898, pp.16-17, 67; Thompson 1903, p.5; Meskens 1994, pp.45-47; Balmer 1956, pp.124-127, 534-542; Crone et al. 1961, pp.396-399

[4] Rouffaer and IJzerman 1929, pp.lxxiv, 433-436; Naber 1917, pp.xxii-xxx, 51; Davids 1986, pp.70-77, 285-287, 313, 355; Davids 1990, pp.275, 282-284, 287; Warnsinck 1939, pp.lxiii-lxviii; Keuning 1940, pp.233-243; Keuning 1946, pp.127-135; Crone 1966

[5] Nautonier 1603, bk.1, pp.46-48, 68; Tarde 1621, p.18; Figueiredo 1625, transl. Le Bon, 1640, addendum pp.25-32; Bib.Nat.Paris (BNP), mss Fr. 19112 *Traicté de la geodrographie*, f.53v; le Vasseur had likely copied this design from the 1630 *Hydrographie* by his fellow Dieppois Jean Guérard; Swedenborg 1734, pp.312-313; compare 'magnetician' Gowin Knight's south polar placement on the Tropic of Capricorn (Knight 1748)

[6] Plancius's mss description of his geomagnetic system, *Van de graden der lancte* (1598) is reproduced in Rouffaer and IJzerman 1929, pp.411-421, with the tabulated declinations on pp.417-420; Crone et al. 1961, pp.400-409; Stevin 1608, pp.164-174; Dijksterhuis 1943, pp.88, 184-185; Struik 1981, p.40

[7] Sarrabat 1727, p.12; Whiston 1721, p.75; Halley 1692, pp.575-576; Leibniz 1749, p.2; Marcorini 1988, vol.1, pp.174, 241, 260; Brit.Mus.London (BML) mss 4433 *Birch papers*, f.64r

[8] Dear 1995, pp.65-66, 158-160; Hall 1983, p.257; Heilbron 1979, pp.171-173; Crombie 1994, pp.633-634; Benjamin 1895, pp.272-277, 291-292; Roller 1959, pp.94, 128-137, 153; Balmer 1956, p.155; Daujat 1945, pp.125-135; Gilbert 1600, transl. Fleury Mottelay 1958, pp.233, 243; Thompson 1903, p.9; Waters 1958, p.246; the idea was also endorsed by Jesuit natural philosopher Niccolo Cabeo (1629, p.218)

[9] Roller 1959, pp.48, 146; Benjamin 1895, pp.170-178; White 1962, pp.131, 176-177; Radelet de Grave 1981, pp.15-16, 21-22; Balmer 1956, pp.62-63; Crombie 1952, p.89; Daujat 1945, p.86; Hellmann 1898, pp.9-10; Still 1946, p.33; Radelet de Grave and Speiser 1975, pp.196, 198; Smith 1992 has shown that many of Peregrinus's ideas had already been voiced during the preceding century

[10] Baldwin 1985, pp.165, 170; ARA (Dutch State Archives, The Hague) 1.04.02/1133 f.84r-v; Cohen 1985, p.134; Roller 1959, p.48; Daujat 1945, p.164

[11] BML mss 4433 *Birch papers* f.78, 200v; Hire 1687, pp.344-351; Birch 1757, vol.4, pp.541 (8 Jun 1687), 543 (22 Jun 1687); Marguet 1917, p.59; BNP mss Fr. 22230 *Bignon papers*, pt.4, no.29, f.332-348

[12] Halley 1692, p.569. Compare this spatial argument to the temporal equivalent as stated around the same time in *Phil.Trans.* by De la Hire: 'But seeing our oldest observations were made but about a hundred years since, and in some particular places only, they only serve to let us know, that if there be a regular motion it must needs be very slow. So that we can conclude nothing certain for the time to come from all that has been hitherto observed;' Hire 1687, p.344

[13] BML Add.mss 4393/4 *Pell correspondence*, f.8-14, 26-91; Birch vol.1, pp.104, 309, 440, 442, vol.2, pp.54-55, 432, vol.3, p.336; Greengrass and Leslie 1995, 8/49/1a, 28/1/8b, 71/16/1a; Anonymous 1673; Bond 1676; Taylor 1954, pp.90-112; Beamont 1971, pp.245-276; Pumfrey 1987

[14] Still 1946, pp.115-126, 167-168; Radelet de Grave 1981, pp.32-33; Ruestow 1973, pp.34-60; Heilbron 1979, pp.35-40; Berkel 1981, pp.121-126; Daujat 1945, pp.251, 313-320, 335-337, 349-395; Fabri's

Physique (Lyon, 1670) devoted treatise 7, bk.1, par.30-263 to magnetism

[15] Kircher 1654, p.346; Cavallo 1800, p.258; Mountaine 1766, p.217, speaking of secular variation in Portugal and Gibraltar (eleven years after a massive earthquake had destroyed part of Lisbon): 'I know not how to account for this considerable encrease, unless those late extraordinary convulsions in the bowels of the earth, upon those several coasts, may be found, by further experiments, to have there influenced the directions of the magnetic needle;' see also Briggs 1967, pp.493-494; Clerc 1788, pp.70, 79-82, 88-92; like Fabri, Buffon deemed deposits of iron ore to affect the global field only once exposed to the air, when they could partake in atmospheric circulation; their general pattern formed two streams, from the equator to each pole; nevertheless, he rejected Cartesian corpuscular mechanisms and vortices

[16] Parallel-needle compasses are listed in ARA 1.04.02/5017 (1655), 1.11.01.01/1789 (1662), ARA 1.04.02/5018 (1675); a logbook example in 1.04.02/5103 *Sion* (6 Oct 1699), expressing surprise at a difference between parallel-needle and traditional compass of 1º15', another indication of expected accuracy on board Eastindiamen

[17] A similar trial was performed by French hydrographer Du Chatelard in Cadiz while with a naval squadron in 1734. The device to be tested was Le Maire's supposedly declination-free compass, compared against the Navy standard and a recent alternative. Du Chatelard performed 12 tests on 8 days, of which 7 at different times of the day. The outcome was, of course, negative; BNP mss Fr. 22230 *Bignon papers* pt.4, no.29, f.332-348

[18] Based on Fournier 1676, pp.413-419 and Henry Phillippes in BML *Sloane* mss 3964, f.10r, Borough's original measurements at Limehouse on 16 Oct 1580 were: 11º17', 11º11'30", 11º30', 11º22'30", 11º15', 11º20', 11º17', 11º14'; Gunter's observations at Limehouse on 13 June 1622 comprised: 6º10', 6º06', 5º54', 5º55', 5º40', 5º40', 6º13', 5º47'; Gellibrand c.s. at Deptford on 12 Jun 1634 obtained: 4º06', 4º10', 4º01', 4º03', 3º55' (a.m.), and 4º07', 4º10', 4º12', 4º04', 4º00', 4º05' (p.m.); Gellibrand's geographical control measurements at St Paul's Cray, Kent on 4 Jul 1634 were: 4º00', 3º55', 3º56', 3º55', 3º58', 3º58', 4º00', 3º58', 4º02', 4º00', 3º59', 3º59', 4º02'; Gellibrand 1635; Pumfrey 1989; McConnell 1980, p.5; Cotter 1981, p.367; Taylor 1954, pp.38, 62,-63, 72-73; Crombie 1994, p.636

[19] Bodleian, *Rigaud* mss 37, f.133 (21 Nov 1728); Musschenbroek 1729, pp.151-234; Malin 1987, pp.20-23, 45; McConnell 1980, pp.5-6; Balmer 1956, pp.215, 224; Still 1946, pp.120, 126; Goodman and Russell 1991, p.326; Canton 1759, pp.399-403; Canton also recorded the temperature in Fahrenheit; compare Canton's solar connection to geomagnetism with that postulated by Gowin Knight (1748) and Jean-Jacques d'Ortous de Mairan (Briggs 1967, pp.494-496); for the first known conjugate sighting of aurora borealis and australis (on 16 Sep 1770, by James Cook and Chinese observers), see Willis et al. 1996, pp.733-742; for 19th-century connections between geomagnetism, meteorology, and other "telluric" phenomena, consult Good 1988 and 1998

[20] Dudley, 1661, vol.2; Waters 1958, pp.159-161, 528-529; Gernez 1947, p.11; Crone et al. 1961, p.411; the third edition of Wright's *Certain errors* (1657) similarly featured a small Mercator's chart with plotted values of declination; Clerc 1788, vol.5 contains 362 pages of tabulated data, followed by the seven charts

[21] Ronan 1970, pp.161-180; Park (1802) vol.3, pp.168-73, Rennel's plane isogonic chart is opposite p.170; Nat.Ship.Mus.Amsterdam, K 38 S 183 RG 004 L.I. 02 (a/b): Nicolaas van Ewyk, printed world map in double polar projection, Amsterdam, 1752; small print at the bottom stated: 'Merkt aan, de Linien getrokken over de landen en by de polen, zyn door de reden daaraan gevoegt, om een beter denkbeeld aan 't werk te geeven; ze zyn door de ondervinding niet geplaatst zoals diegeene, dewelke in de bevarene zeeën gesteld zijn.' (Notice that the lines drawn over the countries and near the poles are added through reasoning, so as to yield a better impression; they are not placed based on experience, as those within the sailed seas.')

[22] Halley 1683; Halley 1692, p.564; Roy.Soc.London *Classif.Pap.* IX(2) 'Magneticks' no.29: John Eames F.R.S. *Of magnets having several poles* (20 Jul 1664): 'Mr Ball produced several loadstones & among them two terrella's, whereof one seem'd to have 4 poles, with a circle passing between them of no virtue at all. Some of the company suggested that it was probable, this stone consisted of 2 stones by nature cemented together by a piece, that had no magnetical quality in it.'

[23] Halley 1716, pp.421-427; Halley reiterated this opinion ten years later, see Armitage 1966, p.182, quoting Roy.Soc Journ.Bk (10 Nov 1726). Compare the 1716 hypothesis by Jean-Dominique Maraldi, ascribing aurorae to exhalations of sulfureous fumes that supposedly caused dry and calm weather; later reports in England and France contained many references to earthquakes, associated with the same fumes within the Earth; Briggs 1967, p.493

[24] Dounot 1611, pp.1v-3, 11-12, 26, 36 ; Turner 1996, p.118; Grandamy 1645, p.80; Fournier 1676, pp.472-473; Waller 1705, p.481

[25] Hellmann 1898, p.87; Wright 1957, p.91; Gilbert 1600, transl. Fleury Mottelay 1958, pp.253-254; Muller 1909, p.16; Davids 1990, pp.285, 288; Stapel 1976, bk.1, pt.2, p.681; Davids 1986, pp.77-80; Halley 1683, pp.214-215; ANP MAR 3JJ 7 no.17 *Copie de l'extrait des registres de l'Academie Royale des Sciences* (24 Nov 1731); Riccioli 1672, p.351; Fournier 1676, p.474; Millet Dechales 1677, p.100; Slikker 1703, pp.49, 53; Valois 1735, p.161; Struick 1768, p.307; Erzey 1777, p.241; Lorimer 1794, p.326; Cavallo 1800, p.100 (quotation)

[26] Benjamin 1895, pp.96-101, 149-151, 156, 203-204; Daujat 1945, pp.88, 111; Still 1946, pp.57-58; Roller 1959, pp.30, 37; Balmer 1956, pp.114-115, 127, 527-537, 544; Smith 1992, pp.52-55, 60; Hackmann 1994, p.174