From ‘magnetic fever’ to ‘magnetical insanity’: historical geographies of British terrestrial magnetic research, 1833-1857

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Contents

Abstract

This thesis explores British-led efforts to observe and map the earth's magnetic field between 1833 and 1857. In doing so, the thesis examines how magnetic instruments, magnetic observers and magnetic instructions were mobilised in and across multiple geographies, from the Canadian Arctic, to the island of St Helena, to Van Diemen's Land in the southern hemisphere and at many sites in between. Interest in terrestrial magnetic research burgeoned and was crystallised during the early nineteenth century in Britain and abroad and resulted in the creation of systems of physical observatories and the organisation of magnetic surveys. This work addresses what it meant to coordinate such a network by scrutinising what is popularly known as "the magnetic crusade", but which was more commonly referred to at the time as the British magnetic scheme. There were several individuals involved in the formation of this scheme but this thesis focuses on two in particular: Edward Sabine and Humphrey Lloyd. In the correspondence of these two figures, we can follow the process by which terrestrial magnetic research was disciplined, its participants educated, its observational data organised and its instruments developed, deployed and used at different stations across the globe. This work seeks to extend and at times complicate our understanding of what it meant to coordinate a big Victorian scientific pursuit and explores among other things the management of instruments in different geographic contexts; the experience of scientific servicemen in the observatory and during surveying efforts; the space in which magnetic data were handled and the processes employed in reducing these data. In all, this thesis aims to recover the several different practices of place that attended the organisation of what was considered in the first half of the nineteenth century to be the greatest scientific endeavour yet pursued.

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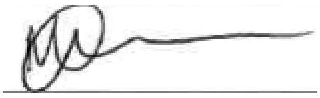
Author’s Declaration

I hereby declare that, except where explicit reference is made to the contribution of others, this dissertation is the result of my own work and has not been submitted for any other degree at the University of Glasgow or any other institution.

Signature: 

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Abbreviations and Conventions

Abbreviations

BGS	British Geological Survey
BL	The British Library
BMS	British Magnetic Survey
NAMMS	North American Magnetic and Meteorological Survey
RS	Royal Society HS – Herschel Papers MM – Miscellaneous Manuscripts MS – Manuscripts General
TNA	The National Archives BJ 3 – Papers of Edward Sabine and the magnetic department

Conventions

Single quotation marks are used except within a quotation, or where the original uses double quotation marks.

Quotations are cited with the original formatting. Italics in quotations are as in the original unless stated otherwise.

To distinguish the two volumes of terrestrial magnetic correspondence held at the Royal Society, Roman numerals are included in the reference. For example, the first volume is referenced as RS MS/119/I and the second as RS MS/119/II.

Chapter 1: Introduction

The magnetic crusade: system of observation

The magnetic crusade or, as it was more commonly referred to at the time, the British magnetic scheme, was a system designed to observe and chart the earth's magnetic field at spots all over the globe, from surveys on sea and on land in high northern and high southern latitudes to fixed stations in Africa, Asia, Europe, the Americas and Australasia. In what follows, I chart the burgeoning fervour for terrestrial magnetic science that accompanied the turn of the nineteenth century and then, in the main body of the work, I explore how this eponymous "magnetic fever" turned into a "magnetical insanity" that consumed its participants and created a deluge of observational data. There was a litany of motivations for the enactment of this scheme. Some were practical, such as the contribution to better navigation that more accurate magnetic charts would provide; and others were more theoretical, such as the search for a law that could explain and predict what was considered one of the last great mysteries of physics: the variation of the earth's magnetic field. Such an investigation had been prompted by figures such as Alexander von Humboldt and Francois Arago, two natural philosophers who understood geomagnetism as one of 'a number of…earth forces which were responsible for the phenomena manifest in or on the earth'.[1] This was known as the cosmical tradition and one of its adherents was the de facto head of the British magnetic scheme, Edward Sabine.

The British scheme took place between 1839 and 1857 and represented the calibration and coordination of masses of instruments and observers to record simultaneous geomagnetic observations on a schedule set to Göttingen mean time but devised in England and Ireland. The scheme was coordinated by Sabine from his "magnetic department" at Woolwich Arsenal, and by Humphrey Lloyd from his Dublin Observatory, built in the grounds of Trinity College. Perhaps the best-known component of the scheme was James Clark Ross's scientific expedition to the Antarctic. This survey was designed to collect geomagnetic data from a region that had remained largely devoid of magnetic observation until the 1830s. The expedition's launch in September 1839 is generally regarded as the beginning of the British magnetic scheme.

[1] Cawood, J., 'The Magnetic Crusade: science and politics in early Victorian Britain', *Isis* 70, 4 (1979), 492-518, 497.

The Antarctic expedition consisted of two vessels kitted out for Antarctic seafaring: HMS *Erebus* and HMS *Terror*. Ross and his two crews would not return until 1843. Collecting geomagnetic data in high southern latitudes was the primary purpose of the journey, to be complemented by additional observations of the figure of the earth, tides, meteorology, distribution of temperature in the sea and on land, currents and depths of the ocean, southern astronomy and Arctic geography.[1] During the voyage, important natural history collections would also be made.[2] Ross's was not the only survey on the British scheme. John Henry Lefroy undertook a land-based magnetic survey in North America between 1843 and 1844, an extensive discussion of which is contained in the second chapter of this thesis. A survey in southern latitudes complementary to Ross's expedition was also carried out by Lieutenant Henry Clerk of the Royal Artillery and Captain T. E. L. Moore of the Royal Navy in 1844.[3] Scientifically-minded ship captains, such as Captains Graves and King, had also made observations during voyages across the globe and deposited data with Sabine's magnetic department in Woolwich.[4]

The primary purpose of the above-designed surveys was to observe in areas of high northern and, particularly, high southern latitudes, and in vast swathes of ocean. In short, these surveys were staged in places that fixed observatories could not reach. However, it was not proposed that these surveys operate in isolation. As John Herschel wrote in 1840, 'if ever magnetic surveys of particular districts can be carried on with advantage, it must be when based on and in concert with a series of regular observations made at stations of reference'.[5] Surveys of the crusade thus operated in tandem with the formation of a network of physical observatories in four of Britain's colonial possessions and at dozens of other overseas locations. Ross's aforementioned expedition was a part of the formation of this network, depositing as it did men and materials at three of the four colonial sites. Lefroy and Frederick Eardley-Wilmot, both of the Royal Artillery, were landed at St Helena and the Cape of Good Hope colony respectively, to construct and direct magnetic observatories there. Joseph Henry Kay, of the Royal Navy, was deposited at Hobarton, Van Diemen's Land for a similar purpose. However, unlike Lefroy and Wilmot, Kay and two assistants were deposited with all the materials necessary to construct a magnetic

[1] Anon, *Report of the President and Council of the Royal Society on the Instructions to be Prepared for the Scientific Expedition to the Antarctic Regions* (Richard and John E. Taylor: London, 1839). The order of activities in the text reflects the order given in the *Report*, although this should not necessarily be taken to infer there was a specific hierarchy of those supplementary observations to be recorded.
[2] Ross, J. C., *A voyage of discovery and research in the southern and Antarctic regions during the years 1839-43*, 2 Vols (John Murray: London, 1847).
[3] Sabine, E., *Observations made at the Magnetical and Meteorological Observatory at the Cape of Good Hope, Vol. I, magnetical observations, 1841 to 1846* (London: Longman, Brown, Green and Longmans, 1851), ii.
[4] Lloyd to Sabine, 2 March 1840, TNA BJ 3/10/137.
[5] Herschel, J., 'Terrestrial magnetism', *Quarterly Review* 66, 131 (1840), 271-312, 304.

observatory, these having been assembled in Chatham prior to the ships setting to sea. The Hobarton Observatory was named Rossbank after James Clark Ross. Charles Riddell, of the Royal Artillery, had been sent separately earlier in 1839 to establish a magnetic observatory in Montreal, Canada, although he would eventually have to relocate the proposed site to Toronto, on encountering several issues in Montreal. These four observatories, in St Helena, Toronto, Van Diemen's Land and the Cape of Good Hope colony, were called the colonial observatories. The history and geography of these stations will be discussed in much greater detail in the third chapter of this thesis. Four further stations, operated by the East India Company, were constructed at Simla, Madras, Singapore and Bombay, the formation of which Edney has in part elucidated.[1] A total of 23 other observatories complied with the British magnetic scheme's schedule, from Milan, Prague, Munich and other stations in Europe, to Philadelphia and Cambridge in the USA, and Cairo in Africa.[2] I chose to explore the historical geography of the colonial observatories and Aden in order to address the lack of critical attention that the colonial observatories and Aden have received as constituent parts of the British magnetic scheme and because, as I note later in the literature review, physical observatories in India have been the subject of much recent critical inquiry. My lack of appropriate foreign language skills also prevented me from properly engaging with the archives of many of the British magnetic scheme observatories – those in Germany, France, Italy, Russia and elsewhere on the continent.

In total, 33 observatories made up the British-led magnetic scheme. These stations formed the network of fixed, physical, observatories that initially operated alongside and in tandem with Ross's expedition, but which would function for more than a decade after Ross had returned to England. Periodisation of the scheme is difficult. The launch of Ross's Antarctic expedition is usually taken to be the beginning of the British scheme, but the date at which the crusade ended is debatable. The Rossbank Observatory at Hobarton, Van Diemen's Land, ceased operations in 1854, the Cape of Good Hope and St Helena observatories having also ceased operating on their original brief in 1846 and 1849 respectively, and the administration of the Toronto Observatory having been turned over to the provincial authorities in 1853. 1854 is then a candidate for the end of the British magnetic scheme. However, Sabine's magnetic department functioned until at least 1857, the date given for the third and final publication of results from the Toronto Observatory. Macdonald has even argued that the second British Magnetic Survey, which took place

[1] Edney, M., *Mapping an Empire: the geographical construction of British India, 1765-1843* (The University of Chicago Press: Chicago and London, 1997).
[2] Chapman, S., and Bartels, J., *Geomagnetism*, Vol. I and II (Oxford University Press: Oxford, 1940); a list is also found in Herschel 'Terrestrial magnetism', 299-300.

sporadically between 1856 and 1861, 'can be understood as an extension of Sabine's Magnetic Crusade', again pushing back the date at which the magnetic crusade ceased.[1] For the purposes of this thesis 1857 will be treated as the year in which the scheme ended, as this was the year Sabine's magnetic department closed and because the second British Magnetic Survey was designed as a repeat of the first, pre-crusade, British Magnetic Survey, 1833-1838, and may well have occurred even if Britain's global magnetic research programme had not been realised in the intervening years. Finally, this second British Magnetic Survey was not undertaken simultaneously with observations from elsewhere, a key tenet of the magnetic crusade programme.

The schedule of observations enacted by the British magnetic scheme between 1839 and 1854, when Rossbank closed, was both exhaustive and exhausting or, as the *Report of the Royal Society* termed it, 'persevering and laborious'.[2] Each observatory was supplied with three magnetometers: a declination magnetometer, which measured the direction and variation of the magnetic field; a horizontal force magnetometer and a vertical force magnetometer, which measured the intensity of the force in its horizontal and vertical components respectively. Each observatory was also equipped with a dip circle (also known as an inclinometer), for measurements of the angle at which a magnetised needle inclined towards the earth, as well as a number of other meteorological, astronomical and mathematical instruments, such as thermometers, barometers, actinometers, hygrometers, anemometers, theodolites and chronometers.[3] The observatories were active for 24 hours a day, six days a week – Sunday being excluded on religious grounds. The magnetic instruments were observed every two hours corresponding to even hours on Göttingen mean time (i.e. 2, 4, 6, 8pm and so on), making 12 periods of observation a day. Within these periods of observation, three observations of each of the magnetometers were recorded at the commencement of each hour in order to provide the data for a mean reading. The Rossbank Observatory swapped the two-hourly schedule for hourly observations, their director Kay being quite the magnetic zealot. This practice was adopted in other observatories later in the scheme but stopped altogether and returned to twice-hourly observations in 1848. A triple observation (i.e. three sets of three observations of each magnetometer) was also made daily at 2pm Göttingen mean time, 'to multiply opportunities for observing remarkable coincidences' simultaneously in different parts of

[1] Macdonald, L. T.., '"Solar Spot Mania": the origins and early years of solar research at Kew Observatory, 1852-1860', *J. Hist. of Astron.* 46, 4 (2015), 469-490, 478.
[2] Anon, *Report of the President and Council of the Royal Society*, 36.
[3] McConnell, A., *Geomagnetic instruments before 1900: an illustrated account of their construction and use* (Wynter: London, 1980), 1.

the globe.[1] The entire scheme was always set to Göttingen mean time, and observers like Kay and Lefroy took great pains to make their observation schedule correspond to the second, although of course their ability to achieve this is questionable. Observations of the wet and dry bulb thermometers and the barometer were to be recorded alongside the magnetic measurements, so that the returns of each observatory could be corrected for temperature and atmospheric pressure. Meteorological observation was not just a bi-product of, or auxiliary to, magnetic observations but, as the list of meteorological instruments given above in part demonstrates, a prime function of the colonial observatories.[2]

In addition to the regular daily observations, each observatory also made absolute determinations of the horizontal and declination magnetometers and the dip circle on at least one occasion each month. On selected days, the observatories on the British scheme were required to keep Term Days, an innovation of Gauss's design that stipulated observations of each of the magnetometers to be taken every two-and-a-half minutes for a full 24-hour period.[3] These Term Days were kept each month and linked all of Britain's colonial and overseas observatories with the European observatories for a full 24-hours of simultaneous observation. The declination magnetometer would be observed at 0m, 5m, 10m etc., the horizontal force at 2m30s, 12m30s, 22m30s etc., and the vertical force at 7m30s, 17m30s, 27m30s etc. It was thought that this arrangement would allow the observer 'sufficient time between the observations to transfer his attention from one instrument to the next without embarrassment or confusion'.[4] It may have saved some embarrassment and confusion but it also certainly created great haste at the Rossbank Observatory, especially because parallel meteorological observations were required as frequently as magnetic readings. The barometer here had been placed outside the observatory 'in consequence of Captain Ross's extreme anxiety to exclude from the magnetical observatory everything containing metal, retaining only one [chronometer] which could not possibly be dispensed with. This arrangement', Kay explained to Lloyd, did not abnormally affect the regular observations, as meteorological instruments were registered before the magnetometers but, on Term Days, it required 'the person who observes the VF [vertical force magnetometer] to set the micrometers and then to *run quickly and record the Barometer*. This requires his absence from the magnetical

[1] Herschel, 'Terrestrial magnetism', 302.
[2] See Naylor, S., 'Weather instruments all at sea: meteorology and the Royal Navy in the nineteenth century', in MacDonald, F., and Withers, C. W. J. (eds), *Geography, technology and instruments of exploration* (Ashgate Publishing: Farnham, 2015), 77-96.
[3] Carter, C., 'Magnetic fever: global imperialism and empiricism in the nineteenth century', *Transactions of the American Philosophical Society* 99, 4 (2009), i-169, 116.
[4] Anon, *Report of the President and Council of the Royal Society*, 37.

observatory, about one minute, or one minute and a half.'[1] The Term Day registry system was briefly changed to six-minute cycles by Lloyd in 1841 but, due largely to Herschel's disagreement with this move on grounds that it would de-sync the colonial and overseas observatories with the European stations, the five-minute cycle was quickly reinstated.[2] For this laborious schedule of daily, absolute and Term Day observation, each of the colonial observatories was staffed by just one director and three assistants, at least initially. In 1841, principally on the urging of Riddell at the Toronto Observatory, an additional assistant was sanctioned for each of the colonial observatories. Even with this supplementary assistant, keeping up with the rigorous schedule of observations was a constant struggle for colonial observatory staff.

As a final part of the overview of the magnetic crusade, it is worth noting that the moniker 'magnetic crusade' was a contemporaneous bestowal, but whether it originated in the British Isles or the United States is not completely obvious. Carter found contemporary usage of the term in a North American textbook published in 1842 by John Farrar and Joseph Lovering on electromagnetism, and in Robert Patterson's address to the American Philosophical Society in 1843, but both Carter and, later, Macdonald, had thought it 'unclear if it [the magnetic crusade] was ever used by the major figures lobbying for the project'.[3] However, Ratcliff recently reported that Lloyd, one of the most important arbiters of the scheme, had used the term in the title of an article in the April 1842 edition of the *Cambridge Miscellany of Mathematics, Physics and Astronomy*.[4] This book, edited by Benjamin Peirce, Professor of Mathematics and Natural Philosophy at Harvard University, was, it proclaimed, 'extensively used in colleges and academies throughout the U. States' and was perhaps the source of Farrar and Lovering's discovery and use of the term. However, it is quite possible that the title of the article was an editorial decision, and not of Lloyd's concoction. The text given in the *Cambridge Miscellany* is almost exactly the same as that which appears in the introduction to Lloyd's *Account of the Magnetical Observatory of Dublin*, an introduction that does not make any reference to the British magnetic scheme as a "magnetic crusade". Nor, from my own research of large parts of Lloyd's correspondence in the 1840s, have I found Lloyd to make any use of the term "magnetic crusade". Perhaps, privately, Lloyd did consider it in these terms and, needing a new title for his article to send to Peirce, had settled on magnetic crusade. Perhaps the

[1] Joseph Henry Kay to Humphrey Lloyd, 16 January 1842, RS MS/119/II/103. Emphasis added.
[2] Carter has more to say on this in 'Magnetic fever', 116-118.
[3] Carter, 'Magnetic fever', xv-xvi; Macdonald, 'Making Kew Observatory', 410.
[4] Lloyd, H., 'History of the present magnetic crusade', in Peirce, B. (ed.), *Cambridge Miscellany of Mathematics, Physics and Astronomy* (James Munroe and Company: Boston, April 1842), 1; Ratcliff, J., 'Travancore's magnetic crusade: geomagnetism and the geography of scientific production in a princely state', *Brit. J. Hist. Sci.* 49, 3 (2016), 325-352, 326.

British magnetic scheme, given its reach and its zeal, was known colloquially by geophysical researchers in the United States as a crusade. Perhaps Lovering, the Professor of Mathematics and Natural Philosophy at Harvard before Peirce and in 1842 Professor Emeritus, had made the remark to Peirce and Peirce had run with it. The matter remains unclear.

Magnetic fever, magnetical insanity

The overview of the magnetic scheme presented above provides a sense of the scale of the project and an indication of the laborious schedule of observation required by the system, as well as some of the components – people, instruments, observatories and surveys – of which it was formed. Having established this, it is possible to provide an explanation of the title of this thesis.

Musselman has written that the observatory sciences 'required the most extreme levels of timeliness, reliability, and sobriety – the kind of moral and physical purity that only missionaries and astronomers were just crazy enough to demand.'[1] The observers on the British magnetic scheme were similarly disciplined or, perhaps, 'crazy'. It was Lefroy, who managed both the St Helena and latterly the Toronto Magnetic Observatory, who coined the term 'magnetical insanity' in a letter in 1844. 'Magnetical insanity' was the condition that Lefroy diagnosed Kay – Director of the Rossbank Observatory – as suffering from, due to the extraordinary number of hours Kay had laboured in his cramped observatory carefully marking the deviations of his instruments.[2] Kay had said as much in his correspondence with Lefroy. This species of magnetical insanity that Lefroy identified was not only limited to Kay at Rossbank. Wilmot, writing (again to Lefroy) in 1845 from his Cape Magnetic Observatory, sombrely recounted how he had 'gone through a sort of mental suffering during the last four years that I should dread to encounter again. At times I felt as if I should go mad'.[3] This madness speaks to the experience of scientific servicemen in attempting to manage their observatories and cope with not only the

[1] Musselman, E. G., 'Worlds displaced: projecting the celestial environment from the Cape Colony', *Environmental History* (2003), 64–85, 77. Thanks to Simon Naylor for alerting me to this work and this quotation.

[2] John Henry Lefroy to Charles J. B. Riddell, 10 December 1844, in Stanley, G. F. G. (ed.), *John Henry Lefroy: in search of the Magnetic North. A soldier-surveyor's letters from the North-West, 1843-1844* (Toronto: Pioneer Books, the MacMillan Company of Canada Limited, 1955), 144.

[3] Wilmot to Lefroy, 10 June 1845, quoted in F. A. Eardley-Wilmot (ed.), *Memorials of Fredk. M. Eardley-Wilmot* (William Clowes and Sons: London, 1879), 45.

punishing schedule of observation but also the plethora of problems caused by the different locales in which their observatories were placed.

I also believe that the term 'magnetical insanity' can be used to describe the British magnetic scheme more broadly. It was an extraordinarily ambitious project, which attempted to coordinate personnel, instruments, materials and data on a truly global scale. The magnetic scheme also seems to represent a kind of insanity because its proponents believed so firmly in the simultaneous use of the same instruments at points distributed over the earth's surface despite, as I show elsewhere, the plethora of ways in which magnetic instruments were continually and differently altered as they were mobilised on the British scheme. If this was a kind of madness, it was contagious and it inspired a zealous and – I choose my words purposefully here – crusading spirit. Sabine considered the work of himself, Lloyd and others in launching their worldwide scheme as 'laying the foundation' for a new '"era of physical research"'.[1] Herschel similarly wrote in 1840 that terrestrial physics ought to supplant astronomy as the primary science of investigation – astronomy having already reached the commanding heights from which its 'theory stands august and stately' – and positioned explorations of geomagnetism as 'the duty of every civilized nation to set on foot'.[2] The inference of such language was that Britain was at the forefront of ushering in this new, global, physical research effort that promoted and extended what Herschel saw as being the epitome of the action of a civilised nation. The officer-observers at the colonial observatories – Kay, Wilmot, Riddell and Lefroy – were the vanguard of this crusading mentality, and their zeal to carry out this scheme could sometimes usurp their high religious principles. For instance, at Rossbank, Kay proclaimed that, having already missed some observations of the irregular disturbance of the field because the disturbance had continued beyond midnight on a Saturday, should the instruments begin to exhibit irregular movements again 'Sunday or no other day shall prevent me from following it up, even if the bell rings for church'.[3]

As Carter has explained, it was a magnetic fever that gripped Britain's scientific community in the 1830s. As I explain in what follows, this fever grew into a magnetical insanity that fuelled a massive, near-incessant and sometimes physically detrimental scheme of observation. The title is therefore also designed to distinguish this thesis from Carter's earlier consideration of the magnetic crusade. Broadly, much of Carter's focus is on those who politicked for the magnetic scheme – particularly Herschel – as part of the magnetic lobby of the 1830s. Less consideration is given to those who experienced the

[1] Sabine to Lloyd, 28 December 1839, RS MS 119/II/82.
[2] Herschel, 'Terrestrial magnetism', 273, 304.
[3] Memorandum by Kay, nd, RS MS 119/II/106.

British scheme as it was played out in the observatories, the surveys and at the magnetic department. These experiences form a large part of this thesis.

Thesis structure

This thesis is split into two halves. The first half considers two geomagnetic surveys: the British Magnetic Survey, 1833-1838, and the later North American Magnetic and Meteorological Survey, 1843-1844. The second half considers the establishment of the colonial magnetic observatories and, in the final chapter, the practices that attended the management, reduction and distribution of observational data accrued from both the above surveys and observatories alike. Both sections concern the mobilities (and sometimes immobilities) involved in the process of the magnetic crusade. These mobilities largely involve different materials: instruments, building materials and data. Surveying practices in the nineteenth century and the mobility of knowledge have been subject to increased scrutiny recently in the history of science and each half of this thesis is designed as a contribution to these discussions. Such a structure also helps to cohere the thesis around significant aspects of British geomagnetic research that have heretofore not been considered in detail.

In the first chapter, I explore the British Magnetic Survey, which took place between 1833 and 1838 and which involved many of the same people who lobbied for and organised the subsequent British magnetic scheme. For several decades now, many histories of science have sought to emphasise the important role of instruments and other material objects in the operation of science. Many too have been attentive to ideas of space and place and the different geographies that are visible in the historical practice of science. This chapter draws on both traditions in its interpretation of a heretofore neglected aspect of Britain's nineteenth-century geomagnetic story: that of the British Magnetic Survey. Far from being a footnote to the more expansive geomagnetic projects then taking place on the continent or the later British worldwide magnetic scheme, this chapter argues that the British Magnetic Survey represents an important instance in which magnetic instruments, their users and their makers, were tested, developed and ultimately proved credible. This chapter also introduces the topic of the relationship between instruments and the data they help to produce. This is a topic that also ties in with the thesis's overall concern with the life-geographies of magnetic instruments in the early nineteenth century. Commonly, explorations of an instrument's deployment and use end with the production of an inscription or inscriptions. The two – instrument and data – are presented as both fundamental to each other and entirely distinct. However, as I show here in the British Magnetic Survey chapter and again in the chapter concerned with the Woolwich magnetic department, instruments and data continued to co-exist beyond the inscriptions they produced in the field. This continued interplay was crucial to the interrogation of results and the construction of publishable data.

The second chapter of the first half of the thesis continues with the theme of surveying, in the form of John Henry Lefroy's North American Magnetic and Meteorological Survey, 1843-1844. This chapter considers how Lefroy and his instruments moved within a network of Hudson's Bay Company forts and outposts, guided by a mixture of French-Canadian voyageurs and Indigenous peoples. This chapter complements and extends historical accounts of Lefroy's survey and examines how, and how well, Lefroy's instruments moved during this far-reaching geomagnetic survey. The recent material turn in the history and historical geography of science provides the framework for a closer reading of the spatial biographies of several of Lefroy's magnetic, meteorological and astronomical instruments. Focusing on the instruments in varying states of repair not only recaptures the materiality of these instruments but also, in looking at the solutions applied to make such objects workable again, adds to a nascent understanding of repair and maintenance in the history of survey science. Focusing on the instruments as objects to be carried and managed also helps to illuminate the oft overlooked role of indigenous and French-Canadian voyageurs in such scientific expeditions.

The third chapter is an exploration of a different but no less fluid aspect of the British magnetic scheme: the establishment and management of the colonial observatories at St Helena, the Cape of Good Hope, Toronto and Van Diemen's Land. The purpose of this chapter is to critically examine what it meant to establish a geophysical observatory in the different geographies of the colonial sites. The experience of scientific servicemen forms a significant part of this discussion, as such experiences help to illuminate the means by which observatories were situated and later managed. This chapter also contributes to discussions of scientific instruments in transit and the (im)mobilisation of knowledge attached to these material objects.

In the final chapter of the thesis, I consider the transmission of magnetic observations from the overseas, colonial, observatories and the removal of these data from manuscript to become the printed results of the magnetic crusade, between 1841 and 1857. The processes adopted by Edward Sabine's "magnetic department" at Woolwich Arsenal to cope with the accumulation of very literal masses of data are considered, as well as the politicking that attended Sabine’s attempts to have this department installed within the space occupied by, and the bureaucracy of, the Board of Ordnance. The magnetic crusade was one of the largest data collecting enterprises of the nineteenth century and a history of its data management processes provides an important contribution to recent attempts to historicise discussions about ‘big data’ and perceptions of information overload. This chapter is crucial because Woolwich occupies only a scant position within the historiography of the magnetic crusade. In the wider historiography of nineteenth-century science, data handling spaces such as Woolwich have also not been the subject of extensive historical scrutiny. I explore in the literature review that follows how data histories and questions about historical data management have become more prevalent in recent years, but this has not yet been extended to the spaces of physical science in the early Victorian era. It is one of the purposes of this thesis to undertake such an exploration.

Collectively, these chapters contribute to several different areas in the history of nineteenth-century science. One of the most significant contributions of the thesis is to our understanding of the experience of scientific servicemen in the operation of a big Victorian scientific project like the British magnetic scheme. These scientific servicemen are an understudied part of the British scientific community in the nineteenth century. While some, like Sabine, have received some historical attention, those who participated on the ground in the transmission of instruments, the establishment of observatories, the observation of instruments and the collation and reduction of data, have received relatively little. A related purpose of this thesis is to better illuminate the contribution of the military to the construction and management of a global system of observation. As Ratcliff has argued, the military as an institution and instigator of science in the nineteenth century has received less attention than it ought to have, given its considerable importance across several big research projects in the nineteenth century, like the magnetic crusade and the transit of Venus enterprises. This thesis will therefore address and resolve this historiographical neglect. In addressing this oversight, this thesis will contribute to understandings of what we mean when we talk about “big science”. Those responsible for creating the architecture of the magnetic crusade were conscious of the enormous magnitude of the observation scheme they built. But, as is explored throughout the thesis and in the conclusion, the “bigness” of the science of the crusade was really an artifice created through the conglomeration and assemblage of a variety of practices that were unevenly employed at different times and different sites throughout the duration of the project, largely dictated by the volatility inherent in the mobilisation of different materials, instruments, people and data across the globe. As I demonstrate in the conclusion, the term "big science" ought not to be seen as a categorisation of projects such as the magnetic crusade, but as a provocation to explore the dynamic processes involved in creating and managing the perception of bigness.

These chapters also represent an exploration of some of the life-geographies of scientific instruments in the early nineteenth century: the spaces in which they were examined and developed and mobilised in the construction of geomagnetic science. Charting the life-geography of a scientific instrument – in this case mostly magnetic instruments – illuminates fieldwork practices in the observatory and at survey sites and can highlight the often-difficult relationship that had to be constructed and managed in situ between practitioners, site, instrument and instruction. As I show, different sites forced observers to think differently about their instrumentation and often required deviation from the printed or verbal instruction by which they had built their knowledge of an instrument. Exploring the different geographic trajectories of instruments can therefore tell us much about the mobility and, indeed, immobility of different magnetic knowledges. This is a particularly pertinent theme in the chapter that explores the establishment of the four colonial observatories and the abandoned establishment of the East India Company's Aden Magnetic and Meteorological Observatory. In both this chapter and that on Lefroy's North American survey, I explore what Baird has termed 'thing knowledge', which posits the materiality of scientific instruments as a kind of knowledge itself, although a knowledge often only made visible in times of disrepair.[1] This thing knowledge is witnessed at each of the colonial observatories, as the directors struggled to understand their instruments after their transferral from Britain to the colonial site and in Lefroy's survey, as each day the state of an instrument was assessed, managed and accommodated anew. In all then, what this thesis aims at is an extensive investigation of the sites of the magnetic crusade and the different practices of place that created, sorted and latterly distributed observational data. Next, this thesis will consider the methodology by which this examination has been constructed.

[1] Baird, *Thing knowledge: a philosophy of scientific instruments* (University of California Press: Berkeley, 2004).

Chapter 2: Methods

This section covers the methods used in the pursuance of a historical geography of nineteenth-century geomagnetic science. First, I introduce the structure of my PhD and several of the archives and sources by which I have assembled my thesis. This is followed by an exploration of archives of scientific data. I describe the data archive at two sites I visited in 2016 and 2017, and use this topic to introduce and discuss broader literature devoted to the archive and, a more recent concern perhaps, science in the archives.

Collaboration with the Royal Society

This PhD has been conducted as a Collaborative Doctoral Partnership (CDP) between the University of Glasgow and the Royal Society, located on Carlton House Terrace, London. It was funded by the Arts and Humanities Research Council (AHRC). Founded in 1660, the Royal Society is the UK's national science academy and has a Fellowship of c.1,600 of the most eminent scientists from across the globe. As well as acting in this capacity, the Royal Society also holds extensive and significant collections related to the history of science in the UK and beyond, given its long list of foreign members. These collections include enormous amounts of correspondence, scientific reports, notebooks, drawings, pictures, paintings, council and committee meetings, referees' reports, scientific instruments (held at Blythe House) and accession documentation. The library also holds texts relating to the history of science published recently and in the last several decades.

I was based at the Royal Society for the majority of my second year of study, but I also conducted visits in both my first and third years. As space was limited, I had no designated desk but worked in the publicly accessible library or, on occasions when it was possible, at an absent staff member's desk in the offices above the library. While this made working at the Society sometimes precarious, I was given a staff card that allowed me to access all levels of the building and to enter the library prior to its daily opening to the public. At the Society, I was invited to the library team's Monday morning meetings to speak about what I would be doing that week and to stay informed about what other members of the team would be occupied with and listen to news regarding the Society. As well as participating in these meetings, I assisted the library by contributing a guest post – based on an aspect of my research – for their 'Repository' blog and by helping to catalogue the hundreds of items that make up the two volumes of correspondence between Edward Sabine and Humphrey Lloyd on the subject of terrestrial magnetism. Again however, because I had no designated workspace, this was done on an *ad hoc* basis and depended on a free computer terminal in the library team's section of the office. In the first year of my PhD, I had conducted several preliminary visits to the Royal Society and it had been decided that helping to catalogue the collections I was working on would be both beneficial to the library and doable in terms of the extra workload it would create. During this time, I also accompanied Keith Moore, the Curator, and, on one occasion, Simon Naylor too, on several visits to the Science Museum's Blythe House repository, which holds both the Royal Society's collection of scientific instruments and several magnetic instruments that I have written about in the thesis, notably dip circles designed by Robert Were Fox and Thomas Charles Robinson respectively. These visits were beneficial and educational, the more so because in my previous Masters research I had only consulted document archives.

At the archives of the Royal Society, much of my research was focused on the correspondence between Sabine and Lloyd, and between Lloyd and the colonial observatory managers. The two volumes amounted to more than 200 items of correspondence, spanned the early 1830s to the 1870s, and were specifically focused on geomagnetic research and the experience of building and managing magnetic observatories, all of which made them an invaluable source. However, my research in this archive also took in a wealth of other correspondence from Sabine, Herschel, Fox and other members of the magnetic lobby; the relevant council minutes and Committee of Physics minutes noted between c.1833 and 1857; and Sabine's notebooks of magnetic data obtained during his attachment to John Ross's voyage in search of the north-west passage in 1818. Evidence obtained from the Royal Society archive and library is used extensively in each empirical chapter of this thesis.

Other archives

Several other archives and repositories were consulted in the making of this thesis. Outwith the Royal Society, I spent most of my time researching at The National Archives (TNA) in Kew, London. The magnetic crusade, being in part a government-financed project, has left behind an extensive archive at TNA. Here, there are long series of correspondence between several of the scheme's actors; sketches and diagrams of observatories; official memoranda from the Board of Ordnance and Sabine's magnetic department; draft scientific reports;

and notes, observations and memoranda jotted down by Sabine at one time or another. Importantly, TNA holds correspondence on terrestrial magnetism that serves as a counterpart to the two volumes of letters held at the Royal Society. At the Royal Society, the letters are those addressed to Lloyd that had been collected and donated by his widow to the Royal Society after his death. At TNA, the letters are those that Lloyd wrote back to the likes of Sabine and so, together, these series form an almost complete geomagnetic conversation.

Three weeks were spent at the Library and Archives Canada (LAC) and the Canada Science and Technology Museum (CSTM), both in Ottawa, thanks to a grant from the College of Science and Engineering Mobility Fund. At LAC, I consulted a microfilm copy of John Henry Lefroy's survey journal and other correspondence from Lefroy during his period of work in Canada. At the CSTM, aided by David Pantalony, Curator of Physical and Medical Sciences, I was given the opportunity to handle some of the original Toronto Magnetic Observatory's instruments – two magnetometers by Thomas Grubb – and time in the museum's library consulting some of the museum's own research reports into its magnetic collections. These research visits formed the basis for my chapter on Lefroy's North American Magnetic and Meteorological Survey. I was also able to aid the CSTM by informing them of the provenance of their magnetometers, which had been made at Thomas Grubb's workshop in Dublin in 1839. Further archival and library visits were made to the British Library – to consult a printed collection of Lefroy's letters – and to the Special Collections of the University of Glasgow's library – to consult expeditionary narratives, BAAS reports, and an early history of the Royal Society by Charles Weld. In compiling the evidence for the final empirical chapter of the thesis – on historical data management and the administration of the magnetic scheme – I explored the geomagnetic data archive of the British Geological Society at the Lyell Centre in Edinburgh. My visit to this archive was prompted by the discovery of a similar but much more disordered geomagnetic data archive at the Ottawa Geomagnetic Observatory. Both archives are described in more detail below.

Building 17, Ottawa Geomagnetic Observatory. British Geological Survey, Lyell Centre, Edinburgh

Savours and McConnell, in their history of the Rossbank Observatory, Tasmania, noted 14 different registers, extracts and graphs within the Archives Office of Tasmania. In length, these 14 items occupied a little under a metre of shelf space.[1] In my own research, I encountered two different magnetic data archives: one within 'Building 17' of the Ottawa Geomagnetic Observatory and one at the British Geological Survey's Lyell Centre in Edinburgh.

Building 17 of the Ottawa Geomagnetic Observatory sits at the far end of the magnetic observatory complex's sprawling site, at the boundary between scientific site and the surrounding woodland. Although the term "observatory" might conjure the image of a single, large, specific, modern and clean room, the Ottawa Geomagnetic Observatory is in fact a collection of seventeen wooden buildings, each of which could be mistaken for large sheds and each of which – excepting number 17 – house an array of working magnetometers and older, shelved, magnetic instruments. The old instruments – of late nineteenth and early-to-mid twentieth century construction – some bastardized at one time or another to modify a different instrument, sit on shelves above their functional descendants: a commercial tri-axial ringcore fluxgate magnetometer, mounted on a tilt-correcting suspension, and an Overhauser Proton Precession Magnetometer.[2] Some of these older instruments had at one point been on display as museum pieces. Here then we had, in the admittedly few buildings I was shown, both museological and operational scientific space – the past, present and future of Canadian magnetic science. The Ottawa Observatory had replaced the Agincourt Observatory in 1969, which had itself replaced the original geomagnetic observatory in Canada, the Toronto Observatory, in 1898. The Toronto Magnetic and Meteorological Observatory had been constructed in 1840. The magnetic observatory had travelled across time and place and with each displacement it had carried pieces of its old self along with it. Part of this story could be read in the instruments, but nowhere was this story more immediately present than in Building 17.

This discreet building was used to store historical geomagnetic data stretching back as far as 1840, the year in which the Toronto Magnetic Observatory was established by Charles Riddell and the British Ordnance Department. The interior organisation of the building was haphazard and the conditions murky, musty and in a general state of disregard. In the corner farthest from the door, amongst boxes of later magnetograms, sat the oldest material, the ledgers containing handwritten eye-observations from the Toronto Magnetic Observatory. Despite the damp conditions, these ledgers had survived remarkably well. It is unclear exactly how much data from the first 13 years of the observatory's operation (i.e.

[1] Savours, A., and McConnell, A., 'The history of the Rossbank Observatory, Tasmania', *Annals of Science* 39, 6 (1982), 527-564, appendix, 557-558.
[2] Ottawa Magnetic Observatory, http://www.geomag.nrcan.gc.ca/obs/ott-en.php [accessed 12 October 2017]

when it was under British administration) is held in this place but efforts to digitise the earliest registers are currently underway, so more will be known soon.

The geomagnetic data archive of the British Geological Survey (BGS) was a lot more formal than that which I encountered in Ottawa. It was also much more significant in scope than either the records of the Rossbank Observatory noted by Savours and McConnell or the ledgers of Building 17. This is because the BGS holds the registers of data that would have been sent to Sabine's magnetic department at Woolwich. There are magnetic returns from all of the colonial observatories, from Ross's Antarctic expedition, Lefroy's North American survey, as well as collections of data from the first British Magnetic Survey and those derived from experiments on magnetic instruments at Woolwich. The route by which the BGS came to house these geomagnetic records is not wholly clear, but it seems most likely that Sabine passed the records from Woolwich to Kew Observatory, which the BAAS took over in 1842 and which became the central observatory for magnetic and meteorological work throughout the nineteenth century.[1] From here, the records were moved in 1900 from Kew to Eskdalemuir in Scotland, and then a decade later in 1910 from Eskdalemuir to the Meteorological Office's Lerwick Observatory. Geomagnetic observations were made at this site until 1968 when responsibility for making observations and storing new and historical observations transferred to the Institute of Geological Sciences, renamed the British Geological Survey in 1984.[2] In 2016, the BGS geomagnetic research team moved from Nottingham to their current site at the Lyell Centre, situated on the Riccarton campus of Heriot-Watt University, Edinburgh, and all geomagnetic records were transferred with them. Thus, the magnetic data archive had been anything but static since the closure of Sabine's magnetic department in 1857.

In visiting this archive, I chose 34 items to view: from pocket notebooks to large, heavy, bound, daily registers, each page of which measured 30x25cm. Contrarily, this archive, though outwardly much cleaner and more ordered than that of Building 17 in Ottawa, held items that displayed the myriad workings, crossings out and reductions, that turned colonial observatory returns into publishable data. At the BGS, the prudent ledgers of Toronto and Rossbank, St Helena and the Cape of Good Hope, had been interacted with,

[1] Macdonald, L. T., 'Making Kew Observatory: the Royal Society, the British Association and the politics of early Victorian science', *Brit. J. Hist. Sci*. 48, 3 (2015), 409-433. Macdonald also gives a list of other works on the history of Kew, for which see p. 412.

[2] This information was communicated to me by Robert McIntosh, by way of Alison Fernie, at the BGS. I would like to thank them both for this. Alison also kindly and genially assisted me in my archival research at the BGS, for which I was and remain very grateful. A debt of thanks is also owed to Susan Macmillan, who amicably walked a largely ignorant student (me) through the science of geomagnetism and current researches in this field, and to Chris Turbitt, for showing me examples of current geomagnetic instrumentation and answering my very simple questions about them!

scribbled on, changed. At the BGS, organised and robust boxes housed disorganised and messy returns, while in Ottawa, organised and robust accounts were kept in disorganised and messy circumstances. The juxtaposition of these two sites invited me to consider what constitutes an archive, and the following section seeks to elaborate on this question with recourse to literature devoted to the concept and practices of the archive. A short review of this literature is necessary not only for considerations of data archives, but because it is pertinent to the whole process by which the thesis that follows was compiled. The BGS and Ottawa geomagnetic archives will be returned to in the final empirical chapter of this thesis.

Rethinking the geomagnetic data archive

The archive, writes Withers, 'as a site of knowledge making is as deserving of attention as … the lecture hall, the pub, the library and the laboratory'.[1] DeSilvey concurs and in her own work emphasises how 'systems of selection and conservation produce, as much as preserve, the items they claim responsibility for'.[2] The archive is not only a repository for knowledge, but a stimulus to, and place for, the production of new knowledges. But, there are several ways in which they archive can be interpreted. It can be, variously and at the same time, a site of authority and meaning and 'a particular expression of classification as one 'way of knowing''.[3] It is, in Foucault's estimation, the 'system that governs the appearance of statements as unique events' and the site in which things are 'grouped together in distinct figures, composed together in accordance with multiple relations, maintained or blurred in accordance with specific regularities'.[4] For Osborne, the archive is something akin to Latour's centre of calculation except that what takes place there is more likely to be interpretation than calculation, which prompts Osborne to define the archive as a 'centre of interpretation'.[5] Withers presents further readings of the archive, from Derrida's configuring of the archive as 'both a place and a reflection of social and institutional authority' and 'site of action', to Lynch's counterpoint, which speaks of 'archival ethnographies' and posits archives as 'salient less as methodological resources

[1] Withers, C. W. J., 'Constructing "the geographical archive"', *Area* 34, 3 (2002), 303-311, 305.
[2] DeSilvey, C., 'Art and archive: memory-work on a Montana homestead', *Journal of Historical Geography* 33 (2007), 878-900, 888.
[3] Quotation from Pickstone, J., *Ways of knowing: a new history of science, technology and medicine* (Manchester University Press: Manchester, 2000), 10-11, 60-82, in Withers, 'Constructing "the geographical archive"', 304.
[4] Quotation from Foucault, M., *The archaeology of knowledge* (Tavistock: London, 1972), 129, in Withers, 'Constructing "the geographical archive"', 304.
[5] Osborne, T., 'The ordinariness of the archive', *History of the Human Sciences* 11 (1999), 85-102, 52.

for historical studies than as historical phenomenon in their own right', an orientation which 'does not negate the scholarly use of archival information' but instead 'shifts attention to archives *in formation* and the localised gathering of histories'.[1]

Withers eventually arrives at a personal definition of the archive as 'the result of contingency, of the haphazard accumulation of stuff' and not as a 'straightforward expression of power', as Derrida or Foucault may more explicitly have positioned it. Cresswell's later work on place, in his case Maxwell Street, Chicago, as an archive, chimes with Withers's contention, as does DeSilvey's work on a Montana homestead. DeSilvey, tasked with trying to archive the erratic, idiosyncratic and multifarious items collected by families over more than a century of dwelling at the homestead, encountered a plethora of challenges thrown up by trying to construct an archive and a story out of objects that often refused the 'emerging order' she tried to create.[2] Eschewing traditional archival techniques which could not contend with the array of artefacts happened upon, DeSilvey employed a more artistic methodology, which sought to tell 'small stories' and to establish distinctions between archive and site 'only to collapse them through gradual dispersals'. In such a way, 'locally mobile objects moved into the boxes and back out again, stitching a continuous relation between the place and its past'.[3] DeSilvey took 'constellations of loosely associated fragments' – hair, scraps of newspaper, old coins, tools, clothing – which were the only materials at hand and used this material 'to craft stories about people and place that might otherwise go untold'.[4]

A derelict Montana homestead and a city market are very different archives to that which exists at the Royal Society or the National Archives, two archives that have been consulted and used extensively throughout this thesis. These latter two sites are more purposeful archives than that which DeSilvey and Cresswell encountered and ordered. However, DeSilvey and Cresswell's work was important in the formation of the final empirical chapter of this thesis – focused on historical data management and administration – because forming a narrative from the disordered nature of the first geomagnetic data archive I encountered in Ottawa was not a straightforward nor familiar task. Geomagnetic data, in previous histories of the British magnetic scheme, have been disregarded. DeSilvey and Cresswell explore the distinction between waste and artefact and this enabled

[1] Derrida, J. (trans. Prenowitz, E.), *Archive fever: a Freudian impression* (University of Chicago Press: Chicago, 1995); Lynch, M., 'Archives in formation: privileged spaces, popular archives and paper trails', *History of the Human Sciences* 12 (1999), 65-88, 83, in Withers, 'Constructing "the geographical archive"', 304, 308.
[2] DeSilvey, 'Art and archive', 880.
[3] DeSilvey, 'Art and archive', 898.
[4] DeSilvey, 'Salvage memory: constellating material histories on a hardscrabble homestead', *Cultural Geographies* 14, 3 (2007), 401-424, 420, 421.

me to approach geomagnetic data from a different perspective. As Cresswell states, 'the same object may be seen as valueless and thus mere waste in one regime and as precious and worthy of being granted durability in another. The value of an object is thus not defined by any sense of its essential properties or functional utility' but by people inscribing value to the object, sometimes on a purely personal basis.[1] At least since Bourdieu have such ideas of value and the archival process been recognised, and since the work of Appadurai and Kopytoff have scholars considered value as a process produced 'by the passage of things in an out of different *regimes of value*'.[2] My work attempts to include the geomagnetic data archive in a new value regime, one in which the returns of the British magnetic scheme can be used not to dismiss the scheme as the apotheosis of Baconian number collecting zeal, but as an indicator of the practice of data handling, packaging and distribution.

While DeSilvey, McGeachan, Till, Mills and others have worked with and written on the process of working with fragments, traces and absences in the archive, Hodder has recently remarked on an aspect of archival research that better resonates with my own archival experience: the problem of abundance.[3] I encountered abundance not only in the data archives of the BGS but in TNA too, which houses the huge quantity of governmental records pertaining to the British magnetic scheme. Here alone, there are over 80 files associated with the administration of the scheme, many of which run to several hundred pages of documentation. Hodder argues that archives are spaces often configured by abundance as much as scarcity, which can be a problem as 'abundance risks drowning out those perspectives that are faintly heard'.[4] For Hodder, one methodological approach that can contend with abundance in the archive is biography or 'life geographies' that 'by bending space *and* time, allow a greater flexibility to chase the scattered remains of transnational lives through the archive'.[5] To an extent, this is a methodology I have also adopted in my archival research, focusing as I have on the different geographies that were important to the life of, particularly, Edward Sabine. However, while Hodder stipulates that biography might not be a good approach for 'those interested in the non-human', I

[1] Cresswell, T., 'Value, gleaning and the archive at Maxwell Street, Chicago', *Trans. Inst. Brit. Geog.* 37 (2012), 164-176, 168.
[2] Cresswell, 'Value, gleaning and the archive', 168.
[3] On fragments, traces and absences, see Lorimer, H., and Philo, C., 'Disorderly archives and orderly accounts: reflections on the occasion of Glasgow's geographical century', *Scottish Geographical Journal* 125 (2009), 225-255; and McGeachan, C., 'Historical geography II: traces remain', *Progress in Human Geography* 42, 1 (2016), 134-147; Mills, S., 'Cultural-historical geographies of the archive: fragments, objects and ghosts', *Geography Compass* 7 (2013), 701-713; Ogborn, M., 'Editorial: Atlantic geographies', *Social & Cultural Geography* 6 (2005), 379-385; Till, K., 'Fragments, ruins, artifacts, torsos', *Historical Geography* 29 (2001), 70-73.
[4] Hodder, J., 'On absence and abundance: biography as method in archival research', *Area* 49, 4 (2017), 452-459, 454.
[5] Hodder, 'On abundance', 456.

would argue the contrary. I use the language of life-geographies or geobiographies in the chapter devoted to Lefroy's survey, in order to follow the different instruments Lefroy carried with him during his North American Magnetic and Meteorological Survey in and through the different spaces in which they were deployed, managed and maintained. Such an approach can also usefully be employed in dealing with the data archive of Britain's magnetic scheme. As I demonstrate in the final empirical chapter of this thesis, it is the geography of the life and death of data that can illuminate some of the more neglected aspects of the scientific process in the early nineteenth century.

Science in the archives has emerged as a subject of scrutiny for a number of authors in recent years. The collection of essays contained within *Science in the archives: pasts, presents, futures* (2017) can be seen as one particular and early attempt to cohere thinking on this subject. Archives, it is claimed, have been 'mostly invisible in accounts of the sites and practices of science'. However, each of the essays presented in this volume show that, today, archives are more and more becoming active sites of science 'whether it is a botanist consulting the type specimen of a plant species in the Linnaean herbarium now housed in a London strong room' or a physician studying an extraordinary case from decades or centuries past or a palaeontologist browsing the fossil compendia of nineteenth-century geologists.[1] It is what Daston terms 'third nature'. Observations, measurements, experiments: all take first nature and turn it into second nature, the point at which analysis can occur. 'But once second nature slips from science present into science past', Daston states, 'collective empiricism requires a third nature: the repository of those findings of second nature selected to endure … the archives of the sciences'.[2] In the pre-electronic era, 'huge amounts of data were collected on a global scale, with the explicit aim of creating data archives that could be endlessly mined' but these archives were also 'bound in space and time to physical archives and analog infrastructures'.[3] In order for these scientific archives to endure, they have had to undergo various transformations and dislocations. Hsia talks of the multiple material remediations that the textual archives of astronomy have undergone going back to the nineteenth century, from observatory reports to astronomy journals, university and professional society publications, and astronomical catalogues through to current day electronic bibliographic databases, all of which has resulted in contemporary astronomers' struggles 'to keep afloat in a "data tsunami" estimated … at 1

[1] Daston, L., 'Introduction: third nature', in Daston, L., (ed.), *Science in the archives: pasts, presents, futures* (The University of Chicago Press: Chicago and London, 2017), 1-16, 2.
[2] Daston, 'Introduction: third nature', 1.
[3] Aranova, E., von Oertzen, C., and Sepkoski, D., 'Introduction: historicizing big data', *Osiris* 32, 1 (2017), 1-17, 16.

petabyte of publicly available material in electronic form and increasing by 0.5 PB each year'.[1]

The scientific archive is prominent in the final empirical chapter of this thesis because it has become a well-used source of information for current day geomagnetic researchers who want to create and use archives of data to construct long-term models of the movement of the earth's magnetic field. The creation and use of the data archive illuminates what Daston calls a 'labour of Hercules': the calibration of methods, instruments, records and observers 'across polities, epochs, and genres'.[2]

Conclusion

This section has sought to explain the avenues of enquiry that I have pursued in the course of my research. It has described the structure of my CDP PhD and the work I undertook at the Royal Society and it has provided information on the archives through which this thesis has been constructed. Latterly, this section also engaged with an archive that has been underexplored by historians of science and used this discussion to introduce wider literature devoted to the archive as a site of knowledge making. As the above demonstrates, the evidence used in this thesis includes documents, objects and – somewhere in-between – data. Large archives of these materials exist and, while this is fortunate, as Hodder has explained, this abundance can create problems that require the use of certain archival methodologies to overcome. Faced with the abundance of documentary and material archives, I chose, like Hodder, to focus on the life-geographies or geobiographies of the subjects I wanted to scrutinise. However, unlike Hodder, I used this strategy to follow both human and non-human subjects in the archive. This methodology is reflected in the structure of the thesis, which is built around the different spaces of the magnetic scheme – observatories, surveys and calculating departments – and how certain individuals and materials moved within these spaces. Employing this strategy with the geomagnetic data archive required a personal re-examination of how archives can be constructed and how the demarcation between waste and artefact is made.

Finally, for a list of the most relevant files consulted at the BGS, the Royal Society, LAC and TNA, please see the appendix to this thesis. This thesis will now turn to discuss the

[1] Hsia, F., 'Astronomy after the deluge', in Daston, *Science in the archives*, 17-52, 37.
[2] Daston, 'Introduction: third nature', 9.

literature in which this work sits, and the literature by which many of my research questions were prompted.

Chapter 3: Literature review

Introduction

This review situates an exploration of British magnetic research in the early nineteenth century within the appropriate temporal and spatial contexts, and is split into two sections. The first section is more specifically concerned with placing the thesis in its historical context, while the second section identifies and analyses the literature from which this thesis has taken its methodological and historiographical cues. To begin the first section, a short history of the longer history of terrestrial magnetic research in Britain and beyond during the seventeenth and eighteenth centuries will be presented, as important changes to the structure of the magnetic community within Britain occurred during this time that disciplined research into the subject of terrestrial magnetism. This will be followed by an examination of certain aspects of the historiography that pertains specifically to the magnetic crusade. This will be followed by a broader look at the history of the physical sciences in Britain in the nineteenth century and the reform of the organisation of science in Britain at this time. Next, the section introduces some of the theoretical and instrumental innovations that took place in Europe at the start of the nineteenth century as well as the systems of observation that were fomented and coordinated from places like the Göttingen and Paris observatories.

The next section of the review discusses the methodological and theoretical underpinnings of the thesis. First, the section details the relevant and significant methodological innovations of sociologists and historians of science working in the 1970s and 1980s that established the field of the sociology of science. It was the questions and perspectives developed by scholars working in this new field from which the contours of the historical geography of science were described, a discussion of which is therefore included in the next part of the review. The principal tenets of the historical geography of science will be discussed and examples provided of literature that pertains to geographies of scientific production and geographies of the circulation of scientific knowledge, as it is to these two areas that this thesis contributes. As a substantial part of this literature has concerned the production, use and movement of objects in and through different spatial contexts, the review will naturally segue into an exploration of what I term object-oriented histories and geographies of science. There will be an emphasis on scientific instruments in situ and on the move but attention is also paid to different objects, especially models, and different methodological approaches to their analysis, such as historical experimentation and replication, because these have a relevance for certain aspects of the thesis. Finally, the last part of this review will discuss two literatures that are particularly pertinent to the final chapter of this thesis: data and the archive. The data archive of the British magnetic scheme is an extensive and underutilised resource, and so this part of the review will explore literature that can provide the means to define what data are and how to approach and use data as a historical artefact.

Section I: The physical sciences between the seventeenth and the nineteenth centuries

Terrestrial magnetism in the seventeenth and eighteenth centuries

Midway through 2017, something happened in Britain that had not occurred for the past 350 years. The agonic line, the line of zero declination or variation between true north and magnetic north, crept over the British Isles from the east, making land, metaphorically speaking, around Lowestoft in East Anglia and Margate in Kent. As Susan Macmillan of the British Geological Survey has explained, the agonic line is due to pass slowly over the British Isles during the next few years. This is a significant event. For the last 350 years, declination in the UK and Ireland has been westerly, meaning that magnetic north has been west of true north. However, the procession of the agonic line is followed by easterly declination, so magnetic north will soon lie east of true north across the British Isles. The resultant shift of the position between magnetic and true north will have implications for compass bearings and the process of swapping between map bearings and magnetic bearings. Certain navigational mnemonics will also become redundant, Macmillan has written, such as “grid to mag, add – mag to grid, get rid.”[1]

Most will not be cognisant of the advance of the agonic, let alone unduly affected by it, but the fact that geophysicists today know that the line has not passed over these shores for the

[1] Macmillan, S., ‘Advance of the Agonic – what does this mean?’, http://britgeopeople.blogspot.co.uk/2017/10/advance-of-agonic-what-does-this-meanby.html [accessed 5 February 2018].

last 350 years is testament to the long history of geomagnetic observation in the British Isles. Observations of the last procession of the agonic line were made by Henry Bond in London in 1657 or, at least, he may have extrapolated the fact from others' observations, the truth is unclear. Two centuries later, and without any obvious evidence, Felgentraeger gave the spot for the observations as Whitehall Gardens.[1] Bond was, by his own admission, 'an Intelligent Mathematician and Teacher of Navigation' and a decade later, buoyed perhaps by his observation of the agonic line and the consequent procession to a westerly declination, he used the nascent *Philosophical Transactions* to print a table predicting the declination in London from 1663 to 1716. The table was meant as a provocation to '*Philosophical* men' everywhere to 'excite new thoughts on the 'doctrine of the *Magnet* and *Magnetical* motions', a subject 'yet so obscure, that what hitherto hath been discoursed and written upon that subject, proves very unsatisfactory to Men that consider the various *Phenomena* and effects of that Body'.[2]

Bond may have been dissatisfied with erstwhile pursuits of magnetic research, but this should not divert from the considerable significance of pre-seventeenth-century geomagnetic study. Terrestrial magnetic observations had been made as early as the twelfth century in China while, according to Jonkers, the first 'dry-pivoted bearing compass' was developed in the West in 1269 by Pierre de Maricourt, better known by his pen name Petrus Peregrinus.[3] However, Kono suggests that an earlier dry pivoted compass was constructed in China in or before 1150. As Kono explains, 'in this compass, an [sic.] wooden turtle has a tail made of a magnetic needle. A thin bamboo stick stands from the baseboard and holds the turtle at the hole made in its belly. The turtle rotates and points to the north because of the magnetic needle'.[4] By the sixteenth and early seventeenth century in Britain, natural philosophers and mariners had started conducting experiments in, and recording observations of, terrestrial magnetism. Nascent attempts to formulate hypotheses on the subject were developed by Robert Norman in *The Newe Attractiue* (1581) and William Gilbert in *De Magnete* (1600).[5] It was Gilbert who, extrapolating from Peregrinus's much earlier experiments with spheres of lodestone, described the earth as behaving like a huge magnet with two poles, a plus and a minus one. Gilbert is considered

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[1] Malin, S. R. C., and Bullard, E., 'The direction of the Earth's magnetic field at London, 1570-1975', *Phil. Trans. R. Soc. Lond.* 299 (1981), 357-423, 388.
[2] Bond, H., 'The variations of the magnetick needle predicted for many yeares following', *Phil. Trans. R. Soc. Lond.* 3, 40 (1668), 789-790.
[3] Jonkers, A. R. T., *Earth's magnetism in the age of sail* (Johns Hopkins University Press: Baltimore, 2003), 42; see also, Jonkers, A. R. T., 'The pursuit of magnetic shadows: the formal-empirical dipole field of early-modern geomagnetism', *Centaurus* 50 (2008), 254-289.
[4] Kono, M., 'Geomagnetism', in Schubert, G. (ed.), *Treatise on geophysics* Vol. 5 (Elsevier: Amsterdam, 2009), 1-7, 4.
[5] Jonkers, *Earth's magnetism*, 62, 66

by many as the father of magnetism for this observation and because of, as Merrill points out, 'his reliance on the (now-called) experimental method'.[1] For the interested reader, there are several detailed chronological accounts of the history of geomagnetic research stretching back to the sixth century B.C. in some instances, for which Chapman and Bartels' extensive volumes still perhaps remain the most comprehensive, nearly eighty years on.[2]

Observation and study of the Earth's magnetic field continued throughout the seventeenth century and led to the discovery of secular (long-term) variation of the force by Henry Gellibrand in 1634, as well as the propagation of several different theories on the number and structure of magnetic poles and the source of the Earth's magnetism. However, the motivation behind early modern magnetic research was as much if not more about the advancement of navigation than the formulation of theory. Indeed, this was one of the 'primary objectives' of the Royal Society from its creation in 1662.[3] Several decades later Edmond Halley furthered this objective through two scientific voyages designed to investigate the applicability of terrestrial magnetic research to navigation.[4] Alexander von Humboldt would later herald these voyages as examples of the first government-sponsored scientific ventures on the subject of terrestrial magnetism.[5] Halley's expeditions and later terrestrial magnetic work, though dogged by controversy and accusations of plagiarism, was significant for an emphasis on visualising the magnetic force and on his assertion of two dipoles, a theory that would be influential until Gauss disproved it in the first half of the nineteenth century.

Halley's first map, that of equal declination across the globe, was produced in 1701, compiled from observations by Halley himself and from other observers' accounts. Halley was the first person, according to Fara, to 'introduce what are now called 'isogonics' but

[1] Merrill, R. T., *Our magnetic Earth: the science of geomagnetism* (The University of Chicago Press: Chicago and London, 2012), 2. Jonkers, *Earth's magnetism*, notes Gilbert as the 'founder of terrestrial magnetism', 66; for more on Gilbert, see Bennet, J., 'Presidential address: knowing and doing in the sixteenth century: what were instruments for?', *Brit. J. Hist. Sci.* 36, 2 (2003), 129-150.
[2] Chapman, S., and Bartels, J., *Geomagnetism*, Vol. I and II (Oxford University Press: Oxford, 1940); Jonkers, *Earth's magnetism*; Courtillot, V., and Le Mouël, J-L., 'The study of earth's magnetism (1269-1950): a foundation by Peregrinus and subsequent development of geomagnetism and paleomagnetism', *Review of Geophysics* 45 (2007), 1-31; McConnell, A., 'Surveying terrestrial magnetism in time and space', *Archives of Natural History* 32, 2 (2005), 346-360; Mills, A. A., 'The lodestone: history, physics, and formation', *Annals of Science* 61, 3 (2004), 273-319; Smith, J. A., 'Precursors to Peregrinus: the early history of magnetism and the mariner's compass in Europe', *Journal of Medieval History* 18, 1 (1992), 21-74; Kono, M., 'Geomagnetism', in Schubert, G. (ed.), *Treatise on geophysics* Vol. 5 (Elsevier: Amsterdam, 2009), 1-7. See p. 4 of this latter volume for an excellent diagram of a Chinese turtle compass.
[3] Jonkers, *Earth's Magnetism*, 84.
[4] Cook, A., 'Edmond Halley and the magnetic field of the earth', *Notes and Rec. R. Soc. Lond.* 55, 3 (2001), 473-490.
[5] Cawood, J., 'Terrestrial magnetism and the development of international collaboration in the early nineteenth century', *Annals of Science* 34, 6 (1997), 551-587, 553.

what at the time were known as Halleyan lines, ‘curves that link together abstract, measured quantities such as temperature or pressure’ or variation. The method of drawing such lines ‘enabled Halley to impose a neat pattern onto the magnetic observations that had previously appeared so chaotic. [Halley] was systematising the world with the same approach as the Enlightenment encyclopaedia compilers, who often used mapping metaphors to describe how they were organising facts into territories and domains’.[1] Fara also highlights how significant maritime men were in the education of natural philosophers such as Halley in the techniques involved with summarising information visually, rather than in long pages of figures.[2] After Halley, the ability of magnetic practitioners to visualise their research became a requirement in the production of credible magnetic science. Sabine, a dedicated student of Halley’s work, was always zealous in the production of magnetic charts. In their attempts to gain funding for a worldwide system of observation, the British magnetic lobby of the 1830s, which included Sabine, lauded the expeditions and researches of Halley as the cornerstone of a once great British tradition of terrestrial magnetic research that had been supplanted by practitioners and theorists on the continent during the eighteenth century.

The opinion of Sabine and others, that British magnetic research had fallen behind that of its near neighbours France and others on the continent by the beginning of the nineteenth century, was not unfounded, but it also obscures the importance of what was a significant century of research, change and development in Britain’s magnetic community. For instance, it was during the eighteenth century, Fara argues, that magnetism was constructed as a legitimate discipline of science dependent on precision measurement. Fara claims that part of the process of establishing magnetism in this way was through the separation and conscious differentiation of magnetic researchers and mathematical practitioners. As Fara states, ‘to establish themselves as elite purveyors of magnetic knowledge, [natural philosophers] widened their separation from men such as instrument makers and navigators with whom they were in close contact and whose techniques they were utilizing’.[3] This differentiation was also reflected in the categorisation of instruments of magnetic research by century’s end. While previously magnetic instruments might all have been marketed as mathematical instruments, by the end of the eighteenth century a

[1] Fara, P., *Fatal attraction: magnetic mysteries of the Enlightenment* (Icon Books UK: Cambridge, 2005), 74.
[2] Fara, *Fatal attraction*, 79.
[3] Fara, *Sympathetic attractions: magnetic practice, beliefs, and symbolism in eighteenth-century England* (Princeton University Press: Princeton, 1996), 124-125.

difference had been created between, for example, an azimuth compass (mathematical instrument) and a variation compass (philosophical instrument).[1]

Although English natural philosophers increasingly sought to occupy a space distinct from instrument makers and mariners in the eighteenth century, this did not mean a wholesale exodus from the workshop to consider purely theoretical magnetic problems, such as the search for a mechanical law to match Newton's law of gravity, first formulated in 1687. On the contrary, 'from the middle of the century, English natural philosophers abandoned the search for a magnetic-force law'.[2] Newton was unable to replicate his earlier success with gravity and others' conclusions on the matter were so varied and often so at odds with each other that enthusiasm waned for such pursuits. Generally speaking, English natural philosophers instead turned to making their knowledge commercially useful, by means of improving navigational practices and instruments at sea. They spent more time scrutinising the instruments and materials they used, such as iron, on which extensive experimentation was conducted to try to provide answers to questions about whether iron changed in weight after magnetisation. Further experiments compared the efficacy of different techniques for making artificial magnets, a subject that later courted controversy at the outset of the magnetic crusade through the allegations contained in the letters to *The Times* of 'Cui Bono', of which more later. It was also in the eighteenth century that the 'first nonmagnetic huts devoted to magnetic observations were built'. One such hut built in Sumatra was described by John MacDonald in 1794.[3]

Many of the themes visible in geomagnetic research in the eighteenth century carried through to the nineteenth. These included the gradual separation of magnetic researchers and mariners, the latter of whom had provided much of the knowledge and techniques adopted by English natural philosophers, the increased scrutiny of the construction of, and materials used in, magnetic instruments, and the establishment of designedly nonmagnetic buildings as spaces of observation. Some nineteenth-century magneticians, such as Sabine, were occasionally quick to bemoan the comparably inferior position of British magnetic research on the European stage at the beginning of the century. However, as Fara has shown, the context that allowed global observation schemes to be undertaken with precision instrumentation by individuals skilled in both the craft and mathematics of geomagnetism in the nineteenth century, was created through the interplay and latterly

[1] Fara, *Sympathetic attractions*, 133; Bennett, 'Presidential address'; Warner, D., 'What is a scientific instrument, when did it become one, and why?', *Brit. J. Hist. Sci.* 23, 1 (1990), 83-93.
[2] Fara, *Sympathetic attractions*, 128.
[3] Courtillot, and Le Mouël, 'The study of earth's magnetism (1269-1950)', 14.

demarcation of differentiated groups of practitioners – natural philosophers, mariners, instrument makers – in the eighteenth century.

The magnetic crusade

Throughout the thesis that follows, literature pertaining to the British magnetic scheme will be touched upon and explored in relation to specific aspects of the system. To avoid repetition, this section will only provide a broad outline of this literature and note which topics have been covered and which remain underexplored. For one, much of what has been written about the British magnetic scheme has concerned two related themes: the origins of the scheme; and the politicking that attended its organisation. The instigation of the scheme in 1839 was the culmination of many years of lobbying – and one failed attempt to launch an earlier iteration in 1834-1835 – by figures such as Sabine, Lloyd, Herschel, Francis Beaufort, William Whewell, and George Peacock; the group Cawood calls the "geomagnetic lobby", although this ought not to imply that they were at all times – or at any time – a homogenous collective. For Cannon, these were the 'Ross conspirators': those who 'handled' the Admiralty, the Royal Society and the BAAS, and who were unable 'to relax a minute until Ross's flag [was] hoisted over his ship' and the Antarctic expedition officially launched.[1] How this group pushed their agenda through the BAAS and the Royal Society and the corridors of power in government and the Admiralty has been used to illuminate the relationship between science and the state in early Victorian England. Cawood's explorations of the origins of the crusade particularly highlighted Herschel's political connections and Sabine's relationship with both the Admiralty and the War Office – two institutions that would help to finance the scheme. Cawood's study therefore highlighted the continued importance of the status of certain individuals in Britain's scientific community in the 1830s and of the Royal Society as a prestigious vehicle that exerted considerable influence, despite the changing landscape of British science heralded by reformers like Charles Babbage and James South and the criticisms levelled at the Royal Society at this time. More recently, Carter has tried to emphasise the particular importance of Herschel – for his status, connections, and organisational capacity – in galvanising support and procuring funding for the magnetic scheme.[2] Cawood is also quick to highlight the role of the newly formed and more

[1] Cannon, S. F., *Science in culture: the early Victorian period* (Dawson: New York, 1978), 251.
[2] Carter, C., 'Magnetic fever: global imperialism and empiricism in the nineteenth century', *Transactions of the American Philosophical Society* 99, 4 (2009), i-169, especially chapter 3.

reformist BAAS in garnering enthusiasm for the scheme and for providing 'the platform for the magnetic lobby to express its ideas which could not be offered by the Royal Society'.[1] Carter's work has gone further than looking only at the relationship between science and the British government domestically by situating his exploration of the magnetic scheme within the context of the British Empire and specifically how this institution 'provided the necessary resources for the creation of a universal inductive geoscience that was shaped by the political and social realities of the state apparatus that sponsored it'.[2]

While the politicking of the geomagnetic lobby has been recorded in detail by the works mentioned above, less has been said about the continued political wrangling attached to the administration of the magnetic crusade as it was undertaken. It is as though, in noting how the morale and leadership of the geomagnetic lobby collapsed once the scheme had been enacted, interest in the administration of the scheme similarly collapses.[3] However, one account that does not follow this trend is Macdonald's study of the politicking – again involving Sabine – that accompanied the transformation of the Kew Observatory into a physical observatory. Macdonald uses this transformation as an example of the kind of institutional and individual political battles that remained a fixture of terrestrial magnetic science specifically and British science more widely in the 1840s and ultimately shaped places like Kew and the colonial observatories. Part of the motivation for the final chapter of this thesis is to similarly explore the continued political machinations attached to the magnetic scheme as it was administered at Sabine's magnetic department at Woolwich. Woolwich has been largely neglected in the historiography of the British magnetic scheme, which is surprising given that the magnetic scheme was a consciously military undertaking, as several of Sabine's clashes with the Board of Ordnance at Woolwich attest to and as I report in the final chapter. The military was involved in the management of several big Victorian scientific pursuits, such as the magnetic scheme, and this thesis illuminates the politics that attended such a process and addresses this historiographical lacuna.

Other foci of research into nineteenth-century geomagnetism include examinations of geomagnetic instruments and some of the sites in which this equipment was deployed and used. An account of these histories is given in the literature review of this thesis alongside broader explorations of scientific instrument use at the observatory and in the field. These histories have provided considerable technical information about magnetic instruments and

[1] Cawood, 'The Magnetic Crusade', 517.
[2] Carter, 'Magnetic fever', xxv.
[3] Cawood, 'The Magnetic Crusade', 513; Cannon, *Science in culture*, 251.

how they were built, set up and observed. They have also explored several of the different contexts in which magnetic instruments were tested and experimented on and the importance of instruments like the compass and materials like the magnetic bar in wider social, cultural and religious contexts in the nineteenth century. However, much less has been written about the mobilisation of these peculiar instruments to the observatory and the management of magnetic instruments in different states of repair in and between field sites. As I demonstrate, focusing on these instruments in such moments leads to reconsiderations of how the space of the observatory was constructed and the field managed and it also illuminates the often-intangible ways in which inexperienced scientific servicemen learned and came to understand the instruments they observed, beyond the limits of the instruction manual. Mobilisation of a magnetic instrument almost invariably led to that instrument being damaged but (happily for the historian, unhappily for the likes of Sabine) this has also provided ample material for histories of instrument repair and management and make overlooked labour visible again.

A final point for consideration in relation to the historiography of nineteenth-century terrestrial magnetic research is Cannon's characterisation of the magnetic scheme as emblematic of what she terms 'Humboldtian science'. This term was designed to combat what Cannon saw as the ahistorical application of 'Baconianism' to the data collecting activities of the crusade and other similar schemes by historians with little understanding of how Bacon's methods were perceived in the nineteenth century.[1] Cannon defined Humboldtian science as a term that could describe 'astronomy and the physics of the earth and the biology of the earth all viewed from a geographical standpoint, with the goal of discovering quantitative mathematical connections and interrelationships – "laws," if you prefer, although they may be charts and graphs'.[2] To this, Cannon added four clarifications: a new insistence on accuracy, not for just a few mixed instruments, but for all instruments and all observations; a new mental sophistication, expressed as contempt for the easy theories of the past, or as taking lightly the theoretical mechanisms and entities of the past; a new set of conceptual tools (isomaps, graphs, theory of errors); the application of these tools not to laboratory isolates but to the immense variety of real phenomena, so as to produce laws dealing with the very complex interrelationships of the physical, the biological and even the human.[3] Humboldtian science was 'the great new thing in professional science in the first half of the 19th century' and 'the accurate,

[1] Cannon, *Science in culture*, especially 74-77.
[2] Cannon, *Science in culture*, 77.
[3] Cannon, *Science in culture*, 104.

measured study of widespread but interconnected real phenomena in order to find a definite law and a dynamical cause.'[1]

Despite Cannon's clarifications, Humboldtian science has been criticised by Secord for the 'vague generalities' it has introduced and latterly by Schaffer, who refers to it as a 'vague notion' that has allowed historians to categorise all sorts of pursuits as 'parts of a newly fashioned global physics'.[2] Dettelbach has in part tried to rescue the term from such criticism, and paint it as a means to demarcate the 'reorganisation of knowledge and disciplines in the early nineteenth century that defined the emergence of natural science out of natural philosophy', but even he recognised that there has been 'little unity to the collection of observational and descriptive concerns that in Anglo-American historiography goes under the name of "Humboldtian Science" … only an encyclopedic dedication to the systematic and precise measurement of as many physical parameters as possible'.[3]

It is true that the British magnetic scheme was partly prompted by a request of Humboldt's contained in a letter to the Royal Society in 1836 and it is also true that the researches of Humboldt, particularly his system of Russian observatories, were an inspiration for the architects of the British scheme. But, it is perhaps more appropriate to view the British magnetic scheme under the rubric of survey science, rather than Humboldtian science. As will be demonstrated in what follows, the scheme was much more diverse and heterogenous than the application of the term "Humboldtian science" allows. To call the magnetic scheme an archetype of the Humboldtian sciences is to neatly belie the distinct set of practices, instruments, individuals, materials and data that interacted over dispersed geographical locations to form the scheme. Approached as one of a number of different nineteenth-century survey sciences, these distinctions are unearthed rather than subsumed under an overly general and homogenous title like Humboldtian science. While the scheme was literally and perhaps intellectually influenced by Alexander von Humboldt, the method by which the scheme was conducted should not be viewed under the umbrella of Cannon's creation. This argument will be returned to in the conclusion, and explored with recourse to the evidence presented in the intervening chapters. It will be suggested that the magnetic

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[1] Cannon, *Science in culture*, 105.

[2] Secord, J. A., 'The Geological Survey of Great Britain as a research school', *History of Science* xxiv (1986), 223-274, 264; Schaffer, S., 'Keeping the books at Parramatta Observatory', in Aubin, D., Bigg, C., and Sibum, H. O. (eds), *The heavens on earth: observatories and astronomy in nineteenth-century science and culture* (Duke University Press: Durham and London, 2010),118-147, 127.

[3] Dettelbach, M., 'Humboldtian Science', in Jardine, N., Secord, J. A., and Spary, E. C. (eds), *Cultures of Natural History* (Cambridge University Press: Cambridge, 1996), 287-304, 304; and Dettlebach, M., 'Alexander von Humboldt between Enlightenment and Romanticism', *Northeastern Naturalist* 8, Special Issue 1 (2001), 9-20, 10.

scheme may be better approached through the tools and definitions associated with the term "big science" and, as mentioned above, survey science. The way that scholars have approached historical instances of big science allows for a greater attention to the myriad and distinct processes that attended projects like the magnetic scheme.

The physical sciences in Britain in the nineteenth century

Development and change within Britain's magnetic community over the course of the eighteenth century is the long context in which the magnetic crusade should be situated. However, to explain the origins of the crusade, an understanding of how British science stood in the early part of the nineteenth century also needs to be arrived at. This was a time of considerable upheaval for the structure of science in Britain. Support for the reform of the governance of science in Britain was considerable and new specialities and disciplines were identified and, increasingly, codified and institutionalised. The British magnetic scheme was part of this general tumult, this reformist drive, at least to some extent. What follows is a brief outline of the position of British science in the early nineteenth century prior to the organisation of the British magnetic scheme and a discussion of how the scheme fits into the wider context of what has been called a revival of the physical sciences both in Britain and more widely in Europe.

Beginning in the 1820s, there were increasingly loud calls from individuals within Britain's scientific community for reform of the organisation of science in Britain.[1] The entrenched individualistic and privileged practice of science in Britain associated with the Royal Society under Joseph Banks (1778-1820) came under increasing pressure from figures such as Charles Babbage, James South and David Brewster. Banks was, Ashworth has written, 'the symbol of all that was wrong' not only with science but with the country. 'His administration', Ashworth continues, 'of aristocratic interests, secrecy, monopoly and patronage was the opposite of the meritocracy, private business ideology, accountability, and system of analysis' propagated by those reformists, such as Herschel, South and Francis Baily, who had founded the Astronomical Society of London in 1820.[2] The founding of the Astronomical Society was a result of the belief – in some quarters – that 'Britain's foremost scientific society, the Royal Society, needed major reform, not least

[1] Cardwell, D. S. L., *The organisation of science in England*, revised edition (Heinemann Educational: London, 1972); Ratcliff, J., *The transit of Venus enterprise in Victorian Britain* (Pickering & Chatto: London, 2008), 25-29.
[2] Ashworth, W. J., 'John Herschel, George Airy, and the roaming eye of the state', *Hist. Sci.* xxxvi (1998), 151-178, 154.

rejection of the practice of electing to membership … aristocrats who typically possessed only limited interests in and even less knowledge of science'.[1] More widely, reformers called for greater government involvement in the promotion of science through national institutions for scientific education and research. Ratcliff has characterised this as a 'prolonged push for the reform of science funding and education that began in the late 1820s' that would eventually result in the founding of such institutions as the BAAS, of which Morrell and Thackray and more recently Withers have provided fulsome histories.[2]

Bowler and Morus have shown how Babbage insisted in his polemical *Reflections on the decline of science in England* (1830) that if the subject of science 'were to be developed properly a scientific profession needed to be established composed of paid and properly funded researchers'.[3] That Sabine was the subject of some of Babbage's harshest rebukes is somewhat common knowledge. Sabine exemplified, in Babbage's estimation, all that was wrong about the conduct of science in Britain at that time. Babbage felt Sabine undeserving and unqualified for the patronage he received, accused Sabine of falsifying geodetic observations and of being subservient to the Admiralty.[4] However, Sabine was not wholly emblematic of the traditional wealthy elite control of science. As Ratcliff rightly points out, Sabine was rather a 'science worker by way of … practical military training'.[5] The clamour for reform arguably reached its apotheosis in 1830, when the reformists' candidate John Herschel was put forward for the presidency of the Royal Society. Although Herschel narrowly lost the leadership contest to the brother of George IV, the Duke of Sussex, and promptly made for the Cape of Good Hope to scrutinise various nebulous objects, the programme of the reformers could not now be ignored and it was 'eventually largely adopted', according to Crowe.[6] Perhaps ironically, one of the most significant indirect consequences of the reformist drive was the British magnetic scheme, headed in large part by Babbage's object of ire, Sabine. The British magnetic scheme was an example of state supported science that employed, educated and paid participants out of the public purse. The scheme was also coordinated through a combination of a reformed

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[1] Crowe, M. J., 'Herschel, Sir John Frederick William, first baronet (1792-1871), *Oxford Dictionary of National Biography* [ODNB], Oxford University Press [OUP], 2004, online edn, http://www.oxforddnb.com/view/10.1093/ref:odnb/9780198614128.001.0001/odnb-9780198614128-e-13101?rskey=KwmNtL&result=1 [accessed 19 February 2018].

[2] Ratcliff, J., *The transit of Venus enterprise in Victorian Britain* (Pickering & Chatto: London, 2008), 25; Morrell and Thackray, *Gentlemen of science*; Withers, C. W. J., *Geography and science in Britain, 1831-1939: a study of the British Association for the Advancement of Science* (Manchester University Press: Manchester, 2010).

[3] Bowler, P. J., and Morus, I. R., *Making modern science: a historical survey* (The University of Chicago Press: Chicago and London, 2005), 332.

[4] Ratcliff, *Transit of Venus*, 27.

[5] Ratcliff, *Transit of Venus*, 27.

[6] Crowe, 'Herschel' *ODNB*.

Royal Society and the BAAS. As Miller points out, 'the promotion of geomagnetic research … went hand in hand with advocacy of institutional change'.[1]

It ought to be noted that the adoption of the reformist agenda was not wholesale, at least not immediately. As Ashworth explains, in the production of authoritative and credible science 'gentlemanly attributes and the right character greatly mattered' and played a 'prominent role' in how knowledge claims were weighed, especially as those claims became more specialised through the nineteenth century.[2] Furthermore, as James Secord states, 'British science remained primarily voluntarist throughout the first half of the nineteenth century'.[3] Science as a paid profession was not truly established until after mid-century. The pursuit of science before such time therefore came with a pecuniary obstacle that not all could overcome. There were some paid positions in the early nineteenth century. Secord identifies 'the paid professionals of natural history', usually fossil identifiers and curators and a few university chairs in science, but these positions tended to yield 'but a pittance and held little appeal'.[4] Science was still 'dominated' by gentlemen amateurs in the early stages of the nineteenth century, but their position as the elite members of the scientific community was increasingly destabilised as the century progressed.[5]

The establishment of the British magnetic scheme was also precipitated by another of the most important developments created by conflicts between traditional and reformist views that arose in the wake of Banks's death in 1820. This was, as Bowler and Morus put it, the 'emergence of specialist societies and journals that were dedicated to the interests of those with common research interests'.[6] In other words, the expansion of research in, and the institutionalisation of, various scientific disciplines, such as geology, meteorology, geography, chemistry, astronomy, geodesy, oceanography and geomagnetism. For Miller, this period of change was particularly vital to the 'transformation of the map of human knowledge by which the category of "physics" gradually replaced that of "natural philosophy" or "mechanical philosophy"', scientific typologies that had been hegemonic since at least the Renaissance in Britain.[7] Buchwald and Hong have similarly argued that physics 'as a separate discipline with distinctive methods – exact, quantitative, and

[1] Miller, D. P., 'The revival of the physical sciences in Britain, 1815-1840', *Osiris* 2 (1986), 107-134.
[2] Ashworth, W. J., 'Commentary: expertise and authority in the Royal Navy, 1800-1945', *Journal for Maritime Research* 16, 1 (2014), 103-116, 104.
[3] Secord, J. A., 'The Geological Survey of Great Britain, 226.
[4] Secord, 'The Geological Survey of Great Britain', 225.
[5] Bowler and Morus, *Making modern science*, 333.
[6] Bowler and Morus, *Making modern science*, 333.
[7] Miller, 'The revival of the physical sciences', 132; see also, Miller, D. P., 'The Royal Society of London 1800-1835: a study in the cultural politics of scientific organization', PhD thesis, University of Pennsylvania (1981).

experimental – can be reasonably well discerned by the end of the first third of the nineteenth century', marking the transformation from eighteenth-century natural philosophy.[1] For Miller, this transformation can be seen in the emergence of networks of mathematical practitioners, Cambridge scholars and scientific servicemen who all, consciously or not, pushed in a 'common direction', and were active in the reform movement of the 1820s and the direction of physics research from the 1830s onwards.[2]

The Cambridge network, including notable figures such as Herschel, George Peacock and Babbage, were particularly influential in changing the landscape of physical research, as has already been noted, but it was the mathematical practitioners and scientific servicemen who created a new methodology by which these new research endeavours operated. Scientific servicemen, members of the armed services – the Navy, the Royal Artillery and Royal Engineers – whose 'crucial training grounds included the Ordnance Surveys of Great Britain and Ireland, the Great Trigonometrical Survey of India, and the military educational establishments where many studied under the leading practitioners' led the way in the collection of physical data.[3] Mathematical practitioners are classified by Miller as groups of individuals based either in the Royal Military Academy or the Royal Military College, or of belonging to London's commercial middle class – 'merchants, city men, stockbrokers, life insurance entrepreneurs and the like.'[4] These were the individuals who appraised and attempted to elucidate hypotheses or laws from collected physical data. In Miller's words, these practitioners advocated the role of mathematics in scientific investigation and presented to the public 'an image of the physical sciences in which mathematics was the unifying language and the degree of mathematization of a science the major indicator of its maturity'.[5] The British magnetic scheme was an excellent example of this newly mathematised scientific practice and the revival of the physical sciences in general.

Miller's work is important in several respects for the thesis that follows, but perhaps its greatest contribution resides in its emphasis on the 'very significant' yet 'largely unsung' role of scientific servicemen in British science in the late eighteenth and early nineteenth centuries, a subject that receives attention throughout this thesis.[6] Two decades after Miller's study, these servicemen remained neglected, having 'only a shadowy presence in

[1] Buchwald, J. Z., and Hong, S., 'Physics', in Cahan, D. (ed.), *From natural philosophy to the sciences: writing the history of nineteenth-century science* (The University of Chicago Press: Chicago and London, 2003), 163-195, 165.
[2] Miller, 'The revival of the physical sciences', 108-109.
[3] Miller, 'The revival of the physical sciences', 112.
[4] Miller, 'The revival of the physical sciences', 108.
[5] Miller, 'The revival of the physical sciences', 134.
[6] Miller, 'The revival of the physical sciences', 112.

the … historiography of Victorian scientific culture' up to 2008, according to Ratcliff.[1] However, attention has been paid in some quarters, much of it quite recently. Ashworth, for instance, has sought in part to shine a light on this perceived historiographical darkness by consciously following Miller's example of demonstrating the 'extremely strong' connections between the military and the nascent Astronomical Society of London in the first half of the nineteenth century as well as the use of artillerymen in cartographic enterprises.[2] Naylor has discussed the role of the Admiralty and naval officers in early nineteenth-century meteorological observation.[3] Cock has likewise examined the 'corps of technical experts' that the British had developed within its armies by the end of the eighteenth century, a 'latent resource which could be harnessed to undertake the multi-disciplinary regional surveys which were characteristic of the Humboldtian approach'.[4] Cock argues that the Navy went further than simply training and deploying scientific servicemen, and was in fact one of the first institutions through which individuals from different groups – civilians and officers – could forge a scientific career and a living for themselves. Widmalm has recently looked at the mobilisation of military personnel in Swedish physical astronomy that resulted from 'a perceived need to systematically collect and evaluate information thought necessary for conducting modern warfare as practised by Napoleon I'.[5] One of the purposes of Macdonald's recent exploration of the political and scientific manoeuvrings that transformed Kew Observatory from a space of astronomical observation to one that accommodated a range of geophysical observations was to illuminate 'the role of the military in securing patronage for, and organizing, science' in the 1830s and 1840s.[6] Finally, Dunn has also recently discussed the (quite literal) place of scientific servicemen aboard voyages of exploration in the early nineteenth century.[7] In conjunction with these works, it is the purpose of this thesis to help illuminate some of the roles played by scientific servicemen in expeditionary contexts – such as Lefroy, of the Royal Artillery, during his North American Magnetic Survey – as well as in the

[1] Ratcliff, *Transit of Venus*, 27.
[2] Ashworth, 'John Herschel', 156, 166-169.
[3] Naylor, S., 'Log books and the laws of storms: maritime meteorology and the British Admiralty in the nineteenth century', *Isis* 106, 4 (2015), 771-797; Naylor, S., 'Weather instruments all at sea: meteorology and the Royal Navy in the nineteenth century', in MacDonald, F., and Withers, C. W. J. (eds), *Geography, technology and instruments of exploration* (Ashgate Publishing: Farnham, 2015), 77-96.
[4] Cock, R., 'Scientific servicemen in the Royal Navy and the professionalisation of science', 1816-55', in Knight, D. M., and Eddy, M. D. (eds), *Science and beliefs: from natural philosophy to natural science, 1700-1900* (Ashgate Publishing: Aldershot, 2005), 95-111, 99.
[5] Widmalm, S., 'Astronomy as military science: the case of Sweden, ca. 1800-1850', in Aubin, D., Bigg, C., and Sibum, H. O. (eds), *The heavens on earth: observatories and astronomy in nineteenth-century science and culture* (Duke University Press: Durham and London, 2010), 174-198, 176.
[6] Macdonald, L. T., 'Making Kew Observatory: the Royal Society, the British Association and the politics of early Victorian science', *Brit. J. Hist. Sci*. 48, 3 (2015), 409-433, 410.
[7] Dunn, R., '"Their Brains Over-Taxed": ships, instruments and users', in Leggett, D., and Dunn, R. (eds), *Re-inventing the ship: science, technology and the maritime world, 1800-1918* (Ashgate: Farnham, 2012), 131-155.

observatory and at the British magnetic scheme's data collection point in Woolwich. The experience of these officers is informative in several areas, from observatory and expeditionary management, to scientific pedagogy and the military bureaucracy through which the magnetic scheme was organised.

Geomagnetic research on the continent

In 1840, John Herschel was entrusted with writing an article to explain the structure and scientific necessity of the British magnetic scheme, launched at the end of 1839. Although he mildly begrudged the task, Herschel wrote elegantly of 'the moving magnetic panorama' of the globe and of nascent attempts to map terrestrial magnetic phenomena.[1] The British magnetic scheme intended to provide the masses of observations required to inform theoretical scrutiny of the earth's restless magnetic field. Such a 'philosophical theory', Herschel wrote, 'does not shoot up like the tall and spiry pine in graceful and unencumbered natural growth, but, like a column built by men, ascends amid extraneous apparatus and shapeless masses of materials'. Continuing with the metaphor of construction, Herschel explained how they were presently 'busied in building and pulling down, casting and recasting our design, piecing together our scaffolding, and securing our foundations for a far greater and more massive edifice' in which to investigate terrestrial magnetism.[2] Herschel went on to narrate a short history of terrestrial magnetic research in Britain – a subject discussed above – and explained in more detail certain aspects of the British magnetic scheme. For example, Herschel noted the role of Lloyd in the design and construction of an 'elegant apparatus' for the measurement of the vertical component of the magnetic field – a component that had not previously been measured – and the same individual's 'geometrical determination of the conditions … under which the instruments or magnetometers … can co-exist in one apartment of moderate dimensions', a design that made savings in materials and labour.[3] However, Herschel did not focus solely on the British role in terrestrial magnetic science. He also paid homage to 'the illustrious Humboldt', Gauss, Weber, Arago, Hansteen and others in Europe to whom so much was owed in the development of geomagnetic research.[4]

Several scholars have sought to unpack and emphasise just how important continental physical research was at this time. Good, for example, argues that continental research

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[1] Herschel, J., 'Terrestrial magnetism', *Quarterly Review* 66, 131 (1840), 271-312, 274, 279.
[2] Herschel, J., 'Terrestrial magnetism', 273.
[3] Herschel, J., 'Terrestrial magnetism', 292.
[4] Herschel, J., 'Terrestrial magnetism', 288.

during the early nineteenth century constituted a profound 'shift in terrestrial magnetic studies' that informed and influenced the formation of the British system.[1] Good neatly summarises this shift:

> First, Alexander von Humboldt (1769-1859) organized a network of collaborative simultaneous measurement of Earth's magnetic variables across Europe and Asia, starting in the 1820s. Second, Christopher Hansteen (1784-1873) proposed a flawed but influential theory involving four magnetic poles. Third, Carl Friedrich Gauss (1777-1855) realized a particular way of reducing magnetic force to 'mechanical', 'absolute' units. This innovation opened the door to a general standardization of electrical and magnetic measurements and their connection to dynamics, it improved the comparison of measurements made in the increasingly far-flung magnetic observatory network, and it contributed to a more critical evaluation of data.[2]

Gauss, in his magnum opus *Allgemeine Theorie des Erdmagnetismus* (1839), also provided a new means of analysing magnetic data that would have a profound impact on the study of geomagnetism and of the creation of models of the earth's magnetic field: spherical harmonics. Spherical harmonic analysis was a method developed by Gauss that allowed him to describe the earth's magnetic field everywhere on the surface of the earth with only a finite number of observations. It is, Merrill explains, 'a generalized type of Fourier analysis applied to a spherical (Riemannian) geometry' that provides 'a quantitative way to extrapolate between measurements'.[3] This last point is important because it is what allowed Gauss to distinguish between internal and external magnetic field sources, a mini-revolution in the study of geomagnetism. Gauss's method was remarkable in its time and is still 'essentially the one used today' in geomagnetic field modelling, according to Merrill.[4] O'Hara and latterly Good have produced significant accounts of Gauss's theoretical contributions to the study of geomagnetism, while O'Hara has elsewhere also emphasised Gauss and Wilhelm Weber's contributions to the field of geomagnetic instrumentation, innovations that allowed for 'observations to be made with a precision previously attained only in astronomy'.[5] Josefowicz has also presented an illuminating account of an exchange

[1] Good, G. A., 'Between data, mathematical analysis and physical theory: research on Earth's magnetism in the 19th century', *Centaurus* 50 (2008), 290-304, 290.
[2] Good, 'Between data', 290.
[3] Merrill, *Our Magnetic Earth*, 26.
[4] Merrill, *Our Magnetic Earth*, 29.
[5] O'Hara, J. G., 'Gauss and the Royal Society: the reception of his ideas on magnetism in Britain (1832-1842)', *Notes and Rec. R. Soc. Lond.* 38, 1 (1983), 17-78, 29; O'Hara, 'Gauss's method for measuring the terrestrial magnetic force in absolute measure: its invention and introduction in geomagnetic research', *Centaurus* 27 (1984), 121-147. Good, 'Between data'; Good, 'Measuring the inaccessible earth: geomagnetism, in situ measurements, remote sensing, and proxy data', *Centaurus* 53 (2011), 176-189;

between Gauss and Herschel that, among other things, outlines Gauss's views on scientific pedagogy and how this departed from Herschel's perspective.[1]

Gauss was not the only theorist to have also contributed to instrument design and construction. Christopher Hansteen was similarly important in this arena and particularly so for Lloyd and Sabine. Hansteen's *Magnetismus der Erde* (1817) was a significant and influential text that posited a four-pole, or two-dipole, theory to explain the earth's magnetism, in like manner to Halley's supposition a century earlier. This theory was later disproved but at the time Hansteen had several adherents on the continent and in Britain. Sabine collaborated extensively with Hansteen in the early part of the nineteenth century through the exchange of magnetic needles, bars and data. Enebakk urged readers to consider the development and exchange of Hansteen's portable magnetometer over and above his theoretical contributions. Enebakk argues that Hansteen was able to establish an early instantiation of international collaboration in terrestrial magnetic study through the distribution of his portable magnetometer and the standardisation of the results of other observers with Hansteen's own remarkably stable magnetised cylinder made by Dollond. Thus, 'just as the metre prototype in Paris became the international standard for measuring length, Dollond's cylinder in Christiania became Hansteen's standard against which all observations of magnetic intensity had to be calibrated. The system's integrity rested on the needle's stability.'[2]

A number of commentators – O'Hara, Good, Cannon, Cawood, Carter and Smith chief among them – have also detailed the importance of the foundation of the Göttingen Magnetische Verein (Göttingen Magnetic Union) by Gauss and Weber in 1834. The Union joined magnetic observatories across Europe, from Munich to Naples to Vienna, under the same system of observation equipped with the same instrumentation, including Gauss's newly invented unifilar magnetometer, with Gauss and Weber's Göttingen Magnetic Observatory as the central coordinating body. The Union was not the first coordinated system of magnetic observatories. Humboldt had formerly established a similarly purposed network of observatories in Russia and northern Asia in 1829, under the control of the Russian Academy of Sciences in St. Petersburg, which demarcated for the first time the three basic measurements to be made at fixed magnetic stations: declination, inclination,

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Garland, G. D., 'The contributions of Carl Friedrich Gauss to geomagnetism', *Historia Mathematica* 6 (1979), 5-29.

[1] Josefowicz, D. C., 'Experience, pedagogy, and the study of terrestrial magnetism', *Perspectives on Science* 13, 4 (2005), 452-494.

[2] Enebakk, V., 'Hansteen's magnetometer and the origin of the magnetic crusade', *Brit. J. Hist. Sci.* 47 (2014), 587-608, 594.

and intensity.[1] Prior to the construction of the Göttingen Magnetic Observatory as the heart of European magnetic observation, Arago had established the Paris Observatory at the forefront of geomagnetic investigation. It was here, between 1817 and 1835, that Arago greatly increased the volume of geomagnetic observations, and it was also, latterly, the space to which new constructions of magnetic instruments were brought and tested for their precision before being sent to different corners of Europe.[2] Arguably, it was Humboldt's stay in Paris and his collaboration with Arago in the 1820s that revealed to Humboldt the need for a greater, more systematic, scheme of geomagnetic observation to complement and extend the value of the series made at the Paris Observatory. Arago's work at the Paris Observatory and Humboldt and latterly Gauss and Weber's coordinated observing schemes and theoretical and instrumental researches contributed to what Locher has called a 'profound renewal … in the earth sciences in the 1830s and 1840s' on the continent.[3] In turn, this stimulated 'much of the impetus toward coordinated, organized investigations of geomagnetism in Britain,' Miller states, because it created 'a perception of British backwardness compared with Continental achievements'.[4] However, an unflattering comparison with European achievement was not the only cause of a revival of the physical sciences in Britain in the first quarter of the nineteenth century. As we have seen, changes to the formation and organisation of science in Britain were equally crucial in creating the context in which British geophysical researchers could thrive, collaborate with and extend the research of their continental counterparts.

Section II: Geographies of science

The genesis of the historical geography of science

To be able to argue that the production of terrestrial magnetic knowledge in the early nineteenth century was contingent on a number of different geographies, and the fact that

[1] Kellner, L., 'Alexander von Humboldt and the organization of international collaboration in geophysical research', *Contemporary Physics* I (1959-1960), 35-48; Smith, J. A., 'Humboldt, Sabine and the "Magnetic Crusade:" the founding of the Toronto Observatory', a research report completed under contract to the Canadian Museum of Science and Technology (1989), 1-74, 7; Lloyd, H., *Account of the magnetical observatory of Dublin, and of the instruments and methods of observation employed there* (University of Dublin Press: Dublin, 1842); Herschel, 'Terrestrial magnetism'.
[2] Cawood, 'Terrestrial magnetism', 576-578.
[3] Locher, F. 'The observatory, the land-based ship and the crusades: earth Sciences in European context, 1830-50', *Brit. J. Hist. Sci.* 40, 4 (2007), 491-504, 491.
[4] Miller, D. P., 'The revival of the physical sciences in Britain, 1815-1840', *Osiris* 2nd Series 2 (1986), 107-134, 128-129.

there exists a rich literature in which to situate such a historical geography, is due to the long process by which a relatively small enclave of sociologists and historians of science, working in the 1970s and 1980s, introduced and normalised a constructivist model of science. For, it was the instigation of a constructivist epistemology in science studies that provided the perspective and the toolkit by which geographies of science could be constructed. Today, constructivism and the sociology of scientific knowledge may be, as Golinski wrote in the preface to the second edition of *Making natural knowledge* (2005), 'less visible' and may have lost 'some of the bloom of its early promise' but it remains important because it 'still informs much historical scholarship at the level of tacit assumptions'.[1] I am certain that in this thesis there are a multitude of such tacit assumptions. Therefore, what follows is a brief history of the emergence of constructivism in the history of science, a description of some of the relevant (for the purpose of this thesis) sociological positions, and a mapping of the way in which historical geographers of science adopted, developed and applied these ideas in their subject area.

While there emerged quite different, sometimes conflicting, methodological approaches to applying the constructivist model, the essential tenet of this school of thought was that 'scientific knowledge is a human creation, made with available material and cultural resources rather than simply the revelation of a natural order that is pre-given and independent of human action'.[2] This was a naturalistic approach to science, which treated science as a craft and scientists as highly-skilled craftspeople as capable of being studied as Amazonian tribespeople.[3] The point was not 'to judge what was or was not "scientific," but to attend to actual practices as they manifested themselves in particular settings'.[4] The unwitting, certainly unintended, catalyst for this change was Thomas Kuhn's *The Structure of Scientific Revolutions* (1962), which Golinski called the 'harbinger of the constructivist movement'.[5] One of the reasons for this was Kuhn's now-familiar paradigmatic explanation of the way science worked. Kuhn posited that scientific knowledge started in confusion, from which would eventually emerge model problems and solutions and a period of relative coherence. This was the first paradigm, in which science was mature and normal and could be directed to solve certain problems. However, this normalcy could be

[1] Golinski, J., *Making natural knowledge: constructivism and the history of science*, New edition (The University of Chicago Press: Chicago, 2005), Preface, xi.
[2] Golinski, J., *Making natural knowledge: constructivism and the history of science* (Cambridge University Press: Cambridge, 1998), 6.
[3] Ravetz, J., *Scientific knowledge and its social problems* (Clarendon Press: Oxford, 1971); Latour, B., *Science in action: how to follow scientists and engineers through society* (Harvard University Press: Cambridge, MA, 1987); Latour, B., 'Drawing things together', in Lynch, M., and Woolgar, S. (eds) *Representation in scientific practice* (The MIT Press: Cambridge, MA, 1990), 19-68, 19-23.
[4] Golinski, *Making natural knowledge*, new edition, preface, viii.
[5] Golinski, *Making natural knowledge*, 4; Kuhn, T., *The structure of scientific revolutions*, 2nd edition (The University of Chicago Press: Chicago, 1970).

riven by a crisis 'when the accumulation of anomalies … would obstruct attempts to continue applying the paradigm'.[1] This, in Kuhn's estimation, led to revolution and, subsequently, the ushering in of a new paradigm able to solve the anomalies and restore the scientific community to normal governance again.

The Strong Programme, originally propagated by Bloor, Barnes, Collins, MacKenzie and Henry at the University of Edinburgh in the 1970s and 1980s, was particularly influenced by Kuhn's work because it provided the means by which to build a constructivist account of scientific knowledge. Those like Bloor and Barnes interpreted Kuhn's paradigms as models because such an understanding pointed towards 'a pragmatic alternative to the traditional philosophical view that science is governed by a logical structure of theory' and presented science 'as an enterprise of practical reasoning governed by accepted conventions rather than by logical deduction from some theoretical structure'.[2] Such an interpretation allowed the Edinburgh school to scrutinise not only the organisation but the content of scientific knowledge in sociological terms and led to the development of the Strong Programme in the sociology of knowledge.[3] The four tenets of the Strong Programme, as articulated by Bloor, held that a sociology of scientific knowledge would be: causal, and so 'concerned with the conditions which bring about beliefs or states of knowledge'; impartial, by which both sides of the true/false, rational/irrational, dichotomies would be treated impartially and require explanation; symmetrical, so 'the same types of cause would explain, say, true and false beliefs'; and reflexive, so that 'in principle its patterns of explanation would have to be applicable to sociology itself'.[4]

The sociology of scientific knowledge (SSK) and its Strong Programme sought to emphasise the work done by scientists, engineers and others involved in the scientific enterprise to construct their particular knowledges. In contradistinction to traditional Whig histories of science, SSK applied the same methodologies to true and false, rational and irrational claims. Whereas Whiggish histories of science explained true beliefs through internal and rational explanations and false claims through external, social or cultural explanations, SSK, by its symmetry postulate, explained both true and false beliefs using the same types of resources.[5] The application of the symmetry postulate allowed for all sorts of new histories of science. After SSK, historians explored rather than dismissed the discontinuities and moments of controversy in science's history and sought to explain why

[1] Golinski, *Making natural knowledge*, 14.
[2] Golinski, *Making natural knowledge*, 15.
[3] Sismondo, S., *An introduction to science and technology studies*, 2nd edition (Wiley-Blackwell: Chichester, 2010), 47.
[4] Bloor, D., *Knowledge and social imagery*, 2nd edition (The University of Chicago Press: Chicago, 1991), 5.
[5] Sismondo, *Introduction*, 48.

one belief triumphed over another, not because it or those who studied it had rationally arrived at some truth waiting there in the material world, but because of a whole host of specific social, cultural, political, technological or, latterly, geographic contexts that had allowed for the construction and acceptance of that particular scientific knowledge.[1]

SSK was not the only school of thought to arise under the broad banner of constructivism in the history of science. According to Golinski, by the 1980s the 'constellation of "science studies"' disciplines was heterogenous', although also 'riven with arguments'.[2] Actor-Network Theory (ANT) was one other school of thought that emerged in the late 1980s and which proved divisive, although similarly influential in the formation of early geographies of science. ANT is most closely associated with Bruno Latour, John Law and Michael Callon and posits that technoscience – i.e. science and technology – is the product of heterogenous networks made up of human and non-human actors that have formed associations as a result of shared interests. ANT, like SSK, deploys the same sociological means to analyse the 'nabobs of this world' and the 'wretched of the earth'.[3] Among other things, ANT is a materialist theory that postulates that science and technology work by 'translating material actions and forces from one form into another'.[4] Translation is a key tenet of ANT that seeks to problematise the relationship between nature and representations of nature, as evinced by Latour's now-familiar exploration of the practice of soil scientists in the Brazilian Amazon. Latour showed how these pedologists used colour charts to transform the soil samples they collected into a uniform code or usable inscription that could be reproduced and transported across the globe.[5] Science succeeded, in Latour's estimation, through making the world into a form that could be stabilised and mobilised into what he termed 'inscriptions' or 'immutable mobiles', a general term used to describe 'all the types of transformations through which an entity becomes materialized into a sign, an archive, a document, a piece of paper, a trace'.[6] For Latour then, the history of science was the history of mobilising objects from the field back to what he termed the

[1] Shapin, S., and Schaffer, S., *Leviathan and the air-pump: Hobbes, Boyle, and the experimental life* (Princeton University Press: Princeton, 2011 [orig. 1985]) and Rudwick, M. J., *The Great Devonian Controversy: the shaping of scientific knowledge among gentlemanly specialists* (The University of Chicago Press: Chicago, 1985).
[2] Golinski, *Making natural knowledge*, 5.
[3] Law, J., 'Notes on the theory of Actor-Network: ordering, strategy, and heterogeneity', *Systems Practice* 5, 4 (1992), 379-393, 380.
[4] Sismondo, *Introduction*, 82.
[5] Latour, B., *Pandora's Hope: essays on the reality of science studies* (Harvard University Press: Cambridge, MA, 1999).
[6] Latour, *Pandora's Hope*, 306-307; Cole, E. *Handle with care: historical geographies and difficult cultural legacies of egg-collecting*, PhD Thesis, University of Glasgow (2016), 44-45.

centre of calculation, from where comparisons with other objects, or reductions or further translations can occur.

Latourian inscriptions, the Strong Programme and SSK were all part of a broader process that enabled historians of science to approach their subject on a level footing and not with their gaze turned skyward at some free-floating and sublime phenomenon that existed beyond the cognition of anyone but those directly involved in it.[1] Studying science became about studying science as practice, rather than science as some inherently rational and removed other. In doing this, new points of focus were created. Technology, the hardware of science, became more than an ancillary aspect of the enterprise but an active agent inseparable from the means by which new scientific knowledges were advanced and other epistemologies discredited. Consequently, those who built, maintained and handled this hardware suddenly became more visible too.[2] Knowledge also became a situated phenomenon, contingent on all sorts of specific social and spatial contexts. The laboratory became an object of study just as much as the specimens and data that passed under its microscopes had been.[3] Scientific spaces were politicised, boundaries delineated and the local, that erstwhile 'locational pathology', transformed into a key component of 'the ontological status of scientific objects and the epistemological standing of scientific statements'.[4] These developments in the field of sociology and the history of science made it possible for Livingstone and others to identify a nascent but distinct new geography in the early 1990s, that of the geography of science. Not that geography of science proffered by Harold Dorn, which harked back, Livingstone wrote, to 'grand old environmentalist histories' and looked very much like an 'ecological constructivism', but a geography of science that probed 'the role of the spatial setting in the production of experimental knowledge, the significance of the uneven distribution of scientific information, the diffusion tracks along which scientific ideas and their associated instrumental gadgetry migrate, the management of laboratory space' and a myriad of other political, social and cultural spatial contexts.[5] All of these, in one form or another, are topics discussed in this thesis in the context of early nineteenth-century British magnetic research. Extended discussions of histories of scientific hardware and geographies of science will be presented in what follows.

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[1] Here, I am paraphrasing the first sentence of Ophir, A., and Shapin, S., 'The place of knowledge: a methodological survey', *Science in Context* 4, 1 (1991), 3-21.

[2] Shapin, S., 'The invisible technician', *American Scientist* 77, 6 (1989), 554-563.

[3] Latour, B. and Woolgar, S., *Laboratory life: the construction of scientific facts*, 2nd edition, (Princeton University Press: Princeton, 1986).

[4] Livingstone, D. N., 'The spaces of knowledge: contributions towards a historical geography of science', *Environment and Planning D: Society and Space* 13 (1995), 5-34, 15; Quotation from Ophir and Shapin, 'The place of knowledge', 5.

[5] Livingstone, 'The spaces of knowledge', 16.

Historical geographies of science

Nowadays, according to Jöns et al, the idea that science has a 'geography and that scientific knowledge bears the marks of particular locations have become … accepted facts'.[1] These particular locations, Livingstone's 'spaces of knowledge', are multifarious and can be analysed in relation to the production, circulation and reception of science.[2] While there is a rich literature devoted to each of these aspects in isolation, recent geographies of science have increasingly considered the production of science at a particular venue alongside the situated processes involved in the mobilisation, diffusion and reception of science: the long process through which science, locally generated in, between, and across a multitude of different sites and scales, achieved universality. This scholarship is part of, and was precipitated by, what Finngean calls the 'spatial turn', a concept for which there exist several thorough introductory surveys, most notably by Finnegan, Naylor, Powell, and Livingstone and Withers.[3] The scale of spatial analysis ranges from venues, sites and the field – broadly construed – to the scale of regions and/or territories: provincial, national and international.[4] Science is shaped at each of these levels. However, as Livingstone has noted, the relationship between science and place is not unidirectional.[5] Scientific practice is shaped by its regional setting but it also helps to define and organise that setting as well. Livingstone used the example of astronomical, cartographic and trigonometric surveying, practices that impose 'rational order on the seeming chaos of nature' that enabled and continues to enable governments to cohere, expand, tax, oppress and exploit their borders and the peoples within them.[6] In a more benign context, Naylor has written on the way in which the Penzance natural history society 'turned Cornishmen and women into natural historians, and Cornwall into a site of

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[1] Meusberger, P., Livingstone, D. N., Jöns, H., 'Interdisciplinary geographies of science', in Meusberger, P., Livingstone, D. N., Jöns, H. (eds), *Geographies of Science* (Springer: Dordrecht, Heidelberg, London, New York, 2010), ix-xvii, ix.

[2] Livingstone, D. N., 'The spaces of knowledge: contributions towards a historical geography of science', *Environment and Planning D: Society and Space* 13 (1995), 5-34.

[3] Finnegan, D. A., 'The spatial turn: geographical approaches in the history of science', *Journal of the History of Biology* 41 (2008), 369-388.; Naylor, S., 'Introduction: historical geographies of science – places, contexts, cartographies', *Brit. J. Hist. Sci.* 38, 1 (2005), 1-12; Powell, R. C., 'Geographies of science: histories, localities, practices, futures', *Progress in Human Geography* 31, 3 (2007), 309-329; Withers, C. W. J., and Livingstone, D. N., 'Thinking geographically about nineteenth-century science', in Livingstone, D. N., and Withers, C. W. J., *Geographies of nineteenth century science* (The University of Chicago Press: Chicago and London, 2011), 1-19; Livingstone, D. N, *Putting science in its place: geographies of scientific knowledge* (The University of Chicago Press: Chicago, 2003); Withers, C. W. J., 'Place and the 'spatial turn' in geography and history', *Journal for the History of Ideas* 70 (2009), 637-658.

[4] See the works listed in cit. 63, 67 and 68; on territories see Smith, C., and Agar, J. (eds), *Making space for science: territorial themes in the shaping of knowledge* (Macmillan: Basingstoke, 1998).

[5] Livingstone, D., 'Landscapes of knowledge', in Meusberger, P., Livingstone, D. N., Jöns, H. (eds), *Geographies of Science*, 3-22, 5.

[6] Livingstone, D., 'Landscapes of knowledge', 5.

civic pride and a scientifically delimited space … achieved through and across a range of sites, from the micro-geographies of museum display cabinets up to the imagined space of national scientific endeavour'.[1] Finnegan has produced a similar history at the national scale through his studies of natural history societies in Victorian Scotland and how local civic culture consciously fashioned itself through, and prided itself on, the local nature of its collections while eschewing any imitation of the 'global ambitions of metropolitan institutions'.[2]

There are abundant examples to confirm how the different venues in which and through which the scientific process occurred were not passive but, in the words of Finnegan, 'active ingredients'.[3] Outram, for example, has discussed the malleable space of the National d'Histoire Naturelle in Paris, and how the expansion of space allotted to George Cuvier's collections in the anatomy gallery, and the concomitant contraction of space for Cuvier's rival Jean-Baptiste Lamarck's collection, lent authority to Cuvier's ideas while damaging those of Lamarck.[4] Alberti has similarly looked at the space of the museum and its almost century-long standing as a site for the production of credible knowledge through, in part, the authority of the curator, from the mid-nineteenth to the mid-twentieth century, at which time 'field-based ethnography' became preferable to traditional 'collection-centred ethnology'.[5] The zoo, the garden, the ship, the hospital, the laboratory, the lecture hall, the pub, the country house and more besides – all of these sites have differently shaped the production of science.[6] Each of these sites provide what Finnegan describes as a 'definite geographical focus for discerning *in situ* the closely linked spatial and social character of scientific practice without ignoring wider channels of scientific exchange'. In

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[1] Naylor, S., 'The field, the museum and the lecture hall: the spaces of natural history in Victorian Cornwall', *Trans. Inst. Br. Geog.* 27, 4 (2002), 494-513, 509; For more on Naylor and the nineteenth century Cornish scientific milieu, see Naylor, S., 'Nationalizing provincial weather: meteorology in nineteenth-century Cornwall', *Brit. J. Hist. Sci.* 39, 3 (2006), 407-433; and Naylor, S., *Regionalizing science: placing knowledges in Victorian England* (Pickering & Chatto: London, 2010).

[2] Finnegan, D. A., 'Natural history societies in late Victorian Scotland and the pursuit of local civic science', *Brit. J. Hist. Sci.* 38, 1 (2005), 53-72, 64; see also, Finnegan, D. A., *Natural history societies and civic culture in Victorian Scotland* (Pickering & Chatto: London, 2009); Withers, C. W. J., and Finnegan, D. A., 'Natural history societies, fieldwork and local knowledge in nineteenth-century Scotland: towards a historical geography of civic science', *Cultural Geographies* 10, 3 (2003), 334-353.

[3] Finnegan, 'The spatial turn', 371.

[4] Outram, D., 'New spaces in natural history', in Jardine, N., Secord, J. A., and Spary, E. C. (eds), *Cultures of Natural History* (Cambridge University Press: Cambridge, 1996).

[5] Alberti, S., 'The status of museums: authority, identity, and material culture', in Livingstone, D., and Withers, C. W. J. (eds), *Geographies of Nineteenth-Century Science* (University of Chicago Press: Chicago, 2011), 51-72, 65.

[6] For familiar examples, see Schaffer, S., 'Physics laboratories and the Victorian country house', in Agar and Smith (eds), *Making space for science*, 149-180; Endersby, J., 'A garden enclosed: botanical barter in Sydney, 1818-39', *Brit. J. Hist. Sci.* 33 (2000), 313-334; Secord, A., 'Science in the pub: artisan botanists in early nineteenth-century Lancashire', *Hist. Sci.* xxxii (1994), 269-315; Stewart, L., 'Other centres of calculation, or, where the Royal Society didn't count: commerce, coffeehouses and natural philosophy in early modern London', *Brit. J. Hist. Sci.* 32 (1999), 133-153. The ship is explored later in this review.

this thesis, the most prominent sites of scientific production are the 'amorphous field' and the observatory, specifically in their nineteenth-century constructions.[1]

In the common imagination, as Aubin, Bigg and Sibum have written, the nineteenth-century observatory conjures up 'images of a neoclassical monument surrounded by delightful gardens, a makeshift camp on a desolate beach, a wooden shack on a university campus, or a refuge on an icy mountaintop'.[2] Superficially, an observatory was any one of these or other imaginings. An observatory was also purportedly a carefully delineated and guarded site in which the observer could work unheeded and undisturbed by unwanted natural and man-made "noise". In this sense, an observatory aimed at a complete triumph over nature and the situation of its location to become the universal view from nowhere. Observatory construction was also a vital aspect of a nation's imperial expansion and self-conscious fashioning as a "civilized" state: it was where colonial expeditions – cartographic, trigonometrical, geodetic, astronomical, geomagnetic and other – were planned and from which such operations were coordinated.[3] McAleer has similarly written about the role observatories played as nodes of the British Empire in the nineteenth century that reinforced a colonial narrative that the British did science while native peoples did not.[4] The construction of the colonial observatories as part of the British magnetic scheme was certainly part of a British imperialist agenda, as Carter has explained.[5]

McAleer has also pointed towards two of the more seemingly mundane but crucial articulations of the observatory abroad: as storehouses for important and expensive instruments from Britain and Europe, and consequently as sites of instrument provision within new networks of scientific endeavour. For example, the Cape Astronomical Observatory 'frequently supplied instruments to expeditions travelling into the African interior' in the nineteenth century.[6] From a similar perspective and using the history of Greenwich Observatory, Higgitt has written about the importance of considering not only the working instruments within an observatory but those 'small instruments, instruments not currently in use, records and publications' that all 'needed to be stored' there too.

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[1] Amorphous field is a term found in Daston, L., 'Introduction: third nature', in Daston, L., (ed.), *Science in the archives: pasts, presents, futures* (The University of Chicago Press: Chicago and London, 2017), 1-16, 2.
[2] Aubin, D., Bigg, C., and Sibum, H. O., 'Introduction: observatory techniques in nineteenth-century science and society', in Aubin, D., Bigg, C., and Sibum, H. O. (eds), *The heavens on earth: observatories and astronomy in nineteenth-century science and culture* (Duke University Press: Durham and London, 2010), 1-32, 1.
[3] Edney, M., *Mapping an Empire: the geographical construction of British India, 1765-1843* (The University of Chicago Press: Chicago and London, 1997).
[4] McAleer, J., '"Stargazers at the world's end": telescopes, observatories and 'views' of empire in the nineteenth-century British Empire', *Brit. J. Hist. Sci.* 46, 3 (2013), 389-413; and Edney, *Mapping an Empire*, 32.
[5] Carter, 'Magnetic fever'.
[6] McAleer, '"Stargazers at the world's end"', 406.

Alongside the instruments, space was also required for the observatory's staff 'to work, eat and sleep'. As such, 'rooms or separate structures that housed portable instruments, books and manuscripts, offices and bedrooms, equipment for carpenters and gardeners, and so on, all appeared on the increasingly crowded site'.[1] Thus, Higgitt demonstrates how observatories could be made up of a complex of buildings rather than just a single main building and could provide space for domestic as well as scientific needs. On the theme of British observatories, Macdonald has also demonstrated how the observatory could become a site of contestation in the politics of Victorian science.[2]

Several of the descriptions and definitions of the observatory related above are in some way predicated on the built environment of the observatory and, while these are legitimate ways of viewing the observatory, there is another perspective from which the observatory can be defined that usefully incorporates its frequently disordered reality. This definition is articulated by Aubin, Bigg and Sibum in their recent and influential volume on the observatory. According to the authors, the observatory can be defined by the different techniques employed within it and the 'practices required to perform successfully at the telescope eyepiece: the calibration, manipulation, and coordination of precision instruments for making observations and taking measurements'.[3] Techniques included 'data acquisition, reduction, tabulation, and conservation, along with complex mathematical analyses (error analysis and celestial mechanics)' as well techniques for 'producing maps, drawings and photographs' and 'material, numerical, and textual – indeed poetic – representations of the heavens and the earth'. Finally, these techniques also 'incorporated the social management of personnel within the observatory as well as international collaborations'. Such techniques were 'developed inside and outside observatories – by instrument makers in their workshops, navy officers on ships, civil engineers in the field, and physicists in their cabinets. But in the observatory they were uniquely assembled. Thereby these techniques helped define a *space* of knowledge: the observatory'.[4]

It follows that the nineteenth-century observatory was realised through the relationships and networks created between instruments and their users, instruments and their makers, instruments and other instruments, and users and their scientific training, all within the specific locale in which the observatory was sited. These relationships could, and frequently were, disrupted in the nineteenth-century observatory. Instruments broke or fell

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[1] Higgitt, R., 'A British national observatory: the building of the New Physical Observatory at Greenwich, 1889-1898', *Brit. J. Hist. Sci.* 47, 4 (2014), 609-635, 613.
[2] Macdonald, 'Making Kew Observatory'.
[3] Aubin, Bigg, and Sibum, 'Introduction', 6.
[4] Aubin, Bigg, and Sibum, 'Introduction', 7.

out of adjustment. Staff transgressed and neglected their duties. Instruction and correspondence was delayed and subject to revision. White ants could eat away the foundations of your instruments' pedestals.[1] Naylor's work on the gradual formalisation of a culture of meteorological inquiry aboard Royal Naval and Hydrographic Office survey ships between the conclusion of the Napoleonic Wars and 1874 has provided evidence for the above. In it, Naylor demonstrates how the multitude of evidence describing the nonconformity of officers, non-standardisation of instruments, irregularity and error in recording observations problematises traditional conception of floating observatories.[2]As Schaffer has also quipped, 'staff and machinery had to behave, but did not'.[3] The artifice of keeping strict, accountant-style, ordered accounts and the reduction of observations were two techniques, Schaffer has written, that helped to disguise the 'messy artfulness' of the observatory and transformed celestial surveys 'into something like the uncanny vision of a single eye'.[4] A focus on the techniques and practices involved in the making of an observatory in the nineteenth century is part of what informs the chapter devoted to the colonial observatories as it provides the framework for understanding these spaces as fluid and contingent assemblages of a number of different human and non-human entities. Often, in the literature concerned with the British magnetic scheme's overseas observatories, their establishment is viewed as an uncomplicated and onetime configuration, which arguably belies the ongoing and frequently changing practices that characterised these spaces.

Terrestrial magnetic research was not only conducted in the observatory but in the field too, during surveys on both land and sea. Defining the field, just like defining the observatory, is difficult. Daston's use of the adjective 'amorphous' to describe the field is apt.[5] The field is not simply a place waiting there in nature but a space designated, marked and inhabited by the field scientist.[6] Kuklick and Kohler define the field through the practices, practitioners and craft skills that inhabit the space.[7] Lorimer and Spedding theorise the field as a spatial entity informed and defined by the 'meanings, intentions and

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[1] Elliot, C. M., 'Magnetic survey of the Eastern Archipelago', *Phil. Trans. R. Soc. Lond.* 141 (1851), 287-331, 322.
[2] Naylor, 'Weather instruments all at sea'.
[3] Schaffer, 'Keeping the books at Parramatta Observatory', 129.
[4] Schaffer, 'Keeping the books at Parramatta Observatory', 131, 120.
[5] Daston, 'Introduction: third nature', 2; for the contested nature of the field, see also Nikolaou, P., 'Archaeology, Empire and the field: exploring the ancient sites of Cyprus, 1865-1876', in Finnegan, D. A., and Wright, J. J. (eds), *Spaces of global knowledge: exhibition, encounter and exchange in an age of empire* (Ashgate: Farnham, 2015), 39-55.
[6] Withers and Finnegan, 'Natural history societies', 336.
[7] Kuklick, H. and Kohler, R., 'Introduction', in Kuklick, H. and Kohler, R. (eds), *Osiris* 2nd Series, Science in the Field, 11 (1996), 1-14, 2.

actions that realise field science'.[1] Moreover, both of the aforementioned joint papers view the field as a public domain, the borders of which 'cannot be rigorously guarded'.[2] The field has also been described in contrast to the study or the laboratory. 'Unlike the study or the laboratory', Driver writes, 'the field is by definition a more open space, constructed and inhabited by a wide range of people practising different kinds of observation'.[3] However, as demonstrated below with reference to comparisons between the observatory and the field, such a flat distinction can be problematic. For Withers and Finnegan, fieldwork and the display and articulation of results from the field are intimately bound things and ought not to be differently categorised, an idea that extends our understanding of the field as something not only made in situ, but made and remade through representations of it in books, articles, museums and natural history societies.[4] Arguments over the definition of the field – and where the field was to be found – are nothing new. Both Outram and Dritsas have written about the tensions between so-called geographers in the field and critical 'arm-chair' geographers in the early nineteenth century and differing interpretations of the field as something collected and transported home to be explored or as something that had to be travelled to and inhabited for science from it to be legitimated.[5]

Conceptual similarities between the field and the observatory arise. Both can be defined by the practices that took place within them and both, if we compare Withers and Finnegan on the field with Schaffer on keeping the books at the Parramatta Observatory, relied on the display of results to construct each space as a legitimate site for science. Furthermore, although observatories were superficially guarded spaces – they had walls and roofs, after all – the degree of dislocation that these walls offered could be minimal. For example, in the third chapter, I show that the colonial magnetic observatories learned to work with, and never truly triumphed over, the locale in which they were situated, despite the efforts of resident engineers and craftsmen. Observatories were also often opened up to the public gaze.[6] In the nineteenth century, conceptually speaking at least, the observatory and the field site were not dissimilar spatial constructions. One could argue that the field was a much more fluid concept than the observatory because field workers often moved through and studied at more than just a single field site but, again, if we consider the nineteenth-

[1] Lorimer and Spedding, 'Locating field science', 14.
[2] Kuklick and Kohler, 'Introduction', 4.
[3] Driver, F., 'Scientific exploration and the construction of geographical knowledge: *Hints to Travellers*', *Finnistera* 33, 65 (1998), 21-30, 22.
[4] Withers and Finnegan, 'Natural history societies', 336.
[5] Outram, 'New spaces'; Dritsas, L., 'From Lake Nyassa to Philadelphia: a geography of the Zambesi Expedition, 1858-64', *Brit. J. Hist. Sci.* 38, 1 (2005), 35-52.
[6] Levitt, T., '"I thought this might be of interest …": the observatory as public enterprise', in *The heavens on earth*, 285-304; and see also Savours, A., and McConnell, A., 'The history of the Rossbank Observatory, Tasmania', *Annals of Science* 39, 6 (1982), 527-564, 534.

century observatory as defined by a common set of techniques and performative practices predicated on the relationships between instruments, users, makers, site, instruction and pedagogy, then we must also consider the observatory as a similarly fluid construct. As I demonstrate throughout the colonial observatories chapter, all of these things, human and non-human, were liable to change and repair and readjustment, requiring concomitant change and repair and readjustment of the space – internal and external – of the observatory.

The circulation of scientific knowledge

This thesis not only relies upon and extends scholarship on the different sites and venues of science, but discusses the movement of different materials and people through and across sites. There is a rich literature in which to base such discussions, although the mobilisation of knowledge arose as a later concern in the history of science than the situation of knowledge did. James Secord, writing in 2004, suggested that histories of science had been too rigidly concerned with origins and producers and 'obsessed with novelty and the places in which novelty begins'.[1] Secord urged historians of science to consider science at all times as a 'form of communicative action' that paid particularly close attention to the circulation of material items and technologies: 'new accounts of the generic development of the field notebook, the experimental register, the museum catalogue, and other documents of practice, as bridging studies between specific passages of technical work and their wider settings'.[2] Almost a decade and a half later, scholarship on the mobilities of scientific knowledge is plentiful and this review can only offer a sample of what is an extensive literature.

The circulation of objects, specimens and instruments – both human and non-human – has been explored by scholars examining different sub-disciplines of science. On specimens, Dritsas considered how crucial travel was in the making of natural history networks and credible malacological science by following six freshwater mussel shells from their point of collection in Lake Nyassa, south-eastern Africa, through the different places and spaces in which knowledge of the specimens was mediated and eventually 'incorporated into Western scientific knowledge'.[3] Terrall later employed a similar methodology to track the movement of insect specimens and the different materials, ideas and practices that

[1] Secord, J. A., 'Knowledge in transit', *Isis* 95, 4 (2004), 654-672, 662.
[2] Secord, 'Knowledge in transit', 667.
[3] Dritsas, 'From Lake Nyassa to Philadelphia', 49-50.

circulated with them in the creation of natural-historical observations in Paris and the French provinces in the eighteenth century.[1] The circulation of scientific texts has produced a rich literature not least because, as Livingstone notes, the 'scientific enterprise is characterised by textual multiplicity': from manuals and instrument handbooks, to observatory and laboratory protocols and results, scientific knowledge travelled extensively in print.[2] Naylor has provided a succinct survey of 'texts and their travels and travails', as he termed it, and similarly robust bibliographies can be found in several works by Ogborn, Driver, Withers and Keighren.[3] Others have similarly considered geographies of scientific texts, and the book itself as, in Anne Secord's words, a 'performative space produced by the practices and actions of both writer and reader.'[4] Anne Secord, in attending to the appropriation of botanical texts and how botanists took on and adapted the guidance therein, extends 'our understandings of the spatial arrangements of the informal networks that characterized British nineteenth-century botany.'[5]

Particularly pertinent for this thesis is Ratcliff's recent study of the production of the *Trivandrum* [*Trevandrum*] *Magnetical Observations* (1874) and its distribution between the Indian subcontinent and Europe.[6] This publication listed the results of three decades worth of observations taken at the Trivandrum Observatory under the supervision of two of its directors, John Caldecott and John Allan Broun, and as part of the worldwide system of terrestrial magnetic research instigated by Sabine, Lloyd and others of the BAAS and Royal Society. Whereas, in the final chapter of this thesis, I discuss how Sabine and his magnetic department in Woolwich, England, accrued and reduced the manuscript data of a number of overseas observatories in order to make these datasets publishable and appropriate for global distribution, Ratcliff considers in detail the labour involved in the production of a single observatory's results and how the 'now-familiar story of growing centres of accumulation in the nineteenth-century sciences in Britain' was in fact

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[1] Terrall, M., 'Following insects around: tools and techniques of eighteenth-century natural history', *Brit. J. Hist. Sci.* 43, 4 (2010), 573-588.
[2] Livingstone, 'Landscapes of knowledge', 12.
[3] Naylor, S., 'Historical geography: knowledge, in place and on the move', *Prog. Hum. Geog.* 29, 5 (2005), 626-634, 626-628; see, for example, Ogborn, M. and Withers, C. W. J., 'Introduction: book geography, book history', in Ogborn M. and Withers, C. W. J. (eds), *Geographies of the Book* (Ashgate Publishing Limited: Farnham and Burlington, VT, 2010), 1-25; Keighren, I., Withers, C. W. J. and Bell, B., *Travels into Print: Exploration, Writing and Publishing with John Murrary, 1773-1859* (University of Chicago Press: Chicago, 2015); Driver, F., 'Scientific exploration and the construction of geographical knowledge: *Hints to Travellers*', *Finnistera* 33, 65 (1998), 21-30; see also Jardine, N., 'Books, texts, and the making of knowledge', in Frasca-Spada, M., and Jardine, N. (eds), *Books and the sciences in history* (Cambridge University Press: Cambridge, 2000), 393-407.
[4] Secord, A., 'Pressed into service: specimens, space, and seeing in botanical practice', in *Geographies of Nineteenth-Century Science*, 283-310, 283.
[5] Secord, 'Pressed into service', 284.
[6] Ratcliff, J., 'Travancore's magnetic crusade: geomagnetism and the geography of scientific production in a princely state', *Brit. J. Hist. Sci.* 49, 3 (2016), 325-352.

'coextensive with, and dependent in various ways upon, the participation of Asian political actors'.[1] As Ratcliff shows, something to which this thesis will also contribute, the publication and distribution of an observatory's data was critical to the construction of scientific and financial value in that institution. Ratcliff's study is also driven by attention to the data archive and a close scrutiny of the products of terrestrial magnetic research in a bid to critically engage with the model of centres and peripheries in the history of nineteenth-century physical research, something that also informs parts of this thesis. Knowledge also travelled through correspondence networks, which scholars such as Anne Secord have shown was vital to the formation and extension of civic and institutional science, on national and international scales.[2] Beyond the page, studies have also sought to reconstruct the manner in which information flowed in conversations shaped or facilitated by different environs. Alberti on conversaziones, the active participation and consumption of science by a Victorian public and how the 'public-at-large used and experienced natural knowledge' is one example, and James Secord on how scientific conversation became shop talk is another.[3] Both have depicted 'the rich array of venues in which scientific conversation took place' and the sorts of ideas, objects and people that were under discussion at different times.[4]

An emphasis on circulation in the history of science can serve other means. For Raj, the term "circulation" acts as a

> strong counterpoint to the unidirectionality of "diffusion" or even of "dissemination" or "transmission," of binaries such as metropolitan science/colonial science or center/periphery, which all imply a producer and an end user. "Circulation" suggests a more open flow – and especially the possibility of the mutations and reconfigurations coming back to the point of origin. Moreover, the circulatory perspective confers agency on all involved in the interactive processes of knowledge construction.[5]

Such a focus results in what Wade Chambers and Gillespie see as the replacement of a 'paradigm of cultural *deficit* … with a paradigm of cultural *difference*', through which the

[1] Ratcliff, J., 'Travancore's magnetic crusade', 330.
[2] Secord, A., 'Corresponding interests: artisans and gentlemen in nineteenth-century natural history', *Brit. J. Hist. Sci.* 27, 4 (1994), 383-408.
[3] Alberti, S. J. M. M., 'Conversaziones and the experience of science in Victorian England', *Journal of Victorian Culture* 8, 2 (2003), 208-230, 208; Secord, J., 'How scientific conversation became shop talk', in Fyfe, A., and Lightman, B. (eds), *Science in the marketplace* (The University of Chicago Press: Chicago, 2007), 23-59.
[4] Livingstone, 'Landscapes of knowledge', 15-16.
[5] Raj, K., 'Beyond postcolonialism … and postpositivism: circulation and the global history of science', *Isis* 104 (2013), 337-347, 344; See also the edition of the *British Journal for the History of Science* that showcased the papers presented at an international conference on 'Circulation and Locality in Early Modern Science', held in October 2007, introduced by Raj, *Brit. J. Hist. Sci.* 43, 4 (2010).

history of science has been 'progressively "decentred"'.[1] Science since the nineteenth century, by their argument, is better understood 'both metaphorically and actually' as a fully institutionalised 'polycentric communications network', which arguably represented a 'revolution in knowledge making more significant for both science and society than the theoretical advances of the seventeenth century traditionally known as the Scientific Revolution'.[2] Recently, consciously global historians of science have likewise tried to produce histories that treat non-Western populations as 'not simply the passive recipients' of knowledge from the centres of Western imperial power but as historical actors with agency through whose activities – medical, botanical, cartographic, cultural or other – 'knowledge of the world was constructed, communicated and contested'.[3] Such histories use a lexicon of terms such as 'contact zone, hybridity, go-betweens, networks and assemblages', to emphasise science as a communicative and non-binary activity.[4] I also employ some of this language in discussing the hybrid geographies through which Lefroy's North American Magnetic Survey passed and the indigenous knowledges and local technical and mechanical expertise that provided the routes of mobility for Lefroy, his party and, crucially, his instruments too.

The circulation of technologies through different spatial and temporal contexts has become such a significant focus for geographies of science that Davies believes the burgeoning literature constitutes what might be called a 'travelling turn'.[5] Naylor has likewise commentated on the increased production of object-led spatial histories, an output that has not diminished since the time of Naylor's article.[6] Although my thesis is largely concerned with the circulation of scientific instruments, historical geographies of science have not been so confined. There are compelling studies of the circulation of specimens, books, letters, models, museum collections and a host of material and human knowledges in a variety of different systems, several of which have already been cited. On the movement of scientific instruments, and in addition to those works identified below in the context of

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[1] Wade Chambers, D., and Gillespie, R., 'Locality in the history of science: colonial science, technoscience, and indigenous knowledge', *Osiris* 15, Nature and Empire: Science and the Colonial Enterprise (2000), 221-240, 222-223.
[2] Wade Chambers and Gillespie, 'Locality in the history of science', 223.
[3] Finnegan, D. A., and Wright, J. J., 'Introduction: placing knowledge in the nineteenth century', in Finnegan, D. A., and Wright, J. J. (eds), *Spaces of global knowledge: exhibition, encounter and exchange in an age of empire* (Ashgate: Farnham, 2015), 1-15, 4.
[4] Finnegan and Wright, 'Introduction', 7.
[5] Davies, G., 'Locating technoscience – the geographies of science and technology', http://www.ucl.ac.uk/sts/locating-technoscience/archive.htm, [originally accessed 21 November 2014].
[6] Naylor, S., 'Historical geography: geographies and historiographies', *Prog. Hum. Geog.* 32, 2 (2008), 265-274, particularly 270-271.

scientific surveying, there are a number of significant volumes.[1] This body of literature draws attention to the 'embodied practices of motion, appropriation and learning' on which the circulation of instruments is, and has been in the past, dependent. As is demonstrated in the chapters concerned with Lefroy's survey and with the colonial observatories, such a focus and the 'spatial imaginaries' involved with it 'collapse simplistic renderings of metropolitan/marginal or global/local to instead argue that practices of precision, value, and error are intertwined with bodies, objects, and cultures'.[2] In other words, the circulation of scientific instrumentation illuminates the hybrid geographies of Lefroy's North American Magnetic Survey and the different hands in which instruments were carried, repaired and maintained far from the traditional "centre".

Besides the specific literature pertaining to the history of the circulation of scientific artefacts, instruments, specimens, books and so on, there are a number of additional historical and cultural geographies that provide the means by which to explore instruments on the move. DeSilvey has identified four terms that historical and cultural geographers have adopted in the pursuit of their material stories, from 'object-biography' and 'spatio-temporal life', to 'social-spatial biography' and 'geobiography'.[3] The concept of geobiography, deployed in the chapter concerned with Lefroy's survey, describes the course of a life 'as it relates to the places lived' and understands objects, artefacts, scientific instruments and so on as 'a process rather than a stable entity, the 'provisional identity of which can depend in large part on 'where they are in their geobiography'.[4] In another context, Lorimer demonstrates how the use of geobiography can create a 'heightened spatial awareness' that arranges life experiences 'according to cardinal sites and pivotal places' through which one can 'lever a life open'.[5] As Spary similarly noted, 'geography is … central to thingness', as 'local uses may fragment the thing's meaning, dis-figure it', which means 'things can be said to possess meaning and value only in relation to their specific circumstances of use and interpretation'.[6] This thesis treats magnetic instruments with the geographical specificity that Spary and others have urged

[1] For example, Withers, C. W. J., and MacDonald, F., *Geography, technology and instruments of exploration* (Ashgate: Farnham, 2015); and Bourguet, M-N., Licoppe, C., and Sibum, H. O. (eds), *Instruments, travel and science: itineraries of precision from the seventeenth to the twentieth century* (Routledge: London, 2002).
[2] Powell, 'Geographies of science, 321.
[3] 'Socio-spatial biography' is a term found in Pike, A., 'Placing brands and branding: a socio-spatial biography of Newcastle Brown Ale', *Trans. Inst. Brit. Geog.* 36 (2011), 206-222; 'Spatio-temporal' is a term found in Hill, J., 'Travelling objects: the Wellcome Collection in Los Angeles, London and beyond', *Cultural Geographies* 13 (2006), 340-366.
[4] Karjalainen, P. T., 'On geobiography', in Sarapik, V., and Tüür, K. (eds), *Place and location: studies in environmental aesthetics and semiotics III*, (Estonian Literary Museum: Tallinn, 2003), 87-92, 87.
[5] Lorimer, H., 'Standards of beauty: considering the lives of A. W. Boucher', *GeoHumanities* (2015), 1-29, 5, 4.
[6] Spary, E., 'Same/difference?', *Journal of Historical Geography* 46 (2014), 110-112, 111.

through attention to the different spaces in which and through which magnetometers, dip circles and magnetic needles were mobilised.

In the preface to the 2011 edition of *Leviathan and the air-pump*, Shapin and Schaffer agree that the original edition of their work, published in 1985, may have helped to precipitate 'the study of scientific hardware' in the history of science, just as Hacking had predicted would happen.[1] In this work, Shapin and Schaffer demonstrate how the construction of experimental science in the seventeenth century was contingent on the integrity of a particular scientific instrument and in so doing helped to spark the beginning of what has retrospectively been dubbed a material turn in the history of science.[2] It ought to be noted that the air-pump did not create this change entirely in isolation: the pioneering investigations of Bennett were also significant in this respect.[3] The material turn was made more explicitly visible through the 1994 *Osiris* special edition: 'Instruments'. The purpose of this issue, Helden and Hankins explained, was to reclaim instruments' importance from the low esteem in which they had been placed by adherents to the 'extreme idealist epistemology' of Alexander Koyré.[4] Koyré's school of thought, popular in the 1940s and 1950s, argued that the history of science was the 'history of theory', and that 'experiment and measurement took place after the fact and were not of prime interest.'[5] At best, 'instruments were considered … "reified theories"'.[6] Running contrary to this, the *Osiris* special edition sought to highlight not only how scientific instruments have determined what can be done, but how 'they also determine to some extent what can be thought.'[7] This last idea had been at the heart of *Leviathan and the air-pump*.

The term "scientific instrument" is something of a misnomer for, as Warner has demonstrated, the term did not likely enter common parlance until the early to mid-nineteenth century.[8] Warner and others have also shown that the term 'scientist' was only

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[1] Shapin, S., and Schaffer, S., *Leviathan and the air-pump: Hobbes, Boyle, and the experimental life* (Princeton University Press: Princeton, 2011 edn, 1985 orig.), xxxiii; Hacking, I., 'Artificial phenomena', *Brit. J. Hist. Sci.* 24, 2 (1991), 235-241, 236.
[2] For use of the term 'material turn' as applied to the late 1980s and early 1990s, see Bennett, J. A., 'Early modern mathematical instruments', *Isis* 102, 4 (2011), 697-705, 698; and Taub, L., 'Introduction: reengaging with instruments', *Isis* 102, 4 (2011), 689-696, 690.
[3] See, for example, Bennett, J. A., 'Robert Hooke as mechanic and natural philosopher', *Notes and Rec. R. Soc.* 35 (1980), 33-48; Bennett, 'The mechanics' philosophy and the mechanical philosophy', *Hist. Sci.* 24 (1986), 1-28.
[4] van Helden, A., and Hankins, T. L., 'Introduction: instruments in the history of science', *Osiris* 2nd Series 9, 'Instruments' (1994), 1-6, 1.
[5] van Helden and Hankins, 'Introduction: instruments', 2.
[6] Bachelard, G., *Les Intuitions Atomistiques* (Boivin: Paris, 1933), 140, quoted in van Helden and Hankins, 'Introduction: instruments', 2.
[7] van Helden and Hankins, 'Introduction: instruments', 4.
[8] Warner, D. J., 'What is a scientific instrument, when did it become one, and why?', *Brit. J. Hist. Sci.* 23, 1 (1990), 83-93, 86.

first (formally) used by William Whewell in 1834.[1] Before such time, what we might now consider scientific instruments were termed "philosophical" instruments, and the scientist a "natural philosopher". However, the use of scientific instrument to describe a wide variety of differently constructed instruments is ubiquitous throughout history of science literature.[2] In this thesis, the term is also deployed to cover several different types of instruments, sometimes for the sake of readability but mainly because I often discuss scientific instruments more broadly as a category of analysis. For instance, there is a heavy emphasis on "scientific instruments on the move" or "scientific instruments in situ" and so on. But the use of appropriate terminology has allowed other historians to distinguish between instruments and chart their particular histories. There were philosophical, mathematical, optical, medical, and musical instruments each, by the seventeenth century, with their own peculiar collective identity. Examples of philosophical instruments include the compass, the pendulum clock, barometer and air pump, among others. Examples of mathematical instruments include 'astrolabes, sundials, quadrants, surveyors' theodolites, or gunners' sights and rules.'[3] The term "maker", like scientific instrument, introduces a level of obfuscation, given that, as Warner noted, it implies the craft of a single individual while in reality the maker was he 'who co-ordinated the activities of numerous anonymous craftsmen and women'.[4] For Bennett, the 'dominant instruments culture up to the end of the seventeenth century' was mathematical, not philosophical. It was both a commercial and scientific enterprise, identifiably distinct as an art and trade, although makers were not tied to any one guild. The development of this section of instrument making is particularly important, Bennett argued, because it was the leading mathematical instrument makers who 'became responsible for building the major instruments in the growing number of astronomical observatories' from the eighteenth century.[5]

Today, there exists an extensive scholarship devoted to investigations of all sorts of different scientific instruments. Even a brief scan through the bibliographies compiled by the Scientific Instrument Commission and the annual *Isis* 'Current Bibliography' will demonstrate this. In addition, since the 1994 *Osiris* special issue 'Instruments', there have been similar thematic issues published by other journals in 1995, 2007, 2009 and 2011.[6] These bibliographies and thematic issues are replete with hundreds of articles by

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[1] See also, Ross, S., 'Scientist: the story of a word', *Annals of Science* 18, 2 (1962), 65-85.
[2] Feld, J. V., 'What is scientific about a scientific instrument?', *Nuncius* 3 (1988), 3-26.
[3] Warner, 'What is a scientific instrument', 83; Bennett, 'Early modern mathematical instruments', 698.
[4] Warner, 'What is a scientific instrument', 86.
[5] Bennett, 'Early modern mathematical instruments', 705.
[6] 'Origins and evolution of collecting scientific instruments', *Journal for the History of Collections* 7, 2 (1995); 'Objects, texts and images in the history of science', *Studies in History and Philosophy of Science* 38, 2 (2007); 'On scientific instruments', *Studies in History and Philosophy of Science* 40, 4 (2009); and 'The history of scientific instruments', *Isis* 102, 4 (2011).

academics, curators and hobbyists on all manner of scientific instruments and associated objects, from the astrolabe to the lactometer, magic lanterns to burning lenses.[1]

However, what counts as "scientific hardware" is not always so manifestly obvious. For Winter, Victorian Liverpool's dockyard walls, and by extension the city itself, were 'literally turned … into an instrument' through the process by which ship captains' corrected the deviations of their ship's compass against compass markings inscribed on the dockyard walls.[2] Scientific instruments could also be much fleshier. For example, Raj has written an account of the way in which, between 1863 and 1885, fifteen 'native Indians – almost all small-time functionaries of the Great Trigonometrical Survey of India' were trained by British Captain Thomas George Montgomerie and transformed into 'intelligent instrument[s] of measure' through the regulation and counting of their stride.[3] This regulation allowed the pundits, as they were known, to record the distances between places they travelled to, in disguise, in territories outside of British control. The use of the term 'instrument' here is appropriate. Their legs were changed from tools, by which their bodies were carried, into instruments, disciplined and observable in what they were doing. Similarly, Schickore shows in her study of late eighteenth and nineteenth century microscopes and the changing methodologies behind their use that in this period the human eye lost its 'epistemological transparency' and was 'relocated in the realm of irregularity and factual contingency', where different types of microscope could also be found.[4] While Schickore stops short of calling the eye a scientific instrument, she demonstrates how, 'to control the unruly eye, new methodological measures were introduced, designed to separate perceptions of microscopic objects from the merely "subjective" visual phenomena produced by the eye itself', just as the microscope was likewise subjected to methodological and material critique at this time.[5]

Naturally, the material history of science extends beyond scientific instruments. One subject that has proved profitable and informative in the last decade has been the study of scientific models, a literature that is relevant to what follows as, in the colonial observatories chapter, the movement of certain magnetic instruments between the category

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[1] These bibliographies, up to 2004, can be found at http://scientific-instrument-commission.org/sic-resources/bibliography [accessed 22 February 2018] and further records can be found through https://data.isiscb.org/ a database derived from bibliographic citations in the *Isis* Bibliography of the History of Science with which the Scientific Instrument Commission merged their bibliographies.

[2] Winter, A., '"Compasses All Awry": the iron ship and the ambiguities of cultural authority in Victorian Britain', *Victorian Studies* 38, 1 (1994), 69-98, 89.

[3] Kapil Raj, 'When human travellers become instruments: the Indo-British exploration of Central Asia in the nineteenth century', in Bourguet, Licoppe and Sibum (eds), *Instruments, Travel and Science*, 158.

[4] Schickore, J., 'Ever-present impediments: exploring instruments and methods of microscopy', *Perspectives on Science* 9, 2 (2001), 126-146, 132-133, 134.

[5] Schickore, 'Ever-present impediments', 134.

of model and instrument, and the tangible and intangible traces this transition could leave, is considered in detail. Hopwood and de Chadarevian have defined models as 'strategic objects of knowledge' that allow for traditional investigations of the wider cultures of science, technology and medicine and, through study of 'their making, distribution, and display', explorations of 'representation … dimensionality' and the production of different knowledges.[1] From Georgian demonstration devices used to diffuse 'esoteric truths of mathematical philosophy', to Edward Mogg's cardboard celestial sphere and its use in early nineteenth-century science education, models and modelling as objects of study have been used to illuminate various different aspects of the scientific process.[2] Dunning, in his study of Alexander Crum Brown's knitted mathematical surfaces, has emphasised how models were more than just visual aids, but pieces of research in their own regard that expanded rather than accompanied the knowledge contained in Crum Brown's research articles.[3] Similarly, Nall and Taub have highlighted models as 'not only end-products of the scientific enterprise – optional representations of work already accomplished – [but] … a central facet of research work' which provide material space for 'inquiry, experimentation, and speculative "play"'.[4] Finally, histories of models, like histories of instruments, can be used to give agency to craftsmen and women less visible in the wider scientific enterprise, as Cornish does in her study of the Krishnagar artisan modellers of an Indian indigo factory whose labour was co-opted 'to serve commercial, exhibitionary and pedagogical ends by an Indian Brahmin working within the Anglo-Indian imperial framework'.[5]

More and more, studies of scientific hardware eschew treating instruments 'as isolated objects, or as icons' and instead, as Taub urges, look to uncover 'how many instruments were used in conjunction with other things, for example other instruments, books, objects, or specimens'.[6] To some extent, this approach could be read as being influenced by Actor-

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[1] Hopwood, N., and de Chadarevian, S., 'Dimensions of modelling', in de Chadarevian, S., and Hopwood, N. (eds), *Models: the third dimension of science* (Stanford University Press: Stanford, 2004), 1-15, 12.
[2] Schaffer, S., 'Machine philosophy: demonstration devices in Georgian mathematics', Osiris 9, 'Instruments' (1994), 157-182, 158; Taylor, K., 'Mogg's celestial sphere (1813): the construction of polite astronomy', *Studies in History and Philosophy of Science* 40 (2009), 360-371.
[3] Dunning, D., 'What are models for? Alexander Crum Brown's knitted mathematical surfaces', *Mathematical Intelligencer* 37 (2015), 62-70.
[4] Nall, J., and Taub, L., 'Three-dimensional models', in Lightman, B. (ed.), *A companion to the history of science*, Adobe Digital Edition (Wiley Blackwell: Chichester, 2016), 791-810, 791.
[5] Cornish, C., 'Curating global knowledge: the museum of economic botany at Kew Gardens', in Finnegan, and Wright, *Spaces of global knowledge: exhibition, encounter and exchange in an age of Empire*, 119-142, 141.
[6] Taub, L., 'On scientific instruments', *Studies in History and Philosophy of Science* 40 (2009), 337-343, 339; There are dozens of such histories. A few examples include Naylor, S., 'Weather instruments all at sea: meteorology and the Royal Navy in the nineteenth century', in MacDonald, and Withers, *Geography, technology and instruments of exploration*, 77-96; Eagleton, C., 'Medieval sundials and manuscript sources: the transmission of information about the navicular and the organum ptolomei in fifteenth-century Europe',

Network Theory, in that it treats objects not as homogenous entities but as networks of a number of contingent parts: 'bits and pieces from the social, the technical, the conceptual, and the textual' that are fitted together and 'converted' into an equally heterogenous scientific product.[1] The thesis that follows does not explicitly draw from the tenets of Actor-Network Theory but it does consider magnetic instruments as part of a constellation of other instruments, as the inscriptions these instruments produced were only valid if made alongside observations of, mainly, meteorological instruments, particularly different kinds of thermometers and barometers. At the colonial observatories, I will also show how observation of the magnetic instruments was undertaken through a sometimes-messy network of correspondence, printed instruction manuals, and gestural knowledge gleaned from earlier tuition.

Whether treated in isolation or as part of wider socio-scientific processes, the choice of which instrument or instruments to focus on is always highly selective. Sometimes, this is in part a reflection of the highly selective process by which some instruments survive through their archiving in museum collections, while others disappear; decisions that can be made on aesthetic, hierarchical, pragmatic or other grounds by museums.[2] The following examples of histories of scientific instruments is similarly selective. This is a reflection not of extant collections but of the needs of the thesis. As such, the curated examples below all speak to one or other aspect of the thesis: the design and use of magnetic instruments in the nineteenth century; the relationship between instruments and instruction manuals; instruments in expeditionary contexts; instruments in states of disrepair; and the use of instruments in the generation and conferral of credibility in the scientific process.

While there is not an enormous literature devoted to the history of geomagnetic instrumentation, notable contributions have been made by Enebakk, Fara, Winter, Bulstrode, Savours, Morrison-Low, and McConnell. Fara focuses on the eighteenth-century trade in magnetic instruments and was concerned especially with the changing relationship between instrument makers, instruments and their users in Britain over the course of the century. The adaptation of navigational instruments to suit the needs of precise mensuration was, Fara reports, one of the means by which eighteenth-century natural philosophers 'consolidated and systematised' magnetic knowledge and established

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in Kusukawa, S., and Maclean, I. (eds), *Transmitting knowledge: words, images, and instruments in early modern Europe* (Oxford University Press: Oxford, 2006), 41-72; Gooday, G. J. N., 'Instrumentation and interpretation: managing and representing the working environments of Victorian experimental science', in Lightman, B. (ed.), *Victorian science in context* (The University of Chicago Press: Chicago, 1997), 409-437.

[1] Law, 'Notes on the theory of Actor-Network', 381.

[2] Taub, 'On scientific instruments', 339.

themselves as 'the new leaders of a public science', distinct from those mariners who had previously been considered the bearers of magnetic knowledge.[1] Winter has also used magnetic instrumentation as the fulcrum for a study on the contested nature of scientific authority. Winter's object is the compass and its role in the creation of scientific authority and public expertise in the first half of the nineteenth century, as viewed through debates between George Airy and William Scoresby over a solution to the problems for navigation caused by the increased use of iron on ships. As Winter shows, this controversy helped to develop different iterations of the compass and different techniques for making it work effectively on new iron-made ships. Focusing especially on Scoresby's work, Winter also demonstrates how the construction of a magnet could be a 'religious and political act' and that through the compass and the magnet we see how the 'religious, scientific, and political aspects of [Scoresby's] work often were not merely closely related but literally the same thing'.[2] Morrison-Low, Multhauf and Good, and McConnell have provided some of the most important technical accounts of nineteenth-century magnetic instruments and their makers.[3] Both Savours and McConnell on the history of the Rossbank Magnetic and Meteorological Observatory, Van Diemen's Land, and Bulstrode's study of Cornish mining practices, also position magnetic instruments within some of the geographies in which they were used in the early nineteenth century. These studies help to form a picture of the way in which magnetic instruments were used in the observatory and in the field, spatial contexts that also figure heavily in the thesis that follows. The work of Savours and McConnell is therefore unpacked in greater detail in the colonial observatories chapter and Bulstrode's earlier in the examination of Lefroy's survey.

The different spatial contexts of scientific instrument use have been an object of inquiry for many historians of science. The observatory is one such spatial context, touched on above and returned to in part below. The use of scientific instruments on voyages of exploration is another spatial context that has been explored through different histories and geographies of science. Some of these have focused on the use of magnetic instrumentation, often within the context of Arctic expeditions. For example, Levere has produced multiple studies that chart the deployment and use of magnetic instrumentation in

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[1] Fara, *Sympathetic attractions*, 145.

[2] Winter, '"Compasses All Awry", 93; see also Bravo, M., 'Geographies of exploration and improvement: William Scoresby and Arctic Whaling (1722-1822)', *J. Hist. Geog.* 32 (2006), 512-538.

[3] McConnell, A., 'Instruments and instrument-makers, 1700-1850', in Buchwald, J. Z., and Fox, R. (eds), *The Oxford Handbook of the History of Physics* (Oxford University Press: Oxford, 2013), 326-357; McConnell, A., 'Nineteenth-century geomagnetic instruments and their makers', in De Clercq, P. R. (ed), *Nineteenth-century scientific instruments and their makers: papers presented at the fourth Scientific Instrument Symposium, Amsterdam, 23-26 October 1984* (Museum Boerhaave: Amsterdam, 1984), 29-53; Morrison-Low, A. D., *Making scientific instruments in the Industrial Revolution* (Ashgate: Farnham, Surrey, 2007); Multhauf, R. P., and Good, G., *A brief history of geomagnetism and a catalog of the collection of the National Museum of American History* (Smithsonian Institution Press: Washington D. C., 1987).

the Canadian Arctic during a number of expeditionary ventures.[1] These are explored in greater depth in the chapter devoted to the North American Magnetic and Meteorological Survey. Recently, Dunn has used the context of the 1818 Arctic voyage of the *Isabella* and *Alexander* under John Ross's command to study that 'most unreliable of instruments, the magnetic compass' and particularly how during this voyage different constructions of the compass were tested and came to be 'afforded different levels of trust and authority'.[2]

Histories of unreliable and faulty instrumentation have created questions about the role of instruments in the creation of credible science in the nineteenth century. While, as Shapin and Schaffer have shown, the integrity of scientific instruments was crucial to the establishment of the experimental episteme from the mid-seventeenth century, the status, integrity and durability of the individual using the instrument was arguably as, if not more, important throughout the seventeenth and eighteenth centuries. The trusted natural philosopher was largely one who met a certain status threshold. In a different but related way, the scientific explorer would often emphasise personal physical hardship and suffering as a narrative trope designed to authenticate their experience and their science. The credibility of the explorer was further linked to their personal and institutional associations at home.[3] Withers has shown how, as the eighteenth century progressed, so natural philosophy became more and more predicated on instruments, their 'manufacture, usage and institutional association' as well as the 'epistemic authority' that precision instrumentation could produce through numbers and measurements.[4]

However, as Withers emphasises, it was not in isolation that instruments produced such epistemic authority. Carefully circumscribed manuals were produced and read alongside the instrument that demonstrated and demarcated the methods scientific travellers ought to follow in order to produce credible science in the field with their instrumentation.[5] As Naylor noted, instruments were unable to speak for their own efficiency but relied for their

[1] Levere, T. H., *Science in the Canadian Arctic: a century of exploration, 1818-1918* (Cambridge University Press: Cambridge, 1993); Levere, 'Magnetic Instruments in the Canadian Arctic Expeditions of Franklin, Lefroy, and Nares', *Annals of Science* 43 (1986): 57-76; Levere, 'Vilhjalmur Stefansson, the Continental Shelf and a new Arctic continent', *Brit. J. Hist. Sci.* 21, 2 (1988), 233-247.

[2] Dunn, R., 'North by Northwest? Experimental instruments and instruments of experiment', in MacDonald, F., and Withers, C. W. J. (eds), *Geography, technology and instruments of exploration* (Ashgate Publishing: Farnham, 2015), 57-75, 57, 71.

[3] Millar, S. L., 'Science at sea: soundings and instrumental knowledge in British Polar expedition narratives, c.1818-1848', *J. Hist. Geog.* 42 (2013), 77-87, 79-80; see also, Kennedy, D., *The last blank spaces: exploring Africa and Australia* (Harvard University Press: Cambridge, MA, and London, 2013), 94; Hevly, B., 'The heroic science of glacier motion', *Osiris* 11 (1996), 66-86.

[4] Withers, C. W. J., 'Science, scientific instruments and questions of method in nineteenth-century British geography', *Trans. Inst. Brit. Geog.* 38 (2013), 167-179, 169.

[5] For further work on the manner in which credible science was produced through texts, see Keighren, I., and Withers, C. W. J., 'Questions of inscription and epistemology in British travelers' accounts of early nineteenth-century South America', *Annals of the Association of American Geographers* 101, 6 (2011), 1331-1346.

accuracy on the competency of their user, for which instruction manuals were important.[1] The status of the observer or experimenter as marker of truth in the production of science was gradually replaced by the ability of an individual to demonstrate the integrity of their instruments and their capacity to make and keep orderly inscriptions. Indeed, it became something of a 'scientific and moral necessity' that users continuously wrote down their observations, maintained accuracy and repeated processes again and again 'so as to be habit forming'.[2]

The paradigmatic shift from the natural philosophical tradition to one of natural science in the early nineteenth century reoriented the emphasis of the scientific process from collection to measurement, and in so doing elevated the position of precision instrumentation and the carefully delineated methods by which they were used.[3] The user was not absented from the process, far from it: they were now expected to be as disciplined, ordered and quantifiable as the precision instrumentation they employed, whether in the field or the laboratory or the observatory. At the astronomical observatory, this need for quantification of the observer led to the 'personal equation' or, as Friedrich Wilhelm Bessel at the Königsberg Observatory first termed the phenomenon, a 'constant difference'.[4] Bessel, Hoffman has written, first used the term in the preface to a report of 1823, in which Bessel noted the difference between two of his observatory's staff in recording the timings of certain transits. Initially, it was thought that more experienced observers would be able to overcome this variable deficiency but, against expectations, it was discovered that 'such differences resisted the remedy of training and increased attention' and therefore concluded that the 'activity of observation partly escaped wilful control'.[5] As Schaffer notes, all sorts of social and material technologies were deployed in the observatory to try to calibrate and quantify the observer once the idea of the personal equation or the constant difference had been recognised. The observatory was reorganised as a factory and 'observers transformed into machine minders' through astronomical discipline and rigorous adherence to accountancy procedures.[6] But it was more than this. In

[1] Naylor, 'Weather instruments all at sea'.
[2] Withers, 'Science, scientific instruments and questions of method', 174; also see, Gooday, G. J. N., 'Instrumentation and interpretation: managing and representing the working environments of Victorian experimental science', in Lightman, B. (ed.), *Victorian science in context* (The University of Chicago Press: Chicago, 1997), 409-437, 412; and Schickore, 'Ever-present impediments', 139-140.
[3] As an example, see Lightman, B., 'Refashioning the spaces of London science: elite epistemes in the nineteenth century', in *Geographies of nineteenth-century science*, 25-50.
[4] Schaffer, S., 'Astronomers mark time: discipline and the personal equation', *Science in Context* 2, 1 (1988), 115-145; Hoffmann, C., 'Constant differences: Friedrich Wilhelm Bessel, the concept of the observer in early nineteenth-century practical astronomy and the history of the personal equation', *Brit. J. Hist. Sci.* 40, 3 (2007), 333-365; see also Rieznik, M., 'The Córdoba Observatory and the history of the 'personal equation' (1871-1886)', *J. Hist. Astron.* xliv (2013), 277-301.
[5] Hoffman, 'Constant differences', 355, 361.
[6] Schaffer, 'Astronomers mark time', 119.

the nineteenth-century observatory, the observer became part of the instrument to be calibrated.[1] As Hoffman put it,

> Tracking the history of the personal equation casts light upon fundamental changes in the conditions of scientific work over the course of the nineteenth century. It does not testify to any distrust in the human observer or allow the conclusion that the observer was framed in wholly different terms to his instruments. On the contrary, a return to the emergence of a phenomenon called 'constant difference' reminds us that the observer ended up aligned with his instruments.[2]

It was through the needs of this process of alignment, Schaffer concludes, that 'networks of the workshop, the laboratory and the observatory became inseparable' because observatories increasingly came to rely on 'precision engineering' and engineers increasingly 'disciplined their [the observatory's] work habits'.[3]

Instrument knowledges

In the closing remarks to his history of the instruments and methods deployed in early nineteenth century geographical exploration, Withers urged future scholars to 'expose the apparent gap between narratives of geographical exploration where truth claims about new findings and the rigours of travel are belied by the facts that the instruments by which new truths were secured constantly broke, malfunctioned or were misread.' For Withers, too little attention had been paid to the 'nature and the fallibility of geography's instruments and to the resultant truth claims'.[4] This lacuna is perhaps surprising, given the influence of Latour's articulation of blackboxing and his general influence in the history of science since the 1980s. Latour's argument is by now familiar. He claimed that 'scientific and technical work is made invisible by its own success' so that 'when a machine runs efficiently, when a matter of fact is settled, one need focus only on its inputs and outputs and not on its internal complexity'. So, as Latour concluded, 'paradoxically, the more science and technology succeed the more opaque and obscure they become'.[5] Or, as Graham and Thrift have put it, 'things only come into visible focus as things when they

[1] Schaffer, 'Astronomers mark time', 118.
[2] Hoffman, 'Constant differences', 364-365.
[3] Schaffer, 'Astronomers mark time', 139.
[4] Withers, 'Science, scientific instruments and questions of method', 176.
[5] Latour, B., *Pandora's hope: essays on the reality of science studies* (Harvard University Press: Cambridge, MA, and London, 1999), 304.

become inoperable'.[1] Otherwise, Baird argued, 'many instruments hide the very materiality they are made from'.[2] And yet, for Schaffer, looking particularly at astronomical instruments around 1800, faults were defaults. Examining instruments in different states of disrepair reveals not only the materiality of the instrument, but histories of maintenance and repair and the systems of instruction, support and abuse that attended different scientific instruments in the past.[3] Examining magnetic instruments in states of disrepair in the observatory and during geophysical surveys forms an important part of this thesis. Magnetic instruments were peculiarly variable constructs, and the ability (or otherwise) of users to both know when an instrument was faulty and to manage that fault in isolated situations is a theme that runs throughout the duration of the magnetic crusade. The 1830s and 1840s was also a very important period in the development of magnetic instrumentation in Britain, and the identification of different faults through trials at specific sites was part of this, as is highlighted particularly in the British Magnetic Survey chapter.

The management of faulty instruments was an important aspect of experimental and observational science, given the significant role played by instruments in the creation of epistemic authority, touched on above. In the creation of trust in the scientific process – in this case information generated through travel – Withers has also demonstrated the importance of adherence to a set of codified methods and developments in written recording practices.[4] However, these records are not always easy to recover or interpret.[5] In these instances, the lack of a clear written record – whether intentional or not – raises questions about how observations or experiments were conducted and, consequently, how credibility constructed. One means by which historians of science have tried to recover this knowledge is through historical experimentation and replication. Although a niche, or at least a more specialised sub-discipline of the history of science, historical experimentation/replication has generated many studies of different instruments and historical methods. A brief survey of this field was offered by Chang in a recent paper on how historical experiment can benefit science education.[6] Willmoth has also recently written about her experience of reconstructing a seventeenth century surveying instrument

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[1] Graham, S., and Thrift, N., 'Out of order: understanding repair and maintenance', *Theory, Culture and Society* 24, (2007): 1-25, 2, quoted in DeSilvey, C., 'Object lessons: from batholith to bookend', in Johnson, N. C., Schein, R. H., and Winders, J. (eds), *The Wiley-Blackwell companion to cultural geography* (Wiley-Blackwell: Chichester, 2013), 146-158, 150.

[2] Baird, *Thing knowledge: a philosophy of scientific instruments* (University of California Press: Berkeley, 2004), 19.

[3] Schaffer, S., 'Easily cracked: scientific instruments in states of disrepair', *Isis* 102, 4 (2011), 707-717.

[4] Withers, 'Science, scientific instruments and questions of method'.

[5] On method, see also Yeo, R., 'Scientific method and the rhetoric of science in Britain, 1830-1917', in Schuster, J., and Yeo, R. (eds), *The politics and rhetoric of scientific method: historical studies* (Reidel: Dordrecht, 1986), 259-297.

[6] Chang, H., 'How historical experiments can improve scientific knowledge and science education: the cases of boiling water and electrochemistry', *Science & Education* 20 (2011), 317-341, 318-319.

– the plane table – from a description produced in Arthur Hopton's *Speculum topographicum: or the topographicall glasse* (1611). Willmoth showed how Hopton did not provide comprehensive instructions for the construction of the instrument, but rather allowed the reader/maker scope to make their own decisions and create a unique plane table based on their skills and the available resources and materials at their disposal.[1]

According to Sibum, what such work aims at is not 'an exact imitation but … a material-aesthetic approximation to the historical performance of experiment'.[2] In his own work, Sibum has highlighted two important points that I believe are relevant to certain aspects of the following thesis. These revolve around the idea of 'gestural knowledge', what Sibum defined as tacit, sense-based skill gleaned from experience of the process in question.[3] Gestural knowledge is not something learned from books but formed of habits acquired and reinforced through continual involvement in a particular scientific culture. Sibum arrived at this understanding through restaging a series of paddle wheel experiments James Prescott Joule undertook in 1850 to determine the mechanical equivalent of heat, alongside an examination of the Manchester brewing culture to which Joule also belonged. Sibum, together with Heering, established through a process of trial and error that Joule had not included aspects of the experimental process necessary to make the experiment produce the kind of results that Joule had obtained. What Sibum discovered was that the thermometric skills Joule had were not those that could be easily explained, but which Joule had learned through his involvement 'in the brewers' form of life' in Victorian Manchester.[4] This was a sensory aspect of experimentation that existed beyond the codification of instruction manuals. Elsewhere, Daston determined a similar phenomenon in the method of scientific observation, through what some field naturalists call "jizz": 'a term of art for the all-at-once-ness of virtuoso perception'. It is the 'sure, swift, and silent, "without pause for mental analysis,"' form of observation 'grounded in long familiarity with the phenomenon in question, be they curlews or streptococcus bacteria'.[5] While Daston agreed that this was a bodily skill, irreducible to a method or model or algorithm, this should not be taken to imply that 'the process is irretrievably tacit, much less mystical'.[6]

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[1] Willmoth, F., ''Reconstruction' and interpreting written instructions: what making a seventeenth-century plane table revealed about the independence of readers', *Studies in History and Philosophy of Science* 40 (2009), 352-359, 358-359.
[2] Sibum, H. O., 'Experimental history of science', in Lindqvist, S. (ed.), *Museums of Modern Science* (Science History Publications: Canton, MA, 2000), 77-86, 81.
[3] Sibum, H. O., 'Reworking the mechanical value of heat: instruments of precision and gestures in early Victorian England', *Studies in the History and Philosophy of Science* 26, 1 (1995), 73-106.
[4] Sibum, 'Reworking the mechanical value of heat', 83.
[5] Daston, L., 'On scientific observation', *Isis* 99, 1 (2008), 97-110, 101.
[6] Daston, 'On scientific observation', 101.

Historical experimentation is one avenue that has attempted to retrieve, inscribe and explain such feats of perception and sense for experimentation but, as Daston has explained, gestural knowledges can also be arrived at through textual sources alone. It is, I argue later, such gestural knowledge and "jizz" that underlies how Frederick Eardley-Wilmot at the Cape of Good Hope Magnetic Observatory in 1841 differentiated between knowing an instrument was in a 'conscientious adjustment', one that would ensure usable results, and when it was only in an 'apparent adjustment', a state that gave the appearance but not the reality of usable results. Familiarity with, particularly, the vertical force magnetometer, gained by frequent and repetitive observations within the specific environment of the Cape Observatory and reinforced through the scrutiny of his data back in Britain, bred in Wilmot a tacit knowledge and awareness not learned elsewhere or through the written instructions that accompanied him to the Cape.

Unpacking the (geomagnetic) data archive

The final empirical chapter of this thesis explores the geomagnetic data archive of the magnetic crusade and how data can be used as a historical artefact of some of the procedures enacted by Sabine at his magnetic department. This final chapter also includes similar but distinct aspects of the literature that are presented here and so this part of the review will act only as an introduction to the topic of data histories. But, what are data? And how can they be approached as a source for histories of science? Questioning data has been the concern of a growing number of philosophers of science and STS scholars, a concern that coalesced recently in the production of a special issue of *Osiris* dedicated to data histories.[1] Much of the motivation behind this burgeoning literature has been attempts to engage with and critique current debates over big data and the scientific process. This debate, popularised both within and outwith the academic sciences, centres on the notion that science has become, in the last decade or so, peculiarly "data-driven", a term which Sepkoski defines as 'an ostensibly recent transformation in which scientific practice has become increasingly centred around massive sets of data and dependent on technologies that facilitate management and analysis of that data'.[2]

In the twenty-first century, discourse on data is rife: big data, personal data, data mining, data storage, algorithms, data security. The argument goes that as a society, and in the

[1] 'Data histories', *Osiris* 32, 1 (2017).
[2] Sepkoski, D., 'Towards a "Natural History of Data": evolving practices and epistemologies of data in paleontology, 1800-2000', *Journal of the History of Biology* 46 (2013), 401-444, 403.

sciences, we are living in an age of information overload; a time in which scientists no longer begin investigations with a hypothesis, but with petabytes of digital data and computer systems and algorithms capable of disseminating, analysing and interpreting these massive datasets to distinguish patterns. This approach, variously known as "data-intensive", "data-driven" or "data-centric" science, is, Leonelli explains, typically associated with 'the emergence of large-scale, multi-national networks of scientists; a strong emphasis on the importance of sharing data and regarding them as valuable research outputs in and of themselves, regardless of whether or not they have yet been used as evidence for a given discovery … [and] the development of instruments, building on digital technologies and web services, that facilitate the production and dissemination of data with a speed and geographical reach as yet unseen in the history of science'.[1] Much of what scholars such as Sabina Leonelli, Bruno Strasser, Elena Aronova and others are trying to achieve through engagement with this subject is to historicize claims about the novelty of big data science and show how relative ideas of "information overload" actually are. It was precisely in order to 'reconstruct a history of "data" in the *longue durée*' and to 'critically examine historical claims about the distinctiveness of modern data practices and epistemologies' that a working group entitled 'Historicizing Big Data' was formed at the Max Planck Institute for the History of Science.[2]

The importance of this debate for my own work is twofold. First, in engaging critically with the idea of big data science, scholars have been forced to think explicitly about what we mean when we talk about data: to ask fundamental questions about what data are, beyond a mark on a page or a sketch in a notebook. And, second, there is now within science studies an emphasis on the need to historicise data, to highlight and emphasise data handling practices, data packaging practices, how data travelled and were used, stored and reused. In other words, the historical 'journeys and deaths of scientific data' are now subjects of inquiry to sit alongside the more established analyses of the journeys of scientific instruments.[3] The final empirical chapter of this thesis will contribute to this historicising of data.

Data can be defined, according to Leonelli, as 'tools for communication, whose main function is to enable intellectual and material exchanges across individuals, collectives,

[1] Leonelli, S., Philosophy of Data Science series, University of Exeter, https://blogs.exeter.ac.uk/exeterblog/blog/2015/01/26/philosophy-of-data-science-series-sabina-leonelli-what-constitutes-trustworthy-data-changes-across-time-and-space/ [accessed 16 October 2017].

[2] Max Planck Institute for the History of Science, Historicising Big Data Working Group http://www.mpiwg-berlin.mpg.de/en/research/projects/DeptII_Aronova_Oertzen_Sepkoski_Historicizing [accessed 16 October 2017].

[3] Leonelli, S., 'Journeys and deaths of scientific data', slidepack, https://www.datastudies.eu/images/downloads/2016/4S-EASST2016-Leonelli.pdf.

cultures, governments'. Making data mobile, making data travel and speak across different research communities in different times and places, is a 'hard-won scientific achievement', but also a critical component of what makes data count.[1] For Leonelli, the mobility of data is one of its fundamental characteristics. In this vein Leonelli continues, arguing that data consist of a:

> specific way of expressing and presenting information, which is produced and/or incorporated in research practices so as to be available as a source of evidence, and whose scientific significance depends on the situation in which it is used. In this view, data do not have truth-value in and of themselves, nor can they be seen as straightforward representation of given phenomena. Rather, data are essentially fungible objects, which are defined by their *portability* and their *prospective usefulness as evidence*.[2]

Data, despite their epistemic value and etymology as 'given' are 'clearly made' through 'complex processes of interaction between researchers and the world, which typically happen with the help of interfaces such as observational techniques, registration and measurement devices, and the rescaling and manipulation of objects of inquiry for the purposes of making them amenable to investigation'.[3]

For Rheinberger and for others, materiality is a key characteristic of data. A discussion of data's materiality is presented in the final empirical chapter of the thesis but it is also worth exploring in part here. Rheinberger argues that experimental or observational traces – the precarious scientific objects immediately formed by intervention or interaction with the material under investigation – can only be transformed into data and from there to 'patterning facts' by being 'brought into a form in which it can be *stored*, and consequently, *retrieved* as well. Much speaks', Rheinberger continues, 'for the assumption that the ability to be stored, that is, to be made *durable,* is the most important prerequisite for transforming *traces* into *data*'.[4] Data, in this analysis, are then examples of Latourian "immutable mobiles"; portable, retrievable, capable of re-enactment. Where traces are 'usually precarious, bound-to-disappear', data are stable entities. While Leonelli's stance on data also seeks to emphasise the 'epistemic importance of the mobility of data', Leonelli does not share Rheinberger, and indeed Latour's, emphasis on the stability of data. For Leonelli, the movement of data from their 'original context of production to a

[1] Leonelli, S., 'What counts as scientific data? A relational framework', *Philosophy of Science* 82 (2015), 810-821, 810-811.
[2] Leonelli, 'What counts', 811. Emphasis in original.
[3] Leonelli, 'What counts', 813
[4] Rheinberger, H-J., 'Infra-experimentality: from traces to data, from data to patterning facts', *History of Science* xlix (2011), 337-348, 344.

database, and from there to a new context of inquiry' is always precarious, as 'the procedures involved in packaging data for travel involve various stages of manipulation, which may happen at different times and may well change the format, medium, and shape of data'.[1]

It follows from Leonelli's analysis that we need to pay attention to data handling practices: how data is packaged, how it is moved, how it is unpacked and in what context. Context is fundamental because, in Leonelli's estimation, data are relational. What counts as data 'depends on who uses them, how, and for which purposes'.[2] Data, it is argued, can be 'circulated independently of the claims for which they are taken as evidence, so as to be used in research contexts other than the one in which they have been produced' but, in order to do so, they must first be appropriately curated in databases.[3] The curation of data in databases and its movement across different research contexts throws up important points. Databases are the place for what Ian Hacking called 'marks', obtained through either measurement or observation of a given organism or phenomenon.[4] For Leonelli, 'these marks constitute unique documents about a specific set of phenomena. Their production is constrained by the experimental setting and the nature of entities under scrutiny' but researchers with different interests might find them and interpret them differently. For this to take place, 'curators' must present data independent of the original claim for which they were produced to substantiate. These are nonlocal data. However, the source of the data – its locality – becomes important when credibility is required for a claim. In other words: '*to evaluate the quality of a claim we need to know how the claim originated*' [emphasis in original]. Or: 'on the one hand, facts travel well when stripped of everything but their content and means of expression; on the other hand, the reliability of facts can be assessed only by reference to how they are produced'. One way to overcome this kind of paradox is through labels and unique identifiers (which allow data to be marks and show themselves applicable to multiple researchers) and by the curator structuring the database through metadata and evidence codes which allow for the storage of as much information as possible about the data's provenance.[5] Data, in Leonelli's configuration, can move between local and nonlocal status depending on the research context, research demands, and the manner in which the data are packaged.

[1] Leonelli, 'What counts', 816.
[2] Leonelli, 'What counts', 817.
[3] Leonelli, 'On the locality of data and claims about phenomena', *Philosophy of Science* 76, 5 (2009), 737-749, 737.
[4] Hacking, I., 'The self-vindication of the laboratory sciences', in Pickering, A. (ed.), *Science as Practice and Culture* (University of Chicago Press: Chicago, 1992), 29-64.
[5] Leonelli, 'On the locality of data', 740-741.

As this section demonstrates, the categorisation of data is complex and multifaceted. Data are material, portable, stable and unstable, local and nonlocal. Data are dependent on various technologies and packaging processes such as the database, an arrangement that does not allow for "raw" data, but instead forces data 'into ontological categories defined by convention (and not necessarily universally agreed upon)'.[1] Data are tools for communication across all sorts of institutional, national and research contexts, as specimens, numbers, texts, photographs, marks and traces that cover the whole spectrum from material thing to abstract representation. Indeed, data can be almost anything that is 'collected, stored and disseminated in order to be used as evidence for knowledge claims'.[2] Data count in relation to specific situations but are always anchored in material manifestations or paper-based technologies. Graeme Gooday has also succinctly demonstrated how seemingly simple measurements could come loaded with moral value in at least four ways:

> in the *presupposition* of a measurement; what was fair to assume about the integrity of previous measurers in the field? In the *performance* of a measurement; did its conduct instantiate trustworthy practices and appropriate experimental virtues? In the *reporting* of a measurement; was the written (published) account an honest and impartial summary of the performance? And in the *ramifications* of a measurement; what benefits – if any – might the quantitative information generated bring to others?[3]

Data, as artefacts, are simultaneously inscriptions – Daston's 'second nature' – and inscribed, or imbued, with all sorts of different value depending on the context in which they are viewed. Data live and later die when their value is no longer perceivable, but death is not final: 'resurrections can occur' but only if a material trace survives, perhaps in an indiscriminate little building on the periphery of some large scientific complex, for example.[4]

Conclusion

This review has provided an overview of both the literature within which this thesis is situated and the literature that inspired the methods I have used to investigate the

[1] Strasser, 'Wonder cabinets to electronic databases', 85.
[2] Leonelli, 'Journeys and deaths of scientific data'.
[3] Gooday, G. J. N., *The morals of measurement: accuracy, irony, and trust in late Victorian electrical practice* (Camridge University Press: Cambridge, 2004), xvi.
[4] Leonelli, 'Journeys and deaths of scientific data'. See also, Daston (ed.), *Science in the archives*.

organisation of the British magnetic scheme. The first section considered what might be called the empirical basis of this thesis: the way in which scholars have approached both the history of the physical sciences in Britain in the nineteenth century and the history of the British magnetic scheme specifically. This section demonstrated how scholars have used the history of the origins of the scheme to speak about the wider politics of British science in this time but have not considered the continuation of this politicking once the scheme got underway. That this should be so is perhaps linked to the fact that the history of the military's involvement in the administration of science in this period has also been neglected for, as I demonstrate in the final empirical chapter of this thesis, it is within such military spaces as Woolwich Arsenal that we see the politics of Victorian science continued, in arguments over space, money, authority and personnel. The first section of this review also sought to establish how scholars have explored the wider European context of British geomagnetic research. Continental geomagnetic research does not form a part of this thesis but it was integral to the British magnetic scheme. Thus, part of this section was intended to illustrate this significance and highlight the considerable scholarship devoted to unpacking its histories. This section was also designed to highlight some of the scholarship to which this thesis does intend to contribute, namely explorations of geomagnetic instrumentation in different contexts.

The first section of this review revealed the historiographical lacunae that this thesis addresses. The second section then introduced some of the sociological, historical and archival theories and methods by which it is possible to do so. Much of the scholarly basis of this thesis is founded in historical geographies of science and particularly geographies of the production of science and geographies of the transmission of science. These literatures have formed many of the questions that I asked of my archival material, both document and object. However, as the above makes clear, it is proper to trace the routes of these literatures and to provenance the now-tacit deployment of many of the ideas first offered by sociologists of science working in the 1970s and 1980s: the translation of inscriptions, the black box, the symmetry postulate. Several parts of this review also focused on examinations of scientific instruments – and scientific hardware broadly writ – from historical and geographical perspectives. Finally, this section and the review overall, finished with an attempt to entwine explorations of the archive and a burgeoning interest in data histories, investigations that inform the final empirical chapter of this thesis. The use of the archive in science and the need to historicise claims about Big Data will only become greater concerns for historians and philosophers of science and this overview was designed to provide a sketch of how some scholars have begun to engage with such topics and how their questions have structured my own explorations of the geomagnetic data archive and historical data management processes.

Chapter 4: Proving instruments credible in the early nineteenth century: The British Magnetic Survey and site-specific experimentation

Introduction

This is the story of the British Magnetic Survey (BMS), 1833 to 1838. However, we start after this survey had ended and another had begun.

In 1842, Lieutenant John Henry Lefroy was en route to Toronto, Canada and the beginning of his North American Magnetic and Meteorological Survey. Accompanying him were two dip circles: one designed by the Cornish instrument maker Robert Were Fox; the other by the Frenchman Henri-Prudence Gambey.[1] These instruments measured the angle at which a magnetic needle inclined to a magnetic pole in different parts of the earth and they relied for their accuracy on both the perfection of their construction and the magnetic needles used within them. Needles were, however, mutable objects: their magnetic strength was not constant. There were good needles, which held their strength or at least degenerated at a consistent rate; and there were bad needles, which seemed to lose strength randomly.

The status of a particular needle needed to be known to an observer so that the many observations recorded with it could later be reduced and made comparable. Not knowing the state of the needle made terrestrial magnetic observation a guessing game. Edward Sabine knew this only too well. After all he was the de facto organiser of Britain's so-called magnetic crusade, which established geomagnetic observatories throughout the British Empire and launched geomagnetic expeditions such as Lefroy's to Canada and the more famous voyage of James Clark Ross to the Antarctic from September 1839.[2] When equipping Lefroy's expedition, it was vital that Sabine provided good, reliable needles. In the frigid high latitudes of the Canadian Arctic it would have been difficult for Lefroy to have found a replacement for a bad one. Writing in the *Philosophical Transactions* in 1846, Sabine spelled out the fact that the needles which had travelled with Lefroy to be used in the Gambey dip circle 'were the same which had been used in the British Survey,

[1] Sabine, E., 'Contributions to terrestrial magnetism. No. VII', *Phil. Trans. R. Soc. Lond.* 136 (1846), 237-336, 240.
[2] Cawood, J., 'The magnetic crusade: science and politics in early Victorian Britain', *Isis* 70, 4 (1979), 492-518.

when they were proved to be free from index error at all inclinations, by the observations of Captain Johnson, R.N. and myself in the Regent's Park'.[1]

What follows is an exploration of the British Magnetic Survey and its significance, as Sabine touches on, in testing and proving instruments of geomagnetic study. In tracing the course of this survey, it will be demonstrated that knowledge of the science of terrestrial magnetism was embodied in the instruments of its study and entwined with specific sites. The experience of handling, testing and modifying magnetic instruments during this domestic survey was a formative one for magnetic instrument users and makers. Work during the BMS led to developments in both magnetic instrument design and the method of their use. As we follow instruments and their users on this survey, we observe how certain places came to act as testing grounds for magnetic instruments in a time before the establishment of specific geomagnetic observatories in the British Isles. Latterly, this chapter will also discuss the difficulties encountered in producing the final report of the first BMS, and the choice that had to be made between weighting the observations of the survey in favour of station or individual error – what might be called the choice between the personal and the geographical equation – in the final reduction of their data. This discussion is followed by a brief description of the afterlife of the first BMS and how it was significant to both the magnetic crusade and in precipitating repeat geomagnetic surveys of Britain and the drawing of Britain's magnetic map.

Course of the survey

Put simply, the BMS was just that: a survey of the earth's magnetic field throughout the British Isles. To be more precise, it was a survey of three properties of the earth's magnetism: the variation of its direction (declination); inclination; and its horizontal intensity. In the BMS, only the horizontal component of the intensity was recorded as instruments for the accurate measurement of its vertical component were only perfected in later years.[2] The BMS produced three substantial publications and several maps of England, Scotland and Ireland showing the isodynamic (equal intensity) and isoclinal (equal inclination) lines in these locales. These maps were the first of their kind for Britain.[3] The British Association for the Advancement of Science (BAAS) was the vehicle

[1] Cawood, J., 'The magnetic crusade'.
[2] O'Hara, J. G., 'Gauss and the Royal Society: the reception of his ideas on magnetism in Britain (1832-1842)', *Notes Rec. R. Soc.* 38, 1 (1983), 17-78, 30, 46.
[3] Enebakk, V., 'Hansteen's magnetometer and the origin of the magnetic crusade', *Brit. J. Hist. Sci.* 47, 4 (2014), 587-608, 606.

by which the survey was launched and it was to this organisation that reports were made and through which they were later published. The principal participants in the survey's execution were Edward Sabine, Humphrey Lloyd, and James Clark Ross. They were later joined by John Phillips, better known for his work in the field of geology and Robert Were Fox, the reputable Cornish instrument maker, geologist and physicist.

Originally, a domestic magnetic survey had been called for by the BAAS in 1831.[1] Prompted by the magnetic survey work of James Dunlop in Scotland in 1830 and a renewal, or 'revival', of interest in the physical sciences in Britain from the late 1810s, the BAAS felt it 'highly desirable that a series of observations upon the *Intensity of Terrestrial Magnetism in various parts of England* be made by some competent individual'.[2] The call went further, requesting that, as a matter of considerable importance, 'a certain number of observations should be made throughout Britain with *the Dipping Needle*'.[3] The competent individual first identified to carry out the survey was William Scoresby – Arctic explorer, natural philosopher and Church of England clergyman.[4] However, Scoresby was unable to carry out the assignment and instead passed the work to his friend Thomas Stewart Traill in Liverpool.[5] The 'standard Hansteen needle belonging to the Royal Society of Edinburgh' was duly passed to Traill for the work.[6] The Hansteen magnetometer previously used by Dunlop for his survey of Scotland was also received by Traill.[7] Traill commenced his observations in early 1832 but only managed to publish eight observations of intensity, taken in Liverpool and Manchester, before career interests took him elsewhere and his brief affair with the British Magnetic Survey was ended.[8] It was shortly after this, in 1833, that Lloyd and Sabine began corresponding with one another and together developed a plan for a more extensive British survey, using a far greater number of instruments and needles.[9] For the purposes of this account, the BMS is understood to have begun only once Sabine and Lloyd had taken command of it.

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[1] 'First Report: 1831', *Report of the first and second meetings of the British Association for the Advancement of Science; at York in 1831, and at Oxford in 1832* (John Murray: London, 1833), 52.
[2] Enebakk, 'Hansteen's magnetometer', 603-604; Gavine, D., 'Dunlop, James (1793-1848)', *Oxford Dictionary of National Biography* [ODNB], Oxford University Press [OUP], 2004; online edn, http://www.oxforddnb.com/view/article/8275?docPos=2 [accessed 4 January 2016]; Miller, D. P., 'The revival of the physical sciences in Britain, 1815-1840', *Osiris* 2nd Series, 2 (1986), 107-134.
[3] 'First Report: 1831', 52.
[4] Morrell, J., 'Wissenschaft in Worstedopolis: public science in Bradford, 1800-1850', *Brit. J. Hist. Sci.* 18, 1 (1985), 1-23; Winter, A., '"Compasses All Awry": the iron ship and the ambiguities of cultural authority in Victorian Britain', *Victorian Studies* 38, 1 (1994), 69-98.
[5] Morrell, J., and Thackray, A., *Gentlemen of science: early years of the British Association for the Advancement of Science* (Clarendon Press: Oxford, 1981), 525.
[6] Morrell and Thackray, *Gentlemen of science*, 524.
[7] Enebakk, 'Hansteen's magnetometer', 605.
[8] 'Second Report: 1832', 557.
[9] O'Hara, 'Gauss and the Royal Society', 24

It was shortly after their epistolary relationship had started that Lloyd and Sabine began terrestrial magnetic observation in Ireland. This initial period of work lasted from 1833 to 1835: Lloyd from his base at Trinity College, Dublin, and beyond, and Sabine in Limerick, where he had been stationed on military service. Initially, however, Lloyd had to confine himself to making only those observations which helped to verify his new method of observing both the intensity and dip of the earth's magnetic field with the same needle, known as the statical method.[1] Lloyd had been prevented from beginning a series of regular geomagnetic observations because the Hansteen magnetometer he required and which he had requested from George Dollond had reached him late.[2] Lloyd was referring to George Dollond, from the dynasty of optical, mathematical and scientific instrument makers, the Dollonds, who had been in operation since the mid-eighteenth century.[3] It was not until July 1834 that observations for the British survey began in earnest.[4]

Over the period 1834-1835 Lloyd, Sabine and, on occasion, Ross, collaborated on the Irish portion of the BMS.[5] Lloyd made the majority of observations 'in the field', i.e. beyond the magnetic stations at Dublin and Limerick. These two places were adopted as sites at which observations were made that could stand as a means for comparison once a series of field observations had been completed elsewhere. Magnetic needles lost strength over time and so it was vital to have a standard to refer to when comparing and reducing results.

Lloyd observed from Ballybunan (nowadays Ballybunion) in the south-west to Carlingford in the east and Strabane in the north and at twenty-one other stations.[6] Sabine and Ross collaborated over the regular exchange of results, needles and instruments for the purpose of verification at other base stations. For instance, on 7 November 1835, Lloyd sent his Hansteen needles to Sabine and asked him to vibrate them – a method of measuring the

[1] For a more detailed explanation of the statical method, see O'Hara, 'Gauss and the Royal Society', 18-19, 21-24.

[2] Humphrey Lloyd to Edward Sabine, 20 November 1833, The National Archives (hereafter TNA) BJ 3/7/2.

[3] Clifton, G., 'Dollond family (per. 1750-1871)', *ODNB*, OUP, 2004; online edn, http://www.oxforddnb.com/view/article/49855/7782?docPos=1 [accessed 5 January 2016]; Barty-King, H., *Eyes Right: The Story of Dollond & Aitchison 1750-1985* (Quiller Press: London, 1986).

[4] 'Observations on the direction and intensity of the terrestrial magnetic force in Ireland, made by The Rev. Humphrey Lloyd, M.A., F.R.S., Captain Edward Sabine, F.R.S., and Captain James Clarke Ross, R.N., F.R.S', *Report of the British Association for the Advancement of Science for 1835*, RS Tracts: 252/10, 117-162, 117. The delay in beginning a regular series of observations was also in part due to the fragile health of Lloyd at this time and his other scientific and university commitments. See TNA BJ 3/7/5.

[5] Carter, C., 'Magnetic Fever: global imperialism and empiricism in the nineteenth century', *Transactions of the American Philosophical Society*, New Series, 99, 4 (2009), i-168, 17; Cawood, J., 'The Magnetic Crusade: science and politics in early Victorian Britain', Isis 70, 4 (1979), 492-518, 504-505; Enebakk, Vidar, 'Hansteen's magnetometer', 605-606; Morrell, J., *John Phillips and the Business of Victorian Science* (Aldershot, 2005), 121-122; Morrell and Thackray, *Gentlemen of Science*, 524-528; O'Hara, 'Gauss and the Royal Society', 24-25.

[6] Sabine, E., 'A memoir on the magnetic isoclinal and isodynamic lines in the British Islands, from observations by Professors Humphrey Lloyd and John Phillips, Robert Were Fox, Esq, Captain James Clark Ross, R.N., and Major Edward Sabine, R.A.', *Report of the Eighth Meeting of the British Association for the Advancement of Science; held at Newcastle in August 1838* Vol VII (London, 1839), 49-195, 171-173.

intensity of the earth's magnetism – in Limerick.[1] They had already been vibrated in London and Dublin but Lloyd wanted Sabine to observe with them in Limerick to serve as a 'double comparison'.[2] To make the observations strictly comparable, Lloyd also provided Sabine with further details of his method of observing those needles. The 1834-1835 Irish series was revisited and extended in later years, through new comparisons of the intensity at London and Dublin, by Lloyd, in 1836; between Dublin and Bangor by Sabine later in the same year; by Sabine again between London and Dublin in 1838; and in a complete series of observations by Ross in 1838, 'at twelve distinct stations throughout the island'.[3]

In July 1836, Sabine travelled from Dublin 'by steamer direct to the Clyde' and from there to twenty-seven other points in the north, east, south and west of Scotland.[4] Sabine first observed in Helensburgh and moved through several of the islands of western Scotland on a yacht provided by James Smith of Jordanhill, President of the Andersonian Institution at Glasgow.[5] Sabine travelled north through a combination of steamers and mail coaches, then descended south along the east coast of Scotland as far as Dryburgh, before heading west through Glasgow to Stranraer and thence back to Dublin.[6] Ross would later observe in Scotland and provide more results for comparison and for computation of the yearly variation of terrestrial magnetic intensity and dip there.[7]

The final portion of the BMS – England and Wales – was complete by 1838.[8] This was carried out by the '"quadruple alliance"'[9] of Sabine, Lloyd, Ross and John Phillips and supplemented by observations made by Robert Were Fox at eight stations from London to the Scilly Isles.[10] Fox had previously recorded a small number of observations in Ireland

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[1] Vibrating the needle was the method by which a needle or cylinder was suspended horizontally, made to vibrate, and the time in which it completed 300 vibrations – within a predetermined arc – measured by a chronometer. A good needle would swing for ten minutes or more before coming to rest, according to Turner, G., *North Pole, South Pole: The Epic Quest to Solve the Great Mystery of Earth's Magnetism* (Awa Press: Wellington, 2010), p. 107. The method is most commonly associated with Christopher Hansteen, who standardised the number of required vibrations to 300. It was replaced by Lloyd's statical method over the course of the 1830s.
[2] Lloyd to Sabine, 7 November 1835, TNA BJ 3/7/21.
[3] Sabine, 'A memoir', 166.
[4] Sabine to Lloyd, 13 July 1836, RS MS/119/I/17; Sabine, E., 'Observations on the direction and intensity of the terrestrial magnetic force in Scotland by Major Edward Sabine, R.A., F.R.S., etc.' *Report of the Sixth Meeting of the British Association for the Advancement of Science; held at Bristol in August, 1836* Vol. V (London, 1837), 97-119, 102.
[5] Sabine, 'Observations on the direction', 97.
[6] Sabine, 'Observations on the direction'; Sabine to Lloyd, 13 July 1836, RS MS/119/I/17.
[7] Sabine, 'A memoir', 50.
[8]There are several instances of error in accounts of the BMS, notably in Cawood, 'The Magnetic Crusade', 505, which states that the last part of the BMS was completed and presented to the BAAS in 1836, rather than 1838. Similarly, Sabine's ODNB entry puts him in Scotland in 1835 instead of 1836 and in England in 1836, where he did not observe to any extent until 1837-38.
[9] Lloyd to Sabine, 3 August 1837, TNA BJ 3/7/40.
[10] Sabine, 'A memoir', 49.

after the 1835 BAAS meeting in Dublin, but this was prior to his involvement with Sabine and others on the BMS. The results were initially published in 1836 in the *Report for the Royal Polytechnic Society of Cornwall* for 1835, but were also included in the final publication of the BMS, printed in 1839. Phillips had written to Lloyd as early as July 1835 to inform him that he had begun to make a series of magnetic observations in the north of England and to ask Lloyd whether he would like to use this series, together with Lloyd's own observations, to draw up a paper for the BAAS. Apparently, Lloyd had 'not the least idea' that Sabine and Ross proposed to 'magnetize' in England at this time and so asked Sabine in a letter whether he (Sabine) and Ross would include Phillips's work in their survey or whether they intended to work alone. If the latter, then Lloyd himself was only too happy to combine his own work with that of Phillips.[1] As it happened, Phillips was invited to join Lloyd and Sabine's project. Phillips was 'flattered' to receive such an invitation and began work on dip observations in Yorkshire in the spring of 1836.[2] These observations formed the basis of a paper revealed to the BAAS in 1836, which put forward Phillips's belief that isoclinal lines 'in flat areas were bent to the south and on hills to the north'. This opinion was criticised by William Scoresby and William Ritchie but defended by Lloyd.[3]

Substantial observations were made throughout England and Wales in the years 1836 to 1838. Lloyd observed here between April and October 1836;[4] Sabine between May 1837 and October 1838;[5] Phillips between June 1837 and March 1838;[6] Ross at various stages between August 1837 and December 1838;[7] and Fox between August 1837 and August 1838.[8] These observations took in large swathes of England and parts of Wales, from Newcastle to York to Aberystwyth, London, Falmouth and at dozens of stations in between. Although Sabine and Lloyd were the principal observers on the BMS and were responsible for the reduction of results and the computation of the isoclinal and isodynamic lines, they benefited greatly from the services of Fox and especially Phillips, due in large part to the latter's extensive observations in the north of England. Morrell muses that Phillips may have joined the BMS 'perhaps as a change from topographical geology'.[9] Indeed, at one stage Phillips regretted to inform Sabine that he meant 'to try the dip, again

[1] Lloyd to Sabine, 27 July 1835, TNA BJ 3/7/20.
[2] Morrell, *John Phillips*, 121.
[3] Morrell, *John Phillips*, 121.
[4] Sabine, 'A memoir', 69, 140.
[5] Sabine, 'A memoir', 81-83, 142.
[6] Sabine, 'A memoir', 71-73, 145-146.
[7] Sabine, 'A memoir', 75-80, 149-150.
[8] Sabine, 'A memoir', 67, 147.
[9] Morrell, *John Phillips*, 121.

in winter, but my other association have called me off to Belemnites & Orthoceratites!'.[1] At this time, Phillips was also engaged in the Ordnance Geological Survey of Britain, begun under the direction of Henry De la Beche in 1835.[2] Phillips was an 'experienced field surveyor', but an inexperienced yet motivated terrestrial magnetic observer.[3]

Most accounts of Britain's nineteenth-century geomagnetic activity do not interact with the BMS beyond this point. Admittedly, it was not the grandest geomagnetic undertaking of this period, especially in comparison to continental exploits and the later British worldwide magnetic scheme. The BMS did not involve the creation of a system of observatories or a great and daring expedition. However, this is no reason to doubt its significance. The BMS sheds new light on the practice of managing instruments on the move and, in consequence, on how scientific knowledge was made, and made credible, on the move. Attention is warranted here because, as Finnegan explains, 'when scientific knowledge travels it transmutes' and investments of labour and resources must be made in order to translate such knowledge and make it applicable from one place to another.[4] How and where this translation occurred is important to understanding the operation of science at this time. Furthermore, the exigencies of the BMS demonstrate how it was often in moments of crisis, when instruments existed in states of disrepair, that new knowledge was arrived at.[5]

Lloyd's practical education and instruments in the BMS

> Have you ever remarked that needles which are very well balanced as long as they remain at home, become unsteady & unsettled when they travel?[6]

A magnetic instrument in the nineteenth century could be a peculiar item. Its magnetised needle – on which the instrument relied – was changeable; sometimes this was gradual, at other times instantaneous. This change or, more specifically, this loss of magnetic force, could occur even if the needle was kept stationary and away from other disturbing elements, such as iron. More commonly however, and as the above quotation from Lloyd

[1] John Phillips to Sabine, 4 November 1837, RS MS/259/994.
[2] Secord, J. A., 'The Geological Survey of Great Britain as a research school', *History of Science* xxiv (1986), 223-274.
[3] Morrell, J., 'Science and government: John Phillips (1800-74) and the early ordnance geological survey of Britain', in Rupke, N. (ed.), *Science and the public good: essays in honour of Margaret Gowing* (Macmillan: London, 1988), 7, 10; Morrell, *John Phillips*, 122.
[4] Finnegan, D. A., 'The spatial turn: geographical approaches in the history of science', *J. Hist. Biol.* 41 (2008), 369-388, 373.
[5] Schaffer, S., 'Scientific instruments in states of disrepair', *Isis* 102 (2011), 706-717.
[6] Lloyd to Sabine, 6 May 1835, TNA BJ 3/7/5

indicates, such change was occasioned by travel. In the bounded and controllable environment of Trinity College, Lloyd's magnetic needles behaved themselves. They could be housed safely away from the disturbing influences of other ferruginous metals and prevented from receiving any jars or concussions which might unduly affect a needle's magnetism. This problem was as geographic as it was scientific. All needles were subject to degradation of their magnetism, but in the more controllable environment of the observatory such loss could be limited, or at least more frequently and easily accounted for. The rate at which a needle changed here was more predictable and manageable. When they travelled, however, when the space through which needles passed became less secure and more contingent on a number of other factors – the weather, the method of conveyance – needles were liable to become 'unsteady & unsettled'. However, such travel was essential to observations of terrestrial magnetism – a science which, after all, aimed to chart a global phenomenon. Needles had to be interchanged, observed and verified by observers in different locations for results obtained with them to be made credible. This almost paradoxical situation – that travel was both essential for, and detrimental to, the science of terrestrial magnetism – is made explicit through the trials and tribulations of the participants and instruments of the BMS.

On 24 January 1834, Lloyd wrote to Sabine to confirm receipt of the latter's 'dipping apparatus'[1] – an 11-inch Dollond circle that Sabine deemed 'inconvenient for carriage' – and its needles.[2] This instrument and its needles had been in circulation for several years already: Sabine had tested the instrument and needles for Captain John Franklin before Franklin had carried it on his second Arctic expedition between 1825 and 1827.[3] It was 'afterwards given by Government to Mr David Douglas to take with him to the Columbia River' but Douglas had learned to observe with Sabine's even larger dip circle, a copy of which Sabine then produced for him and swapped for the 11 inch Dollond.[4]

Sabine had sent the instrument and its needles to Lloyd for inspection. Lloyd was 'quite at a loss what to say about them'. The error due to friction in the instrument was far higher than that of Lloyd's own 'small & light needles' and Lloyd says he 'never was so convinced of [their] superiority'. Though the eminent astronomer and director of the Brussels Royal Observatory Adolphe Quetelet had told Lloyd 'not to get a circle of less than 8 inches', Lloyd explains that he is guided by his own 'experience' in this matter and believes Sabine would be of his opinion had he 'taken a <u>single observation</u> with my little

[1] Lloyd to Sabine, 24 January 1834, TNA BJ 3/7/5.
[2] Sabine to Lloyd, 28 July 1834, RS MS/119/I/3.
[3] Franklin, J., *Narrative of a second expedition to the shores of the Polar Sea in the years 1825, 1826, and 1827* (Johhn Murray: London, 1828), xvi.
[4] Franklin, *Narrative*, xvi.

4½ circle'.[1] The dip circle that Lloyd refers to here is likely that of Thomas Charles Robinson's construction, an instrument maker who worked out of Devonshire Street, London.[2] It is referred to as such in Sabine's memoir of the BMS in 1838.[3] Similar, though not exact, examples of such an instrument now reside on the shelves of the Science Museum's stores at Blythe House in London.[4] They are rather unassuming objects: light, of simple construction, much less visibly robust than the dip circle produced by Robert Were Fox in 1832 and much less cumbersome than an 11-inch Dollond circle, constructed in the 1820s, which also sits on the shelves at Blythe House. Lloyd's Robinson circle was evidently made to be easily transported, but not to withstand the rigours of observations on ships – as the Fox-type circle was – and so was ideally suited, as Lloyd makes clear, to the BMS and, specifically at his time of writing to Sabine, to Lloyd's survey work in Ireland. Or so Lloyd thought.

Lloyd encountered several instances of error in his dip circles during this early portion of the BMS. Sabine and Ross were to meet similar difficulties with their own instruments. In fact, such were their problems, Sabine was compelled to remark, in the final publication of the BMS, that the Irish results 'are those which were the earliest obtained … which had consequently the disadvantages of less experience in the observers, and less perfection in the instruments'.[5] One such instance of instrumental error occurred within Lloyd's 1835 Irish series, although Lloyd did not realise this until 1837. Lloyd was at this time revisiting his earlier work, in order that he might provide Sabine with 'some postscript on the subject of the Irish lines' for the cumulative report on the BMS that Sabine was putting together for the BAAS.[6] 'Let me tell you of a magnetic mishap which has occurred to me', Lloyd wrote to Sabine, 'which had well-nigh thrown discredit upon all my dip circle observations'. The problems had arisen when Lloyd had purposefully destroyed the balance in two of his dipping needles, 'so that they rested nearly in the horizontal position' and could therefore be used for intensity measurements.[7] The results procured with the instrument after this time appeared to Lloyd so anomalous that he was 'compelled to reject them altogether'.[8]

[1] Lloyd to Sabine, 24 January 1834, TNA BJ 3/7/5.
[2] McConnell, A., *Geophysics and geomagnetism: catalogue of the Science Museum collection* (HMSO, Science Museum: London, 1986), 30.
[3] Sabine, 'A memoir', 68.
[4] The Science Museum holds 6-inch Robinson dip circles, such as Phillips used in his dip observations in England in 1837 and Ross in Scotland in 1838. Object Number 1876-789.
[5] Sabine, 'A memoir', 188.
[6] Lloyd to Sabine, 3 August 1837, TNA BJ 3/7/40.
[7] Sabine, 'A memoir', 106.
[8] Sabine, 'A memoir', 106.

In 1837, Lloyd wrote out these observations again on paper and took a 'good stare at them'. The result of this inspection was 'the conviction that the varying positions of the needle could not be the result of the Earth's magnetism & gravity alone & that some disturbing force had intervened'. Lloyd 'could think of nothing likely to produce these effects unless magnetism in the dipping apparatus itself – and on putting [this] hypothesis to the test of experiment it was fully verified!' Lloyd found the magnetism 'was greatest in the graduated limb, the very part in which, from its proximity to the needle, it must operate most powerfully'.[1] This was a mortifying realisation, as 'it was possible that all [his] observations might have been so much lost labour & that [his] share in the Irish lines only serve to misplace them!'.[2] Lloyd had then to consider the 'painful question' of 'how far the numerous results obtained with this instrument were vitiated by this newly-discovered source of error' and, if so, what the probable limits of error were.[3] It is worth noting here that the use of a magnetic instrument was not solely limited to observations in the field. Its journey in the hands of its user continued in the later assessment of the veracity of the data produced. In other words, one's journey (the instrument's) did not end when the other's (data) began. Instruments and the data they captured were viewed side-by-side and one could only be assessed proximate to the other. In writing the history of early nineteenth-century British magnetic science it is important to similarly account for both the instrument and the data, and not to construct an artificial divide between the two. This is an idea that will be returned to in the final chapter of the thesis, which discusses magnetic data and reference instrumentation in greater detail.

For the avoidance of a calamitous mistake, Lloyd was forced to take apart his dipping apparatus and perform a series of experiments on it to determine the strength of the magnetism in various parts of the instrument. Lloyd gave this account of his experiment:

> I separated the divided circle from the apparatus, and placed it on a horizontal support of wood. Three strong pins in contact with the inner edge of the limb, and dividing it equally, were then driven into the support, so as to prevent the limb from having any motion, except one of rotation in its own plane. A magnetic bar, whose length was nearly equal to the diameter of the circle, was then supported delicately within it, and the deviation of the bar from its undisturbed position was observed in the different positions of the limb with respect to it. It was thus found that most parts of the limb exerted a sensible disturbing effect upon the needle … a detailed examination of the effects in this position showed that there was a disturbing centre

[1] Sabine, 'A memoir', 107.
[2] Lloyd to Sabine, 12 October 1837, TNA BJ 3/7/42.
[3] Sabine, 'A memoir', 107.

> of ferruginous matter in the neighbourhood of each of these points, and that it was to the action of these centres that the anomalies in the observations … alluded to were owing.[1]

This was a troublesome experiment but one necessary to the correction of his results.[2] In this experiment, Lloyd disassembled and reassembled his knowledge of the dip circle anew and through so doing worked towards a solution for one of the dip circle's several possible fallibilities. In the end, Lloyd salvaged his results and in doing so urged others, particularly Ross, to test for a similar source of error in their own dip circles. Lloyd highlighted the circle Ross used at Westbourne Green in 1835 as Lloyd believed that some of the 'discordant results' Ross had obtained with it might be explained this way and similarly made credible.

More and more in the eighteenth and early nineteenth centuries, the epistemic authority of natural philosophers and the results they published were contingent upon the instruments they used: on their 'manufacture, usage and institutional association' as Withers put it.[3] As Naylor has also made clear, the determination of an instrument's accuracy in the early nineteenth century was to a large extent reliant on the person or persons operating them. Instruments and their users had to undergo a process of trial and negotiation before their credibility as a reliable instrument, and a reliable observer, could be known.[4] This process was at work, in the subject of terrestrial magnetism, during the BMS.

At the beginning of the BMS, Lloyd was a relative novice in the field of geomagnetism, having primarily been concerned with physical optics before this time.[5] Though obviously cognisant of geomagnetism by 1834, he still considered himself 'the pupil' to Sabine, his 'master on these subjects'.[6] A strong dose of modesty undergirds this statement, but it was not without some truth. By 1840 however, Lloyd was known – at least by James David Forbes, Professor of Natural Philosophy at the University of Edinburgh – as the 'British

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[1] Sabine, 'A memoir', 107.
[2] Sabine, 'A memoir', ; see also Reeves, N., '"To demonstrate the exactness of the instrument": mountainside trials of precision in Scotland, 1774', *Science in Context* 22, 3 (2009), 323-340, 329.
[3] Withers, C. W. J., 'Science, scientific instruments and questions of method in nineteenth-century British geography', *Trans. Inst. Brit. Geog.* 38 (2013), 167-179, 170; for a parallel case in a different context, see Sibum, H. O., 'Reworking the mechanical value of heat: instruments of precision and gestures in early Victorian England', *Studies in the Hist. and Phil. of Sci.* 26, 1 (1995), 73-106.
[4] Naylor, S., 'Weather instruments all at sea: meteorology and the Royal Navy in the nineteenth century', in MacDonald, F., and Withers, C. W. J. (eds), *Geography, technology and instruments of exploration* (Ashgate: Farnham, 2015), ch. 4.
[5] O'Hara, J. G., 'Lloyd, Humphrey (1800-1881), physicist and university administrator', *ODNB*, OUP, 2004, online edn, http://www.oxforddnb.com/view/10.1093/ref:odnb/9780198614128.001.0001/odnb-9780198614128-e-16840?rskey=9LvMuQ&result=1 [accessed 8 December 2017].
[6] Lloyd to Sabine, 24 January 1834, TNA BJ 3/7/5.

Oracle' on the subject of geomagnetism and expert on the instruments of its study.[1] This was a considerable leap. In part, it can be attributed to Lloyd's theoretical exchanges with Carl Friedrich Gauss during the 1830s.[2] Knowledge of the system of observatories Gauss and Weber set up in 1834 to accommodate the simultaneous observation of terrestrial magnetism in Europe was formative for Lloyd, their results of the 'highest interest'.[3]

However, it was Lloyd's experience of personal involvement in a magnetic survey that translated this knowledge into practical understanding. Lloyd, writing at the end of his series of Irish observations, remarked to Sabine that were he to undertake the work again he would adopt the precaution of taking 'contemporaneous observations … at some fixed station with a standard needle' as 'the only way of eliminating the irregular fluctuations' which Lloyd felt were 'very considerable' and made correct observation difficult.[4] Although he was well aware of Gauss and Weber's system at this time, he needed to experience working with magnetic instruments and undertake magnetic survey work to understand what was required in successful geomagnetic science. His experience of travelling with, handling, using, altering and testing magnetic instruments during the course of the BMS was a critical part of Lloyd's terrestrial magnetic education: one which – alongside his theoretical exchanges with Gauss – enabled him to become such a feted geomagnetic investigator. Lloyd's experience here, and the educative effect it had for him, is one more indication of the need, called for by Schaffer, to study instruments in their varying states of disrepair.[5]

The trouble with needles

Sabine, unlike Lloyd, was a renowned magnetic observer by the time of the BMS, having been attached as a scientific officer to several expeditions from 1818.[6] He had travelled to, and used magnetic instruments in, some of the most extreme climates in the world. However, his ability as a magnetic surveyor and his understanding of the instruments at his disposal was still tested during the exigencies of the BMS, as this section will demonstrate.

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[1] James David Forbes to Lloyd, 19 March 1840, St Andrews University Library, msdep7, Letterbook III, 84-86, quoted in Carter, 'Magnetic fever', 17.
[2] O'Hara, 'Gauss and the Royal Society'.
[3] Lloyd to Sabine, 17 November 1835, TNA BJ 3/7/22.
[4] Lloyd to Sabine, 17 November 1835, TNA BJ 3/7/22.
[5] Schaffer, 'Scientific instruments'.
[6] A full list of Sabine's expeditions can be found in Good, G. A., 'Sabine, Sir Edward (1788-1883), army officer and physicist', *ODNB*, OUP, 2004, online edn, http://www.oxforddnb.com/view/10.1093/ref:odnb/9780198614128.001.0001/odnb-9780198614128-e-24436?rskey=pQ34J3&result=1 [accessed 8 December 2017].

In the later magnetic crusade, needles and instruments would have to travel thousands of miles to stock observatories and accompany surveyors. But travel on any scale could unduly affect an instrument or a needle's ability to work correctly. Sabine experienced this almost as soon as his Scottish survey began in 1836. For this series, Sabine planned to use only one needle.[1] It was kept in a case 'securely and immoveably [sic.]' although 'the soft iron keep which connected its poles' did allow for a certain amount of spring owing to its own elasticity.[2] On disembarking from the steamer which had carried him on the short journey between Helensburgh and the island of Great Cumbrae in the west of Scotland, 'there being a good deal of sea, the case containing the needle fell from the table to the deck'. The fall occasioned a slight jar to take place: slight, 'but still sufficient to be audible'.[3] Sabine was immediately suspicious that even such a minor accident might have affected the needle. He was proved right in this after his next set of observations, which showed a 'greater difference from the Helensburgh results than was likely to be due to the geographical distance between them'. The needle's natural magnetic degeneration had been accelerated or, as Sabine put it, the needle had been brought 'at once to its permanent state'. Comparison of results at Dublin at the end of the series was the final confirmation Sabine needed: it showed Sabine that his needle had indeed suffered a severe and immediate change to its magnetic strength. The garden of Trinity College, Dublin was the place Sabine had established as his base station for the Scottish series prior to his arrival at Helensburgh and the place to which he had to return to test the veracity of his observations.

Several things can be gleaned from this incident. First, it demonstrates the importance of establishing particular places to stand as base stations. The credibility of results relied on this in the science of terrestrial magnetism because of the degradation of strength experienced by the tools of its trade: needles. These sites came to act as examples of Gieryn's 'truth spots': sites which were understood to 'lend a special credibility to scientific claims'.[4] During the BMS, base stations were not (yet) observatories but more public, less controlled or controllable spaces such as gardens and parks. They were though a fixed point in time and space by which a needle's state could be assessed and results compared to a standard. Base stations were the means by which observations in different locales could be translated into one common set of results. Field-based observations did not travel from the spot on which they were made to the map, the table, or the page on

[1] Sabine, 'A memoir', 106.
[2] Sabine, 'Observations on the direction', 106.
[3] Sabine, 'Observations on the direction', 106.
[4] Gieryn, T. F., 'City as truth-spot: laboratories and field-sites in urban studies', *Social Studies of Science* 36, 1 (2006), 5-38, 5; and Gieryn, T. F., 'Three truth-spots', *Journal of the History of the Behavioral Sciences* 38, 2 (2002), 113-132.

which they were represented without first being compared and reduced in accordance with observations from a predetermined base station. These base stations were the necessary space through which instruments, users and their observations travelled and were scrutinised before their trustworthiness and veneer of universality could be applied.

Furthermore, Sabine's troubles remind us that needles did not always travel well. They were susceptible to even the slightest of jars. Because of the nature of a magnetic needle, their users only came to understand them 'by degrees' and over time, as Phillips once remarked to Lloyd.[1] By this, Phillips meant that it took time for an observer to understand what sort of needle he was in possession of: what the strength of its magnetism was, how gradually its strength declined and how well it performed on the move. Such an appreciation could only be arrived at over time and with frequent comparisons of observations made at a specific place.

Understanding that needles and instruments could be altered by travel also draws attention to the skills and experience that a geomagnetic observer needed to have to observe correctly. An observer needed to know when an instrument was and was not working correctly and what needed to be done to resolve mechanical problems. Lloyd, and to an extent Sabine, gleaned such knowledge during the BMS and their time spent handling magnetic instruments. This was not information that could be easily absorbed and put into practice through reading instructions alone; nor could it easily be taught in a few days. As is explained in greater detail in the chapter on the colonial observatories of the later British magnetic scheme, learning the art of magnetic observation was an embodied and sensuous practice that relied on literally handling instruments and learning their idiosyncrasies.

Successful observation of terrestrial magnetism, as Professor Charles Daubeny explained in his opening address to the British Association's 1836 meeting, was a terribly difficult thing to achieve. It required the collection of data 'from such a variety of isolated points, distant one from the other, both in time and place', which were 'dependent for their accuracy upon the occurrence of favourable circumstances' and demanded from the observer 'an uncommon union of skill and experience'.[2] Observers needed practical experience to be educated in such a difficult science. For its participants, the BMS provided such an education.

[1] Phillips to Lloyd, 23 July 1837, RS MS/119/I/32.

[2] Daubeny, C., 'Address by Professor Daubeny', in Anon., *Report of the Sixth Meeting of the British Association for the Advancement of Science* (John Murray: London, 1837), xxi-xxxvi, xxiii.

Instrument makers and the BMS

Education in geomagnetism was not just available to the observers of the BMS. Instrument makers also developed their craft at this time. Lloyd's experience of finding that the limbs of the dip circle he had used in Ireland had been magnetised led him to question the workmanship of English instrument makers. He believed French makers were more careful than English dip circle manufacturers to avoid crafting dip circles liable to exhibit such a 'vice'.[1] By 1838, and the end of the BMS, this opinion had changed, and Lloyd was much more assured of English instrument makers' capabilities. During the course of the BMS, the instrument maker T. C. Robinson – who designed many of the English dip circles – was afforded the opportunity of developing his art and tweaking his dip circle construction. He did this in conjunction with James Clark Ross and Mr. Frodsham in London in 1837.[2] Though it was the cause of Lloyd's ire, the most frequent problem with Robinson's dip circles was not the presence of magnetism throughout its construction: it was the imperfect curvature of the axle, which meant the needle could not return to rest correctly.

On Ross's behest, Robinson had four needles made on the model of continental needles, believed to be of superior construction at that time, by which 'the axle, instead of being permanently fixed to the needle, was secured in its place merely by strong friction, and could be taken out, turned a portion of a circle on its own centre of rotation, and replaced'. Robinson and Frodsham – whose chronometers were 'so well known for their excellence' – each made an axle for these needles. After successive trials of the axles in different positions it was determined that 'Mr Frodsham's axle proved the best'. However, now Robinson 'with this experience', replaced the axles 'of the other three needles with three which should be the workmanship of his own hands'. The needles were again tested in different positions of the axle and the results, though not completely perfect, showed significant progress had been made.[3] These trials 'fully impressed Mr Robinson with the necessity of employing more effectual means for ensuring a true figure to the axles of dipping needles; and in several which he has since made, and which have been carefully

[1] Sabine, 'Observations on the direction', 106.
[2] It is not clear which of the Frodshams this was, as there were several working in the instrument trade at this time. It is likely to have been either William James Frodsham – who equipped Sabine with chronometers in 1822 and again in 1823 for Sabine's scientific work on the west coast of Africa and later Spitzbergen measuring the shape of the earth – or his son, Charles Frodsham, who had his own chronometer-making premises in London in the 1830s. See Mercer, V., *The Frodshams: the story of a family of chronometer makers* (The Antiquarian Horological Society: Kent, 1981), 8, 28, 76-77.
[3] All quotations from Sabine, 'A memoir', 56.

examined, he has proved successful'.[1] Results of experiments with these axles in June and July 1838 showed 'a great improvement' had been made in Robinson's circles.[2]

Dip circles of Robinson's construction would later take part in the magnetic crusade – on Ross's Antarctic expedition and at the colonial observatories – having been proved credible and trustworthy through trials staged because of the work of the BMS.[3] The 'discordant results' that Ross had made in Westbourne Green in 1835, which Lloyd had referred to in his letter to Sabine of 12 October 1837, had been made with a Robinson dip circle. Much more accurate observations at the same site in 1837 and 1838 by Robinson, Ross and Phillips with a Robinson circle were presented by Sabine in his 1838 BAAS Report on the BMS and showed 'how great an improvement has been effected in our English dipping needles since that period'.[4] The performance of the Fox-type dip circle – constructed in Falmouth by Thomas Brown Jordan – during the BMS was also praised and said to indicate 'the great care bestowed on their workmanship'.[5] The Fox-type's performance in Ireland in 1835 is one of the reasons John Franklin gave his support for it to be taken on an expedition to the Arctic that was commanded by George Back. Franklin, writing to Fox, expressed how 'pleased' he was 'with the result of your [Fox's] observations in Ireland' and with the modifications which Fox has made with the needle. Because of this, Franklin was 'convinced that the Instrument must be adopted when its comprehensive merits & uses are known' and trusted that Francis Beaufort – first secretary of the Admiralty – would 'yield to [his] solicitation and allow it to be taken'.[6]

The BMS was a testing ground in which magnetic instruments – particularly of English origin – made their reputation; when instruments such as Robinson's dip circles were literally and metaphorically deconstructed, in order to have their reliability re-established anew.[7] Sabine remarked on this again in a letter to John Herschel in July 1839: 'The dipping needle is much improved of late years', Sabine assured Herschel, as shown by its performance at Westbourne Green in 1837 and 1838 by those who 'have cooperated in the deduction of the Isoc[linal] & Isod[ynamic] lines in Britain'.[8]

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[1] Sabine, 'A memoir', 53.
[2] Sabine, 'A memoir', 54.
[3] McConnell, A., *Geophysics and geomagnetism*, 53; see also Savours, A. and McConnell, A., 'The history of the Rossbank Observatory, Tasmania', *Annals of Sci.* 39, 6 (1982), 527-564. Appendix 2 shows that two 9-inch Robinson dip circles were present at this observatory, 558.
[4] Sabine, 'A memoir', 56.
[5] Sabine, 'A memoir', 56.
[6] John Franklin to Robert Were Fox, 26 March 1836, Library and Archives Canada, MG 24 H67, quoted in Levere, 'Magnetic instruments', 62.
[7] Similarly, Fox's dip circle travelled to many different places, most notably Canada, before being used during the British magnetic scheme.
[8] Sabine to John Herschel, 12 July 1839, RS HS/15/46.

Site-specific experimentation

Westbourne Green crops up again and again during the BMS, as does Regent's Park. Both these London parks became intimately connected with the science of terrestrial magnetism. It was known, as Daubeny noted, that the execution of this science was predicated on the collection of data from a variety of different points, 'distant one from the other, both in time and place'.[1] But it also needed specific sites in which, and through which, the results of its labour could be made credible. It was to these places – Westbourne Green, Regent's Park – that instruments, of questionable reliability, were sent, where they were measured and where they later returned to have their reliability confirmed. The physical sciences in the nineteenth century more and more relied upon the credibility of instruments for their authority.[2] In the case of geomagnetism, its mobile instruments first had to spend time in a fixed location. It was then and it was there that instruments could be observed and their comparative reliability assessed.

The trial of Robinson's dip circles at Westbourne Green is one such example of what could be called the site-specific experimentation of terrestrial magnetic science. Westbourne Green was where anomalous results had first been discovered in James Clark Ross's series of dip observations in 1835 and was where his dip circle had to return in 1837 and 1838 for the improvement of Robinson's construction to be verified. Similarly, when Lloyd informed Ross in March of 1837 that he had developed a new method by which 'bad' observations of dip might be made into good 'true' ones, Ross proposed that 'the process should be tried upon his observations with different needles at Westbourne Green which give results so wide apart at present'.[3] In that instance it was not only the needles on trial, but a new method of Lloyd's creation. The trials could not have happened elsewhere. The venue in which the trials of Ross's dip circle and the new method of Lloyd's took place mattered. Westbourne Green was more than a pleasant backdrop for these trials; it was an active part of them. It was the place that allowed Lloyd's method, Ross's dip circle, to escape place. Following Gieryn's argument, Westbourne Green acted as a 'truth-spot', or 'the place of provenance' that enabled the 'transit of some claims from merely local knowledge to truth believed by many all around'.[4] In order to prove that Ross's dip circle

[1] Daubeny, 'Address', xxiii.
[2] See, for instance, Withers, 'Science, scientific instruments and questions of method'; Bourguet, M. N., Licoppe, C., and Sibum, H. O. (eds), *Instruments, travels and science: itineraries of precision from the seventeenth to the twentieth century* (Routlegde: London, 2002).
[3] Lloyd to Sabine, 28 March 1837, TNA BJ 3/7/35.
[4] Gieryn, 'Three truth-spots', 113.

or Lloyd's method could be used "elsewhere" it had first to be proved in a specific "somewhere" – in this case at Westbourne Green.

Regent's Park, northeast of Westbourne Green, was another significant site for British geomagnetism. Sabine had observed there as early as 1821 to ascertain the absolute dip in London and it continued to serve as the location for this determination throughout the 1820s and 1830s. It was also where Sabine had tested Franklin's instrument and needles before the latter's Arctic expedition in 1825. Like Westbourne Green, it functioned as an authorised site for the testing of magnetic instruments. As we saw in the introduction to this work, it was in Regent's Park, during the BMS, that Sabine proved those needles which accompanied the Gambey dip circle in Canada with John Henry Lefroy to be 'free from index error at all inclinations'.[1]

In the BMS, Regent's Park was also returned to again and again in order to provide observations from other stations in the British Isles with a standardised comparison. In order to unite all the observers, who used different instruments and observed at different times of the day and of the year, in one complete survey, such a standard measurement at Regent's Park was necessary. There was even a specific space within this park in which observations needed to be made: the nursery garden.[2] It was important to adhere to such specifics. Ross had to delay answering Sabine's request for him to participate in the BMS in July 1834, because he first needed to find out whether he could 'have access to those parts of the Regent's Park where you [Sabine] have before made your magnetic observations'. Ross knew the importance of 'those parts'; he was not completely sure of his role until he could get to the same spot to observe.

In Dublin, terrestrial magnetic observation was undertaken in the Provost's Garden at Trinity College – specifically at the garden's centre point.[3] It was hoped that this spot would be far enough removed from disturbing forces that accurate observations could be made. It was where the absolute determination of the dip and intensity for Dublin – and Ireland as a whole – was made. This garden was therefore used as the standard comparison for other observations made during the Irish survey, just as Regent's Park had been for the English portion of the BMS. This was not a random choice; Sabine had come and looked at the spot when he and Lloyd were still only talking of making geomagnetic observations together.[4] Sabine evidently liked the spot for he gave it his blessing. Lloyd too was

[1] Levere, 'Magnetic instruments', 64.
[2] Sabine, 'A memoir', 141; and Sabine, E., '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', *Phil. Trans. R. Soc. Lond.* 112 (1822), 1-21, 8.
[3] Lloyd to Sabine, 6 May 1835, TNA BJ 3/7/19.
[4] Lloyd to Sabine, 6 May 1835, TNA BJ 3/7/19.

convinced of the authority of his spot. He assured Sabine that 'we may place much confidence in the final result' of his determination of the horizontal intensity in 1835 because it had been made in this space, which was jointly decided to be free of other disturbing influences.[1]

Through Lloyd and Sabine's discussions and the belief that the spot in the garden was far enough away from any magnetic material but still close enough to provide ready access, this site became an authorised place in which legitimate geomagnetic observations could occur. It was to this site, 'our old station in the Provost's garden' as Lloyd describes it to Sabine, that Alexander Dallas Bache - esteemed American physicist and one of the earliest and most influential proponents of geomagnetism in that country – came and observed in order to make a comparison of the horizontal force in Philadelphia and Dublin in late 1836.[2] When Lloyd was away from Dublin, Sabine made sure someone else gave him access to the Provost's Garden to allow him to make observations at the correct spot.[3] To have made them elsewhere in Dublin would have been pointless. They would not have been comparable; they would not have been as assured of their authority. Terrestrial magnetism was a global phenomenon but its accurate study in the early nineteenth century relied on the observer inhabiting such specific spaces. The argument here is the argument that threads its way through many geographies of science: 'that science depends on the manufacture and management of different spaces … to accomplish its objectives and establish its credentials'.[4]

Changing places

In July 1837, Lloyd drew up plans for his new magnetic and meteorological observatory in Dublin. He ordered instruments and sketched out how the space would be organised. Significantly, Lloyd told Sabine that the observatory would be built in the Provost's Garden, 'somewhere about the spot where you took your last observations', meaning the spot at which they had been making observations since 1835.[5] This site had been designated as the only place in which such observations could be made accurately, so it made sense to build the observatory there. It had been constructed as a legitimate site

[1] Lloyd to Sabine, 6 May 1835, TNA BJ 3/7/19.
[2] Lloyd to Sabine, 10 December 1836, TNA BJ 3/7/34.
[3] Sabine to Lloyd, 8 October 1836, RS MS/119/I/18.
[4] Finnegan, 'The spatial turn', 383.
[5] Lloyd to Sabine, 26 July 1837, TNA BJ 3/7/39.

through the observational work carried out during the BMS. The observatory was to bind up and wall in the site and by so doing bear the mark of this site's legitimacy.[1]

Regent's Park did not follow the same path; no geophysical observatory was ever built there. It existed as a venue for the production of credible geomagnetic science for a relatively brief period of time. Sabine had observed the dip there as early as 1821 and, between himself and Ross, at many times subsequently. The annual decrease of the dip over these years in England was computed through the comparison of observations at this place. Sabine calculated the rate of decrease in England between 1821 and 1837 to have been 2.4 minutes, a result that Sabine felt 'extremely unlikely to be more than a tenth in error'.[2] Sabine was confident in his claim to such a small degree of error. It might have something to do with Sabine's own bullish nature: he had always been a suspiciously accurate observer and probably wanted to remain as such.[3] It certainly does have a lot to do with the refinement of dip instruments by 1837, as has already been noted, and more experience in handling them. However, I argue that such confidence was also entwined with the site at which Sabine observed. Here, like the Provost's Garden in Dublin, was a spot that had been visited again and again during the 1830s and over the course of the BMS. It was a trusted site; if there were disturbing elements in the park they were known about and could be accounted for. It was a legitimate site for the observation of terrestrial magnetism and it gave Sabine confidence in the results achieved there.

At least it did for a time. Comparable observations made at Kew showed that the dip observed in Regent's Park was probably slightly higher than it ought to have been. By 1838, Sabine had lost confidence in his Regent's Park observations because of the Kew results. Although the locality made the 1821 and 1837 dip results more 'strictly comparable', Sabine now felt that it was not a site 'in which we can feel confident that no change may have occurred in regard to magnetic influence'. Sabine concluded that, in 1838, 'the Regent's Park is … not so eligible a situation…for magnetic experiments as it was in 1821'.[4] The built environment of London had encroached on the site and Regent's

[1] For images of the Dublin Observatory as well as a short history of its life, see Robinson, P. R., 'Geomagnetic observatories in the British Isles', *Vistas in Astronomy* 26 (1982), 347-367, 348, 355.
[2] Sabine to Lloyd, 23 November 1837, RS MS/119/I/42.
[3] Charles Babbage was a fierce critic of Sabine's near-perfect pendulum experiments in the 1820s. See Babbage, C., *Reflections on the decline of science and on some of its causes* (R. Clay: London, 1830); see also Anderson, K., *Predicting the weather: Victorians and the science of meteorology* (University of Chicago Press: Chicago and London, 2005), 143-145 for further, later, examples of Sabine's suspiciously precise results.
[4] Sabine, 'A memoir', 64.

Park had shown itself to be insufficiently capable of keeping the outside world and its magnetic influences out.[1]

Regent's Park, Westbourne Green and Trinity College were terrestrial magnetic 'truth-spots', at least for the majority of the 1820s and 1830s. These were sites that brought certain actors together – in this case observers and instruments – and facilitated certain practices necessary to the construction of trust in this science.[2] It was such 'situated practical activity' that allowed knowledge claims to be made in a form that made them credible elsewhere.[3] However, these were also mutable spaces. They were not 'rigorously guarded'; they existed somewhere between the public and private domain, field and observatory science.[4] In the case of Regent's Park, the public domain expanded too far and irreversibly changed its suitability as a site for terrestrial magnetic observation. In the Provost's Garden this was prevented by the erection of physical walls to the outside world. Lloyd's observatory provided a new, safe, controlled environment for the observation of the earth's magnetism. Other geomagnetic or more broadly geophysical observatories were built around the same time or slightly later than Lloyd's in Britain and elsewhere in the world.[5] These observatories, and the practices they engendered, have been the focus of significant recent studies.[6] The BMS reminds us that before such institutions housed regular terrestrial magnetic research, observers had to construct their own, more fluid, more public, places that could lend their observations the credibility they required.

Putting to print

> To alter it now, would not be breaking a single limb, but would be disjointing every bone in my body.[7]

[1] On another example of keeping out disturbing influences, see Forgan, S. and Gooday, G., '"A fungoid assemblage of buildings": diversity and adversity in the development of college architecture and scientific education in nineteenth century South Kensington', *Hist. of Universities* 13 (1994), 153-192.
[2] Gieryn, 'Three truth-spots'; Gieryn, 'City as truth-spot'; and for further analysis, Naylor, S., 'Introduction: historical geographies of science: places, contexts, cartographies', *Brit. J. Hist. Sci.* 38, 1 (2005), 1-12, 6.
[3] Powell, R. C., 'Geographies of science: histories, localities, practices, futures', *Prog. Human Geog.* 31, 3 (2007), 309-329, 312.
[4] Kuklick, H., and Kohler, R. E., 'Introduction', in Kuklick, H., and Kohler, R. E. (eds), *Science in the field*, *Osiris* 2nd series, 11 (Chicago, 1996).
[5] Robinson, 'Geomagnetic observatories'.
[6] Aubin, D., Bigg, C., and Sibum, H. O., *The heavens on earth: observatories and astronomy in nineteenth-century science and culture* (Duke University Press: Durham and London, 2010); MacDonald, L. T., 'Making Kew Observatory: the Royal Society, the British Associaiton and the politics of early Victorian science', *Brit. J. Hist. Sci.* 48, 3 (2015), 409-433.
[7] Sabine to Lloyd, 8 April 1839, RS MS/119/I/49.

The above was Sabine's pained response to Lloyd's questioning of the proper weighting of Ross's observations of dip recorded at Westbourne Green, results which were to be included in the final report on the BMS. The report, Sabine wrote, was already with the printer and had, by this stage, already been through several revisions, many of which had been suggested by Lloyd. Lloyd had, variously, 'changed the value' of all his Irish dip results and doubled up two tables into one with the consequence of throwing off the numbering of all the other tables in the report and reweighted results made with his (Lloyd's) own needles. Sabine's dismay was palpable:

> The days of labour, and days of delay, which these successive changes oblige, are no longer mine to give. My nights have been for some time broken & my health is sinking in the vain attempt to complete a report … [and] I am now … come to the point at which I must say stop! … I have carefully abstained from expressing the downright wretchedness which all these changes & delays occasion … & I am sure you do not suspect a fiftieth part of it, or you would I am sure, from the kindness of your disposition, feel that these refinements may be bought too dear.[1]

In a previous letter, Sabine had similarly expressed his consternation and anguish at a suggested revision of Lloyd's urging, on the subject of how to weight the different stations in which observations of intensity had been taken. Lloyd, it would appear, had had a change of mind and thought that stations at which two different needles had been used for stations that had been visited on multiple occasions should be weighted double in comparison to other stations at which neither of these had occurred, because of the consequent diminished instrumental error. Sabine countered that the 'probable error of stations far far exceeds the probable error of observations' and that the report had not been written with Lloyd's new idea in mind. To alter how they had dealt with the stations while the report was ready to be printed would be 'days & days of labour' and 'all for what!' asked Sabine. To double a certain station's importance would only, exacerbate, Sabine alleged, the problem caused by the fact that, for example, Ross's Irish observations had been taken mainly at stations in the south of the country.[2] Sabine's criticism of Lloyd's new plan actually foreshadowed criticism of both the first and second British magnetic surveys – on both of which Lloyd and Sabine participated – by later geomagnetic

[1] Sabine to Lloyd, 10 June 1839, RS MS/119/I/50.
[2] Sabine to Lloyd, 8 April 1839, RS MS/119/I/49.

surveyors of Britain.[1] Ironically, the lack of geographical distribution between Sabine's English stations in the second BMS was particularly singled out for criticism.[2]

Eventually, in midsummer 1839, Lloyd sent Sabine his last manuscript of revisions which, he wrote, concluded his 'unhappy portion of the memoir'.[3] If Sabine had been dismayed to receive so many revisions from Lloyd, Lloyd was equally dismayed to have had to make them. In one particularly unhappy letter, Lloyd had written to Sabine concerned that *all* of the results from *each* station might have to be recalculated, in order to be weighted with a greater emphasis on station error rather than on the error of observation, because their emphasis on the latter had led them to give double or sometimes treble weight to some stations 'merely because different observers have been there with different instruments'.[4] In other words, Lloyd felt that he and Sabine had reduced the observations of the BMS with too much emphasis on the potential errors of each observer: the so-called 'personal equation', although Lloyd did not use that exact term. The personal equation, as Schaffer has explained, appeared in the early nineteenth century as a 'label for the worrying fact that astronomers seemed to differ from each other in the times they recorded for transits'.[5] A host of social and material technologies – such as the introduction of practices indistinguishable from the accounting office and the commodification and control of time within the observatory – were developed that disciplined and ordered the observer in order to calculate and mitigate the personal equation.[6] Outwith the observatory, this marshalling of the personal equation was difficult to manage, especially with individuals working on an often ad-hoc basis, such as Phillips, and with different instrumentation and different needles. The disciplining of the observer could only be achieved retrospectively, through the interchange of needles and data, and the later reduction of data to common times and base stations from which some sort of accepted standard of accuracy could be achieved. The only 'proof' that Phillips could offer Lloyd to attest to the accuracy of his observations and of his skill as an observer was his little book of observations, from which had to be extracted both Phillip's range of probable error in recording observations and the probable error due to the irregularities of the locality in which observations were taken: the personal and the geographical equation.[7]

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[1] Rücker, A. W., and Thorpe, T. E., 'III. The Bakerian lecture: A magnetic survey of the British Isles for the epoch January 1, 1886', *Phil. Trans. R. Soc. Lond. A* 181 (1890), 55.
[2] Rücker and Thorpe, 'A magnetic survey', 293.
[3] Lloyd to Sabine, 13 June 1839, TNA BJ 3/9/104.
[4] Lloyd to Sabine, 7 May 1839, TNA BJ 3/9/91.
[5] Schaffer, S., 'Astronomers mark time: discipline and the personal equation', *Science in Context* 2, 1 (1988), 115-145, 116.
[6] Schaffer, 'Astronomers mark time'.
[7] Phillips to Lloyd, 23 July 1837, RS MS/119/I/32.

Lloyd would eventually capitulate, and admitted that extra weighting by station was in fact 'scarcely advisable in the case of different observers because though the station is the same, the individual place of obs[ervatio]n is sure (almost) to be different. And', crucially, he added, 'we have as yet no data to determine within what limits local errors usually prevail'.[1] However difficult it was to calculate and mitigate against the idiosyncrasies of individual observers and different constructions of magnetic instruments in the reduction of observations for the final report of the first BMS, it was apparently easier than knowing the precise geology of each station and the character of the exact spot where the instrumentation was set up for observation. Still, this is not to allege that the character – including sometimes the geology where known – of each spot was not recorded by the observers. It was simply, as Lloyd noted, that their data were sometimes cruder than the data they possessed on most of the individuals and instruments that had been tested on multiple occasions in places such as Regent's Park and Westbourne Green. It was perhaps in part as a result of the lack of definitive corroborative geological and geomagnetic data for the British Isles that Robert Were Fox drew isodynamic lines onto a copy of John Phillips's *Geological Map of the British Isles*, produced in c.1838. We might speculate that Fox was, in overlaying these visualisations of magnetic lines and geological strata, trying to uncover a greater understanding of how local station errors had affected the results of the magnetic survey of which he had been a significant part. Whether this was the case, the map by itself is an effective visualisation of the BMS and what it was enacted to achieve. The map represents all the labours of Sabine, Lloyd, Ross, Phillips and Fox and the way in which they had attempted to create a kind of localised *physique du globe*. In surveying the magnetism of the British Isles, they had also been at the mercy of, and tried to accommodate and record alongside their magnetic measurements, different meteorological, climatic and geological conditions.

The BMS was not a planned precursor to the magnetic crusade, to allege otherwise would be anachronistic. In the 1830s, individuals such as Sabine and Ross certainly harboured more extensive geomagnetic plans, but the government's decision to back such plans was not granted until after the observations of the BMS had been taken. However, the report of the BMS was still an important pedagogical tool for the later crusade. Officers at the colonial observatories were furnished with 'copies of the magnetic survey' and, given that Lloyd wrote this in late 1839 and that the BMS was the first national survey of its kind, it seems more than likely that the magnetic survey to which Lloyd referred was the report of

[1] Lloyd to Sabine, 11 May 1839, TNA BJ 3/9/93.

the BMS lately printed.[1] Captain C. M. Elliot, the EIC officer originally charged with superintendence of the Singapore Observatory in the EIC's portion of the British magnetic scheme, consulted a copy of the report of the BMS in the reduction of his 'Magnetic Survey of the Eastern Archipelago', 1846-1849. He was 'indebted', he wrote, to Lloyd and Sabine's 'joint Report on the Magnetic Isoclinal and Isodynamic Lines in the British Islands' because he had adopted the method employed in that report to compute the magnetic lines drawn in his chart of the eastern seas.[2]

Although getting the report of the survey into an agreed version appropriate for print was an overwrought and protracted affair, the report was eventually published in 1839 by the BAAS in their annual publication. The BMS report, or 'memoir', of the survey that Sabine and Lloyd produced amounted to 147 pages roughly divided into sections concerning the dip, variation and intensity of the earth's magnetic force in England and Wales, Ireland and Scotland. The report was not only important as 'having been the first complete work of its kind planned and executed in any country as a national work, coextensive with the limits of the state or country … embracing the three magnetic elements'.[3] It was also, as its distribution to the colonial observatories and its use by Elliot demonstrates, a teaching tool that communicated information regarding the experience of making observations in the field, uniting observations with a base station and the method used to calculate isodynamic and isoclinal lines. As Sabine later remarked, the first BMS also presented an example which was 'speedily followed by the execution of similar undertakings' in other parts of the globe, such as the 'Austrian and Bavarian dominions' and precipitated the 'construction of general magnetic maps of the globe' and supplied the 'best kind of data' for the elucidation of the secular change in the distribution of the earth's magnetic field.[4]

The magnetic survey of Britain, 1833-1838, was not the only one to be undertaken in the nineteenth century. Two more were completed in the British Isles before the century was out. The second such survey was again requested by the BAAS and coordinated by Sabine, in stages, between 1856 and 1861. Sabine observed in England in the summers between 1858 and 1861; Lloyd again had charge of the Irish segment along with, at his request, Professors Galbraith and Haughton and George Johnstone Stoney Esq.; and 'Scotland and the islands to its North and West were placed, with the consent of the Committee of the Kew Observatory, in the able hands of Mr. [John] Welsh, the superintendent of that

[1] Lloyd to Sabine, 11 December 1839, TNA BJ 3/9/122.
[2] Elliot, C. M., 'Magnetic survey of the Eastern Archipelago', *Phil. Trans. R. Soc. Lond.* 141 (1851), 287-331, 289.
[3] Sabine, E., 'Report on the repetition of the magnetic survey of England, made at the request of the General Committee of the British Association', *Report of the British Association for the Advancement of Science* 31st Meeting (1861), 250-279, 250.
[4] Sabine, 'Report on the repetition of the magnetic survey of England', 250, 251.

establishment'.[1] Sabine and Lloyd (with his associates) reduced their portion of the observations and Frederick John Evans, superintendent of the Compass Observatory of the Royal Navy at Woolwich at this time also helped to draw up the isogonic lines (lines of equal declination), compared these lines with those of the former British survey and deduced the mean secular change of the declination in the time between the two surveys. Welsh was unable to reduce his portion of the observations on account of his death in 1859, 'accelerated it is feared by his too persistent exposure in the second year of the Scottish Survey'.[2] Welsh undertook his portion of the survey with 'accuracy and completeness', Sabine later wrote, 'and with a devotion which was but too great'.[3] Balfour Stewart was therefore left with the task of reducing and publishing the observations in the report of the BAAS for 1859, observations that did more for Scotland than the survey of twenty years hence, according to Stewart.[4] Stewart's supposition is borne out in part by a map held in the archives of the British Geological Survey that shows each station observed at in the first and second British surveys and how many times these individual stations had been used. The stations are marked in pencil on what amounts to tracing paper, with different colours used to demarcate which of declination, inclination and intensity was observed at each place. When compared to the map engraved for the final memoir of the first survey, it is clear that Welsh had indeed written Scotland, especially the north of Scotland, more firmly into the magnetic geography of the British Isles (fig. 4.1).[5]

Conclusion

It should now be clear why in 1846 Sabine felt it necessary to remark that the needles for the Gambey dip circle that Lefroy used in Canada had been proved free of error through observations made as part of the BMS in Regent's Park. Needles, it was known, had to be 'well trained' before they could be trusted.[6] It was appropriate that Sabine referred to the specific place – Regent's Park – in which this proving took place. Regent's Park, like

[1] Sabine, 'Report on the repetition of the magnetic survey of England', 251.
[2] Sabine, 'Report on the repetition of the magnetic survey of England', 251.
[3] Sabine, E., 'Contributions to terrestrial magnetism. No. XII. The magnetic survey of the British Islands, reduced to the epoch 1842', *Phil. Trans. R. Soc. Lond.* 160 (1870), 265-275, 266.
[4] Stewart, B., 'On some results of the magnetic survey of Scotland in the years 1857 and 1858, undertaken, at the request of the British Association, by the late John Welsh', *Report of the British Association for the Advancement of Science* 29th Meeting (1859), 167-190.
[5] British Geological Survey [BGS], National Geological Records Centre, Container [Con.] 80001374, Item MHM – 85007128.
[6] Sabine to Lloyd, 9 February 1837, RS MS 119/I/26.

Fig. 4.1 Detail of a map of the stations visited in the first and second magnetic surveys of Britain. Each vertical line indicates an observation made during the first BMS and each horizontal an observation made during the second BMS. BGS Con. 80001374 Item MHM – 85007128. Author's photograph.

Westbourne Green and the Provost's Garden in Trinity College, Dublin, were the spaces in which the science of terrestrial magnetism was anchored; where credibility could be ascertained and authority lent to the needles or instruments that passed through there before institutions such as the Kew physical observatory were established and used for these ends. In conjunction with observers, instruments and instrument makers during the BMS, these sites helped to confer legitimacy on the science of terrestrial magnetism in the first half of the nineteenth century. Many of the instruments that travelled and were a key part of the later magnetic crusade – Robinson's dip circles, Gambey's dip circles and Fox's dip circle – were assessed during the BMS and at these specific sites. However, that is not to say that this was the only arena in which this happened – Fox's circle travelled to Canada during the 1830s with George Back as well.

The discussions between Lloyd and Sabine on how to publish the final report of the first BMS demonstrate that the art of geomagnetic surveying was not one of simply placing an instrument on the ground, noting the oscillations of the needle and then moving onto the next spot. Geomagnetic surveying was a practice that involved knowing much more than just how to observe a magnetic instrument. It required at least a basic understanding of the landscape and the geological situation in which the instrument was to be placed and the compilation of an accurate record of the character of different spots and the different atmospheric and climatic conditions at the time of observation. Coordinating and reducing the results of this cooperative survey also meant understanding the different amounts of probable error occasioned by individual or instrument or station. The individual, the instrument, the choice of location, all could confer a kind of legitimacy or trustworthiness to the observations recorded, but only when each of these were known and the idiosyncrasies of each calculated and weighed one against the other. The discussion between Sabine and Lloyd on this point, and the fact that Lloyd nearly caused the rewriting of their entire final report because he could not conclusively decide between weighting by individual or by station error, demonstrates how it was during the first BMS that questions on best geomagnetic surveying practice were posed and some solutions offered. It was, as Sabine was always quick to point out in his papers, the first national work of its kind and so it was perhaps to be expected that questions about how best to perform and present this sort of geomagnetic survey would arise. The first BMS helped to shape the practice of geomagnetic surveying and would become an important pedagogical tool for the officer-observers of the later magnetic crusade and a catalyst for the further elucidation and drawing of Britain's magnetic cartography. In their 1981 book on the BAAS, Morrell and Thackray rightly point out that prior to 1839, geomagnetic activity in Britain was primarily concerned with the perfecting of magnetic instruments.[1] By foregrounding the instruments as the locus of this investigation, we are now better able to appreciate the how and the where of this perfecting.

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[1] Morrell and Thackray, *Gentlemen of science*.

Chapter 5: Instruments on the move in the North American Magnetic and Meteorological Survey, 1843-44

> Our first carrying place was here; the distance trifling. Off shoes and stocking and over the rocks and through the stream carrying Barometers and such precious things.[1]

Introduction

Terrestrial magnetic observation was undertaken during the magnetic crusade at selected sites across the globe and on a number of mobile surveys. The mobile and the static observations were to be made, as much and as near as possible, simultaneously. Perhaps the most vaunted survey of the crusade – certainly in the 1840s at least – was James Clark Ross's voyage of expedition to Antarctic waters. Ross's voyage arguably marked the beginning of the British magnetic scheme. It had launched from Margate on 30 September 1839 as both a surveying voyage and as a means of conveyance for the instruments and personnel required to establish observatories in St Helena, the Cape of Good Hope and Van Diemen's Land.[2] However, Ross's was not the only mobile survey which contributed magnetic data to the crusade. Several others did. For one, in 1844, Lieutenant Henry Clerk of the Royal Artillery and Captain T. E. L. Moore of the Royal Navy embarked on an expedition in the hired bark *Pagoda* to complete a 'magnetic survey of a portion of the Southern Hemisphere which had not been included in Sir James [Clark] Ross's expedition'.[3] The other, and the focus of this chapter, was John Henry Lefroy's survey of geomagnetism and meteorology in British North America, Hudson's Bay Company (HBC) possessions and the Northwest Territories, today collectively known as Canada, which took place between May 1843 and November 1844. In avoidance of both this convoluted

[1] Library and Archives Canada, John Henry Lefroy MG24-H25, microfilm reel M-2314, *Journals of Sir John Henry Lefroy*, 2 Vols, Vol. I, [hereafter JLVI], 6 May 1843, 15.
[2] Interestingly, Ross saw the start date as 5 October 1839, the date at which HMS *Erebus* and HMS *Terror* passed the Lizard. It was, Ross later wrote, 'the last point of the coast of England seen by us, and from which therefore we took our departure'. From Ross, J. C., *A voyage of discovery and research in the southern and Antarctic regions during the years 1839-43*, Vol. II (John Murray: London, 1847), 3.
[3] Sabine, E., *Observations made at the Magnetical and Meteorological Observatory at the Cape of Good Hope, Vol. I, magnetical observations, 1841 to 1846* (Longman, Brown, Green and Longmans: London, 1851), ii.

nomenclature and any anachronisms, the survey will be referred to as the North American Magnetic and Meteorological Survey when a name is required.

The first section of this chapter will trace the origins of the survey and provide a description of the chain of events that led to the employment of Lefroy to command the survey. Following this section, the chapter introduces the principal actors of the survey, both human and non-human, in more comprehensive detail than has heretofore been offered in other histories. This section will also give a brief outline of the course of the survey, and some of its most important staging points. Having therefore foregrounded the survey, the next section will move on to explore the myriad ways in which instruments were damaged and altered in their mobilisation through the North American northlands. This short section will include examples of accidents that befell several of the different kinds of scientific instruments Lefroy brought with him: meteorological, astronomical, mathematical as well as magnetic. The section that follows will consider in closer detail just two of these instruments – the Fox dip circle and the Gambey dip circle – to explore and partly challenge some of the myths attached to these instruments, both in the nineteenth century and in contemporary scholarship. After considering so many of the different accidents that occurred during the survey, the chapter will move on to consider some of the fixes and repairs that sought to address the damage done and restore different instruments to some semblance of a working state. It will be shown how important Lefroy's own small fixes were, and how much reliance was placed on the local craftsmen employed at various Hudson's Bay Company forts. These forts were not only important staging points for the survey to resupply and rest, but also significant sites of repair that enabled Lefroy's survey to travel to and observe in the confines of the Arctic Circle. As this chapter will then demonstrate, an emphasis on the materials of the survey does not preclude attention to the human actors involved in the survey but, rather, such a perspective can help to emphasise the multiplicity of people who formed an active part of the magnetic survey and who helped carry, fix, transport, safeguard, organise and guide the survey. The crucial role of these individuals as the maintainers and safe keepers of instruments on this mobile survey ought to be made more visible, and it is through a consideration of the materials of the survey that it is. Before the conclusion is reached, the chapter will close with a section on how the temporary observatories of Lefroy's survey were established and how they were operated from within two of the HBC's forts. As this section demonstrates, the experience of incessant observation at these sites in the North American wilderness blurred the lines between human and non-human actors, and this point is supported with reference to other similar instances in which instruments began to take on a life of their own, both during and beyond the parameters of the British magnetic scheme.

Origin of the Survey

A magnetic survey in North America was wanted because of the surprising discovery that 'the highest isodynamic lines of the northern hemisphere were closed and irregularly elliptical curves, extending across the North American Continent nearly in a north-west and south-east direction, and having their central point, or the *point of maximum of Force*, approximately in 52° north latitude, and 270° east [90°W] longitude'. Observations in the neighbourhood of this phenomena were, Sabine explained, 'objects which presented themselves amongst the most important desiderata for our present knowledge, and as likely to have a peculiar value at a future period in respect to the Ætiology of the science [of terrestrial magnetism]' and research that 'might serve to elucidate the laws of those secular changes, which, in our present ignorance of the causes of the earth's magnetism, seem even more mysterious than the apparently complex relations of contemporary phenomena'.[1] Lefroy also understood his survey to be a mirror and a continuation of the surveying exploits of James Clark Ross and 'confidently' hoped to provide Sabine's magnetic department with 'as large a body of results as will in some degree answer the questions that must grow out of those Ross is obtaining at the opposite Pole'.[2]

John Herschel, then Chairman of the Committee of Physics of the Royal Society, supported the idea of a North American magnetic survey and the Royal Society formally proposed one in HBC territories to the HBC leadership in 1841, to which they received a favourable reply. The Ordnance Department was also in agreement. Again, following a representation from the Royal Society, Lord Vivian, then Master-General of the Ordnance and demonstrably committed to furthering the scientific work of the department, 'was pleased to annex the survey in question to the duties of the Toronto Observatory, and to add for that purpose an officer and a non-commissioned officer' to the observatory. The Deputy Adjutant-General, Major General Sir Hew Dalrymple Ross, K.C.B., and the Treasury also concurred and extra pay to the officer and non-commissioned officer,

[1] Sabine, 'Contributions to Terrestrial Magnetism. No. VII', *Phil. Trans. R. Soc. Lond.* 136 (1846), 237-336, 238. Emphasis added.
[2] Lefroy to Sabine, [received] 10 August [1843?], TNA BJ 3/35/7.

together with £130 for the purchase of instruments and £50 a year for three years for the ‘contingencies of the survey’ were granted.[1]

Edward Sabine had recognised the potential importance of observations in HBC territories ‘even before 1830’, according to Binnema.[2] This is evidenced by a letter Sabine sent to Professor Renwick in the United States, in which he offered his thoughts on the current state of geomagnetic studies in general and identified the region around Great Slave Lake in the HBC territories as ‘the field for observations of the very highest importance on the subject of the magnetism of the globe’.[3] That a survey in these parts did not occur earlier than 1843 was due to the difficulty of recruiting the right individual to undertake the venture, and not a reflection of the HBC’s reticence to assist. Indeed, the HBC’s London governor John H. Pelly and North American governor George Simpson, proved only too willing to accommodate scientific research; the more so if it was part of a prestigious survey like the British magnetic scheme.[4]

Lefroy’s survey is important to the history of the magnetic crusade and the history of nineteenth-century science for several reasons. For one, it was the only sanctioned overland survey of the crusade. Frederick Eardley-Wilmot – he who established the Cape of Good Hope Magnetic and Meteorological Observatory – also undertook something of an overland magnetic survey but this was almost entirely of his own concoction. As Wilmot explained to Herschel in 1844, an opportunity had arisen in September 1842 ‘of going round the colony with my friend Mr Menzies (the circuit judge for the Sept. assizes)’. Wilmot took this opportunity and travelled along in a horse wagon for a time with only his personal Fox-type dip circle, a thermometer and a barometer’.[5] In contrast, Lefroy’s survey had been formally proposed by the Royal Society and equipped by Sabine and his magnetic department. It was to be an extensive undertaking, its scale significant for the fact that it had to rely on a network of sites belonging to an organisation – the HBC – other than the British government and on the ‘invisible labour’ of individuals both at HBC Forts and along the way, in the form of guides and *voyageurs*.[6] The journals Lefroy kept, the letters he penned, and the subsequent diary, scientific report and autobiography he wrote touch on this aspect of the survey and are also some of the most extensive records of how,

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[1] Sabine, ‘Contributions VII’, 239.
[2] Binnema, T., *Enlightened zeal: the Hudson’s Bay Company and scientific networks, 1670-1870* (Toronto: University of Toronto Press, 2014), 212.
[3] Sabine, E. ‘Observations on the magnetism of the Earth, especially of the Arctic regions; in a letter from Capt. Edward Sabine, to Professor Renwick’, *American Journal of Science and Art* 17, 1 (1830), 151.
[4] Binnema, *Enlightened zeal*, 213.
[5] Frederick Marrow Eardley-Wilmot to John Herschel, RS HS/7/5, 8 November 1844.
[6] Shapin, S., ‘The invisible technician’, *American Scientist* 77, 6 (1989), 554-563; and Binnema, *Enlightened zeal*.

and how well, instruments moved and were used during far-reaching geomagnetic surveys. And, for Binnema, Lefroy's survey was important because it strengthened ties between the HBC and the British government and because the survey was a significant part of the process of incorporating Toronto into the HBC's 'knowledge network'; an incorporation which helped make Toronto a central node in scientific networks in North America.[1]

The story of the North American Magnetic Survey has been told in several different historical and near contemporary accounts. However, in historical accounts it is often subsumed into larger national or institutional histories and is not by itself the subject of an extended inquiry.[2] Suzanne Zeller has provided probably the most robust accounts of Lefroy's survey. In one instance, the survey is used to support a history of the creation of a scientific community and legacy in Canada and, in the other, Lefroy and his survey are positioned in relation to wider narratives of the Humboldtian traveller and Humboldtian networks.[3] In like manner, Binnema uses the story of the survey as one of several staging posts that help elucidate his history of the involvement of the HBC in a host of scientific knowledge making enterprises from 1670 to 1870. For Binnema, the geomagnetic survey also serves to illustrate as clearly as any aspect of the history of science in the HBC, that, although historians have often emphasised how scientists and companies acted as agents of empire, 'empires and companies were at least as likely to act as agents of science'.[4] Levere's work on Lefroy is perhaps the exception here. While Levere has used Lefroy as part of a much wider and longer narrative of science in the Arctic, he has also provided one of the more detailed, albeit brief, studies of the materials used on Lefroy's survey.[5] One of the principal aims of this chapter is to extend Levere's study of the scientific instruments of Lefroy's survey with a fuller examination of the diary, autobiography, published letters, manuscript letters and contemporary journals of Lefroy and to frame these instruments within recent scholarship on the historical geographies of instrument use.

Sabine himself had wanted to carry out a magnetic survey in 1839, prior even to the launch of Ross's Antarctic voyage. He had contacted the HBC about this expedition and later wrote to Lloyd in the spring of 1839 informing him that the HBC had offered Sabine a canoe and that he had already planned his route from Montreal to York Fort via Lake

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[1] Binnema, *Enlightened zeal*, 210.
[2] For examples, see Zeller, S., *Inventing Canada: early Victorian science and the idea of a transcontinental nation* (McGill-Queen's University Press: Montreal, 2009); Binnema, *Enlightened zeal*.
[3] Zeller, *Inventing Canada*; and Zeller, 'Humboldt and the habitability of Canada's Great Northwest', *Geographical Review* 96, 3 (2006), 382-398.
[4] Binnema, *Enlightened zeal*, 200.
[5] Levere, T. H., *Science in the Canadian Arctic: a century of exploration, 1818-1918* (Cambridge University Press: Cambridge, 1993); and Levere, 'Magnetic instruments in the Canadian Arctic expeditions of Franklin, Lefroy, and Nares', *Annals of Science* 43 (1986), 57-76.

Superior and on the way back to Quebec to observe at 'Moose Rain'.[1] However, at this time both he and Lloyd were frantically trying to complete their report on the British Magnetic Survey. It had caused Sabine many anguished days and nights trying to incorporate the frequent revisions of Lloyd in time for it to be printed.[2] Its publication had already been postponed once in October 1838 ('our poor report, alas! Must be suspended') and Sabine was unwilling to allow this to happen again.[3] Sabine was forced to choose between his 'Canadian project' and 'our British Report' and, he wrote to Lloyd, he 'sacrificed the first!'. The HBC's cooperation was what Sabine had wished for but he would have to have been in Montreal on 1 May 1839 and this, he explained, he 'cannot do so, without abandoning the B. Report, so, the step is taken & regrets are useless'.[4] However, as Sabine later wrote, 'the project of a North American magnetic survey…was not suffered to drop'.[5] Instead, a new candidate for Sabine's 'Canadian project' was sought.

Charles J.B. Riddell, the first director of the Toronto Observatory, was next identified for the survey. This can be inferred from two sources. In April 1840, Lloyd wrote to Sabine to say that he ought not to put the idea of a magnetic survey into Riddell's head, implying that Sabine had thought of and written to Lloyd to express just that idea;[6] and in June 1841, shortly before command of the survey was passed to Lefroy, Lefroy expressed to Sabine his happiness at hearing of the imminent extension of the magnetic scheme and that his gladness that 'Canada' had fallen to Riddell, by which he must have been implying a survey of Canada because Riddell had been in charge of the Toronto Observatory site for over a year by this point.[7] A change of mind on Sabine's part must have occurred in the course of 1841 – perhaps because Riddell was considered too capable an observatory director to lose to a survey – and so Charles Wright Younghusband, assistant to Riddell at Toronto, was identified as the most suitable candidate.

Lieutenant Younghusband, R.A., was only twenty years old when it was intended that he should 'conduct a magnetic survey of the Northwest Territories in British North America'

[1] Sabine to Lloyd, April 1839, RS MS/119/I/66.
[2] Sabine to Lloyd, 8 April 1839, RS MS/119/I/49.
[3] Sabine to Lloyd, 23 October 1838, RS MS/119/I/58.
[4] Sabine to Lloyd, April 1839, RS MS/119/I/66.
[5] Sabine, 'Contributions VII', 239.
[6] Lloyd to Sabine, 27 April 1840, TNA BJ 3/10/151.
[7] Lefroy to Sabine, 4 June 1841, TNA BJ/3/81/28. That Lefroy still thought Riddell was to take charge of the survey and not Younghusband at this point is down to the infrequency of communication between England and St Helena.

in 1841, according to Thiessen.[1] Sabine had ascertained by personal communication with the HBC that 'for a public undertaking of this nature, the Company is ready to furnish gratuitous canoe conveyance in the territories belonging to them'. The contingent costs were estimated at £50 a year, an extraordinary figure given that the HBC would later bill the Treasury £1,277, and eventually compromised at a figure of £850.[2] Younghusband was deemed the best candidate for the survey because of his 'presence on the spot' (he was at this time acting as Riddell's assistant in the Toronto Observatory) and because of his 'zeal'.[3] However, when Riddell was invalided home at the beginning of 1841 due to persistent episodes of diarrhoea, Younghusband was forced to temporarily take over the directorship of the Toronto Observatory.[4] 'Is it not fortunate', Lefroy wrote to Lloyd in February 1841, 'that we had Younghusband on the spot, and sufficiently trained to take Riddell's place during his sick leave? Had it not been for my pet survey as you call it, that would not have been the case, and our best colonial observatory (as yet) would have sustained a very inconvenient interruption in its operations'.[5] However, Younghusband being placed in temporary charge of the Toronto Observatory meant that the magnetic survey was again without an individual to lead it.

Enter, finally, Lefroy. Lefroy had been the director of the St Helena Magnetic and Meteorological Observatory from the outset of the magnetic scheme. He was contacted by Sabine about the possibility of his involvement in a North American magnetic survey in August 1841, to which Lefroy responded that the prospect of this gave him 'pleasure' and 'satisfaction' and after hearing the news he could scarcely think of little else but 'bivouacs and birchbark canoes for a day or two'.[6] The management of the St Helena Observatory had been fatiguing and confining work, with limited opportunity for an active Artilleryman to exercise himself. Lefroy had expressed to Sabine on several occasions his desire to undertake survey work. Not long after he had moved into his permanent observatory on St Helena, Lefroy wrote to Sabine to explain his fascination with the geology of the island and his wish to make a survey of it and a 'Geological Map of it to show situation and direction of dykes' but was 'afraid of being stopped by a prohibition to take sketches or surveys'.[7] Lefroy proposed a similar survey of St Helena's geology and local magnetic

[1] Thiessen, A. D., 'Part VI – Lieutenant C. W. Younghusband, R. A., Acting Director of Her Majesty's Magnetical Observatory, Toronto, 1841-44', *Journal of the Royal Astronomical Society of Canada* 35 (1941), 205-224, 205.
[2] Lefroy, J. H., *Diary of a Magnetic survey of a portion of the Dominion of Canada chiefly in the North-Western Territories, executed in the years 1842-1844* (Green and Co.: London, 1883), x.
[3] 'Printed letters relating to the magnetic survey of Canada', TNA BJ 3/27/46-47.
[4] Riddell to Lloyd, 20 March 1841, RS MS/119/II/20.
[5] Sabine to Lloyd, 22 February 1841, RS MS/119/I/100.
[6] Lefroy to Sabine, 17 August 1841, TNA BJ 3/81/35 and 24 August 1841, TNA BJ 3/81/36.
[7] Lefroy to Sabine, 19 October 1840, TNA BJ 3/81/18.

attraction a year later for which he said he would only require Fox's portable dip circle.[1] In late 1840, Lefroy had also informed Sabine that if Sabine could not get 'the African survey' underway before the initial three-year period of the magnetic scheme expired then there was 'nothing [he] should like better than to undertake it with Wilmot [director of the Cape magnetic observatory]'.[2] That Lefroy was still trying to conduct his St Helena survey and the fact that both he and Wilmot were rebuffed in their attempts at an African survey makes it clear that Sabine and/or Lloyd had indeed advised against the undertaking of surveys while observatories still needed to be managed.

Observatory work was considered superior to undertaking mobile surveys or, at least, this was the message Sabine and Lloyd wanted to be understood by the colonial observatory directors. Magnetic surveying ought not to be viewed, Sabine wrote to Herschel in 1839, 'as an object of more interest or importance in Riddell's opinion than the conduct & performance of the observatory'.[3] Lloyd expressed a similar sentiment to Sabine and the colonial observatory directors on several occasions throughout 1841 and 1842. Lloyd spoke of the 'magnetic survey fever' that had overcome some, especially, but not limited to, Wilmot.[4] 'It was Herschel's article in the Quarterly', Sabine opined to Lloyd in January 1842, 'which first set Wilmot eager for a survey' and not anything Sabine had written either publicly or privately.[5] Sabine agreed with Lloyd's efforts to dampen Wilmot and others' desire for observation outside of the observatory.[6] Sabine felt Lloyd's words were 'more likely to tranquilise him [Wilmot] at his observatory work' than anything he could write.[7] A survey could be performed at the Cape, even a magnetic survey, by someone like Thomas Maclear, director of the Astronomical Observatory there, but this individual would not be able to run a magnetic observatory, according to Lloyd.[8]

Mobile magnetic surveys required much less accuracy in their observations. Extreme accuracy was 'both impracticable and unnecessary'.[9] Surveys required instruments to be much hardier even to the extent that this compromised their precision – such as was supposedly exemplified by Fox's dip circle – and the same could be said of the observer on a mobile magnetic survey.[10] Such an idea in part explains why Lefroy might finally have

[1] Lefroy to Sabine, 2 August 1841, TNA BJ 3/81/34.
[2] Lefroy to Sabine, 12 December 1840, TNA BJ 3/81/21.
[3] Sabine to Herschel, [1839?], RS HS 15/331.
[4] Lloyd to Sabine, 15 January 1841, TNA BJ 3/11/178.
[5] Sabine to Lloyd, 12 January 1842, RS MS/119/I/104.
[6] Lloyd to Sabine, 16 May 1841, TNA BJ 3/11/202.
[7] Sabine to Lloyd, 12 January 1842, RS MS/119/I/104.
[8] Lloyd to Sabine, 15 January 1841, TNA BJ 3/11/178.
[9] Riddell to Lloyd, 26 October 1843, RS MS/119/II/39.
[10] It was, after all, the instrument recommended for observations at sea and in all climates in Herschel, J., (ed.), *A manual of scientific enquiry* (John Murray: London, 1849), 19-21.

been chosen to head up the North American magnetic survey. He was not considered the best magnetic observer. Lloyd consistently remarked upon the extent to which Lefroy was 'at a loss' when difficulties occurred in St Helena and feared that 'poor Lefroy will never make an observer' as he had 'no tact in overcoming practical difficulties, even of the simplest kind'.[1] However, he was organised and committed to the magnetic scheme such that he voluntarily chose to be stationed in St Helena, the most isolated and probably the most challenging environment to be situated in. We know that Lefroy volunteered for this site because Wilmot considered Lefroy's swift (or swifter than originally expected) return to England as a 'reward' for Lefroy's 'kindness in insisting upon going to such a place as St Helena instead of letting me [Wilmot] do so, to whom it more properly belonged'.[2]

Lefroy departed St Helena in February 1842. Lefroy took lodgings at Woolwich, home of Sabine's magnetic department, and spent part of June with 'Mr Robert Weare [sic.] Fox at his residence at Penierrick, near Falmouth, for purposes of instruction in the manipulation of his Dip circle' before departing for Quebec in July aboard the *Prince Regent* transport, a journey which took 42 days to complete.[3] On both this journey and that from St Helena to England, Lefroy busied himself practising and making magnetic observations, early practice of handling magnetic instruments on the move. From Quebec, Lefroy took an 'excursion' to the United States to meet the American magneticians and to make observations at eleven stations 'in the chief cities and colleges of the Eastern States', such as Harvard University, which would connect his forthcoming survey with the nascent geomagnetic observations of these American sites.[4] Indeed, Lefroy aimed to make simultaneous observations with the 'American Magnetic Observatories' on the days when the HBC Brigade he was to travel with planned to halt for at least 24 hours.[5] Lefroy eventually arrived in Montreal on 15 September and Toronto on 23 October 1842.

The survey and its actors

Lefroy took over the running of the Toronto Observatory from Younghusband upon his arrival and for the next six months. The work of the observatory had 'fallen terribly in

[1] Lloyd to Sabine, 15 May 1841, TNA BJ 3/11/201 and 8 February 1841, TNA BJ 3/11/188.
[2] Quotation from a letter from Wilmot to Lefroy, March 1842, in Lefroy *Autobiography*, 56.
[3] In Lefroy, *Autobiography*, the date given is 14 July, 59; but in Lefroy, *Diary*, the date is 20 July, vii.
[4] Sabine, 'Contributions VII', 239; and Lefroy *Autobiography*, 61; and Zeller, *Inventing Canada*, 127.
[5] Lefroy to George Simpson, 25 September 1842, quoted in Thiessen, A. D., 'Part V: magnetic survey of a portion of the dominion of Canada, chiefly in the North-Western Territories, executed in the years 1842-1844', *Journal of the Royal Astronomical Society of Canada* 35 (1941), 141-150, 146.

arrears', as Lefroy noted in his *Autobiography* and as others have commented since.[1] Younghusband had struggled with the unremitting observations and reductions that were required at the observatory and the physical condition of the observatory was similarly dire. The dismissal of both Bombardier Thomas Menzies (for drunkenness) and his replacement Acting Bombardier John McNaught (for being untrained and unskilled in observatory work), together with Riddell's departure, had left the Toronto Observatory severely shorthanded.[2] 'All in all,' Smith has noted, 'it seemed as though Lefroy had assumed a hopeless task'.[3] In March 1843 Lefroy travelled to Boston to take charge of a set of new transportable magnetometers devised by Riddell and constructed by Jones which had finally arrived from England. After returning to Toronto for three weeks Lefroy left once again, this time to Montreal, where he arrived on 22 April 1843.

The survey had not yet begun, but already certain instruments had suffered from the exigencies of travel. Between Toronto and Montreal, Lefroy, together with Henry, had to travel in a 'common open country waggon [sic.], filled with straw, in a sharp frost', as navigation on Lake Ontario was not yet open. The effect of the jolting upon his instruments was 'disastrous'.[4] The Gambey and the Fox dip circles were 'shaken to pieces', the Gambey 'literally' so and the Fox 'almost'. The Gambey, Lefroy wrote to Younghusband from Montreal, consisted of little more than 'loose parts lying about in a box' by the end of its transit. The theodolite was similarly shaken apart and, although Lefroy carried the barometers on his shoulders the entire way, 'a little mercury' managed to escape one of them.[5] More problematic for Lefroy, Lloyd's static needles lost force from the effect of the jolting to such a degree as to entirely disconnect the subsequent observations from those intended to be the base series, taken at Toronto. The same applied to Fox's needle C, and a new base had to be taken for both, at Fort William (Station LXIX). 'The instruments were reinstated, as well as possible, before starting'.[6] Lefroy was more sanguine in his assessment in his *Autobiography*, saying of the altered state of the instruments that 'there was no help for it, and they were put in order again without much trouble'.[7] However, Lefroy noted in his contemporary survey journal that, on the day the canoes launched from Lachine, he had 'found such difficulty in turning Fox in azimuth as to fear a considerable

[1] Lefroy *Autobiography*, 63; for example, see Smith, J. A., 'Humboldt, Sabine and the "Magnetic Crusade": the founding of the Toronto Observatory', *Research report for the Canadian National Museum of Science and Technology* (1989), 1-74, 30-32.
[2] Smith, 'Humboldt, Sabine and the "Magnetic Crusade", 31-32.
[3] Smith, 'Humboldt, Sabine and the "Magnetic Crusade", 34.
[4] Lefroy, *Autobiography*, 64.
[5] Lefroy to Younghusband, 25 April 1842, in in Stanley, G. F. G. (ed.), *John Henry Lefroy: in search of the Magnetic North. A soldier-surveyor's letters from the North-West, 1843-1844* (Toronto: Pioneer Books, the MacMillan Company of Canada Limited, 1955), 6.
[6] Lefroy, *Diary*, 2.
[7] Lefroy, *Autobiography*, 64.

injury to the axis' which he later discovered was due to the screws of the level coming through the copper plate and grating 'upon the under'.[1] Although the Fox had been 'reinstated', it now existed on the margins of a state of disrepair.[2]

The list of instruments Lefroy brought to North America was long. Levere has written a concise and highly informative account of the instruments Lefroy took with him on his survey, but the list he presents is limited to the main magnetic apparatus Lefroy carried and precludes a full appreciation of the extent of the meteorological, mathematical and astronomical instruments also included in the survey inventory. The full list runs as follows:

1. One Declination Magnetometer and Bifilar, in one box, with canvas cover and straps complete with spare tube and suspension pins and spare therm[ometer].

2. Inclinometer, in box, with [*same as above*].

3. Declinometer (2, 4 inch & 1, 3 inch coll. needles), the box carrying also:

- spare 3½ inch bars
- 1 pair 2 inch bars
- The brass table tops for the legs of inclinometer
- A spare stirrup with revolving mirror made at Toronto, for vibrat[ing] all the smaller bars

4. Fox's dip circle complete, with two intensity needles A and C and one reversing needle B.

5. Gambey's dip circle, complete with a pair of Lloyd's needles and thermometer.

6. A theodolite.

7. A portable transit instr.

8. A repeating reflecting circle.

9. A small 4½ sextant, the property of Lieut. Younghusband.

10. An artificial horizon, with iron mercury bottle, also a box wood ditto.

11. Two Newman's iron cistern barometers nos. 33 – 119

12. One actinometer from observ[ator]y

13. One azimuth compass of the Committee's construction. 4 spare pivots.

[1] JLVI, 1 May 1843, 5; and JLVI 2 May 1843, 11, 7.
[2] Schaffer, S., 'Easily cracked: scientific instruments in states of disrepair', *Isis* 102, 4 (2011), 706-717.

14. One Kater's ditto.

15. Thermometer:

- 1 Newman's for boiling point of water.
- 1 ? registering in copper case, pierced and polished .
- 1 Newman's standard mercury.
- 3 Newman's merc[ury] max.
- 2 Newman's Spirit min[imum].
- 1 Newman's max with black bulb.
- 1 wet bulb Hygrometer, 2 therm[ometers].
- 1 Daniel's ditto, with ether
- 3 Therm[ometer]s merc[ury] purchased at Montreal, two of them max registering, one common mercury graduated to - 35°.

15. Three cylinders capable of holding any of Newman's thermometers (standard excepted) polished copper, double in the lower part and pierced with holes so dispersed that those in the outer and inner case are not opposite.

16. A copper case to carry ditto.

17. Six year's meteorological forms from Professor Espy, for distribution.

18. One lanthorn [sic.] and fire lamps for illuminating the instr. at night. Also a few wax candles in canteen (cir. 400lbs).

19. Two of the Admiralty dip books (Capt. Ross's form), one half full.

20. Two Dip books for Fox.

21. 1 100 feet measuring tape.

22. A small Dollond common telescope.

23. One or two spare lots of legs, from the old transport[able] magnet[ometer].

24. A large box for stationery and miscall. stores.

25. Lind's wind gauge from the observ[ator]y.[1]

This is a more considerable list than the one Lefroy later offered in his *Diary of a Magnetic Survey* (1883). The *Diary* list, which is Levere's source, does however offer up additional information on the makers of some of the instruments and Riddell's *Magnetical*

[1] 'List and specification of articles taken by Lieut. Lefroy on the magnetic survey to the northwest', 30 April 1843, TNA BJ 3/35/15.

Instructions for the Use of Portable Instruments (1844) gives some of their contemporary prices. Briefly: the Fox dip circle weighed 37lbs. in the box and cost £26 2s; the Gambey 27lbs.; the theodolite was made by Thomas Jones and weighed 10½lbs.; the declination magnetometer weighed 25lbs [the maker is not given but it was probably Jones] and cost £12; the original transportable declinometer was by Weber but subsequently replaced by the 'much superior instrument made by Jones' under Riddell's instruction and cost £14;[1] the transportable bifilar was also made by Jones, weighed 22lbs. and cost £19 10s; the inclinometer mentioned above was an induction inclinometer of Lloyd's design and Jones's construction that weighed 18lbs. and cost £15; the committee from which the azimuth compass came was the Admiralty Committee and was constructed by John Barrow; and the repeating-reflecting circle was made by George Dollond and weighed 25lbs.[2] Lefroy, prior to the survey, estimated in a letter to George Simpson that altogether the instrumentation necessary to 'obtain any magnetic results of value may be brought well within the compass of 50lbs. weight'.[3] In reality, Lefroy's magnetic apparatus alone weighed well over 50lbs and together with the meteorological, mathematical and astronomical instruments Lefroy also packed – to obtain 'magnetic results of value' – Lefroy carried around 180lbs of scientific instrumentation on the survey.[4] As Levere rightly points out in a footnote, the weight of instrumentation is 'not a trivial point when everything had to be packed into canoes and carried across portages'.[5]

Together with this list, Lefroy outlined the other necessities for his journey, such as a gun and a rifle, canteens, cassettes, other luggage, portable inkstands, bedding, blanket, one-and-a-half gallons of wine, tobacco, tea, powder, shot and balls. Extra clothing for his assistant was purchased at a total cost of £6 16s 0½d and included a pea coat, a red flannel shirt, a pair of shoes, a lowland Scotch cap, a grey cloth jacket, two chamois leather shirts and two chamois leather drawers.[6] Lefroy also gave red shirts to Baptiste and Roubillard, two of the French-Canadian voyageurs on the survey, 'by way of uniform'.[7]

[1] Lefroy, *Diary*, 1.
[2] All weights from Lefroy, *Diary*, 1, and all prices from Riddell, *Magnetical Instructions for the use of Portable Instruments adapted for Magnetical Surveys and Portable Observatories* (London: W. Clowes & Sons, 1844), 98-99.
[3] Letter from Lefroy to George Simpson, Governor-in-Chief of Rupert's Land, forwarded to Colonel Sabine, Montreal, 25 September 1842, quoted in Thiessen, 'Part V', 146.
[4] Lefroy, *Diary*, 1.
[5] Levere, *Magnetic instruments*, 66.
[6] 'List', TNA BJ 3/35/15.
[7] JLVI, 9 May 1843, 22.

The first observations of the survey were made proximate to Hudson's Bay House at Lachine on 30 April 1843.[1] The next day the canoes – 'canots de maitre', able to accommodate 13 or 14 voyageurs and up to four passengers – departed from Isle d'Urval and headed up the Ottawa River. The course of Lefroy's route is traced in several accounts of his survey and so here I will provide only a brief outline.[2] Lefroy and company headed northwest. They stopped at several important HBC outposts – e.g. Norway House, York Factory, Cumberland House – navigated both Lake Superior and Lake Winnipeg and traversed many difficult portages, the 'Rat Portage' being probably the most infamous, on their way to Fort Chipewyan, which the party reached on 23 September 1843 and where they wintered until 5 March 1844. Along the way Lefroy and his assistant Henry had made magnetic and meteorological observations almost daily, as the weather allowed. At Fort Chipewyan, Lefroy and Henry established a temporary observatory in which, working 12 hour shifts each, they almost ceaselessly recorded magnetic and meteorological observations at hourly intervals during daylight hours and every 2 minutes during magnetic disturbances, from 16 October 1843 to 29 February 1844.[3] Leaving Fort Chipewyan on 3 March on snowshoes, three 'trainaux' (sledges) and a cariole, Lefroy and his party trekked to Fort Simpson, where a second temporary observatory was established from March to May 1844. When the ice broke on 25 May 1844, Lefroy headed instantly for Fort Good Hope, reaching there on 29 May. This was the farthest north they would reach, and the occasion on which they 'touched the confines' of the Arctic Circle.[4] This was the apotheosis of Lefroy's survey, after which point the party turned south and made their way to Montreal via several of the same HBC posts they had visited on their outward journey. Lefroy and his party made their way (noisily) into Toronto on 18 November, before the survey ended on 25 November 1844 in Montreal. At the culmination of the survey, the party had covered close to 6,000 miles and observed at over 300 stations.

Initially, Lefroy had travelled as part of the HBC 'Brigade for the northern department', led by John Maclean. Lefroy was to be afforded two hours a day for observations (should the weather be conducive to this activity), four hours at each post they stopped at and twenty-four hours on days that coincided with the magnetic Term Days.[5] However, this arrangement changed after just a few days. Two voyageurs were placed at Lefroy's disposal – Edouard Genereux and Pierre Roubillon – 'to carry the instruments over

[1] For a full list of the dates and places of Lefroy's survey see, Lefroy, *Diary*, 187-190; or Thiessen, 'Part V', 149-150.
[2] *Ibid.*
[3] Lefroy to Younghusband, 13 December 1843, in Stanley, *John Henry Lefroy*, 69.
[4] Lefroy, *Diary*, v.
[5] Thiessen, 'Part V', 148. Quotation in letter from George Simpson to the Gentlemen in charge of Districts and posts in the Service of the Honorable Hudson's Bay Company, 26 April 1843, Thiessen, 'Part V', 148.

Portages, pitch [his] tent, and be otherwise useful' and Lefroy's canoe was 'detached' from the Brigade to allow more time for observations. This new organisation was 'an improvement on the previous arrangement' but only lasted until Fort William – reached at the beginning of June 1843.[1] Here, Lefroy's

> connection with the Hudson's Bay Company canoes was entirely dissevered. The large canoes, called *Canots de maître*, then went on no further than this point; the number and length of the portages precluding their further employment, a lighter canoe, called the *Canot du Nord*, came into use, one of which was appropriated to myself by the directions of Sir George Simpson, with a guide and a supply of provisions, and henceforward I commanded the disposition of my own time, subject only to the necessity of getting on.[2]

Time was always a scarce commodity, whether with the Brigade or without. Lefroy described his initial routine in a letter to Younghusband shortly after the canoes had first departed Lachine:

> We start about ½ p 3 every morning, stop for breakfast about ½ p 7 when I observe for time and Var[iatio]n, and for dinner about ½ p 1. The other canoes proceed immediately after dinner, mine remains behind while I observe Gambey and Fox. This takes about 2 hours, we then follow, and overtake them after they have encamped, usually about 8 oclock – take supper and lie down until the cry of *lève! lève!* turns us out before three in the morning. The discomfort of this mode of travelling is chiefly a want of time for washing, dressing and so on.[3]

It is not clear from the above whether Lefroy and Henry observed Gambey and Fox for the full two hours or whether this included the time needed to set up and take down the instruments. On Term Days, setting up and adjusting the transportable magnetometers and the induction inclinometer required up to two hours.[4] Lefroy also noted once that he (along with, probably, Henry and others) 'packed up the instr., struck the tent' and was afloat in the canoe 'in less than 40m from the last observ', although it is unclear whether this included the transportable magnetometers or just the quotidian instrumentation.[5]

Lefroy's comments to Younghusband seem to have described an average day of observation. At other times, observations could take up almost the entire morning. For instance, on 19 September 1843, Lefroy reported having spent from 0715 to 1125 making

[1] Lefroy, *Diary*, 61.
[2] Lefroy, *Diary*, 79.
[3] Lefroy to Younghusband, 20 May 1843, in Stanley, *John Henry Lefroy*, 13.
[4] Lefroy, *Diary*, 90.
[5] JLVI, 20 July 1843, 138.

observations.[1] It was also not uncommon for observations to be taken at dinner time for one to three hours.[2] When daylight shortened, evening observations had to be made by candlelight, something not easily done. Wind and rain were two of the most frequent barriers to observation outdoors by candlelight. For instance, Lefroy 'decided not to keep' the Term Day of 20 September 1843 because by then the nights were 'so long, so much candle light in the open air would have been necessary and so much chance of wind etc. as to make it unadvisable'.[3] On a separate occasion, Lefroy did not observe in the evening because he had 'strained [his] eyes considerably in examining the axles of Fox's needles' during the day.[4] Early in the survey, Lefroy also used the evenings for observational practice, something evidenced by his feeling 'uncommonly savage at the cry of Leve! Leve! about ¼ to 4, [as he] had been practising lunars until past 12 o'clock'.[5]

While Lefroy's separation from the full contingent of the HBC Brigade in June 1843 had alleviated some of the pressure of time Lefroy felt, his survey still needed to complete its northward navigation to Fort Chipewyan before winter set in. On occasion, this need meant shortening or foregoing observations. For example, on 10 July 1843, Lefroy did not complete his afternoon observations because he wanted to keep moving while the wind allowed it. Lefroy was the more aggrieved as well because, he wrote, 'we had a tolerably pretty spot also. A level floor of smooth granite running out from a sandy beach which was covered with a beautiful wild pea, while a thicket of aspen spruce and willow screened us on one side from the wind'. Such an excellent example of the temporary and fleeting sites used for observation were to be cherished because often (as Lefroy encountered later the same day) the spots they halted at were 'very bad'. A 'wet and sandy beach where the surf dashed within a few feet of the tent' for example, or a beach of shingles, or on the 'swampy soil' of the Long Portage.[6] These individual and continually changing sites had to be negotiated by Lefroy and Henry in the context of changing weather conditions and, importantly, the changed and ever-changing condition of the magnetic and meteorological instruments they carried, to which we now turn.

Instruments: moving, changing, changed

[1] JLVI, 19 September 1843, 253.
[2] JLVI, 14 May 1843, 30; and 11 June 1843, 62.
[3] JLVI, 20 September 1843, 256.
[4] JLVI, 12 July 1843, 122.
[5] JLVI, 14 May 1843, 31.
[6] JLVI, 10 July 1843, 118; and swampy ground from JLVI, 19 July 1843, 136.

Lefroy's instruments altered dramatically during his survey. To an extent, this was to be expected 'under the circumstances of a long land journey'.[1] Even so, the catalogue of injuries Lefroy's instruments suffered and the repairs that had to be undertaken were extensive. Changes in the state of the instruments Lefroy carried occurred for several reasons. First, there were many seemingly mundane accidents. The thermometer that worked in tandem with the inclinometer, which Lefroy was carrying with the intention of trying to 'unite the broken column, fell from pocket on stooping for something, and broke'.[2] Lloyd's needles were twice almost lost. On one occasion, a Mr Ross 'let them fall into the stream just before encamping' after which they 'floated down, but the canoe recovered them about 3 miles down'.[3] Two days later, Lefroy dropped the same needles out of his Macintosh pocket at a portage.[4] That the readings made by Lloyd's needles later seemed anomalous would suggest that these needles had suffered a loss of magnetic strength as a result of their falls and brief river excursion, although Lefroy in his journal believed that 'no cause can be given for such an occurrence'.[5]

Some of the most significant accidents and breakages occurred with the meteorological instruments Lefroy carried, which is perhaps unsurprising given that these were some of the most fragile. A spirit thermometer 'fell from the place on which it had been supported all night, and got broken'.[6] Both of the barometers were similarly put out of use: no.11 was simply 'broken in the canoe', and no.119 broken because it 'had been so placed in the canoe that the cistern end projected a little, unobserved, beyond the gunwale, and on approaching the shore it came violently in contact with the overhanging stem of a tree'.[7] The loss of both barometers was a 'sad disappointment' to Lefroy.[8] Previous to their final demise, one of the barometers had also been used by a French-Canadian child as rock-throwing target practice: 'well he was not an Indian', Lefroy drily observed in his journal, 'or it had been a 'gone' barometer.'[9] Newman's maximum registering thermometer no.10 was broken at the first 'carrying place', i.e. a portage, only a few days after the survey had first embarked.[10] A second 'New. Max therm.' was broken not long after, 'in the water, apparently by the force of the current'.[11] Before the canoes had even launched from Lachine, Lefroy's servant, had 'let the box of thermometers fall from the hand cart on

[1] Lefroy, *Diary*, 38.
[2] JLVI, 26 July 1843, 150.
[3] JLVI, 7 May 1843, 17.
[4] JLVI, 9 May 1843, 21.
[5] JLVI, 24 May 1843, 43A.
[6] JLVI, 11 June 1843, 61A.
[7] First quotation from JLVI, 22 May 1843, 41; second from Lefroy, 16 June 1843, *Diary*, 87.
[8] Lefroy to his mother, 6 June – 1 July 1843, quoted in Stanley, *John Henry Lefroy*, 27.
[9] JLVI, 24 May 1843, 42A.
[10] JLVI, 6 May 1843, 15.
[11] JLVI, 15 May 1843, 33.

which it was going down, on to the stones, breaking two thirds of the contents'. Only one hygrometer and 'one or two' thermometers managed to escape this 'most unfortunate piece of clumsiness'.[1] It is not clear whether Lefroy had a chance to replace all the broken thermometers before the survey properly launched. In addition, several of the mathematical and astronomical instruments were also damaged. For instance, the circle of the theodolite was 'much bent' by a fall at the Francois River.[2] The brass plummet was also 'abstracted…from the Theodolite box' by a group of Chipewyan children which Lefroy 'endeavoured in vain' to recover.[3] One of the glasses of the artificial horizon was smashed when Henry dropped it at a portage.[4]

Finally, there were also the many and varied ways in which Lefroy's magnetic instrumentation was altered as it was mobilised through the northlands, some of which have already been related. In one of the more bizarre incidents, after stopping and setting up instruments on 20 June 1843, Lefroy was surprised to witness a stray calf blunder into his equipment. Lefroy was attempting at the time to observe the meridian altitude of the sun but instead observed the calf knock over his Gambey dip circle and smash the cover 'to pieces'.[5] By this unfortunate accident the Gambey was 'rendered for the time unserviceable', Lloyd's needle A 'which was on it at the moment, was ruined' and a deviation of the survey's route to take in the Red River settlement, and lower Fort Garry specifically, was required.[6]

There were four particularly precious instruments that travelled with Lefroy: the three transportable magnetometers and the induction inclinometer. These were precious because they measured the earth's magnetic force in absolute, rather than relative terms, and were the instruments employed on magnetic Term Days to observe simultaneously with all observatories on the British magnetic scheme. These instruments were to be set up only at particularly long stoppages, at Forts, and within the temporary observatories at Fort Chipewyan and Fort Simpson. Precious as they might have been and as infrequently used as they were in comparison to the other instruments, they also suffered. On two separate

[1] Lefroy to Sabine, [received] 10 August [1843?], TNA BJ 3/35/7.
[2] JLVI, 30 May 1843, 49A.
[3] JLVI, 16 September 1843, 248.
[4] JLVI, 2 September 1843, 220.
[5] JLVI, 20 June 43, 79.
[6] Lefroy, 20 June 1843, *Diary*, 89. To an extent, this accident was fortuitous. Had the calf not blundered in and broken the Gambey, Lefroy would not have altered his route to take in the Red River Settlement and would then not have bumped into Sir George Simpson, North American Governor of the HBC. It was Simpson at the Red River who advised Lefroy to head not for Moose Factory as originally intended but instead to make for Fort Chipewyan and overwinter there. See Lefroy, *Autobiography*, 74; or John Henry Lefroy and Sir John Richardson, *Magnetical and Meteorological Observations at Lake Athabasca and Fort Simpson…and at Fort Confidence in Great Bear Lake* (Her Majesty's Stationery Office: London, 1855): ix-x.

occasions when the transportable magnetometers were set up, they were blown down. The declinometer, used to measure the variation of the magnetic force, escaped largely unharmed from its fall, although the theodolite in use alongside it had its vertical and its horizontal limbs bent and 'bruised'.[1] On the occasion when the transportable bifilar magnetometer was blown over, both its suspension tube and thermometer were broken.[2]

Damage to the limbs, or the body of the apparatus, were not the only problems to afflict the magnetic instruments. The most frequent concern was that of needles contracting rust because of extended 'exposure' to the environment outside their boxes and the instruments they worked in on a long overland journey.[3] Axles were also frequently put out of shape. On 24 July 1843, Lefroy reported on the state of his eight needles at this early point in the survey. Rust had not yet set in but already Lloyd No. 2 had a 'sensible bend at the shoulder of the front axle'; Fox C's back axle shape was not good; Gambey 1's sides were 'not quite straight lines'; and the polish on half of them had already begun to wear away.[4] Fox A seems generally to have 'worked with very tolerable freedom, not as a positively good one, but not as a positively bad one' although some irregularity was noticed with the weight at 4.0 grams seemingly 'due to a bruise on the axle'. Fox B 'did not work freely' and 'ceased to vibrate almost instantly'; and Fox C was so often found to be irregular in its force that Lefroy 'condemned the axle and substituted a spare axle for it' in August 1843.[5] Two new Lloyd's needles were forwarded to Lefroy in 1844 at Norway House but 'they proved to be about 0.2 inch too long for the [Fox] dip circle, and were never used'.[6] This marginal but significant error speaks to what Schickore identifies as 'the individual differences between instruments produced by the very same maker' that frequently came to the fore in the nineteenth century.[7] To overcome such manufactured discrepancies and to continue working with instruments that more and more came to exist in a state of disrepair, Lefroy was continually required to both affect his own remedies and to apply to the smiths and armourers of various HBC outposts for more technical repairs. Such fixes will be described later as, next, this chapter will consider instances of disrepair to two specific instruments – the Fox-type and Gambey dip circles – and how their condition in the field compares to their representation in the printed press.

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1 JLVI, 16 May 1843, 34A.
2 JLVI, 26 July 1843, 150.
3 Lefroy, *Diary*, 33.
4 JLVI, 24 July 1843, 145.
5 Lefroy, *Diary*, 30, 31.
6 Lefroy, *Diary*, 20.
7 Schickore, J., 'Ever-present impediments: exploring instruments and methods of microscopy', *Perspectives of Science* 9, 2 (2001), 126-146, 136.

The Fox and Gambey

The Fox-type dip circle, which was a supposedly and designedly robust and portable instrument, is remarked upon several times in Lefroy's journals for having presented a number of mechanical and methodological challenges, brought on by the exigencies of the survey and the instrument's own inherent fallibilities. For example, in equalising the weight of the dip circle's hooks, one of the hooks 'became partially broken from the shank'.[1] On several other occasions, problems with the axle are mentioned. Lefroy later wrote that

> The instrument, as then turned out by George, of Falmouth, was very perfect but easily put out of order. The bearing points of the axles were minute cylinders of steel, resting in jewels like those of a watch. Each jewel consisted of two parts, a ring of ruby and a plane of ruby facing it. The rings were very apt to get broken, and their edge to be chipped in the operation of putting in the needle. In this way two sets were rendered unserviceable.[2]

Lefroy therefore carried a spare axle on the survey. Owen Stanley, second lieutenant on George Back's Arctic expedition of 1836-37, had experienced similar problems with the Fox in the Arctic. He commented that 'the dipping needle gave me the most trouble the weights used in ascertaining the intensity being so small & delicate as not to be easily handled with cold fingers'.[3] The Fox-type dip circle was a much-valued magnetic instrument of the nineteenth century. In the *Admiralty Manual of Scientific Enquiry* (1849) this instrument is heralded as having 'contributed more to a knowledge of the geographical distribution of terrestrial magnetism than any other recent invention'.[4] For anyone enquiring about the Fox-type in the historical encyclopaedia *Instruments of Science* (1998), they will find a 'robust dip circle suitable for expeditionary use on land and sea'.[5] However, as Bulstrode has demonstrated, often 'the performance of the [Fox] dip-circle did not live up to its reputation'.[6] In addition to Stanley's lamentations, the hydrographer and naval officer Edward Belcher, the French explorer Antoine Thomson d'Abaddie d'Arrast, and the director of the Singapore magnetic and meteorological observatory Captain C. M. Elliott reported difficulties in making the Fox 'settle' on the move and in

[1] JLVI, 29 May 1843, 48A.
[2] Lefroy, *Diary*, 22-23.
[3] Quotation from Levere, 'Magnetic instruments', 62.
[4] Herschel, *A manual of scientific enquiry*, 19-20.
[5] Bud, R., and Warner, D. W., *Instruments of science: an historical encyclopedia* (New York and London: Garland Publishing, 1998), 177.
[6] Bulstrode, J., 'Men, mines and machines: Robert Were Fox, the Dip-Circle and the Cornish system', (Unpublished MA thesis, University of Cambridge, 2014), 29.

situ.[1] The 'construction of myth' around the robustness and reliability of the Fox-type was 'one way of obscuring the anxieties' created by the 'pervasive problem of indiscipline', according to Bulstrode.[2]

To some extent, a myth has also been constructed about the portability of the Gambey-type dip circle. However, in contradistinction to the Fox, the myth here was that this instrument was too delicate for extended observations on the move. If the Fox supposedly compromised sensitivity and precision for toughness and durability, the Gambey was of the opposite construction. Levere writes that 'Gambey's instruments were sensitive' and not suited to 'conditions of movement', particularly those experienced at sea. Fox's apparatus was a 'solution' to this issue of over-sensitivity because it replaced the conventional suspension of the dip needle – 'the free rolling of the axis on agate planes' – with a construction employing agate jewel-cup bearings.[3] This feature was an adaptation adopted by Fox from the fields of clock making and early nineteenth-century precision mining technologies.[4] Fox's apparatus was recommended explicitly by name in the *Admiralty Manual of Scientific Enquiry*, the *Report of the Committee of Physics, including Meteorology* (1840) and the *Revised Instructions for the Use of Magnetic and Meterological Instruments* (1842) as 'the instrument necessary for observations of the inclination and relative intensity' and that 'found most generally convenient, being available both at sea and on land…in all magnetic latitudes'.[5] Gambey's apparatus was not named. An 'ordinary dip circle' does appear as a suggested addition to the usual magnetic surveyor's stock, but this was as likely to mean one of Robinson's design – such as that which accompanied James Clark Ross to the Antarctic – as that of Gambey.[6]

All of this is not to say that the Gambey dip circle was not well-liked or that it did not travel prior to Lefroy's survey. Quite the opposite: it was in continuous employment in Britain and Ireland – on the British Magnetic Survey and at Lloyd's Dublin Observatory – as well as on the continent from the 1810s. Sabine wrote in his 1838 report on the British Magnetic Survey that 'the excellence' of dip circles made by Gambey was 'too well known' to require any additional comment in his published report.[7] But this instrument was

[1] Elliott, C. M., 18 March 1849, RS MS/257/485, quoted in Bulstrode 'Men, mines and machines', 29.
[2] Bulstrode, 'Men, mines and machines', 29.
[3] Levere, 'Magnetic instruments', 60.
[4] Bulstrode, 'Men, mines and machines', 4.
[5] Anon, *Revised instructions for the use of the magnetic meteorological observatories and for the magnetic surveys* (London: Richard and John E. Taylor, 1842), 42.
[6] Anon, *Revised instructions*, 43.
[7] Sabine, E., 'A memoir on the magnetic isoclinal and isodynamic lines in the British Islands, from observations by Professors Humphrey Lloyd and John Phillips, Robert Were Fox, Esq, Captain James Clark Ross, R.N., and Major Edward Sabine, R.A.', *Report of the Eighth Meeting of the British Association for the Advancement of Science; held at Newcastle in August 1838* Vol VII (London, 1839), 49-195, 56.

often commented on in the correspondence of leading geomagnetic physicists. In 1837, Sabine told Lloyd that he could not 'imagine a better instrument for the changes of Dip' than the circle Gambey had made for Adolph Kupffer on the continent.[1] On several occasions the Gambey is noted as being 'certainly a most beautiful' instrument and one that also did 'beautifully' out of its 'examination' during the British Magnetic Survey.[2] It was accounted 'the best' dip circle then in use in a report of the magnetic committee of the BAAS to the Lords Commissioners of the Admiralty on the subject of magnetic observations in India in the spring of 1838.[3] And Lloyd in 1837 'would as soon distrust the pope as Gambey's circle' in Sabine's hands, a comment as much on the trust imbued in this instrument as it was on Sabine's skill as an observer.[4]

However, despite this private acclaim, the Gambey was never as synonymous with survey work as the Fox was in the printed literature of the magnetic crusade, at least prior to the publication of Lefroy's survey's results. As this publication showed, the Gambey apparatus Lefroy employed was the same which had participated in the magnetic survey of the British Isles and that which, Sabine wrote, had 'since travelled with Lieut. Lefroy over the continent of America to the Arctic Circle and back, having been used at more than 100 stations during that journey' and, Sabine continued, 'it should be recorded, to the credit of the excellent artist by whom it was made, that it is still in use apparently quite unimpaired'.[5] In point of fact, although the Fox was lauded for its ability to work in all latitudes, it did not travel as far north as the Gambey and did not 'touch the confines' of the Arctic Circle with Lefroy. Instead, it seems to have been left at the site of Lefroy's temporary observatory at Fort Chipewyan on Lake Athabasca when they quit this place on 5 March 1844, and headed to Fort Simpson. Lefroy was restricted in what he could bring with him on this part of the journey because he and Henry were to travel by cariole and snow-shoe, accompanied by only two other sledges. Lefroy told Sabine that because of this restriction he brought along only 'the magnetometers, azimuth, Gambey, small sextant etc., and theodolite, *a complete equipment in short*'.[6] Presumably, though Lefroy does not mention it explicitly, this was the same equipment with which the company travelled to Fort Good Hope, the most northerly point of the entire survey, when the ice cleared on 25 May 1844. In any case, Lefroy must have had his Gambey dip circle because he recorded a

[1] Sabine to Lloyd, 28 October 1837, RS MS/119/I/30.
[2] Sabine to Lloyd, 9 October 1837, RS MS/119/I/40; Sabine to Lloyd, 21 October 1837, RS MS/119/I/41.
[3] 'Report of the Magnetic Committee to the Lords Commissioners of the Admiralty on the subject of magnetic observations in India', 20 April 1838, RS MS/119/I/56.
[4] Lloyd to Sabine, 12 October 1837, TNA BJ 3/7/42.
[5] Sabine, 'Contributions VII', 323.
[6] Lefroy to Sabine, 27 March 1844, in Stanley, *John Henry Lefroy*, 98. Emphasis added.

dip here of 82° 55' 9", the survey's greatest.[1] A complete equipment as outlined by manuals on geomagnetic survey work could look quite different to a complete equipment as proscribed by the local conditions which an observer encountered. Excepting the two wagon-related injuries it incurred, the Gambey apparatus is largely invisible in Lefroy's survey journal, survey diary, survey letters and autobiography. Its consistent functioning within the harsh environment and peripatetic nature of magnetic survey work has rendered it almost invisible.

Fixes

Histories of maintenance and repair are still largely to be written.[2] It is a topic of 'growing interest for geographers', but these efforts have tended to fall outside the realm of the history of science.[3] According to MacDonald and Withers, 'we have paid too little attention to fallibility and to how truth claims about science and exploration were made despite, not because of, the instruments used'.[4] As Schaffer pointed out in 2011, 'some histories of broken instruments and their fixes might help'.[5] The previous section was an answer to the first part of Schaffer's request, and the following speaks to the latter.

In writing his post-factum *Diary*, Lefroy hoped to demonstrate in part 'the perplexities of a magnetic observer out of reach of skilled mechanical assistance'.[6] To some extent, this is true. There were no (human) Foxes, Gambeys, Lloyds or Newmans at large and on hand to help in the places to which Lefroy and his instruments travelled. Lefroy could and did rely on his own reasonable personal knowledge of the mechanics of his instruments. He filed, straightened, remounted and sometimes recycled instruments to restore their functions. For example, when the Fox-type dip circle 'became partially broken from the shank', Lefroy 'endeavoured, apparently with success to fix [the problem] with Blowpipe' after which he was able to continue observing the Fox.[7] Later, in September, when Henry broke one of the

[1] Lefroy, *Autobiography*, 93.
[2] For some attempts to do so though, see Schaffer, 'Easily cracked'; Edgerton, D., *The shock of the old: technology and global history since 1900* (London: Profile, 2008); and Werrett, S., 'Recycling in early modern science, *Brit. J. Hist. Sci.* 46, 4 (2013), 627-646.
[3] See Caitlin DeSilvey, 'Object lessons: from Batholith to bookend', in Johnson, N. C., Schein, R. H., and Winders, J. (eds), *The Wiley-Blackwell companion to cultural geography*, (Chichester: Wiley-Blackwell, 2013): 146-158. DeSilvey gives a concise summary of the kinds of things these works have focused on, from Cornish harbours to post-Katrina New Orleans.
[4] MacDonald, F., and Withers, C. W. J., 'Introduction: geography, technology and instruments of exploration', in MacDonald, F., and Withers, C. W. J., (eds), *Geography, technology and instruments of exploration* (Farnham: Ashgate, 2015), 1-14, 10.
[5] Schaffer, 'Easily cracked', 708.
[6] Lefroy, *Diary*, 30.
[7] JLVI, 29 May 1843, 48A.

glasses of the artificial horizon, Lefroy 'was obliged to take the back glass of the actinometer and cut it for a new glass'.[1] The actinometer became, in the mobile, isolated, context of the Northwest Territories, not only an instrument but a resource, a recyclable object. This incident perhaps also speaks to the hierarchy of instrumentation in Lefroy's survey: what could be bastardized and what could not be spared. That the course of the survey was changed because the Gambey dip circle was damaged in its bovine collision and needed to be repaired also serves to illustrate this hierarchy. Some, however, could be coped without. For instance, several of the barometers and thermometers were also smashed and broken – some quite early in the survey – but Lefroy only mentions procuring one replacement Dollond spirit thermometer from a Mr Swanston at Fort William at the end of May 1843.[2]

Although Lefroy did manage the state of several of his instruments by his own hand and resources, he also relied in great part on the network of HBC forts through which the survey passed and specifically on the armourers or blacksmiths that worked in these places. The most notable example of such reliance occurred at Fort Garry, also known as Stone Fort, within the Red River settlement, which Lefroy and company reached on 28 June 1843. The party remained at Fort Garry until 4 July in order to have repairs to the dip circle and other articles effected.[3] The 'tangent screw of azim[uth] limb of inclinometer' which was 'crooked and occasioned irregularity in the motion' was repaired; the 'footscrew of vibration box [was] straightened from bend caused by fall at L. Huron'; the 'vertical limb of theodolite which was bent by [the same] fall as above [was] flattened; and Lefroy 'allowed the armourer to try to straighten the bent axle of Lloyd no.1, it being quite useless in that condition'. For this the armourer 'first took out the temper [and] afterwards rehardened it'. For this last fix Lefroy wrote that the armourer 'appears to have succeeded'.[4] Lefroy also stated that the armourer's repairs to the dip circle were 'very neatly executed'. Once again however the humble wagon proved to be a dip circle's nemesis as, when it was moved from lower Fort to upper Fort Garry (where Lefroy was residing) 'it was shaken to pieces by 21 miles transport in a cart without springs' even though it was packed appropriately. Lefroy 'had to take it all to pieces and tighten all the screws', an operation which did not seem to require much time as Lefroy was observing the dip later the same day.[5]

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[1] JLVI, 2 September 1843, 220.
[2] JLVI, 2 September 1843, 50A.
[3] Lefroy, 29 June 1843, *Diary*, 94.
[4] JLVI, 1 July 1843, 101.
[5] JLVI, 3 July 1843, 104.

This stop was a deviation from the original intended route of travelling from Fort Alexander to Norway House, a fact which demonstrates the importance of certain HBC outposts and the knowledge that skilled mechanical assistance was sometimes, though not always, within reach during the survey.[1] In certain respects, comparison can be made with Lefroy's time in St Helena, where Lefroy also felt as if he had been 'thrown only on one's own resources'. This despite the fact that there were workmen in the colony who were not only capable of repairing instruments but who were willing and able to 'pick holes in the coat of a London artist' and make alterations to instruments to improve their functionality, such as occurred with Lefroy's anemometer.[2] Prior to departing for St Helena, Lefroy had expected that the blacksmiths on the island were capable only of 'rough work, but not fine or nice work'.[3] In this supposition Lefroy seems to have been proved wrong. Such blacksmiths and armourers were arguably the invisible maintainers of significant elements of the British magnetic scheme.

A multiplicity of hands: indigenous and other

The labour of Fort armourers is not the only example of the invisible work that maintained Lefroy's survey. Both the French-Canadian voyageurs and Indigenous guides who accompanied Lefroy are also often overlooked in accounts of Lefroy's survey. Thinking about the materiality of the survey – of the non-human actors – is, perhaps ironically, one way of making these individuals visible. The material perspective illuminates the multiplicity of different hands through which instruments passed and pays attention to the fact that although this survey is remembered as Lefroy's survey, it was dependent and contingent upon the capacity of a number of other individuals – from Lefroy's servant, to his assistant Henry, to the various French-Canadian voyageurs and local Indigenous guides – to carry and keep safe the fragile instruments. As Lefroy put it in a letter to his mother prior to the survey,

> You cannot think what an anxious business has been the conveyance of so many Instruments safely from Toronto by land, and with every care several of them have suffered a good deal – nor will my uneasiness upon this score be soon relieved *for*

[1] Lefroy to his mother, 6 June – 1 July 1843, in Stanley, *John Henry Lefroy*, 31.
[2] Lefroy to Sabine, 31 August [1840?], TNA BJ 3/81/16; and Lefroy to Sabine, 17 November 1840, TNA BJ 3/81/20.
[3] Lefroy to Sabine, [1839?], TNA BJ 3/81/2.

> *the canoes are unloaded every night, and every night will put it in the power of a clumsy voyageur to ruin my hopes*.[1]

These 'clumsy' voyageurs were men such as Edouard Genereux, one-eyed Pierre Roubillon, Pierre Blondin, Narcisse Arel, and Baptiste Ayot – the 'Sancho Panza of the party' – among others.[2] There were also a number of Indigenous peoples who participated in the safe passage of the survey and its instruments: Laurent Tewakewassin and "Louis", both Iroquois, Baptiste Sateka, and, two Chipewyans, Gougro – who went 'by the agreable [sic.] name of the "Man-Eater"' – and Assagai.[3] It was the role of these individuals specifically to carry the entire material inventory of the survey over portages – distances that ranged from one or two miles to twelve miles and could take up to two days to traverse. Lefroy explained the process in a letter to his sister Isabella in October 1844:

> When we arrive at such a place, the canoe is unloaded, taken out of the water, carried across by land, by two of the men, and then the loading carried over to it…The canoe weighs about 400lbs, and two men have to carry it on their shoulders. I have a box weighing 100lbs. Someone has the pleasure of carrying that, and so of everything. 180lbs is considered a full load, if compact. They have to go and return as often as necessary until every thing is carried… I always carry something, more indeed than most gentlemen in this country, for the sake of example, and because I have many small separate packages requiring constant care and watchfulness.[4]

Lefroy was always keen, in his memoirs and in his letters, to point out that he carried a 'tolerable burden, even for a *bourgeois*', which included 'gun, barometer, dish, haversack with books and axe' at these crossing places.[5] By this admission, it seems Lefroy did not carry the majority of his instruments, and even the instrument he did carry, a barometer, was for the majority of the survey broken. For a couple of reasons, it is important to note that the majority of the time in which the instruments were carried on the survey it was by the hands of someone other than Lefroy. It is true, as MacDonald and Withers and Dunn and Naylor have all pointed out, that using instruments is, as much as anything, a story of training and disciplining the user to manipulate technology. Instrument use was an embodied practice that bred dexterity and regularity in both the user and the object.[6] It is

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[1] Lefroy to his mother, 25 April 1843, in Stanley, *John Henry Lefroy*, 4. Emphasis added.
[2] Lefroy, *Autobiography*, 75.
[3] JLVI, 11 September 1844, 237; and Lefroy to his mother, 30 September 1843 – 2 January 1844, in Stanley, *John Henry Lefroy*, 65.
[4] Lefroy to Isabella [sister], 18 October 1844, in Stanley, *John Henry Lefroy*, 129.
[5] Stanley, *John Henry Lefroy*, xx. Emphasis in original.
[6] MacDonald and Withers, 'Introduction', 9-10; Dunn, R., 'North by Northwest? Experimental instruments and instruments of experiment', in *Geography, technology and instruments of exploration*, 57-75; and

also true, I would argue, that we ought not to dismiss the dexterity, sensitivity and skill with which voyageurs and Indigenous peoples unloaded, carried – sometimes for many miles across steep and swampy ground – and reloaded the hundreds of pounds' weight of instrumentation that made up Lefroy's survey on hundreds of occasions, sometimes incessantly on the days they encountered many small portages. As we have seen on several other occasions, the scientific equipment that travelled on this survey was fragile and liable to break at even the slightest of rough treatment. Lefroy made it clear in the letter to his mother above how easy it would have been for a 'clumsy' voyageur to *ruin* the hopes of his survey. But, in the hands of a competent voyageur, Iroquois, Chipewyan or other indigenous party member, instruments were made to move safely and thus their state of existence – broken or usable – made static. They did not "use" the instruments, but they managed them in arguably as important a way as Lefroy did.

Alongside their management of the state of the instruments, the survey crew also managed the state of the canoes in which Lefroy and the instruments mostly travelled. There are numerous references in Lefroy's field journal to the fact that frequent stops were required for 'gumming' of the canoe. The canoe was an important space for the survey. It was both carrier and carried. It provided a space for Lefroy and Henry to sleep following the exhausting ritual of Term Day observations and, occasionally, it was made into a space from which to observe while moving, as Lefroy did with the actinometer on 25 August 1843, although he did not consider the observations 'so good as a shore one'.[1]

In his recent book on the history of the relationship between the HBC and science, Binnema has explained how, 'aboriginal people routinely served not only as trappers, but also as guides, couriers, and hunters for traders throughout the HBC territories'.[2] 'Native expertise' had similarly been the context in which several attempts to find the Northwest Passage had been made in the early nineteenth century, the local knowledge of Indigenous peoples being 'impeccable' because, Levere notes, they had 'travelled widely' and had a 'pretty fair idea of neighbouring topography for many days' travel'.[3] The Indigenous people of Lefroy's survey fulfilled all the roles Binnema highlights, but Lefroy largely noted their prowess as guides. Even when Laurent – Lefroy's first guide – 'got completely bewildered' for a time 'among the archipelago of small low-wooded islands, all singularly alike, which fills the centre of the Lake of the Woods' – the wonder, Lefroy wrote, was

Naylor, S., 'Weather instruments all at sea: meteorology and the Royal Navy in the nineteenth century', in *Geography, technology and instruments of exploration*, 77-96.

[1] JLVI, 25 August 1843, 204.

[2] Binnema, *Enlightened Zeal*, 31.

[3] Levere, *Science and the Canadian Arctic*, 4.

> not that the Iroquois lost his way, but that they should know it at all: that over a line of some three thousand miles these Indians know every stone and stump, and are able to guide a canoe without compass through intricate channels in which a European eye is lost at once.[1]

Lefroy relied heavily on such native expertise. Elsewise, for navigation, Lefroy had only his instruments and John Franklin's route maps; maps that had been made during Franklin's 1819 Arctic expedition and which, while 'very creditable' to the officers that made them, 'were at the best imperfect'.[2]

Mobile/static, inanimate/animate

One of the most significant aspects of Lefroy's survey was the fact that, in consultation with HBC Governor Sir George Simpson and not with Sabine, Lefroy decided to deviate from his originally stated plan of heading for (and turning around at) Moose Factory, in order to make for Fort Chipewyan on Lake Athabasca, 'the most northerly station which could be conveniently reached in the season'.[3] Lefroy wintered here in order to establish a temporary observatory for continual geomagnetic observations in a high northerly latitude (58˚ 43' N. from Greenwich) and in the region of the focus of the earth's magnetic intensity in the north. It was a spot unlikely to be reached again for such observations in the near future. Fort Chipewyan was described by Lefroy as a 'square palisaded enclosure of mean appearance, with a sort of tower at each angle' with a total population of about 35.[4] It was the 'poorest' Fort Lefroy had yet encountered.[5] 'A vacant hut to the east of the dwelling' was given up to Lefroy as an observatory. The addition of a chimney was made and 'with the help of a half-breed carpenter' three small windows were put in, 'the lower half of each being of parchment' as well as pedestals for the instruments.[6] Materials for the construction of the observatory were provided by the recycling of an old boat house.[7] The log observatory measured 18 x 13 feet. No iron was used in its construction. The three parchment windows also contained a small panel of glass and were 'so disposed as to

[1] First quotation in Lefroy, 18 June 1843, *Diary*, 88; and the second quotation in a letter from Lefroy to his mother, 6 June – 1 July 1843, in Stanley, *John Henry Lefroy*, 28.
[2] Levere, *Science and the Canadian Arctic*, 4.
[3] Lefroy, J. H., and Richardson, J., *Magnetical and Meteorological Observations at Lake Athabasca and Fort Simpson and at Fort Confidence in Great Bear Lake* (London: Longman, Brown, Green, and Longmans, 1855), x.
[4] Lefroy, *Autobiography*, 83.
[5] Lefroy to Younghusband, 13 December 1843, in Stanley, *John Henry Lefroy*, 66.
[6] Lefroy, *Autobiography*, 83.
[7] Lefroy to Younghusband, 13 December 1843, in Stanley, *John Henry Lefroy*, 67.

throw light on the scales of the instruments'.[1] The work took three weeks to complete and so observations did not begin until 16 October 1843, 'after which date they were made hourly, day and night, by Corporal Henry and [Lefroy], and on all occasions of magnetic disturbance at intervals of about two minutes, for hours together' (fig. 5.1).[2]

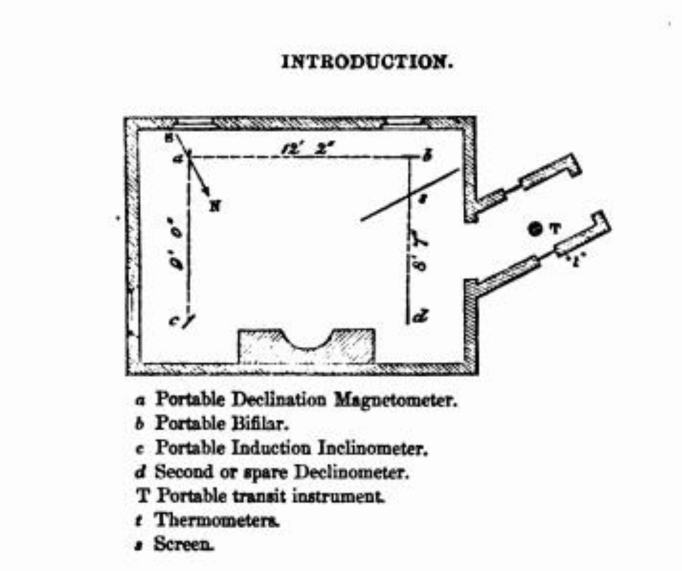

Fig. 5.1: Temporary observatory at Fort Chipewyan: 'The Bifilar was screened from the direct action of the fire by a leather curtain, the Inclinometer was screened by the projection of the chimney; the whole were mounted on firm wooden pillars disconnected from the floor', from Lefroy, *Autobiography*, xi.

Lefroy gave a more specific account of their daily work schedule in the volume of results he and John Richardson compiled and had published in 1855. According to Lefroy, 'the system of relief adopted to carry out a series of hourly observations with only one assistant, was this: A observed from 8pm to midnight, and on retiring aroused B, who observed from 1 to 5am; he in turn retiring, again aroused A, who resumed the observations at 6am, and so on for four hours alternately'.[3] It was a fatiguing system of observation but one which was kept up with little omission during the four-and-a-half months of their stay. The observatory was 'kept habitable' by a near constant fire in the fireplace which also ensured 'a more *uniform* temperature than we should probably have without it, besides being absolutely necessary'.[4] Despite the fire, the internal temperature ranged from +61˚ Fahrenheit on 19 October to as low as -1.2˚ Fahrenheit on 22 January. The temperature over a single 24-hour period also fluctuated.[5] In January 1844 it became 'necessary to cover the eye-pieces of my [Lefroy's] sextant with leather, and to be very careful not to

[1] Lefroy and Richardson, *Magnetical and Meteorological Observations at Lake Athabasca*, x.
[2] Lefroy, *Autobiography*, 83.
[3] Lefroy and Richardson, *Magnetical and Meteorological Observations at Lake Athabasca*, xi.
[4] Lefroy to Younghusband, 13 December 1843, in Stanley, *John Henry Lefroy*, 68.
[5] Lefroy and Richardson, *Magnetical and Meteorological Observations at Lake Athabasca*, xi.

touch objects of metal'.[1] However, the winter was a relatively mild one, according to Lefroy. The only discomfort, he wrote to Younghusband, arising from the wind passing through the parchment windows.[2] Extremes of cold were usually most sharply felt on Monday morning, 'the room not being occupied on the Sunday'.[3]

Within the observatory, as the picture above (fig. 5.1) shows, Lefroy set up "the Transportables": the declination magnetometer, the bifilar magnetometer and the induction inclinometer, together with the spare declinometer, portable transit instrument, and thermometers. Lefroy remarked to Younghusband that the 'little observatory' was 'very complete' and that the instruments worked 'excellently' within it.[4] The only misfortune occurred when Lefroy broke the level of the transit instrument in December. He succeeded in making one anew with one of the spare glass tubes but it had 'no delicacy' and he could not 'manage to keep the spirit from evaporating, although it is kept out in the cold', a further example of Lefroy's, admittedly limited, mechanical nous.[5]

At Fort Chipewyan, Lefroy stayed in a 'little den about 12 feet square, with one glass window and one of parchment' that opened out into the Hall, 'the great feature of all houses here' and the space in which the Indigenous peoples lived and slept when at the Fort. In going in and out of his room at night 'to and from the observations, in the dark, [Lefroy] used to be constantly trampling over some sleeping savage and hear him muttering and groaning as if he dreamed that a herd of buffaloes was going over him'.[6] This one remark speaks volumes: the white bourgeois trampling over Indigenous peoples as he seeks to extend his own enlightened scientific agenda and the First Nation man dreaming of buffaloes, an animal pushed to near extinction by white colonialists in the 1800s. The space at the Fort Simpson temporary observatory, which Lefroy inhabited at the end of March 1844, was less allegorical.

At Fort Simpson (61˚ 51.7' N.), the instruments and Lefroy were both put up in a 'detached wooden building on the north side of the principal house', making the space simultaneously an observatory and a 'dwelling-room': i.e. Lefroy's sitting room and bedroom (fig. 5.2).[7]

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[1] Lefrot, *Autobiography*, 87.
[2] Lefroy to Younghusband, 13 December 1843, in Stanley, *John Henry Lefroy*, 68.
[3] Lefroy and Richardson, *Magnetical and Meteorological Observations at Lake Athabasca*, xi.
[4] Lefroy to Younghusband, 13 December 1843, in Stanley, *John Henry Lefroy*, 67.
[5] Lefroy to Younghusband, 13 December 1843, in Stanley, *John Henry Lefroy*, 68.
[6] Lefroy to Isabella [sister], 25 December 1843, in Stanley, *John Henry Lefroy*, 84.
[7] Lefroy and Richardson, *Magnetical and Meteorological Observations at Lake Athabasca*, xi.

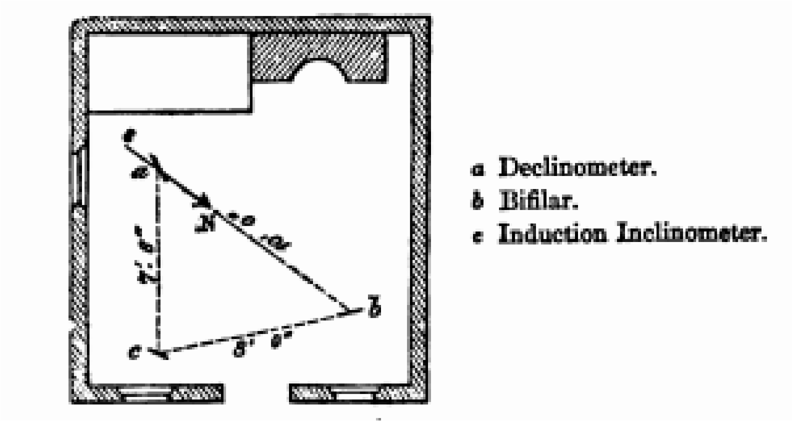

Fig. 5.2: The observatory-cum-dwelling-room at Fort Simpson. The reduced space meant a reduction in the apparatus employed in the observatory and the necessity of calculating the effect produced by each of the instruments upon one another, from Lefroy and Richardson, *Magnetical and Meteorological Observations at Lake Athabasca*, xii.

A different timetable of observation was employed here, as Lefroy told his sister that he occupied himself in the observatory after breakfast until dinner, rather than the shift work which was the norm in Chipewyan. Term Days must have been a cosy affair. Lefroy continually revisited a letter he had been writing to Riddell because 'the observatory being my sitting and sleeping room, Henry's perpetual locomotion at the Term observations, which are going on today, makes me indisposed to do anything else.'[1] Lefroy 'lived' at the temporary observatory and had his bed 'in a sort of berth in one corner close alongside of *my beloved magnetometer'*.[2]

Lefroy felt affection for his instruments because they often transgressed the easy distinction of human and non-human actors on the survey. It was common for Lefroy to talk of his instruments receiving 'bruises' to their limbs and their shoulders and it was equally common for him to refer to magnets 'contracting' rust as one might contract a disease. Frequently, the definite article is dispensed with when referring to the Gambey or Fox dip circles, making it seem as though Lefroy was observing alongside these artists, and not their products. These instruments were active participants and, as Bulstrode puts it in a different expeditionary context, 'persistently failed to be mere tools or machines'.[3] Owen Stanley, the first individual to use the Fox-type in the Canadian Arctic, talked of the instrument as his '"darling child"'.[4] Bulstrode also reports how James Clark Ross was said to be so devoted to his beloved pendulum that that his hammock swung close to it. As Bulstrode wryly remarks, 'Ross was so committed [to the pendulum] that his behaviour

[1] Lefroy to Riddell, 24 April 1844, in Stanley, *John Henry Lefroy*, 118.
[2] Lefroy to Fanny [sister], 22 October 1844, in Stanley, *John Henry Lefroy*, 133. Emphasis added.
[3] Bulstrode, 'Men, mines and machines', 28.
[4] Cambridge University Library MS Add. 9942/27. Fox, Caroline, Journal Entry, Stanley, Owen, 27 January 1844, 03/02, 1844, quoted in Bulstrode, 'Men, mines and machines', 28.

imitated the instrument'.[1] The specific beloved magnetometer, if we are to believe it was the one Lefroy lay closest alongside as per his description and the image of the temporary observatory above, was the declinometer. Lefroy was not alone in his affection for this instrument. At the observatory he had left behind in St Helena, Lefroy's successor as observatory manager, Captain Smythe, and his assistants similarly gave their declinometer life. On 20 January 1844, an otherwise lovely day on the island, a 'great misfortune' occurred: the suspension thread of the declinometer gave way. It had been in wear since the beginning of February 1842, Smythe lamented in a letter to Sabine, it was 'known to us, and we mourned for it as for a friend departed'.[2] It was, for Lefroy at Fort Simpson and Smythe on St Helena, truly as Bulstrode notes: 'the machine ran away with them and developed a life of its own'.[3]

Legacy and Conclusion

Despite the toil and the incessant, fatiguing, nature of observation on the North American Magnetic and Meteorological Survey, Lefroy, soon after his return to Toronto, was angling for an opportunity to resume survey work. In December 1844, having apparently heard of the possibility of Ross making a voyage to the Arctic Circle early in 1845, Lefroy proposed in a letter to Riddell – now an assistant at the magnetic department in Woolwich – to make a simultaneous inland expedition. However, this next expedition could not hope to be achieved even as early as spring because, as Lefroy wrote, 'the necessary repairs to my instruments can hardly be made in time'.[4] Clearly, the instruments that had returned with Lefroy from his North American survey had returned in a deteriorated state if Lefroy was estimating fully three to four months to make their repairs. The instruments had suffered but they had also produced consistent, credible, observations. As has been demonstrated by the preceding, this was as a result of different forms of management: Lefroy's own mechanical skill, the assistance of HBC Fort armourers, and the safe and sensitive carriage of the instruments in and outside of the canoe all through the North American landscape.

The point of illustrating the amount and frequency of the breakages that happened to Lefroy's instruments during the survey is, then, not to try to demonstrate that the survey was a failure or that Lefroy was an incompetent surveyor. Both are false. Lefroy's survey was an extraordinary *collective* feat of scientific endeavour that amassed magnetic

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[1] Bulstrode, 'Men, mines and machines', 29.
[2] Smythe to Sabine, 6 February 1844, TNA BJ 3/43/3.
[3] Bulstrode, 'Men, mines and machines', 29.
[4] Lefroy to Riddell, 10 December 1844, in Stanley, *John Henry Lefroy*, 144.

observations from more than 300 stations across the northlands of North America. Lefroy's survey remained the 'main standard and reference for magnetic observations in western North America for the next three decades' and Lefroy was labelled a '"highly trustworthy traveller, and one accustomed to rigorous and exact observations"' by the Austrian author and magnetic researcher Carl Weyprecht in 1874.[1] For his own part, Lefroy remained bitter and frustrated about the legacy of the survey. Lefroy wrote in his autobiography that he felt the observations made to be 'not of much interest', and served only to swell 'the volume of wasted labour' for nobody that he was aware of 'even tried to sift them or deduce comprehensive results', notwithstanding his own attempts to do so in his *Athabasca* volume. Even this, he lamented 'has never been noticed because the interest of the whole inquiry was largely factitious'.[2] In this last accusation, Lefroy was partly wrong. Lloyd at least had attempted to sift through, reduce and do something with Lefroy's observations. In 1874, Lloyd called the results Lefroy obtained at Lefroy's two temporary observatory sites – Fort Chipewyan and Fort Simpson – 'probably the most remarkable contribution to our knowledge of the phenomena of magnetic disturbances' which, importantly, revealed 'the fact that, in addition to the maximum of mean disturbance of the declination which prevails in Canada and the United States at 10 p. m., there is another and *much* greater maximum at 5 a. m., in which the easterly movement greatly preponderates over the westerly'.[3] Considering the fragility of most of the instruments of the survey, the extreme environment and climate through which Lefroy and company bore them, and the several different modes of transport they travelled by – wagon, canoe, cariole, horse, sledge, on backs and in hands – the instruments of the North American Magnetic and Meteorological Survey survived remarkably well and, as can be seen, remained sufficiently workable to make a voluminous amount of credible and significant observations.

Davis Baird has argued that 'many instruments hide the very materiality they are made from'.[4] Without the breakages which occurred along the way, this would have been true of Lefroy's instruments. The only other references to the instruments in Lefroy's journals except for those made in moments of disrepair is simple statements such as 'Obsd with Fox' or 'Observed dip with both of Gambey's needles'. To use an oft-cited remark of Latour's, 'scientific and technical work is made invisible by its own success. When a machine runs efficiently, when a matter of fact is settled, one need focus only on its inputs and outputs and not on its internal complexity. Thus, paradoxically, the more science and

[1] Smith, 'Humboldt, Sabine and the "Magnetic Crusade", 36.
[2] Lefroy, *Autobiography*, 73.
[3] Lloyd, *A Treatise on Magnetism, General and Terrestrial* (London: Longmans, Green, and Co., 1874), 217.
[4] Baird, D., *Thing knowledge: a philosophy of scientific instruments* (Berkeley: University of California Press, 2004), 19.

technology succeed, the more opaque and obscure they become'.[1] Or, as Stephen Graham and Nigel Thrift have written, 'things only come into visible focus as things when they become inoperable'.[2] This is when the materiality of Fox, Gambey, the magnetometers and the meteorological instruments becomes tangible and graspable. The point of looking for and exploring instruments in varying states of disrepair is, then, to recapture a semblance of their materiality and, following Schaffer, to understand how instruments were managed in altered states and to increase an awareness of the importance of repair and maintenance in mobile scientific practice and how this was 'dependent on relations between makers, users, and travellers'.[3] To this last point I would also add, in the specific context of Lefroy's survey, that focusing on instrument failure and repair also illuminates the particular network of HBC outposts through which Lefroy and his party travelled and in which instruments and magnetic needles were mended and reanimated.

'Each needle has its personal history' wrote Lefroy in his post factum Diary.[4] Arguably, this could be taken further to say that each needle – even each instrument – has also a personal geography. We might call this an instrument's 'object biography', 'spatio-temporal life', or 'social-spatial biography'.[5] Just as Pike distinguishes the 'geographical notion of entanglements' to demonstrate that brands and branding are inescapably intertwined with spatial associations and connotations and, crucially, that 'such attachments shape and are shaped by the agents involved', so we ought similarly to pay attention to the geographical entanglements involved in the biographies of Lefroy's instruments.[6] DeSilvey's favourite term for this, and perhaps my own too, is an object's 'geobiography'.[7]

A geobiography, as Pauli Tapani Karjalainen defines it, is 'the expression of the course of a life as it relates to the places lived'.[8] It is part of understanding objects, artefacts, scientific instruments, as more of a 'process rather than a stable entity', and that the 'provisional identity' of a thing can depend in large part on 'where they are in their

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[1] Latour, B., *Pandora's hope: essays on the reality of science studies* (Cambridge, Mass., and London: Harvard University Press, 1999), 304.
[2] Graham, S., and Thrift, N., 'Out of order: understanding repair and maintenance', *Theory, Culture and Society* 24, (2007), 1-25, 2, quoted in DeSilvey, 'Object Lessons', 150.
[3] Schaffer, 'Easily cracked', 710.
[4] Lefroy, *Diary*, 18.
[5] 'Socio-spatial biography' is a term found in Pike, A., 'Placing brands and branding: a socio-spatial biography of Newcastle Brown Ale', *Transactions of the Institute of British Geographers* 36 (2011), 206-222; 'Spatio-temporal' is a term found in Hill, J., 'Travelling objects: The Wellcome Collection in Los Angeles, London and beyond', *Cultural Geographies* 13 (2006), 340-366.
[6] Pike, 'Placing brands and branding', 206.
[7] See DeSilvey 'Object Lessons', 147.
[8] Karjalainen, P. T., 'On geobiography', in V. Sarapik and K. Tüür (eds), *Place and location: studies in environmental aesthetics and semiotics III*, (Tallinn: Estonian Literary Museum, 2003), 87-92, 87.

geobiography'.[1] Spary has similarly noted how 'local uses may fragment' a thing's meaning, 'dis-figure it' and so things can only be said to 'possess meaning and value … in relation to their specific circumstances of use and interpretation. Geography, in other words, is central to thingness'.[2] In other words, it is not only people and the passage of time that imbues value and meaning in a thing, but the geographies in which and through which it existed. For one example of this, we might profitably turn to the dip circle. Levere has rightly pointed out that a traditional, temporal, biography of the dip circle in the long nineteenth century reads largely as one of conservatism and stability of design – as indeed was the case for other magnetic instruments in this period. To read the geobiography of a nineteenth century dip circle is to read a much more unsettled and uneven biography of the object.

As was demonstrated in a previous chapter, the Gambey dip circle that Lefroy took with him to North America had previously been used during the British Magnetic Survey, 1833-38. As part of this survey, the Gambey was not only an instrument of observation but of experimentation and standardisation too, particularly in the spaces of London's Regent's Park and Westbourne Green.[3] Briefly, the Gambey was employed at these sites as an instrument against which to critique English-made dip circles and through which to calibrate and develop these same circles. These parks were shaped as spaces of site-specific experimentation by the Gambey and by extension helped shape what the Gambey – a French instrument – ironically embodied in this time and place: the emergence of British specialism in the art of terrestrial magnetic observation and the construction of instruments accurate and reliable enough for it to be a credible pursuit. The perspective of this work in many ways follows the precedent set by Bulstrode's persuasive and cogent study of the geographical entanglements – of Cornwall and Cornish mines – attached to the construction, popularisation and distribution of Fox's dip circle in the early 1830s.[4]

In like manner, reading the geobiography of Lefroy's instruments, most notably the dip circles, we discern the frequently changing and ultimately changed significance of such apparatus as they related to the places of the survey. The dip circles were frequently rendered unusable or untrustworthy during their time in the often-harsh territories through which Lefroy and his party passed. And, as has also been shown, these instruments were put back together by local HBC armourers or by Lefroy himself using what resources he could muster in the places he found himself in, and maintained as much as possible in their

[1] DeSilvey, 'Object Lessons', 147.
[2] Spary, E., 'Same/difference?', *J. Hist. Geog.* 46 (2010), 110-112, 111.
[3] Goodman, M., 'Proving instruments credible in the early nineteenth century: The British Magnetic Survey and site-specific experimentation', *Notes and Records of the Royal Society* 70, (2016), 251-268.
[4] Bulstrode, 'Men, mines and machines'.

reconstructed states by Indigenous guides and French-Canadian voyageurs. In other words, what the Gambey and the Fox, or indeed several of the other instruments, came to represent, was the physical manifestation of the combination of skills and knowledges of British and continental instrument makers together with local craftsmen, facilitated by indigenous labour. Seen in this way, these instruments represent a disruption to the traditional dichotomy of the centre and the periphery, the metropole and the wilderness, in which terms nineteenth-century imperial science is sometimes framed. A geobiography of Lefroy's instruments shows that the passage of Lefroy's survey was one taken through hybrid spaces and, in passing through, these instruments were themselves made hybrid.

Chapter 6: Establishing observatories on the British magnetic scheme

> It is evident…that the date at which the Observatories should be regarded as fully effective for the different objects proposed in the Instructions of the Royal Society, must be taken at a later period than that of the commencement of the observations.[1]

> In having us for your cooperators, you will perhaps bear this in mind…that we have not Grubb & Dollond & Simms at our command – but must make the best we can of circumstances.[2]

Introduction

In his privately circulated autobiography, mostly written between 1886 and 1889, John Henry Lefroy made it clear that he did not want to discuss the time he had spent establishing the magnetic and meteorological observatory on St Helena. He will say 'nothing' of the 'troubles and difficulties in starting the magnetic observatory. We all encountered them' he says, 'and got over them with more or less loss of time and patience'.[3] The 'we' in this sentence were Lefroy (at St Helena), Charles Buchanan James Riddell (at Toronto), Frederick Marrow Eardley-Wilmot (at the Cape of Good Hope), and Joseph Henry Kay (at Van Diemen's Land, known today as Tasmania). These were, with the exception of Kay who was an officer in the Royal Navy, the Royal Artillery officers charged with establishing, directing and maintaining what were known as the colonial magnetic observatories. These observatories were part of a much larger magnetic and meteorological observatory network which included East India Company directed sites in Madras (now known as Chennai), Singapore, Simla and Bombay (now more commonly referred to as Mumbai) as well as three observatories in the British Isles at Dublin, Greenwich and Makerstoun and dozens of others across continental Europe, Russia, the United States of America and Asia.

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[1] Sabine, E., *Observations made at the magnetical and meteorological observatory at Toronto in Canada, Vol. I – 1840, 1841, 1842* (Longman, Brown, Green, and Longmans: London, 1845), 13.
[2] Frederick Marrow Eardley-Wilmot to Humphrey Lloyd, 12 May 1841, RS MS/119/II/80.
[3] Lefroy, J. H., *Autobiography of General Sir John Henry Lefroy*, ed. Lady Lefroy, printed for private circulation (n.p., n.d.), 42.

According to Humphrey Lloyd, the number of participating observatories by 1842 stood at 33.[1] Of these 33, I will be discussing in detail just five: the four colonial observatories plus the observatory-that-never-was, at Aden. This selection was designed to address the lack of critical attention that the colonial observatories and Aden have received as constituent parts of the British magnetic scheme and because I am unable to properly engage with the archives of many of the 33 observatories – those in Germany, France, Italy, Russia and so on – because of a lack of foreign language skills. As I also noted in the literature review, physical observatories in India have been the subject of recent historical inquiry. The aim of the following discussion is to illuminate what it really meant to "establish" the colonial observatories and, following this, how the officers in charge of these institutions operated, negotiated and maintained these sites, and the instruments within them, in order to observe, inscribe and transmit the earth's magnetic field back to the centres of calculation (Dublin and Woolwich) in Britain. This is a story of the arrangement and adjustment of human and non-human actors to create a credible and workable site of scientific observation; of the multifarious and mutable spaces of the magnetic observatory; and of the local, situated, nature of conducting a geographically disparate scientific project. Correspondence between the observatory directors with Lloyd in Ireland and Edward Sabine in England, together with Sabine's own reflections on the colonial observatories in the *Proceedings* and the *Philosophical Transactions* of the Royal Society, the published results of each observatory as well as miscellaneous sources such as the account books of the St Helena and Toronto observatories, can all speak to the experience of establishing and running observatories where Lefroy was himself unable to.[2]

The chapter will be divided thematically with each section providing a different perspective on what it meant to construct and establish the colonial observatories of the magnetic crusade. Each of these sections is also concerned with the physicality and the materiality of the observatory as a thing in itself, and of the instruments which were so vital to an observatory's initial foundation. First, the chapter will begin with an introduction to the Dublin Observatory, which can be seen as the model on which the colonial observatories were built, before moving on to provide summary accounts of the lives of each of the colonial observatories. After providing this brief overview, the first section will move on to describe the different ways in which the colonial observatories evolved during their existence and argues that rather than categorising these spaces as

[1] Lloyd, H., *Account of the Magnetical Observatory of Dublin, and of the instruments and methods of observation employed there* (University of Dublin Press: Dublin, 1842), 6-7.

[2] Strictly speaking, the observatories were magnetic *and* meteorological observatories but throughout this chapter they will be referred to as magnetic observatories or, simply, observatories, for the sake of readability and because the focus of my research is the history of geomagnetic observation.

fixed, established, spaces, it is more appropriate to see them as spaces in continual states of *being* fixed and *being* established. In framing the colonial observatories in this way, it is also possible to discover some of the many different geographies of their making, which have heretofore been neglected.

The second section of this chapter is concerned with moving instruments into the colonial observatories. The act of getting instruments to the observatories was a long process, and did not start at the colonies. The magnetic observatories' instruments began their journey in England and Ireland, with their design, manufacture and testing, before they were sent aboard various ships to their respective destinations. It is argued that as the instruments were moved, so the knowledge contained within them moved and shifted for the observers who had to unpack them in the colonies. As they reassembled and readjusted the instrumentation in their different situations – spaces remarkably different from the spaces in which they had learned the art of magnetic observation – so too were the observatory directors required to reassemble and readjust their own knowledge of the instruments. It is argued that the instruments were the physical embodiment of the observers' knowledge of the science of terrestrial magnetism, and as this knowledge travelled so it was altered and had to be remade.

The third section of the chapter explores the relationship between instrument and user within the magnetic observatory, using the vertical force magnetometer as an example of the complex nature of this relationship and how it changed over time. If there is one thing which unites the correspondence of all the colonial observatories, it is the vertical force magnetometer. In this instrument, we see reflected the technical, mechanical and existential doubts which plagued all of the observatory directors and the genuine physical hardship of magnetic observation in isolated, distant, extreme climes. The changing relationship between observer and instrument will be explored and it will be shown how the officer in his observatory was expected to negotiate the spaces between instrument, printed instruction, locality and correspondence in order to make the observatory work. The concluding section will define the colonial magnetic observatories as an assemblage of different, fluid, material and human parts and situate these sites as important links to the history and historical geography of nineteenth century science, travel and observatory practice.

The observatories

Dublin Observatory

The magnetic observatory had to be mapped out and defined before it travelled to the colonial site for assembly. In theory, the colonial observatory director did not design the observatory in situ; it was designed for him by Gauss and by Lloyd years before any officer had been chosen for scientific duty although, as I will explain later, the reality of this was more complicated. The colonial observatories can trace their lineage back to Gauss and Weber's Gottingen Magnetic Observatory. This observatory had been completed in 1833 and a description of it was translated into English and printed in *Taylor's Scientific Memoirs* of 1841.[1] This observatory was the model on which Lloyd based his Dublin magnetic observatory, completed in 1838, although his observatory accommodated three magnetometers rather than Gottingen's one. Gottingen was not the first magnetic observatory to be built in the nineteenth century; magnetic observatories had existed prior to the establishment of the Göttingen Observatory and the network of associated magnetic observatories known as the *Magnetische Verein*. For instance, Alexander von Humboldt had founded an observatory at Berlin in 1829 and this site networked with new observatories in Petersburg and Kazan and extant observatories from Moscow to Siberia to make simultaneous observations of declination changes in 1830.[2] Prior to 1833 there was also a magnetic observatory at Paris under Francois Arago, another at Milan under Kreil, and several Russian institutions organised by Adolph Kupffer.

The difference between Humboldt's observatories and the Gottingen establishment was that the latter employed Gaussian instrumentation and methods of observation which, according to Lloyd, were 'far more perfect and exact than any before employed'.[3] The Gottingen magnetic observatory was the intellectual and material forebear of the Dublin observatory, and the Dublin Observatory was the head of the 'family' of observatories of the magnetic crusade and the ideal type on which the colonial observatories were constructed.[4]

The Dublin Observatory was built in the gardens of Trinity College approximately 160 feet from the nearest building and made of Portland stone – sourced from the valley of Dublin – that had been 'submitted to a rigid examination, and found to be entirely devoid of any

[1] Gauss, C. F., and Weber, W., 'Results of the observations made by the Magnetic Association in the year 1836', in Taylor, R. (ed.), *Scientific memoirs, selected from the transactions of foreign academies of science and learned societies and from foreign journals*, Vol. II., (Richard and John E. Taylor: London, 1841), 20-97; For more, see O'Hara, J. G., 'Gauss and the Royal Society: the reception of his ideas on magnetism in Britain (1832-1842)', *Notes and Rec. R. Soc. Lond.* 38, 1 (1983), 17-78.
[2] Lloyd, *Account of the Magnetical Observatory of Dublin*, 2; O'Hara, 'Gauss and the Royal Society', 27.
[3] Lloyd, *Account of the Magnetical Observatory of Dublin*, 2.
[4] Sabine to Lloyd, 17 January 1840, RS MS/119/II/85.

magnetic influence'. The walls were studded internally 'for the purpose of maintaining a uniform temperature' and to protect from damp; all nails were made of copper; and other metallic fastenings such as locks and hinges were made of brass, 'no iron whatever being used in any part of the structure'.[1] The floor of the instrument room was made of wood but the instrument piers were 'embedded in masonry and were isolated from the wooden floor and its supports'.[2] The interior was divided between one principal room, thirty-six feet long by sixteen wide, and two smaller rooms, with 'projections' on the longer sides. Light came through a dome at the top and two windows placed at the northern and southern ends. Six pillars of Portland stone were placed in the principal, magnetic, room: three which supported the magnetic instruments, two the transit instrument and one the theodolite. This was a heavily circumscribed, delineated and technical space. For instance, the pillar on which the theodolite sat was 'in the line in which the magnetic meridian [passed] through the axis of the pillar A [for a magnetometer]' and intersected 'the meridian of the transit, the distance [between these two pillars] being five feet'. In addition, to provide for the change in position of the theodolite pillar due to the alteration of the declination over time, there was a 'low stone wall beneath the floor, nine feet long and three feet wide, the middle line of which is in the meridian of the transit' and on this was placed 'a massive square base for the pillar, the position of which may be altered…when required, provision having been made in the flooring for this removal'.[3] The spaces between the magnetic instruments were also carefully managed: 30 feet between pillars A and B, and 19 feet between pillars A and C and B and C. In 1842, a separate, twelve-foot square building far removed from the main observatory was constructed 'for absolute measures of the inclination and intensity' which was comprised of studded walls, a double door, a single window with a blind for excluding direct sunlight on the instruments and three granite pillars resting on solid masonry beneath and not attached to the floor (fig. 6.1).[4]

In short, the archetypal magnetic observatory, as based on Dublin Observatory, was made of non-magnetic *stone*. The building regulated heat and light and air currents and could adapt to changes in the method of observation. The dimensions and knowledge of the Dublin observatory travelled with the observatory directors. Riddell, first director of the Canadian observatory, described the need for a similar type of observatory to the Commanding Engineer in Montreal. 'The building must be of stone', Riddell reported,

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[1] All quotations from Lloyd, *Account of the magnetical observatory of Dublin*, 12.
[2] Robinson, P. R., 'Geomagnetic observatories in the British Isles', *Vistas in Astronomy* 26 (1982), 347-367, 348.
[3] Lloyd, *Account of the Magnetical Observatory of Dublin*, 12.
[4] Lloyd, *Account of the Magnetical Observatory of Dublin*, 12.

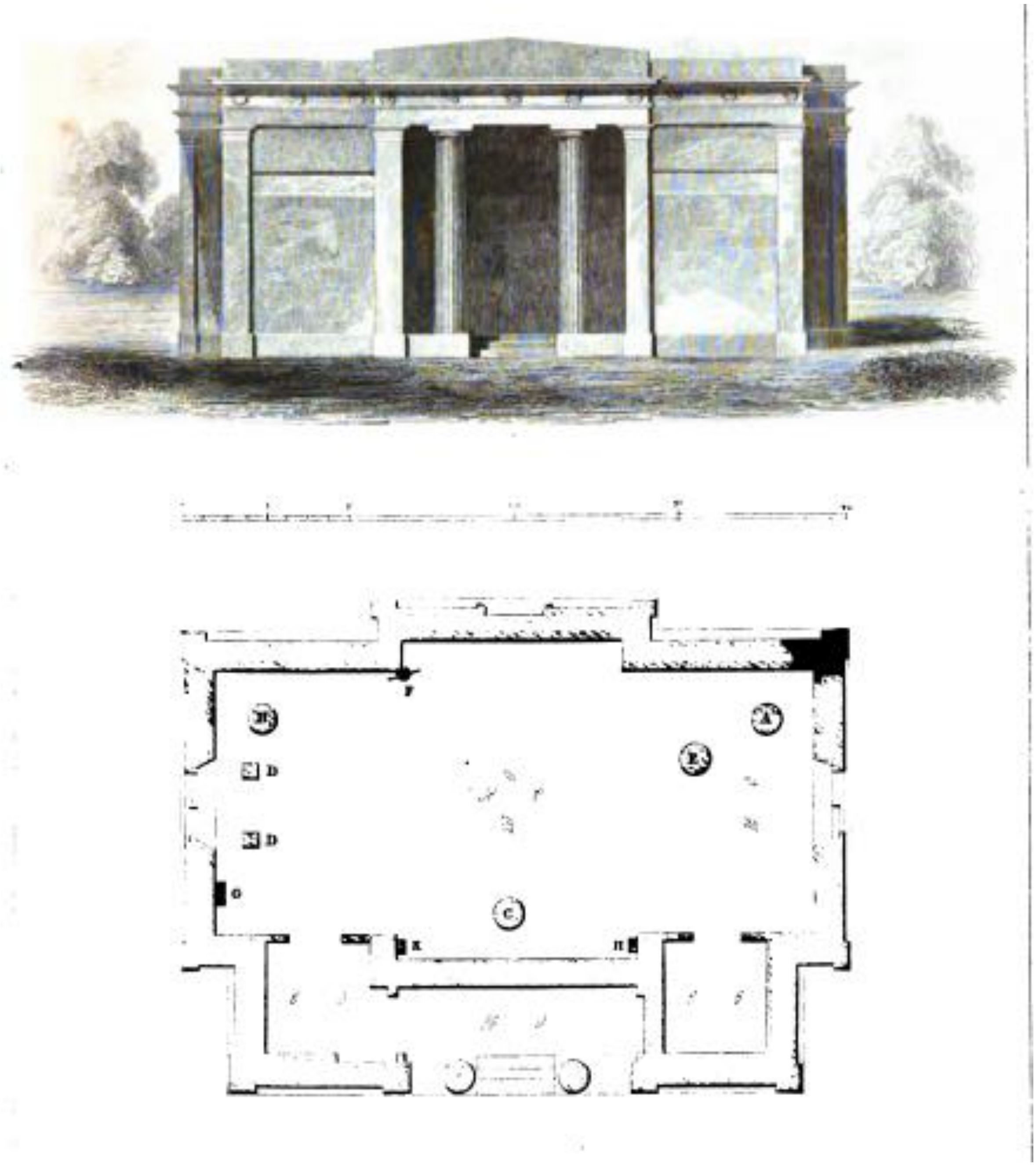

Fig. 6.1: Front elevation and plan of building, showing the disposition of the instruments at the Dublin Observatory, from Lloyd, *Account of the Magnetical Observatory of Dublin,* Plate I.

'about two feet thick and plaistered [sic.] inside and with double windows, in order to keep a uniform temperature', according to the instructions given to him by Lloyd.[1]

Dublin was where each of the artillery officers had been trained prior to their deployment, and instructions relative to their own observatory's construction were based on this design and taken with them to the colonies.[2] Sabine had written as much to Lefroy when he

[1] Riddell to Oldfield, 30 September 1839, quoted in Thiessen, A. D., 'The founding of the Toronto Magnetic Observatory and the Canadian Meteorological Service', *Journal of the Royal Astronomical Society of Canada* 34 (1940), 308-348, 312.
[2] Kay was not instructed here.

recruited Lefroy for magnetic observatory work in the spring of 1839, telling him that 'the Dublin Observatory … will be the model on which the fixed observatories will work'.[1] The officers were required to assemble their knowledge of the ideal magnetic observatory in the landscape to which they were sent; they had to translate this idea of a magnetic observatory into a physical reality using a mixture of materials sent with them from England and those which could be procured in the colony. This had to be done within the context of the imperial-military bureaucracy of which the officers and their observatories were a part, meaning a reliance on the Office and Board of Ordnance in England for authorisation and the Royal Engineers and Ordnance Storekeeper in situ for money, materials and labour. Some of the difficulties inherent in this bureaucratic system will be explored in detail in the fourth and final empirical chapter of this thesis but for now it is important to focus simply on the materials needed for, and the actual building of, the colonial magnetic observatories and what this can usefully tell us about the geographies of terrestrial magnetic science at these sites.

By and large, the colonial magnetic observatories did not end up resembling the Dublin magnetic observatory. This is not entirely surprising given that each observatory was *supposed* to be built over the course of approximately two months, rather than the year it took to construct the Dublin Observatory, and were initially only expected to operate for three years. The plan and instruction Lloyd sent with the officers was consequently simpler than his Dublin Observatory, although the observatories were still expected to be built of stone. Let us turn now to look, briefly, at a potted history of each of the colonial magnetic observatories.

St Helena Magnetic and Meteorological Observatory

Lefroy and his detachment of non-commissioned officers arrived at St Helena on 31 January 1840 having been transported there with the observatory instruments aboard the *Terror*. St Helena was chosen as a magnetic observatory site because it was 'close to the line of minimum intensity' on the globe.[2] Together with James Clark Ross, Lefroy chose a site for the construction of his observatory which was spacious, remote from the island's garrison and situated atop an elevated plain. These were the advantages. The disadvantage was, however, the local irregularities in the magnetic force occasioned by the 'nature of the

[1] Sabine to Lefroy, 10 April 1839, quoted in Lefroy, *Autobiography*, 33.
[2] Anon, *Report of the President and Council of the Royal Society on the Instructions to be Prepared for the Scientific Expedition to the Antarctic Regions* (Richard and John E. Taylor: London, 1839), 11.

soil'.[1] Lloyd, in Dublin, was also concerned about this. He wrote to Sabine in April 1840 to express that it was a 'pity that St Helena was selected' because the 'local attraction is so considerable' and might preclude useful measurements of the secular changes of the magnetic force, one of the main motivations for the establishment of fixed observatories in the first place.[2] Although Sabine assured him otherwise, Lloyd was afraid they may have made a 'false step' in choosing St Helena.[3] The local irregularities would have to be incorporated and allowed for in the results, creating a different object of observation outwith the instruments, and introducing further steps in the process of reducing observations, for which see the final empirical chapter of this thesis.

Lefroy did not build and inhabit his observatory immediately; rather, he set up a temporary observatory in two rooms of Longwood House – the residence built for Napoleon Bonaparte's exile – in which his declination and bifilar magnetometers were mounted on casks and securely fixed to the floor, until the instruments and Lefroy's detachment moved into the permanent observatory upon its completion in August 1840.[4] The construction of the observatory was not without its tribulations. There being not enough wood on the island to build of that material, stone was used instead (something which also separates St Helena from the other colonial observatories).[5] Given the impracticality of sourcing stone from elsewhere, all stone needed to be quarried from the island. Here, the advantage of the observatory occupying the only elevated plain on the island became a distinct disadvantage, as the carts – of which there were only four on the island – laden with stone, had to climb 1,740 feet and five miles from the quarry in Jamestown to reach the observatory site. This, coupled with the availability of only twelve labourers and incessant rain led to severe delays in the building of Lefroy's observatory.[6] Sabine had hoped that the St Helena observatory would be in operation six weeks after landing Lefroy at the island.[7] In the end it took six months to construct and move the instruments into the observatory. Conforming with the standard dimensions expected of a magnetic observatory at this time, the observatory consisted of 'one principal room of 45 x 16 feet, of two smaller rooms each 16 x 12 feet, and of an octagonal room of 9 feet between the sides, surmounted by a rotatory dome for the transit theodolite' (fig. 6.2).[8] The front looked north-east and was screened by a veranda which protected the meteorological instruments.

[1] Lefroy to Sabine, 2 March 1840, TNA BJ 3/81/11.
[2] Lloyd to Sabine, 27 April 1840, TNA BJ 3/10/151.
[3] Lloyd to Sabine, 4 May 1840, TNA BJ 3/10/152.
[4] Sabine, E., *Observations made at the magnetical and meteorological observatory at St. Helena, Vol. I – 1840, 1841, 1842, and 1843* (Longman, Brown, Green and Longmans: London, 1847), 9.
[5] Lefroy to Lloyd, 3 March 1840, RS MS/119/II/88.
[6] Lefroy to Sabine, 2 March 1840, TNA BJ 3/81/11; Lefroy to Sabine, 9 May 1840, TNA BJ 3/81/13.
[7] Sabine to Lloyd, 5 July 1839, RS MS/119/I/70.
[8] Sabine, *Observations made at the magnetical and meteorological observatory at St. Helena*, 9.

The observatory was in operation for almost nine years until its termination in the spring of 1849.[1]

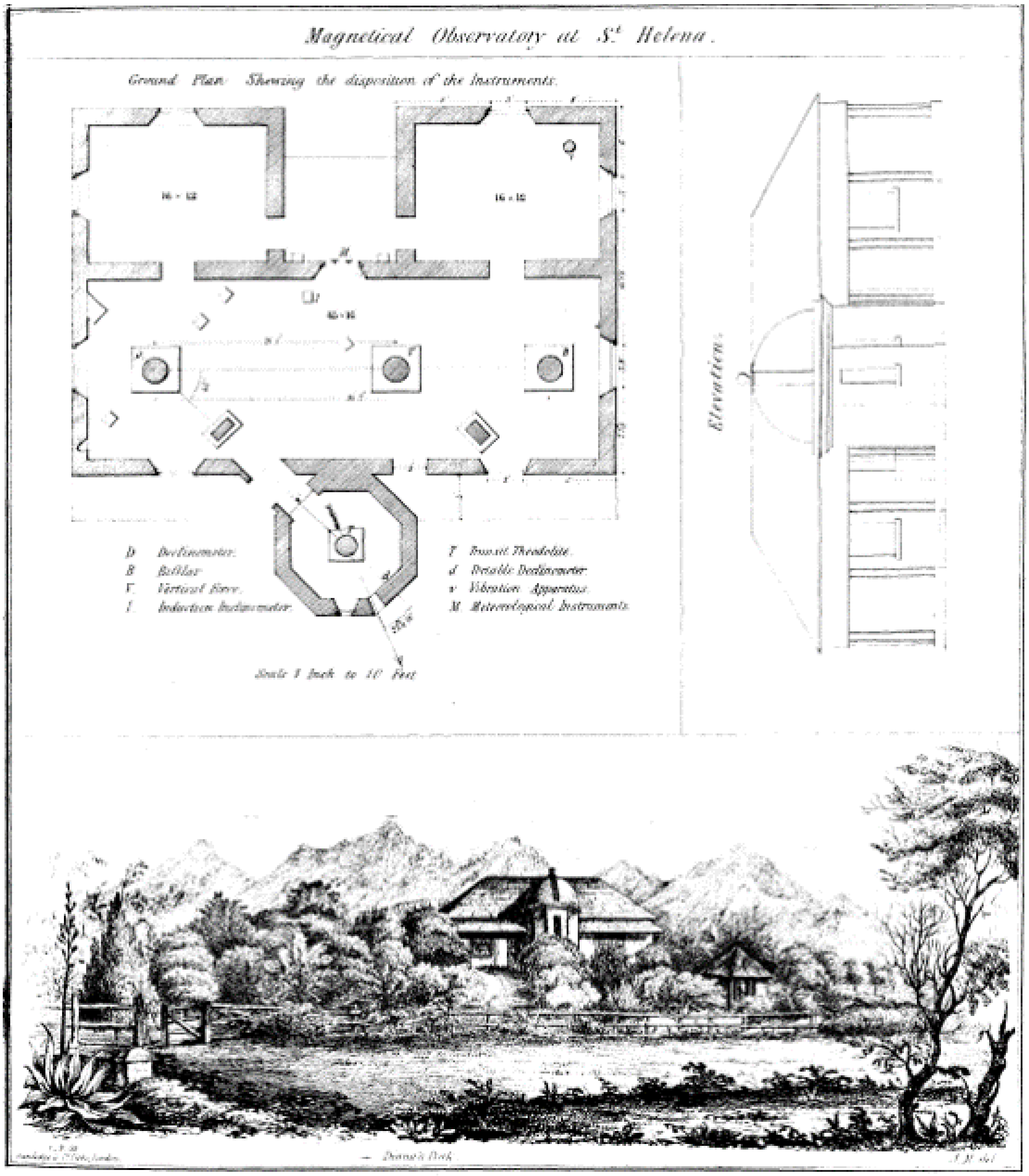

Fig. 6.2: Situation and plan of building showing the disposition of the instruments, St Helena Observatory, from Sabine, *Observations made at the magnetical and meteorological observatory at St. Helena*, 10.

[1] Carter, 'Magnetic fever', 143.

The Cape of Good Hope Magnetic and Meteorological Observatory

Lieutenant, later Captain, Frederick Eardley-Wilmot, together with three non-commissioned officers and two gunners and drivers of the Royal Artillery landed at the Cape of Good Hope on 18 March 1840.[1] Wilmot was lucky to have gotten as far as the Cape, having been washed off his feet by the surf and nearly drowned at St Paul's Rocks in the Atlantic.[2] The Cape of Good Hope was chosen as a magnetic observatory site because of its important position in a southern latitude.[3] Wilmot's first impressions of the Cape were of an 'indifferent sort of place … clear, dusty & windy' and the specific site for the observatory 'secluded and solitary'.[4] Wilmot brought with him instruments destined for the observatory, as well as 'copper bolts, nails, and other sundries' brought over from England to be used in its construction, all of which filled his initial accommodation at the Cape to such an extent that he was 'almost unable to walk about without tumbling'.[5] The detachment and its instruments had travelled aboard and been deposited at the Cape by H.M.S. *Erebus*, under Ross's captaincy. The establishment of the observatory was a tortuous experience for Wilmot. He and his instruments would not move into the building until February 1841, 11 months after landing at the Cape. By this time, Wilmot had grown so frustrated that he was 'inclined…to kick the whole of Cape Town, its people are so slow'.[6]

In contrast to the other colonial stations, Wilmot did not have the opportunity to occupy a temporary observatory while the permanent building was under construction. The neighbouring astronomical observatory did not possess any 'unoccupied room in which the instruments might be temporarily established' and Wilmot was unwilling to work outdoors in the day because 'Mr Maclear [director of the astronomical observatory] once had a large telescope blown over by a sudden gale', nor at night because it was deemed 'unsafe'.[7] As such, observations did not begin until April 1841.

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[1] Sabine, E., *Observations made at the Magnetical and Meteorological Observatory at the Cape of Good Hope, Vol. I, magnetical observations, 1841 to 1846* (London: Longman, Brown, Green and Longmans, 1851), i. The detachment was later supplemented by the addition of a fourth non-commissioned officer in August 1841, see Byham to Sabine, 6 September 1841, TNA BJ 3/27/76.
[2] Lefroy, *Autobiography*, 39.
[3] Anon, *Report of the President and Council of the Royal Society*, 11.
[4] Wilmot to Lloyd, 12 May 1841, RS MS/119/II/80; Wilmot to Lloyd, 20 April 1840, RS MS/119/II/74.
[5] Wilmot to Lloyd, 31 March 1840, RS MS/119/II/73.
[6] Wilmot to Lloyd, 9 February 1841, RS MS/119/II/75.
[7] Sabine to Sir Huw Dalrymple Ross, 10 November 1840, TNA BJ 3/27/21-24; Wilmot to Lloyd, 12 May 1841, RS MS/119/II/80.

The ground on which the magnetic observatory was eventually built belonged to the Admiralty and was attached to the astronomical observatory there. ‘A convenient site’ for the magnetic observatory was chosen by the Governor of the Colony, Sir George Napier, with the ‘permission’ of Thomas Maclear of the astronomical observatory. The building, or, more accurately, the buildings of the magnetic observatory, were constructed by Colonel Lewis of the Royal Engineers, ‘in conformity with instructions from England’.[1] Sabine described the magnetic observatory thus:

> The Observatory was 48 feet long by 28 wide in the interior, built of 12 inch logs, weather boarded and painted on the outside, with lath and plaster on the inside, a space of a foot being left between the interior plaster and the logs; it had a pitched roof, covered with felt as a protection against changes of temperature, and painted. All metal fastenings were either of cooper or of zinc. The instrument room occupied the whole length of the building upon the north side, which was further protected from the influence of the sun by a closed verandah, having doors at each end. The floor of the instrument room was of Purbeck paving-stone; the pedestals upon which the instruments were supported were of sandstone, six feet in length, imbedded in masonry to the depth of two feet, and disconnected from the floor. Separate buildings were [later] erected for Osler’s Anemometer, and for the instruments to be employed in the Absolute Magnetic determinations.[2]

The magnetic observatory remained in operation for five years. In this time, Wilmot was absent from the observatory on three occasions: twice to help defend the colony – first against the Boers in 1842, and latterly against the Caffres in 1846 – and once to return to England for a leave of absence for the greater part of 1843.[3] According to Carter, the instruments of the Cape magnetic observatory were transferred to the neighbouring Cape astronomical observatory in 1845.[4] However, Sabine stated that ‘the detachment of Artillery was withdrawn from England’ only in July 1846, at which point the charge of geomagnetic and meteorological observations passed to Maclear at the astronomical observatory.[5]

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[1] Sabine, *Observations made at the magnetical and meteorological observatory at Toronto in Canada, Vol. I.*, i.
[2] Sabine, *Observations made at the Magnetical and Meteorological Observatory at Toronto in Canada, Vol. I.*, i-ii.
[3] Sabine, *Observations made at the Magnetical and Meteorological Observatory at the Cape of Good Hope, Vol. I,* ii.
[4] Carter, ‘Magnetic fever’, 143.
[5] Sabine, *Observations made at the Magnetical and Meteorological Observatory at the Cape of Good Hope, Vol. I,* ii.

Toronto Magnetic and Meteorological Observatory

Canada was chosen to be a part of the magnetic scheme because it contained the point of maximum intensity in the northern hemisphere.[1] Toronto eventually became the designated site for the magnetic observatory but it was never supposed to be so. The site originally chosen to situate the Canadian observatory was on St Helen's Island, Montreal. Riddell, the officer sent from England to become the observatory director, reached Montreal at the end of September 1839 and originally decided that St Helen's was '"the only site or ground belonging to the Ordnance at Montreal suited for a magnetic observatory"'.[2] Alexander Dickson, Deputy Adjutant General of the Royal Artillery, had also thought this original spot a 'very quiet and eligible situation'.[3] This proved false. After consultation with a Captain Bayfield, R. N., of the 'Surveying Schooner *Gulnare*', Riddell decided that it would be necessary to 'ascertain by experiment…that the proposed site of the magnetic observatory is free from all magnetic influences' before building started.[4] This was on 14 October; by 17 October, Riddell had concluded that, based on observations of variation made on St Helen's Island by Captain Bayfield, St Helen's 'would not be an eligible situation for the magnetic observatory' because of the 'local attraction' of its immediate vicinity. Following further advice from Bayfield, Riddell decided that it would be appropriate to move the intended site of the observatory to Toronto, which Bayfield assured Riddell was free from such magnetic influences.[5]

Toronto may have been free of disturbing magnetic influences, but it was not without other issues. Information and instruction regarding the change of site from Montreal to Toronto arrived in Toronto before Riddell did. However, Colonel Ward, commanding the Royal Engineers in Toronto, had received authority only to build a single apartment, fifty by twenty feet, 'which was hardly sufficient for the needs of the observatory'.[6] A temporary observatory was established in an unused barracks at Old Fort York but this building required 'considerable repair' in order to maintain a regular and equable temperature and was unsuited for anything more than a few months occupation.[7] Two sites for the

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[1] Anon, *Report of the President and Council of the Royal Society*, 11.
[2] Riddell to Headquarters, 4 October 1839, quoted in Smith, J. A., 'Humboldt, Sabine and the "Magnetic Crusade": the founding of the Toronto Observatory', *Research report for the Canadian National Museum of Science and Technology* (1989), 1-74, 26.
[3] Alexander Dickson to Sabine, 13 November 1839, TNA BJ 3/27/5.
[4] Riddell to Oldfield, Commanding Royal Engineer at Montreal, 14 October 1839, in Thiessen, 'The founding of the Toronto Magnetic Observatory', 314.
[5] Riddell to Oldfield, 17 October 1839, in Thiessen, 'The founding of the Toronto Magnetic Observatory', 315.
[6] Smith, 'Humboldt, Sabine and the "Magnetic Crusade"', 27.
[7] Smith, 'Humboldt, Sabine and the "Magnetic Crusade"', 27.

permanent observatory were proposed and rejected – one being too close to the summer drill ground of the garrison, the other unhealthily situated in the midst of swamps and too far from town – before a request for land from the Upper Canada College was approved by the President and Council of this institution, on 28 December 1839.[1] However, work did not start immediately owing to the lateness of the season, and it was not until the following spring that construction began in earnest.

According to a letter sent by Riddell to Colonel Oldfield in Montreal soon after Riddell's arrival there, the magnetic observatory was required to be made of stone, the walls to be approximately two feet thick, as per the dimensions given to Riddell by Lloyd 'by whom all the instructions relating to the system of magnetic observations have been prepared'.[2] However, Riddell and the Toronto Observatory could not fulfil this stipulation and the observatory was eventually built of logs. The reasons for this change of plan were geographical and geological. First, stones would have to have been transported from between forty and fifty miles away, something difficult in the summer months and next to impossible during the winter, the season in which materials were procured for the building. This stone also exhibited, according to Riddell, a slight magnetic effect when warmed, which would have introduced a variable magnetic effect in the observatory. A different type of stone, Kingston Blue stone, which did not possess magnetic properties, could have been used, but this was quarried some 180 miles away from Toronto. Considering all of this, Riddell 'thought the best course to adopt was to return to the logs', an idea Riddell had previously floated in Montreal.[3] The wooden, permanent, observatory was completed on 5 September 1840 and inhabited by officers and instruments three days later.[4] The Toronto Observatory remained in operation for fifteen years before it was forced to move location due to increased magnetic disturbance caused by the electrification of tramway tracks.[5] For the first thirteen of these years it was a part of the British magnetic scheme, before it was handed over to the provincial authority in 1853.

[1] Smith, 'Humboldt, Sabine and the "Magnetic Crusade"', 28.
[2] Riddell to Oldfield, 30 September 1839, in Thiessen, 'The founding of the Toronto Magnetic Observatory', 312.
[3] Riddell to Lloyd, 4 March 1840, RS MS/119/II/7.
[4] Smith, 'Humboldt, Sabine and the "Magnetic Crusade"', 29; and Thiessen, 'The founding of the Toronto Magnetic Observatory', 339.
[5] 'Ottawa Magnetic Observatory, *Natural Resources Canada*, http://www.geomag.nrcan.gc.ca/obs/ott-en.php [accessed 18 February 2018].

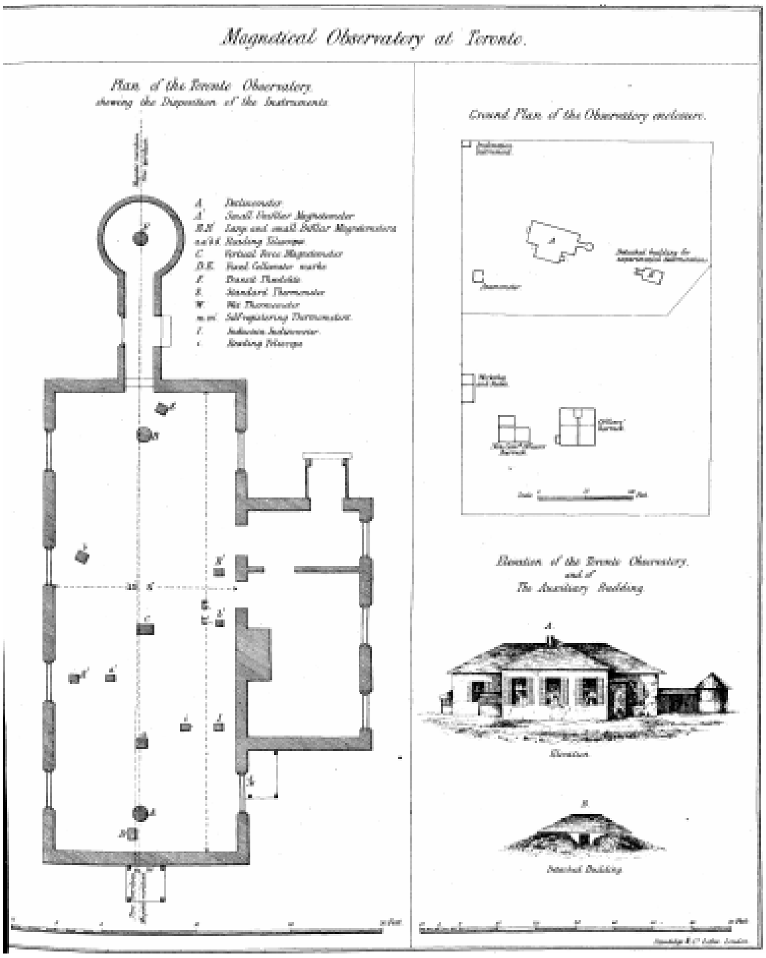

Fig. 6.3: Plan and elevation of the Toronto Magnetic Observatory, from Sabine, *Observations made at the Magnetical and Meteorological Observatory at Toronto*, 18.

Rossbank Observatory, Van Diemen's Land

Van Diemen's Land was chosen as a magnetic observatory site because it roughly corresponded to the location of the point of maximum intensity in the southern hemisphere.[1] In stark contrast to the three Royal Artillery staffed colonial observatories, the Admiralty-administered Van Diemen's Land Observatory was completed quickly and without issue. There were three important reasons for this: preparations for its construction had been made by the Governor of Van Diemen's Land, John Franklin, prior to the arrival of the observatory's director, Joseph Henry Kay, his detachment and the instruments; most of the materials for its construction had been assembled in Chatham Dockyard and transported to site on HMS *Terror*; and it was built by two hundred convict labourers. The other point on which the Van Diemen's Land Observatory was unique was that it had a name: Rossbank. This name was a homage to the observatory's "founder", James Clark

[1] Anon, *Report of the President and Council of the Royal Society*, 11.

Ross.[1] It was Ross who, on the day after the *Erebus* and *Terror* reached Van Diemen's Land in August 1840, chose the precise location of the observatory, and it took two hundred convict labourers just nine days to construct it. This meant Rossbank was ready in time for simultaneous observations to take place with other magnetic crusade observatories already established, on the Term Days of 27 and 28 August, 1840.[2] Ross remained to help Kay and his detachment calibrate the magnetometers and then departed for the Antarctic in November 1840.

According to Lefroy, who travelled with Kay on *Terror*, Kay was 'the life of the party, full of animal spirits … a capital performer on the flute' and 'always good humoured'.[3] His animal spirits and zeal were visible in his organisation of Rossbank. Rather than follow the agreed plan of observing every two hours, Kay and his staff observed hourly, day and night and, on Term Days, Rossbank carried out observations of the three magnetometers simultaneously (rather than one followed by another) every two and a half minutes.[4] Kay was able to do this in part because of the support Rossbank had from the surrounding community, as it provided volunteer observers for Term Days. A list of these volunteers appears in the second volume of Ross's *A Voyage of Discovery and Research in the Southern and Antarctic Regions*.[5] Kay was certainly, at least initially, an assured fellow. He admitted that he did not 'possess great mathematical acquirements', having joined the navy at a young age, but thought that observation required 'in itself … little more … than common sense.'[6] This was a commonly held opinion of observation as a scientific methodology.[7]

The experience of managing and calibrating observatory instruments; the unremitting labour involved in making hourly observations; and the lack of any immediate technical assistance in the colony, eventually stripped Kay of his good humour and his assumptions on the nature of geomagnetic observation but it did not dampen his zeal or industry. In 1842, after close to two years of labouring at the pedestal, Kay still talked of how 'the Ross Bank observatory will yield to none, and…be second to none, in the mass & value of the

[1] Savours, A., and McConnell, A., 'The history of the Rossbank Observatory, Tasmania', *Annals of Science* 39, 6 (1982), 527-564, 529.
[2] Savours and McConnell, 'The history of the Rossbank Observatory', 531.
[3] Lefroy, *Autobiography,* 38.
[4] Kay to Lloyd, 12 November 1840, RS MS/119/II/102.
[5] Ross, J. C., *A voyage of discovery and research in the southern and Antarctic regions during the years 1839-43*, Vol. II (John Murray: London, 1847), 3.
[6] Kay to Lloyd, 12 November 1840, RS MS/119/II/102.
[7] For example, Airy felt that 'an idiot with a few days' practice may observe very well.' Quotation from Schaffer, S., 'Keeping the books at Parramatta Observatory', in Aubin, D., Bigg, C., and Sibum, H. O. (eds), *The heavens on earth: observatories and astronomy in nineteenth-century science and culture* (Duke University Press: Durham and London, 2010), 118-147, 120.

data they will collect'.[1] Kay was as good as his word in many respects. In May 1842, he outlined how the VDL observatory had already by then 'kept 22 Term Days … with observations every 2½ minutes, making 528 hours of uninterrupted observation & this making a <u>mean average</u> of 7392 carefully noted positions of each instrument.'[2] None of the other colonial observatories matched this output. Rossbank was, technically, the longest lasting colonial magnetic observatory as it functioned from 1840 to 1854, with Kay at the helm for thirteen of those fourteen years.[3]

Constructing the geographies of the colonial magnetic observatories

> What a fine family of children the Dublin Observatory will have![4]

What were these colonial magnetic observatories? Theoretically, they were scientific spaces carefully curated to allow for the unimpeded and undisturbed observation of the earth's magnetic force at disparate points on the earth's surface at the same time and under the same conditions. They were small islands of scientific neutrality governed by the interaction of the same instruments operated in the same way at the same time and in the same climatic conditions. They were, as Carter has ably demonstrated, extensions of the British imperial will and indicative of a colonial advancement of British science. They were also military spaces. They were staffed by artillery and naval officers and non-commissioned officers and were run accordingly. Wilmot left his post at his observatory twice in order to take part in military engagements in the colony and the same was expected of all the other colonial observatories. The officers of the observatories were to be treated in the same manner as officers on regular duty in the colonies although, as the final chapter of this thesis will touch upon, this was not always demonstrated in practice.

The nature of the system of which the colonial magnetic observatories were a part made the observatories into domestic spaces. The length of time and frequency with which the observers were expected to be with their instruments coupled with the fact that any instance of irregular magnetic disturbances had to be observed too, whether in the middle

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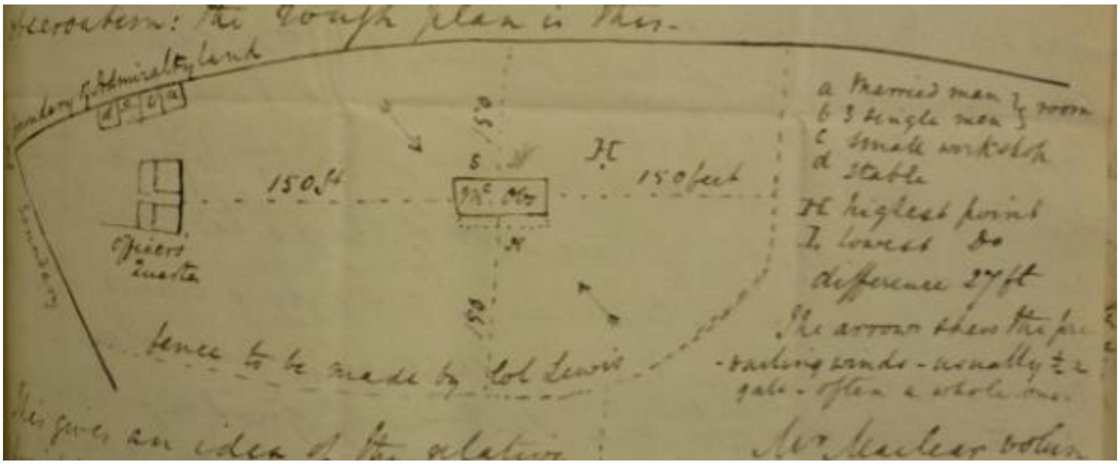
[1] Kay to Lloyd, 16 January 1842, RS MS/119/II/103.
[2] Kay to Lloyd, May 1842, RS MS/119/II/107.
[3] Savours and McConnell, 'The history of the Rossbank Observatory', 528 and appendix, 561; Toronto Observatory was in operation longer but was turned over to provincial administration in 1853.
[4] Sabine to Lloyd, 17 January 1840, RS MS/119/I/85.

of the night or the middle of the day, meant that the observatory staff had to live close by, and sometimes within, the observatory. Wilmot's observatory at the Cape initially had an 'assistant's sleeping room' (fig. 6.4). Charles Wright Younghusband, a temporary director at Toronto on two separate occasions, struggled with a 'crowded observatory' because of the marriage of some of his assistants in 1843 and 1844.[1]

To some extent, the colonial magnetic observatories were something of a novelty. As touched upon above, they were an extension of the British imperial centre and an embodiment of British physical science. This led to the carefully guarded spaces of the observatory being opened to public scrutiny and wonderment and to the positioning of the observatory as an important aspect of colonial society. On Van Diemen's Land, there was a keen will within the colonial island community to use the observatory as a means to 'show that Van Diemen's Land, which is the maximum point of intensity in magnetism is not the minimum one in all that appertains to the social relations of life'.[2] This could have humorous and in some ways significant implications for the observatory. The *Hobart Town Advertiser* reported on 'one of the fashionable *belles*, who was gratified with a view of the different "-meters" in the Observatory, and on leaning over to obtain "a sight", set the instrument vibrating, which afterwards took the officer in charge three days to make good the reckoning. It is said the fair lady had on a steel bust, which did so much unintentional mischief'.[3] Farcical as this anecdote may seem, it does in part demonstrate how heterogeneous the space of the observatory could be.

[1] Smith, 'Humboldt, Sabine and the "Magnetic Crusade"', 36.
[2] Savours and McConnell, 'The history of the Rossbank Observatory', 533.
[3] *Hobart Town Advertiser*, 16 October 1840, quoted in Savours and McConnell, 'The history of the Rossbank Observatory', 534.



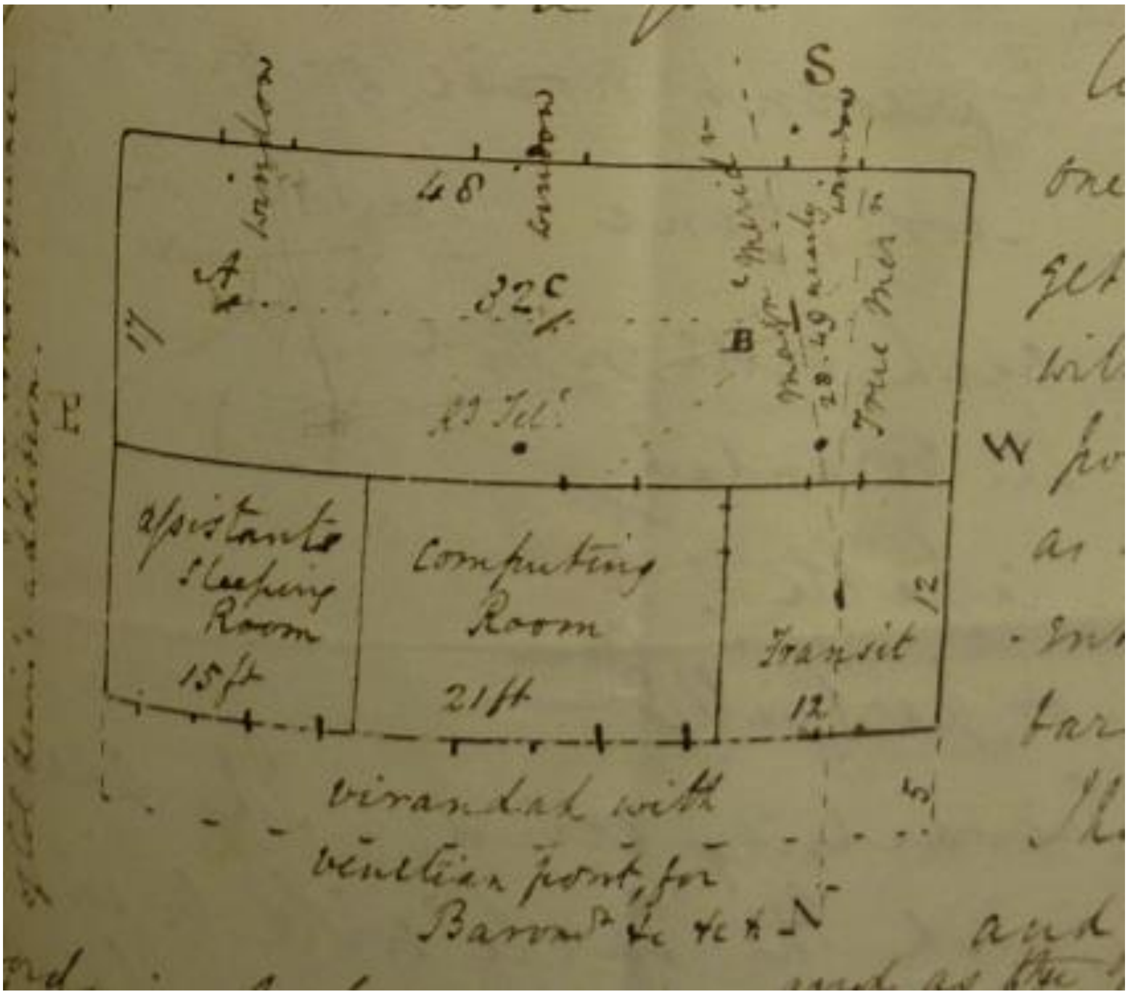


Fig. 6.4: Sketches of the placement and plan of the Cape of Good Hope Observatory by Wilmot. RS MS/119/II/74.

Fixing and establishing the colonial magnetic observatories

Of the many stories to be told about the colonial magnetic observatories, most, in this thesis and elsewhere in the secondary literature, are taken from the correspondence of observatory directors or Sabine, Lloyd, Herschel and the like, or else gleaned from scientific reports, instruction manuals and personal remembrances, such as autobiographies. Broadly speaking, these sources highlight and emphasise the human experience of working in a magnetic observatory or else they look at and analyse the vast pools of data these sites produced. The observatory is present as the multifaceted space in which these different stories took place, but it is not regularly focused on as a thing in itself: as a physical and fallible set of variously sourced materials assembled together at a certain point of space and time. The expense account book of the St Helena observatory is one source which can help to shed light on the materiality of the observatory itself, and extend our understanding of the "fixed, established, observatories" to one of observatories in a continual state of *being* fixed and of *being* established. By no means is it the only source capable of doing this. There are occasions in the observatory director's correspondence which also highlight the physicality of the observatory. Taken together, these sources show the colonial magnetic observatories to be fluid, evolving, spaces throughout their existence.

The St Helena account books detail how much continual labour was required to maintain an observatory as a space for terrestrial magnetic observation past the point at which we conventionally understand an observatory to have been 'established'. For instance, money was paid in September and October of 1841 and again in April and May 1842 for artificers and labourers.[1] In December 1842, money out of the contingent expenses – £3 0s 2½d – was paid for 'materials and work for a green baize door between the Transit Octagon and the Observatory and a canvas screen to shade the meteorological instruments';[2] three separate labourers were paid for cutting grass, framing and thatching a roof for the Dip House;[3] 'a carpenter' was paid £3 for 'various jobs done for the Magnetical Observatory between the 1st January 1843 and the 30th June 1844', something that William Smythe, director of the St Helena observatory from February 1842, describes elsewhere as a bargain considering how many little jobs the carpenter performed at the observatory.[4] This carpenter's assistance was also especially valued as 'mechanics here [St Helena] not only

[1] Account book of St Helena Observatory, 31 October 1842, TNA BJ 3/41.
[2] Account book of St Helena Observatory, 30 March 1843, TNA BJ 3/41.
[3] Account book of St Helena Observatory, 8 July 1844, TNA BJ 3/41.
[4] Account book of St Helena Observatory, 8 July 1844, TNA BJ 3/41.

get immense wages, but very frequently are not to be got for any price, more particularly at such a distance from the town', another small reminder of how the geography of an observatory's placement, and the local economy in which it was built, could affect its ability to operate effectively.[1] Repairs and alterations were still being made towards the end of the observatory's life-cycle in 1848 and 1849.

The alterations to the St Helena Magnetic Observatory were done for a variety of reasons. A dip house, which Smythe referred to as his '"lodge in a garden of cucumbers"', was constructed at a place 'sufficiently remote from the magnets of the observatory', as per the revised instructions of the Royal Society, to facilitate more accurate determinations of magnetic inclination.[2] It is the small building visible in the sketch of the St Helena magnetic observatory included in the first printed volume of its observations by Sabine on the right of the observatory as you view it (fig. 6.2). The roof of the dip house was thatched in July 1844 because Smythe had been, he reported, at work for the four months previous 'almost incessantly in the Dip House…[and] found the heat almost killing', a problem that the thatched roof 'perfectly removed'.[3] For similar reasons of trying to create spaces better suited for the accurate determination of the earth's magnetic force in absolute measure, an underground room was constructed at St Helena by the Royal Engineers – a department Smythe had always tried to avoid using for alterations because of their expense and tardiness and who Smythe drily commented could never 'be accused with anything approaching to a run'.[4] However, as the underground room was to be built under the chapel at Longwood House, the Engineers were required. As with the dip house, this underground room also underwent changes to try and make it a more secure site, and one less exposed or at least more adapted, to the climate of St Helena. A closed drainage system was installed to replace the pump which had, on very wet days, meant the need for the observatory staff to pump water from the underground room twice a day. Observing the instruments was not always the chief employment of St Helena Observatory staff. The new drainage system kept the room much drier and negated the need for this particular, extra-scientific, activity.[5]

Similar out- and subterranean buildings, as well as alterations and repairs to the main observatory building, were made at all of the colonial observatories. At the Cape, Wilmot extended his observatory with the construction of an 'underground house' for absolute

[1] William James Smythe to Sabine, 15 July 1844, TNA BJ 3/43.
[2] Smythe to Sabine, 10 December 1844, TNA BJ 3/43; Anon, *Revised instructions for the use of the magnetic meteorological observatories and for the magnetic surveys* (Richard and John E. Taylor: London, 1842), 4.
[3] Smythe to Sabine, 15 July 1844, BJ 3/43.
[4] Smythe to Sabine, 6 February 1844, BJ 3/43.
[5] Smythe to Sabine, 8 August 1846, BJ 3/43.

determinations of magnetic intensity and through the placement of a large cedar barrel, bought from a condemned slaver, partly in the ground and partly heaped over with dirt in which Wilmot suspended a needle, at a distance of about 100 feet from the main observatory building (fig. 6.5).[1] At Van Diemen's Land, a detached building for experimental determinations and observations of the absolute force was erected at a considerable distance from the main observatory in November 1844.[2] Changes were also made to the space of the main observatory. In May 1843, a wooden partition running the entire length of the instrument room and lined with blankets was put up and, in addition, the wooden sides of the building were made 'air tight' by banking earth around the building 'to prevent the entrance of air currents between the wooden plates of the building and its stone foundation'.[3] The alterations at Van Diemen's Land and at the Cape were made in order to create an observatory capable of keeping a more regular and equable temperature than had been displayed in the first couple of years of their operation. The internal temperature of Kay's Rossbank Observatory had been described by Lloyd as the 'serious defect' of the observatory and consequently of the results they were able to produce.[4] Keeping temperature, just like keeping the books or keeping the time, was vital for the accordance of results made by the network of observatories in the magnetic crusade. The final chapter of this thesis will explore in greater detail the process involved in applying corrections to observations to allow for such things as the variable temperature of the observatory.

At the Cape, the changes created spaces more capable of maintaining a regular temperature. The barrel, Wilmot wrote to Lloyd, worked 'most capitally – a few spirit lamps would raise it very shortly to a sufficient temp[erature] to boil a beef steak, while the sun's heat has scarcely any effect upon it'.[5] The underground house Wilmot described as 'a perfect Palace for vibrations' which exhibited only half a degree of temperature variation over a three-day period.[6] Temperature variation was, however, still a problem within the main observatory building and the same could be said for Rossbank, despite Kay's attempts to control air currents with blankets and earth. An extra measure was needed, one developed by Johann von Lamont and described in the revised instructions of the Royal Society.

[1] Wilmot to Lloyd, 5 June 1841, RS MS/119/II/81.
[2] Savours and McConnell, 'The history of the Rossbank Observatory', 540.
[3] Sabine, E., *Observations made at the magnetical and meteorological observatory at Hobarton, in Van Diemen Island and by the Naval Expedition, Vol I – commencing with 1841* (Longman, Brown, Green, and Longmans: London, 1850), x.
[4] Lloyd to Kay [copy by Kay], 26 August 1841, RS MS/119/II/105.
[5] Wilmot to Lloyd, 10 July 1841, RS MS/119/II/82.
[6] Wilmot to Lloyd, 3 September 1841, RS MS/119/II/84.

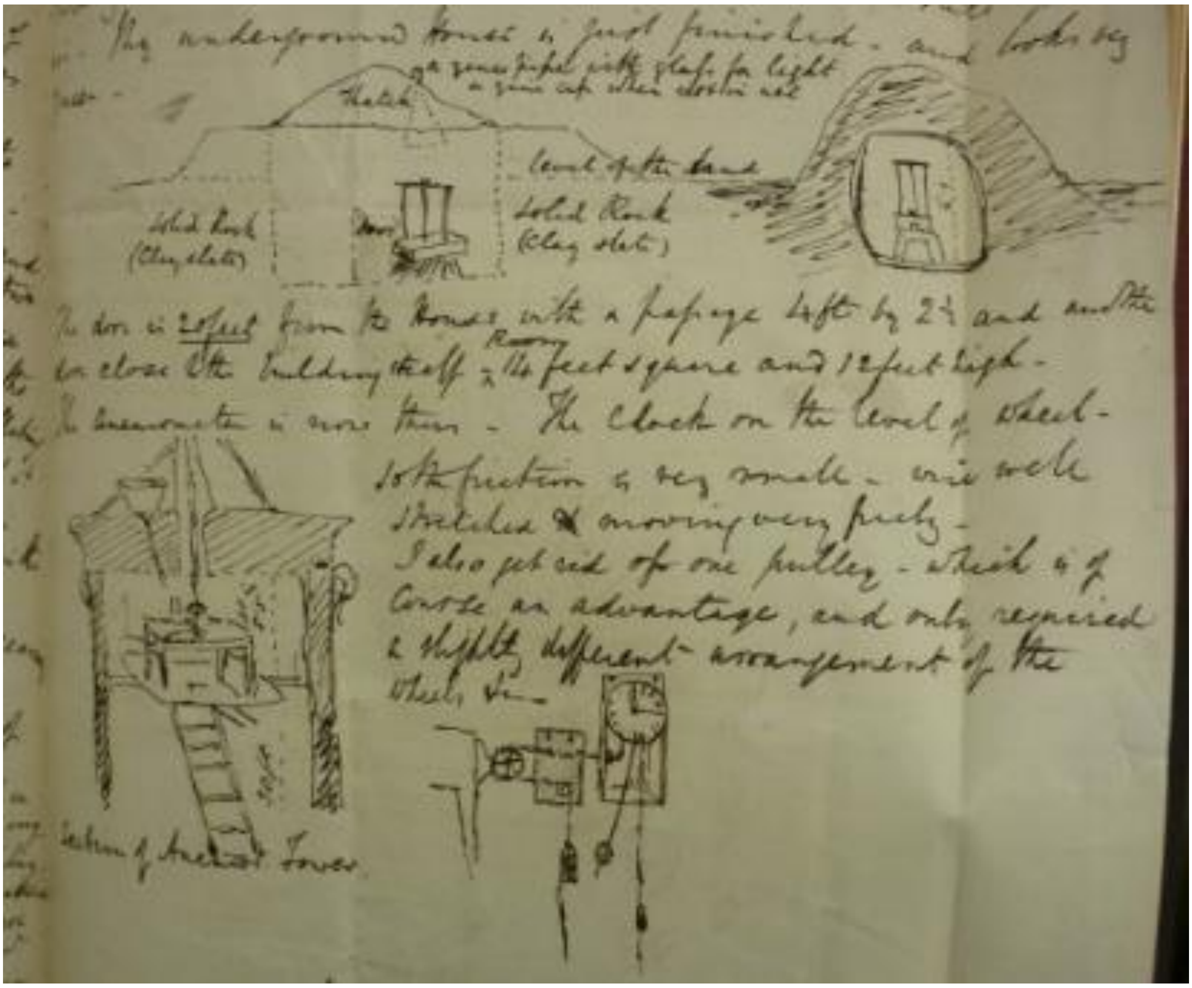


Fig. 6.5: Sketch by Wilmot of his subterranean cedar barrel (top right), underground house (top left), anemometer tower (bottom left) and a clock (bottom right). RS MS/119/II/83.

Fig. 6.6: Example of a detached, underground, building for absolute determinations built at the Toronto Magnetic and Meteorological Observatory. Sabine, *Observations made at the Magnetical and Meteorological Observatory at Toronto*, 18.

Lamont had investigated why it was that 'the changes of position of two magnets similarly suspended frequently disagree, even in places close to each other' and had concluded that this was caused by 'the operation of aerial currents generated within the box [of the magnetometer] by the changes of temperature, and which, by their circular movement, keep the magnet deflected from its true position'. The 'remedy' for this 'evil' was to make the apparatus as air tight as possible and remove the circulation of air currents 'by septa [i.e. partitions] properly placed'. It was therefore recommended that 'the edges of the declinometer and bifilar magnetometer be carefully closed, and that the magnet in each instrument be surrounded with an interior box of a narrow rectangular form'. Airy, Astronomer Royal at Greenwich Observatory throughout the years of the British magnetic scheme, had surrounded his magnets with a double case of wood, 'each being gilded within and without for the purpose of guarding against the effects of radiant heat' and this had been found 'effectual'.[1]

At Van Diemen's Land, Kay did not enclose the bifilar and the declinometer but the bifilar and the vertical force magnetometer, possibly because the latter was the most troublesome instrument in the observatory. And, in addition to this, rather than only enclosing each instrument with extra wooden partitions, Kay filled the interval between the two cases – about three inches – with clay.[2] This was an intriguing alteration of Kay's and it neatly encapsulates how the process of translating the idea of an ideal physical observatory into a working physical observatory in different locales required the observatory to be deeply embedded in its environ. Soil and clay were earthy elements the ideal observatory sought to escape from but, ironically, in order to achieve such a terrestrial displacement, the observatory and its instruments within had to more closely embrace these elements.

The ideal magnetic observatory, as described by Gauss and Weber and as constructed by Lloyd in Dublin, was a space which excluded all magnetic influences other than that of the earth's magnetic force. In this ideal construction, the interior of the magnetic observatory was a rigidly bounded and regulated space, defined by the relationship between calibrated magnetic instrumentation that interacted with the observer through the medium of the reading telescope and only sparingly through touch. In this idealised space, time, temperature, wind, rain were all either excluded or regulated and made measurable and conformable to other magnetic observatories. To Lloyd's mind, what gave this ideal observatory its greatness was its unity; in other words, the almost total annihilation of its geography, save for its positionality in reference to the earth's magnetic force. The ideal

[1] All quotations from Anon, *Revised instructions,* 11-12.
[2] Sabine, E., *Observations made at the magnetical and meteorological observatory at Hobarton*, x.

magnetic observatory was indicative of the view from nowhere.[1] However, taking the ideal magnetic observatory from the page and placing it in different landscapes gave it a great many somewheres and different geographies.

One of the first geographies of note is made visible in the physical construction of the colonial magnetic observatories. In fact, many of its geographies stem from this point. It mattered whether the observatory was built of stone or of wood. This made a fundamental difference to the observatory as a space of science and observation. The St Helena Observatory, made of stone, was different to the Toronto, Van Diemen's Land and Cape observatories, made of wood. At the latter three, temperature variation was a significant and perpetual problem. The Toronto Observatory's magnetic instrument room could not be warmed – stoves would have meant too much iron in the vicinity – and so the temperature inside often reflected the temperature outside. Temperature variation at Van Diemen's Land outside the observatory could range as much as thirty to thirty-five degrees in the summer; and inside the observatory from four to twelve degrees.[2] This may seem a small range but it was the observatory's most serious defect, according to Lloyd.

Applying temperature corrections was a laborious undertaking that required both an experimental and a mathematical acumen, a capacity for which was not part of the criteria in the decision to employ officers of the Royal Artillery and Royal Navy as observers. It was a 'physiognomical [sic.] fact', said Smythe, that 'no <u>director</u> can hear the words "Temperature Corrections" spoken without exhibiting a very remarkable and by no means agreable [sic.] change of countenance'.[3] And this from the stone building of the St Helena observatory, which never experienced temperature variations as pronounced and influential as the other colonial stations did. At Toronto, the volume of necessary temperature corrections created backlogs in the reduction of observations and was described by Lloyd as a 'very <u>large</u>' problem in the results of the observatory.[4] Even by 1842, shortly before the returns of the Toronto Observatory were expected to be published, Riddell, by this time at Woolwich, was concerned that the entirety of the results would have to be re-corrected for temperature.[5] In contrast, temperature corrections at the St Helena Observatory were never as extensive or as much of an influence on the observatory's results.

However, the materials of St Helena's construction created their own problems. Different types of stone were used in the building, several of which were 'varieties of green stone

[1] Shapin, S., 'Placing the view from nowhere: historical and sociological problems in the location of science', *Trans. Inst. Brit. Geog.* 23, 1 (1998), 5-12.
[2] Kay to Lloyd, 2 March 1841, RS MS/119/II/104.
[3] Smythe to Sabine, 6 February 1844, TNA BJ 3/43.
[4] Lloyd to Riddell, 14 November 1840, RS MS/119/II/17.
[5] Riddell to Lloyd, 6 May 1842, RS MS/119/II/25.

trap, trachytic porphyry, and cellular lava' and all of which were magnetic; the cellular lava even possessed polarity which could, Sabine wrote, deflect the 'magnet of the declinometer several divisions of the scale when placed at the distance of 19 inches from the magnet.'[1] The material of the observatory had therefore to be taken into consideration in arranging, adjusting and observing the instruments.

Whether of stone or of wood, the magnetic observatory could still be a porous entity in one specific way: insects. The ants at the Cape were 'fellows that would lift one out of bed almost'[2] and on one occasion Wilmot, having adjusted and observed the vertical force magnetometer, found its results so 'singular' that he was 'sorely puzzled about it, and did not discern till the end of the day, that a tolerably sized insect was perambulating up and down the bar'. This, he wrote, of course made his results 'quite useless'.[3] In June of the same year Wilmot wrote to Lloyd to complain that 'the insects have again annoyed [him] notwithstanding repeated washing of the box with corrosive sublimate & spirit of wine. They can hardly get in, as the box is air tight'.[4] Similarly, at Van Diemen's Land, Smythe, in one of his first letters to Lloyd, humorously noted that he 'saw in the papers that the pope had anathematized the performers of animal magnetism' and could only hope 'that this includes insects that have the vanity to recreate themselves with a see-saw on the V.F. needle'.[5] As with the anecdote of the lady with the steel bust at Rossbank, these sorts of episodes may appear throwaway but they help to establish a picture of the observatory as a less rigidly defined and exclusionary space than these sites can appear in the literature.

In part as a reaction to the problem of temperature variation at the observatories and in part because of a need to create isolated spaces for the more accurate determination of the absolute values of the earth's magnetic force, the magnetic observatory evolved a lot during its short lifespan. It is perhaps better, at least by 1842, to describe magnetic observatories as magnetic observatory *complexes*, such was the number of different dwellings – scientific, domestic and military – under its umbrella. The colonial stations came to resemble, to use Savours and McConnell's paraphrasing of Ross's description of the Van Diemen's Land site, more of a 'pretty-looking village' than one singular, homogenous observatory building.[6]

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[1] All quotations from Sabine, E., *Observations made at the magnetical and meteorological observatory at St. Helena*, 9-10.
[2] Wilmot to Lloyd, 15 March 1841, RS MS/119/II/77.
[3] Wilmot to Lloyd, 26 March 1841, RS MS/119/II/78.
[4] Wilmot to Lloyd, 5 June 1841, RS MS/119/II/81.
[5] Smythe to Lloyd, 4 April 1842, RS MS/119/II/96.
[6] Savours and McConnell, 'The history of the Rossbank Observatory', 533.

The evolution of the site of the magnetic observatory was part of a process designed to make these sites function independently of their local environment. Alterations were made to reduce unwanted disturbances – what is now commonly referred to as magnetic ‘noise’ – in the observatory. The observatory needed to be a place of magnetic quietude to be a place capable of producing credible and usable geomagnetic measurements. However, to achieve this, the observatory often had to become more embedded, more a part of the landscape in which it sat, more of a ‘somewhere’, before it could become more of a ‘nowhere’. This was perhaps made most visible at the Rossbank Observatory. Here, not only was Van Diemen’s Land earth banked around the sides of the observatory – to help prevent air currents within – but two of the three most important magnetic instruments – the bifilar and the vertical force magnetometer – were enclosed in extra cases packed with the clay of Van Diemen’s Land. At the Cape, Wilmot buried some of his instruments in the ground in a cedar barrel heaped over with soil. And at St Helena, the Royal Engineers constructed an underground room for Smythe’s magnetic observatory beneath the chapel of the residence built for the exile of Napoleon. All of these changes were to make terrestrial magnetic observations at the observatory less subject to the disturbance of their local surroundings but this was achieved by literally and figuratively extending and embedding the observatory and many of its instruments within their localities. It was a zero-sum game: geographies were introduced to remove other geographies. The colonial magnetic observatories were fixed, established, observatories but, at the same time, they were perpetually *being* established, and *being* fixed. They were not absolute, concrete entities, but permeable objects subject to a number of different, local, geographies. The next two sections of this chapter will go on to explore how the observatory’s instruments can similarly be approached from this perspective.

Geographies of magnetic instrument travel

Once the physical materials needed to assemble a magnetic observatory had been put together, the instruments could be moved in.[1] This was a complicated process. The three principal magnetic instruments for the Cape, and for the other three colonial magnetic observatories, had been built at two different workshops in England and Ireland. The declination and horizontal force magnetometers had been constructed by Thomas Grubb and his staff in Dublin and the vertical force magnetometer had been built by Thomas

[1] As is explained later in this chapter with regard to the Aden Observatory, the instruments were also part of the initial process involved in correctly siting and aligning the observatory.

Charles Robinson – under Lloyd's instruction – in London from his shop at 38 Devonshire Street, London.[1] Once constructed, the magnetometers were transported aboard HMS *Erebus* and HMS *Terror*. Wilmot and his instruments at the Cape had travelled on *Erebus*. As was discussed with regard to the British Magnetic Survey, the first chapter of this thesis, even short distance travel could have serious consequences for the condition of instruments and the efficacy of magnetic needles and travel to the Cape was on a much larger scale than travel within the British Isles.

The holds of the *Erebus* were 'hot and damp' spaces that delicate magnetic observatory instruments were not accustomed nor acclimatised to. The catalogue of afflictions caused by such a space as this was long indeed, and proved extremely 'disheartening' for Wilmot.[2] The heat, Wilmot says, 'melted the lids (that they were glued) off the boxes' of his dipping needles and how Robinson – the maker – could send this instrument and its needles 'with no other fastening but glue to the backs' Wilmot could not comprehend. 'Every iron & steel article' arrived 'rusty' and 'completely spoilt';[3] the standard barometer was broken; the portable barometer 'so nearly stewed that the ivory ring which marked with the various corrections, was burst open'; the ivory scales of the 'beautiful wet bulb' [thermometer] were bent and the screws burst out 'by the warping'; all glued cases melted out and open; 'sliding boards of the anemometer table shrunk'; the needle for the vertical force magnetometer rusted;[4] and the axle of the dip circle affected, Wilmot believed, from the warping of its box in transit.[5] This last problem, the injurious state of the Robinson dip circle, was particularly troublesome.

Initially, Wilmot's concern was with rust on the needle of the dip circle, however it soon became apparent that the axle had also been disturbed in some way, despite the fact that it had no rust on it, possibly as a result of the warping of its box in transit. Wilmot had taken observations of dip with the instrument three days after arriving at the Cape but the discordant results had 'completely puzzled' him and so he did not continue to make observations at any regular intervals.[6] Wilmot had found 'differences of 20°' in observations taken with the dip circle even in the 'same spot, time & circumstances' and thus he says it would have been 'quite childish' to have continued inclination observations

[1] For more information on Robinson and some of his instrument designs, see Stock, J. T., 'Thomas Charles Robinson and his balances', *Journal of Chemical Education* 45, 4 (1968), 254-257. For more on Grubb, see Elliott, I., 'Grubbs of Dublin: telescope makers to the world', in *Science and technology in nineteenth-century Ireland* (Four Courts Press: Dublin, 2011), 47-61.
[2] Wilmot to Lloyd, 20 April 1840, RS MS/119/II/74.
[3] Wilmot to Lloyd, 31 March 1840, RS MS/119/II/73.
[4] Wilmot to Lloyd, 20 April 1840, RS MS/119/II/74.
[5] Wilmot to Lloyd, 9 February 1841, RS MS/119/II/75.
[6] Wilmot to Lloyd, 19 February 1841, RS MS/119/II/76.

until a new dip circle could be sent from Robinson.[1] Wilmot informs Lloyd that he had immediately – in March 1840 – taken the precaution of sending to Robinson for a new circle.[2] And yet a new circle did not arrive until sometime in July 1841, fully sixteen months after Wilmot had written to Robinson. Communication between the Cape and Britain was often slow, but this delay was particularly curious.

The problem, according to Sabine's communication with Wilmot on the matter, lay with Robinson not being able to make the same circle 'without a pattern'.[3] Wilmot's wonderment at this statement is palpable. Not once, he wrote to Lloyd, could he have imagined 'that any maker would let a carefully executed instr[ument] leave his shop without having some account of its dimensions'.[4] In a later letter, Wilmot again expressed his distress at the situation with the dip circle, telling Lloyd that:

> the matter of the dip has caused me more pain than I can describe to you. I have oilstoned and scrubbed the needle till it appears a hopeless task – so deep is the rust eaten in. If I could have anticipated for a moment the necessity of a pattern for Robinson to work by, it should have been sent to him the instant I opened the box.[5]

It seems by this statement that what was required was for Wilmot to have sent the dip circle back to Robinson so that Robinson could construct a new circle from the dimensions of the old, rust eaten, injured one. Robinson apparently had no recording of the instrument's construction other than the instrument itself. What might explain this?

It is possible that the lack of a plan on Robinson's part has something to do with the amount of testing, development and construction of new dip circles that took place as a direct result of the British Magnetic Survey. Throughout 1837 and the summer of 1838, Robinson had been engaged in trying to improve, specifically, the design of the axle on his dip circles. He had produced different iterations of axles on the continental (read: Gambey) model, presumably by working from actual examples of these types of dip circles and through this process had learned to produce dip circles with more effective axles by the 'workmanship of his own hands'.[6] While this is possible, it still does not adequately explain the lack of Robinson having a plan for the dip circle. One additional reason for the delay might simply have been the poor state of Robinson's health from late 1840 until his

[1] Wilmot to Lloyd, 9 February 1841, RS MS/119/II/75.
[2] Wilmot to Lloyd, 19 February 1841, RS MS/119/II/76.
[3] Wilmot to Lloyd, 9 February 1841, RS MS/119/II/75.
[4] Wilmot to Lloyd, 19 February 1841, RS MS/119/II/76.
[5] Wilmot to Lloyd, 15 March 1841, RS MS/119/II/77.
[6] Sabine, E., 'A memoir on the magnetic isoclinal and isodynamic lines in the British Islands, from observations by Professors Humphrey Lloyd and John Phillips, Robert Were Fox, Esq, Captain James Clark Ross, R.N., and Major Edward Sabine, R.A.', *Report of the Eighth Meeting of the British Association for the Advancement of Science; held at Newcastle in August 1838* Vol VII (London, 1839), 49-195, 52.

death in July 1841 of chronic bronchitis.[1] In this time, Robinson became increasingly less communicative with Lloyd and Sabine. Robinson being unable to work without a plan may in reality have been an excuse to disguise the fact that a slowly declining Robinson was simply unable to work at all.

When they did arrive in July 1841, the replacement dip circle and its needles were in 'splendid order', according to Wilmot. His only gripe was that they should have been dipped in melted wax rather than tallow, 'as it would then peel off at once' and that the needles should have been allowed less horizontal movement in the box as they had been blunted thereby.[2] Clearly, improvements had been made in the way in which scientific instruments were carried at sea. Although the results Wilmot observed from this new instrument were not initially as satisfactory as he wished, Wilmot saw this as part of the process of him becoming expert in the use of this new iteration of the Robinson dip circle, rather than any fault in the instrument itself.[3]

The difficulties created by the exigencies of travel on magnetic instruments were not confined to Wilmot at the Cape, but his certainly suffered more explicitly than instruments sent to the other colonial observatories. Lefroy's St Helena instruments survived the journey from England in comparatively good condition, which is all the more remarkable because Lefroy departed England with Wilmot on the same voyage. However, unlike Wilmot, Lefroy travelled on the *Terror*, captained by Henry Crozier, and so, presumably, did his instruments.[4] The *Terror* and the *Erebus* were similar ships – both of them bomb vessels – and yet while Wilmot's instruments warped and rusted significantly in the sweaty bowels of the *Erebus*, Lefroy's remained largely unchanged aboard the *Terror*, although one of his needles was touched by rust. The only serious injuries among Lefroy's instruments were those which befell the standard barometer – which, like many barometers in this period, leaked some mercury – and the minimum registering thermometer for terrestrial radiation, which broke.[5] Another of Lefroy's barometers, made by Newman, was also put out of action by a loss of mercury, but this was as a result of its being transported over the rough roads of Pico Ruivo, one of many short stops the Ross party made on their way to Antarctica, and not when it was in transit on the ship.[6]

The instruments Riddell took charge of in establishing the magnetic observatory at Toronto also, according to Riddell, 'escaped wonderfully well' despite a three-month journey from

[1] Stock, 'Thomas Charles Robinson', 256.
[2] Wilmot to Lloyd, 10 July 1841, RS MS/119/II/82.
[3] Wilmot to Lloyd, 7 Aug 1841, RS MS/119/II/83.
[4] Lefroy, *Autobiography*, 35.
[5] Lefroy to Sabine, 2 March 1840, TNA BJ 3/81/11.
[6] Lefroy, *Autobiography*, 35.

England and a 500-mile inland journey.[1] His maximum and minimum thermometers were broken, as was one of the long glass tubes of a magnetometer, but generally all survived intact. It could have been a very different story altogether though. A storm during the crossing led to 250 barrels of ammunition, stored underneath the instrument boxes, being thrown overboard and, Riddell says, if his 'own people had not been there' his instruments 'would of course have all gone'.[2] Kay, director at Rossbank, also seemed to have no explicit issue with how his instruments had travelled. Although Wilmot was the only one of the colonial observatory directors to experience the damaging effects of travel on magnetic instruments during the magnetic crusade, his predicament was not an isolated one, nor the most extreme example; that honour goes to Henry Yule and the bungled attempt to set up Aden as one of the several East India Company directed magnetic observatories.

Aden: the magnetic observatory-that-never-was

> …and on arriving at Aden he found that the intended observer was dead, the observatory not commenced, and the instruments all broken.[3]

Henry Yule is best known to posterity as an eminent geographer and scholar of Central Asia, noted especially for his acclaimed two-volume work on the travels of Marco Polo, first published in 1871.[4] Yule had joined the East India Company's Military College, at Addiscombe, in 1837, and 'having passed out at the head of the college in 1838, he spent a year training at the headquarters of the Royal Engineers at Chatham'.[5] In 1840 he was appointed to the Bengal Engineers and ordered to India for service in the Khasia Hills. However, although it is absent from Felix Driver's entry on Yule in the *ODNB*, Yule's journey to India first included a stop at Aden to report on the water supply and to 'deliver a set of meteorological and magnetic instruments for starting an observatory there'.[6] Prior to Yule's journey, in May 1840, Yule had travelled to Dublin to receive instruction from

[1] Riddell to Lloyd, 8 October 1839, RS MS/119/II/2.
[2] Riddell to Lloyd, 8 October 1839, RS MS/119/II/2.
[3] Yule A., 'Memoir of Sir Henry Yule', in Yule, H. (ed.) and Cordier, H. (trans.), *The book of Ser Marco Polo, the Venetian, concerning the kingdoms and marvels of the East*, , 3rd edn, 1 (1903), xxvii-lxxxii, xxxiv.
[4] Driver, F., 'Yule, Sir Henry (1820-1889)', *Oxford Dictionary of National Biography* [ODNB], Oxford University Press [OUP], 2004, online edn, http://www.oxforddnb.com/view/10.1093/ref:odnb/9780198614128.001.0001/odnb-9780198614128-e-30291?rskey=4UGGU8&result=2 [accessed 19 February 2018].
[5] 'Yule, Sir Henry', ODNB.
[6] Yule, A., 'Memoir of Sir Henry Yule', xxxiv.

Lloyd for ‘7 or 8 days’ on magnetic instrumentation. Lloyd described Yule as an ‘intelligent young man’ but one who ‘has had no experience of instruments’. This does not seem to have perturbed Lloyd for Yule was ‘only the bearer of instruction to Aden after all’.[1] Herschel did not appear to know this, and listed Yule as the director of the Aden Observatory in his *Quarterly* article.[2] However, it was understood, by Lloyd and Yule himself, that Yule would simply be conveying the instruments and ‘communicating the knowledge of the new instruments’ to a Lieutenant Western, already at Aden, who was to become the observer.[3] However, on arrival in Aden, Yule discovered that Western had died some months previously.

As a result of Western’s death, it fell to Yule to construct and establish the magnetic observatory at Aden, a duty for which he did not feel at all qualified as ‘it is not merely by learning the manner of adjustment of the new magnetical instruments that an observer will be qualified’ to operate an observatory correctly ‘but that in order to do so he must be well accustomed to astronomical observations, and to the use of delicate instruments generally’.[4] As Lloyd had made clear to Sabine some months previously, Yule was not experienced in the use of scientific instruments or observing techniques. To add to this, Yule was also absolutely unprepared and unable to deal with the change which had occurred in the instruments during transit from England.

On unpacking the instruments, Yule found that of the 55 magnetic and meteorological instruments and their accompanying articles sent from England, only 33 arrived in the same state in which they had departed. Of this, only *one* of the several magnetic instruments and large magnetic bars arrived unscathed. Yule applied to the Commanding Officer of the Bengal Engineers in Aden for a committee to be formed to examine the instruments and report upon their state. This report was then sent back to East India House and communicated to Sabine through Colonel William Henry Sykes.

The report Yule sent, compiled from examinations by Cpt. J. Kelly of the Ordnance, Lieut. F. Ayrton of the Artillery and Lieut. J. A. Curtis of the Bengal Engineers, detailed the state of the instruments. All three of the barometers were ‘broken at the upper end’. The vertical force magnetometer ‘was found to have a play of two or three inches in its case from the insufficiency of the glued binders’, the glass cover was ‘cracked in the corner’, and one of the cross wires was broken’. The inner case was, according to the report, ‘quite insufficient for the weight of the instrument’. The axle of the dip circle had been deranged and a needle

[1] Lloyd to Sabine, 27 May 1840, TNA BJ 3/10/154.
[2] Herschel, ‘Terrestrial magnetism’, *Quarterly Review* 66, 131 (1840), 271-312, 300.
[3] Yule to James E. Melvill, 22 September 1840, RS MM/11/148.
[4] Yule to Melvill, 22 September 1840, RS MM/11/148.

had also rusted. Perhaps the greatest injury, however, occurred to the horizontal and declination magnetometers:

> The Circular wooden frame works intended to inclose these two Instruments on being lifted out of the packing case fell to pieces. They appear to have been constructed in a manner unadapted to this climate being composed of small pieces of wood ingeniously built together but retained in their places only by a band of thin mahogany veneer. This having warped and split no other bond remained … in the boxes containing the smaller parts of the apparatus all the little bindings of wood intended to keep the Articles in place were found loose, having been attached by glue only, and the Articles in consequence adrift in the boxes.

The four large magnets which these instruments used had also been found to be 'covered entirely with rust on one side and for two or three inches at each end on the other side'. Yule et al believed that this was a consequence of heat during transit and damp being admitted 'previous to leaving England' because 'the magnets were found in the innermost of 4 wooden cases besides being inclosed in soldered tin'.[1] The damage suffered to the barometers was 'a matter of deep regret' to Yule, as these were the cases to which his care was 'most particularly directed on route'. But, he said, it was almost impossible to keep these instruments from harm given that their route contained '<u>five</u> transhipments and a passage of four days across the desert';[2] and, at this time, 'the overland journey really meant so; tramping across the desert to Suez with camels and Arabs', an experience 'not conducive to the preservation of delicate instruments'.[3] Yule suggested that if another barometer was sent out with another observer, that it should be carried in a single case and 'carried constantly by hand … in a vertical or oblique position'.[4]

Yule could not, and would not, attempt to establish an observatory at Aden because of the state of the instruments and especially because the 'four large magnets belonging to the two great magnetometers which form the *essence of the Observatory* are in such a state of rust' as to be unusable.[5] Yule stated that

> this would appear to be fatal to the immediate establishment of the Observatory and even to its erection in a satisfactory manner as Professor Lloyd's instructions forwarded with the sketch of the building, state distinctly that it is necessary in order to procure an equilibrium among the instruments that the building should be

[1] 'Proceedings of the committee assembled to inspect and report upon such magnetical instruments as may be laid before them by Lieut. Yule, Bengal Engineers, 21 September 1840, RS MM/11/149.
[2] Yule to Melvill, 22 September 1840, RS MM/11/148.
[3] Yule, 'Memoir of Sir Henry Yule', xxxiv.
[4] Yule to Melvill, 22 September 1840, RS MM/11/148. Emphasis added.
[5] Yule to Melvill, 22 September 1840, RS MM/11/148. Emphasis added.

> erected with its axis forming a certain angle with the meridian, which angle is to be determined from a formula of the Professor's depending on experiments on the various strength of the individual magnets to be used. It appears then that to commence the erection of an Observatory on a proper principle we must await the arrival of fresh magnets from England.[1]

From this, we see how the magnetic observatory was not constructed or "established" in order for the magnetic instruments to inhabit it; such an observatory was built from, and around, the instruments. Further supporting evidence for this comes from Riddell in Canada. Riddell arrived in Canada before his instruments did. On communicating to the Commanding Engineer at Montreal – prior to the move to Toronto – on the dimensions and materials needed to build a magnetic observatory, Riddell explained that the position of the instrument pillars, which needed to be built and placed before the floor was laid, could 'not be exactly determined until the arrival of the instruments'.[2] In other words, construction could not begin until the instruments said so. It was the instruments and the complex interplay between magnets, angles and meridians which decided how and where an observatory was situated. One answer therefore to the question, posed at the beginning, on what a magnetic observatory was in the nineteenth century, can be found in its instruments. These were the "essence" of the observatory and they defined its assemblage.

In the end, Aden was abandoned as a potential magnetic observatory and Yule obtained his leave of the place, although not without some chagrin on the part of Sabine. Sabine felt that had there been 'a competent person at Aden to have directed the operations' the instruments could have been salvaged and the observatory along with them. All it would have taken, Sabine complained in a letter to Herschel, was for the magnetised bars to have been cleansed of rust and the wooden box of the magnetometers to have been replaced 'by one of the same dimensions made on the spot'. The injuries, barring that of the barometer, were of 'no great consequence' and Sabine further castigated Yule for not being aware that he was 'furnished with a book of instructions for the use of the instruments published expressly by the Royal Society [in which] all the instruments named in Lt. Yule's report, without exception, are described therein, and directions given for their use'. Sabine's belief in the power of the manual as a complete pedagogical tool is clearly demonstrated here and runs contrary to the Gaussian, German model of scientific pedagogy at this time, which stipulated that, according to Josefowicz, 'no manual could sufficiently account for

[1] Yule to Melvill, 22 September 1840, RS MM/11/148.
[2] Riddell to Oldfield, 30 September 1839, in Thiessen, 'The founding of the Toronto Magnetic Observatory, 312.

everything' and it was only through 'experience' and 'direct interaction' of a more experienced teacher that knowledge of observation or experimentation could be learned.[1]

Sabine could not understand how the barometers for the Toronto Observatory, 'similar in all respects and similarly packed to those sent to Aden' could have arrived safely in Toronto 'after a sea voyage and an inland transport of more than 400 miles' whereas the Aden barometers suffered irreparable damage. Sabine dallied with the idea of sending out a new EIC officer to Aden furnished with new magnetic bars and a portable barometer (which, Sabine added, should be carried 'in his hand or 'on his back'[2]) but in the end the instruments were simply transferred to the Bombay observatory, the EIC having informed the Royal Society in December 1840 that 'the Court are not enabled at the present time to render the services of a qualified officer available for the separate charge of the proposed observatory at Aden'[3]. Prior to Yule's departure for Aden, Lloyd had confessed to Sabine that he felt Aden to be a 'villainous station' and a 'most unpropitious spot for magnetism.'[4] Though Lloyd had been talking specifically about the magnetic nature of the volcanic rock on which Aden stood, his comment still turned out to be remarkably prescient.

Sabine's reaction to news of the Aden debacle is illuminating. For one, it arguably demonstrates Sabine's lack of experience of working in hotter climes. The majority of Sabine's magnetic expeditionary work had been centred on the Arctic or Britain.[5] Sabine was defined by the Arctic and by the extreme north. In a geobiography of Sabine, the Arctic would be prominent. He knew first-hand of the fallibility and delicacy of a magnetic instrument in certain geographies but not the problems that excessive heat and humidity might incur; problems that affected both the potential Aden Observatory and the actual Cape of Good Hope Observatory. This lack of understanding is visible in Sabine's comparison of the successful transmission of instruments to Canada with that of the unsuccessful transmission of instruments to Aden. Sabine seems to have been unaware – or chose to remain unaware – of the problems encountered by magnetic (and, often, meteorological) instruments exposed to extremes of heat, and instead expressed the opinion that if instruments could survive the extremes of the Arctic, they could survive anywhere, as this was a part of his lived scientific experience. The Arctic defined Sabine's

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[1] Josefowicz, D-C., 'Experience, pedagogy, and the study of terrestrial magnetism', *Perspectives on Science* 13, 4 (2005), 452-494, 480.
[2] All quotations from a memo sent by Sabine to Herschel, 21 November 1840, RS HS/15/110.
[3] James C. Melvill to Lord Northampton, 16 December 1840, RS MM/11/151.
[4] Lloyd to Sabine, 9 June 1840, TNA BJ 3/10/157.
[5] Good, G. A., 'Sabine, Sir Edward (1788-1883)', *ODNB*, OUP, 2011, online edn, http://www.oxforddnb.com/view/10.1093/ref:odnb/9780198614128.001.0001/odnb-9780198614128-e-24436?rskey=Kgot60&result=2 [accessed 19 February 2018].

life and career and influenced the way in which he in turn defined the limits to which instruments could be exposed.

It was not only Sabine who underestimated the potential of warmer climates to unduly affect an instrument; instrument makers too did not include such a concern in their craft. Yule wanted to bring Sabine's attention to 'the utter insufficiency of glued fastenings and veneering for the cases and frames of instruments sent to this climate'.[1] Wilmot at the Cape also questioned how Robinson could have sent out dip circles 'with no other fastening but glue to the backs'[2] and complained that all glued parts of cases had melted and opened and that his portable barometer had been 'stewed' and as a result partly 'burst open'.[3] Robinson, Grubb, Jones et al did not make their instruments, did not glue and fasten them, depending on where in the world they were going to be sent. They did not make magnetic or meteorological instruments with the geography of the magnetic crusade in mind. The user was instead supposed to be capable enough to subvert different climatological conditions to make the instrument work, a theme also present in Schaffer's telling of the 'Bombay case' and scientific instruments in states of disrepair more generally. Both the account above and Schaffer's articles show how managing scientific instruments in different states and in different circumstances was a salient part of nineteenth-century observatory practice. It was Yule's 'competency' and lack of ability to adhere to the Royal Society's printed instructions which Sabine questioned, not the integrity of the instruments nor really the method of their conveyance, except in commenting on the proper mode of carrying a portable barometer. Observatory managers were not to 'demand perfection' but rather be prepared and 'be able to improvise repairs on site'.[4]

According to Lloyd, Captain John T. Boileau, director of the East India Company's magnetic and meteorological observatory at Simla, had found on arrival in Simla that 'his boxes [had] met with the same mishap as those of Aden', as the 'whole apparatus' – Lloyd is not specific on the identity of the apparatus – despite being packed 'outer case & all, in an airtight tin case' had allowed in some moisture which was then 'boiled up into steam of high tension'. However, unlike the situation that unfolded in Aden, Boileau 'was not long in putting [the instruments] to rights'.[5] Boileau, who had spent more time with Lloyd at Dublin than Yule's weeklong visit and who had been an engineer in the Bengal Engineers for two decades by the time of his posting to Simla, was evidently more adept at handling,

[1] Yule to Melvill, 22 September 1840, RS MM/11/148.
[2] Wilmot to Lloyd, 31 March 1840, RS MS/119/II/73.
[3] Wilmot to Lloyd, 20 April 1840, RS MS/119/II/74.
[4] Schaffer, S., 'The Bombay case: astronomers, instrument makers and the East India Company', *J. Hist. Astron.* xliii (2012), 151-180, 151, 157; Schaffer, S., 'Easily cracked: scientific instruments in states of disrepair', *Isis* 102, 4 (2011), 706-717, 709, 710, 712.
[5] Lloyd to Sabine, 19 December 1840, TNA BJ 3/10/170.

and making work, instruments in disrepair. The problem in Yule's case was that at no point did he – or Lloyd – believe that he was to become the observatory manager: he was only the carrier of instruments and of instruction, both to be deposited with Lieutenant Western on arrival in Aden. But Western had died and Yule did not possess the technical or scientific acumen necessary to prevent the debacle that followed.

In terms of the way in which instruments, or at least needles, were packaged prior to transportation, Sabine may have taken something from the experience of Wilmot, Yule and Lefroy having each found some or all of the needles they travelled with blighted by rust. In late 1842 Sabine was in contact with Michael Faraday on just this subject. With the needles in a zinc box, Faraday advised Sabine to

> take a piece of well & recently twined line – powder it – wrap the needles with plenty of this powder about them in a piece of [?] paper…using not more than enough to make two or three folds about the line & needles then put the packet into the zinc box filling the box up with the powdered lime & soldering the box up <u>air tight</u>. The lime should be good neither slaked nor carbonated. The…paper is to make a packet that [will] keep in the middle of the lime during the journey… No more lime is wanted than will fully surround the needles ½ of an inch in thickness. More however will not do any harm.[1]

The plight of the observatory directors with the problem of rust had perhaps not gone unheeded by Sabine despite, as is especially visible in the Aden case, the overriding belief that the work of a magnetic observatory could still be completed even if the needles or bars exhibited rust.

The magnetic observatory instruments of the British scheme were as much pieces of knowledge in and of themselves as they were instruments for the accumulation of knowledge of the earth's magnetic force. Knowledge of observing techniques was bound up in the physical structure of the instrument and it was through bodily interaction with these instruments that the observatory officers had first learned their craft. Indeed, this was probably the most important aspect of their training according to their instructor, Lloyd, and the reason that both Lloyd and Sabine were so eager to make sure that Grubb finished the sets of magnetometers before the officers arrived in Dublin for tuition. Lloyd even suggested that they defer the visit of the officers until Grubb was certain he could produce the instruments in time.[2] Just as was in evidence during the British Magnetic Survey prior to the crusade, understanding of terrestrial magnetic study was often arrived at in the

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[1] Michael Faraday to Sabine, 17 November 1842, TNA BJ 3/31/33.
[2] Lloyd to Sabine, 19 May 1839, TNA BJ 3/9/96.

making and unmaking of the instruments of its study. It is important to consider the exigencies of travel on the instruments which made the colonial magnetic observatories because of these wider points. In moments of transit, these instruments were knowledge on the move. When the instruments suffered due to travel, so too did the observatory director's ability to competently and quickly establish his observatory and make credible observations.

Instruments did not have to be explicitly damaged for this geography of terrestrial magnetic science to become visible. Each of the original colonial observatory directors and, often, their successors, initially struggled to produce usable results. As previously mentioned, each (excepting Kay) had learned the techniques of magnetic observation with their instruments – the same ones that would travel to their observatories – in Dublin and in Woolwich. On exploring their instruments in the different localities to which they had been sent, each observatory director found their instruments in some manner different from how they had experienced them before. As these instruments changed, as they warped and fell to bits in their boxes, so the knowledge which the observatory directors had bound up in them was distorted. The printed instructions, that commodity which Sabine initially seems to have heralded as the supreme guide to working with the instruments, could not change, could not adapt to the change which the geography of the magnetic crusade had engendered in the instruments. It is small wonder Yule did not seek to try and use the printed instructions that were sent with him to revivify the instruments he transported. When Yule told Sabine that 'among the instruments transmitted' he had found several which he had no acquaintance whatever, and which were never alluded to by Professor Lloyd' he was, in more ways than one, correct.[1]

Conscientious and apparent adjustment: the vertical force magnetometer, touch and change in the magnetic observatory

It was not only during transportation that alteration to an instrument's state could occur. Wilmot felt that some of his magnetic instruments – possibly the magnetometers or the dip circle, though Wilmot does not make this explicitly clear – had been changed by what he calls, 'the ill effects of fingering'. Wilmot's had been the 'show instr[uments]' when the officers had all been at Woolwich to meet Sabine and others. The instruments had

[1] Yule to Melvill, 22 September 1840, RS MM/11/148.

obviously been handled extensively by the inexperienced future observatory directors and, though Wilmot 'followed about wiping', it was, he lamented, 'impossible to prevent the ill effects of fingering'.[1] What exactly this 'fingering' did to Wilmot's instruments, i.e. how precisely it manifested itself in the results, is unclear, but Wilmot reported puzzling and discordant results with all his instruments – his dip circle and vertical force magnetometer were the cause of his most frequently erroneous results – at one time or another and presumably he saw some of these as attributable to their being practised on at Woolwich.

What Wilmot seems to have been suggesting in the above passage is that physical interaction with a magnetic instrument left an indelible mark on the instrument, a mark that might have led to changes in the nature of the instrument. The observer, treating the instrument as a model, left literal and metaphorical prints upon the object of their inquiry. This marked a kind of reversal: rather than the instrument being the object used to make inscriptions, the instrument was itself inscribed upon. While Wilmot's frantic wiping may have lifted the literal, visible, prints left by the officers at Woolwich, it could not resolve the intangible change that these instances of touch occasioned.

The potential of human touch to negatively affect his instruments created a curious practice of Wilmot's at the Cape Magnetic Observatory. Wilmot decided to cover his instruments and reading telescopes 'with dust' so that he could 'see if anybody touches them' when he was himself not present in the observatory.[2] The potentially deleterious effect of dust on the smooth functioning of a magnetic instrument or reading telescope was secondary, in Wilmot's estimation, to the problems that could occur when magnetic instruments and humans came into contact. Wilmot's affected state of mind regarding the interplay between observer and instrument even extended to a belief that 'the magnets move when I go near them' though his men approached them without issue.[3]

The complex relationship between observer and instrument, object and touch, is observable in other aspects of the magnetic observatory and was in many ways embodied in the experience of using the vertical force magnetometer at the Cape. The vertical force magnetometer (commonly referred to at the time, and hereafter in this chapter, as the VF) was 'the instrument used in determining the changes of the *vertical component* of the magnetic force' by means of 'a magnetic needle resting on agate planes, by knife-edges, and brought to the horizontal position by weights'. In principle, changes in the vertical force could be inferred from the VF by noting changes in the position of the needle relative to the mean inclination of the place of observation and the times of vibration of the needle

[1] Wilmot to Lloyd, 31 March 1840, RS MS/119/II/73.
[2] Wilmot to Lloyd, 12 May 1841, RS MS/119/II/80.
[3] Wilmot to Lloyd, 26 March 1841, RS MS/119/II/78.

in the vertical and the horizontal planes.[1] In other words, it only worked when combined with the observations of another instrument (the dip circle) and when the relative strength of the needle was known. To do the latter meant removing the needle from its supports, and vibrating it, which, as I will now demonstrate, was problematic.

The defining feature of the magnetic crusade, that which gave it 'unity and greatness' was, according to Lloyd, 'that the same plan of observation is followed out in all … distant stations, by observations strictly simultaneous, made according to the same instrumental methods, and with the same instrumental means'.[2] In reality, that which truly united the observatories was not quite 'the same instrumental means' but rather the same instrumental problems. No instrument represented this more than the VF, which displayed a range of the geographies of the magnetic crusade: of distance, communication, travel, and the situated nature of magnetic observation.

The VF was developed by Lloyd between late 1838 and early 1839 and is first mentioned in a letter to Sabine in January 1839.[3] Savours and McConnell have provided a neat summary of it:

> the magnetic needle, 12 inches long, carried a wire cross at each end attached by a copper ring, distance between the crosses being 13 inches. It rested on agate planes which, through a solid copper support, were fixed to a massive marble base. The needle, which could be lifted off the planes, was balanced by screwing small brass counterweights along an attached cross-piece. The position of the needle was observed through micrometer microscopes on the base of the instrument.[4]

In May 1839 Lloyd called the VF 'a complete success' and 'the <u>best kit</u>' which he had made 'in the magnetic way', although he also noted that the '<u>first adjustment</u>' of it was 'troublesome & difficult'.[5] The VF, in contrast to the other magnetometers, was to be made by Robinson rather than Grubb, and cost £26 in January 1840; by way of comparison the declination and bifilar magnetometer by Grubb cost £35 each.[6]

By December 1839, after the observatory directors had been trained and sent off for their respective stations, Lloyd's opinion of the VF had changed, stemming from a conversation he had had with Wilhelm Weber in Göttingen.[7] On returning to Dublin following this exchange, Lloyd resolved to examine the VF, which he did at Dublin and, later, through a

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[1] Anon, *Report of the President and Council of the Royal Society*, 31.
[2] Lloyd, *Account of the magnetical observatory of Dublin*, 6.
[3] Lloyd to Sabine, 24 January 1839, RS MS/119/I/60.
[4] Savours and McConnell, 'The history of the Rossbank Observatory', 549.
[5] Lloyd to Sabine, 9 May 1839, TNA BJ 3/9/92.
[6] Sabine to Lloyd, 13 January 1840, RS MS/119/I/84.
[7] Lloyd to Sabine, 31 March 1840, TNA BJ 3/10/149.

comparison with results obtained by Captain Boileau in London. The initial trials Lloyd made at the Dublin Observatory with two VFs in different rooms showed 'unfavourable' results though Lloyd maintained that 'the thing [the VF] is all sound; the difficulties are of a practical kind'.[1] Later comparisons with Boileau's experiments in London produced 'good, but not quite satisfactory' results.[2] Lloyd began to understand that the instrument worked well theoretically, but that the practicalities of working with it on a regular basis – including its adjustments – made the instrument difficult and unpredictable. Wilmot echoed Lloyd's concerns in March 1841, a month after moving into the observatory and trying to get all his instruments into the correct arrangement, when he confessed to Lloyd that 'while the theory of the instrument is as simple as possible, the practice of it is not without its difficulties'.[3]

The problem was that each time the needle of the VF was removed and replaced, it became difficult to get the instrument back into the same adjustment, i.e. each time the user and the object interacted beyond observation, the user left a mark that could occasion similar consequences to those which Wilmot and his 'show instruments' experienced. 'The mere drawing out of the bar and replacing it', according to Riddell at the Toronto observatory, always produced a change'.[4] Wilmot came to describe it as a 'most puzzling instrument' whose movements 'appear so inconsistent'. Wilmot could 'not understand how the needle can be moved at each observation for it is almost impossible to put it down quite true upon the supports, and if not it vibrates for a long time and renders it difficult for one observer to get its true place'.[5] The problems inherent in working with the VF were by no means exclusive to Wilmot. Lefroy experienced similar issues in his St Helena Observatory. Lefroy's VF needle was found to be 'encrusted with rust' on unpacking it at St Helena, and he frequently described his 'great difficulty in adjusting it [the VF]'.[6] The needle seemed to lose magnetism at random, was subject to inexplicable movements that prevented it from coming to rest properly, and contracted more rust about its knife edges, all of which led Lloyd to inform Lefroy in July 1841 that his VF needles were 'good for nothing'.[7] Consequently, Lefroy proposed to 'omit all that was given for Vertical Force up to our quitting the House to the Observ[ator]y' (i.e. from February to August 1840) and even felt disposed to 'throw overboard' the rest of the observations to December 1840.[8]

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[1] Lloyd to Sabine, 21 December 1839, TNA BJ 3/9/123.
[2] Lloyd to Sabine, 31 March 1840, TNA BJ 3/10/149.
[3] Wilmot to Lloyd, 31 March 1841, RS MS/119/II/79.
[4] Riddell to Lloyd, 26 May 1840, RS MS/119/II/12.
[5] Wilmot to Lloyd, 5 June 1841, RS MS/119/II/81.
[6] Lefroy to Lloyd, 10 April 1840, RS MS/119/II/89; Lefroy to Lloyd, 29 May 1840, RS MS/119/II/90.
[7] Lloyd to Lefroy, 5 July 1841, TNA BJ 3/81/31.
[8] Lefroy to Sabine, 13 November 1841, TNA BJ 3/81/40.

Boileau, who had helped Lloyd in the trials of the instrument in London, found difficulty using the VF when stationed at the Simla Magnetic Observatory, for which he blamed both the manner in which Robinson packed the magnets – 'very bad' – and the extreme delicacy of the instrument that caused it to oscillate at the lightest of footfalls in the vicinity.[1] In Canada, Riddell likewise struggled to get his VF into a cooperative adjustment. In March 1840, he filed away at the VF needle's centre point for fully six hours to try and create a better equilibrium.[2] In April of the same year Riddell spent three days trying to resettle the planes 'so that the knife edges may have correct bearings' yet still he could not manage the intractable nature of the VF.[3] At the Rossbank Observatory, Kay found himself frequently 'alarmed at the extraordinary movements of the Vertical Force'. The instrument had initially been put into adjustment by James Clark Ross, something which had instilled confidence in Kay as he felt he lacked the 'mathematical acumen' to understand the instrument himself.[4] Ross then supplied Kay with a new VF in the spring of 1841 which possessed 'more sensibility than the former instrument'.[5] 'Sensibility' was expected in an observatory instrument for these were designedly delicate and sensitive objects created to perceive minute changes in the earth's magnetic field. The VF was a particularly acute example of this; in fact, for Kay, the new VF that Ross had supplied was too emblematic of this and Kay struggled in his observatory to diminish 'the extreme sensibility of the instrument.'[6]

Through his consistent struggles with the adjustment and observation of the VF, Wilmot, at the Cape of Good Hope Magnetic Observatory, concluded that the VF existed in two different states. On the one hand, it was 'easy enough', explained Wilmot, to get the VF into an 'apparent adjustment', but this state was 'not one which shall ensure proper results'.[7] What was required of Wilmot, and of his brother officers at the other observatories, was to get the VF into what Wilmot termed a 'conscientious state of adjustment', in other words a state of adjustment that would produce reliable, consistent and credible observations. In some respects, this idea of the 'apparent' vs the 'conscientious' speaks to the idea of 'experimenter's regress', as described by Collins and Pinch and usefully summarised by Dunn in a recent study on the testing of different iterations of the magnetic compass on the 1818 Arctic voyage of the *Isabella* and

[1] Boileau to Sabine, 16 April 1841, RS MS/257/194.
[2] Riddell to Lloyd, 4 March 1840, RS MS/119/II/7.
[3] Riddell to Lloyd, 2 April 1840, RS MS/119/II/10.
[4] Kay to Lloyd, 12 November 1840, RS MS/119/II/102.
[5] Kay to Lloyd 16 January 1842, RS MS/119/II/103.
[6] Kay to Lloyd, 2 March 1842, RS MS/119/II/104.
[7] Wilmot to Lloyd, 12 May 1841, RS MS/119/II/80.

Alexander under Captain John Ross.[1] Experimenter's regress stipulates that the correct result of an experiment is producible only when the apparatus used is functioning properly, but we can only check the proper functioning of said apparatus by whether or not the experiment returns the correct result. In other words, to know that the VF was in a 'conscientious' state of adjustment and producing credible results, Wilmot first had to know what those results ought to look like. In an 'apparent' state of adjustment, the VF might have appeared to be steady and balanced, but the results achieved with it would have been discordant and outside of what was expected. In this way, the VF was, following Dunn, both an instrument of observation and an instrument under observation. Through sending observations of the magnetic field made with the VF back to Lloyd in Dublin and Sabine's magnetic department in Woolwich, Wilmot and the other observers were assembling data as much about the instrument that created the data as they were about the vertical component of the magnetic field, data that helped to make visible the line between conscientious and apparent adjustment. Only gradually, through correspondence with Lloyd and Sabine and the exchange of experience, information and data, could the observatory directors come to understand their experience of the VF and learn the difference between its different adjustments.

To create a conscientious state of adjustment meant not only knowing what observations to expect but also the making and maintaining of a space that could limit the possibility of magnetic disturbance; the possibility of too much bodily interaction with the instrument; and the effects of variable temperatures and currents of air within the observatory. Getting a magnetic instrument into the proper adjustment did not mean simply adjusting that instrument, but managing and adjusting the spaces around it too. This was why Wilmot and the others were required to continually modify their observatories, why Wilmot covered his instruments in dust to make sure no one else interfered within them outside the allowable limits, and why the Cape assistants had such a 'wholesome dread of <u>iron</u>' that they left their pen knives in a fence 150 feet from the observatory.[2]

At the Rossbank Observatory, it was not until 1842 that Kay received more complete instruction on how to appropriately adjust the VF and how changes needed to be made in the arrangement of the instruments within the observatory to assist in this, and he only received this 'by the accidental circumstance of Sir J. Franklin having had one of the

[1] Dunn, R., 'North by Northwest? Experimental instruments and instruments of experiment' in MacDonald, F., and Withers, C. W. J., (eds), *Geography, technology and instruments of exploration* (Ashgate: Farnham, 2015), 57-75.

[2] Wilmot to Lloyd, 26 March 1841, RS MS/119/II/78.

amended reports sent to <u>him</u>'.[1] It is presumably the *Revised Instructions for the use of the Magnetic and Meteorological Observatories* (1842) prepared by the Committee of Physics of the Royal Society to which Kay was referring here. One of the alterations in the adjustment and use of the VF this report outlined was to arrange the VF so that it was at a right angle to the magnetic meridian rather than, as Ross had placed it, within the magnetic meridian. News of this change was unwelcome, not because it occasioned a great deal of upheaval in Kay's observatory but because had Kay received Lloyd's letters on the subject – dated 6 May and 6 July 1841 – and the revised instructions six months earlier than he eventually did on 28 March 1842, he could have communicated the change in arrangement to Ross. Kay knew Ross was in error about the VF placement – and thus also the arrangement of the instruments in total – and worried that all past and future observations of the portable observatories would be in error, especially so as the mutual action of the instruments was greater on the ships than it was at Rossbank.[2]

Ross had also advised Kay to vibrate the VF hourly by carefully lifting it off the planes, a misplaced piece of advice that probably caused further problems. Kay wrote that both he and Ross were '<u>misled</u>' in this respect 'by page 34 of the Instructions'.[3] The passage in the original instructions Kay referred to concerned the method of absolute determinations by the VF. In the case of observations of absolute intensity, the instructions indicate that, in order to guard against alteration of the 'magnetic moment of the magnet' of the VF which would then 'afford no means of separating this portion of the effect from that due to a change in the earth's magnetism', the observer must employ 'means analogous to those employed in the determination of the absolute value of the horizontal intensity; and accordingly one or other (or both) of the methods proposed for this determination should be occasionally resorted to'.[4] The 'means analogous' referred to here can be found on page 28 of the *Instructions* and concern the horizontal force magnetometer, or bifilar magnetometer, and these again focused on how to separate the magnetic moment of the bar from observations of the earth's magnetic force. The ratio of the quantities of magnetic moment and earth's magnetic force, the instructions say, 'is to be determined by *removing the bar from its stirrup*, and using it to deflect the suspended bar of the declination instrument' by a method of Gauss's formation.[5]

Kay had indeed been misled. The revised instructions of 1842 admitted that even under the most favourable circumstances, 'and with the best management, the results obtained with

[1] Kay to Lloyd, 2 March 1842, RS MS/119/II/104.
[2] Kay to Lloyd, 2 March 1842, RS MS/119/II/104.
[3] Kay to Lloyd, 2 March 1840, RS MS/119/II/104.
[4] Anon, *Report of the President and Council of the Royal Society*, 34.
[5] Anon, *Report of the President and Council of the Royal Society*, 28. Emphasis added

the balance-magnetometer [i.e. the VF]' were 'undoubtedly inferior in accuracy to those of the bifilar magnetometer'. This inferiority was caused, the report alleges, by 'the large influence which the unavoidable errors of workmanship have on the position of equilibrium of a magnet supported on a fixed axle.' The report essentially goes on to explain how the VF was so sensitive to minute disturbances of different parts of the instrument that it was unsuited to determinations of long-term change but could still be used, 'with proper management' to measure diurnal changes and 'momentary fluctuations' with 'tolerable fidelity'.[1] Essentially, the instrument had been rebranded. It was referred to in the new instructions as a 'balance magnetometer' not a vertical force magnetometer, and it was no longer to be used for its original purpose of charting periodical or secular change. The instrument itself remained the same; it had not been added to or retooled in any capacity. But the method of its operation, the way in which it was (not) handled, the manner of its arrangement in relation to the other magnetometers, all of this had changed and in so doing had changed the purpose of the instrument. It was not only the VF that changed as a result of the continually altering instructions for its use. Lefroy later wrote to Riddell and commented on a letter he had received from Kay, dated June 1843. It was, Lefroy remarked, 'an amusing epistle', in which Kay declared 'that the many inconsistencies of the various orders and instructions he received drove him mad, and he laboured for a long time under a species of magnetical insanity'![2]

The relationship between instrument, printed instruction, correspondence and observatory management at this stage of the magnetic crusade is instructive and perhaps also emblematic of wider issues involved in the organisation of a global scientific project at this time. On this point, Lloyd's response to news of the problems Riddell and Lefroy were having with the VF is particularly informative. He wrote to Sabine in June 1840 that:

> Riddell's mischief did <u>not</u> arise from inefficient means of adjustment; for he <u>did</u> get the instrument into adjustment & it afterwards <u>went out</u> again, for some cause or other which it is not very easy to gather from his description. But <u>supposing</u> there was <u>not</u> weight enough in the screw, <u>what on earth</u> is to prevent them adding to it? If they cannot do so simple an affair as this, they can do nothing. Dip the head of the screw in molten lead, for example…you seem to treat the Obsy directors as the merest children.

[1] Anon, *Report of the President and Council of the Royal Society*, 37.
[2] Lefroy to Riddell, 10 December 1844, in Stanley, G. F. G. (ed.), *John Henry Lefroy: in search of the Magnetic North. A soldier-surveyor's letters from the North-West, 1843-1844* (Toronto: Pioneer Books, the MacMillan Company of Canada Limited, 1955), 145.

> 'I must say, however, that neither Riddell nor Lefroy seem to have any notion of mechanical contrivance…Undeviating adherence to rule is an excellent quality in a subaltern; but something more is wanting in the commander of a detachment'.[1]

Lloyd's comments here speak to several different points. There was a level of expectation, as exhibited by Sabine's remarks on the Aden debacle, that an observer would possess the capacity to follow the word of the printed instructions but at the same time also have the "competency" to manage when plans, or rather instruments, went awry. This competency extended to, from Lloyd's understanding, the ability to make technical and mechanical alterations to instruments and to have the wherewithal to make such decisions autonomously, i.e. outwith direct assistance or authorisation from the centre of operations.

Lloyd saw officers like Riddell and Lefroy as the 'commanders' of their observatories, not as the 'merest children', as Sabine seemed to have at this point, or as 'subalterns' blindly following a higher authority. They were supposed to be able, in Lloyd's understanding, to negotiate the spaces between authority, instruction, experience and action. These officers were not expected to be the "obedient drudges" or "capable idiots" of Airy's configuration but competent individuals capable of bridging the gap between centre and periphery through their own expertise and experience. Lloyd's comments above show that the expectation and reality of the ability of the officers was not as close as Lloyd wanted.

Lloyd's remarks above also speak to ideas about pedagogy and the terrestrial magnetic observer. The original observatory directors, excepting Kay, had all travelled to Dublin for instruction, demonstration and practice in the art of geomagnetic observation. They had then, as Wilmot's comments about his instruments as 'show instruments' attests to, all convened at Woolwich for further practice with magnetic instruments. Lloyd also travelled to Woolwich and, for Kay, to Chatham to continue the officers' education.

The form of tuition offered in Dublin was a series of classes or lectures on the subject of terrestrial magnetism, followed by, once Grubb had completed them, instruction on, and practice with, the officers' own magnetic observatory instruments.[2] Practice with the instruments seems to have continued later in Woolwich too. However, in both Dublin and in Woolwich too it appears that this training was not sufficient or, at least, the involvement of Lloyd and/or Sabine in proceedings was not sufficient enough. On a personal level Lefroy found Lloyd to be 'the most delightful of men': genial, good natured and patient. However, Lefroy described the officers' stay in Dublin as 'not long enough for thorough

[1] Lloyd to Sabine, 9 June 1840, TNA BJ 3/10/157.
[2] The main source for this is the collection of correspondence between Lloyd and Sabine, found at TNA BJ 3/9.

mastery of our work' and, indeed, 'premature' because Lloyd 'had not himself matured his plans.'[1] Lefroy seems also to have had misgivings about their training – or lack thereof – at Woolwich, post-Dublin. Lefroy, replying to what must have been a firm letter from Sabine, expressed how 'we [Lefroy, Wilmot and Riddell] thought we might have received more specific directions from the first – "you will have to do so and so, you are expected to know so and so"' about 'ordinary problems in practical astron[omy]' such as 'finding the latitude, longitude, height above the sea, mode of getting the true meridian mark…keeping the rates and finding the errors of our timekeepers and so on'. Lefroy continued that he had 'been at all times aware that to you [Sabine] I might apply for all the information I needed' but he had not in all instances because of an 'inadequate estimate of the need of this preliminary preparation, and ignorance of the points on which to seek information'.[2] What emerges from Lefroy's letter is the same image which appears later in the crusade too: that of an expectation of the officers that they, as 'commanders' and not 'subalterns' would have the wherewithal to teach themselves, or to determine for themselves at least, the right course of action in terms of observatory and instrument management.

That the artillery officers' stay in Dublin was too short and less instructive than that required, is borne out in Riddell's request, sent as he was trying to erect his permanent Toronto Observatory, for the equations necessary to achieve equilibrium in the observatory between all the instruments on their pedestals.[3] Riddell had evidently not been taught this process and neither it seems was he furnished with a copy of Lloyd's paper on the subject, which was first read to the Royal Irish Academy in February 1839 (fig. 6.7).[4] Lloyd wrote to Riddell in April 1840, 'quite in despair', because none of his previous letters seemed to have reached Riddell. Lloyd had written 'in November [1839] … and four or five times since'.[5] These letters included information regarding the correct establishment of a magnetic observatory and of the instruments within it and were clearly meant as a form of pedagogy as much as the printed instructions were or the time in Dublin had been. Similarly, Kay at Van Diemen's Land both bemoaned and cherished the correspondence he received from Lloyd. 'The fact is', Kay wrote to Lloyd in 1842, 'your letters to me written a year ago, and only just received a short time back, have done more in a few plain words towards "oiling the hinges of my understanding" than the whole of the printed report'.[6] Kay had not travelled to Dublin for instruction and had only a 'short acquaintance' with

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[1] Lefroy, *Autobiography*, 35.
[2] All quotations from Lefroy to Sabine, 17 June 1839, TNA BJ 3/81/3.
[3] Riddell to Lloyd, 4 March 1840, RS MS/119/II/7.
[4] Lloyd, H., 'On the mutual action of permanent magnets, considered chiefly in reference to their best relative position in an observatory', *The Transactions of the Royal Irish Academy* 19 (1843), 159-176.
[5] Lloyd to Riddell, 14 April 1840, RS MS/119/II/8.
[6] Kay to Lloyd, May 1842, RS MS/119/II/107.

Lloyd in Chatham prior to his deployment on magnetic service.[1] Lloyd did all that was in his power, he told Sabine in the spring of 1842, by 'the way of letter writing' to Kay, in order to 'remedy the first mistake' of Kay having had no previous training or, 'at all events, none from me [Lloyd]'.[2] The correspondence of Lloyd with observatory directors represents one of the spaces in which the colonial magnetic observatories were constructed. It was through these letters that knowledge of techniques of observation, of arrangement of instruments, of mechanical changes to instruments and so on, were passed during the crusade but these things, these letters, were subject to problems engendered by the massive geography of the crusade, which meant they often arrived too late to prevent misadjustments or to correct observational practice.

Conclusion

John Herschel, writing in 1840 shortly after the British scheme had been launched, forwarded the idea that a 'philosophical theory does not shoot up like the tall and spiry pine in graceful and unencumbered natural growth, but, like a column built by men, ascends amid extraneous apparatus and shapeless masses of materials'.[3] In my interpretation, what Herschel meant here was the entire magnetic project, the quest for a mathematical theory to explain the earth's magnetic force, was contingent upon the

[1] Kay to Lloyd, 12 November 1840, RS MS/119/II/102.
[2] Lloyd to Sabine, 10 April 1842, TNA BJ 3/12/238.
[3] Herschel, 'Terrestrial magnetism', 272.

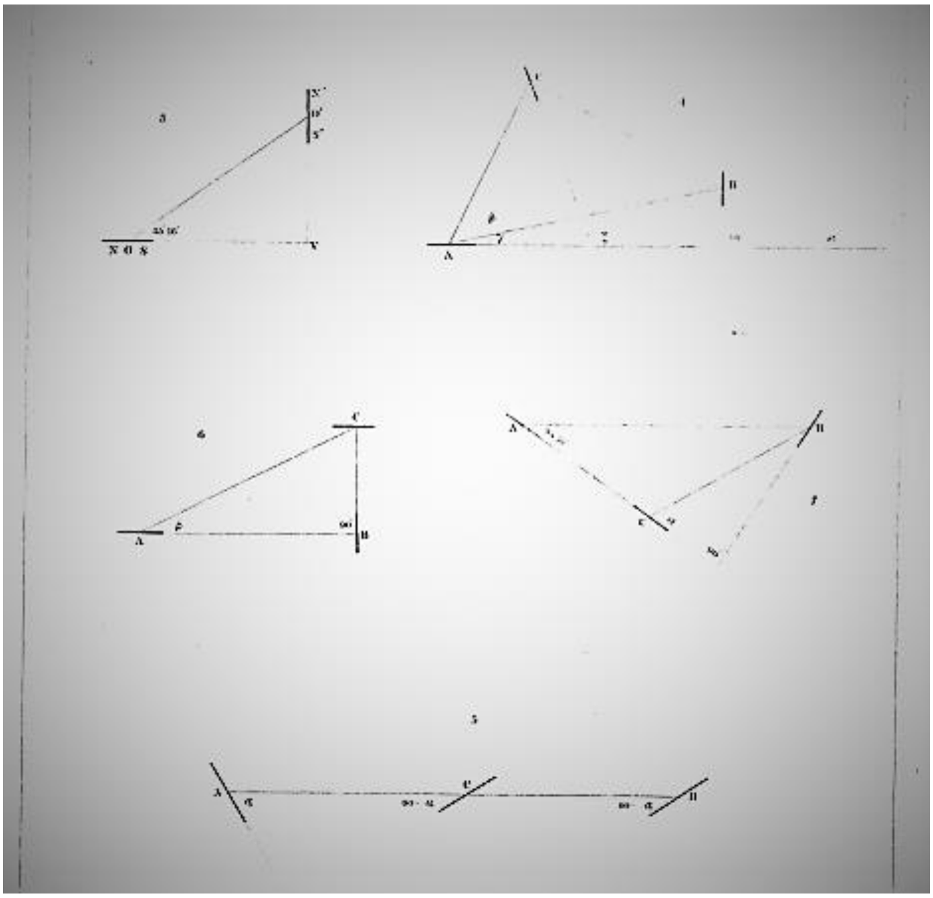

Fig. 6.7: The placement of magnetic instruments in the observatory. From Lloyd, 'On the mutual action of permanent magnets', plate I.

administration, governance and the seemingly mundane, 'shapeless' materials of its making. These materials were the instruments and the magnetic needles and bars but they were also the literal building materials of the fixed observatories: the wood, the stone, the copper nails and hinges, the green baize doors and the thatching, the cedar barrels and the clay and the earth of the different localities in which the observatories were situated. Understanding the observatories as fallible assemblages of diverse materials draws attention to the situated nature of the colonial magnetic observatories, the geographies of their making and the geographies inherent in the production of terrestrial magnetic observation at these sites. The magnetic observatory can be understood as an assemblage because it can be defined as a composition of 'heterogeneous elements…human and non-human, organic and inorganic, technical and natural'.[1] The observatory was a constantly emerging, multiple and indeterminate space built of and by artillery and naval officers, magnetic and meteorological instruments, clay, wood and nails, scientific instruction and circumscribed organisation and the autonomous, in situ, decisions of isolated observers. As previously argued, the colonial observatories were perpetually *being* established and did

[1] Anderson, B., and McFarlane, C., 'Assemblage and Geography', *Area* 43, 2 (2011), 124-127, 124.

not have conclusive, fixed, shapes but were instead indicative of a space of 'multiple co-existences', which were often fragile and provisional and susceptible to the 'gaps, fissures and fractures' that accompany 'processes of gathering and dispersing'; all of which have been emphasised in critical geographical studies of assemblages.[1]

A process of perpetual readjustment and realignment was not only in evidence in the physical structure of the magnetic observatory. Such processes can also be seen within the biographies of the instruments sent to the observatories, and especially in those magnetic instruments that formed the 'essence' of the observatory, to use Yule's terminology. This was most strongly in evidence with regards to the vertical force magnetometer's existence at the observatory but can also be seen in instruments such as the dip circle, which Smythe at St Helena was regularly having to put back into adjustment as late as 1846.[2] The account books of St Helena provide additional proof of this. As this source shows, costs were incurred on a regular basis throughout the observatory's existence on the island for the repair of old instruments or transit of new iterations.[3] The correspondence of Wilmot and others from their colonial stations further illuminates the diverse life-geographies of instruments. These letters, and especially those of Wilmot, describe in detail the passage of magnetic instruments through different geographies and the manner in which this could alter the nature and the very definition of the instrument. In Woolwich, Wilmot's instruments were models, training apparatus, demonstration devices: approachable, touchable and instructive. On their passage in the bowels of HMS *Erebus*, the instruments were damaged and undone. At the Cape they had to be reassembled again as magnetic instruments but in this new space they became guarded objects: sparingly approached for fear of derangement, covered in dust for fear of transgressive touch, often more quizzical and puzzling than instructive. While the instruments had purportedly returned to their designed purpose through their transferral to the Cape and arrangement in the magnetic observatory, the legacy of their positioning as demonstration devices in Dublin and Woolwich remained and obstructed their efficacy, according to Wilmot.

While some tactile marks remained, others did not. As the officers' experience of learning to use the instruments was so practical, so inimitably linked to handling the instruments, that arguably their knowledge of magnetic observation was contained to some extent within the materiality of the instruments. Accordingly, when these instruments suffered so too did the knowledge of their use. Reassembling instruments to correspond with the state in which they had existed in Dublin and Woolwich was therefore not only a mechanical

[1] All quotations from Anderson and McFarlane, 124.
[2] Smythe to Sabine, 8 August 1846, TNA BJ 3/43.
[3] See, for example, the accounts of '15 February 1841', 'January 1844', '1 July 1847', TNA BJ 3/41.

matter, but an epistemological one too. The revised instructions of the Royal Society were supposed to embody the changes but as the experience of Kay and others attested to, this was not always so.[1] The remaking of knowledge of the instruments and of accurate observation was made in the negotiation between the observatory directors' experience of being in their respective locales, the printed instructions (both new and old), and correspondence between the observatories and Lloyd or Sabine.

[1] Lloyd says as much to Herschel in a letter of 8 June 1842, TNA BJ 3/12/241.

Chapter 7: Follow the data: administering science at Edward Sabine's magnetic department, Woolwich, 1841-1857

Introduction

This paper discusses how and where the eye-observed and hand-noted record of the magnetic crusade was lifted from the precarity of manuscript to the supposedly secure and accessible space of the printed page. Histories that have mentioned 'data' and the 'magnetic crusade' in the same sentence have generally done so to illustrate the perceived failure of the British scheme to match its 'hyperbolic claims' and the part Sabine played in this.[1] Morrell and Thackray have set the precedent in this regard. They argued that Sabine was 'not too bothered' that the magnetic crusade did little except to establish a foundation for correct knowledge of the elementary facts and failed to discover any serious theoretical results because so long as the data were accumulating, so the opportunities for Sabine to 'develop his career through his open-ended project' also continued to accumulate.[2] From Morrell and Thackray's perspective, continually amassing data was crucial in the extension and preservation of Sabine's 'private magnetic empire'.[3] However, in using data only in this regard, Morrell and Thackray overlook, and have possibly caused others to overlook, a historical artefact that can illuminate some of the processes, geographies and politics that were involved in the organisation and publication of the results of the British magnetic scheme.

It is to discussions of data as a historical artefact that this chapter will contribute. First, the chapter will engage with the materiality of data, broadly conceived, and with the specific material form in which geomagnetic data were recorded and transported. Next, an example of these data will be described and a close reading of this material will be offered before the chapter moves on to discuss in detail the practices and strategies employed at Sabine's magnetic department to process and manage the deluge of manuscript data received from, specifically, the colonial observatories. This department has received little historical attention but it can tell us much about the organisation of calculation and the continued

[1] Ratcliff, J., *The transit of Venus enterprise in Victorian Britain* (Pickering & Chatto: London, 2008), 24.
[2] Morrell, J., and Thackray, A., *Gentlemen of Science: early years of the British Association for the Advancement of Science* (Oxford, 1981), 529,
[3] Morrell and Thackray, *Gentlemen of Science*, 524

involvement of scientific instrumentation beyond the point of initial observation as so-called "reference instruments". Following this, the chapter will discuss how the magnetic department was established within the military administration of Woolwich Arsenal. This section seeks to demonstrate how spaces other than the observatory and scientific institutions like the Royal Society could become sites of the politics of Victorian science and, by doing so, highlights the significant role of the military in organising Victorian science. The final section of this chapter considers the afterlives of magnetic data and how geophysicists have used the archive to circumvent the procedure enacted by Sabine's magnetic department, a discussion that has relevancy for recent investigations of science in the archives.

The materiality of (magnetic) data

Data collected as part of the British magnetic scheme had a very clear and hefty materiality. The scheme took place in roughly the same period that Hacking associates with the avalanche of printed numbers – 1820-1840 – and it certainly exemplified such rhetoric: Herschel expected no less than 1,958,040 magnetic observations would be recorded, reduced and published between 1839 and 1842, a number that must have been surpassed many times over given the scheme's eventual near two-decade lifespan.[1] Along with mobility and the capacity to act as 'tools for communication … [that] enable intellectual and material exchanges across individuals, collectives, cultures, governments', Leonelli counts materiality as one of the most significant characteristics of data: 'the format and the medium through which [data] are conveyed'. [2] Strasser and Edwards maintain that 'all data without exception have a material aspect', even those data today stored on the "cloud" rely on infrastructures of servers and cables. Data's materiality was obvious to the directors of the colonial magnetic observatories. Data always occupied a large physical space there. The following account of the zealous observational practices of the Rossbank Observatory ought to illustrate this point.

Officers of the Royal Artillery and the Royal Navy had been chosen to manage the colonial observatories not for their mathematical acumen but for their industry. The officers were always keen to remind Lloyd and Sabine of this, especially in the first few years of the magnetic scheme. For instance, Kay, at Van Diemen's Land, wrote that he did not possess

[1] Herschel, J., 'Terrestrial magnetism', *Quarterly Review* 66, 131 (1840), 271-312, 303; Hacking, I., 'Biopower and the avalanche of printed numbers', *Humanities in Society* 5 (1982), 279-295.
[2] Leonelli, S., 'What counts as scientific data? A relational framework', *Philosophy of Science* 82 (2015), 810-821, 810-811.

'great mathematical acquirements' because from an early age he was learning his profession as a seaman 'on the wide ocean'.[1] Lefroy likewise did not pretend to possess 'mathematical or other acquirements' when he was first selected for the magnetic service by Sabine.[2] However, from the outset their zeal and enthusiasm for the scheme of observation was considerable. Kay, without prompting and against the printed instructions of the Royal Society, was the first to exchange the schedule of two hourly observations for hourly observations at his observatory and, on Term Days, observed all three magnetometers every two-and-a-half minutes simultaneously, the latter practice requiring volunteer observers to be procured from the wider society of Hobarton. 'So now I think you will perceive', boasted Kay, 'that we have doubled our observations in every way'. Initially, Kay had only Messrs Dayman and Scott as his assistants, but Rossbank maintained hourly observations day and night until 1848, when this system was stopped. They were motivated by a desire to furnish results unsurpassed by any of the other colonial stations, to show that, as Kay bullishly stated, 'the Ross Bank observatory will yield to none, and…be second to none, in the mass & value of the data they will collect'.[3] Kay was as good as his word. On Term Days alone by May 1842, Kay and his assistants had 'kept 22 Term Days … with observations every 2½ minutes, making 528 hours of uninterrupted observation' and a 'mean average of 7392 carefully noted positions of each instrument.'[4] When Kay talked of the mass of observations they had made, he meant it quite literally. For Kay, the greater the physical mass of the data his observatory recorded, the more valuable his observatory became. Mass had value and Rossbank would 'yield to none' in the size of its collection.

Observations were recorded on thousands of pieces of paper and copied into the various register books regularly sent out from England, and regularly returned thence. On 17 December 1841, for example, the account book of the St Helena Observatory recorded '2 Term Day books, 4 quire of fine writing demi, Ruling, printing, cold pressing and binding the same in half-bazil, extra cloth sides and lett[ered] Term Day – books, St Helena, 1842-1843' to cost the Observatory £4. 7s. 4d. out of the contingent expenses from Allen & Co., London.[5] On top of the Term Day registers there were also the daily registers, abstract books, miscellaneous observation books, books to record magnetic disturbances and sheets of paper bound together to form magnetic dip books.[6]

[1] Kay to Lloyd, 12 November 1840, RS MS/119/II/102.
[2] Lefroy to Sabine, 11 April 1839, TNA BJ/3/81/1.
[3] Kay to Lloyd, 16 January 1842, RS MS/119/II/103.
[4] Kay to Lloyd, May 1842, RS MS/119/II/107.
[5] Account book of St Helena Observatory, 17 December 1841, TNA BJ/3/41.
[6] Account book of St Helena Observatory, 24 June 1843, TNA BJ/3/41.

Each observatory's records had to be returned to Sabine's magnetic department at Woolwich. Here, these data were moved from the precarity of the manuscript to more secure printed copy. In this transferral, these data were often (but not always) first copied from the pages of the returns to the pages of the magnetic department's clerks, where they were summed into means, separated, reduced or discarded, analysed, cleaned, tabulated again and finally sent to the press (fig. 7.1). This was the 'procedure' carried out by Sabine's magnetic department through what might be referred to as 'paper-based technologies'; two terms that are explained below and in relation to the magnetic department in the section that follows.

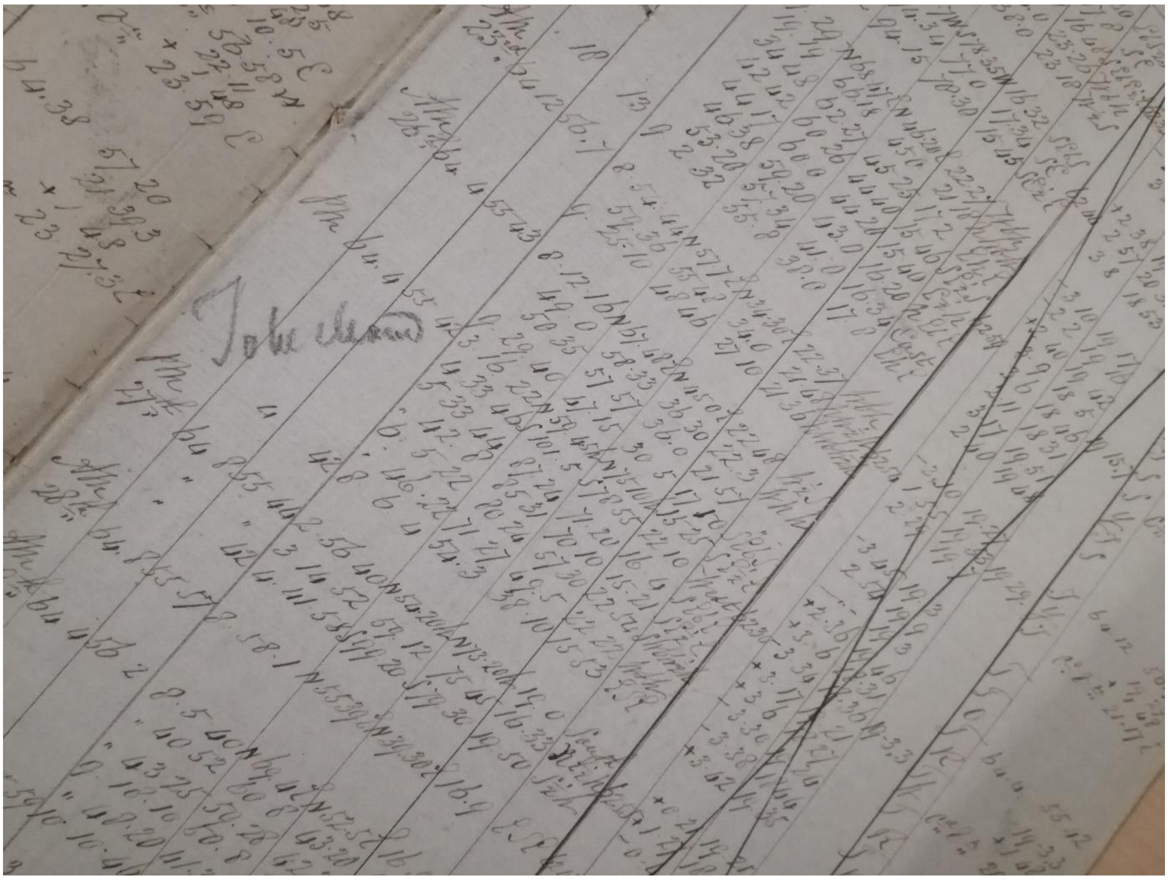

Fig. 7.1. 'To be cleaned'. Declination observations from HMS *Erebus*, September 1842 to March 1843. Author's photograph from the British Geological Survey, National Geological Records Centre, Geomagnetism Archive Books [hereafter BGS], Container 80001374. Item MHM-85007115. Consulted at the Lyell Centre, Edinburgh.

Today, the movement of data between differently curated databases is managed by computer or cloud-based technologies. These technologies can increasingly handle dizzying amounts of data and it is vital that they do. The "data tsunami", which astronomers are struggling to keep afloat in, Hsia has explained, currently stands at one petabyte of publicly available material in electronic form and is 'increasing by 0.5 PB [petabytes] each year, a rate expected to rise sharply as new telescopes and arrays come online'.[1] Talk of data tsunamis and the data deluge has naturally segued into discussions of information overload: whether scientists can possibly hope to utilise the vast pools of data they are amassing. Such fears of information overload have been common in the history of science and current concerns ought not to be taken as indicating the arrival of a new big data paradigm. Strasser sees no novelty in the amount of data accrued and used today because 'comparing amounts of information across worlds that had different technologies to manage it is pointless' and because there is 'no common metric to compare today's petabytes of scientific data with yesterday's analog images, for example, of scientific objects'.[2] Strasser rightly cites Robert Darnton's assessment that 'every age was an age of information, each in its own way'.[3]

As Strasser and Edwards have argued, current concerns with big data are best understood 'by looking at past situations where, in widely different contexts, people were confronted with large amounts of data and devised solutions to deal with it'.[4] In studying the format and medium Carl Linnaeus employed in his data handling practices, Müller-Wille and Charmantier use the term 'paper-based technologies', a term that encompasses the several materials and methods Linnaeus developed to assemble and manage large quantities of data: 'common place books, tabular arrangements and dichotomous diagrams'.[5] In a similar vein, Nasim has explored the 'mundane and common' yet overlooked strategies and technologies by which nineteenth-century astronomical observations were recorded: 'namely, the notebook and the pencil'.[6] Nasim's studies closely examine the observing books of several significant nineteenth-century astronomers. Such examinations, Nasim argues, force us to consider 'how certain kinds of select information – whether numerical, descriptive, or visual, or some combination thereof – were entered, ordered, supplemented, and processed on a series of bound or unbound paper', what Nasim calls 'the procedure'. The procedure, Nasim continues, is the 'self-imposed rhythm and systematic routine of sketch-making or note-taking done on paper with some sort of stylus' and, as such, is 'a set of mediating factors that facilitate data extraction, processing, analysis, and synthesis in

[1] Hsia, F. 'Astronomy after the deluge', in Daston, L., (ed.), *Science in the archives: pasts, presents, futures* (The University of Chicago Press: Chicago and London, 2017), 17-52, 37.
[2] Strasser, B. 'Data-driven sciences: from wonder cabinets to electronic databases', *Studies in History and Philosophy of Biological and Biomedical Sciences* 43 (2012), 85-87, at 86.
[3] Darnton, R., 'An early information society: news and the media in eighteenth-century Paris', *The American Historical Review* 105, 1 (2000), 1-35, at 1.
[4] Strasser, B., and Edwards, P. N., 'Big data is the answer … but what is the question?', *Osiris* 32, 1 (2017), 328-345, 330.
[5] Müller-Wille, S., and Charmantier, I., 'Natural history and information overload: the case of Linnaeus', *Studies in History and Philosophy of Biological and Biomedical Sciences* 43 (2012), 4-15, 4, 6.
[6] Nasim, O. W. 'Extending the gaze: the temporality of astronomical paperwork', *Science in Context* 26, 2 (2013), 247-277, 247.

such a way as to finally be publishable and consumable by the scientific gaze'.[1] One further implication of this, according to Nasim, is that it makes the systematic use of paper and stylus as 'proper and legitimate astronomical instruments in their own right'.[2]

Reading magnetic returns

The form of the registers in which geomagnetic data were recorded was designed in November 1839 by Captain John T. Boileau, the officer who then became the director of the East India Company's Simla Magnetic and Meteorological Observatory. The forms were drawn up by Boileau in consultation with Lloyd during a month-long stay in Dublin, Boileau being both a 'capital observer' and 'full of ingenuity & contrivance'.[3] There were nine forms altogether: one for the daily observation of the three magnetometers and one for observation of the same on Term Days; one form for the observation of the meteorological instruments on a daily basis; and six forms for the abstracts of daily, triple, monthly and Term Day observations of the magnetometers and the other magnetic and meteorological instrumentation. These forms were contained in each of the five books which were kept at the colonial observatories: the day-book, Term Day-book, miscellaneous register, abstract book and a book for curves. The forms designated when, by Göttingen time, and in what order, instruments were to be observed and in what manner observations were to be recorded and sent back to the centre of operations.[4]

The forms were meant to be – or, rather, had to be – as simple as possible to follow, given that they had not been drawn up before the observatory directors left for their different colonial stations and had to be sent on later. Riddell, at the time of his arrival in Canada in October 1839, had certainly not been fully educated on the matter of inscription. He wrote to Lloyd at this time requesting 'an exact form of the manner in which you wish all the observations to be entered in order that they may be all alike' and asked also 'whether the observations should be sent home in books whenever an opportunity offers, or monthly on sheets'.[5] The act of recording the instruments had obviously been of lesser importance than the act of handling the instruments in Lloyd's tuition of the officers, something that

[1] Nasim, 'Extending the gaze', 251.
[2] Nasim, 'Extending the gaze', 251.
[3] Lloyd to Sabine, 18 November 1839, TNA BJ 3/9/118.
[4] Anon, *Report of the President and Council of the Royal Society on the Instructions to be Prepared for the Scientific Expedition to the Antarctic Regions* (Richard and John E. Taylor: London, 1839), 107-120.
[5] Riddell to Lloyd, 8 October 1839, RS MS/119/II/2.

supports and is supported by the fuller exploration of the hasty Dublin education given in the colonial observatories chapter of this thesis.

What can be gleaned from looking at one of these register books, as a source by itself? If we interrogated another material source such as a scientific instrument, we might want to know simple things such as its weight and its dimensions, whether it was complete or not. We may ask questions about provenance – of the material and of its maker – and about how the instrument was put together. We might try to discover through which places and through whose hands the instrument had passed on its way to the museum store, and how it had been archived when it arrived in this place. We would examine the condition of the object and try to infer what this could tell us about how (and how carefully) the instrument was used and whether it had been repaired or modified during its usable life. And, perhaps, were we to follow Arnold and Soderqvist's urging, we would also reflect on the physical actuality and visceral presence of the object, and how this did or did not create an emotional or sensuous response in us.[1] Can the application of a similar set of questions tell us anything meaningful about the observation books of the colonial observatories?

One of the daily register books of the Toronto Observatory will serve as an example (fig. 7.2). A selection of these were viewed, by the author, in Building 17 (of 17) of the Ottawa Geomagnetic Observatory, the direct descendant of the Toronto Observatory in Canada. The Toronto books had been archived – or more aptly deposited – in a slightly shabby and musty outbuilding along with historical data from the Ottawa Observatory, which was established in 1969, and the Agincourt Observatory, the site which had replaced Toronto as the national geophysical observatory in 1898. The daily register books themselves were large, heavy, bound volumes containing dozens of pages – each single page about 30 x 20cm – of observations of the three magnetometers – three separate observations of the declination and horizontal force magnetometer and their means, and two observations of the vertical force magnetometer and its means – taken at six-minute intervals. At the bottom of each page, readings of the thermometers of the horizontal and vertical force magnetometers were recorded, together with the dry and wet bulb thermometers and the barometer. Underneath these is a short one-line report on the weather conditions. These meteorological observations were taken at the top of each hour. The books were originally blank, but were ruled in black and red ink by the Toronto Observatory staff to create the forms which Boileau and Lloyd had devised. The inscriptions were made in black ink. The paper for the book had likely been procured from Allen and Co. of London.

[1] Arnold, K., and Soderqvist, T., 'Medical instruments in museums: immediate impressions and historical meanings', *Isis* 102, 4 (2011), 718-729.

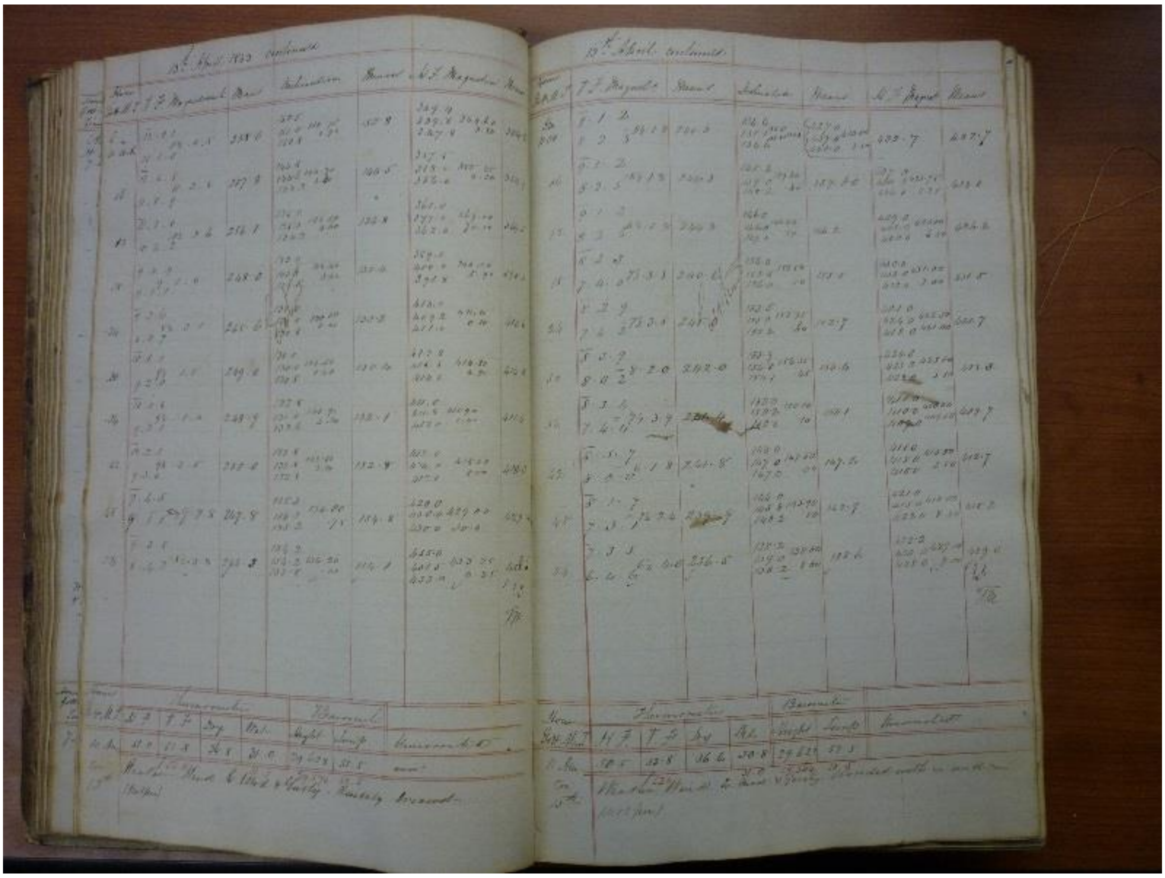

Fig. 7.2: The daily register of the Toronto Observatory, 13 April 1842. Ottawa Geomagnetic Observatory collection [no catalogue]. Author's photograph.

For the observations of 13 April 1842 for each six-minute interval at 10am and 11am Göttingen time – equivalent to 4am and 5am in Toronto – each magnetometer, as aforementioned, was recorded multiple times and the means of these calculated and included in a separate column. There are ten rows on each page, which means twenty different recordings of the vertical force magnetometer and thirty of each of the other two magnetometers, a total of eighty 'eye-observations' to be made each hour: eighty instantaneous glances at the deflection of a needle, to paraphrase Graeme Gooday.[1] At the bottom of each column is a tick mark and in the corner of each page two sets of initials, indicating that two different observatory assistants checked the observations for their accuracy, and that a third, perhaps the director at this time, Younghusband, may have taken

[1] Eye-observations is a term from Sabine, E., 'XI. Records of the magnetic phenomena at the Kew Observatory – no. IV. Analysis of the principal disturbances shown by the horizontal and vertical force magnetometers of the Kew Observatory, from 1859 to 1864' *Phil. Trans. R. Soc. Lond.* 161 (1871), 307-319, 307. It was to distinguish those made by the hand and eye from those made by photographic self-registering equipment, which was gradually installed from the late 1840s, see Gooday, G. J. N., *The morals of measurement: accuracy, irony, and trust in late Victorian electrical practice* (Cambridge University Press: Cambridge, 2004), xiv.

the initial observations. The initials on 13 April belong to Acting Bombardier Thomas Menzies and one other, whose mark is illegible.

This one page alone tells us several things. To start with, it details some of the categories of observations that the observatory was tasked with recording; it tells us how many observations were required in an hour of watching the three magnetometers and, if we extrapolate out, how long it roughly took for a single observation to be recorded (seconds!); it tells us something about the division of labour within the observatory and to what extent observations were checked for accuracy; and it demonstrates – even though the register book left in Ottawa is a copy – in what material form the data of the colonial observatories was returned to the centre. It ought finally to be noted that, as the picture above demonstrates, the registers were kept in a very neat and tidy fashion, conveying both a sense of pride in the diligence of their record-keeping (fig. 7.2). As Schaffer has argued, keeping the observatory books in such a meticulous and clean state was part of the designedly business-like accountancy that occurred in astronomical and later geophysical observatories in the nineteenth century. It was a means of putting observatories on the map, of constructing its good reputation and maintaining the fantasy of observatory work as a 'capitalist enterprise, with prudent ledgers, patient accountants, disciplined observers, well-oiled machinery, and precision values as sources of profit'.[1] It was through such means as prudent ledgers that Toronto could create an artifice that belied the messy reality of an observatory often beset by issues of delays to returns, drunken personnel, uncooperative equipment and a poorly maintained building.[2]

Counting at the magnetic department

In the following section I will demonstrate what, in a different context, Nasim has referred to as 'the procedure': the 'self-imposed rhythm and systematic routine' that facilitated not only the extraction of data but also its 'processing, analysis, and synthesis in such a way as to finally be publishable and consumable by the scientific gaze'.[3] The mediatory steps

[1] Schaffer, S., 'Keeping the books at Parramatta Observatory', in Aubin, D., Bigg, C., and Sibum, H. O. (eds), *The heavens on earth: observatories and astronomy in nineteenth-century science and culture* (Duke University Press: Durham and London, 2010), 118-147, 124.
[2] Some of this was touched on in the first chapter of this thesis on the colonial magnetic observatories. More on the problems Toronto experienced can be found in Thiessen, A., D., 'The founding of the Toronto Magnetic Observatory and the Canadian Meteorological Service, *Journal of the Royal Astronomical Society of Canada* 34 (1940), 308-348; and Thiessen, 'Part VI – Lieutenant C. W. Younghusband, R. A., Acting Director of Her Majesty's Magnetical Observatory, Toronto, 1841-44', *Journal of the Royal Astronomical Society of Canada* 35 (1941), 205-224.
[3] Nasim, O. W., 'Extending the gaze', 251.

involved in processing the returns – reductions by temperature, ambient iron, pressure and time, the different tabulations and checks and the identification of the irregular and the 'normal' observations – that gave to the results of the magnetic scheme a 'temporal thickness': data that were durable and trustworthy enough to circulate geographically and temporally in print. The procedure for doing so was enacted by the humble pencil, pen and paper. Nasim has argued for the entry of these items into the category of 'proper and legitimate astronomical instruments in their own right' because they were, together with the hand and the eye at the telescope, 'integral to the gradual discerning and systematic stabilizing of something barely visible'.[1] Through following the data back to Woolwich, I agree with Nasim's assessment. For it was here that the observations of the crusade were given their final form, where the peculiarities of the observer and the instrument and the geography of the observatory were identified and separated and from where final observations were distributed. And it was the systematic use of the pencil and pen and paper that enabled this, a point explored in what follows.

In one respect, the Woolwich magnetic department was simply an esemplastic organisation, as it was here that they received the hourly, regular, returns from the colonial observatories, already summed into hourly and daily means by the colonial observatory staff, along with the corresponding tables of temperature at which observations had been recorded with the different instruments. Each observation, as Sabine outlined above, was subject to four different reductions at Woolwich and the resultant data published both individually and summed into means. Sabine also remarked that some of the data received from the Toronto Observatory was simply 'printed from the original manuscripts'.[2] In so doing, the scheme conformed with the opinion expressed by Herschel to Sabine before the publication of results began, that data be published for theorists everywhere in both its original form and summed into means, rather than being presented exclusively as the latter. The staff at Woolwich were usually engaged in the regular reductions of the monthly returns, together with administering and cohering the observations together and putting them to print. Until, that is, the returns needed to be investigated and separated into those which exhibited irregular changes to the magnetic force and those which existed within a calculated range of normalcy. For this, several different things needed to happen to make the observations talk, the first of which was correcting for temperature variance.

[1] Nasim, 'Extending the gaze', 251; and Nasim, O. W., *Observing by hand: sketching the nebulae in the nineteenth century* (University of Chicago Press: Chicago, 2013), 1.
[2] Sabine, *Observations made at the magnetical and meteorological observatory at Toronto in Canada, Vol. III*, ix.

It has been noted elsewhere in this thesis how important it was to (try to) keep a regular temperature within the magnetic observatory. As we have seen, the observatory directors employed different strategies for achieving this, from partitioning rooms and adding verandas to surrounding instruments and magnetic rooms with dirt and clay. Still, periodically, experiments to determine the effect of temperature on the different magnets of each individual observatory had to be made and the results returned to Woolwich. This process required that the temperature of the magnet itself be recorded and coefficients calculated from the results. The need to calculate the temperature corrections was not, as we have also seen, a favourite task of the directors and was one particular area in which observatories could fall into arrears. For instance, in the first publication produced by the Woolwich magnetic department, in 1843, on unusual magnetic observations recorded by the magnetic scheme up to 1841, 'all the materials required for the complete reduction of the observations' had not been received in time to be included. The coefficients for the temperature corrections of the horizontal and vertical force observations of the Antarctic expedition and those of the Van Diemen's Land Observatory, as well as the vertical force observations from St Helena and the Cape of Good Hope, were missing, 'the necessary experiments for their determination not having yet been made'.[1]

Despite the labour of experiment and calculation that temperature corrections entailed, each observatory director had to undertake them at one time or another and some, such as Lefroy, showed themselves to be quite competent and serious experimenters. In fact, during his second stint in charge of the Toronto Observatory, the calculation of an accurate temperature coefficient became one of 'earnest and even anxious consideration' for Lefroy and something in which he departed from the method provided to him by the instructions of the Royal Society. These instructions were though followed by Wilmot (and, temporarily, Clerk) at the Cape and by Kay at Hobarton.

These instructions described how to test for the effect of temperature on the magnetic moment of a bar by causing it to deflect a freely suspended magnet and by observing the deflections produced when the bars were differently heated.[2] The officers were directed to fill a copper trough (provided) with water in which the magnetic bar was to be placed 'in the east and west line, passing through the centre of the suspended magnet'. The magnet and the thermometer were then to be left a sufficient time for both to 'take the common temperature of the surrounding fluid' before the temperature was raised by degrees through

[1] Sabine, E., *Observations on days of unusual magnetic disturbance made at the British colonial magnetic observatories, part I – 1840-1841* (Longman, Brown, Green, and Longmans: London, 1843), iii.
[2] Anon, *Revised instructions for the use of the magnetic meteorological observatories and for the magnetic surveys* (Richard and John E. Taylor: London, 1842), 38.

the replacement of the tepid water with hotter water so as to furnish results in a range from the temperature of the air up to 90˚ Fahrenheit. Should it be a particularly hot day on which the experiments were to take place, then the scale was to be extended in the opposite direction, with the use of ice. Wilmot's first attempts in 1841 and Clerk's in 1843 were deemed unsatisfactory by Sabine, but acceptable results were produced in experiments of 1844. However, Wilmot's experiments had to be repeated by Riddell at Woolwich in 1847, to ascertain what other value of the coefficient might be obtained at different thermometric temperatures, Wilmot's experiments having only been taken at about 78˚ or 79˚F, rather than within the aforementioned range. The experiments were also repeated two years later, again at the Cape, by Thomas Maclear, who had taken over proceedings at the magnetic observatory on its transferral from the Ordnance to the Admiralty in 1849. However, on this occasion Maclear used copper troughs filled with olive oil, the temperature of which could be altered by the introduction of hot water or pounded ice between the troughs. The same course was adopted with the magnet of the bifilar magnetometer. The use of olive oil in place of water was not wholly explained by Maclear, the famed astronomer only alluding to the fact that he had chosen the 'medium of olive oil' because of the nature of the experiments and because experience had taught him 'how difficult it is to ascertain the exact temperature of a metallic bar'.[1]

In the third volume of results from the Toronto Observatory, Sabine reproduced a report from Lefroy describing the alternative process of experimentation he had adopted to arrive at the ratio by which the magnetic moment of a bar was changed by each increase of 1˚F, the coefficient that would allow Lefroy to reduce his bifilar observations to a common temperature. Lefroy, in 1848, set up his bifilar magnetometer for 'a direct experiment with the magnet suspended precisely as employed in the hourly observations', a schedule of observation which had in that year been terminated. For the experiment, the 'magnetometer was enclosed by boards extending from the floor to the ceiling, in a space sufficiently large to include also a copper stove'. The 'inner case of gilt wood was removed, and the outer one was slightly raised by wedges to allow the air in the [magnetometer] box to acquire the temperature of the rest of the room' and the stove was always in the same position, 'whether heated or otherwise'. Lefroy reported to Sabine that,

> The experiments were made by kindling a fire and keeping up the temperature for three days, then allowing it to go out, and opening the communication with the external air for the same length of time. There were five cold and three hot

[1] Sabine, E., *Observations made at the magnetical and meteorological observatory at the Cape of Good Hope, Vol. I, magnetical observations, 1841 to 1846* (London: Longman, Brown, Green and Longmans, 1851), xxxi.

> alternations, each of the three days [and] the readings were taken every half-hour from 6 a.m. to 11 p.m. … To compute the results two abstracts are formed, one containing the half-hourly observations on the fifteen cold days, the other those of the twelve hot days.[1]

Lefroy further explained to Sabine the process of calculation – correcting means, multiplying by the ratio of scale coefficients and so on – by which he arrived at a figure of 1.74 'as the equivalent in scale divisions for 1° of Fahrenheit', a figure which was remarkably close to Sabine's own calculations, as we will see below.[2] The difficulty of maintaining an equable and regular temperature at the colonial observatories necessitated the kinds of experiments described above and also demonstrated how the officer-observers who manned these stations were not simple data collectors but could be, at least in one regard, adept experimenters too. After all these were not, as Maclear remarked, straightforward experiments.

What the preceding also demonstrates is that in the biography of a magnetic instrument, the story does not end after the instrument's initial observations. Instruments were involved in the whole terrestrial magnetic scientific process. Needles were experimented with to deduce their relationship to temperature. These needles were also regularly returned to Woolwich for further inspection and comparison. The silk used to suspend the needles of the magnetometers was also regularly tested to find out the amount of torsion each suspension exhibited so that coefficients could be arrived at and applied to the results of observation. We have also witnessed how important were the proxy instrumentation set up at Sabine's magnetic department and how it was crucial that these instruments exist proximate to the results of the colonial observatories as they were being reduced. These magnetic instruments were then at one and the same time instruments of observation, instruments of experimentation, and instruments of inspection and calculation.

To return to the Toronto temperature results, it is clear that Sabine was impressed with Lefroy's alternative temperature experiments, and he remarked on the 'care which must have been taken in conducting the experiments under the very difficult condition of regulating artificial temperatures in air heated by a stove'.[3] Lefroy's deviation from the printed instructions was probably not wholly surprising to Sabine. Lefroy had exhibited similar tendencies in the adjustment of his instruments during his time at the St Helena

[1] Sabine, *Observations made at the magnetical and meteorological observatory at Toronto in Canada, Vol. III*, v.
[2] Sabine, *Observations made at the magnetical and meteorological observatory at Toronto in Canada, Vol. III*, vi.
[3] Sabine, *Observations made at the magnetical and meteorological observatory at Toronto in Canada, Vol. III*, vi.

Observatory. He was, Lloyd lamented, 'always taking new theoretical notices into his head, which are always sure to be wrong'. Ironically, given Lefroy's later accuracy in establishing coefficients, Lloyd wrote in the same letter that Sabine would 'have something to do in putting his [Lefroy's] results into a shape fit for publication on account of the numerous breaks, *erroneous coefficients* … etc. etc.'.[1]

It ought also to have been little surprise to Sabine that Lefroy was so capable at manipulating and regulating air temperature within the observatory space, given the difficult conditions in which Lefroy had laboured at, and been able to produce credibly accurate results from, the temporary observatories of his magnetic survey in the Northwest Territories. One might suggest that Lefroy's alternative choice of temperature experiment was in part inspired by having witnessed a Cree 'sweating bath' during his magnetic survey in late 1843. In this part sanitary, part religious ceremony, the Crees built a small lodge in which three people could fit, heated the inside to an 'intolerable temperature' – which Lefroy, having passed a thermometer to one of the participants, recorded at over 140˚F – and then, once they could stand it no more, the cover was thrown off and the 'performers' huddled together and exposed themselves to the freezing temperature outside.

The actual value of the coefficient, according to Sabine, of the scale of change corresponding to 1˚F, was 1.63. The close approximation with Lefroy's figure of 1.74 provided more testament of Lefroy's diligence and experimental accuracy. Sabine had arrived at his figure through working with the 'whole body of the observations' from Lefroy's observatory from 1843 to 1848, the number of which 'considerably exceeded 100,000 in number' and each of which had been passed through 'several distinct processes'.[2]

The coefficient 1.63 as formulated by Sabine and his Woolwich magnetic department was applied to each individual observation of the Toronto Observatory from 1843 to 1848, to reduce these observations to a uniform temperature of 55˚F. The rationale for doing so was simple: it cleared the observations from the influence of temperature on the magnetism of the bar, but retained 'whatever effects may have been due to disturbances'.[3] In other words, reduction to a common temperature of a large set of observations separated the quotidian from the extraordinary. The recognition and recording of magnetic disturbances was a key focus of each of the colonial observatories from the outset and always remained

[1] Lloyd to Sabine, 8 February 1841, TNA BJ 3/11/188. Emphasis added.
[2] Sabine, *Observations made at the magnetical and meteorological observatory at Toronto in Canada, Vol. III*, vi, viii.
[3] Sabine, *Observations made at the magnetical and meteorological observatory at Toronto in Canada, Vol. III*, vii.

an important portion of the magnetic scheme, sitting alongside the determination of absolute values and secular changes, and acting as a 'preliminary step' to obtaining results 'from which a correct knowledge and analysis of the progressive and periodical changes were to be obtained'.[1] From a historical perspective of the magnetic crusade, the importance of the calculation of the disturbed observations is in the light it sheds on some of the processes undertaken by the staff at Sabine's Woolwich magnetic department.

Reporting specifically on the separation and analysis of the larger disturbances in the observations of the Toronto bifilar magnetometer, Sabine explained that 'the first step taken at the Woolwich office was to rewrite the whole of the observations of the five years', a number in excess of 35,000 observations, in 'scale divisions' reduced to a 'convenient approximate mean temperature', 55˚F. For this, tables were drawn up by two NCOs, 'each working independently of the other, and having the correctness of the work proved by the accordance of the two independent computers' and superintended by the principal clerk in the office, Sgt Magrath. The same process and division of labour was practised in calculating the daily and hourly means of the observations with an additional check being made by comparing the results with the daily and hourly uncorrected means computed at Toronto. Once these tables had been formed by the Woolwich staff, they were passed to Sabine, who would look for anomalous observations indicative of an irregular change in the magnetic force. Sabine carried out this task by scanning through the tables of numbers marking 'provisionally with a pencil every observation which differed 14 scale divisions or more from its normal'. 14 scale divisions was not an arbitrary range. It was, rather, a 'convenient minimum limit' for detecting the largest disturbances 'being on the one hand a greater departure from the normal value than could reasonably be ascribed to any other cause than that of a disturbance in the earth's magnetism, whilst on the other hand the number of disturbances that would be thereby separated would form a sufficient body to permit their periodical laws (if such existed) to be investigated'.[2]

Once Sabine had made his provisional identifications, he then recomputed the 'normals' having omitted the potentially disturbed observations, and then compared afresh all of the observations, including those potential disturbances, and continued this process until he was satisfied that the 'normal in every case included every observation which differed less than 14 scale divisions from itself, and excluded every observation which differed 14 scale

[1] Sabine, E., 'On the means adopted in the British colonial magnetic observatories for determining the absolute values, secular change, and annual variation of the magnetic force', *Phil. Trans. R. Soc. Lond.* 140 (1850), 201-219; and Sabine, E., 'On what the colonial magnetic observatories have accomplished', *Proc. R. Soc. Lond.* 8 (1856), 395-413, 398.

[2] Sabine, *Observations made at the magnetical and meteorological observatory at Toronto in Canada, Vol. III*, ix.

divisions or more from itself'.[1] This process allowed Sabine to identify anomalous observations that might indicate a disturbance in the force. These results were then 'marked finally with a surrounding ring in ink' before all of the numbers and corrections were passed back to be examined by a separate computer. From here a table was formed by two computers, again working separately, containing the marked disturbances during the five years arranged chronologically, showing the day, the hour, and the amount of disturbance. These tables were then passed to Sabine, who 'proceeded to distribute the disturbances according to the years, months, and hours of their occurrence, separating them into disturbances increasing and disturbances decreasing the force'. These were the tables then finally included in the third volume of the Toronto Observations to illustrate normal results corrected for temperature and observations exhibiting irregular disturbances, at least of the horizontal component of the earth's magnetic force. The number, incidentally, of observations identified – marked initially in pencil then ringed in ink – in which the amount of disturbance equalled or exceeded 14 scale divisions in the five years was 2,968 of the more than 35,000 bifilar observations passed from Toronto to Woolwich between 1843 and 1848.[2] The same analysis of the horizontal force returns of the St Helena Observatory from September 1842 to August 1847 found 2,620 irregular observations out of almost 36,000 observations.[3]

All the observations, checks and reductions could not be allowed to exist only on manuscript for too long. As Herschel had pointed out, manuscripts were precarious and too likely to be subject 'to the casualties of time and accident'.[4] The observations had to be put to print. The form in which this publication was made mattered to the contents within. The British magnetic scheme was, as many natural philosophers had pointed out both before and after its inception, a prestigious and potentially incredibly significant scientific project. Any publication stemming from Sabine's magnetic department had to reflect this fact not only in the comment and observations it offered, but in the form in which the results were presented.

To this end, it was decided that the publications ought to be made in quarto, and not in the smaller, cheaper, alternative of octavo. Both Herschel and Lloyd had subscribed to this view. Lloyd especially urged the adoption of the quarto format, for it engendered a small

[1] Sabine, *Observations made at the magnetical and meteorological observatory at Toronto in Canada, Vol. III*, ix-x.
[2] Sabine, *Observations made at the magnetical and meteorological observatory at Toronto in Canada, Vol. III*, x.
[3] Sabine, E., *Observations made at the magnetical and meteorological observatory at St. Helena, Vol. I – 1840, 1841, 1842, and 1843* (Longman, Brown, Green and Longmans: London, 1847), x.
[4] Herschel to Sabine, 27 January 1841, TNA BJ 3/27/38.

additional expense but greatly improved the respectability of the publication.[1] What is more, it would make the publication consistent with other foreign publications. As an example Lloyd added that the Russian magnetic system was printed in quarto. Quarto was, according to Lloyd, the form 'adopted by almost every scientific society in its Transactions' and only those less important and 'more ephemeral productions' were consigned to the octavo format.[2] The magnetic data and the format in which it was disseminated mutually reinforced a sense of quality and respectability. The collected data itself, stemming from such a far-reaching, well-supported and well-financed project as the British magnetic scheme was worthy of the quarto format, and at the same time made respectable and imbued with quality by the adoption of the more expensive quarto format. This is also further evidence of Gooday's emphasis on the *reporting* of a measurement or measurements as one of the four morals of measurement, as he terms it, which coalesced to provide a standard of measurement capable of eliciting faith or trustworthiness in the results. Gooday argues that the creation of trust (or, indeed, mistrust) could be contingent on non-human factors. For instance, it could be 'the hardware itself, the materials out of which it was made, the techniques used to make it, or the theories employed in interpreting its performance'.[3] I would only add that the discussion of the formatting of the magnetic scheme's results arguably shows that the creation of trust in results was also contingent on the material form in which measurements were presented, and not only on the materials out of which they were made.

What Sabine and his magnetic department had done, in their organisation and tabulation of data, was move geomagnetic data from a local to a nonlocal configuration. This was a significant part of the data packaging process that occurred at Woolwich. Sabine's department collected what Ian Hacking has called 'marks', those pencil and ink records obtained through observation and measurement in the magnetic observatory and on surveys and returned to Woolwich.[4] These marks, to use Leonelli's words, constituted 'unique documents about a specific set of phenomena', the construction of which was 'constrained by the experimental setting' in which they were made.[5] In order to make these marks durable and accessible elsewhere as data, Sabine and his assistants – the curators –

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[1] Lloyd to Sabine, 28 January 1841, TNA BJ 3/11/185.
[2] Lloyd to Sabine, 3 May 1841, quoted in a (copy of a) letter from Sabine to Colonel Fox, 30 April 1841, TNA BJ 3/27/49-50. The discrepancy in dates here either reflects the fact that the copy of the Sabine letter was made after the receipt of Lloyd's letter or that Sabine had begun his letter to Fox on 30 April, but not sent it until after receipt of Lloyd's letter.
[3] Gooday, *The morals of measurement*, 2.
[4] Hacking, I., 'The self-vindication of the laboratory sciences', in Pickering, A. (ed.), *Science as Practice and Culture* (University of Chicago Press: Chicago, 1992), 29-64, 48.
[5] Leonelli, S., 'On the locality of data and claims about phenomena', *Philosophy of Science* 76, 5 (2009), 737-749, 740-741.

formed databases, which presented data stripped of the locality of its making: nonlocal data. When printed, this was the form in which data was circulated to people, institutions and countries across the world and how it was stored for posterity. For Rheinberger, 'the ability to be stored, that is, to be made *durable*, is the most important prerequisite for transforming *traces* into *data*'.[1] Storage confers durability, which in turn makes marks and traces into data and furthermore makes data portable, retrievable and capable of re-enactment. This is how so much magnetic crusade data has survived over time and remained capable of inclusion in twentieth-century collections.

Observations were taken but not made at the colonial magnetic observatories. The demarcation resides in what, as Gooday has asked, 'actually *constituted* the 'measurement' of a physical quantity'.[2] In early nineteenth-century geomagnetism, observation of the instruments in the observatory was just the first step in measuring the earth's magnetic force. These inscriptions were recorded loaded with contingency. As has been explored elsewhere in this thesis, one of these contingencies was the capability and performance of the instruments, to include of course the often ambiguous and frequently errant behaviour of the magnetic bars; another, the capability and performance of the observers. Varying temperatures could equally disturb observational accuracy and so too could breaks in the series of observations, which happened on occasion due to faulty instrumentation and personnel problems. And then of course there were the irregular changes in the force which skewed the returns and had to be separated. The question is, how did Sabine and his department identify, quantify and address such disruptions and produce publications fit for scientific and public consumption? The answer is found by following the data from the point of its extraction – at the observatory – to the point of its making – at Woolwich.

It is somewhat axiomatic to point out but for there to be magnetic 'returns', there had to be somewhere to which they could be returned. In the case of the British magnetic scheme, this place was the Woolwich magnetic department, and it is to this place that the chapter will now turn. Woolwich was not supposed to be the single central point of the aggregation, tabulation and publication of the crusade's results. As is explained in more detail below, Lloyd's Dublin Observatory was intended to help fulfil this role alongside Woolwich and thereby create a dual centre. When it became apparent that this would not happen, Sabine was prompted to establish his magnetic department as a considerable (and legitimate) part of an otherwise wholly military space, the headquarters of the Board of Ordnance. This search for legitimacy was played out on several different fronts,

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[1] Rheinberger, H-J., 'Infra-experimentality: from traces to data, from data to patterning facts', *History of Science* xlix (2011), 337-348, 344.
[2] Gooday, *The Morals of Measurement*, xiv.

concerning appropriate space, personnel and designation. At all times, Sabine's striving for a proper establishment at Woolwich was centred around the very tangible deluge and piling up of returns from, mainly, the colonial observatories and the Polar surveys of Lefroy and Ross. These materials created a very real politics of data: who was to reduce and publish them? In what form? Where in Woolwich and at whose expense was this to take place? What ought to be sent here, and what ought not? This place also played a very central part in the biography of the crusade's data itself, and so in exploring what happened at Woolwich we also explore what happened to make data from all over the globe – collected in different climates, collected on the move and in situ – commensurate, credible and publishable.

Making the magnetic department count at Woolwich

From the beginning, blank forms for the monthly reports were sent out to each colonial observatory. These forms provided the template for how each of the observatories on the British magnetic scheme should note their geomagnetic observations. Initially, the returns of the colonial observatories were simply accumulated by Sabine at Woolwich, and checked over for their accuracy at this site and with Lloyd in Dublin. The returns were to remain in Sabine's hands 'until the decision of the Master General [of the Ordnance] regarding their publication'.[1] This decision was made in late 1840 when Sir Hussey Vivian, Master General, asked Sabine to consider what the best course of action regarding publication would be. Lloyd was convinced of the need to publish the results and so too was Herschel, so long as 'individual observations' and not exclusively statements of 'daily, monthly, or annual means' were printed, as well as the curves. 'Of the necessity of publication there can be no doubt', Herschel continued, for

> the record of one of the greatest, perhaps the very greatest scientific operation that has ever been undertaken, ought not … to be consigned to the casualties of time and accident in manuscript, nor indeed can their full import be extracted by one theorist, or in a single generation. Like astronomical observations, they ought to be at once secured from the possibility of destruction, and rendered accessible to computists of every age and nation by publication and by active dispersion among scientific persons and institutions, and public libraries in every civilized country.[2]

[1] Sabine to Sir Huw Dalrymple Ross, 10 November 1840, TNA BJ 3/27/21-24.
[2] Herschel to Sabine, 27 January 1841, TNA BJ 3/27/38.

Publication of the results in this manner conformed with Herschel's own universal scientific methodology which, influenced in part by Humboldt and in part by Baconian methodology, argued that theories could be developed to unify different fields of science if enough observational data were collected and, importantly, published for the deliberation of theorists across all nations.[1] Herschel's comments also speak to perceptions of the fragile and insecure nature of the paper-based, hand-inscribed observations that were sent back from the observatories. While still in this form the observations were vulnerable and required replication through printing technologies to become durable and retrievable for contemporary and future theorists.

Due to Sabine's consultation with Herschel and Lloyd, a plan for the publication of the colonial observatories' returns was not provided to the Board of Ordnance and the Treasury for several months. When it did materialise, this plan estimated an annual charge of '£1018 for the octavo publication, or £1188 for the Quarto, inclusive of superintendence, computers and clerks work, and an office in which the work may be done'.[2] This estimate did not include consideration for the publication of the Van Diemen's Land Observatory, which remained under the formal administration of the Admiralty in 1841. Eventually, in February 1841, the decision that the publication of the Ordnance observatories' results would be made at Woolwich under Sabine's superintendence and with Riddell's assistance was taken through the agreement of the Marquis of Northampton, President of the Royal Society, and the Master General of the Ordnance, Sir Hussey Vivian. The Treasury would foot the bill, there being no money for the publication of the 'magnetic experiments' at the disposal of the Ordnance.[3]

The centre of calculation and publication for the British magnetic scheme was located at the headquarters of the Royal Artillery, Woolwich, and more specifically at the Royal Military Repository and Royal Artillery Regimental Institution there. The Institution had been resurrected from an older regimental society that had 'died a natural death on the breaking out of the American War'.[4] It had been remade by Lefroy and Wilmot in 1838 for the purpose of providing inquisitive officers such as themselves with better instruction in science and mathematics. Lefroy, when later stationed at St Helena, was happy to hear

[1] Carter, 'Magnetic fever', 27-35.
[2] Plan and estimate for the publication of the returns from the ordnance magnetic observatories', TNA BJ 3/27/27-29.
[3] CR Fox to Sabine, 2 December 1840, TNA BJ 3/27/31.
[4] Lefroy, J. H., *Autobiography of General Sir John Henry Lefroy*, ed. Lady Lefroy, printed for private circulation (n.p., n.d.), 28.

from Sabine that Sabine's office had been established at the Institution, remarking that this move would indeed give his and Wilmot's fledgling society 'a leg to stand on'.[1]

Sabine for his part was not so cheered. He had hoped to construct a physical observatory at Woolwich and had tried to put a proposal through the Royal Society for a grant to support one in the summer of 1840.[2] This would have been a place for the trial and improvement of both instrument and user as well as a suitable venue for the reduction and publication of the colonial observatory returns but nobody, lamented Sabine, had supported him in his proposal.[3] A physical observatory – though not necessarily one adhering to Herschel's definition of that term – was in 1842 established in Britain at the Kew Observatory by the BAAS, a tale which Macdonald has recently provided an illuminating account of.[4] Despite the fact that Lloyd's Dublin Observatory had been privately touted by Sabine as the head of the family of the colonial observatories at the outset of the crusade, Dublin's role was slowly diminished and accumulation and calculation of returns concentrated at Woolwich as the magnetic scheme developed.

However, this diminution of Dublin's role had not been Sabine's original intention and Sabine had in fact asked Lloyd if Dublin could be used as a site for the computation of returns. In December 1840, when Sabine was ruminating on how to publish the magnetic scheme's results, he wrote at least twice to Lloyd on the subject. In the first, Sabine insisted that there would be 'no one to interfere with your directing the publication of their observatories in the manner you [Lloyd] see best' and, in a second letter on the cost of computation Sabine had asked, 'why should not that part of the work be done in Ireland, by the same persons who perform such operations for you [Lloyd]?' especially as they already knew, Sabine had thought, the routine of reduction.[5] Sabine did not always envisage concentration of the magnetic scheme at Woolwich, as these letters attest to, but rather a geographic and institutional division of labour. However, Lloyd was not keen on Sabine's plan, telling Sabine that he did not see 'that we should gain anything by having half the clerks work done here & the other half in London. It would', Lloyd continued, 'only be increasing the total amount of trouble & losing something on the score of [the] system'.[6] Besides, Lloyd thought that one computer and one clerk would be sufficient to work

[1] Lefroy to Sabine, 22 September 1841, TNA BJ 3/81/38.
[2] Carter, 'Magnetic fever', 109.
[3] Sabine to Lloyd, 8 May 1841, RS MS/119/I/97.
[4] Macdonald, L. T., 'Making Kew Observatory: the Royal Society, the British Association and the politics of early Victorian science', *Brit. J. Hist. Sci*. 48, 3 (2015), 409-433.
[5] Sabine to Lloyd, 4 December 1840, RS MS/119/I/91 and Sabine to Lloyd, 13 December 1840, RS MS/119/I/92.
[6] Lloyd to Sabine, 19 December 1840, TNA BJ 3/10/170.

through eight sheets of results, all that he thought would be obtained from the Ordnance observatories on a regular basis, and these could easily be accommodated at Woolwich.

Lloyd was increasingly marginalised as the scheme and the publication of its results forged ahead. Lloyd, unlike Sabine who could be relieved of his other, military, duties for the duration of the magnetic scheme, had still to run his Dublin Observatory, which was itself 'miserably in arrears in regard to reductions' and behind schedule with regard to its own publication and Lloyd would 'do nothing further for the public cause' until they were 'got forward'.[1] Lloyd besieged Sabine not to bring him any more 'magnetic papers' nor even to show him any when Sabine next visited Dublin as he could not 'do in "half an hour" what you [Sabine] can' and Lloyd had 'very few multiples of half an hour to spare'.[2] On top of the work created by the observatory, at the time that the direction and organisation of the publication of the magnetic scheme's results was up for consideration, Lloyd was contending with lectures starting again at Trinity College Dublin, an upcoming examination for a fellowship, two papers going through the press and a commission from the government on how best to dispose of a grant of £5,300 lately bequeathed to the Dublin Society.[3] Lloyd still consulted the returns of the colonial observatories when he could and corrected many of the errors of their directors – for which he felt he ought to be formally recognised by the Master General of the Ordnance as, without Lloyd's assistance, the Ordnance 'could not have easily accomplished what they have done' – but this consultant role was all he believed should be expected of him in the future.[4] He wished, he told Sabine, to have his own department – i.e. consulting observations and corresponding with the observatories – to himself and 'to leave to others theirs'. New duties – i.e. forming the publications – had been put upon him which he had 'never bargained for' and were more than he had originally undertaken to do or that he had time to pursue.

The resultant lack of a 'headquarter Observatory', such as Dublin would have otherwise become, in which other observatories' instruments could be 'prepared and verified', constants carefully determined, and from which new instruments could be devised, tested and sent out for use and to which 'practical difficulties of all kinds which may present themselves' could be referred, was an obvious regret to Sabine and something only ever 'imperfectly remedied by the Woolwich establishment'.[5] Still, Sabine had been forced to

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[1] Lloyd to Sabine, 5 January 1841, TNA BJ 3/10/177 and Lloyd to Sabine, 10 March 1841, TNA BJ 3/11/191.
[2] Lloyd to Sabine, nd, TNA BJ 3/11/190.
[3] Lloyd to Sabine, 28 April 1841, TNA BJ 3/11/195.
[4] Lloyd to Sabine, 4 May 1841, TNA BJ 3/11/196.
[5] Sabine, E., *Observations made at the magnetical and meteorological observatory at Toronto in Canada, Vol. III – 1846, 1847, 1848* (Longman, Brown, Green, and Longmans: London, 1857), 'Introduction', xviii, xix.

settle for rooms at Woolwich and as such in the spring of 1841 petitioned the Board of Ordnance for suitable space and personnel. Such a space would not be particularly difficult to construct, according to Sabine, but it had to fulfil a few different purposes. The reason it needed to be at or near Lefroy and Wilmot's Institution, Sabine explained in a letter to the Board, was twofold. Building new rooms would likely cost in excess of £300 and the Institution already had partially occupied rooms that could be converted to the cause for only £60 or £70. The list of interior fittings required was not long: two stoves, a large sharing table, a small drawing table, two desks for the superintendents, three desks for the clerks, five stools, six chairs, six supports for the instruments as well as six smaller ones, a range of shelves for the books and returns, a couple of partitions and two cupboards.[1] The magnetic department would also begin life with one 'press for the deposit and arrangement of Returns', later joined by a second.[2] Although the list of fittings and furniture was not long, the rooms were still in an unfinished state more than three months after the work had been requested. The delay to the fitting up of the instrument room was particularly inconvenient, wrote Riddell to the Royal Engineer Major Vicars. More instrument stands were wanting, portions of the floor still needed to be taken up so that the stands could sit detached from the floor, a door that swung outwards was to be fitted together with a green baize inside door and several smaller items, such as the fitting of locks on the drawers, the painting of walls and the provision of an office table were also not fulfilled by the time of Riddell's letter in October 1841.[3] Just as the Royal Engineers on St Helena could never 'be accused with anything approaching to a run', so it was at Woolwich.[4]

Sabine needed two rooms, both large and well-lit for the clerks to be able to write and draw up the publications, one to have a 'part partitioned off for the use of the superintendent and a second room of nearly the same size in which the instruments employed in the magnetic observatories and surveys [could] be set up for reference and instruction'.[5] The Royal Society had already purchased and placed at Sabine's disposal a set of these reference instruments, copies of the apparatus that had been sent to all of the colonial observatories. The space in which the instruments sat was constructed as a simulacrum or proxy observatory room, a fact testified by Riddell's instructions to the Royal Engineers to build stands detached from the floor at specific heights and at specific distances one from the other and to make sure there was an 'inside porch to the instrument room' with a 'green

[1] Sabine to Colonel Sir Hew Ross, 24 May 1841, TNA BJ 3/27/58-59.
[2] Sabine to Byham, 21 September 1843, TNA BJ 3/79/103.
[3] Riddell to Major Vicars, R. A., 15 October 1841, TNA BJ 3/79/17-18.
[4] Smythe to Sabine, 6 February 1844, TNA BJ 3/43/3.
[5] Sabine to Hew Ross, 24 May 1841, TNA BJ 3/27/58-59.

baize inside door', presumably, it can be inferred, to mitigate disturbing air currents.[1] These were all very similar instructions to those that had travelled with the officers in charge of constructing the colonial observatories. Woolwich after all, given the diminution of Dublin's role, had to serve as both a centre for the reduction and publication of returns and as a headquarter observatory.

Preparing results for the press involved the 'condensation of … materials into the smallest compass within which they can be brought consistently with useful development and lucid arrangement'.[2] This process of reduction was not only mathematical. It also required a kind of simulation of the practices and processes by which the colonial observers were collecting their results to fully understand, and often to diagnose the problems within, their results, by dint of proxy instruments within a proxy-observatory space. Woolwich was as much a centre of interpretation as it was a centre of calculation in this respect.

The geomagnetic data sent back were not givens or facts but products assembled in situ by an observer and separated from the circumstance of their creation in their transmission to Woolwich. At Woolwich, the processes involved in the extraction of the data and the data itself needed to be reassembled through re-association with the instruments of their making (through the proxy instruments) and the conditions of their observation (through correspondence) before their veracity could be established and their reduction begun, or their mendacity discovered and queries sent back to the original observer.[3] At Woolwich, the observations were not examined in isolation. They were 'judged' in conjunction with the interrogation of instruments and methods and 'their defects remedied' thereby.[4] The material output of a colonial observatory was not a wholly stable entity. Rather, the observatory returns needed continued evaluation, reduction and tabulation before they could be made durable and stable enough to be meaningfully mobilised within the scientific community.

While suitable accommodation for the magnetic department was simply, albeit slowly, located and fitted up, the appropriate employment of (and support for) personnel at Woolwich was a much more contentious issue. Sabine had originally requested one officer as an assistant and two non-commissioned officers to act as clerks. Sabine felt that the officers and the two clerks would be capable of performing 'the greater part if not the whole of the calculations' and that by this the 'expense of civil computers might be either

[1] Riddell to Vicars, 15 October 1841, TNA BJ/3/79/17-18.
[2] Sabine, E., *Observations made at the magnetical and meteorological observatory at Toronto in Canada, Vol. I – 1840, 1841, 1842* (Longman, Brown, Green, and Longmans: London, 1845), 16.
[3] Such queries were regularly forwarded to Younghusband during his temporary directorship of the Toronto Observatory. See for example TNA BJ 3/40/1a, 1b, 5 and 10.
[4] Sabine to Byham, 28 April 1845, TNA BJ 3/79/132-134.

wholly or in great measure avoided' but admitted that one or two additional clerks might be needed during the course of the work, especially as Sabine had been informed by Charles Edward Trevelyan, assistant secretary to the Treasury, in the spring of 1841, that it was proposed to include the Van Diemen's Land Observatory results in the publication of the magnetic scheme, despite this observatory being administered by the Admiralty. Sabine also informed the Board of Ordnance that the extra pay for a NCO acting as a clerk amounted to £22 16s 3d per annum.[1] The matter of extra pay became a sticking point for the Ordnance and one of a number of financial wrangles between the Board and Sabine, which ultimately centred on the position of the magnetic scheme within the Ordnance and of the officers employed in its service.

Sabine argued that it had always been standard practice, in both the Navy and the Army, to double the pay of officers employed in scientific service. The circumstance of 'the reduction and publication of such an enormous mass of materials as is daily accumulating by the incessant labours of four observatories' required 'the individual attention and the unremitting daily labour of several hours, of both the officers, who consequently cannot during its continuance, perform their ordinary military duty' and their efforts would barely be provided for without extra pay. However, the Board and the Master General of the Ordnance disagreed and instead proposed that it 'may be a matter for consideration, when the work is printed and given to the public, how far a reward in some shape should not be bestowed on the parties'.[2] Sabine was irked by this response, and offered up several reasons why the Board's decision ought to be revised.

At a personal level, Sabine pleaded that in taking on superintendence of the magnetic scheme's publication he had given up other opportunities of personal advantage and had also been removed from the command of a battalion at Woolwich, with the consequent loss of 3d a day. Extra money was further required to maintain an international system of observation and Britain's role as its leader through face-to-face communication with Lloyd in Dublin and other magneticians on the continent, things not covered by the usual regimental pay. As to Sabine's assistant officers, Sabine argued that they would have to work twice the number of hours that other Royal Artillery officers spend on 'extra duties' but with only a third of the leave. However, the argument that Sabine forwarded most strongly and the one to which he returned again and again in his letter to the Board was

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[1] Sabine to Colonel Fox, 30 April 1841, TNA BJ 3/27/49-50; Boase, G. C. (revised by Washbrook, D.), 'Trevelyan, Sir Charles Edward, first baronet (1807-1886)', *ODNB*, OUP, 2016, online edn, http://www.oxforddnb.com/view/article/27716?docPos=2 [accessed 6 November 2017].
[2] Byham to Sabine, 2 June 1841, TNA BJ 3/27/63-64.

that under the arrangements the Board had proposed there would be a need to employ a civil computer in addition to the military clerks.[1]

Sabine had recently received observations from the Admiralty portion of the magnetic scheme: some from Ross's Antarctic voyage and some from the Van Diemen's Land Observatory. The labour of reduction involved with the printing of these observations would, Sabine alleged, 'be equivalent to the returns from four such observatories as those of the Ordnance'. Sabine had envisioned that this work 'should be performed, on the most economical mode, by military clerks, instructed and closely superintended by my assistant. But,' he wrote, 'as it cannot be expected that this officer should undertake so laborious a duty & be responsible for the accuracy of the work, for another department, without any advantage whatsoever to himself, it must be done by civil computers.' Sabine further explained that the number of regular magnetic observations recorded at and returned from the colonial observatories exclusive of the miscellaneous observations and the meteorological observations – which were almost as numerous as the magnetic data – was about 3,900 per month. For the complete reduction of the magnetic returns, Sabine helpfully explained, 'four corrections are requisite, to be computed and applied to each observation. If the corrections are applied only to the mean quantities…there will still remain some thousand calculations in each month' requiring at least two civil computers, as well as clerks for drawing the curves, working under Sabine's immediate direction. Without the additional support of civil computers Sabine feared that reduction and calculation would fall into arrears.[2]

The threat of the employment of civil computers at Woolwich worked as Sabine had likely intended it to. The secretary of the Treasury wrote to the secretary of the Ordnance in July 1841 to explain that Sabine should be granted an extra 3d per day as recompense for the loss of his battalion pay and furthermore that any travel expenses of officers junior to Sabine occasioned by the magnetic scheme would be met by the Treasury. It was the 'wish' of the Lords of the Treasury that 'the employment of Civil computer[s] should be altogether avoided' and so they had moved to placate Sabine and maintain a strict military administration. However, it was repeated that Sabine's assistants would not receive consideration for extra allowance until the publication had been drawn up and distributed.[3]

Other disputes between Sabine and the Board of Ordnance occasionally arose. One concerned the issue of Sabine's lodgings. At the beginning of the publication of the returns, Sabine was not quartered at the Repository as a field-officer as he thought he ought

[1] Sabine, confidential letter [to the Board of Ordnance] 7 June 1841, TNA BJ 3/27/65-68.
[2] Sabine, confidential letter [to the Board of Ordnance] 7 June 1841, TNA BJ 3/27/65-68.
[3] Trevelyan to Secretary of the Ordnance, 12 July 1841, TNA BJ 3/27/69-70.

to be but rather at the Arsenal, where he was not entitled to a 'pecuniary allowance' and instead provided with 'coals and candles and forage for one horse' together with 'barrack stores' at his office.[1] Sabine was not content with this situation. He had designs on some vacant Hospital rooms on Woolwich Common – the quarters of a late Colonel Jones – situated near to his office and applied to a Lord Bloomfield for them. Bloomfield was not initially forthcoming with a response but eventually communicated to Sabine that he would not be allocated these rooms and that they had instead been given to an officer junior to Sabine because Bloomfield wanted to retain the quarters for officers on garrison duty. Bloomfield had thereby not given Sabine his right to the choice of quarters based on seniority, which, according to Sabine, went against Her Majesty's regulations. Ever the administrator, Sabine especially urged his claim to be quartered on the Common rather than the Arsenal because on the Common he could 'more efficiently and with far more dispatch perform the duties entrusted to [him]'.[2] Sabine was not, it would appear, successful in his bid for quarters on the Common. However, he was able to procure for himself compensation at the rate of £120 per annum together with the attendance of soldiers as servants, in lieu of quarters.[3]

At almost the same time of year, an incident occurred which was far more unpalatable to Sabine. According to Sabine, at the 'General Orders of the Regiment of the 9th August' [1842] it was stated that he, Sabine, was 'seconded as holding a civil appointment'. Writing to the Deputy Adjutant General of the Royal Artillery, at this time Sir Hew Dalrymple Ross, Sabine iterated that he was decidedly not employed on a *civil* appointment but was instead charged with the superintendence of the Ordnance magnetic observatories by, and reporting directly to, the Master General. It would 'ill become me' Sabine explained,

> either on my own account or on that of the Officers employed with me on this duty, to be indifferent to the terms in which our employment is characterised in the General orders and returns of the Regiment, or to be insensible to the real distinction between a public service performing by the Officers and Soldiers of the Royal Artillery under the immediate orders of the Master General, and a Civil appointment in the ordinary meaning and acceptation of the term.

Sabine then outlined four reasons why the magnetic department under his direction was an official part of the Royal Artillery. These ranged from a reiteration of the fact that Sabine was acting under the command of the Master General, to the original orders of the Deputy

[1] H Ross to Sabine, 13 October 1841, TNA BJ 3/27/80 and 81.
[2] Sabine to H Ross, 24 December 1841, TNA BJ 3/27/88.
[3] Byham to Sabine, 9 November 1842, TNA BJ 3/27/116.

Adjutant General Sir Alexander Dickson that the magnetic service was one which the 'Regiment of Artillery were called upon to perform' under Sabine as a principal Staff Officer of the Corps, to the strict regimental order and hierarchy which existed at both the magnetic department at Woolwich and the Ordnance observatories in the colonies. If it pleased Ross, Sabine requested that the Master General be asked to continue to refer to both Sabine and all of the other officers on the magnetic service as '"employed on the service of the Ordnance Magnetic Observatories" in the same manner that the Officers of the Royal Engineers employed on the analogous service performed by that Corps are designated as employed on the Trigonometrical Survey'.[1] Both of these incidents – of Sabine's lodging and the correct designation of his service – revolved around recognition of the magnetic scheme and Sabine's magnetic department as the legitimate business of the Ordnance.

This sort of politicking between Sabine, the magnetic scheme and the Board of Ordnance was played out at Woolwich but it was also played out at the colonial observatories. On St Helena, Lefroy had to battle with the island's Governor, the Respective Officers (made up of the Ordnance Storekeeper, the Commanding Royal Engineer and Commanding Royal Artillery Officer) on the island and the Board of Ordnance in England simply for the grant of forage, i.e. the grant of a horse to use on the island.[2] All other officers on the island had been granted forage for one horse, but Lefroy was forced to petition the Governor, then the Respective Officers and then, through Sabine, to run the request through the Board of Ordnance and ultimately the Treasury. Lefroy had started the process of an application at the beginning of 1840, soon after arriving on the island and recognising the isolation of his observatory site from Jamestown – the only town on the island – and the 'hilly and fatiguing character of the roads all over the island' over which he often had to carry his instruments.[3] Despite the reasonable nature of the application it was not until January 1842, two years later, that Lefroy was eventually granted forage, by which time he had been informed of his relocation to Toronto anyway.

At each of the Ordnance observatories there were also regular difficulties and delays caused by a perceived lack of authority on the part of the observatory directors when dealing with the Ordnance Storekeeper and the money to be defrayed for observatory requirements, such as the replacement of instruments and the extra pay of observatory assistants. For example, on taking up his new position as Director of the Toronto

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[1] Sabine to Hew Ross, 18 August 1842, TNA BJ 3/79/77-78.
[2] Explanation of the make-up of the Respective Officers found in Lefroy, *Autobiography*, 26; the dispute over forage is remarked on in several letters in TNA BJ 3/81, but particularly 7, 8, 9, 35 and 37.
[3] Lefroy to Sabine, 12 February 1840, TNA BJ 3/81/7.

Observatory in late 1842, Lefroy found that the Ordnance Storekeeper had no authority, and could not receive any from Lefroy, to make certain changes at the observatory that had already been authorised by the Board of Ordnance relative to the extra pay of an additional NCO out of the contingent expenses. Lefroy was required to write to Sabine and request that Sabine grant the Storekeeper the authority. Lefroy, despite being director of the observatory, was not recognised as having the requisite authority to make certain changes there where money was involved.[1] The position of Lefroy and the other officers at the colonial sites was, in the early stages of the scheme at least, precarious, something which the slow bureaucracy of the Ordnance only exacerbated. Sabine's attempt to establish Woolwich as his centre of calculation, properly financed, staffed and supported, is further evidence of the difficulty of working within the bureaucracy of the Ordnance and of marketing the magnetic scheme as a legitimate pursuit of military personnel. It would be reductive to simply think of Sabine's wrangling with the Board of Ordnance as further evidence of his careerism and opportunism. Sabine did of course benefit from his role in the scheme and his petitioning of the Board for better lodgings, but these incidents also helped to carve out a niche for the magnetic department at home and abroad and situated the department effectively within the strictures of a military administration.

The publications and afterlives of magnetic data

In the 1840s and 1850s, the staff at the Woolwich magnetic department worked fastidiously to make the data of the crusade commensurable and valuable. In more recent years, geophysical scientists have sought to unravel and circumvent the construction of these data by delving into archives of manuscript returns. Research paradigms have shifted since the nineteenth century and geomagnetic physicists today have different uses for the data accrued by Sabine's magnetic department. The reductions that Sabine and his assistants made and the omissions of certain data from publication, are now sometimes viewed as having obscured the record. Physicists working on long-term models of the earth's magnetic field now desire more individuality in their data; they want individual ships' log-books and they want to reinstate missing data.

Herschel was correct when he argued that the full import of the record of the British magnetic scheme could not be extracted from one theorist or in a single generation, although he would certainly have hoped to have been proven wrong. It is fairly common

[1] Lefroy to Sabine, 25 October 1842, TNA BJ 3/27/117.

knowledge, or at least it is commonly remarked upon in histories of the magnetic crusade, that the scheme, voluminous and simultaneous and well-financed though it was, did not deliver the all-encompassing physical theory capable of explaining and predicting the variance of the earth's magnetic force. The discovery of such a theory had been the most important motivation guiding the establishment of the scheme, even if publicly the benefits to navigation were trumpeted more loudly. However, in the end, very few of the 'predictions concerning the practical and theoretical potential of the scheme were borne out'. One of these, Cawood reports, was the revelation in 1851 of a 'correlation between the sunspot cycle and the periodicity of magnetic storms' and, later in the same year, Sabine also discovered 'that the daily variation of magnetic intensity consisted of two superimposed variations, one deriving from within and one external to the earth'.[1] When Lefroy talked bitterly of his North American survey as so much wasted labour, having done little more than swell the volumes of data then accumulating without the requisite application of analysis, he could really have been – and possibly he was – commenting on the magnetic scheme as a whole. As Ratcliff has shown, this kind of mountainous compilation of data without subsequent analysis was not specific to the magnetic crusade but 'typical' of other 'large research programmes in the nineteenth century'.[2]

It was not known in the 1840s that a definitive physical theory was not going to be discovered and so Sabine and his magnetic department worked to produce and distribute a great many copies of their magnetic publications. 350 copies of the initial results of the Toronto Magnetic Observatory were to be published according to a letter from the Treasury to the Board of Ordnance in 1841, with plates for an additional 150 copies to also be prepared and bound 'as they may be required'.[3] Sabine wrote a list, undated but likely to have been drawn up in 1841 at around the same time as the Treasury's letter, of the number of copies proposed to be sent to various institutions and individuals. For the 51 'magnetic observatories engaged either wholly or partially in performing the same operations' an average of three copies per observatory were required, making a total of 153 books; for 'individuals active cooperating', another 62; for 'public libraries and institutions', a further 78; and, finally, 25 copies to be completed 'for sale to meet immediate demand'.[4] This made an overall total of 318 copies to be published and distributed. By the time of the completion of the volume of magnetic disturbances in 1843 – the first to be produced by

[1] Cawood, J., 'The magnetic crusade: science and politics in early Victorian Britain', *Isis* 70, 4 (1979), 492-518, 516.
[2] Ratcliff, J., *The transit of Venus*, 24.
[3] Trevelyan to Board of Ordnance, 19 May 1841, TNA BJ 3/27/54-55.
[4] Sabine, '<u>List</u> showing what number of copies is proposed to be presented to each Institution and Individual and what number it is proposed to reserve for sale', TNA BJ 3/27/25-26.

the magnetic department – Sabine had written to Byham, secretary of the Ordnance, to inform him that he had more names to be added to the list of 318 recipients, but that at least 250 copies were to be sent immediately to those named on the original list.[1]

The list of observatories included the colonial and EIC observatories, Dublin, Greenwich, Göttingen and many more in Europe, Asia and the Americas such as Naples, Trevandrum, and Philadelphia. The list of 'distinguished individuals' was comprised of almost equal numbers of British and foreign-based parties, from Christie, Beaufort and Snow Harris to Quetelet, Oersted and Biot. The public libraries and institutions that were chosen as candidates to receive copies ranged from the Royal Society, to the University libraries of Oxford, Dublin and Glasgow and overseas to the societies and academies of science of places such as Norway, Berlin, and Padua, among many others.[2] Whether or not these places and people received all, some or any of the various publications of Sabine's magnetic department is unknown. As a rough indication that the list Sabine produced perhaps exaggerated the final number of places that received every publication, the University of Glasgow Special Collections library holds only one of the magnetic department's outputs, *Observations of days of unusual magnetic disturbance* (1843), which was the first published output of the magnetic department at Woolwich.

Sabine's magnetic department produced three volumes of results from the Van Diemen's Land Observatory, three from Toronto, two from St Helena, and one from the Cape of Good Hope.[3] Between 1842 and 1845 this department also reduced and prepared publications of three volumes of Antarctic expedition results, the *Observations of days of unusual magnetic disturbance* mentioned above, Riddell's *Magnetical instructions for the use of portable instruments* (1844) and the first Toronto volume.[4] These were all substantial publications. As an example, the third volume of the Toronto results printed in 1857 comprised of 455 pages of magnetic and meteorological observations made between 1846 and 1848, as well as 126 pages on the adjustments of instruments at the observatory, the formation of abstracts and other comments on the results and/or the instruments. Four plates were also included in this publication, which illustrated the mean effects of larger disturbances over time, the daily variation of the different magnetic elements corresponding to the lunar cycle, and annual and semi-annual means of the variation of declination, inclination and intensity at Toronto. This was a fairly standard publication of

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[1] Sabine to Byham, 20 September 1843, TNA BJ 3/79/102.
[2] Sabine, 'List', TNA BJ 3/27/25-26.
[3] Geomagnetic Yearbooks and Historical Articles, *British Geological Survey*, http://geomag.bgs.ac.uk/data_service/data/yearbooks/yearbooks.html [accessed 19 February 2018].
[4] Sabine, 'Observations reduced and publications prepared at the Superintendent's Office at Woolwich from 1842 to the spring of 1845', TNA BJ 3/79/134.

the results of a colonial observatory. For comparison, the *Observations of days of unusual magnetic disturbance* was only around 140 pages. As well as these volumes of results, Sabine also produced 15 articles entitled 'Contributions to Terrestrial Magnetism' for the *Philosophical Transactions of the Royal Society*, the first published in 1840 and the last in 1877. These varied in length and substance and touched on several aspects of British magnetic research, from Lefroy's North American and Ross's South Polar surveys to the reduction of domestic and foreign observations to common epochs and common regions.

As previously touched upon, the collation of many of these data over the years enabled Sabine in 1852 to posit a correlation between geomagnetism and the sunspot cycle, specifically that the mean monthly range of magnetic variations rose and fell in a ten-year cycle, which coincided with Schwabe's earlier discovery of a ten-year sunspot cycle. As Macdonald has explained, Johann Lamont in Munich had noticed a similar geomagnetic pattern earlier than Sabine, but it was Sabine who was able to connect it to the sunspot cycle, because Schwabe's discovery had been referenced in Humboldt's *Cosmos* (1845), which Sabine's wife Elizabeth Leeves translated between 1849 and 1858.[1] It was in response to Sabine's discovery that John Welsh, superintendent of the Kew Observatory in 1852, pushed for the establishment of a photographic sunspot observing effort, a project that was latterly realised.[2] It was only through passing the results of the observatories through the distinct processes mentioned above that Sabine and his magnetic department were able to separate the regular and the irregular returns, and it was 'by means of the disturbance-variations so determined, that the coincidence between the phenomena of the solar spots and the magnitude and frequency of magnetic disturbances was first perceived and announced' by Sabine.[3]

Beyond Sabine's application of magnetic crusade data to prove the geomagnetic-sunspot correlation, nothing truly substantial was derived from the outputs of the magnetic department during the nineteenth century. For instance, Sabine's latter 'Contributions' of the 1870s mainly reorganised and reduced datasets to common epochs and regions, but did not offer much in the way of analysis. However, the usable life of magnetic crusade datasets is far longer than that of the instruments that recorded them or indeed the lives of

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[1] Macdonald, L. T.., '"Solar Spot Mania": the origins and early years of solar research at Kew Observatory, 1852-1860', *J. Hist. of Astron.* 46, 4 (2015), 469-490, 474-475; on Elizabeth Leeves, see Good, G. A., 'Sabine, Sir Edward (1788-1883), army officer and physicist', *ODNB*, OUP, 2004, online edn, http://www.oxforddnb.com/view/10.1093/ref:odnb/9780198614128.001.0001/odnb-9780198614128-e-24436?rskey=pQ34J3&result=1 [accessed 8 December 2017].
[2] Macdonald, '"Solar Spot Mania", 476-477.
[3] Sabine, E., 'On what the colonial magnetic observatories have accomplished', 400; for Sabine's announcement of the correlation between geomagnetism and solar spots, see Sabine, E., 'On periodical laws discoverable in the mean effects of the larger magnetic disturbance', *Phil. Trans. R. Soc. Lond.* 142 (1852), 103-124.

the people like Lefroy who gathered them. For data, geomagnetic and other, death (through disuse) is not final: 'resurrections can occur', as long as some material trace of the data survives.[1]

There have been several attempts, recently and in the 1960s and 1980s, to collect together into compendia the extraordinary magnetic datasets that survive in even the earliest books by Stevin (1599), Kircher (1643) and Wright (1657) and later, larger, texts such as those of Mountaine and Dobson (1757), Hansteen's *Magnetismus der Erde* (1819), and Sabine's 'Contributions', particularly those of 1868, 1872, 1875 and 1877.[2] These compendia of historical data provide the basis for producing models of the geomagnetic field at past epochs, which can be used 'in attempts to study the evolution of certain core processes'.[3] Such historical data are generally regarded as reliable but with certain caveats attached about the 'cruder' instruments by which they were recorded. One of the most fundamental difficulties in using historical magnetic data is not the instrumentation but the lack of observations of the total intensity of the magnetic field, as 'until about 1850, the available data were restricted almost entirely to observations of the direction of the field'.[4] Sabine's 'Contributions' were some of the earliest collections of data that bucked this trend, constituting over 10,000 observations of declination, inclination but also, importantly, total intensity. An emphasis on the collection of accurate absolute magnetic observations was an important aspect of Sabine and Lloyd's magnetic scheme, as evidenced by the effort that was put into creating spaces where this could happen – such as the underground room at St Helena and Wilmot's cedar barrel at the Cape. The compendia that have been created from all of these historical datasets are substantial and quite extraordinary in their range – chronologically as well as geographically – and formulation of historical geomagnetic models.[5] Still, two particular problems remain.

For one, 'existing compilations have barely scratched the surface of the number of surviving original sources' despite considerable archival work on the part of scientists and scholars such as Jonkers, Jackson and Murray; and, two, printed versions of data – Sabine's 'Contributions', for instance – are 'sometimes incomplete, and values … truncated, rounded, or reduced to grid'.[6] The latter problem also applies to certain

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[1] Leonelli, S., 'Journeys and deaths of scientific data', slidepack, https://www.datastudies.eu/images/downloads/2016/4S-EASST2016-Leonelli.pdf [accessed 17 October 2017].
[2] Barraclough, D. R., 'Historical observations of the geomagnetic field', *Phil. Trans. R. Soc. Lond. A* 306 (1982), 71-78, 72.
[3] Barraclough, 'Historical observations', 71.
[4] Barraclough, 'Historical observations', 71.
[5] Give a list?
[6] Jonkers, A. R. T., Jackson, A., and Murray, A., 'Four centuries of geomagnetic data from historical records', *Review of Geophysics* 41, 2 (2003), 1-35, 2.

compendiums of historical data, as we will see later. Sabine's compilations of data from 1820 to 1870 may be comprehensive but they are incomplete in that 'data were omitted by Sabine when forming his compilation from the original sources'.[1] Sabine organised his 'world compilations' by region rather than by voyage or survey and only for the zones 40-85˚N, 0-40˚N and 0-40˚S, data from greater than 40˚s having been omitted (except for Ross's South Polar observations, made between 1840 and 1845).

Lefroy had had similar qualms with the completeness of the record of the North American Magnetic and Meteorological Survey, 1843-44, as reported by Sabine in 'Contributions VII' (1846). The later publication by Lefroy and Richardson of *Magnetical and Meteorological Observations at Lake Athabasca and Fort Simpson and at Fort Confidence in Great Bear Lake* (1855) and by Lefroy of his *Diary of a Magnetic Survey* (1883) were both in part motivated by the desire to provide all of the recorded observations, including those omitted by Sabine in his 'Contributions'. Lefroy wrote in his diary that although he thought of Sabine as the 'pioneer of the modern science of terrestrial magnetism in Britain' and praised Sabine's 'Contributions' for their substantial scope, he was concerned that 'features of merely local interest were apt to disappear' – such as those produced by local disturbance and irregularity due to the geology of a station – if the only record of the survey was that produced by Sabine. Sabine had apparently only included four of Lefroy's stations which exhibited local irregularities and disposed of the rest. However, this, according to Lefroy, was 'doing less than justice to a very interesting feature presented in all extensive magnetic surveys and one which, when the subject of earth currents comes to be better understood, will probably prove to possess considerable importance'.[2] Lefroy was not far from the mark in his supposition here as for those, beginning in the 2000s, who wanted to create more detailed models of the historical movement of the earth's magnetic field, new datasets based on Sabine's datasets had to be created, in which 'missing data' from individual voyages could be 'reinstate[d]' from sources such as Lefroy's *Diary* and original manuscripts.[3]

In creating later collections of geomagnetic observations, a certain amount of further truncation and reduction of the data was necessary, something which has not always been successfully managed. For example, Barraclough has written that the summary of data provided in the catalogue of Veinberg and Shibaev (1969), one of the most comprehensive collections of historical magnetic data, 'leaves much to be desired'. Barraclough notes a

[1] Jackson, A., Jonkers, A. R. T., and Walker, M. R., 'Four centuries of geomagnetic secular variation from historical records', *Phil. Trans. R. Soc. Lond. A* 358 (2000), 957-990, 961.
[2] Lefroy, J. H., *Diary of a magnetic survey of a portion of the dominion of Canada chiefly in the North-Western Territories, executed in the years 1842-1844* (London: Green and Co., 1883), xvi.
[3] Jackson et al, 'Four centuries of geomagnetic secular variation', 961.

number of inaccuracies in his assessment of this work and also remarks that 'the concept of reducing the original observations to relatively widely spaced points and epochs' such as Veinberg and Shibaev did, 'has inherent defects'. 'Since the corrections employed in the reductions can only be known approximately', Barraclough continues, 'the procedure further degrades the already rather low quality of the data'.[1]

Data then, of the magnetic crusade and indeed of a great many other expeditionary voyages of the sixteenth to the nineteenth centuries, have survived and been made available through several different research collections. However, this portability and this durability has its cost. As Barraclough above notes, the procedures involved in maintaining and continually reducing data has degraded its value. Now, in the twenty-first century, efforts have been made to access more archival collections, to seek out the original manuscript sources that informed publications such as Sabine's 'Contributions' and through these sources to reinstate missing data and recover individual, unprocessed, observations which have not been forced into and through different ontological categories demarcated by different researchers.[2] For example, Jackson et al, in their attempt to build on earlier historical magnetic work by Jackson (1985) and Bloxham (1989) investigated collections of French naval and hydrographic service ships' log-book data held at the Archives Nationales and the Bibliotheque National in Paris.[3] The Ottawa Geomagnetic Observatory is also shortly to make available online scans of the register books of the original Toronto Observatory so that historians and physicists alike can learn from its contents. These projects and researchers share Lefroy's much earlier belief in the need to recapture local data and the marks and traces originally put down by those like Lefroy and Wilmot and Kay. Through this, researchers are able to quantify the errors of historical magnetic observations and develop models of, for instance, 'the flow at the top of the core'.[4] Stripping data back to its local configuration through archival investigations confers a legitimacy and authenticity to research in the movement of the earth's magnetic field over time that research based solely on the reams of printed and reduced data may not.[5]

Herschel wrote that the record of such an important project as the British magnetic scheme could not be left to the precarity of manuscript. Publication and proliferation of copies of the results was an absolute necessity in Herschel's and many others' minds. To do this meant reducing and condensing observations to fit different categories of results – irregular, regular, daily, monthly, yearly, by area, by time and by temperature – a process

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[1] Barraclough, 'Historical observations', 77.
[2] On this point, see Gitelman, L., (ed.), *Raw data is an oxymoron* (MIT Press: Massachusetts, 2013).
[3] Jackson et al, 'Four centuries of geomagnetic secular variation'.
[4] Jonkers et al, 'Four centuries of geomagnetic data', 2.
[5] Leonelli, 'On the locality of data', 740-741.

which solidified and made durable the geomagnetic traces recorded by observatories and surveys and expeditionary voyages. Packaging up data in this way allowed the results of the British magnetic scheme to travel widely and be ‘consumable by the scientific gaze’ both at the time and posthumously.[1] As was mentioned above, the processes employed at Woolwich ultimately turned local marks and traces into nonlocal data. However, intriguingly, it appears that in the twenty-first century geomagnetic physicists increasingly want to strip back this nonlocal data, to discover its origin and reinstate its localness in order to reprocess the information to fit the categories needed to construct long-term computer models of the earth’s magnetic field’s movement. Rather than try to reverse engineer data, especially as the steps by which observations were reduced were often unclear, geomagnetic physicists (historical geomagnetic physicists) are turning to the archive, provided of course that the data still exist there in manuscript. The packaging of data and the need of researchers today to transform the magnetic data they use from nonlocal to local provides further evidence for Leonelli’s point that data, despite their epistemic value and their etymology as ‘given’ are ‘clearly made’ through ‘complex processes of interaction between researchers and the world’ and the ‘rescaling and manipulation of objects of inquiry for the purposes of making them amenable to investigation’.[2] Manuscripts were indeed a fragile material for the record of the magnetic crusade, as Herschel pointed out, but the unmediated pen and pencil marks they contain have a kind of scientific durability precisely because they have not undergone the multiple mediatory steps or packaging processes required to publish results at the Woolwich magnetic department.

Conclusion

This chapter has provided a biography of data by charting what Leonelli calls ‘data journeys’.[3] The chapter has attempted to follow data as it was mobilised at and from the colonial observatories to Sabine’s magnetic department at Woolwich and from there into print and forward in time to near-present day. We observed how physicists who use historical geomagnetic data have attempted to circumvent the procedures introduced at Woolwich by going back to the archive and looking for those original traces of observations. This chapter has tried to engage with several recent philosophies and

[1] Nasim, ‘Extending the gaze’, 251.
[2] Leonelli, ‘What counts’, 813.
[3] Leonelli, ‘Journeys and deaths of scientific data’.

histories of data to take seriously data as an historical and, crucially, material artefact of the scientific process capable of being followed and interrogated alongside the scientific instrumentation that helped create them. Using data in this way is conspicuous by its absence in the literature. The circulation, transfer and adaptation of data should not only be a philosophical or STS concern but a geographical one too, and arguably could be included in future forays into the 'mobilities of knowledge'.[1] Making traces into data and data into 'patterning facts', as Rheinberger terms it, has to take place *somewhere* and it is important to explore these somewheres – such as Woolwich – as sites of the production of scientific knowledge because, at least in this example, it was in this place that observations – data – were constructed through myriad processes and within various administrative and bureaucratic contexts. The data that made their way through the printing press were not 'givens', they were made, despite what their etymology might contend.

Daston has written that 'observation is everywhere and nowhere in the history and philosophy of science'. Observation is 'ubiquitous' and is an 'essential scientific practice in all the empirical sciences, both natural and human' but it is so often 'invisible because it is generally conceived to be so basic as to merit no particular historical or philosophical attention'.[2] However, as Daston argues,

> scientific perception – especially when elevated to the level of systematic observation, often in carefully designed setups – is disciplined in every sense of the word: instilled by education and practice, checked and cross-checked both by other observers and with other instruments, communicated in forms – text, image, table – designed by and for a scientific collective over decades and sometimes centuries.[3]

Following the data illuminates such 'carefully designed setups' and it allows us to view the systems and procedures and the tools used to both check and cross-check observations and take data in one form, stabilise it, and deliver it in another form; what we might call data handling and data packaging. Examples of such a methodological practice are beginning to materialise, though not yet in histories of the physical sciences. Adopting this kind of methodology opens up both exciting new avenues of exploration and potentially important new archives for historians and geographers of science to consider.

Finally, many histories of the survey sciences have paid critical attention to the hardware of surveys, and the use of instruments and note-taking devices as they were mobilised in the field. However, at least in the case study presented here, both of these items existed and

[1] Jöns, H., Meusberger, P., and Heffernan, M. (eds), *Mobilities of Knowledge* (Springer International Publishing, Online, 2017).
[2] Daston, L., 'On scientific observation', Isis 99, 1 (2008), 97-110, 97.
[3] Daston, 'On scientific observation', 102.

were used beyond the field again, at the centre of calculation *and* interpretation that was Sabine's magnetic department. The instruments may not have been those that travelled in the field and only acted as a reference of fieldwork practices, but it is important to consider the instruments' ongoing involvement in the creation and transformation of traces and marks from the field into data 'publishable and consumable by the scientific gaze'.[1] Likewise, the pencil, pen and paper were not only used to record observations in the field but, as part of the 'procedure' and 'paper-based technologies' of the magnetic department, managed and mobilised the field anew. As has been stressed here and elsewhere, we ought not to be misled by the etymological roots of data: data are not givens but constructs, made not in the field but in the spaces between the field and the publication of results. This chapter has attempted to emphasise not only how this process occurred as part of the British magnetic scheme's global surveying efforts, but where, and within what system, this process happened.

[1] Nasim, 'Extending the gaze', 251.

Chapter 8: Conclusion: historical geographies of the magnetic crusade

Summaries

The first chapter considered the five years leading up to 1839 through a focus on the British Magnetic Survey (BMS), undertaken by Lloyd, Sabine, Ross, Phillips, and Fox. The BMS does not figure in accounts of the magnetic crusade, at least not to any meaningful extent. This chapter therefore outlined the BMS – its course, its participants, the instruments used – and scrutinised three sites that acted as spaces for the experimentation and development of magnetic instrumentation: Regent's Park and Westbourne Green in London and the gardens of Trinity College, Dublin. In both this chapter and the second chapter, instruments were followed as they were mobilised in and through different spots for the purpose of terrestrial magnetic observation. The second chapter, which focused on Lefroy's North American Magnetic Survey, considered the passage of instruments over a much greater distance than occurred during the BMS, while both surveys were used to illuminate some of the difficulties encountered in making geomagnetic observations on the move and how observers managed their instrumentation in such situations. In Lefroy's survey, we also saw how important the French-Canadian voyageurs and indigenous crewmen were to the safe passage of magnetic instruments during an often-precarious journey that involved multiple forms of transportation. However careful they were, accidents occurred – often only when instruments were set up for observation and not in transit – but it is in these instances that it is possible to glimpse other members of the previously invisible labour that maintained Lefroy's survey: the network of Hudson's Bay Company fort armourers that reassembled and revivified several of Lefroy's precision instruments and magnetic needles that had been dropped, blown over or even run over in the course of the survey.

Next, the thesis moved on to consider the establishment of the colonial observatories in Toronto, the Cape of Good Hope, St Helena and Hobarton, Van Diemen's Land, as well the East India Company observatory-that-never-was in Aden. It was argued that the historiography pertaining to the magnetic crusade specifically and the observatory sciences more broadly had not closely considered what it meant to establish an observatory in the early nineteenth century. To address this gap, the experience of scientific servicemen –

officers of the Royal Artillery and Royal Navy – in constructing and latterly inhabiting and managing their observatories was explored in close detail. Making their observatories function properly was a continuous process at all the observatory sites, as exemplified by the multitude of unforeseen structural additions to the observatory – barrels for dip houses, underground rooms and earthen insulation – and the many changes to the state of, and instructions for, the instruments at the observatory. One particularly significant aspect of this chapter was a consideration of the complex relationship between scientific pedagogy, geographies of magnetic instrument travel, and local observatory management as seen through the experiences of Wilmot, Kay, Smythe, Riddell and Lefroy. The starkest portrayal of the precarity of this relationship was captured in the story of the Aden debacle, but each observatory directors experience spoke to this complex relationship to some degree.

Finally, the thesis closed with an extended look at both the enormous accumulation of data the magnetic crusade entailed, as well as the military administration through which this took place. There was a particular focus on the machinations involved in the establishment of Sabine’s magnetic department at Woolwich and, latterly, the tools and processes used at this department to manage the manuscript returns received from overseas observatories and surveys and transform them into publishable material. The British magnetic scheme broadly took place in the same period as, and exemplified, what Hacking has described as an avalanche of printed numbers. One part of the contribution of this final chapter was an examination of how this avalanche was created in a military-scientific institution such as Sabine’s magnetic department. This chapter followed the printed numbers through time to explore how geomagnetic data gathered as part of the British scheme are utilised by geomagnetic scientists today and what this can tell us about science in the archives and the life, death, and reuse of data. What follows next is an outline of the conclusions reached by the amalgamation of all the historical strands summarised above before a more detailed look at each conclusion and its significance.

The conclusion will now move on to consider some of the most significant, collective, outcomes of the chapters presented above.

Conclusions

This thesis has added to three particular aspects of the history and historical geography of Victorian science. First, this thesis has explored the life-geographies or geobiographies of several magnetic instruments – and sometimes meteorological instruments too – as they were mobilised as part of the magnetic crusade. Magnetic instruments have been considered in their moments of use at the various colonial observatories and have been followed through moments of transition and carriage – to the observatories and as part of Lefroy's survey especially – and the consequences of these mobilisations have been explored. Ironically, one of the consequences of the mobilisation of instruments was the immobilisation of knowledge about and knowledge contained within the instrument, something commented upon in the colonial observatories chapter and concluded below. A focus on instruments in transit has brought together historical geographies of scientific pedagogy, histories of the often-invisible labour involved in surveying at this time, histories of repair and maintenance, and histories of observation and experimentation in the observatory. In the chapter devoted to the British Magnetic Survey, magnetic instruments were explored as they were mobilised, tested and developed in various spatial contexts – an important but previously unexplored aspect of these instruments' journeys. Later, at Sabine's magnetic department at Woolwich, it was also shown that the use of an instrument did not finish at the observatory once an inscription had been made, because it was at Woolwich that proxy instruments were set up to assist Sabine and his staff in their reduction of the original observatory returns.

Second, this thesis has considered the experience of those scientific servicemen who participated in the organisation and execution of the magnetic crusade as well as the military institutions that were crucial to the scheme's administration. Scientific servicemen and the significance of military institutions to early Victorian science are represented by only a small portion of the historiography of the magnetic scheme and more widely that of nineteenth-century science. In seeking to address this lacuna, this thesis has drawn upon the correspondence of each of the colonial observatory directors as well as their later diaries and memoirs where these exist. However, the thesis has also arrived at an understanding of the military's involvement in the administration of the scheme by shaping, foregrounding and using data as a historical artefact. This is because, by following the data, we are led to consider Sabine's magnetic department at Woolwich, a department that sat within and was staffed by military personnel, but which had to fight to be recognised as a legitimate part of the military establishment.

Third, and related to the above, this thesis represents a conspicuously geographic approach to the history of terrestrial magnetic research in Britain and in its colonies during the first half of the nineteenth century. An explicit geography of magnetic research reveals the multitude of places, spaces and scales in and across which this science operated and by which it was shaped. Much of what has been written about two of the most important spaces of magnetic science – the magnetic observatory and the field – has focused not on the space itself but on what the institute of the observatory or the course of a survey meant to a wider community and to the magnetic crusade network. For example, much of the emphasis of Savour's and McConnell's exploration of the Rossbank Observatory on Van Diemen's Land was on how the observatory sat within the local colonial community and how important this site was to the magnetic scheme both as a whole and specifically to Ross's Antarctic voyage. While these authors provide a section devoted to the instruments used in the observatory, this is essentially a description of the technical detail of the instruments and their schedule of observation and says little about the experience of managing the space of the observatory or the instruments within it. Similarly, the Toronto Observatory and Lefroy's North American Magnetic Survey, two aspects of the magnetic scheme that have received concerted attention, are often written about for what these two things say about the importance of science and scientific institutions in the creation or delineation of a nascent national scientific community in Canada. These are of course legitimate and much needed histories. However, what has been missing is a much closer scrutiny of these scientific sites and the practices that took place within them as well as the instruments that inhabited them.

This conclusion will now move on to consider these three points in greater detail, through an exploration of the contribution of this thesis to historical geographies of scientific instruments and to the recovery of the experience of scientific servicemen as they were employed on a conspicuously big scientific project.

(Im)mobilities and the experience of scientific servicemen

Much of what has been written about the history of the British magnetic scheme has treated the establishment of observatories as an uncomplicated event and by so doing marginalised the experience of scientific servicemen like Lefroy, Wilmot, Kay, Riddell and others. As I have argued, the process of establishing a physical observatory abroad did not begin abroad but at places like Woolwich and Dublin, the places in which observatory instruments were first encountered and the knowledge of their use and the materiality of their construction examined, packaged up and transported. As Yule, in Aden, put it, these instruments formed the essence of the magnetic observatory: the devices that reckoned the site of the observatory and decided the angles and measurements of its construction. To understand the creation and management of an observatory we must consider the life-geographies of the instruments necessary to its creation and management. We ought also to consider the magnetic observatory as a moving assemblage of instruments, materials, people, ideas and instruction that were mobilised through a variety of different geographies. In histories of the magnetic scheme, such a consideration has been missing. Instead, there has been a fixation on the political manoeuvring that engendered the creation of overseas observatories. In some histories of the politics of early Victorian science, the creation of the British magnetic scheme and its constituent magnetic observatories is made into an end product, the culmination of the magnetic lobby's haranguing of government and Admiralty figures. However, such a perspective can obfuscate the fluid and processual nature of the placement, construction and later management of the magnetic observatory.

The adoption of a definition of the magnetic observatory predicated on the practices and techniques that occurred within this space – such as was presented in the chapter devoted to the colonial magnetic observatories – allows for several important insights. The aforementioned definition allows the observatory to become a messier and more contingent space because it reflects the fact that instrument use was an embodied activity based on changeable instruction, susceptible to the differences between observers and reliant on the formation of an intangible understanding between user and instrument. As the colonial observatories chapter emphasised, instruction was apt to change and was frequently delayed, which affected the functionality of the observatory space and the officers' ability to manage the instruments within it. In these moments the traditional role of the observatory as nodal extension of the centre was undermined because officers came to rely on a specific, local, understanding of how to manage the observatory. In other words, the centre/periphery model is disrupted when we consider that the observatory as a scientific site was contingent not only on knowledge gleaned from and sent from the centre, but on locally informed practice too. Instruments were the conduits by which this relationship was disrupted; the instruments that changed and warped in the hot bowels of the *Erebus* and *Terror* or else, displaced from the space in which they had been learned and relocated within permeable and porous observatory walls, became puzzling and discordant and differently responsive. These instruments and the embodied knowledge of their use had to be reassembled anew in their new surroundings and with it the way the observatory looked and functioned changed from the ideal-type example (Dublin) to one more locally specific and contingent. Earth was banked in and around the main observatory building and its instruments; barrels transformed into semi-subterranean observation chambers; dust settled on instruments to act as a deterrent to and a trace of unwelcome touch; extra doors, double windows and partitions installed; instruments tinkered with by local workmen; and barometers placed outside the observatory so that observers were forced to rush in and out of the building.

The material structure of an instrument could be altered in its journey to the observatory. This change could be significant and entail adaptations of the ideal-observatory space to be made in situ; an example of a disruption to the traditional centre/periphery dichotomy. Something similar can be glimpsed in the passage of instruments during Lefroy's North American Magnetic Survey. The instruments Lefroy took with him to the North were not wholly fragile but nor were they decidedly robust. Even the Fox, feted for its hardy construction, was susceptible to damage on overland surveys such as that carried out by Lefroy. That Lefroy's instruments were able to travel with him so long and touched the confines of the Arctic Circle with him was because of a series of repairs enacted within a network of Hudson's Bay Company fort armourers and because of the diligence of the French-Canadian voyageurs and indigenous peoples who, through their careful transportation and handling of the instruments, maintained these precarious wares through the high northern latitudes in which they travelled and worked. The observations Lefroy recorded came to be contingent on local knowledge and local mechanical skills and did not simply represent the easy transmission of a centralised method and means of enquiry to the passive margins of the British Empire. Rather, some of the inhabitants of these marginalised places were the active agents by which the working agency of magnetic and meteorological instrumentation was retained. Bourguet, Licoppe and Sibum have written that 'local, situated and embodied practices on the one hand and global, universal knowledge on the other are always reshaped, rewoven and redefined with respect to one another' and that relations between the 'local and the global, far from being unidirectional, instead provides the impetus for a dynamic and open-ended process'.[1] It is this process, the mobilisation, maintenance and repair of instruments and other materials, that Lefroy's survey and the establishment of the colonial observatories can both readily illuminate. In a geography of these events, the travel of instruments disrupted 'vertically graded and centralised forms of organisation' and resulted in the distribution of horizontal and complex centres of instrumental expertise.

A geography of the magnetic crusade contains both mobilities and immobilities of scientific knowledge, the latter not as frequently explored in the literature. Efforts to mobilise magnetic instruments led to the immobilisation of knowledge about the

[1] Bourguet, M. N., Licoppe, C., and Sibum, H. O., 'Introduction', in Bourguet, M. N., Licoppe, C., and Sibum, H. O. (eds), *Instruments, travels and science: itineraries of precision from the seventeenth to the twentieth century* (Routlegde: London, 2002), 1-19, 14.

instrument. This was displayed most visibly at the Cape and at Aden but all the colonial observatory directors admitted to as much to some extent. Lloyd's experience of undertaking survey work as part of the British Magnetic Survey similarly demonstrated this idea. All of Lloyd's dip circle results obtained in his survey of Ireland were almost rejected, the needle of the dip circle (or perhaps some other cause) having induced magnetism in other parts of the instrument. This Lloyd only discovered much later, when he was required to completely reassess the observations he had recorded. Just as Lloyd remarked that very well-balanced needles could become unsteady and unsettled when they travelled away from home, so too could the attendant embodied knowledge of their operation, a thing akin to Sibum's 'gestural knowledge' but also something more than that. The knowledge of magnetic observation was embodied in both the user and the instrument. When an instrument was mobilised and exposed to the exigencies of travel, it was apt to change and, with it, the knowledge embodied by the instrument could be changed too. The magnetic instruments were the knowledge, in this regard.[1] This material-thing knowledge could be highly place-specific. The case of the vertical force magnetometer is pertinent here, but it also applied to others of the instruments at one time or other. The way the vertical force instrument operated at the colonial site was different, or appeared different, to how it had functioned in the Great Hall of Trinity College, Dublin, the space in which it had been initially learned. The instrument at the Cape or St Helena or elsewhere was a different instrument entirely because the knowledge embodied by it and the practical knowledge of the working of the instrument embodied by the officers had remained in Dublin. On encountering the new version of the instrument at the colonial site, a new practical education was needed, supplemented by the often-confusing messages and instructions sent from Woolwich and Dublin.

From such an understanding, we ought not to be misled by Airy and others' supposition that any old drudge could be taught to observe tolerably well in not more than a few days. Nor was it only by the delayed and mixed instruction received from England and Ireland alone that those at the observatory were able to overcome the different geographies in which their instruments were situated and through which they had travelled to arrive at the observatory, though certainly these pieces of correspondence were vital in confirming or criticising adopted practices. Learning to observe, at least learning to observe magnetic instrumentation, was a complex, embodied, and situated activity that required more than a passing knowledge of the instrument or a skim through the instruction manual. It required the accumulation of situated experience to be able to observe and distinguish between an

[1] Baird, *Thing knowledge*, 18.

instrument in a conscientious, working, adjustment, and an instrument that only gave the appearance of such. This was beyond knowing if an instrument was in a state of disrepair: cracks, rust, warping of the frame, these could all be more visibly apparent. But a magnetic instrument out of adjustment was a more intangible, intractable, problem. Little wonder that Smythe and his assistant observers were so despondent when the suspension thread for the declinometer they had been using for the two years up to 1844 suddenly snapped. This little thread was known to them. It held not only the magnetised bar of the instrument, but Smythe and company's accumulated knowledge too. Its particular torsion force was known and could be accounted for in their adjustment and observation of the instrument without undue labour. In its breaking, a new thread needed to be attached, and a period of familiarisation undergone. The snap of the thread represented the snap of the instrument's thing knowledge. That Smythe and his staff 'mourned for it as a friend departed' is of little surprise.[1] Wilmot also thought that, after twelve months without an observatory because of delays to its construction, he would not 'be very clever at it [observation]'. His books on meteorology and 'magneticals' were 'all strewed in hopeless confusion on the ground', the knowledge in them not the kind of embodied knowledge he would require.[2]

Humboldtian Science? Big Science? Survey Science? A geography of the magnetic crusade

This thesis makes a substantial contribution to research on the history of early nineteenth-century terrestrial magnetic research in Britain, which is also relevant to other studies in the history and historical geography of Victorian science. Some of these are empirical, others more conceptual. Empirically, this thesis has revealed the relationship between Edward Sabine and Humphrey Lloyd in much greater depth than has been presented in previous histories of the magnetic scheme. The 1830s was a vital period in the development and institutionalisation of geomagnetic science and it is through Sabine and Lloyd's extensive correspondence that we find details of this process: the testing and development of new and less liable instruments; the means by which magnetic surveys ought to be organised, conducted, and their results arranged; the tangible, intangible and constantly emerging techniques by which novices were trained in the magnetic art; and how the architecture of one of the biggest and most ambitious scientific projects ever

[1] Smythe to Sabine, 6 February 1844, TNA BJ 3/43/3.
[2] Wilmot to Lefroy, 7 January 1841, quoted in F. A. Eardley-Wilmot (ed.), *Memorials of Fredk. M. Eardley-Wilmot* (London: William Clowes and Sons, 1879), 25.

undertaken was designed, revised, implemented and maintained. It was often – though not always – through the correspondence of Sabine and Lloyd that other empirical additions of this thesis were unearthed. For example, in their letters about the British Magnetic Survey, we see the demarcation of testing grounds like Westbourne Green and Regent's Park to act as truth spots in the construction of instruments trustworthy enough to make British geomagnetic research credible and legitimate. The British Magnetic Survey had been largely neglected in the historiography of early nineteenth-century geomagnetism and yet many of the practices and discussions that attended its organisation and execution were vital to the way that British magnetic science developed. Latterly, this thesis also discussed Woolwich as a site of data management and distribution. Sabine's magnetic department is largely missing from histories of the British magnetic scheme and this thesis has sought to address that absence by demonstrating how important the different practices undertaken at the magnetic department were to the construction and distribution of results.

What underlies all the empirical contributions of this thesis is an exploration of the different practices of place that attended British-led investigations of the earth's magnetism in the early nineteenth century. In doing so, it has been shown how distinct, dynamic and malleable were the methods and processes used to overcome the multiple geographies through which geomagnetic research was conducted. At the magnetic observatories, these included covering and submerging magnetic observatory buildings and instruments in clay and soil; and reducing the tactile relationship between man and instrument. On magnetic surveys, these changeable processes could include the in-situ repairs and recycled fixes that a fort armourer could offer; or the testing and tinkering with axles that certain sites encouraged. At the magnetic department, there were, as we saw, a range of steps taken to mediate and interrogate the returns of the magnetic scheme that created different datasets. The examination of these practices in many respects refutes or at least resists the application of the term Humboldtian science.

In the literature review of this thesis, it was alleged that the term Humboldtian science ought not be applied to the machinations of the magnetic scheme because of the general nature of this term and because it obfuscates the distinct and diverse reality of how the scheme was enacted. It was suggested that the magnetic scheme ought to be thought of as one of a number of nineteenth-century survey sciences and that the scholarship associated with the term "big science" may help to better understand the organisation of the magnetic scheme. Thinking through the lens of survey science and big science enables a better understanding of the different manifestations of the relationship between individuals, hardware, place and data that accompanied the magnetic scheme. In the intervening chapters, these diverse relationships have been presented in detail. What follows is the argument for approaching the magnetic scheme not as the archetypal Humboldtian science, but as an example of big Victorian science – with some caveats.

For some already, the magnetic crusade is an example of Victorian "big science". However, as Ratcliff has written, the weight of this fact has not been reflected in the historiography and it seems rather, Ratcliff pointedly remarks, that 'the bigger science of a period often was not the most significant or visible science in historical or even contemporary terms'.[1] But, as Ratcliff has argued, 'the entire landscape of scientific culture looks different' if we study the processes by which big science was constructed. 'For example, from this angle the military emerges as the central institution of Victorian science', a subject that is 'almost entirely absent from the historiography of Victorian science'.[2] Significant parts of this thesis have been written with the intention of addressing this historiographical lacuna. Often, in histories that purport to demonstrate something about the politics of Victorian science, the emphasis has been on institutions such as the BAAS, the Royal Society and places such as Greenwich and Kew observatories. Recently however, the Admiralty has received due attention for the role it played in actively facilitating and engaging in scientific research in the Victorian era, and some of the machinations and politicking by which this was achieved have been considered.[3] The Army has perhaps not yet been given such thorough treatment. While the Royal Society and the BAAS were the channels through which the magnetic crusade was devised and support garnered, the execution of the project relied on an institution elsewhere, the Ordnance, based at Woolwich Arsenal.

It was in and through this institution that Sabine's magnetic department operated, where correspondence and results were received and instructions and authorisations sent out for all the colonial observatories and the surveys of Lefroy and Ross. It was not an observatory but it did act as a proxy or pseudo observatory in some regard because here instruments were set up and consulted and experimented with so as to cohere and reduce the

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[1] Ratcliff, J., *The transit of Venus enterprise in Victorian Britain* (Pickering & Chatto: London, 2008), 21.
[2] Ratcliff, *The transit of Venus*, 21.
[3] For example, Waring, S., 'The Board of Longitude and the funding of scientific work: negotiating authority and expertise in the early nineteenth century', *Journal for Maritime Research* 16, 1 (2014), 55-71; Cock, R., 'Scientific servicemen in the Royal Navy and the professionalisation of science', 1816-55', in Knight, D. M., and Eddy, M. D. (eds), *Science and beliefs: from natural philosophy to natural science, 1700-1900* (Ashgate Publishing: Aldershot, 2005), 95-111; Naylor, S., 'Weather instruments all at sea: meteorology and the Royal Navy in the nineteenth century', in MacDonald, F., and Withers, C. W. J. (eds), *Geography, technology and instruments of exploration* (Ashgate Publishing: Farnham, 2015), 77-96.

observations sent back from overseas. Dublin may have been the progenitor of the colonial observatories in that their design was ostensibly based on this institution, but it never came to act as a "headquarter observatory" in the manner that Sabine might have wished it had. The magnetic department that Sabine built had to be made to fit within the existing bureaucratic structures of the Ordnance, a struggle that was played out at both Woolwich and to some extent in the colonies and which often revolved around the allocation of space and the need to maintain the crusade as a military scheme. There really was no level at which the greatest scientific undertaking the world had ever seen – per Whewell in 1857 – was not a military endeavour. The de facto head of the scheme, Sabine, was an officer of the Royal Artillery and so too were the directors of the colonial observatories and the leaders of the various magnetic surveys. The assistants at the observatories were all Non-Commissioned Officers, gunners, drivers, bombardiers and the like. Repairs to the observatories and occasionally to the instrumentation at the observatories were carried out by members of the Royal Engineers. Sabine's magnetic department was situated at Woolwich and staffed by NCOs. Even the computers were drawn from the ranks of the Royal Artillery, the Board of Ordnance having baulked at Sabine's suggestion that civilian computers might be used. The networks of administration, authorisation and allocation of funds needed to maintain the operation of the scheme were all run through the Board of Ordnance. This thesis has delineated how these military spaces and bureaucratic procedures were activated during the course of the magnetic crusade. It has also sought to highlight the experience and practice and, ultimately, the importance, of scientific servicemen in the observatory, in the field, and in the centre of calculation and interpretation, Woolwich.

The term "big science" has been invoked to describe a number of other scientific forays. Ratcliff applies the term to the Transit of Venus enterprises of the eighteenth and nineteenth centuries, and to the intervening magnetic crusade project. Schaffer and Shapin use the term to describe Boyle's air-pump enterprise because the device came to be so elaborate and expensive that it became an example of 'seventeenth-century "big science"'.[1] For Daston, the 'first wave of Big Science' did not occur until the late nineteenth century, with the creation of enormous scientific compendia projects like the Carte du Ciel and the Corpus Inscriptionum Latinarum (CIL).[2] These compendia – the former to catalogue and map the positions of millions of stars, the latter to collect together masses of public and

[1] Shapin, S., and Schaffer, S., *Leviathan and the air-pump: Hobbes, Boyle, and the experimental life* (Princeton University Press: Princeton, 2011 edn, 1985 orig.), 38.
[2] Daston, L., 'The immortal archive: nineteenth-century science imagines the future', in Daston, L., (ed.), *Science in the archives: pasts, presents, futures* (The University of Chicago Press: Chicago and London, 2017), 159-184, 160.

private Latin inscriptions – were ‘conceived on a scale of a decade to a century or more’, archival behemoths designed as ‘the modern age’s answer to ancient pyramids and medieval cathedrals’ that contained ‘the working materials that nineteenth-century scholars and scientists imagined would enable their successors to conduct research for centuries (if not millennia) to come’.[1] Ratcliff has remarked that “big science” was a term that emerged after World War Two ‘as a label for the enormous scientific projects of modern times’.[2] Aranova et al have argued that it was a term ‘first coined in the 1960s by physicist Alvin Weinberg’ to describe ‘enormous scientific undertakings that were incredibly costly, involved hundreds or even thousands of investigators, adopted a corporate-style management structure, and tended to monopolize support and attention from public and private sources’.[3] Agar has likewise positioned big science as a ‘product of the … long 1960s’ and distinct from the mode of organisation resplendent in several nineteenth-century scientific research projects.[4] However, Daston has located the first use of the term from several decades earlier, in 1890. According to Daston, it was in this year that Theodor Mommsen coined the term “Big Science” (*Grosswissenschaften*) in an address to the Berlin Academy of Sciences. Mommsen had been ‘the moving spirit’ behind the establishment of the Carte du Ciel project.[5]

It follows that big science, as a term or concept is rather vague. There is no single unifying definition of what makes a scientific project worthy of the moniker. Relatively high cost (for the period in question), perhaps, or a (selective) globality or maybe a certain temporal coverage, but these are still inexact measures. Certainly, Big Science must exist on a conspicuously big scale but this ought also to be qualified. The bigness of the Carte du Ciel and CIL and, to an extent, the magnetic crusade, did not exist at a single point in time but was scattered: collective collection efforts began and ended at different stages and a temporal bigness is only seen in retrospect. Use of the organs of state – such as the military – for the accumulation of scientific data might also be considered a characteristic of big science, in which case the magnetic crusade certainly qualifies as one of the earliest examples (if not the earliest) of Victorian Big Science. In reality, Big Science is all or any or one of these things – there can be no definitive criteria. Better then, perhaps, to view “big science” not as a straightforward categorisation but as a provocation. In whatever era encountered, it is perhaps more instructive to ask not what defines this or that science as an

[1] Daston, ‘The immortal archive’, 159.
[2] Ratcliff, *The transit of Venus*, 21.
[3] Aranova, E., von Oertzen, C., and Sepkoski, D., ‘Introduction: historicizing big data’, *Osiris* 32, 1 (2017), 1-17, 3.
[4] Agar, J., *Science in the twentieth century and beyond* (Cambridge University Press: Cambridge, 2012), 330.
[5] Daston, ‘The immortal archive’, 160.

example of big science but to ask how it created the *appearance* of bigness. From this, it follows that, as Finnegan has argued,

> "Big science" might be approached not as a single and monolithic entity uniformly stretched across global space but rather as a dynamic conglomeration of practices, materials and people differently assembled in different places and relying on the translation and transformation – more than straightforward diffusion – of data and theories.[1]

The provocation is then a call to unearth and explain the dynamic conglomeration of practices, materials and people that successfully create the artifice of big science. Such a provocation undergirds this thesis. It has sought to deconstruct the scale and scope of the magnetic crusade by looking at the translation and transformation of several different facets of the crusade: the design of the ideal-type observatory from Dublin to the different geographies of four colonial sites; the (quite literal) transformation of magnetic instruments as they were mobilised to the colonial sites and as part of geophysical surveying efforts; the difficult translation of instrument knowledges from one place to another, or, how the embodied, tactile processes by which officers learned to become accustomed to their instruments were not easily mobilised; how small mountains of data were returned, interrogated, reduced and made publishable and the journey of those datasets in life, death and eventual rebirth through geomagnetic scientists' engagement with the archive. The magnetic crusade was a conspicuously big science but this was in several respects only an artifice, created through the myriad translations, transformations and geographies in which, through which and by which it operated. It is time, perhaps, to consider other of the stereotypical nineteenth-century Humboldtian sciences through the same lens as that through which the magnetic crusade has been looked at here and to reject the vagaries and totalising effect of Humboldtian in favour of the methodology proposed by big science or survey science.

[1] Finnegan, 'The spatial turn', 384-385.

Appendix 1: Archive Files

RS MS/119 (2 Vols) These files contain hundreds of letters sent to Humphrey Lloyd from Edward Sabine, the directors of the colonial magnetic observatories and several other important figures and represent a wealth of information on the history of early nineteenth-century geomagnetic research.

RS MS/257 These files, arranged alphabetically by correspondent, contain letters of Edward Sabine sent to and received from dozens of individuals in the domestic and foreign scientific community.

TNA BJ 3 The papers and correspondence of both Edward Sabine and his magnetic department. There are 84 files in this series. In researching this thesis, I delved into almost all of these at one time or another but those used most extensively are:

- **7-13** Correspondence of Sabine and Lloyd between 1833 and 1848.
- **27 and 28** Letters to the magnetic department from the Office of Ordnance and the Deputy Adjutant General's Office. These offer an overview of the administration of the British magnetic scheme.
- **35** Letters to Sabine from Lt. Lefroy at Toronto Observatory, 1841-1845.
- **38** Letters to Sabine from Lt. Lefroy at Toronto Observatory, 1848-1851.
- **40** Out-letters to the Toronto Observatory from the magnetic department.
- **41** Account book of the St Helena Observatory.
- **43** Letters to Sabine from Capt. Smythe at the St Helena Observatory, 1842-1852.
- **79** Letter books: out-letters from the magnetic department, 1839-1854.
- **80** Letter books: out-letters from the magnetic department, 1839-1853.
- **81** Letters to Sabine from Lt. Lefroy at St Helena Observatory, 1839-1842.

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