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NAVIGATIONAL ENTERPRISES IN EUROPE AND ITS EMPIRES, 1730–1850

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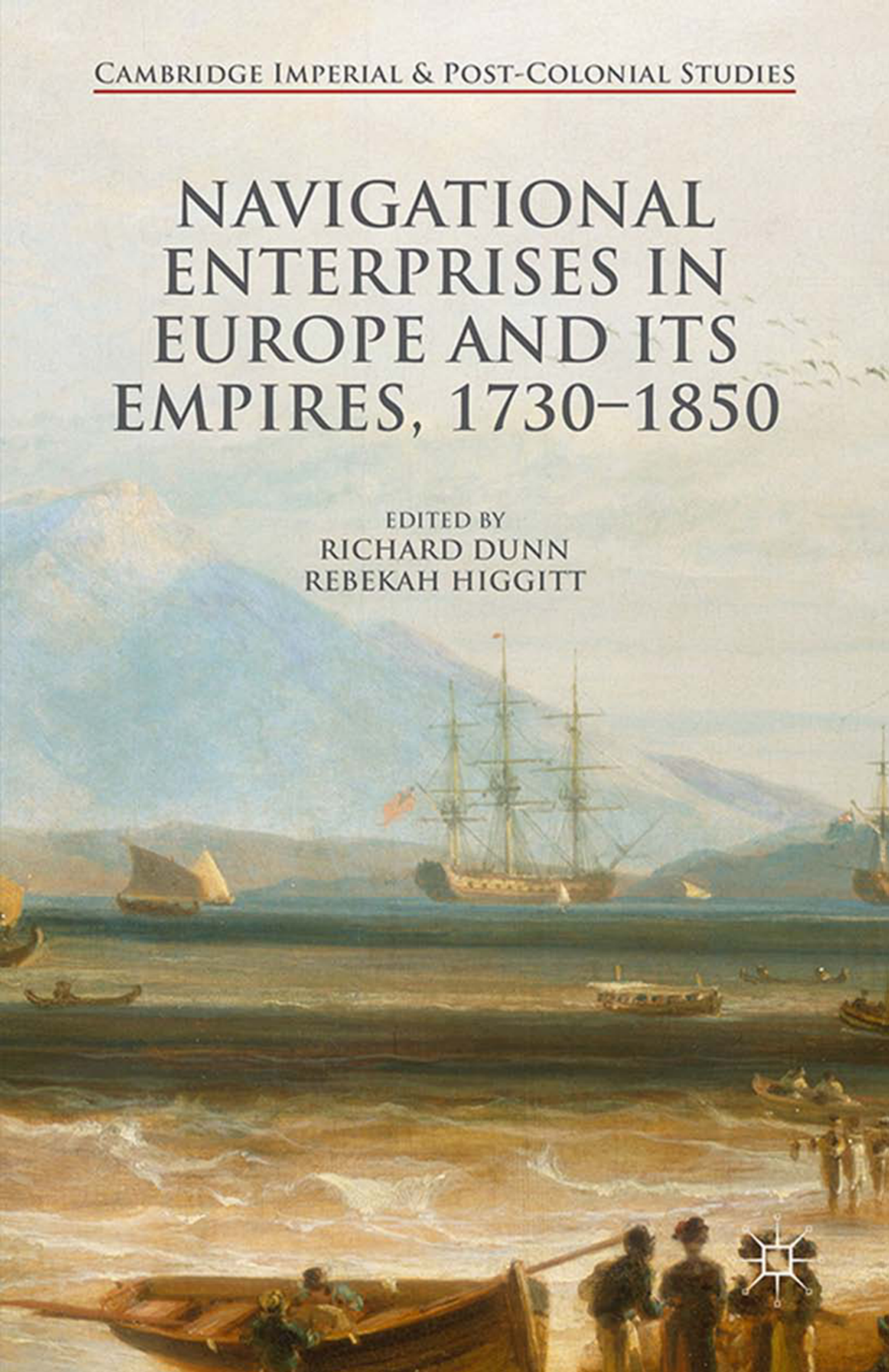

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Series Standing Order ISBN 978–0–333–91908–8 (Hardback)
978–0–333–91909–5 (Paperback)
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Navigational Enterprises in Europe and its Empires, 1730–1850

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Softcover reprint of the hardcover 1st edition 2015 978-1-137-52063-0

First published 2015 by
PALGRAVE MACMILLAN

Palgrave Macmillan in the UK is an imprint of Macmillan Publishers Limited, registered in England, company number 785998, of Houndmills, Basingstoke, Hampshire RG21 6XS.

Palgrave Macmillan in the US is a division of St Martin's Press LLC, 175 Fifth Avenue, New York, NY 10010.

Palgrave Macmillan is the global academic imprint of the above companies and has companies and representatives throughout the world.

ISBN 978-1-349-56744-7 ISBN 978-1-137-52064-7 (eBook)
DOI 10.1057/9781137520647

This book is printed on paper suitable for recycling and made from fully managed and sustained forest sources. Logging, pulping and manufacturing processes are expected to conform to the environmental regulations of the country of origin.

A catalogue record for this book is available from the British Library.

A catalog record for this book is available from the Library of Congress.

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1

Introduction

Rebekah Higgitt and Richard Dunn

The story of eighteenth-century navigation has usually been told as a British one, focused on the successful search for a means of establishing longitude at sea. The desire for heroic stories about individuals – their discoveries, inventions and triumphs – has often reduced this history to that of one man, John Harrison, a clockmaker whose work inspired the development of the marine chronometer. The essays in this collection challenge such assumptions, which have proliferated in academic and popular literature.[1] They insist on plurality, in the places, people, problems, solutions and circumstances that furthered the development of eighteenth- and early nineteenth-century maritime practice. They expand the story in terms of geography and time. This collection has two overriding aims: to present work on and the historiography of non-British experiences in the development of new navigational techniques and instruments; and to examine their use in practice, demonstrating that the available methods were complementary rather than exclusive, and that when and how they were used was contingent on local, national and other circumstances. The chapters also reveal the slow process of technological development and adoption, the varying roles of states and institutions and the international and local characteristics of this process.

The contributions to this volume to an extent take the well-known longitude story for granted, so we present a basic outline here.[2] As nations developed ambitions for maritime exploration, expansion and trade, navigation generally and longitude in particular became topics for discussion and development, at sea and in more idealized, land-based contexts. Although by no means the only area of interest, determining longitude was identified as a matter over which there was considerable doubt and in which mathematicians, astronomers and instrument-makers might make significant improvements. Longitude and latitude were typically established using dead reckoning – estimating the ship's position relative to the last known location by tracking speed and heading, taking into account winds, currents and other conditions. Observations of the altitude of the Sun or pole star could also be used to determine latitude with reasonable accuracy. Longitude lacked these reference points, although it had been known since ancient times that the difference in longitude between two locations is equivalent to their time difference. Observing the Sun or stars could establish local time whether on land or sea. The question was how to find the local time of a distant location with which to compare it.

The view that the development of new instruments and practices could solve the problem was evident in published works on astronomy and navigation and in the initiation of rewards and prizes by states with maritime ambitions. Spain offered royal rewards from 1567, the Dutch States from 1600, the British government from 1714 and the French Académie des sciences from 1720. The British example is the best known; Parliament in 1714 passed the Longitude Act, which appointed commissioners to judge submitted and trialled methods. The Act provided rewards of up to £20,000 if longitude could be found or kept to within half a degree. There were lower rewards for less precise methods and for ones that were usable within 80 miles of the coast. The Act also offered incentives to bring promising ideas to trial.

By 1714, there was a well-established set of methods, which, as Isaac Newton put it to Parliament, were 'true in theory but difficult to execute'.[3] They included the production of a timekeeper that, despite motion and changing conditions on board ship, could keep the reference time (local time at a known location) with sufficient accuracy that it could be compared with local time on board ship to establish difference in longitude. This was an obvious desideratum, but it presented technical challenges, which individuals such as Christiaan Huygens, Robert Hooke and Henry Sully explored. Other methods, based on astronomy, determined reference time from the motions of celestial bodies, comparing their predicted positions at the reference location with that observed at sea. Interest in astronomical methods had led to the establishment of two observatories with royal and government patronage: the Observatoire de Paris (founded 1667) and the Royal Observatory, Greenwich (1675).

One astronomical method was to observe the regular eclipses of Jupiter's satellites. Galileo Galilei, who discovered them in 1610, immediately sought to use them to establish longitude, developing a theory of their motions and methods to facilitate their observation. Although he had limited success, subsequent work turned his approach into a fruitful method for establishing longitude on land. From that date, there was ongoing experimentation with devices that might facilitate the delicate observation of Jupiter's satellites at sea. The precision required meant that observations of the much closer Moon seemed to offer greater hope. One of the possibilities that astronomers explored was the lunar distance method, which relied on measuring the Moon's position relative to the Sun or stars. The main challenge was the complexity of lunar motion, which is affected by the gravitational pull of both the Earth and the Sun. This problem ultimately defeated Newton, who had tried to tackle it in his *Principia Mathematica*.

By the 1750s, however, many of the technical problems were beginning to be overcome. The use of mirrors and lenses on observing instruments led to the development of instruments – octants, sextants and reflecting circles – that improved the accuracy of shipboard observations. Ongoing mathematical and theoretical work, much of it done on the European Continent, and observational work, particularly in Britain, came together in Tobias Mayer's theory of lunar motion, which, though imperfect, was potentially good enough for navigation. Meanwhile, developments in horology, which had revolutionized the accuracy of land-based astronomy in the seventeenth century, were beginning to be applied to marine timekeepers.[4] John Harrison's first sea clock ('H1') had a promising trial in 1736, gaining the support of the Royal Society of London and the first of a series of rewards from the Commissioners of Longitude, which helped support Harrison's eventual development of his famous sea watch ('H4'). In France, the Académie offered rewards for mechanical timekeepers and received, in the 1750s, sealed descriptions of such instruments by Pierre Le Roy and Ferdinand Berthoud.

While timekeeping and astronomy have been seen as the two key – and rival – methods for finding longitude, it is clear from this book's chapters and recent research on the Board of Longitude that they were neither the only methods, nor perceived as rivals, except, perhaps, by individuals seeking rewards. In fact, it was understood that these two methods worked best when used together. Timekeepers were, although not as simple to use as it might be thought, quicker and easier than astronomical methods and ideal for keeping track of time and longitude between opportunities for checking their going, either on land or at sea. Timekeepers are, however, subject to cumulative errors and can go wrong in various ways. Only astronomy could find longitude, rather than simply track it.

One should not ignore other navigational methods, either. There were techniques already in use, including dead reckoning, depth sounding and visual markers such as patterns of currents, coastal features, birds, fish and plants. New techniques did not replace these practices; they were used in conjunction with them. There were also techniques that have come to be dismissed, either at the time or in subsequent historiography. These include the use of signals, despite the rocket scheme put forward by William Whiston and Humphrey Ditton in 1714 having been ridiculed. Another was navigation using the patterns of the Earth's magnetic field, usually variation (or declination), the angular difference between true and magnetic north. For this method to be widely used, the patterns would have to be mapped, with readings of variation plotted on a chart. The patterns were complex and changed over time, however, ultimately making this method impractical. Nevertheless, the method was used in particular sea areas, notably where lines of equal variation were close together and ran nearly north-south.

There is, however, no straightforward line to be drawn between the development of methods in observatories, studies and workshops and their use at sea. Ideas had to sound plausible to interest officials and potential patrons. Astronomical and other data had to come together with effective hardware before either could be put to work. Prototype instruments had to be developed into effective and affordable commodities; governments and trading companies had to invest in infrastructure and training. All methods had to be tried and tested, not just to gain recognition, rewards and contracts but also – slowly – to become trusted elements of maritime practice. A research project on the British Board of Longitude undertaken by the University of Cambridge and the National Maritime Museum (NMM) (funded by the Arts and Humanities Research Council) has begun to tell the story. This volume reinforces and broadens it, geographically and thematically.[5] In presenting more nuanced accounts of the development and practice of navigation, therefore, this book complements the new history of the Board of Longitude.[6]

The chapters in this book evolved from papers given at workshops and conferences associated with this research project and the NMM. In particular, 'Oceanic Enterprise: Location, Longitude and Maritime Cultures 1770–1830', held in January 2013 at the project's partner organization, The Huntington, in San Marino, California, situated the Board's work within broader contexts of European empire, trade and exploration. The chapters by John Gascoigne, David Philip Miller and Simon Werrett are based on papers delivered there. Another workshop and a major conference, both at the NMM, led to chapters by Guy Boistel, Karel Davids, Michael Kershaw, Juan Pimentel and Martina Schiavon.[7] Jane Wess first presented a version of her chapter at a conference on Joseph Banks at the NMM in 2011.[8] Finally, a session at the 2013 International Congress of Science, Technology and Medicine led to the chapters by Jacob Orrje and Danielle Fauque.[9]

Although our core themes run throughout the book, it is divided into four sections that reflect the organizing principles of their chapters. The first section examines the question from particular national and imperial contexts, correcting histories that largely ignore non-British contributions and reflecting historical accounts often available only in other languages. These chapters reveal why the nation is unsatisfactory as a unit for exploring this history, both because people, ideas and instruments flowed between states, and because empires brought together spaces of widely divergent histories and practices. They emphasize how the contexts in which new longitude techniques were introduced affected their take-up and use. These contexts include states – their support for new methods, as well as their relationships with, and self-perceptions relative to, other nations. Particular sea routes that challenged national and merchant navies constituted another important factor.

Pimentel focuses on the Iberian context, including the shifting reference points for longitude – prime meridians – that the Spanish Empire employed, reflecting imperial ambitions in the Atlantic or claims to scientific modernity by the Real Observatorio de Cádiz. He situates longitude within a longer history of navigation and exploration, in which the ability to measure latitude figures as a significant advance and the politics of maps and meridians influence practice. The chapter by Davids tells the Dutch story, which also opens with consideration of meridians, noting the official adoption in the 1820s of the Greenwich meridian. This reflects both the extent to which British publications and charts were in widespread use by then, and a turn within the Netherlands after the Napoleonic Wars from French to British navigational technologies. Through an examination of the role of the Dutch Longitude Committee, Davids explores the relationship between state-supported provision, within a decentralized group of provinces, and maritime practice. Decisions about the use of different techniques were pragmatic and dependent on circumstances.

The two following chapters shift to France, which was more intimately bound up with the British story than is usually acknowledged. As Boistel shows, French contributions were fundamental to the mathematics and astronomy behind the development of the lunar distance method for use at sea, which the Board of Longitude's publication of the *Nautical Almanac* from 1767 successfully supported. His chapter explores the importance of particular individuals, who, if positioned within government or Académie, could promote or undermine particular lines of enquiry. They were also participants within an international correspondence network of astronomers and mathematicians. Schiavon takes the story forward to the 1795 founding of the Bureau des longitudes, exploring the histories that its recently digitized minutes reveal. She considers the Bureau less as a response to British maritime dominance, although this was used as a rhetorical strategy, than as a unique institution that brought together particular strands of scientific enquiry during the Revolutionary period, just after the closing of the Académie, and beyond. Building on the skills, techniques, instruments and objects of study associated with maritime navigation, and renegotiating the relationship with the nation's observatories and other institutions, Schiavon shows that geodesy was to become a key focus within the strategically placed institution.

The book's second section further develops the discussion about international correspondence and influence, by investigating specific examples of transnational encounter, appropriation and cooperation. Spain and the Netherlands had dominated maritime exploration and trade until the eighteenth century, and promoting new longitude techniques was a way to reassert their primacy. Elsewhere there was a felt need to 'catch up' to become part of modern Europe. In some cases, these efforts led to more effective implementation of training than found in Britain and France. Although these various nations established different traditions of training and support for navigation, their institutions and scientific practitioners were in constant communication. Nations did not seem to regard new approaches to navigation as state secrets or even, beyond the interests of instrument makers, commercially sensitive. Britain and France, close neighbours and rivals, pursued navigation and metrology through cooperation underpinned by competition. Rivalry could be expressed through imitation and collaboration.

In his case study, Orrje reveals some of the means by which information circulated and how one individual might take on different identities to ease communication. He follows the Swedish astronomer Bengt Ferrner's visit to London in 1759–60, a crucial moment in the development and support of the new longitude methods. Ferrner's journal provides fascinating insight into London's central role in generating interest and support for a range of methods, from the workshop in which John Harrison was making his famous timekeepers to the marine chair, designed by the otherwise little-known Christopher Irwin for observing Jupiter's satellites from a ship. We see London as an international community and, again, the porous nature of national identity among overlapping communities of maritime, trade, political and scientific interest. In different contexts, being a Swede or a man of science might more easily facilitate information sharing. Werrett also notes the significance of people travelling to Britain to learn about instruments and practices. Russia had a long history of learning from Britain, whether by importing experts or sending cadets for training. Yet Werrett points out that the information flow was by no means one way: individuals based in Russia played important roles in developing or trialling new ideas. Looking at the practice of navigation in Russian voyages of exploration, we see that new and foreign by no means trumped tried and tested. Again, the complementary nature of old and new methods, and the pragmatics needed in practice, are revealed within 'complex relationships of trust in different instruments and personnel' (p. 111).

Several chapters emphasize the links between observations on land and at sea. Observatories were established to support the production of predictive tables, known land locations were used to check and correct longitudes established at sea, portable observatories were used to fix longitudes on land during voyages, and the use and creation of accurate charts crucially underpinned accurate navigation. We should also note the similarities of practice and personnel that linked land-based and maritime surveys. Kershaw's chapter focuses on collaborative work by British and French governments (including, in the 1820s, their boards of longitude) to establish the difference in longitude between the two most important centres of astronomical data for navigation and survey, the Greenwich and Paris observatories. He reveals the networks of trust that had to be established to carry out this work, between individuals, instruments and techniques, and the careful and repeated comparisons made between different, embodied standards of length. Imperial yards were made to meet French *toise*, which were in turn used to establish the metre. Fieldwork, in the form of triangulation, created a more accurate knowledge of the difference of longitude than astronomy, though both sets of data fed into the long-pursued question of the shape of the Earth.

The two chapters in the third section take us back to sea without losing sight of the complementary nature of observations made on land and at sea. They focus on the role of sea voyages in trialling instruments and techniques, showing that when the position of land was known, it could be used to assess the results given by a new instrument, while the developing methods could be used to fix locations of less well-known places. Possibilities and requirements of accuracy were always contingent. From different perspectives, Fauque and Gascoigne look at French sea voyages. Those that Fauque examines took place between 1767 and

1772 for the trial of timekeepers by Le Roy and Berthoud, often alongside other methods and ideas, usually submitted for Académie prizes. She shows a process independent of developments in Britain, although she is clear that both clockmakers were profoundly influenced by Harrison's success with H4 and what they could learn of its mechanism. As well as their personal ambitions, the chapter reveals the competing ambitions of the Académie and Ministère de la Marine, and a range of motivations behind the voyages. As well as testing, these included imperial diplomacy and a display of French scientific interests and capabilities.

Gascoigne turns to the Pacific as a high-profile and ambitious testing-ground for navigational techniques. His focus is on elite, well-equipped voyages, which, like Cook's for Britain and the Russian voyages explored by Werrett, had sufficient resources to put lunar distances and timekeepers fully to work. These transnational similarities must, however, be read alongside the peculiar national circumstances shaping their purpose, organization and instrumentation. The voyages were test sites as well as proving grounds for methods that could not yet be relied on, yet were used to fill in blank spaces on maps. The establishment and relocation of longitudes by those with the right knowledge, training and equipment, was presented as a conversation between and among the civilized. Gascoigne shows that, in these contexts too, different methods of establishing longitude – at sea, on land, by astronomy, timekeeper and dead reckoning – were deployed in complementary ways. Particular circumstances might encourage trust in one method over another but, even in the nineteenth century, the most valued of sea-based determinations was astronomical, provided that conditions were good and repeated observations could be averaged.

The final section maintains the focus on practice at sea but moves away from voyages of scientific trial and exploration to more routine voyages that lacked prestige, novel equipment and civilian or specialist skill in astronomy and the use of instruments. Chapters by Wess and Miller take us back to the British context with accounts of navigation in the merchant and Royal navies that complicate older narratives. Wess, exploring evidence from navigation textbooks and log books from the Royal Navy and East India Company (EIC), challenges the idea of a 'golden age' for the lunar distance method before the era in which ships might expect to carry and rely on a set of at least three chronometers. She argues that although mathematics was presented as transformational to eighteenth-century navigation, both by those promoting its utility at the time and those writing histories of navigation in the twentieth century, it was actually a check on the adoption of the lunar distance method. The 'ugly' and unintuitive mathematics needed to produce longitude determinations was, it seems, a bar to quick and general adoption beyond elite voyages of exploration. Its use would, it seems, wait until the nineteenth century, once chronometers became more widely available.

Miller, looking at the routine long-distance voyages of EIC ships, brings the story into the nineteenth century. Here, too, we see the full suite of methods in regular use: the message is of plurality and complementarity. So too in the relationship between observations at sea and on land, particularly with the development of EIC-sponsored observatories that could supply time for rating chronometers, creating an international support network. Dead reckoning long persisted, alongside increasing trust in the reliability of chronometers, although Miller reveals the extent to which their use was far from simply reading off the longitude from their dials. Chronometers began to be used to survey coastlines, although good practice insisted on the use of astronomical determinations too. Miller explores EIC attempts to ensure that officers were trained in the new methods, and the networks of teachers that supported this through certification, as well as pre-printed log books and instructions. Although these two chapters push adoption of these methods further into the nineteenth century, the EIC still appears to have been more consistent and earlier in its take-up than the Royal Navy. Key, however, is the evidence that approaches to navigation depended on route and location. Experience and local conditions led to decisions about what techniques would be most reliable and useful at different times, with particular moments in the voyage being ones when, perhaps after relying on dead reckoning, it was judged necessary to '"get serious" about longitude again' (p. 238), with a flurry of observations.

As Miller writes, 'Neither technological determinism identifying an instrument as *the* solution, nor singular method determinism, captures how longitude was established in practice' (p. 224). The chapters in this book demonstrate this, revealing the flaws in the simplified accounts that claim that the invention of the marine chronometer in the mid-eighteenth century solved the 'longitude problem'. They also show that accounts that present the story solely as a British one tell only part of the story, ignoring not only the contributions of Frenchmen, Swiss and Hanoverians to horology and astronomy, but also the wider contexts in which navigation was practised, tried, taught, borrowed and improved. London and Paris undoubtedly remained centrally important locations for supporting innovation and selling instruments, and for texts and training. However, both cities were porous, with manifold influences feeding into them and carried well beyond.

Notes

1. The famous popular narrative is Dava Sobel, *Longitude: The True Story of a Lone Genius Who Solved the Greatest Scientific Problem of His Time* (New York: Walker, 1995). Also influential is William J. H. Andrewes (ed.), *The Quest for Longitude* (Cambridge, MA: Harvard University Collection of Historical Scientific Instruments, 1996). Much academic literature has challenged more simplistic narratives, although many general works on exploration, empire, maritime travel and science in this period have unquestioningly used them.
2. Much of the following is based on Derek Howse, *Greenwich Time and the Longitude* (London: Philip Wilson, 1997) and Richard Dunn and Rebekah Higgitt, *Finding Longitude: How Ships, Clocks and Stars Helped Solve the Longitude Problem* (Glasgow: Collins, 2014).
3. Isaac Newton, 'Report to the Lords of the Admiralty on the Different Projects for Determining the Longitude at Sea', Newton Papers, Cambridge University Library Add. 3972, fol. 32r, Cambridge Digital Library <http://cudl.lib.cam.ac.uk/view/MS-ADD-03972/63> [accessed 20 April 2015].
4. In this book 'timekeeper' generally denotes sea clocks and watches developed before the 1780s. Although in occasional use earlier, 'chronometer' in the current understanding of the term, denoting a more standardized product, was first used for John Arnold's 1780s timekeepers.
5. 'The Board of Longitude 1714–1828: Science, Innovation and Empire in the Georgian world' (2010–15), RCUK Gateway to Research <http://gtr.rcuk.ac.uk/project/4BC922B5–7B60–47CE-A71A-76E6BB9542D0> [accessed 20 April 2015]. The Principal Investigator was Simon Schaffer, Co-Investigators Richard Dunn and Rebekah Higgitt, postdoctoral researchers Alexi Baker and Nicky Reeves, doctoral students Katy Barrett, Eóin Phillips and Sophie Waring, and Engagement Officer Katherine McAlpine. *Ships, Clocks & Stars: The Quest for Longitude* was at the National Maritime Museum, Greenwich, July 2014–January 2015. The project team contributed to the digitization of the Board's papers, funded by Jisc, for Cambridge Digital Library, launched in 2011 <http://cudl.lib.cam.ac.uk/collections/longitude> [accessed 20 April 2015].
6. Alexi Baker (ed.), *The Board of Longitude 1714–1828: Science, Innovation and Empire* (London: Palgrave Macmillan, forthcoming).
7. 'Longitude and Survey: International Relations', NMM, June 2012; 'Longitudes Examined', NMM, July 2014.
8. 'Exploring Empire: Sir Joseph Banks, India and the "Great Pacific Ocean" – Science, Travel, Trade and Culture, 1768–1820', NMM, June 2011.
9. 'Empires of Longitude: International Perspectives on Navigation, Mapping and Science', 24th International Congress of History of Science, Technology and Medicine, Manchester, July 2013.

Part I

National Enterprises

2

A Southern Meridian: Astronomical Undertakings in the Eighteenth-Century Spanish Empire

Juan Pimentel

In a treatise on geographical history published in 1752, Father Murillo, a Spanish Jesuit, reviewed the different places used as references for calculating longitude; in other words, where the 'prime meridian' had been situated throughout history:

> Pytheas of Marseille began from Thule or Iceland; Eratosthenes, followed by the Arabs, from the Pillars of Hercules or the Strait of Gibraltar; Hondius from the Hesperides or Cape Verde; Gerardus Mercator from between the islands of Corvo and Flores; Ptolemy from the Canary Islands; Blaeu, who was followed by the Dutch, from the island of Tenerife; Ricciolo from the island of La Palma. The French, by order of Louis XIII, placed the Prime Meridian for the measurement of longitude on the island of El Hierro [...] Now the English have moved it again to London, and not to be outdone the French have set it in Paris. And one day they will put the equinox and the tropics in London or Paris.[1]

Obviously, Murillo's ironic remarks about Britain and France are a commentary on the social character of mathematical geography. Although the Equator does not yet pass through Greenwich, what does pass through there, as everyone knows, is the Prime Meridian. The agreement reached at the Washington Conference of 1884 consolidated the pre-eminence of British astronomy and cartography and the central role of the Royal Observatory, Greenwich, over the previous century and a half.

If the establishment of meridians is a social convention, if every map is a 'controlled fiction',[2] if geography as a whole is a 'contested enterprise',[3] what can be said about history? One can tell the story of the quest for longitude from many viewpoints. It has been usual to tell the story at a national or imperial level, or even a heroic one (the subtitle of Sobel's *Longitude* is eloquent: *The True Story of a Lone Genius Who Solved the Greatest Scientific Problem of His Time*).[4] It is important, however, to ask whether it is possible to escape this pattern and recount the history of longitude from places other than Greenwich. As Dunn and Higgitt have pointed out, 'the quest for longitude was an international story'.[5] As such, it is a story of collaboration and competition, in which colonial rivalries, the traffic of objects and the interchange of knowledge intersected. This chapter will examine Spanish contributions up to the attempts to create a reference point at Cádiz Observatory, another attempted 'imperial meridian', a southern meridian to calculate distances and to prepare a modern cartography of Spanish America and the Pacific.[6]

Dividing and measuring the world

By the sixteenth century, it was already clear that the national scale was of little use for mapping the past. Key Iberian figures include Pedro Nunes (1502–77), a Portuguese mathematician of Jewish origins and professor at the University of Salamanca, who discovered the loxodromic curve. He also invented a device for measuring longitudes, which made it possible to distinguish small fractions of angles, a forerunner of the vernier.[7] Before him came Abraham Zacuto (1452–1510), another Jew who also gave classes in Salamanca and whose influential *Almanach perpetuum* was first published in 1496. A contemporary of Nunes, Alonso de Santa Cruz (1505–67), wrote a *Libro de longitudes* (about 1567), one of the first systematic studies of the subject. Later in the century, Juan López de Velasco (1530–98) organized a programme to observe eclipses and lunar motions in the Iberian Peninsula; his programme would have a descendant in the work of the fifth Astronomer Royal at Greenwich, Nevil Maskelyne.

Santa Cruz and López de Velasco were cosmographers in the Casa de Contratación, which organized geographical information and traffic in the New World. Although the Iberian countries had many cosmographers, pilots and naturalists, what is more significant is their institutions that produced and managed knowledge.[8] Without science, how could they have studied the flora and fauna of the Indies? How would they have been able to exploit the mines and extract resources? How would this be possible if they had not reached America and rounded the

Cape of Good Hope – the two most important events in the history of humanity since the birth of Christ, as writers from the earliest chroniclers of the Indies to Adam Smith declared? Yet the 'black legend' that has demonized the Spanish Empire has no doubt also passed on some of its clichés to the traditional historiography of science, which for decades has looked down on the Iberian countries.[9]

Patricia Seed, a professor not in Salamanca or Lisbon, but in Irvine, California, and author of the *Oxford Map Companion* (2013), has repeatedly affirmed that the greatest conquest in maritime exploration was the determination of latitude, not that of longitude.[10] She is not alone: Alison Sandman, historian of Iberian cosmography, has also problematized the utility of determining longitude at sea for oceanic navigation.[11] The intention of this chapter, however, is not to discuss latitude or the sixteenth century, nor to deliver an apology for Iberian science. I merely wish to remind readers that the astronomical determination of position is an old subject, quite extensive and completely social. If, since the development of ancient Greek mathematical geography, it has been recognized that knowledge of the Earth depends on celestial observations, we should also recognize the complement: that astronomy has always been a terrestrial and mundane discipline.

Let us look at two questions relating to the most mundane subject in the world: its ownership. Spain and Portugal had the felicitous idea of dividing the world between them in the treaties of Tordesillas (1494) and Zaragoza (1529), by which they drew a meridian and anti-meridian, dividing the world into two hemispheres, with the agreements sanctioned by papal bulls from Spanish-born Pope Alexander VI. There is a story that the king of France, visibly angry, asked the Pope which clause of Adam's will referred to this division. This would perhaps not have presented an insoluble problem, for according to the abbé de Vallemont, a seventeenth-century French intellectual, the Spaniards said that Adam had been the first king of Spain, and that in the Middle Ages they had set the first meridian in Toledo because at the moment of creation God had placed the Sun over Toledo.[12] This most likely referred to the Toledo of the three cultures (Jewish, Christian and Islamic) and the *Tablas alfonsíes*, which as early as the thirteenth century contained the movements of the planets, Sun and Moon.[13] Considering for a moment that image of the Sun over Toledo on the day of creation, or of Adam as the first king of Spain, one can see that the British are not the only people to have placed themselves at the centre of the world (nor are the French the first to have mocked the alleged superiority, centrality or priority of their neighbours and rivals).

In any event, the Portuguese and the Spanish continued their litigations into the eighteenth century, sending experts and commissions to carry out triangulations and measurements in the jungles of the Orinoco and Amazon. Yet there was no way to agree the position of the meridian 370 leagues west of Cape Verde, the line stipulated by the Treaty of Tordesillas, even in 1750 when the Treaty of Madrid, revised by the Treaty of San Ildefonso in 1777, attempted to fix the boundaries between Spanish and Portuguese territories in Brazil. Against this background of frontier rivalries, there were conflicts over the possession of the colony of Sacramento, opposite Buenos Aires on the River Plate, and the Jesuit Reductions in Paraguay, an episode depicted in the film *The Mission* (dir. by Roland Joffé, 1986). These conflicts generated considerable scientific activity.[14] Expeditions to demarcate Spanish-Portuguese boundaries saw the development of the geographical work of Francisco Requena, the botanical explorations of Pehr Löfling (one of the principal disciples of Linnaeus), the natural historical work of Félix de Azara and the first astronomical observations of Juan de Lángara, a mariner who, as will be seen, was to play a leading part in an important chapter in the determination of longitude at sea.

On the other side of the world it is also interesting to see what happened in Australia, a continent divided in two before it was even discovered. When New South Wales was founded in 1788, Arthur Phillip, the first governor, took possession of a territory whose limits were Cape York to the north, Van Diemen's Land (Tasmania) to the south and a meridian placed 17° from the Moluccas, just on the Zaragoza anti-meridian, to the west. Although the French had disputed Adam's will in the fifteenth century, now, at the end of the eighteenth, the British were claiming it for themselves.[15] Just as the Dutch had acquired a large part of the colonial space dominated by the Portuguese, the British, in a sense, were hoping to replace the Spaniards in theirs, including the Pacific, the former 'Spanish lake'.

Years before, a seminal experiment in the history of Spanish science had taken place. In 1736, two young mariners, Jorge Juan and Antonio de Ulloa, were included in the geodesic expedition to Quito, in the Viceroyalty of Peru. The Académie des sciences in Paris was launching a worldwide experiment. While Maupertuis was measuring a degree of the meridian in Lapland, Godin, Jussieu and La Condamine were doing so near the Equator. The aim was to resolve the controversy over the shape of the Earth, one of the battles between Cartesians and Newtonians: the former thought the Earth was *prolate*; the latter declared that it must be *oblate*.

Thanks to the works of Lafuente and Safier, we now know much about the expedition to Peru; for example, the native sources for the cartography and pharmacopeia that circulated as far as the *Encyclopédie*, and the difficulties of carrying out spherical trigonometry in the Andes.[16] The instruments expanded and chromatic aberration made observations difficult. They had to be repeated time and time again. There were wars of figures and calculations, and conflict between participants on their return. In his research into the relationships between culture, psychology and anthropology in the context of maritime navigation, Hutchins echoed the declaration of the Nobel prize-winner Herbert Simon: 'Solving a problem simply means representing it so as to make the solution transparent'.[17] In the mid-eighteenth century, measuring a degree of a meridian was a problem whose solution was far from transparent.

The expedition may not have offered conclusive proof of the true shape of the Earth, but it did have other consequences. Juan and Ulloa returned to Spain as converts to the language of Newtonian mathematics and astronomy, and tried to prompt scientific activity from the state. This was in the mid-century, just as Father Murillo was joking that the Equator and tropics would end up passing through London or Paris. Murillo made another interesting comment: 'We Spaniards do not make systems, because we do not make maps, and we follow the system of those maps that we have'.[18] Juan and Ulloa understood the need to make *systems*, that is, to create institutions, qualified personnel and continuous practice. It was necessary to revive a scientific tradition that had badly deteriorated.

In the middle of the eighteenth century, Spain lacked a modern map of the Iberian Peninsula and her American dominions. The authorities tried to obtain them by various means. First, they sent young men to be educated in Paris. One of these was Tomas López (1730–1802), who spent eight years in the Collège Mazarin and the workshop of Bourguignon d'Anville. On his return in 1760, he set to work preparing a map of Spain whose story is significant. As an official geographer, López worked for decades on various source materials, attempting to reconcile inconsistent information from parishes and town halls, old maps and books that used different scales and methods. The astronomical coordinates were unreliable. López was unable to complete the map in his lifetime: his sons had to do so. The story sounds like something from Greek myth or a tale by Borges, the result a fragmentary mosaic.[19] To draw the map of a country it was necessary to have a network of reliable correspondents, standardized instruments and a consistent programme of observations. It needed an institution and a sustained plan: in a word, a *system*. Indeed, in one of his theoretical texts, López, noting the lack of international consensus concerning the establishment of a prime meridian, declared:

> It would be greatly advantageous for all, to agree on one unique prime meridian and fixed point on the globe through which the prime meridian should pass, but on this matter men of the most diverse ambitions are in dispute, not even those of the same nation being in agreement.[20]

A similar fate befell the *Mapa geográfico de America Meridional* (1775) produced by Juan de la Cruz Cano (1734–90), another of the students sent to Paris. It was also an old-fashioned map, but it was to be of the whole of South America. Yet the government rejected it, since it jeopardized Spanish interests in its conflict with Portugal, and prohibited its publication.[21]

It was in this environment that Jorge Juan and Ulloa argued for and supported the creation of several scientific institutions. One of these was the Real Observatorio de Cádiz, founded in 1753, together with the Academia de Guardias Marinas. Jorge Juan gained the support of Prime Minister Ensenada and appointed Louis Godin as director of the Observatorio. He had astronomical instruments bought, mostly from Britain: pendulums by Ellicott, an achromatic telescope by Dollond, reflecting telescopes by James Short and Edward Nairne, a quadrant by George Adams and above all a mural quadrant from John Bird, like the one in Greenwich and similar to that used by Tobias Mayer in Göttingen. The mural quadrant was a fundamental piece of equipment, a costly instrument whose installation was as complicated as it was decisive for future programmes of observations.[22]

Jorge Juan (1713–73) was director of the Academia and wrote several books on naval construction and astronomical navigation. The first, his *Examen marítimo* (1771), may be considered one of the greatest texts of its time on fluid mechanics. The second, the *Compendio de navegación* (1757), later corrected and enlarged by other mariners and known as *Las Lecciones de navegación,* became the reference manual for several generations of midshipmen.[23] Published within a year of his death, his *Estado de la astronomía en Europa* (1774) recounted progress over the previous 20 years in the astronomical and chronometric determination of longitude.

Years before, he and his colleague Antonio de Ulloa had published the *Observaciones astronómicas y físicas* (1748), which detailed the operations carried out in the Andes to determine the shape and size of the Earth (Figure 2.1). At that time, Jorge Juan was living in London, where he was performing a role that extended from industrial espionage in the Thames

Figure 2.1 Frontispiece from Jorge Juan and Antonio de Ulloa, *Observaciones astronomicas* (Madrid: Juan de Zuniga, 1748) © National Maritime Museum, Greenwich

dockyards to academic and diplomatic work. While obtaining secret information on British movements in the South Seas and trying to hire qualified artisans and carpenters (like the Catholic, Richard Rooth), he was also accepted as a Fellow of the Royal Society. During those years, he learned first-hand of Short's improvements in the construction of reflecting telescopes and of Harrison's advances with his third sea clock, 'H3'.[24]

After his stay in London, and then as director of the Academia in Cádiz, Jorge Juan undertook a systematic programme of observations from the Observatorio and reformed the syllabus, introducing algebra and geometry as indispensable parts of the mariner's training. Following a model that in certain aspects imitated the French (in being more bureaucratic and centralized) and in others the British (like Greenwich, the Real Observatorio was oriented towards nautical astronomy), Jorge Juan set the foundations of a *system*, the requisite for drawing a map.[25] In other words, he put Cádiz on the map.

Charting the empire, or putting Cádiz on the map

Between 1765 and 1774, Cádiz began to be included among the ports of call for French travellers who were practising with marine timekeepers and lunar distances. In 1768, Jean-Dominique Cassini landed there, in 1768–69 the comte de Fleurieu, and in 1771–72 *La Flore*, on which Verdun de la Crenne, Alexandre-Guy Pingré and Jean-Charles de Borda were bound for the Canary Islands to fix the position of the Pico del Teide and verify conventional methods of navigation.[26]

In addition, several sailors trained at the Cádiz academy began to take part in astronomical experiments at an international level. In 1769, for example, Vicente Doz and Salvador de Medina travelled with Chappe d'Auteroche to Baja California to measure the transit of Venus.[27] Joaquín Velázquez de León, another astronomer from New Spain (in other words, a *criollo*, or Spaniard born in America), collaborated with them. As mentioned previously concerning the relationship between Iberian monarchies in the Renaissance, the nation can be an inappropriate category for analysing the history of science. The same is true here, bearing in mind the contributions made from New Spain in colonial times.

Velázquez de León, mentioned above, Ignacio Bartolache and Antonio León y Gama, also New Spaniards, should find a place in any summary of Enlightenment astronomy, however brief. León y Gama combined his observations of the skies with a passion for archaeological remains. In his pioneering work on the Coatlicue and the Sun Stone, two colossal sculptures exhumed from the main square of Mexico City in 1790, he sought to dovetail pre-Columbian and European history by comparing the uses of their respective observations of eclipses. This was a felicitous idea: natural phenomena and science as the main thread of the histories of the Old and New Worlds.[28] This was, incidentally, at the time when Kant and Herder were outlining their ideas for a cosmopolitan or universal history.

Regarding voyages directly related to the determination of longitude, however, one should look at those Juan de Lángara (1736–1806) undertook to the Philippines between 1765 and 1773.[29] After the capture of Manila by the British in 1762, Spain decided to use the route around the Cape of Good Hope, instead of crossing the Pacific from Acapulco or Lima. The end of the Seven Years' War in 1763 marked the moment when the Pacific became part of the strategy of the European nations.

On the first of his voyages to Manila aboard the *Buen Consejo* (1765–67), Lángara had already acknowledged the problems of determining longitude at sea, even in ports of reference such as Rio de Janeiro. In his view, it was necessary 'to have in the ports master [azimuth] compasses which should be compared with those that are taken to sea for such voyages'.[30] After rounding the Cape of Good Hope, the *Buen Consejo* reached Manila, where, in collaboration with French astronomer Guillaume Le Gentil, Lángara calculated longitude from the immersions of Jupiter's satellites.[31] He twice returned to the same destination aboard the frigate *Venus*, in 1769–70 and 1771–73. It was on the second voyage that the lunar distance method was used for the first time on a Spanish ship, performed by Lángara and José de Mazarredo (1745–1812), another young seaman destined for a brilliant future in the Spanish Navy. They had tried to obtain copies of the *Nautical Almanac* in Gibraltar. Having failed, they performed their calculations based on their own observations, measuring the angular distances between the Moon and the eye of Taurus (Aldebaran). This took place in February 1772 near the archipelago of Trindade e Martim Vaz in the South Atlantic. Days later, they acquired a *Nautical Almanac* in the Dutch colony of Table Bay (Cape of Good Hope).[32] Mazarredo would introduce the lunar distance method to Spanish mariners. He published Jorge Juan's *Lecciones de navegación*, adding a 100-page chapter on the calculation of longitude. Although it was only published in 1790, the book had been circulating among students in manuscript since 1777.

Worse befell one of the most innovative reports produced in the Iberian world, a work that would have been ground-breaking if it had been published; yet it remained unpublished and unknown until recently. The report outlined the lunar distance method and proposed a collective programme for the preparation of tables of lunar movements at different geographical locations.[33] It was written in 1767, probably in Brazil, by the Portuguese Jesuit José Monteiro da Rocha (1734–1819), mathematician, astronomer and right-hand man to the Marqués de Pombal in his reforms of the University of Coimbra, where he founded the Observatório Astronómico in 1772 and published astronomical almanacs from 1803. The story of Monteiro da Rocha confirms some of the constant features of Iberian science: the leading role of the Jesuits in mathematics and astronomy before their expulsion by reformist governments that transferred institutional responsibility for these subjects to the military and, to a lesser extent, the universities; the invisibility of many of their achievements because they were not published; and the need for the two Iberian countries to produce their own almanacs and nautical tables, their own methods, and to calculate distances and make measurements that placed Coimbra or Cádiz at the centre of their operations.[34]

Teaching reforms in the Spanish Navy culminated years later in the introduction of a Higher Studies Course in Cádiz (1783). The four-year course trained a generation of scientific officers well versed in algebra, spherical trigonometry, the calculation of longitude and nautical astronomy, who were to play a leading role in the huge programme of hydrographic surveys in America and the Pacific in the last 20 years of the century. This reformist plan and the pedagogical renewal inspired by Jorge Juan in the middle of the century thus bore its best fruit 30 years later. His intellectual heirs, led by the Secretario de Marina e Indias, Antonio Valdés, were able to create the teaching structure – the *system* – needed for the vast programme of hydrographic reconnaissance in America and the Pacific.[35]

Initially, the outline of the Iberian Peninsula was mapped. Vicente Tofiño (1732–95), a mariner trained in the time of Jorge Juan, and already a prestigious astronomer throughout Europe and director of the Academia de Guardia Marinas, directed the operations that culminated in the *Atlas marítimo de España* (1787).[36] Under his command were Dionisio Alcalá Galiano, José Espinosa y Tello, José de Lanz and Alejandro Malaspina, later to be distinguished figures in their own right. They followed the system used by Picard and La Hire for mapping France, combining terrestrial and maritime operations. They measured time with the help of eight Berthoud watches belonging to the Observatorio. They also used instruments from one of the six shipments acquired by João Jacinto Magalhães, the Portuguese polymath based in London since 1763, for the Spanish Navy (including achromatic telescopes, quadrants, pendulums).[37] The Navy thus managed to produce the first truly modern map of the Iberian Peninsula, something achieved neither by the Jesuits of the Seminario de Nobles (by now expelled), nor by the programme in Ensenada's time of sending young recruits to study abroad (as was the case with Tomás López, mentioned previously).

It was not long before Tofiño's project spread to America and the Pacific, notably with the Malaspina expedition between 1789 and 1794. This was the most encyclopaedic expedition ever mounted, for aboard the corvettes *Descubierta* and *Atrevida* they practised not only astronomy, geography and botany, but also ethnography, political economy and comparative history.[38] The expedition included many of the men Tofiño had chosen for the *Atlas de España* and many of the same instruments. The objective was to map the coast from Cape Horn to Alaska and the Philippines, as well as to chart courses for merchant shipping. It would take too long to give details of the results here, but suffice to say that of the three volumes of the catalogue prepared by María Dolores Higueras, one is wholly devoted to the astronomical and hydrographic work.[39] Felipe Bauzá, Espinosa y Tello, Alcalá Galiano and the other scientific officers produced a wealth of cartographic work consisting of 240 drafts, 182 charts in different stages of completion, and 28 magnificent copper-plate maps.

As was now becoming more common, the officers of the *Descubierta* and *Atrevida* measured longitude with a combination of astronomical means and mechanical instruments (timekeepers). They had chronometers by Arnold and Berthoud, a Ramsden astronomical quadrant, sextants from Wright and Stancliffe. They observed the eclipses of Jupiter's satellites, the occultation of stars by the Moon, and so on. And of course, they took copies of the *Connaissance des temps* and the *Nautical Almanac*. It was at this time that Cádiz began to produce its own astronomical almanacs and tables, taking the Observatorio as the prime meridian. Meanwhile, the Malaspina expedition was fixing the position of the American coast in relation to Cádiz, the new southern meridian, by establishing temporary observatories (Figure 2.2) in locations from Montevideo to Nootka, as well as Bennelong Point, now the site of Sydney Opera House, and in Vava'u.[40]

Between 1785 and 1810, Spain organized many hydrographic expeditions to the Northwest Coast, the Straits of Magellan and the Caribbean, the former two as frontier regions of Spanish dominion, the third as the principal hub for transatlantic trade. The explorations of the Northwest Coast included those headed by Francisco Bodega y Quadra, the Lima-born mariner who acted as Spanish representative in the negotiations with George Vancouver after the Nootka crisis of 1789, and that led by Alcalá Galiano and Cayetano Valdés in command of the schooners *Sutil* and *Mexicana* (1792). Voyages in the far south included the four of José de Moraleda that between 1787 and 1796 charted the intricate archipelago of Chiloé and Patagonia. The Gulf of Mexico and the Caribbean were covered by the Atlas de la América septentrional (1792–1810), a hydrographic project designed by Mazarredo and led by Cosme de Churruca (1761–1805), another of the outstanding mariners of that generation. Alexander von Humboldt made great use of the project's results.[41] The Spaniards – although not always – were also able to take advantage of their own tradition of exploration, as with the magnificent maps based on these voyages, which Felipe Bauzá (1764–1834), formerly responsible for the charts and maps of the Malaspina expedition, had engraved once he was appointed as director of the Depósito Hidrográfico in 1797, a position he held until his exile in 1823.

Spanish sailors made some remarkable contributions in the field of the calculation of longitude at sea. Gabriel Císcar (1769–1829), for instance, one of the minds behind the Higher Studies Course and perhaps the best mathematician in the Spanish Navy, wrote several texts on astronomical navigation. He was also responsible for introducing a new system of weights and measures into Spain. In 1803, he published his most

Figure 2.2 ‘Las experiencias de la gravedad en Puerto Egmond de las Islas Malvinas’, showing Malaspina (right) and his assistant, pencil drawing, by Juan Ravenet, 1794 (España. Ministerio de Defensa. Archivo del Museo Naval AMN Ms.1723 (19))

valuable contribution to longitude literature, a graphical method for correcting lunar distances.[42]

The outstanding Spanish figure in the field of longitude research was José Mendoza y Ríos (1762–1816), a mathematician and astronomer trained in the Seminario de Nobles and in the Navy, whose *Tratado de navegación* (1787) ultimately replaced the manuals of Jorge Juan and Mazarredo.[43] Like them, Mendoza y Ríos was aware of the need to 'make systems', as well as of the deficiencies of Spanish technology for astronomical navigation: in other words, of dependence on texts and instruments manufactured abroad. From the 1780s, Mendoza y Ríos undertook a project known as the 'Spanish Maritime Library', with the intention of preserving a collection of maps and scientific instruments and providing a place for discussion and the advancement of the navigational sciences. This project was the forerunner of the Depósito Hidrográfico, created after the return of the Malaspina expedition and taking advantage of the rich material it had collected in Spanish America. This in turn was the precursor of the Museo Naval, Madrid.

In 1789, Mendoza y Ríos was sent abroad to obtain information that might be useful to the Spanish Navy and to make contact with intellectuals, booksellers, mapmakers and instrument-makers. First, he went to Paris, taking with him a young colleague, José de Lanz, later a well-known mathematician and engineer, and author of an important monograph on machines.[44] In 1792, Mendoza y Ríos formed part of the Hispano-French commission to undertake calculations on the measurement of the arc of the Paris meridian, the campaign that led to the creation of the metre. However, the Revolution obliged him to move to London, where he established contacts in scientific circles and with instrument-makers, and settled for the rest of his life. In London, he acted as the Spanish Navy's representative, becoming an authority on nautical astronomy. He had already published in the *Connaissance des temps* for 1791 a work on the determination of latitude by measuring two heights of a star and the period between the observations. Once in London, he was accepted in 1793 as a Fellow of the Royal Society, proposed by Joseph Banks and seconded by Henry Cavendish, Nevil Maskelyne and James Watt. He published two works on astronomical position-finding at sea, which included new advances in the method of lunar distances, the first printed in Madrid (1795), the second in the *Philosophical Transactions* (1797).[45] In subsequent years, he designed a set of navigation tables, including a new method of clearing the lunar distance, simplifying the laborious calculations with logarithms and natural sines, using the haversine formula for the first time. He published these tables first in

Spanish (1800) and then in English (1801), a publication that was later expanded and published in two further editions (1805 and 1809).[46] As noted previously, it was necessary to make the lunar distance method into a truly transparent solution. However, it was also necessary to make it simpler. Delambre praised Mendoza y Ríos's ingenious tables, although he complained that they were 70 pages long.[47] Transparency, simplicity, portability: simplicity may be the seal of truth, but portability was the only guarantee of effective circulation.

Mendoza y Ríos's other contribution was technological, in suggesting further improvements to the reflecting circle, an instrument previously used, following modifications by Jean-Charles de Borda, to measure the meridian arc. Mendoza's suggestions included a 'flying nonius', a divided circular ring that could be rotated within the degree scale to allow more precise reading of the scale, and a new design for the handle to allow the instrument to be used in any position. He published his ideas, 'On an Improved Reflecting Circle', again in the *Philosophical Transactions*.[48]

The decline of the southern meridian

Yet all this work to create an institution, to train experts, to rationalize and submit the empire to the engraver's tools and to geometry, came to nothing in the following years. When the Malaspina expedition returned to the Iberian Peninsula, Spain was at war with France. Malaspina was condemned for his liberal views, imprisoned and ultimately exiled. The material produced by the expedition was censored, and remained unpublished for nearly a century. The only work that saw light, and that after some delay, was the *Memorias sobre las observaciones astronómicas en distintos lugares del globo*, published by José Espinosa y Tello, the only material felt to be uncontaminated by political ideas.[49] The neutrality of astronomy was always a widespread fiction. Nonetheless, herein lay the details of the scientific operations carried out by the Spanish officers, the foundations for the most complete and accurate portrait of the Spanish Empire. Geographical locations in longitude from the Philippines to Cape Horn were fixed in relation to Cádiz. It was the swansong of the Empire. The following year the revolution broke out and the Emancipation movement began in Latin America.

The biographies of these mariners reflect the fate of nautical astronomy in the years of the Napoleonic Wars. Many, including Alcalá Galiano and Churruca, who died at the battle of Trafalgar in 1805. Others, including Espinosa y Tello and Mendoza y Ríos, fled to London during the Napoleonic invasion, although the latter's exile was more or less voluntary. But the fact is that Spain suffered a considerable brain drain of scholars and intellectuals, for soon there was another exile, that of the *afrancesados*, the 'Frenchified', who escaped when absolute monarchy was reinstated in Spain in 1814. This was the case for José de Lanz, Mendoza y Ríos's outstanding disciple, and Felipe Bauzá, who ended up in London, where he was buried in Westminster Abbey, while his fabulous collection of maps stayed forever in the British Library, despite his wish that it be returned to Spain.

The Real Observatorio de Cádiz went into decline. It was even mooted that it might close, with its activities transferred to the Observatorio Astronómico de Madrid, a fine building by the neo-classical architect Villanueva, located in the Retiro Park, Madrid's answer to London's Hyde Park, next to the Real Jardín Botánico and the Museo del Prado. The Prado was another magnificent building by Villanueva, though not yet a famous art gallery, having been designed to house a scientific and technological complex that, significantly, never came into being. These three scientific establishments in the heart of Madrid were looted during the French invasion of 1808. Nothing remained of the fabulous 25-foot Herschel telescope that the Observatorio Astronómico had housed, and which had been acquired through the mediation of Mendoza y Ríos.[50]

In Cádiz the observatory buildings were abandoned: the Oficina de Efemérides, the watchmakers' workshop and the instrument repair shop. The collapse of the institution that had introduced modern mathematics, physics and astronomy into Spain is eloquent. An inventory from 1827 reveals that of 431 instruments, 21 were old, unsaleable but particularly useful, 74 saleable and deteriorated, 100 missing and 104 old, useless and only saleable as scrap.[51] If the diaspora of the mariners reflects the atomization of the Enlightenment scheme, the deterioration of the Cádiz instruments, 'moth-eaten, with their locks, brackets and hinges rotten with mould', represents the paralysis of that institutional model. [52]

All this leads us to conclude with a comment from a Spanish anthropologist who had the accuracy of an astronomer. Julio Caro Baroja said that Spain, contrary to the stereotype, was not a conservative country:

> The idea that Spain is a traditionalist and conservative country is one of the falsest, and one that has been repeated for many years. Rather, Spain is a country that destroys and devours its past and the sons it bears, in successive generations. It is a sort of Saturn, as depicted by Goya in one of his most terrifying paintings.[53]

Perhaps this is why we find it so commendable that the Board of Longitude project has cast 'a vivid light on the role of the British state in encouraging invention and discovery'.[54] Although no scientific tradition has a single centre, nor a single address, or perhaps because of that, and because somehow we are all projected in certain places, one cannot avoid thinking of Greenwich as a central and admirable place, even though the Equator, until some catastrophic event corrects the situation, does not yet pass through it.

Notes

Research Projects HAR2010–15099, HAR2014–52157-P.

1. Pedro Murillo Velarde, *Geographia histórica*, 10 vols (Madrid: Oficina de Gabriel Ramírez, 1752), I, 13.
2. This is the classic phrase that the map historian John B. Harley borrowed from the cartographer Philip C. Muehrcke and popularized in John B. Harley, 'Silence and Secrecy: The Hidden Agenda of Cartography in Early Modern Europe', *Imago Mundi*, 40 (1988), 57–76, later included in John B. Harley, *The New Nature of Maps. Essays in the History of Cartography* (Baltimore: The Johns Hopkins University Press, 2001).
3. David Livingstone, *The Geographical Tradition. Episodes in the History of a Contested Enterprise* (Oxford: Blackwell, 1992).
4. Dava Sobel, *Longitude* (New York: Walker & Co., 1995), a readable book that has made our subject widely popular.
5. Richard Dunn and Rebekah Higgitt, *Finding Longitude: How Ships, Clocks and Stars Helped Solve the Longitude Problem* (Glasgow: Collins, 2014), p. 9.
6. I refer to the phrase coined by C. A. Bayly, *The Imperial Meridian. The British Empire and the World, 1780–1830* (London & New York: Longman, 1989); see also David Todd, 'A French Imperial Meridian, 1814–1870', *Past and Present*, 210 (2011), 155–86.
7. On Pedro Nunes, see the works of Henrique Leitão, including his edition of his complete works: Pedro Nunes, *Obras*, ed. by Henrique Leitão, 6 vols (Lisbon: Academia das Ciências, Fundação Calouste Gulbenkian, 2002–12); Henrique Leitão, 'Maritime discoveries and the discovery of Science: Pedro Nunes and Early Modern Science', in *Más allá de la Leyenda Negra: España y la Revolución Científica. Beyond the Black Legend: Spain and the Scientific Revolution*, ed. by Víctor Navarro and William Eamon (Valencia: CSIC, 2007), pp. 89–104.
8. The latest contributions from a particular establishment to the history of institutions relating to cosmography in Renaissance Spain include Maria Portuondo, *Secret Science: Spanish Cosmography and the New World* (Chicago: University of Chicago Press, 2009); Arndt Brendecke, *Imperio e Información. Funciones del saber en el dominio colonial español* (Madrid: Iberoaméricana, 2012); Antonio Sánchez, *La espada, la cruz y el padrón* (Madrid: CSIC, 2013).
9. On the 'black legend', see Joseph Pérez, *La leyenda negra* (Madrid: Gadir, 2009); Ricardo García Cárcel, *La leyenda negra: historia y opinión* (Madrid: Alianza, 1998).

10. Patricia Seed, 'Latitude' <http://www.ruf.rice.edu/~feegi/> [accessed 27 February 2015] shows how much of the world was discovered thanks to the knowledge of latitude and of longitude: the latter is much smaller.
11. Alison Sandman, 'Latitude, Longitude, and the Ideas about Utility of Science', in *Más allá de la Leyenda Negra*, ed. by Navarro and Eamon, pp. 371–81.
12. Vallemont's comment is included in Tomás López, *Principios geográficos aplicados al uso de los mapas* (Madrid: Imprenta de Joaquín Ibarra, 1775), p. 36.
13. Laura Fernández, *Arte y ciencia en el scriptorium de Alfonso X el Sabio* (Seville: Universidad de Sevilla, 2013).
14. José Sala, 'La ciencia en las expediciones de límites hispano-portuguesas: su proyección internacional', *Dynamis*, 12 (1992), 23–33; Manuel Lucena Giraldo, *Laboratorio tropical: la expedición de límites al Orinoco (1750–1767)* (Caracas: Monte Avila-CSIC, 1993); Tamar Herzog, *Frontiers of Possession: Spain and Portugal in Europe and the Americas* (Cambridge, MA: Harvard University Press, 2015).
15. Juan Pimentel, *En el Panóptico mar del sur* (Madrid: CSIC, 1992), pp. 39–55.
16. Antonio Lafuente and Antonio Mazuecos, *Los caballeros del punto fijo. Ciencia, política y aventura en la expedición geodésica hispanofrancesa al Virreinato del Perú en el siglo XVIII* (Barcelona: Serbal, 1987); Neil Safier, *Measuring the New World. Enlightenment Science and South America* (Chicago: University of Chicago Press, 2008).
17. Quoted in Edward Hutchins, *Cognition in the Wild* (Cambridge, MA: MIT Press, 1995), p. 117.
18. Murillo, *Geographia histórica*, I, 13.
19. Carmen Líter, *La obra de Tomás López: imagen cartográfica del siglo XVIII* (Madrid: BNE, 2002).
20. López, *Principios geográficos*, p. 35.
21. Horacio Capel, *Geografía y matemáticas en la España del siglo XVIII* (Barcelona: Oikos-Tau, 1982), pp. 186–94.
22. Antonio Lafuente and Manuel Sellés, *El Observatorio de Cádiz (1753–1831)* (Madrid: Instituto de Historia y Cultura Naval, 1988), pp. 171–81; Francisco J. González, *Instrumentos científicos del Observatorio de San Fernando en los siglos XVIII, XIX y XX* (Madrid: Ministerio de Defensa, 1995); Francisco González, 'Péndulos astronómicos y cronómetros marinos de la Armada: El Observatorio de San Fernando y los antecedentes del patrón nacional de tiempo (1753–1957)', *Asclepio*, 50.1 (1998), 175–98.
23. Nuria Valverde, *Un mundo en equilibrio. Jorge Juan (1713–1773)* (Madrid: Fundación Jorge Juan-Marcial Pons, 2012), pp. 135–41.
24. Valverde, *Un mundo en equilibrio*, pp. 87–126.
25. Lafuente and Sellés, *El Observatorio de Cádiz*, pp. 83–91.
26. Manuel Sellés, *Navegación astronómica en la España del siglo XVIII* (Madrid: UNED, 2000), pp. 217.
27. Salvador Bernabeu, 'La comisión española en la expedición de Chappe d'Auteroche', in *Ciencia, vida y espacio en Iberoamérica*, ed. by José Luis Peset, 3 vols (Madrid: CSIC, 1989), III, 15–35; Salvador Bernabéu, *Las huella de Venus. El viaje a Nueva España de Chappe d'Auteroche (1767–1768)* (México: Breve Fondo Editorial, 1998).

28. Juan Pimentel, 'Stars and Stones. Astronomy and Archeology in the works of the Mexican polymath Antonio León y Gama (1735–1802)', *Itinerario*, XXXIII (2009), 61–77.
29. Salvador Bernabéu, 'Ciencia ilustrada y nuevas rutas: las expediciones de Juan de Lángar al Pacífico, 1765–1773', *Revista de Indias*, 47.180 (1987), 447–67.
30. Quoted in Bernabéu, 'Ciencia ilustrada y nuevas rutas', p. 456.
31. On this subject, see Albert Van Helden, 'Longitude and the Satellites of Jupiter', in *The Quest for Longitude*, ed. by William J. H. Andrewes (Cambridge, MA: Collection of Historical Scientific Instruments, 1998), pp. 85–100.
32. Bernabéu, 'Ciencia ilustrada y nuevas rutas', p. 462; Lafuente and Sellés, *El Observatorio de Cádiz*, p. 167; Capel, *Geografía y matemáticas*, p. 237.
33. Jose Monteiro da Rocha, 'Methodo De achar a Longitude Geográfica no mar y na terra Pelas observaçõens y cálculos da Lua Para o uso da Navegação Portugueza' (Method for finding geographic longitude at sea and on land by observations and calculations of the Moon for the use of Portuguese navigation), Ms. 511 da Colecção Pombalina, Biblioteca Nacional, Lisbon.
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36. Capel, *Geografía y matemáticas*, pp. 239–54.
37. Thomas F. Glick, 'Imperio y dependencia científica en el XVIII español e inglés: La provisión de los instrumentos científicos', in *Ciencia, vida y espacio en Iberoamerica*, ed. by José Luis Peset, 3 vols (Madrid: CSIC, 1989), III, 59–63; Isabel Maria Malaquias and Manuel Fernandes Thomaz, 'Scientific communication in the XVIIIth century: The case of John Hyacinth de Magellan', *Physis*, 31.3 (1994), 817–34.
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49. José Espinosa y Tello, *Memoria sobre las observaciones astronómicas hechas por los españoles en distintos lugares del globo, que han servido para formación de las cartas de marear*, 2 vols (Madrid: Imprenta Real, 1809).
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51. Lafuente and Sellés, *El Observatorio de Cádiz*, p. 431.
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54. Board of Longitude, Cambridge Digital Library <http://cudl.lib.cam.ac.uk/collections/longitude> [accessed 27 February 2015].

3

The Longitude Committee and the Practice of Navigation in the Netherlands, *c.* 1750–1850

Karel Davids

In 1826, the Netherlands switched meridians. Officially, the meridian of Greenwich became the prime meridian for Dutch seafarers rather than that of Tenerife. The shift was the result of advice from the Longitude Committee to the Secretary of the Navy. The Committee, which the Admiralty of Amsterdam originally established in 1787 as the 'Committee concerning matters relating to the determination of Longitude at Sea and the Improvement of Charts',[1] argued that other seafaring nations no longer used the meridian of Tenerife as their reference and that Dutch seafarers also rarely used it as a prime meridian. Naval officers had already switched to the Greenwich meridian because they often used British charts.

Since 1815, charts published by the Longitude Committee had indicated longitude relative to Paris as well as Greenwich and Tenerife. By the 1820s, the Committee considered the proliferation of prime meridians 'ridiculous' and a source of 'error, uncertainty and inaccuracy'. If nations would only decide on a common prime meridian, it continued, the meridian of the Peak of Tenerife would doubtless be the best candidate since it would allow 'a good division of the globe': Europe would lie in the eastern hemisphere, the Americas in the western. But as this outcome was hardly to be expected, the choice was really between the meridians of Greenwich and Paris. Furthermore, since 'England was the first seafaring nation' and most charts and relevant nautical reports came from there, the Committee concluded, preference should be given to Greenwich.[2] This conclusion was accepted without further debate. On 26 July 1826, King William I issued a decree: henceforth, the Greenwich meridian would be the basis of the lunar tables in the Dutch nautical almanac.[3] The first such almanac based on Greenwich was produced for 1828.

The switch of meridians in the 1820s, and the elaborate arguments deployed to justify the move, was significant for several reasons. First, it marked the formal acknowledgement that the British, rather than the French or Dutch, had become the dominant power in the field of navigation. Second, this acknowledgement concerned the recognition of an existing state of affairs rather than the perception of a supposed mastery regarding methods and theory. Third, it clearly demonstrated that navigational practice could change irrespective of rules or incentives originating from maritime authorities. In 1826, the rules were adapted to practice, not the other way round.

This chapter concerns the relationship between state-supported provision and seafarers' practice in the field of navigation in the Netherlands between about 1750 and 1850. When, how and why did Dutch state agencies become more involved in the field of navigation and what exactly did their role imply? How and to what extent did they depend on input from Britain or France? What was their impact on the practice of navigation, and how to explain this impact? To answer the first two questions it is necessary to look back to the last decades of the eighteenth century, before returning to the second quarter of the nineteenth to address the third.

Growing state involvement, *c.* 1750–1820

The creation of the Longitude Committee in 1787 was the climax of a growing concern among state agencies in the Dutch Republic with the practice of navigation, which had become evident from the middle of the eighteenth century. Around 1750, the Admiralties of Amsterdam and the Maze (which were by far the most important of the five Admiralties that existed in the Netherlands since 1597) joined with local governments and the Dutch East India Company (VOC) to found schools (zeemanscolleges) in Amsterdam and Rotterdam for training naval officers, masters and mates.[4] All the Admiralties at this time introduced statutory examinations for naval officers and mates. Each, with the exception of Friesland, appointed a permanent examiner.[5] In addition, regulations issued from the late 1740s specified the information that officers and mates had to record in their ships' journals.[6]

The establishment of the Longitude Committee was a clear sign that state agencies were prepared to substantially increase their involvement in the practice of navigation. The first Committee consisted of Jan Hendrik van Swinden, professor of mathematics, physics, astronomy, logic and metaphysics at the Athenaeum Illustre in Amsterdam, Pieter

Nieuwland, Van Swinden's star pupil, who would become lecturer in mathematics, astronomy and navigation at the Athenaeum in 1789, and Gerard Hulst van Keulen, the leading producer of nautical books, charts and nautical instruments in the Netherlands, and also an accomplished teacher of longitude-finding: in 1786, Van Keulen had taught the lunar distance method to a select group of pupils at a recently founded nautical school, the Kweekschool voor de Zeevaart, in Amsterdam.[7]

Although a single Admiralty created the Committee and all members were initially based in Amsterdam, it quickly assumed more than regional importance. A permanent correspondent at the Admiralty of Rotterdam, Jacob Florijn, was appointed in 1789 and communication with other Admiralties was established as well. After 1795, the Longitude Committee served as an agency of the newly centralized Dutch Navy. Apart from a temporary abolition between 1811 and 1815, it continued to operate until 1850.[8]

The chief tasks of the Committee were to encourage the use of new methods for determining longitude at sea by making relevant data and tools available to Dutch seamen, and to improve the accuracy of nautical charts for the benefit of naval and merchant vessels. One of its principal activities was the publication of guides and manuals on navigational matters and the translation and adaptation of the British *Nautical Almanac*, which would help Dutch seamen put the lunar distance method into practice.[9] Van Swinden was the main author of a manual on the method (*Verhandeling over het bepaalen der lengte op zee, door de afstanden van de maan tot zon of vaste sterren*) and of a treatise on the construction and use of octants and sextants (*Verhandeling over de inrichting en het gebruik der octanten en sextanten van Hadley*), published under the aegis of the Committee in 1787 and 1788 respectively. Nieuwland's and then Florijn's task was to recalculate the lunar tables in the *Nautical Almanac* for the meridian of Tenerife. When Abraham van Bemmelen, lecturer in mathematical sciences at the Fundatie van Renswoude, succeeded Van Swinden on the Committee in 1800, Florijn and Van Bemmelen undertook this task together.[10] The main reason the Committee published its own nautical almanac was that, in contrast to the 1820s, Dutch seamen still predominantly plotted their position with reference to the meridian of the Peak of Tenerife rather than that of Greenwich. The first *Almanach ten dienste der zeelieden* edited by the Committee appeared in 1788. Its publication was the climax of a process started in the late 1760s, by which the lunar distance method gradually became more widely known. In the winter of 1769–70, Pybo Steenstra, lecturer in mathematics, astronomy and navigation at the Athenaeum

Illustre in Amsterdam, gave a series of lectures on the method of finding longitude by lunar distances with particular reference to techniques for 'clearing the distance' developed in France. The text of this course appeared in print a year later, together with a model of a 'nautical almanac'. Steenstra was also the first to undertake the task of translating the British *Nautical Almanac* into Dutch, together with a set of accompanying tables, for use by Dutch seafarers. The first installment of this almanac, like that of the Longitude Committee, appeared in 1788, but the project was discontinued due to Steenstra's death in the summer of that year.[11] The Committee aimed at a much broader audience than just naval officers. It also began a series of transactions on navigational and cartographic topics, consisting of reports by Dutch naval officers and foreign seamen, as well as contributions by commissioners.[12]

The Admiralties of Amsterdam and the Maze also took decisive steps to ensure that the new navigational methods would be used on their ships. Naval officers were no longer required to purchase all the equipment for oceanic navigation themselves. From 1788, the naval authorities issued sextants and almanacs on a regular basis and occasionally supplied officers with timekeepers.[13] The Admiralty of the Maze stipulated explicitly that henceforth captains should record in their journals when they had taken lunar distances to determine longitude, and with what result.[14]

The regulations concerning navigation in the Navy were standardized after the merger of the five Admiralties into a single naval organization in 1795. Jacob Florijn, who had been examiner of officers and mates in Rotterdam since 1771, was appointed as Mathematician and Examiner-General of the Navy and promoted from correspondent to full member of the Longitude Committee as successor to Pieter Nieuwland, who had died in 1794. Teaching naval officers also became one of Florijn's duties for a while.[15] A separate institute for Navy midshipmen, provided with state-of-the-art navigational manuals and instruments (including a sextant, a marine chronometer and a copy of Ferdinand Berthoud's *Traité des horloges marines*) was finally founded in 1803.[16]

In his new position as superintendent of navigational matters from 1795, Florijn saw to it that the new schools for masters and mates of the Navy in Amsterdam and Rotterdam taught the lunar distance method, using the publications of the Longitude Committee. He also supervised the examinations of mates of the Navy, which became compulsory in 1795. These were taken before an examiner in Amsterdam or Rotterdam, with a report sent to Florijn and the examiners in turn examined by Florijn himself. Lunar distances were part of the examination for both second and first mates.[17] In order to ensure that the method was applied at sea, Florijn was further authorized to inspect all journals of naval vessels to check whether the lunar distance method had been used, by whom and with what result.[18] The rules concerning the contents of ships' journals that had been in force at the Admiralty of the Maze since 1788 were extended to the entire Navy. Finally, the Navy Board decided in 1802 to grant commanding officers a lump sum of 700 guilders to buy the instruments, charts and books needed for oceanic navigation, specified in a standard list drawn up by Florijn in consultation with the supreme commander of the Navy, Jan Willem de Winter. For each subsequent voyage, commanding officers would receive an additional 100 guilders to supplement and maintain their equipment.[19]

These changing arrangements concerning navigation related in part to the dynamics of state formation in the Netherlands and its evolving relationship with other European powers. Accurate charts and methods for finding longitude were, after all, part of the arsenal of maritime rivalry. One must also look, however, to the investment needed to put the new longitude methods into practice. Deploying lunar distances or longitude by chronometer demanded the availability of particular sets of aids, which involved larger and more prolonged investment than had been common in navigational technologies before the end of the eighteenth century. Lunar distances required the ongoing availability of a nautical almanac, containing tables of the astronomical data needed for calculating longitude, as well as a set of sextants and octants to measure angles. Buying a nautical almanac and an octant was not excessively expensive; an *Almanach ten dienste der zeelieden* cost 3 guilders in the 1790s and 1 guilder and 40 cents in the 1830s. Prices of octants in the late eighteenth century ranged between 8 and 50 guilders, depending on type, size and quality.[20] The greatest obstacles, at least in the early years, were the costs of sextants and the expenditure needed for the continued production of up-to-date almanacs.[21] A sextant cost about 150 guilders in the 1790s, which was five times the monthly salary of a naval captain (excluding his income from provisioning); in 1815, prices varied between 50 and 240 guilders, which amounted to between one fifth and one third of the monthly salary of a captain not on active duty.[22] Only the persistent efforts of salaried calculators and comparers, and of a publisher willing to bear the commercial risk, could sustain almanac production.

To apply the chronometer method, a ship should also carry three timekeepers (one alone could not be trusted). Marine chronometers were far more expensive than sextants. The prices of chronometers bought by the Dutch Navy in the 1800s ranged from 1000 to 2400 guilders. The cheapest timekeeper in the early 1850s still cost between 500 and 600 guilders.[23] Although some naval officers bought timekeepers privately, the purchase and upkeep of a single chronometer (let alone three) was beyond the means of most.[24] The adoption of the new methods for finding longitude thus required a prolonged, high-level investment that few individual navigators or private entrepreneurs were able, or willing, to make. As in Britain and France, state agencies in the Netherlands thus took on a more active role in supporting this adoption.

Links with Britain and France, *c.* 1750–1820

A key question concerns the extent to which Dutch state agencies were dependent on input from Britain or France after the middle of the eighteenth century. Like nautical almanacs, the majority of marine chronometers initially came from abroad. Of the 18 timekeepers purchased by the Dutch Navy between about 1790 and 1815, ten came from France (mostly by Berthoud and Bréguet), five from Britain and two from the Netherlands, while the origin of one chronometer is unknown. The few privately owned timekeepers were of British manufacture, Arnold being the principal supplier.[25] Yet after a while, the Dutch succeeded in developing a small, but viable, domestic productive capacity. The Amsterdam clockmaker Friedrich Knebel began producing marine chronometers in 1806; a few dozen timekeepers left his workshop in the decades that followed. After the Napoleonic period, other clockmakers in the Netherlands entered production too, notably the firms of Hohwü, Cranenberg and De Casseres.[26] Although the relative proportions of the supply of British and French chronometers to the Dutch Navy were reversed after 1815 – mirroring the ascendancy of the meridian of Greenwich over that of Paris – Dutch timepieces began to hold their own. Of 85 timekeepers in the possession of the Dutch Navy in 1835, 74 came from Britain, one from France and nine from the Netherlands.[27] Nonetheless, the reorientation towards Britain did not preclude Dutch experts from sometimes criticizing the qualities of the *Nautical Almanac*, or borrowing vital information on the regulation of chronometers from France.[28]

Octants and sextants were not only imported from Britain; from an early date, they were also made in the Netherlands. Jacobus Kley in Rotterdam, and Johannes van Keulen, Benjamin Ayres and Jacobus and Johannes van Wijk in Amsterdam began producing octants in the 1740s and 1750s. A sales catalogue issued by the Van Keulen firm in 1777 shows that seamen could choose from a variety of types and sizes, with quality and price to match.[29] About 1780, the Van Keulen firm in Amsterdam – then under the management of Gerard Hulst van Keulen – and John Cuthbertson in Rotterdam also started the production of sextants. In contrast to Cuthbertson, Van Keulen employed a dividing engine, which he bought from its inventor, London instrument-maker Jesse Ramsden at about the same time, to graduate his instruments speedily and accurately. In 1791, Van Keulen obtained a patent from the States of Holland for a dividing engine of his own design, which he put to use shortly afterwards. The total number of sextants made under Van Keulen's supervision has been estimated at 500.[30] As long-time supplier of charts and navigational aids to the VOC and a member of the Longitude Committee from 1787 until his death in 1801, with the designated role of printing and publishing the books and charts produced by the Committee, Van Keulen was in a privileged position to sell instruments to the VOC and the Navy. In the first half of the nineteenth century, other producers entered the market for sextants. The most successful at the end of the Napoleonic Wars was Jan Marten Kleman, who in 1814 landed the contract for supplying sextants to the Dutch Navy.[31]

The Longitude Committee as such did not maintain formal links with British or French institutions such as the Board of Longitude or Bureau des longitudes. Minutes of the Board of Longitude contain a few references to the work of Cornelis Douwes, instructor at the zeemanscollege in Amsterdam, who in the 1740s devised a double-altitude method to determine latitude at sea, with a set of tables that facilitated its use. In 1768, after a British officer had sent a copy of Douwes's tables from Amsterdam to London, the Board gave the Dutchman a reward of £50 'for his trouble in correcting, improving and illustrating' his 'solar tables'.[32] Nonetheless, there is no mention in the Board of Longitude archive of the establishment of the Longitude Committee in the Netherlands 20 years later.

Contacts did exist between the members of the Committee and scholars in France, but seemingly on an individual basis. Van Swinden corresponded with many of the leading lights in mathematics, astronomy, physics and natural history, including Lalande, Laplace, Delambre, Méchain, Bonnet, Grégoire and Cuvier.[33] He participated in the meeting of the International Commission of Weights and Measures in Paris in the late 1790s and had the honour of being asked to present its results to the Assemblée legislative and the Institut national des sciences et des arts in July 1799.[34] Thanks to Van Swinden's contacts with French scholars, advances in the field of longitude determination were quick to reach the Netherlands. In November 1787, for instance, Lalande informed him that Borda had just published a description of his newly improved reflecting circle. Likewise, Jean-Baptiste Joseph Delambre sent

Van Swinden a copy of the tables the Bureau des longitudes published in 1806.[35] His network served as a conduit for information in the reverse direction as well, with Van Swinden sending books of his own to the Bureau des longitudes on several occasions. A copy of the fourth edition of the manual on the determination of longitude from lunar distances and of the treatise on the construction and use of octants and sextants, which he had written as a member of the Longitude Committee, arrived at the Bureau in Paris in 1802.[36]

By contrast, Florijn, despite having no correspondents abroad, was well informed about events in his field beyond the borders of the Netherlands. He preferred to keep abreast of new developments by reading books, journals and transactions in foreign languages, mastering English and French as well as German. Florijn's translation of Rennell's observations on the ocean current near the Isles of Scilly, published as the second part of the *Verzameling van berichten* of the Longitude Committee in 1794, for example, reveals his familiarity with periodicals in English, such as the *Philosophical Transactions* and *American Museum*.[37] The inventory of his library, drawn up after his death in 1818, also shows that he possessed relevant books on the determination of longitude in French and English. Moreover, his collection of 60-odd maps and 200 separate printed charts included, alongside many items from the Van Keulen firm (almost 90 per cent), charts from Britain and France. Among the latter were charts of the islands and coasts of Scotland and England by Mackenzie, Downey, Grosvenor, Stephenson, Jefferys, Heather, and Tovey and Ginver, charts of the Mediterranean, the Baltic Sea and the coasts of France, Spain and Portugal by Knight, Watson and Hamilton Moore, and charts of Saint-Domingue and the Grand Banks of Newfoundland by French cartographers.[38] The preponderance of British materials among the cartographic aids of foreign provenance again reflects the reversal in the relative influence of Britain and France after the Napoleonic Wars. This stood in contrast to the period when Dutch naval officers had begun to consult foreign charts on their voyages across the Atlantic and along the coasts of France and Spain in the mid-eighteenth century; after Dutch charts, they chose French rather than British ones.[39]

Seafarers and state agencies, *c.* 1820–50

The VOC and the Navy were the first to feel the impact of the Longitude Committee's activities. The VOC began issuing the almanacs and manuals published by the Committee to all ships sailing to Asia in 1788. Ships' journals show that VOC navigators were applying the lunar distance method in the 1790s.[40] An increasing number of naval officers started to do so as well. By the end of the first decade of the nineteenth century, about 60 per cent of midshipmen and perhaps one third of mates in the Navy were familiar with the new method for finding longitude at sea.[41]

When the Longitude Committee was reinstated in March 1815, its assignment combined the functions of the old Committee and of the Mathematician and Examiner-General of the Navy.[42] As well as the annual publication of a nautical almanac and the improvement of charts, it now was responsible for the examination of naval officers. Johan Frederik Lodewijk Schöder, a former instructor at the institute for naval midshipmen who became professor of mathematics at the University of Utrecht in October 1815, joined Florijn and Van Bemmelen as a new Committee member. While Florijn took charge of examinations, Van Bemmelen concentrated on the almanac and Schröder concerned himself with the revision of charts. The Committee also continued the publication of its transactions, started in 1788, on topics in cartography and navigation.[43] In addition, the Navy's timekeepers were entrusted to the care of the Committee between 1824 and 1832.[44]

From the 1820s, the Longitude Committee also became directly involved in state-sponsored efforts to influence the practice of navigation in the merchant navy. In 1823, the central government of the newly formed Kingdom of the Netherlands (comprising the territories of the former Dutch Republic and the Austrian Netherlands) introduced an ambitious scheme to improve the knowledge and skills of masters and mates in the merchant marine. State-paid lecturers in mathematics and navigation, who offered free instruction to anyone wishing to be trained as a pilot for the Navy, were appointed in the four main port cities of Amsterdam, Rotterdam, Antwerp and Ostend. The courses encompassed the whole of navigation, from elementary mathematics to the determination of latitude by double altitudes, the use of sextants and timekeepers and the determination of longitude by lunar distances. In addition, the state introduced optional exams on the theory of navigation for mates in the merchant marine. These were similar to the (compulsory) ones for mates in the Navy, which were graded from fourth to first mate. The then chairman of the Longitude Committee, Schröder, drew up the regulations for the exams and public lecturers. In addition, the Longitude Committee was charged with testing the competence of newly appointed lecturers and, twice a year, examining candidates for the different ranks of mate in the Navy and merchant marine.[45]

In the northern part of the Kingdom (which split in 1830 after the Belgian Revolt), the grand scheme of 1823 ended largely in failure. An

1851 inquiry that took evidence from 168 captains of ships chartered by the Nederlandsche Handel-Maatschappij (which, as successor to the VOC, handled most of the transport of goods between Asia and the Netherlands) revealed that 106 (63 per cent) had never sat an examination. Eighteen had received a certificate from the Kweekschool voor de Zeevaart. No more than 26 per cent had passed a test of competence before examiners of the Longitude Committee or some other examining board. It also seems that merchant seafarers barely attended the public lectures in Amsterdam and Rotterdam.[46]

The limited appeal of the state examinations among merchant seamen was probably largely due to their theoretical content and the peculiar composition of the board of examiners. After Van Bemmelen's death in 1822, the Longitude Committee came to be dominated by professors of the universities of Leiden and Utrecht and of the institute for naval cadets (Koninklijk Instituut voor de Marine) in Medemblik, who had no first-hand knowledge of the merchant navy or even of seafaring. It was not until 1838 that the Minister of the Navy, defying academic expectations, chose to fill vacancies on the Committee by appointing those with more seafaring experience and/or familiarity with practices in the merchant marine. At its dissolution in 1850, the Committee consisted of a single professor, a naval officer and two lecturers in navigation.[47] One of the latter, Jacob Swart, boasted rich experience in teaching and publishing in the field of navigation: after several years as instructor at the Kweekschool voor de Zeevaart, he had become a state-paid lecturer and lecturer to the shipmasters' society, the College Zeemanshoop in Amsterdam, as well as assistant-manager of the Van Keulen firm.[48]

The fiasco of the state-sponsored project of 1823 did not, however, imply that merchant seamen were averse to change. The renewed expansion of the Dutch merchant marine after the Napoleonic Wars – from 1,097 ships in 1824, to 1,781 sailing ships and 12 steamers in 1850, to 2,397 sailing vessels and 41 steamships in 1858 – coincided with a remarkable spread of new educational facilities, a significant increase in the supply of navigational aids and an impressive drive for self-improvement among navigating personnel.[49]

Navigation schools, lectureships and special courses on navigation sprung up all over the country after 1815 – not just in the big port cities in Holland, but also in small towns and villages in Groningen, Friesland and Zeeland, on the Wadden Islands and along the coasts of the Zuyder Zee. Initiatives by local governments, private individuals and organizations of navigators satisfied the growing demand for navigational knowledge and skills, without any need for state involvement. In the early 1850s, local authorities in many towns in the maritime provinces finally instituted their own examining boards, offering exams in navigation and seamanship on a voluntary basis. The boards were composed of ship-owners, insurers and shipmasters, as well as teachers of navigation and experts in nautical astronomy.[50]

Sextants, octants, chronometers and other nautical instruments were not only purchased from abroad (mostly from Britain), but were also produced in sizeable numbers in the Netherlands. Facilities for checking and maintaining timekeepers for the merchant marine were created in Amsterdam and Rotterdam. Jacob Swart, who had already assumed this task for the Navy (taking over from the Longitude Committee) in 1832, opened an observatory for this purpose on the premises of the Van Keulen firm eight years later.[51] As assistant-manager, and from 1840 full manager, of the firm, Swart was responsible for an increased output of new or revised nautical tables, guides, tracts and manuals on navigation and seamanship intended to satisfy the increasing demand for up-to-date professional literature, which had been evident since the late 1820s.[52] After the dissolution of the Longitude Committee, he also continued, on behalf of the Van Keulen firm, the publication of the Dutch nautical almanac and of the long-running series of transactions on topics in navigation and cartography, which he turned into a fully-fledged nautical journal.[53]

Navigators in the merchant navy, meanwhile, showed equal eagerness to improve themselves. Between about 1815 and 1850, over a dozen shipmasters' societies (zeemanscolleges) were founded in the maritime provinces of the Netherlands. Originally created as mutual insurance funds for masters, these zeemanscolleges soon assumed a variety of additional functions. They subsidized schools of navigation. They founded libraries and reading rooms supplied with newspapers, journals and professional literature. They accelerated flows of information about shipping movements by introducing distinctive flags for their members and transmitting data on ships sighted at sea to ship-owners and other interested parties ashore. Lastly, they supported the advancement of science by encouraging their members to collect data on winds, currents and other marine phenomena for the benefit of the international project of meteorological and oceanographic research initiated by Matthew Fontaine Maury, Christopher Buys Ballot and others.[54] Assembling this data at sea required considerable professional skills. Shipmasters who wished to participate were instructed to take a sextant, chronometer, azimuth compass, barometer and thermometer, each of which had to be checked by experts before they left port.[55] By 1860, hundreds of merchant masters were taking an active part in the programme.

Conclusion

The Longitude Committee in the Netherlands had different functions from those of the Board of Longitude in Britain or the Bureau des longitudes in France. The Committee, originally created by the Admiralty of Amsterdam, did not assess new proposals for solving the problem of finding longitude at sea, nor did it support voyages of discovery or coordinate astronomical research. The prime purpose of Longitude Committee was to make knowledge of new methods and instruments for the determination of longitude, as well as up-to-date astronomical and cartographic data, accurately and easily accessible to Dutch seafarers.

One can explain this difference in functions by differences in the development of the state. Even though the admiralties of the Dutch Republic were established as institutions of the Union, not of the separate provinces, naval organization in the Netherlands, like the organization of the state as a whole, retained a decentralized character until the very end of the Old Regime. Powers in the Dutch Republic were to a large degree diffused among provinces, cities and corporations, such as the VOC. Central state agencies only had a limited range of functions. The Batavian Revolution of 1795 admittedly led to an increase in centralization in the state, but financial constraints long put a brake on expansion of its sphere of action. The assignment of the Longitude Committee thus remained rather modest after the Revolution.

The main sources of information for the Committee were in Britain and France, with the emphasis clearly shifting to Britain after the Napoleonic Wars. The Committee's activities had much greater impact in the Navy than in the mercantile marine, especially after its members became responsible for examining officers and mates. In the merchant marine, changes in navigational technology from the 1820s onwards owed more to initiatives from local governments, private entrepreneurs and organizations of navigators than to state initiatives. By the middle of the nineteenth century, the Longitude Committee had largely lost its relevance outside naval circles.

Notes

1. The Dutch title was 'Commissie tot de zaaken, het bepalen der lengte op zee en het verbeteren der zeekaarten betreffende'.
2. *Almanak ten dienste der zeelieden voor het jaar 1828* (The Hague: Algemeene Landsdrukkerij, 1827), pp. 212–17.

3. Index op verbalen, 1826, National Archives, The Hague (hereafter NA), Archief Marine, 1813–1900, no. 3912; verbaal, 1 August 1826, no. 334; agenda, 15 July 1826, no. 2929; the original letter from the Longitude Committee has been destroyed; verbaal, 30 November 1826, no. 341.
4. Karel Davids, *Zeewezen en wetenschap. De wetenschap en de ontwikkeling van de navigatietechniek in Nederland tussen 1585 en 1815* (Amsterdam/Dieren: De Bataafsche Leeuw, 1986), pp. 327–28.
5. Davids, *Zeewezen en wetenschap*, p. 298.
6. Ibid., p. 307.
7. J. C. M. Warnsinck, *De Kweekschool voor de Zeevaart en de stuurmanskunst 1785–1935* (Haarlem: Joh. Enschedé en Zonen, 1935), pp. 37–38.
8. G. D. Bom, *Bijdrage tot de geschiedenis van het geslacht 'Van Keulen'* (Amsterdam: H. G. Bom, H.G. 1885), pp. 61–63, 65–67, 69; Davids, *Zeewezen en wetenschap*, p. 188. The temporary abolition of the Committee was due to the annexation of the Netherlands by the French empire in 1811.
9. Bom, *Bijdrage tot de geschiedenis*, pp. 64–74.
10. Florijn to Van Swinden, 16 March, 28 March and 4 June 1803, University Library, Leiden, Ms. BPL 755.
11. Pybo Steenstra, *Openbaare lessen over het vinden der lengte op zee* (Amsterdam: Yntema en Tieboel, 1770), pp. 78–87; Pybo Steenstra, *Zeemans almanach en sterrekundige dagtafels voor het jaar 1788 (1789, 1790)* (Amsterdam: Gerard Hulst van Keulen, 1787); Pybo Steenstra, *Benoodigde tafels bij de zeemans-almanach van wijlen P. Steenstra [...] na deszelfs dood vervolgd en uitgegeven door Jac. Florijn* (Amsterdam: Gerard Hulst van Keulen, 1789).
12. This series appeared under various titles, initially as *Verzameling van berichten over gewigtige onderwerpen der navigatie*.
13. Davids, *Zeewezen en wetenschap*, pp. 186, 190.
14. Extract resolution Admiralty of the Maze, 7 October 1788, NA, Archieven Admiraliteitscolleges (hereafter AA), Aanhangsel XII (Collectie Florijn) no. 2.
15. Davids, *Zeewezen en wetenschap*, p. 328.
16. Ibid., pp. 263, 328–29.
17. Ibid., pp. 293, 328.
18. Instructie voor de examinatoren van 's Lands stuurlieden 19 January 1797, NA, Collectie P. van der Heim nr. 178 no.76; Instructie voor den mathematicus en examinator van 's Lands zee-officieren, 14 September 1795, NA, Archief Marine 1795–1813 Aanhangsel I nr. 55 article III.
19. Verbaal Agent van Marine 7 December 1801, De Winter to Florijn, 14 and 26 April 1802, copy letter Florijn to Raad van Marine, 30 April 1802 extract resolution Raad van Marine 16 June 1802. NA, AA, Aanhangsel XII (Collectie Florijn) nr. 2.
20. Davids, *Zeewezen en wetenschap*, pp. 189, 228; see also advertisement for the *Almanach ten dienste der zeelieden* in *Tijdschrift toegewijd aan het zeewezen*, 2 (1832), p. 83
21. See Jane Wess's chapter in this volume on the mathematical complexities of the method.
22. Davids, *Zeewezen en wetenschap*, pp. 189, 254–55, 257; J. R. Bruijn, 'Zeevarenden', in *Maritieme Geschiedenis der Nederlanden*, ed. by G. Asaert et al., 4 vols (Bussum: De Boer Maritiem, 1977), III, 146–90, esecially pp. 170–71.

23. Davids, *Zeewezen en wetenschap*, pp. 262–63; J. Vos, *Verhandeling over de tijdmeters* (Breda: Broese & Comp., 1857), p. 1.
24. Davids, *Zeewezen en wetenschap*, p. 186. Compare N. A. M. Rodger, *The Command of the Ocean. A Naval History of Britain, 1649–1815* (London: Penguin, 2004), pp. 382–83.
25. Davids, *Zeewezen en wetenschap*, pp. 184–86, 260–61, J. H. Leopold, 'The Third Seafaring Nation. The Introduction of the Marine Chronometer in the Netherlands', *Antiquarian Horology*, 22 (1996), 486–500.
26. Davids, *Zeewezen en wetenschap*, p. 260; Willem F. J. Mörzer Bruyns, 'The Astronomical Clocks of Andreas Hohwü: A Checklist', in *Making Instruments Count. Essays on Historical Scientific Instruments presented to Gerard L'Estrange Turner*, ed. by R. G. W. Anderson, J. A. Bennett and W. F. Ryan (Ashgate: Aldershot, 1993), pp. 454–70 (p. 456).
27. Based on H. Spek, *Tijdmeters en waarnemingshorloges van de Departementen van Marine en Koloniën in de negentiende eeuw* (Oegstgeest: MEOB, 1982). The provenance of one timekeeper is unknown.
28. See, for example, critical remarks on the *Nautical Almanac* by Jacob Swart in the *Tijdschrift toegewijd aan het Zeewezen*, 3 (1832), 124–25 and references to a report of the Bureau des longitudes on the rate of chronometers in Vos, *Verhandeling over de tijdmeters*, pp. 95–114.
29. Willem F. J. Mörzer Bruyns, *Schip Recht door Zee. De octant in de Republiek in de achttiende eeuw* (Amsterdam: Edita, 2003), pp. 110–22.
30. Mörzer Bruyns, *Schip Recht door Zee,* pp. 123–27.
31. Davids, *Zeewezen en wetenschap*, pp. 252–55.
32. Board of Longitude, 'Confirmed minutes', 11 April 1767, 2 May 1767 and 18 June 1768, Cambridge University Library, RGO 14/5 <http://cudl.lib.cam.ac.uk/view/MS-RGO-00014–00005> [accessed 23 December 2013].
33. Karel Davids, 'Florijn, Van Swinden and the Longitude Committee (1787–1818)', in *Le Bureau des longitudes (1795–1930): Context national et international*, ed. by Martina Schiavon and Laurent Rollet (Nancy: PUN-éditions universitaires de Lorraine, forthcoming).
34. Ken Alder, *The Measure of All Things: The Seven-Year Odyssey that Changed the World* (New York: Simon & Schuster, 2002), pp. 266, 422; Rob Rentenaar (ed.), *Van Swindens vergelijkingstafels van lengtematen en landmaten*, 2 vols (Wageningen: Centrum voor landbouwpublikaties en landbouwdocumentatie, 1971), II, 46–47.
35. Davids, 'Florijn, Van Swinden'.
36. Ibid.
37. Jacob Florijn, *Waarnemingen over een stroom welke dikwyls stand grypt ten westen van Scilly […] door James Rennell […] uit het Engelsch vertaald, en met eenige aantekeningen vermeerderd door Jacob Florijn* (Amsterdam: Gerard Hulst van Keulen, 1794), especially Introduction, A3 (and footnote) and Appendix.
38. Davids, 'Florijn, Van Swinden'.
39. Davids, *Zeewezen en wetenschap*, p. 224.
40. Ibid., pp. 189–91.
41. Ibid., pp. 256–57.
42. The Dutch title was Mathematicus en Examinator-Generaal van 's Lands zee-officieren.
43. Bom, *Bijdrage*, pp. 66–68, 72.

44. Elly Dekker, 'Frederik Kaiser en zijn pogingen tot hervorming van het "sterrekundig deel van onze zeevaart"', *Tijdschrift voor de Geschiedenis van de Geneeskunde, Wiskunde, Natuurwetenschappen en Techniek*, 13 (1990), 23–41 (pp. 26–27).
45. Karel Davids, 'Het zeevaartkundig onderwijs voor de koopvaardij in Nederland tussen 1795 en 1875. De rol van het Rijk, de lagere overheid en het particulier initiatief', *Tijdschrift voor Zeegeschiedenis*, 4 (1985), 164–90 (pp. 165–68).
46. F. J. A.Broeze, *De Stad Schiedam. De Schiedamsche Scheepsreederij en de Nederlandse vaart op Oost-Indië omstreeks 1840* (The Hague: Martinus Nijhoff, 1978), p. 23; Davids, 'Zeevaartkundig onderwijs', pp. 169, 175–76.
47. Bom, *Bijdrage tot de geschiedenis van het geslacht 'Van Keulen'*, pp. 67–69; Dekker, 'Frederik Kaiser', pp. 25–27.
48. Warnsinck, *De Kweekschool voor de Zeevaart*, pp. 175–86; Davids, 'Zeevaartkundig onderwijs', pp. 174, 188.
49. Karel Davids, 'Technological Change and the Professionalism of Masters and Mates in the Dutch Mercantile Marine, 1815–1914', *Collectanea Maritima*, V (1991), 282–305 (p. 282).
50. Davids, 'Zeevaartkundig onderwijs', pp. 170–74, 176, 181–86; changes in examination regulations had been discussed between the central government and the Boards of Commerce of Amsterdam and Rotterdam since 1846; Davids, 'Technological change', pp. 287–88
51. Mörzer Bruyns, 'Astronomical Clocks'; Mörzer Bruyns, 'De instrumenten van de firma Van Keulen', in *'In de Gekroonde Lootsman'. Het kaarten-, boekuitgevers- en instrumentmakershuis Van Keulen te Amsterdam 1680–1885*, ed. by E. O. van Keulen, W. F. J. Mörzer Bruyns and E. K. Spits (Utrecht: HES, 1989), pp. 61–71 (pp. 68–70); Dekker, 'Frederik Kaiser', pp. 27, 31; Karel Davids, 'Van vrijheid naar dwang. Over de relatie tussen wetenschap en zeewezen in Nederland in de 19de en vroege 20e eeuw', *Tijdschrift voor de Geschiedenis van de Geneeskunde, Wiskunde, Natuurwetenschappen en Techniek*, 13 (1990), 5–22 (p. 10).
52. Karel Davids, 'Een huis vol handboeken. Het Huis van Keulen en de vakliteratuur voor de zeeman', in *'In de Gekroonde Lootsman',* ed. by Van Keulen, Mörzer Bruyns and Spits, pp. 44–60 (pp. 55–57).
53. Bom, *Bijdrage tot de geschiedenis*, pp. 26, 69, 72–73; Warnsinck, *De Kweekschool voor de Zeevaart*, pp. 186–90.
54. Davids, 'Technological Change', pp. 289–91; Davids, 'Van vrijheid naar dwang', pp. 8–12. These zeemanscolleges were different from the navigation schools (also called 'zeemanscolleges') discussed above (see note 4).
55. *Reglement voor de Commissie ter bevordering van het onderzoek naar de verschijnselen op de oceaan te Rotterdam* (Rotterdam, 1854), article 32.

4

From Lacaille to Lalande: French Work on Lunar Distances, Nautical Ephemerides and Lunar Tables, 1742–85

Guy Boistel

In the 1630s, when Galileo Galilei sought a longitude reward from the Dutch States for his curious floating telescopic device for observing eclipses of Jupiter's satellites, lunar distances were under intense discussion in France between Cardinal Richelieu and the French savant Jean-Baptiste Morin.[1] Drawing on the work of sixteenth-century authors including Gemma Frisius, Johannes Werner and Peter Apian, Morin set down 13 propositions outlining astronomical and computational methods for finding longitude from the Moon, including lunar distances, lunar altitudes, meridian transits and hour angles.[2] Morin also described the 'clearing' of observations for refraction and parallax.

As Parès has explained, while the true angular distance would be the main objective of computations in the mid-eighteenth century, for Morin it was just one step in the process of defining the Moon's coordinates. From Morin's point of view, longitude difference had to be obtained by comparing the estimated coordinates of the Moon, deduced from lunar altitude and distance observations, with those computed from astronomical tables and tabulated in almanacs. In the mid-seventeenth century, however, stellar positions were not precisely known; the lunar motions had not been solved (predictions still being subject to errors of 30 to 50 arcminutes); instruments for measuring angles were not sufficiently accurate; and astronomers did not have precise and reliable timekeepers. The principles were understood; the instruments had to be improved. Nonetheless, eighteenth-century astronomers, including abbé Nicolas-Louis de Lacaille and canon Alexandre-Guy Pingré, were well aware of Morin's contributions and used them as the basis of their own methods.

At the end of the seventeenth century, two increasingly dominant maritime states, France and England, established royal observatories, each with the remit of helping to solve the problem of determining longitude at sea. In England, as in France, the Astronomer Royal was instructed 'to apply himself with the most exact Care and Diligence to the rectifying the Tables of the Motions of the Heavens, and the places of the fixed Stars, so as to find out the so much desired Longitude of Places for perfecting the Art of Navigation'.[3] In France, the astronomers Jean Picard and Philippe de La Hire significantly refined the national map by determining terrestrial longitudes from observations of the eclipses of the two first satellites of Jupiter, as proposed by Galileo.[4]

Like Britain, France initiated rewards and academic prizes for solving the longitude problem and improving navigation at sea. Two years after the passage of the British Longitude Act, the French regent Philippe d'Orléans suggested establishing rewards, although the promise was not fulfilled. In 1720, the Académie royale des sciences elected to use a bequest from comte Rouillé de Meslay to fund a biannual prize with special reference to navigation. Although this was arguably to have only minor impact on nautical astronomical research in France, the proposed and actual prizes meant that the French government needed experts to examine projects claiming rewards.[5] As this chapter explores, the different ways in which appointed experts and astronomers interpreted their roles was to have a significant impact on the development of the theory and practice of navigation in France and elsewhere. Their work influenced in particular that of the British Astronomer Royal, Nevil Maskelyne.

Expertise as a constraint on research in France

Efforts to determine the shape of the Earth in the early eighteenth century had an explicit connection with the desire of astronomers and seafarers to improve navigation. Behind the debate between Cartesians and Newtonians over the new physics lay the desire to know the shape of the Earth for navigational purposes. Indeed, one direct consequence of the expedition to Swedish Lapland (1736–37) was to put astronomers and members of the Académie more firmly in charge of improving navigation. Shortly after Pierre-Louis Moreau de Maupertuis returned from Lapland, the Ministre de la Marine, Jean-Frédéric Phélypeaux de Maurepas, made him the ministry's adviser for nautical sciences, as 'official responsible for the improvement of navigation and of the navy in all its forms'.[6] As part of his new role, Maupertuis was to publish textbooks for seafarers. These works, whose astronomical content must be read with this demand in mind, included the *Élements de géographie* (1740), *Discours sur la parallaxe de la lune* (1741), the very strange *Astronomie nautique* (1743) and the *Traité de la loxodromie tracée sur la véritable surface de la Mer* (1748).[7] The phrase 'very strange' qualifies the *Astronomie nautique* because it proved a complete failure. Written for shipwrecked sailors, it presented analytical methods primarily for measuring latitude using pen and paper saved from the shipwreck. As one can imagine, naval schools never adopted it, and, as will be argued later, Maupertuis's views hindered Lacaille's aspirations to disseminate the lunar distance method.

Pierre Bouguer succeeded Maupertuis in 1745. Bouguer had begun his career as a professor of hydrography and mathematics before gaining fame as a mathematician and member of the 'Peru expedition', the second geodetic investigation of the shape of the Earth, which was sponsored by the Académie (1733–44). Bouguer was then the best expert the Ministre de la Marine could find for institgating improvements to the navy. As early as 1726, however, he had signalled his rejection of mechanical timekeeping for longitude determination during his travels to the Equator, and maintained this stance in his reports as expert adviser between 1749 and 1758.[8] Such an authoritative rejection of timekeepers was to hold sway at the Académie until news of John Harrison's sea watch (H4) reached France in the 1760s.

Following Bouguer's death in 1758, his role was divided between the mathematician Alexis-Claude Clairaut and the astronomer Pierre-Charles Le Monnier both were experts on lunar tables and their nautical uses, yet had differing views and methods.[9] Jérôme Lalande replaced Clairaut after his death in May 1765.[10] Given responsibility for improving the navy, these savants worked under the control of the Ministre de la Marine, without interference from their peers or the Académie. Improving nautical astronomy was considered a task not for naval officers but for the scientific elite: royal astronomers and members of the Académie. Many of the books by these savants, mathematicians and astronomers – Maupertuis, Bouguer, Clairaut, Le Monnier and Lalande – should, therefore, be read in the context of their role as 'official responsible for the improvement of navigation', giving a more consistent and deeper view of their scientific activity.

Examining their works in this light, one may draw some conclusions about the nature of their expertise and influence. With some hindsight, for example, one might say that three of the five had a negative influence on the development of scientific navigation in France.

- Maupertuis failed to answer the Ministre de la Marine's demands and, for a while, halted Lacaille's attempts to develop nautical astronomy and lunar methods;[11]
- Bouguer discouraged timekeeping research during the 1750s, despite knowing skilled clockmakers (the Lepaute and Leroy brothers) with relevant expertise;[12]
- Lemonnier disseminated old astronomical and nautical methods; he was not able (or did not wish) to follow new developments in Newtonian celestial mechanics, something that overshadowed his quarrels with Lacaille, Clairaut and Lalande (his ex-pupil).

There were deep disagreements and controversies within and between the main scientific academic organizations, too: within the Académie (between factions, as well as individually between d'Alembert and Clairaut, Le Monnier and Lacaille, Lalande and Le Monnier Lalande and Cassini III, and others); within the Académie de marine in Brest (e.g. Le Monnier was excluded in 1771); and between successive Secrétaires d'état de la Marine because of an overly mercurial and hesitant French naval policy (in particular under Maurepas, Rouillé, Choiseul, Praslin, de Boynes and de Sartines).[13]

The first observations of lunar distance at sea: Abbé Lacaille and Jean-Baptiste d'Après de Mannevillette, 1749–51

After the passage of the Longitude Act in 1714 and Isaac Newton's partial development of lunar theory, it seemed clear to astronomers that the Moon was the only natural clock that could be used regularly at sea. In 'A proposal of a method for finding the Longitude at sea within a degree' in the *Philosophical Transactions* for 1731, Edmond Halley offered a method based on observations of occultations of a star by the Moon for correcting lunar tables and calculating the ecliptic longitude of the Moon to within two arcminutes. In principle, the method was sufficiently precise for lunar methods at sea, and Halley's paper showed how the observation of a single angular distance between the Moon and a fixed star could help the seafarer determine longitude. Years later, Lacaille and the Jesuit professor of hydrography and astronomer in Marseille, Father Esprit Pezenas, would write that Halley's method was merely a variant of Morin's lunar altitude method, in which the lunar distance was only an intermediate step towards calculating longitude, not the endpoint.[14] Halley's paper, and the 1742 publication of his *Astronomical Tables* with the corrections of lunar tables from observations of a Saros, would also play an important part in Lemonnier's and Pingré's longitude developments.[15]

In 1742, Nicolas-Louis de Lacaille, 'adjoint astronome' of the Académie, took charge of the computation and publication of the *Ephémérides des mouvements célestes*. Computed for a period of ten years, they provided the astronomical data needed to compute calendars. Lacaille was aware of Halley's 1731 paper and of the recent requests to renew the methods of nautical astronomy. In 1742, while working on the fourth volume of the *Ephémérides* for 1745–55, Lacaille thought of adding considerations of new ways of finding longitude at sea from lunar distances. Hearing that Maupertuis, his superior at the Académie and the official 'responsible for the improvement of the navigation', was shortly to publish the *Astronomie nautique* (1743), however, he shelved his plan, assuming that Maupertuis's book would deal with longitude at sea. As it turned out, it did not.

A new opportunity arose, however, when Lacaille met Jean-Baptiste d'Après de Mannevillette, an officer of the Compagnie des Indes based in the port of Lorient in Brittany who had good relationships with instrument makers in Paris. In June 1749, having made improvements to the octant, Mannevillette was the first naval officer to apply the lunar distance method at sea, near Cape Verde, using an octant of Caleb Smith's design.[16] Well trained in mathematics, Mannevillette later said that he was able to determine longitude with an accuracy of between 5 and 15 'lieues marines' or marine leagues (25 to 45 km); in other words, to an accuracy greater than that required by the 1714 Longitude Act.[17] His results were published much later, in 1775, in the *Neptune François* collection of sea charts.[18]

Mannevillette's voyage to the Cape of Good Hope (1750–54) with Nicolas-Louis de Lacaille helped rekindle practical interest in the lunar distance method. Both used the method to determine the longitude of Santiago in the Cape Verde Islands with considerable accuracy in November 1750. Several determinations of longitude differences were also made by lunar distance (from Antares) in Rio de Janeiro in January 1751. Given his skill in determining stellar positions, improving tables of atmospheric refraction and correcting tables for solar motions, not to mention his familiarity with Clairaut's work on lunar theory, it is not surprising that Lacaille could deploy and correct the tables required for carrying out the lunar distance method. After completing his work on lunar parallax, geodesy and stellar cataloguing in Cape Town, Lacaille developed his ideas on the voyage back to France. In 1754, he sent a memorial on his new method to the Académie.[19] Noting that most seafarers lacked the scientific training to carry out the lunar method, he argued that it should be adapted and put 'within the reach of ordinary sailors':

> During this sea voyage, I occupied myself in making trials of the method of observing longitude at sea by means of the distances of the Moon from some zodiacal fixed star. Following my departure from France, I made numerous investigations to facilitate the practice of the method proposed by Mister Halley. I recognized that it was useless to look for another way of using the Moon for the longitude; that it was solely a question of making the calculation easy for ordinary sailors.[20]

Lacaille also proposed computing the predicted lunar distance from the Sun and other key stars every three hours in a 'nautical almanac', the model Maskelyne would apply ten years later.

Nevertheless, there was no consensus within the Académie. Lacaille's main rival, the astronomer Pierre-Charles Le Monnier was attempting to publish the first nautical almanac entirely devoted to the lunar altitude method (as well as that of hour angles).[21] Alexandre-Guy Pingré performed the computations. Although the resulting almanac, the *État du ciel*, was published four times between 1754 and 1757, seafarers do not seem to have used it because of its complex computations. Nonetheless, with this publication in progress and with Lacaille at sea near Isle de France, Le Monnier was able to stop Lacaille's project.

Lacaille's graphical method

After his return to France in 1755, Lacaille was able to defend his proposal. He read a memorial to the Académie in 1759, which set out his plan for a pre-calculated table of lunar distances and added a graphical method to avoid the long and difficult calculations normally required by the lunar distance method. His ideas were later promoted by Jérôme Lalande, who was elected in 1759 to take charge of the computation and publication of the *Connaissance des temps* (hereafter *CDT*). Lalande had some original views on the *CDT* and its contents, notably adding new scientific matter that in many cases can be found only there. The volume for 1761, for instance, included Lacaille's procedures and methods for calculating lunar distances. In 1755, he also published the *Ephémérides des mouvements célestes*, astronomical tables for ten years (1755–65), in which he further considered lunar distances and his longitude method. In 1760, Lacaille also published a revised edition of Bouguer's *Nouveau traité de navigation*, which expanded on his graphical method for calculating lunar distance, which Bouguer had previously overlooked.

Lacaille's graphical method derived from an idea proposed in 1692 by Father Paul Hoste, S. J., professor of hydrography for the navy in Brest, and explained more carefully by Bouguer in the first edition of the *Nouveau traité de navigation* (1753). A navigation teacher called Griffon, for whose 1748 memorial to the Académie Lacaille was the academic referee, also taught the method in Saint-Malo.[22] Griffon proposed a developed version of Hoste's method. The basis was to draw a circle representing the celestial sphere with the observer at the centre, and then plot the pole and the equator at right angles to each other. To determine local time, one had to plot the Sun's path, which was easy because the only data required was solar declination, which mariners could look up in almanacs. Following Bouguer's elaboration, with worked examples, in the *Nouveau traité de navigation*, Lacaille extended it to the determination of the apparent angular distance between the Moon and Sun. In doing so, he transformed the computations of spherical trigonometry into graphical operations of simple geometry, using only ruler, compass and the four basic operations, something easily within the grasp of the common seafarer.[23] Lacaille, however, did not specify the elements needed to clear or correct the angles for lunar parallax.

For his voyage to St Helena in 1761, Nevil Maskelyne took the *CDT* for 1761 and Lacaille's ephemeris. In a letter published in the *Philosophical Transactions* the following year, he discussed Lacaille's method, in particular the graphical approach, agreeing with Lacaille on one point: the practical dispositions needed for the lunar distance method. Like Lacaille, he felt it best to have three observers measuring the two altitudes and the angular distance simultaneously, thus avoiding the need to calculate by interpolation the small but significant horary motion of the Moon, which could be a source of error.[24] As Sadler has explained, Maskelyne was unable to use the graphical method because of an error Lacaille made in the example calculations.[25] In fact, the error came from Lalande in the *CDT*s for 1761 and 1762, which had mistakenly swapped the figures for Regulus and Aldebaran in the examples for 8 July 1761.[26]

Like the chevalier Jean-Charles de Borda after him, Maskelyne thought that Lacaille was too pessimistic about the accuracy with which one could measure the angular distance between the Moon and a star.[27] Nonetheless, Lacaille was the first astronomer to study the propagation of errors, drawing on Roger Cotes's *Harmonia Mensurarum* (edited by Robert Smith, 1722). Lacaille believed an accuracy of about four minutes of arc was possible; Maskelyne and Borda gave one minute (of arc) for the angular distance, and preferred to develop the lunar distance method without simplified techniques.

In fact, Borda's opposition to Lacaille's methods went deeper. As already noted, Lacaille had argued that lunar distances had to be adapted to seafarers' use, a view echoed in Alexis Rochon's and Lalande's beliefs that the astronomer's task was to simplify and popularize:

> We have simplified through tables all the other parts of the longitude calculations [...] This part, however, greatly lengthens the longitude method and prevents many seafarers from engaging in these studies: if they [seafarers] continue to neglect these observations at the risk of their fortunes and lives, it is the astronomers' duty to lessen the difficulties and to remind them of the vital matters at stake.[28]

In the same year as Rochon's memorial, however, Borda condemned the use of graphical methods, which had 'the drawback of having men, only too inclined to it, becoming used to a process in some way automatic'. Borda concluded elsewhere that teaching navigators how to calculate properly was the best way for them to avoid the difficulties and inconveniences of calculation. Borda's expectations were high, his opinion of seafarers low: 'It is about time that seafarers ceased looking at the mathematical and physical sciences as having no practical use in navigation and its progress. Without the help of science, the navy would still be in its infancy'.[29] In the eyes of many mathematicians, examiners and savant-officers of the French navy, it was up to seafarers to rise to the demands required by the new navigational methods, rather than for mathematicians to simplify solutions and contrivances to circumvent direct calculation.[30]

French and Portuguese attempts to publish nautical almanacs

The development of lunar methods for determining longitude at sea was directly connected to the establishment of nautical almanacs, not only in France, but also in Portugal. In France, the process began in the early 1750s with a dispute over the contents of the *CDT*. The abbé

André-François Brancas de Villeneuve proposed modifying the *CDT* to transform it into a nautical almanac, and published *Éphemérides cosmographiques* between 1750 and 1755. In April 1755, he sent a memorial to the Ministre de la Marine and the astronomer Joseph-Nicolas Delisle – one of Lalande's former teachers – in which he castigated the astronomers and exhorted them to concentrate on their real work: producing tables for longitude determinations.[31] Brancas added that such a nautical almanac should be published two or three years in advance. While his other proposals were sometimes idiosyncratic, Brancas set out all the principles, known from Delisle and Lalande, for creating a nautical almanac.[32] At the same time, Lacaille and Le Monnier were working on proposals for new nautical ephemerides to help seafarers. Nor were these the only attempts to publish nautical almanacs in France. In several ports, small nautical almanacs existed, called (with local variations) *Étrennes maritimes et curieuses, Étrennes nautiques*, *Étrennes nantaises*, and similar. These gave the times of rising and setting and the declinations of the Sun and Moon, information needed to determine local time from the altitude of either body; in other words, the basic astronomical elements used in Hoste's and Bouguer's graphical methods.

Likewise, in 1758 the Portuguese Jesuit Eusebio da Veiga published the *Planetario Lusitano,* a type of nautical almanac, a year before the suppression of the Jesuit order began.[33] This latter event disrupted maritime scientific education for several years in France, and more so in Portugal, since Jesuits mainly provided maritime education in both countries. During the 1750s, therefore, there was significant activity regarding the improvement of existing ephemerides in France and Portugal, and in encouraging astronomers to produce the necessary tools for finding longitude at sea.

Lalande, Jeaurat, Maskelyne and lunar distances in the *Connaissance des temps*

Elected as the new director of the *Connaissance des temps* in 1759, Lalande worked over the next decade or so to develop lunar distance methods, despite considerable resistance from the Académie. In 1772, for example, the Académie de marine in Brest proposed translating the British *Nautical Almanac* and publishing an edition of Dunthorne's and Witchell's formulae for clearing lunar distances. Initially, however, the Ministre de la Marine refused to grant the privilege for printing and forbade any translation, considering it an *allégence* to France's maritime rival; there was also a question of rivalry between the two royal academies. Only after several exchanges with the Ministre did Lalande, who was a member of both academies (Paris and Brest), gain authorization to add lunar distance tables to the *CDT*. As Jim Bennett has shown, there was only a weak notion of state secrecy regarding longitude discoveries.[34] There were no secrets regarding astronomical and nautical computations, with such information circulating easily and quickly between Maskelyne and French astronomers at the end of the eighteenth century.[35]

Lalande and his pupils Edme-Sébastien Jeaurat and Pierre Méchain used the work of Maskelyne's computers to complete French ephemerides between 1772 and 1785, as well as lunar distance tables reduced to the Greenwich meridian, before beginning to compute the same tables, reduced to the Paris meridian after 1790, with the help of the first ever full-time lunar distance computer, Louis-Robert Cornelier-Lémery.[36] The need for purely French nautical ephemerides became even more pressing when Lalande discovered in 1803 that Maskelyne had made errors of five to six seconds in the positions of the stars needed for lunar distance and other tables:

> We are occupied these days in recalculating from observations Maskelyne's 34 stars which we've used with complete confidence, and I find it is necessary to add 5 or 6 seconds to the right ascensions. So we'll have to correct all our catalogues, all our tables and all our longitudes of the observed planets! This old pen pusher, lazy drunkard, miser, has usurped our trust. He's very rich, he should have got himself a computer and checked, more than once, this important result.[37]

Lunar tables for longitude: Mayer versus Clairaut, or empiricism versus theory?

To understand how theoretical knowledge circulated within the network of European astronomers and *géomètres* (mathematicians), it is necessary to examine how they developed astronomical tables of the Moon's motions between 1750 and 1770 as the basis for computations of the lunar distance elements of nautical almanacs and ephemerides. In this context, it is important to remember that in 1765 Tobias Mayer received a posthumous reward for his lunar tables. To achieve an accuracy of half a degree of longitude, as specified by the Longitude Act, the tables had to be able to give the ecliptic longitude of the Moon to within one arcminute.

Less well known than the award to Mayer's widow is a letter (in English) dated 11 April 1765 from Alexis Clairaut to John Bevis, claiming an equal part of the reward.[38] Clairaut was generally known neither for his involvement in the development of nautical astronomy nor for his commitment to improving navigation. His letter, however, claimed that his lunar tables were superior to Mayer's. There was some history to this work. Despite repeated efforts, Newton had failed to formulate a complete theory of the Moon's motions, leaving to later mathematicians and astronomers the task of solving by approximation the three-body problem; that is, the Keplerian problem of the motions of two celestial bodies, but also taking into account the perturbations caused by a third body. In fact, this problem has no analytical and exact solution. It can be solved only by successive approximations, the theory of perturbations. The Moon's ecliptic longitude is determined by the addition of terms that appear as smaller and smaller corrections to the elliptical Keplerian orbit. Beginning in 1743, Clairaut, Euler and d'Alembert developed such a theory in an atmosphere of intellectual competition and rivalry over the motions of both the Moon and comets.[39]

But how did these *geomètres* construct the tables from which astronomers might compute navigational terms and, most importantly, the lunar distances published in nautical ephemerides? Moreover, how did astronomers correct or adjust the theoretical computations of the Moon's position against observed lunar positions? Eighteenth-century *geomètres*, mathematicians and/or astronomers had very different, and often unequivocal, opinions on these questions. On one hand, some argued, tables could be obtained only from theory. On the other, it seemed necessary to make corrections of the Moon's position by reference to practical observations: the theory was modified, for example, with a term obtained from the computation of the mean of O – C (observed minus computed). In other words, the result was empirical. Clairaut developed his tables with 22 terms obtained theoretically; Mayer developed his with 26 terms corrected from observations.

The divergence between theoretical and empirical developments in celestial mechanics seems to have begun when Bouguer wrote of the empiricism of Mayer's lunar tables in 1754. For Bouguer, the correction of theoretical terms by means of observations could only be a short-term solution and was intellectually unsatisfactory. Clairaut followed the same line, which he repeated in his letter to Bevis:

> as I have done it by the meer theory, it is to hope that their agreement with the observations will hold more constantly than that which is

> grounded upon an empirick method, which may be good for a time not very distant from the observations made use of in the confection of the tables, and disagree afterwards.[40]

For Clairaut and d'Alembert, correcting lunar tables by observation was not the correct way to develop lunar theory:

> We can even observe that in the equations M. Mayer uses for his tables, the values of the coefficients are not exactly the same as those he extracted from theory; from which it would appear that the tables of M. Mayer were partly drawn up from observations, by a sort of trial-and-error method, combined with the principal results derived from theory.[41]

Leonhard Euler wisely noted in 1765 that the three-body problem was highly complex and that mathematicians needed time to complete the project; it was premature to argue about it. Even when Euler's son, Johan Albrecht, wrote in 1766 that Clairaut and Mayer's lunar tables were sufficiently accurate for calculating lunar distances, he added that the problem was still not solved; the necessary approximations and theory remained incomplete.

A number of French astronomers helped Clairaut with his calculations; apparently, both he and d'Alembert disliked number crunching. Delisle performed the calculations for the lunar tables published in 1754. In 1763–64, Bailly, Jeaurat and Pingré did the same for the second edition of Clairaut's theory of the Moon's motions (Saint-Petersburg, 1765) and for the 1764 annular eclipse of the Sun. Clairaut's and Mayer's lunar tables were also tested in 1764, based on their predictions of an annular eclipse of the Moon on 1 April. Mayer's tables suggested that the eclipse would not be seen in Paris, Clairaut's that it would. Clairaut won this test, since the eclipse was indeed observed in Paris. The astronomers Cassini III, Bailly and Pingré subsequently recommended that the Académie compute the lunar elements published by Lalande in the *CDT*. These had been based on Clairaut's tables, considered as 'pure' theoretical tables. One also can understand the rejection of Mayer's tables by some French astronomers, Lacaille and Lalande aside, in the light of his failure to answer Lacaille's (and d'Alembert's) challenge to explain the fundamentals of his theory of lunar motion.[42]

Most French astronomers defended and used Clairaut's lunar tables until the late 1780s. Edme-Sébastien Jeaurat began comparisons between Clairaut's and Mayer's tables in June 1759, completing major studies for

1759, 1764, 1776–79, 1780 and 1781–82, the last two also performed by Cornelier-Lémery. In the first three studies, Clairaut's lunar tables came out well: the discrepancies of the errors (O – C) were similar to those of Mayer's tables. Both sets of tables proved to be accurate, with a mean error about one minute of arc for the ecliptic longitude of the Moon. At the beginning of the 1780s, however, Jeaurat and Lémery pointed out that the discrepancies in Clairaut's lunar tables were increasing because, unlike his rivals, Clairaut had not included the secular acceleration of the Moon. Subsequently, Lémery mainly used Euler's tables, computed from his second theory of the lunar motions, until the beginning of the nineteenth century, when the tables of Bouvard, Bürckhardt, Damoiseau and Plana, which were all based on Laplace's celestial mechanics, superseded them.[43]

Conclusion

The development of theories and practices for finding longitude at sea by lunar methods followed different courses in Britain and France in the mid-eighteenth century. British astronomers mainly focused on Halley's methods for lunar distances until the publication of the *Nautical Almanac* in 1767. French astronomers, by contrast, were aware of Jean-Baptiste Morin's methods and explored them in the 1740s and 1750s. From the 1750s, there were also attempts in France to develop and adapt ephemerides and nautical almanacs for the needs of seafarers. Lacaille played an important and significant role in this. He developed the lunar distance method and gave a model for a nautical almanac containing pre-computed angular distances between the Moon and a bright star every three hours. His works were a source of inspiration for Maskelyne in Britain.[44] Lalande promoted Lacaille's graphical method in the *Connaissance des temps*. Not fully explained, either by Lacaille or by Lalande, this method was tried and discussed by Maskelyne during his sea trials, but was abandoned. Nevertheless, the need expressed by Lacaille to develop simplified methods for 'ordinary sailors' began to be met by Jérôme Lalande and abbé Alexis Rochon at the end of the eighteenth century.

Nevil Maskelyne had certainly read Lacaille and Lalande by the start of the 1760s. But what did he know of Le Monnier and Pingré's attempts to produce the *État du Ciel* ephemeris? What did he know of French debates on longitude? And what influence did they have on Maskelyne's own thinking about these problems and their solutions? Maskelyne's journal of his voyage to St Helena in 1761 offers some evidence.

On page after page, his work shows knowledge of celestial mechanics, mainly from Clairaut's early works and Mayer's 1755 tables, and draws from them every element concerned with the problem of determining longitude at sea: angular distance, clearing for the effects of parallax and refraction.[45] Maskelyne had clearly read Lacaille and Lalande in the *Connaissance des temps*, as well as Bouguer's *Nouveau traité de navigation* as revised and expanded by Lacaille in 1760. The *Nautical Almanac* appears, therefore, to be an adaptation of the French ephemeris and a realization of Lacaille's earlier proposals for a nautical almanac. The full extent of what Maskelyne owed to Lacaille, however, has yet to be fully explored.

Notes

1. Jean Parès, 'Jean-Baptiste Morin (1583–1654) et la querelle des longitudes de 1634 à 1647' (Thèse de Doctorat de 3ième cycle, Paris, E.H.E.S.S., Université Paris-I, 1976).
2. Jean-Baptiste Morin, *La science des longitudes de Jean-Baptiste Morin* (Paris: aux dépens de l'Autheur, 1647), pp. 21–40, for Morin's 13 propositions.
3. Royal warrant, quoted in Derek Howse, *Greenwich Time and the Longitude* (London: Philip Wilson/National Maritime Museum, 1997), p. 42.
4. Guy Picolet (ed.), *Jean Picard et les débuts de l'astronomie de précision au XVII*e *siècle* (Paris, CNRS Éditions, 1987). See also, J. D. North, 'The Satellites of Jupiter, from Galileo to Bradley', in *The Universal Frame. Historical Essays in Astronomy, Natural Philosophy and Scientific Method* (London: Hambledon Press, 1989), pp. 185–214.
5. Guy Boistel, 'L'astronomie nautique au XVIIIe siècle en France: tables de la Lune et longitudes en mer' (Thèse de doctorat en épistémologie, histoire des sciences et des techniques, Centre François Viète, Université de Nantes, 2001), part I, pp. 30–75; Guy Boistel, 'Pierre-Louis Moreau de Maupertuis: un inattendu préposé au perfectionnement de la navigation (1739–1745)', in *Annales 2003 de la Société d'histoire et d'archéologie de l'arrondissement de Saint-Malo* (2004), 241–61.
6. French title: 'préposé au perfectionnement de la navigation or de la Marine sous toutes ses formes'; see Boistel, 'L'astronomie nautique' and Boistel, 'Pierre-Louis Moreau de Maupertuis'.
7. Boistel, 'Pierre-Louis Moreau de Maupertuis'.
8. Guy Boistel, 'Pierre Bouguer, commissaire pour la marine et expert pour les longitudes: un opposant au développement de l'horlogerie de marine au XVIIIe siècle?' in *Pierre Bouguer (1698–1758), un savant et la marine dans la première moitié du XVIII*e *siècle* (= *Revue d'histoire des sciences*, 63 (2010)), 121–59.
9. Boistel, 'L'astronomie nautique'; Guy Boistel, 'La Lune au secours des marins: la déconvenue d'Alexis Clairaut', *Les génies de la science*, 23 (2005), 28–33; Guy Boistel, 'Au-delà du problème des trois corps: Alexis Clairaut et ses tables de la Lune à vocation nautique (1751–1765)', in *Actes du congrès d'histoire des sciences*

et des techniques, Poitiers 20–22 mai 2004, ed. by A. Bonnefoy and B. Joly (= *Cahiers d'histoire et de philosophie des sciences*, hors-série (2006)), 20–29.

10. Boistel, 'L'astronomie nautique'; Guy Boistel, 'Lalande et la Marine: un engagement sans faille mais non désintéressé', in *Jérôme Lalande (1732–1807), une trajectoire scientifique*, ed. by Guy Boistel, Jérôme Lamy and Colette Le Lay (Rennes: Presses Universitaires de Rennes, 2010), pp. 67–80.
11. Boistel, 'L'astronomie nautique'; Boistel, 'Pierre-Louis Moreau de Maupertuis'.
12. Boistel, 'Pierre Bouguer'.
13. Boistel, 'L'astronomie nautique'; Boistel, 'Lalande et la Marine'.
14. Guy Boistel, 'L'observatoire des jésuites de Marseille sous la direction du Père Esprit Pezenas (1728–1763)', in *Observatoires et patrimoine astronomiques français*, ed. by Guy Boistel (= *Cahiers d'histoire et de philosophie des sciences*, 54 (2005)), 27–45.; Boistel, 'L'astronomie nautique', part III.
15. Halley derived the name from the Chaldean cycle of 223 lunar periods. After each Saros cycle, the irregularities of Moon's motions are supposed to be the same. This cycle was used to compute eclipses of the Sun by the Moon. For the origins of the word *saros* and the uses of the cycle for the correction of lunar tables by Halley, Legentil de la Galaisière, Le Monnier Pingré and d'Alembert in particular, see Boistel, 'L'astronomie nautique', part III, pp. 299–302 and part IV, pp. 508–70.
16. A Caleb Smith quadrant appears in 'Portrait of a Merchant Captain', by Robert Willoughby, oil on canvas, 1805, National Maritime Museum, Greenwich, BHC3130.
17. Jean-Baptiste d'Après de Mannevillette, 'Principes du calcul astronomique, provenant du cabinet de d'Après de Mannevillette' (*c.*1754), Fonds du Dépôt des Cartes et Plans, Service Historique de la Défense, Paris, Vincennes, SH 53.
18. See Boistel, 'L'astronomie nautique', part III, pp. 281–500, for a study of Lacaille's and Mannevillette's works, longitude determinations at sea and methods; see also Eric Saunier (ed.), *Autour de d'Après de Mannevillette: savant navigateur havrais du siècle des Lumières* (Le Havre: Centre Havrais de Recherche Historique, 2008); Manonmani Filliozat, 'D'Après de Mannevillette, capitaine et hydrographe de la Compagnie des Indes (1707–1780)' (Thèse de l'École des Chartes, Paris, 1993).
19. Nicolas-Louis de Lacaille, 'Projet pour rendre la méthode des longitudes sur mer pratiquable au commun des navigateurs', 1754, A.N., Marine, 2 JJ 69 (J.-N. Delisle's papers), item 16; Nicolas-Louis de Lacaille, 'Instruction détaillée pour l'observation et le calcul des longitudes sur mer par la distance de la Lune aux étoiles ou au Soleil', 1754, A.N., Marine, 3 JJ 13, items 3 and 9.
20. Nicolas-Louis de Lacaille, 'Relation abrégée du Voyage fait par Ordre du Roi au cap de Bonne-Espérance, par M. l'abbé de la Caille', *Histoire et Mémoires de l'Académie Royale des Sciences…pour l'année 1751* (Paris: Imprimerie royale, 1755), *Mémoires*, read on 13 November 1754, pp. 519–36; see also *Procès-verbaux des séances de l'Académie Royale des Sciences*, 13 November 1754, Tome 73, fols 475–93 (fol. 491): 'Je m'étais beaucoup exercé à ces sortes d'observations, et j'avais reconnu qu'il étoit inutile d'avoir recours à une autre façon d'employer la Lune pour les longitudes; qu'il ne s'agissait uniquement que d'en rendre le calcul pratiquable au commun des marins'.

21. Boistel, 'L'astronomie nautique', part II, pp. 158–60; part III, pp. 386–412.
22. 'Mémoire présenté par le Sieur Griffon', 23 August 1748, Archives de l'Académie des sciences, Paris, Quai Conti, Pochette de séance.
23. Boistel, 'L'astronomie nautique', part III, pp. 479–500.
24. Nevil Maskelyne, 'A Letter from the Rev. Nevil Maskelyne M.A., F.R.S., to Rev. Thomas Birch, D.D., Secretary to the Royal Society, Containing the Results of Observations of the Distance of the Moon from the Sun [...] in Order to Determine the Longitude of the Ship from Time to Time', *Philosophical Transactions,* LII (1761), 558–77; Donald H. Sadler, 'Lunar Distances and the *Nautical Almanac*', *Vistas in Astronomy*, 20 (1976), 113–21; see also Boistel, 'L'astronomie nautique', part III and part IV for the computations of the horary motion of the Moon in the tables of Clairaut, Mayer and d'Alembert.
25. Sadler, 'Lunar Distances'.
26. Jérôme Lalande, *Connaissance des temps pour l'année 1761* (Paris: Imprimerie royale, 1759), pp. 192–93.
27. Guy Boistel, 'De quelle précision a-t-on réellement besoin en mer? Quelques aspects de la diffusion des méthodes de détermination astronomique et chronométrique des longitudes en mer en France, de Lacaille à Mouchez (1750–1880)', *Histoire & Mesure*, XXI (2006), 121–56.
28. Jérôme Lalande (ed.) *Histoire générale des Mathématiques par M. Montucla*, 4 vols (Paris: H. Agasse, 1799–1802), IV, 581: 'On a simplifié par des tables toutes les autres parties des calculs de la longitude [...] Cependant cette partie allonge beaucoup la méthode des longitudes et empêche beaucoup de navigateurs de s'occuper de ces recherches; s'ils négligent encore ces observations au risque de leurs fortunes et de leurs vies, c'est le devoir des astronomes de leur aplanir les difficultés et de les rappeler à de pressants intérêts.' See also, Alexis Rochon, *Mémoire sur l'astronomie nautique* (Paris: Imprimerie de Prault, 1798).
29. Le Chevalier de Borda and Pierre Lévêque, 'Rapport sur le mémoire et la carte trigonométrique présentés par le Citoyen Maingon, Lieutenant de Vaisseau', *Procès-Verbaux de l'Académie des sciences de l'Institut de France*, I, séance du 11 Vendémiaire an VII (2 October 1798) (Paris: Gauthier-Villars, 1910), pp. 465–73: 'Elles [les méthodes graphiques] ont l'inconvénient d'habituer à un travail en quelque sorte automatique, des hommes qui n'y sont déjà que trop disposés' (p. 472); 'Il est temps que les marins cessent de regarder les sciences mathématiques et physiques comme inutiles à la pratique de la navigation et à ses progrès. Sans le secours des sciences, la Marine seroit encore dans l'enfance' (p. 473).
30. For more detailed studies, see Boistel, 'De quelle précision'; Guy Boistel, 'Training Seafarers in Astronomy: Methods, Naval Schools and Naval Observatories in 18th- and 19th-century France', in *The Heavens on Earth: Observatories and Astronomy in Nineteenth-Century Science and Culture*, ed. by David Aubin, Charlotte Bigg and H. Otto Sibum (Durham, NC: Duke University Press, 2010), pp. 148–73.
31. Archives nationales, fonds Marine, 2 JJ 69 (Delisle's papers), fol. 109.
32. Boistel, 'L'astronomie nautique', part II, especially pp. 160–62.
33. Jefferson dos Santos Alves, 'O Planetario Lusitano de Eusébio da Veiga e a Astronomia em Portugal no século XVIII', *Revista Brasileira de História da Ciência*, 6 (2013), 340.

34. Jim Bennett, 'The Travels and Trials of Mr Harrison's Timekeeper', in *Instruments, Travel and Science. Itineraries of Precision from the Seventeenth to the Twentieth Century*, ed. by Marie-Noëlle Bourguet, Christian Licoppe and Heinz Otto Sibum (London: Routledge, 2002), pp. 75–95.
35. See, for example, letters between Maskelyne and Jeaurat, National Maritime Museum, Greenwich, REG09/000037 <cudl.lib.cam.ac.uk/view/MS-REG-00009–00037/670> (1775) and <cudl.lib.cam.ac.uk/view/MS-REG-00009–00037/684 and 685> (1789) [accessed 10 February 2015]; see also, Danielle Fauque, 'La correspondance Jérôme Lalande et Nevil Maskelyne: un exemple de collaboration internationale au XVIII^{e} siècle', in *Jérôme Lalande*, ed. by Boistel, Lamy and Le Lay (Rennes: Presses Universitaires de Rennes, 2010), pp. 109–28.
36. Boistel, 'L'astronomie nautique', part II.
37. Lalande to Flaugergues, Viviers, 14 May 1803 (repeated 27 July), quoted in Simone Dumont and Jean-Claude Pecker, *Jérôme Lalande. Lalandiana I. Lettres à Madame du Pierry et au juge Honoré Flaugergues* (Paris: J. Vrin Dumont, 2007), p. 176: 'Nous nous sommes occupés ces jours-ci à recalculer par les observations les 34 étoiles de Maskelyne dont nous nous servions avec une pleine sécurité, et je trouve 5 à 6 secondes à ajouter aux ascensions droites. En sorte qu'il faudra corriger tous nos catalogues, toutes nos tables et toutes nos longitudes des planètes observées! Ce vieux barbouillon, ivrogne paresseux, avare avait usurpé notre confiance. Il est fort riche, il aurait dû se procurer un calculateur et vérifier plus d'une fois cet important résultat'. Thanks to Simon Schaffer for help with the translation.
38. 'Copy of a Letter from M. Clairaut to Dr. Bevis, dated Paris, 11 April 1765, from the English Original, in his own Hand', *Gentleman's Magazine*, XXXV (1765), 208.
39. Boistel, 'L'astronomie nautique', part IV and bibliography.
40. 'Copy of a letter from M. Clairaut'.
41. Jean-Le-Rond d'Alembert, 'Recherches sur quelques points d'astronomie physique. De la manière la plus simple de calculer analytiquement & astronomiquement les mouvements de la Lune', *Opuscules Mathématiques*, VI (Paris, David, 1773), mémoire XLV, pp. 1–46 (pp. 43–44): 'On peut meme observer encore que dans les equations que M. Mayer emploie pour ses tables, les valeurs des coefficients ne sont pas exactement les mêmes que celles qu'il a tirées de la théorie; d'où il paroît résulter que les tables de M. Mayer ont été dressées en partie sur les observations, par une espèce de tâtonnement, combiné avec les résutats principaux que la théorie fournit.'
42. Boistel, 'L'astronomie nautique', part IV; see also, Jacques Gapaillard, 'La correspondance astronomique entre l'abbé Nicolas-Louis de Lacaille et Tobias Mayer', *Revue d'histoire des sciences*, 49 (1996), 483–541.
43. Boistel, 'L'astronomie nautique', part IV for a study of Clairaut's lunar tables.
44. For another example, see Fernando B. Figueiredo and Guy Boistel, 'José Monteiro da Rocha (1734–1819) and the international debate in the 1760s on astronomical methods to find longitude at sea: its proposals and criticisms of the method of lunar distances of Lacaille' (forthcoming), concerning a manuscript of 1765–66, Biblioteca Nacional de Portugal, Lisbon, Colecção Pombalina, Ms.511. Monteiro da Rocha wrote about lunar distance methods

and his trials on a voyage between Brazil and Portugal. His readings and inspiration were Lacaille, Lalande (longitudes and *Connaissance des temps*) and Maskelyne's *British Mariner's Guide*. See also Fernando Figueiredo, 'José Monteiro da Rocha (1734–1819)', in *The Biographical Encyclopedia of Astronomers*, ed. by Thomas Hockey et al. (2nd edition, Dordrecht: Springer, 2014), pp. 513–15.

45. Nevil Maskelyne, 'Journal of Voyage to St Helena', 1761, Cambridge University Library, RGO 4/150 <cudl.lib.cam.ac.uk/view/MS-RGO-00004–00150/1> [accessed 10 February 2015].

5

The Bureau des Longitudes: An Institutional Study

Martina Schiavon

> Et voilà donc la chose la plus capricieuse du monde, voilà donc le sentiment le plus éminemment mobile, qui n'a de prix que par ses inspirations chatouilleuses, qui ne tire son charme que de la soudaineté des désirs, qui ne plaît que par la vérité de ses expansions, voilà l'amour, enfin, soumis à une règle monastique et à la géométrie du bureau des longitudes!
>
> Honoré de Balzac, 1829[1]

At the beginning of the nineteenth century, the novelist and playwright Honoré de Balzac, a fine illustrator of French society, set out the popular image of the Bureau des longitudes: an institution created to control and govern everything, from the physical to the emotional.[2] With extraordinary lucidity Balzac described one of the principal ambitions of nineteenth-century science, which, to use the words of Ernest Renan, was to 'organize humanity scientifically'.[3] Writers, philosophers and educators, as well as men of science, were confident that science would be the tool to explain and understand the world.

Created in 1795, the Bureau des longitudes was concerned with scientific and technological practices, being a place to study and recognize the rules for the management of physical observation. The title 'bureau', or office, reveals the utilitarianism of the revolutionary period, as with the Bureau de consultation pour les arts, which, during its existence from 1791 to 1796, rewarded artisans for 'their discoveries and their works in useful arts'.[4]

In the 220 years since its foundation, the Bureau des longitudes has seen many transformations while maintaining its function as a significant scientific and technological institution: a privileged and independent place to supervise the development of French science and technology.[5]

Yet no comprehensive history exists that shows us *how*, starting from a navigational problem, the Bureau came to embrace many branches of science and their application, from astronomy, metrology, geodesy and celestial mechanics to earth sciences and, more recently, space science. A history of the Bureau can also offer a new understanding of the development of French science and technology. More broadly, it can propose a new way to understand how science, from the French Revolution to the Third Republic, continued the process – started in the second half of the seventeenth century – of becoming autonomous and, by establishing its own rules, a kind of institution.

After presenting the Bureau's exceptional archive, which will soon be digitized, the principal aim of this chapter is to explore the changing role of the Bureau des longitudes and allied practices as a basis for a more complete history of this institution.

An archive

The principle sources for a study of the Bureau des longitudes are the minutes (procès-verbaux) of its meetings. The secretaries of the Bureau wrote the minutes (typewritten from the early twentieth century), which detail discussions during meetings, incoming and outgoing correspondence, the election of new members. The minutes also include the subjects to be examined in the Bureau's publications: the *Connaissance des temps* and the *Annuaire du Bureau des longitudes* from 1795; the *Annales du Bureau des longitudes* from 1877 to 1949.[6] They also document recommendations made or questions addressed to government bodies and responses to questions posed directly by the minister. For the period 1795–1932, the minutes comprise about 28,000 documents, bound into 28 volumes. These include unedited letters, scientific and technical papers received from or written by members, and preparatory studies submitted for the consideration of the Bureau prior to possible publication. The minutes also provide information on scientific expeditions organized by the Bureau in France and abroad.

These documents form a set of unedited manuscripts of exceptional historical interest that allow us to study the activities and the evolution of the Bureau, via the discussions and exchanges of its prestigious members, over more than two centuries. With regard to its membership, it is important to note that between 1795 and 1854 the Bureau met at the Observatoire de Paris, in the city centre. After the separation of the Bureau and the Observatoire in 1854, the Bureau was moved, from 1875, to the Institut de France, 23 quai de Conti.[7] This strategic position in the centre of Paris made it easy for members to attend meetings. From the beginning of the nineteenth century, its members were men of science and naval and military officers, chosen from the mathematical sections of the Académie des sciences (the officers principally came from the 'geodesy and navigation section'), and recognized 'artistes' or precision instrument makers. The central location also allowed members of the Bureau to keep in direct contact with government departments, notably the Ministère de la Marine and the Ministère de la Guerre, and to attend meetings of other important scientific societies, such as the Académie des sciences, the Council of the Observatoire de Paris (not far from the Institut de France) and the Société française de physique, located inside the building of the Société de l'industrie pour l'encouragement nationale, 4 Place Saint-Germain-des-Prés (just behind the Institut). The proximity of these scientific and administrative institutions allowed free and easy circulation of men, practices and technology.

Historians of science and technology have generally neglected the long-term history of the Bureau. An exception is the study by Jean-Marie Feurtet, although it is limited to 1795–1854.[8] In general, historians have given too much importance to the period during which the Bureau directed the Observatoire de Paris (1795–1854), and too little to its subsequent autonomous existence. To consider the Bureau secondary to the Observatoire means accepting the biased histories of, for example, Urbain Le Verrier or Guillaume Bigourdan. According to Le Verrier, membership of the Bureau offered a well-recompensed job for little intellectual work. This view, however, demands context: Feurtet has shown that from October 1853, after the death of François Arago, Le Verrier tried to take over direction of the Observatoire. To realize his plan, he had first to close down the Bureau. Thanks to the credibility he had acquired within the scientific community for his discovery of Neptune, and because of his close links to government, he was able to question the utility of the Bureau and attempt to have it dismantled. He didn't succeed but, after a commission was appointed to investigate, the Bureau was separated from the Observatoire in 1854.[9] Further studies have shown that after 1854 the Bureau provided a centre for opposition to Le Verrier's directorship (or 'dictatorship') of the Observatoire.[10] If we follow Le Verrier's claims, our image of the Bureau is of a declining institution, a sort of duplication of the Académie des sciences or, in Crosland's words, a set of sinecures.[11] This view suggests poor administration by the French government, which might recompense its experts twice – from the Bureau and from the Académie – for their work. As Feurtet has observed, this encourages an image of science as a game of winners and losers, in which the Bureau figures as the loser.[12] When considering the conflict between Le Verrier and the members of the Bureau, his low opinion of the Bureau's work is not surprising, but it would be a sin of historical anachronism to think that the Bureau served no real function. In fact, the great variety of actors who attended the Bureau and the technical subjects studied within it had, in many cases, a direct role in the administration of state, in the control and development of its economic activities. To demonstrate this, it is sufficient to consider the development of computation studies for the publication of ephemerides, which would also serve state administration and control.[13]

Another problematic source is Guillaume Bigourdan, who, around 1920, wrote a history of the Bureau to 1854.[14] Bigourdan did quote some documents that have since been lost, but he gave too much importance to the direction of the Observatoire, with the Bureau figuring merely as a prism through which to view the other institution.[15] However, when the Bureau became independent from the Observatoire, it found its own position within the context of France's existing scientific and technical institutions. Feurtet shows clearly the potential and richness of a historical study centred on the Bureau's minutes. However, he does not cover the period after 1854, despite its importance in the context of the rivalry between Le Verrier (at the Observatoire) and the astronomical members of the Bureau. After 1854, some members of the Bureau sought to develop its role, among them Hervé Faye and Captain François Perrier, who progressively involved the Bureau in an international program of geodetic studies.[16]

Some historians have considered the Bureau des longitudes as a technical service offered to the state.[17] Certainly, almost until 1932, the Bureau had its place in the history of scientific and technical expertise in France and was a central location for the administration of French science and technology. The opportunity now is to study the Bureau as an independent institution with its own processes and practices.

On the creation of the Bureau des longitudes: comparing the French and British boards

> The success of the English at different times, especially during the war of 1761, has sufficiently shown that naval superiority often decides the outcome of war. One of the most effective ways to stifle British tyranny is to deploy the means by which this state, which previously played only a secondary role in the political order, has become a colossal power. Now the English, wholly convinced that without

> astronomy there could be neither commerce nor navy, have spent incredible sums to push this science to the point of perfection.[18]

On 25 June 1795, abbé Henri Baptiste Grégoire, Représentant du peuple at the French national assembly, could not have employed a bolder image to provoke the creation of a board of longitude. His rhetoric was clear: he evoked one of the worst maritime disasters to befall the French Navy during the Seven Years' War against Britain, after which France lost its possessions in southern India (and even Dominica in the Caribbean). He proposed to reinforce astronomy via the creation of a French board of longitude. There was a degree of misunderstanding of the *image* of the British board in France (and perhaps vice versa): it is unlikely that astronomy would have had any effect on this disaster, or that it underpinned Britain's success. Grégoire's argument, however, was not completely wrong. Looking at the French context, the creation of the Bureau des longitudes seems like a pretext to enable the reassembling of scientific expertise after the closure of the Académie royale des sciences in 1793. In fact, apart from its analogous name, the French Bureau was quite different from the British Board.

Before the period of co-existence of the two boards, the British one was a group of Commissioners of Longitude. It was only later that it became an institution or established body with its own rules and regular administration. This eventually happened with the appointment in 1765 of Nevil Maskelyne as Astronomer Royal and Commissioner of Longitude. Maskelyne became editor of the *Nautical Almanac and Astronomical Ephemeris*, giving him a defined role within the Board. Moreover, the same officials now met regularly, and the Commissioners became known as the Board of Longitude.[19]

The French and British boards coexisted between 1795 and 1828. Recent studies have shown that the Bureau was not simply an imitation of the Board, and that, particularly in the last decade of the Board's existence, their mutual influence was significant.[20] At the Bureau, meetings were always regular because its tasks included direction of the Observatoire de Paris and publication of the *Connaissance des temps* (which had been edited, before its dissolution in 1793, by the Académie royale des sciences) and the *Annuaire du Bureau des longitudes* (which functioned to regulate the Republican calendar and other questions of state importance, such as the diffusion of the metric system).[21] The commissioners of the Bureau met weekly and received a salary from the French government. The Bureau was, therefore, a scientific 'institution' from its foundation. After the 1793 dismantling of the Académie, the

Bureau had to test political reaction to the creation of a savant assembly. It was conceived as a small academy: an intimate and decisive place of power to manage the Observatoire de Paris. Unlike the British Board, the Bureau took on an administrative role. It also underwent a very different evolution and a continuing existence. A crucial element to study, therefore, is the way in which its members managed to build a strong alliance between astronomy and activities that helped define nineteenth-century science: geodesy, cartography, study of precision instruments and geosciences.[22]

An institutional project

The Bureau des longitudes was conceived as a scientific institution and this probably represents the most striking difference from the British Board of Longitude. From a historical point of view, it is not easy to give a precise definition of a scientific institution, since this depends on period and context. However, we can start from a vague definition of an official place in which one can practise science (even if the definition of science is likewise variable). It is clear that the Bureau never lacked political authority: as discussed, it was the re-establishment of a smaller Académie and its members received a salary from the government for attending meetings and providing expertise.[23] This political authority is probably the strongest feature of the Bureau (compared to the Board), and was never called into question in the French context, even during the many attempts to dissolve it. The Bureau was also a social system with its own rules, as in the practices and usages shared by its members. In addition, 'institution' can indicate a system of tacit rules, and even beliefs and behaviours, as well as signifying that its members had relative autonomy and could reproduce scientific practices over time.[24]

The evolution of the Bureau came about through four acts of parliament. The first was the Law of 1795, which established its composition at ten members and four 'astronomes adjoints'. Then came the decrees of 1854 and 1862, which raised the membership to 13 (with the notable addition of one each from the Ministère de la Guerre and the Ministère de la Marine) plus one titular and two non-titular 'artistes' (precision instrument makers) and two more 'membres adjoints'. The decree of 1874 then decreased the titular members to nine (the member from the Ministère de la Guerre was retained while that from the Ministère de la Marine became a 'membre adjoint'), with four 'membres adjoints' and three non-titular 'artistes'. The final act of parliament was the current decree of 1970, with 13 titular members (three from the Académie des sciences, nine whose work relates to astronomy or earth sciences and the environment and one 'artiste' specializing in the study of scientific instruments), 32 corresponding members (who may be foreign) and an unspecified number in extraordinary service representing various activities within the Bureau.[25]

Although the Bureau was periodically under threat, as it was in 1854, the legislation demonstrates that it maintained a strong institutional character. This is evident from the direction of the national ephemeris; the provision of expertise and counsel to the state; the redefinition of its operational researches from navigation to geodesy, metrology, earth sciences and, later, space sciences; the strong influence of naval and military men; and close ties with the political and scientific worlds. These characteristics are crucial in the sense that the Bureau's broad range of functions may have helped to secure, in the long term, its uniqueness among the existing scientific institutions.

A comprehensive historical study of the minutes of the Bureau will test this working hypothesis, focusing particularly on periods of crisis in which the Bureau was obliged to 'renew' its remit; for instance, in 1854–70, after separation from the Observatoire and under the influence of a group of members coordinated by Hervé Faye.[26] In fact, it was precisely during times of crisis that members freely discussed the meaning of doing science, the significance of being a place of expertise for the state and the need to be an authority in scientific matters. Because the proposed study will cover a long period (1795 to 1932), it will be able to study the institutional evolution of 'science' as practised within the Bureau, between informal patterns of sociability, networking and patronage and professionalization (and internal conflict). Finally, it will uncover the reciprocal influences between science and its different external manifestations, be they social, practical, technical, economic, cultural or even political.

A meeting place for science, the military and *arts*

Between the eighteenth and twentieth centuries, the Bureau des longitudes played a leading role in the organization and development of astronomy and celestial mechanics in France, in the adoption of the metric system, in the definition and implementation of time-scale references, and in the transmission of time signals. The Bureau also played a crucial intermediary role between government and science, in the sense that it was the promoter and organizer of important geodetic and astronomical expeditions, including those to observe solar eclipses and transits of Venus and to measure a meridian arc. In addition, it played a decisive role in the promotion of maritime exploration and the creation of astronomical observatories in France and overseas.[27] The Bureau kept in close contact with the Bureau international des poids et mesures to develop research on metric standards. Its action was essential in spreading the use of the French unit of measurement of length, in metrological questions, and in the establishment of a national measurement system. In 1919, it played a decisive role in the creation of the Bureau international de l'Heure to unify world time.[28] Between 1795 and 1932, the Bureau was a scientific and technical institution that played an essential role in supplying expertise and counsel to the government; it was the international voice of French science and technology.

The first committee of the Bureau des longitudes comprised ten members: two geometers, Pierre-Simon de Laplace (1749–1827) and Joseph-Louis Lagrange (1736–1813); four astronomers, Joseph Jérôme Lefrançois de Lalande (1732–1807), Jean-Dominique Cassini (1748–1845), Pierre Méchain (1744–1804) and Jean-Baptiste Joseph Delambre (1749–1822); two former navigators, Jean-Charles de Borda (1733–99) and Louis Antoine de Bougainville (1729–1811); a geographer, Jean-Nicolas Buache (1741–1825) and an 'artiste' (astronomical instrument maker), Simon Noël Caroché (1767–1813).

Over time, the Bureau was attended by an assembly of prestigious men of science selected from the mathematical sections – astronomy, geography and navigation – of the Académie des sciences (Arago, Jean-Baptiste Biot, Le Verrier, Faye, Joseph Liouville, Henri Poincaré, Louis Claude de Saulces de Freycinet, Charles-François Beautemps-Beaupré, Jean Jacques Anatole Bouquet de La Grye, Jean-Baptiste Philibert Vaillant, François Perrier, Léon Bassot, Robert Emile Bourgeois, among others) who were also naval and hydrographic officers, artillery men and geodesists. From 1795 to 1932, the Bureau was, therefore, the meeting point of French science, industry, navy and army: as a place of variable geometry, its contribution to the development of science and technology urgently calls for study.

Although the biographies of the majority of prestigious scientific members of the Bureau are in many cases well acknowledged,[29] we do not yet know what kinds of collaboration and exchanges took place with naval and military officers, nor with the artisans admitted as members of the Bureau. The artisans' membership is important and peculiar to the Bureau. Caroché was the first of a series of 20 'artistes' operating within the Bureau from 1795 until 1946, including not just chronometer-makers, but also precision instrument makers. They include Henri Gambey (nominated in 1831), Louis Bréguet (nominated in 1843, becoming president of the Bureau in 1852), Jules Carpentier (nominated in 1897), Amedée Jobin (nominated in 1910) and others.[30] In France, in 1795, the admission of artisans into the French institution was not controversial. Of course, this does not mean that in Britain or in Germany instrument makers were not considered crucial to scientific enterprise. The members of the British Board, for instance, questioned whether the rational principles of experimental natural philosophers could reproduce the knowledge of skilled artisans. Thus in 1766 the Board asked John Bird to reproduce in writing his method of manufacturing astronomical instruments,[31] and in 1779, Joseph Banks created the Glass Committee to study and reward new methods for the production of flint glass for achromatic telescopes. Nevertheless, artisans (who could enter the Royal Society) were not appointed as Commissioners of Longitude.[32] Focusing on a comparative and long-term study of artisans' membership of the Bureau, we could understand their changing status over time and in particular contexts. For instance, in Germany (Bavaria), the question of admitting artisans as members of the Royal Academy of Sciences in Munich was debated during the 1820s. Even though universally recognized for his manufacture of astronomical instruments, Joseph von Fraunhofer never enjoyed the privileged status of experimental natural philosopher. In Bavaria, Jackson has shown, artisans were rarely granted the status of experimental natural philosophers and were rarely admitted into scientific institutions, for three reasons. First, the importance of secrecy to the artisanal trade was seen as anathema to German savant society, whose members prided themselves on the openness of scientific knowledge. Second, craftsmen were members of a commercial nexus and financial interests could be seen as tainting their work. Third, savants thought that artisans only manipulated pre-existing materials and did not create anything: in this sense, artisans were slaves of craft rules, which were considered the antithesis of creative and scientific knowledge.[33]

The question also concerns what science is. Joseph von Baader and Julius Konrad Ritter von Yelin criticized not only artisanal knowledge as insufficiently creative to be considered scientific knowledge but also the fact that Fraunhofer was not university educated. Baader continued by emphasizing that Fraunhofer had never attended a Gymnasium: 'The Academy must not become a corporation of artistes (Künstler), factory owners (Fabrikanten), and artisans (Handwerker)'.[34] Another notable point in Jackson's study is the particular position of Johann Georg von Soldner, the scientific director of the Bavarian ordnance surveying project and director of the Royal Observatory at Bogenhausen, near Munich. Soldner, who had contributed to the theory and practice of astronomy and geodesy, tested Fraunhofer's achromatic telescopes for his observatory in 1818. He then defended Fraunhofer's admission to the Bavarian Academy, deliberately using his name in the same sentence as Newton: 'I consider this discovery of Fraunhofer's [the exact measurement of solar spectrum] to be the most important one in the area of light and colours since Newton'.[35]

Fraunhofer's example is important as a further comparative study of the role of instrument makers admitted into scientific institutions. In fact, in France the first commission of the Bureau, as proposed by Grégoire, did not identify an artisan as a member.[36] Questions of secrecy and links to commercial matters seem to have been the real reason for admitting Caroché as a member of the Bureau. Lalande was interested in the recipe for flint glass that Caroché had learned during his apprenticeship with Alexis-Marie Rochon, director of the Optical Institute in Passy.[37] Caroché was also rewarded many times by the Bureau de consultation pour les arts[38] and had visited (with Cassini IV) Jesse Ramsden's and William Herschel's workshops in England in 1788. He was therefore considered the ideal candidate 'to replace Herschel in this country'.[39] Grégoire himself thought that Caroché represented the prototype of the ideal savant, poor and ingenious, a model for the French astronomer. In this sense, the nomination of an artisan could also be linked to a model of science enlarged to the world of arts as defended by Grégoire, who also founded the Conservatoire des arts et métiers.[40] Moreover, the astronomical community's admiration of Caroché was sufficient to overcome the reluctance of some members of the Bureau, Laplace in particular, who opposed admitting those ignorant in mathematics.[41]

The exceptional status Caroché enjoyed until his death in 1813 wasn't passive: he found allies within the Bureau among the observers whose interest in the modification and construction of new telescopes meant that he offered essential and undisputable skills. This helped justify his membership.[42] Likewise, the progressive involvement of the Bureau in geodesic questions fostered contact and collaboration between astronomers and military officers. Technical problems relating to terrestrial observations offered a strong argument for collaboration between officers and artisans, which further reinforced the latter's membership in the Bureau.[43] From Caroché to Amédée Jobin (nominated in 1921), almost all the 'artistes' of the Bureau had direct contact with naval and military officers regarding nautical problems, terrestrial observations and other technical matters. This is important for two reasons. Most military officers shared an ideal of devotion to the unselfishness of science, to nation and the public cause (an ideal expressed in Saint-Simonianism); and this kind of education originated at the Ecole Polytechnique. Most of the 'artistes' admitted to the Bureau shared with officers and scientific practitioners, throughout the nineteenth century, the same education at the Ecole Polytechnique. This may explain the privileged relationships between these actors, in particular between officers and artisans. Encouraging Amédée Jobin, his pupil at the Ecole Polytechnique, to buy the former Laurent workshop, founded by Soleil in 1813, Alfred Cornu began the formation of a French optical community.[44] He explicitly said that the communication between Jobin and his clients would be easier because the clients were often his former *camarades*.[45] Even though Jobin kept his manufacturing secrets and looked for innovation through friendly contacts, he didn't seek the large-scale industrial development of his workshop. He also granted access to his workshop to scientific practitioners and officers who were directly involved in the production process. What Jobin looked for, was the *prestige* of science. Making a prism astrolabe for an officer or an interferometer for a scientific practitioner rather than binoculars in series, entering a scientific institution or being admitted to savant societies: this was Jobin's goal. We can understand how the admission of an artisan to the Bureau helped elevate his status and inaugurated a particular form of relationship between scientific practitioners, artisans, and naval and military officers that should be more thoroughly investigated.[46] The minutes of the Bureau therefore constitute an essential resource to study not only the development and circulation of precision instruments over the long term but also the constitution of a French community of artisans whose archives are dispersed and often overlooked.

Exchanges between actors

One of the principal purposes of studying the history of the Bureau is to understand its way of working: the forms of sociability and relationships between its members, and the information on science and technology they exchanged. The aim is to reconstruct the type of 'exchanges' which took place within the Bureau. What kind of information did members exchange? How did theoreticians, practitioners, instrument makers and politicians appropriate theoretical and practical mathematics in ways that made sense for each profession? Historians know that it can be difficult to get information on how new proposals and scientific ideas are accepted and circulated among different actors and professions. It is even more difficult to find documents that testify to exchanges between scientific practitioners and between scientific practitioners and technicians, military and political actors. In the case of the minutes of the Bureau, we have exceptional archives for such a study. However, the minutes can only guide us in the historical reconstruction of exchanges. In fact, because they were addressed to the minister responsible for the Bureau, the minutes are sometimes too brief, just an abstract of the meeting.[47] While in the best examples they name the speaker and the topic of his contribution, elsewhere quotations are impersonal ('a member said', 'the board thinks that'). These silences may, however, offer clues as to which subjects were contentious. Sometimes, the place of discussion was not the Bureau but another scientific institution, such as the Observatoire de Paris or the Académie des sciences. For this reason, it would be necessary, when reconstructing exchanges between members, to compare the minutes with other archives, or even with publications, to reconstruct the fire of battle. In this sense, the institution is permeable: the Bureau, in particular, was not a closed place. Although many scientific and technical decisions were made inside it, its members communicated with the outside. The Bureau served to harmonize conflicts that often originated elsewhere or to better explain theoretical questions to technicians (or technical problems to theoreticians), or as a lobbying platform for science and technology.

The digitized minutes of the Bureau will be invaluable for understanding such scientific and technical exchanges, revealing the form of sociability practised by its members. These exchanges might be oral, material (papers, technical documents, networks of projects) or even object-centred. In all cases, the reconstruction of the exchanges shows us the necessity of comparing the minutes to other archives. Thus, the minutes allow us to complete existing studies. The Bureau's archives show us how to situate not only actors and events, but also moments at which specific scientific and technical arguments took on significance. In some cases, we can reconstruct with surprising detail the way in which different communities might combine or harmonize their points of view. The unedited documents preserved with the minutes contain, in some cases, events previously ignored by historians.[48] In particular, the minutes give us the names of secondary actors, such as technicians and tradesmen, whose contributions were as essential as those of the 'great names' with which we are familiar. Through the minutes, the historian will also have a new way of studying career strategies of mathematicians or astronomers.[49] The minutes additionally set up a sort of map by which historians can reconstruct the rich interactions between theoretical and applied astronomy, mathematics and applied mathematics. In addition, the extreme richness of the Bureau's minutes and the long period they cover allow us to discover new uses for old knowledge. For instance, during the various worldwide geodetic, polar and oceanographic expeditions, we find essential data for studying climate change. Comparing today's agricultural crops with those documented in 1901–07 during the meridian arc measurement in South America will offer a strong argument, historical and scientific, for the politics of those who seek to control the transformation of the territories and propose appropriate conservation measures.

Conclusion

The minutes of the Bureau des longitudes form an exceptional archive. Studying them, we can discover the role played by the Bureau and its expertise in scientific and technical matters, in the organization and development of theoretical and practical questions relating to astronomy, earth sciences and navigation. The Bureau played a crucial role in the development of metrology, celestial mechanics and geodesy, and organized and supported many scientific expeditions all over the world. Several studies have examined these questions, but they have rarely connected the Bureau with them. In general, such studies have not shown the Bureau's central role as coordinator and promoter of scientific enterprises at a national and international scale. The current research project on the Bureau will study these various subjects through the prism of this institution.

A key point will be the comparison between the Bureau and the Board of Longitude in the period of their coexistence.[50] However, as sketched in this chapter, the comparison could go beyond this period. We need to compare in particular the personalities of Airy and Le Verrier in order to transcend the obstacle of their historiographies.[51] The two astronomers knew each other and, as directors of two important observatories, agreed in the need to organize them strictly.[52] They had a similar discourse on the non-utility of their respective boards. Airy stated, 'there certainly was no use in maintaining the Board any longer'.[53] Meanwhile Le Verrier simply hated the Bureau because of its control of the Observatoire de Paris. Considering the discussions of the commission charged with reorganizing the Observatoire in 1854, it is clear that there was some sort of confusion between the Observatoire (as scientific establishment) and the Bureau (as institution and arbiter in scientific matters).[54] Likewise, Le Verrier thought that the status of the Bureau was inappropriate with regard to the provincial observatories: he recalled in 1864, 'as to the provincial observatories, using the picturesque expression of the astronomer Zach, [the Bureau has] toppled [them] completely'.[55]

It is also clear that, their similar names aside, the Bureau des longitudes and the Board of Longitude were very different. From its foundation onwards, the Bureau maintained an interest in the popularization of astronomy,[56] in instrumental questions (one consequence being the admission of artisans as members), in integrating and studying the question of computation in astronomy (in Britain the question of the computation of the *Nautical Almanac* was separate from that of astronomical computation more generally).[57]

Lastly, the two boards evolved very differently. In France a key moment was 1854, the date of the separation of the Bureau from the Observatoire. Claiming a permanent director for the Observatoire, Le Verrier nearly eradicated the *raison d'être* of the Bureau. However, some members realized that it had to be maintained, among them marshal Vaillant, who proposed placing the Bureau under the control of the Ministère de la Marine. In the meantime, he affirmed that when separated from observatory astronomy, the Bureau could devote itself to tasks of practical utility, above all in navigation and geodesy. Biot, by contrast, wanted the Bureau to become the pinnacle of astronomy in France. Even though astronomy always assumed Parisian centrality, it is clear that the principal question was how to think about the Bureau once it was detached from the affairs of the Observatoire. In the end, the question was what the Bureau as an independent scientific institution might be. Answering such questions took time. The Bureau seems to have become such an institution under Faye, around the 1860s, when it was assigned the task of promoting certain branches of science and their applications – not only navigation and celestial mechanics but also geography, astronomy, geodesy and earth sciences. The role of the Bureau was also assured in the design and construction of new astronomical instruments, in giving assistance to travellers, geographers and sailors instructed to undertake scientific observations, and in testing their instruments. Considering Faye's views on science and in particular his interest in practical astronomy and geodesy, it seems clear that the Bureau offered him the ideal place and the means to realize his personal projects.[58]

Just before the dissolution of the Board of Longitude, there had been similar moves in Britain regarding geodesy and earth sciences, instrumentation and the development of international astronomy. We can imagine that an analogous revival might have taken place there; for instance, from the work of Edward Sabine. The interest and development in the matter of geodesy, astronomy, navigation, earth sciences, and instrumentation were the same: it might have been possible to reorganize the Board. However, this was not the case. One could certainly ask why.

The comparative study of the minutes of the Bureau and the Board will surely help answer such questions. The comparative study of particular personalities will also help in understanding how Faye managed to renew and transform the Bureau within the national context of antagonism to Le Verrier's dictatorship and the international context of competition in science and technology. This leaves the reader with intriguing questions, but clearly shows that the minutes of the Board of Longitude and of the Bureau des longitudes offer extraordinary archives for comparative research. Those of the Bureau also detail, over many years, the nature of its activities as an international scientific institution.

Notes

With thanks to Richard Dunn and Rebekah Higgitt for their careful reading and comments, and to Simon Schaffer for his helpful discussions.

1. Honoré de Balzac, *Physiologie du mariage ou meditations de philosophie écletique sur le bonheur et le malheur conjugal*, new edition (Paris: Charpentier Libraire éditeur, 1840), p. 176.
2. Other references to the Bureau des longitudes in Balzac include: 'Les difficultés surexcitent le génie des employés, qui souvent sont des gens de lettres, et qui se mettent alors à la recherche de l'Inconnu avec l'ardeur des mathématiciens du Bureau des Longitudes: ils fouillent tout le royaume'; Honoré de Balzac, *Œuvres complètes de M. de Balzac, La comédie humaine; 2. Etudes de mœurs. 2e livre, Scènes de la vie de province. Les célibataires: Pierrette* (Paris: Acamédia, 1842–48) <http://gallica.bnf.fr/ark:/12148/bpt6k1012816> [accessed 27 April 2015].
3. 'Organiser scientifiquement l'humanité, tel est donc le dernier mot de la science moderne, telle est son audacieuse, mais légitime prétention'; Ernest Renan, *L'avenir de la science – Pensées de 1848,* 24th edition (Paris: Calmann-Lévy Editeurs, no date), p. 37.
4. The Bureau was appointed by the government for expert and technical tasks: performing experiments and tests, fabricating expensive machines, publishing books useful to the progress of the arts; see Dominique de Place, 'Le Bureau de consultation pour les arts, Paris, 1791–1796', *History and Technology*, 5 (1988), 139–78 (quote on p. 139).
5. The Bureau des longitudes still exists and consists of 13 titular members (three 'au titre de l'Académie des sciences'), 32 correspondents (four of whom are foreign scientists) and some members in 'extraordinary service' (representing the Centre national d'études spatiales, the Institut national de l'information géographique et forestière, Météo France, the Observatoire de Paris and the

Service hydrographique de la Marine). It has retained its role of publishing an astronomical ephemeris and has responsibility for the computations carried out, since 1998, by the Institut de mécanique céleste et du calcul des éphémérides; see Nicole Capitaine, 'Le Bureau des Longitudes: Activités et missions issues de son histoire', paper given at the *Conférence de l'Académie de Marine*, 23 November 2011.

6. All are available on the Gallica website, http://gallica.bnf.fr.
7. The location was inaugurated on 3 October 1875, but the Bureau informally met there from December 1874; see Guy Boistel, *L'observatoire de la Marine et du Bureau des longitudes au parc Montsouris, 1875–1914* (Paris: IMCCE/E-dite, 2010).
8. Jean-Marie Feurtet, 'Le Bureau des Longitudes (1795–1854): de Lalande à Le Verrier' (unpublished doctoral thesis, Ecole nationale de Chartes, Paris, 2005).
9. For the conflict between Le Verrier and the Bureau, see Feurtet, 'Le Bureau' pp. 383–494. Le Verrier remained antagonistic towards the Bureau; see anonymous, 'Origine de la haine de M Le Verrier pour le Bureau des longitudes et de l'appui qu'il trouve auprès du Maréchal Vaillant', Archives nationales de France (hereafter AN), F/17/3719.
10. Martina Schiavon, 'Hervé Faye, la géodésie et le Bureau des longitudes', in *Hervé Faye (1814–1902) ou l'art de la rupture*, ed. by Guy Boistel, Stéphane Le Gars and Colette Le Lay (= *Bulletin de la Sabix*, 55 (2014)), 31–44.
11. 'Indeed one suspects election to many posts in the Bureau as becoming little more than a sinecure, although there was a revival of activity in the 1860s'; Maurice Crosland, *Science under Control: The French Academy of Sciences 1795–1914* (Cambridge: Cambridge University Press, 2002), p. 144.
12. Feurtet, 'Le Bureau', p. 50.
13. See for instance Theodore M. Porter, *The Rise of Statistical Thinking 1820–1900* (Princeton: Princeton University Press, 1986), or more recent works on computers in France (Guy Boistel, 'Un 'Bréviaire' pour les astronomes et les marins: la Connaissance des temps et les calculateurs du Bureau des longitudes, de Lalande à Loewy (1772–1907)', *Archives internationales d'histoire des sciences*, 64/172–73 (2014), 463–80) and Britain (Mary Croarken, 'Human Computers in Eighteenth- and Nineteenth-century Britain', in *The Oxford Handbook of the History of Mathematics*, ed. by E. Robson and J. Stedall (Oxford: Oxford University Press, 2010) pp. 375–406). On the application of geodesy, see Martina Schiavon, *Itinéraires de la précision. Géodésiens, artilleurs, savants et fabricants d'instruments de précision en France, 1870–1930* (Nancy: PUN-éditions universitaires de Lorraine, 2014).
14. Guillaume Bigourdan, 'Le Bureau des longitudes: Son histoire et ses travaux, de l'origine (1795) à ce jour', *Annuaire pour l'an 1928 publié par le Bureau des longitudes*, A.1–A.72; 1929, C.1–C.92; 1930, A.1–A.110; 1931, A.1–A.151; 1932, A.1–A.117; 1933, A.1–A.91; see also Guillaume Bigourdan, 'La réorganisation du Bureau des longitudes, en 1854 et 1862', *Comptes rendus du congrès des sociétés savantes de Paris et des départements, tenu à la Sorbonne en 1929* (Paris: Imprimerie nationale, 1931), 23–34.
15. The letters now lost are quoted in Bigourdan, 'La réorganisation'.
16. See Schiavon, *Itinéraires de la précision*, pp. 66–124. On the relation between geodesists and the members of the Bureau, see Schiavon, 'Harvé Faye'.

17. See Jerôme Lamy, 'Le Bureau des longitudes: La gestion des instruments et les régimes de savoirs au XIX^e^ siècle', *Revue d'anthropologie des connaissances*, 1 (2007), 167–188. Many studies have examined particular aspects of the Bureau: Guy Boistel has studied the observatory administrated by the Bureau and the Navy in the Parisian parc Monstouris in 'Instruire les marins avec les moyens du bord: l'observatoire de la Marine et du Bureau des longitudes au parc Montsouris', *Les Génies de la science*, 28 (2006), 28–33; Guy Boistel, *L'observatoire de la Marine*; Guy Boistel, 'Un observatoire pour la formation des militaires, des géographes et des explorateurs en plein Paris: l'observatoire de la Marine et du Bureau des longitudes au parc Montsouris, 1875–1915', in *La (re)fondation des observatoires astronomiques sous la IIIe République*, ed. by J. de la Noë and C. Soubiran (Pessac: Presses universitaires de Bordeaux, 2011), pp. 127–46. Other studies that mention the Bureau include Peter Galison, *L'empire du temps. Les horloges d'Einstein et les cartes de Poincaré* (Paris: Robert Laffont, 2003), although Galison had not studied the Bureau's minutes; Suzanne Débarbat, 'L'Observatoire de Paris, le Bureau des longitudes et les observatoires de provinces', in *Observatoires et patrimoines astronomique français, Cahiers d'histoire et de philosophie des sciences,* ed. by Guy Boistel (Lyon/ENS Éditions, 2005), pp. 65–87; Danielle Fauque, 'Les origines du Bureau des longitudes', *Cahiers Clairaut*, 55 (1991), 34–39; 56 (1991), 31–37; 57 (1992), 31–37; Martina Schiavon 'Geodesy and Map-Making in France and Algeria: Between Army Officers and Observatory Scientists', in *The Heavens on Earth: Observatories and Astronomy in Nineteenth Century*, ed. by D. Aubin, C. Bigg and H. Otto Sibum (Durham, NC and London: Duke University Press, 2010), pp. 199–224.
18. 'Le succès des Anglais à diverses époques, et spécialement dans la guerre de 1761, n'ont que trop prouvé que la supériorité de la marine décide souvent des résultats de la guerre. Une des mesures les plus efficaces pour étouffer la tyrannie britannique, c'est de rivaliser dans l'emploi des moyens par lesquels cet Etat, qui ne devait que jouer qu'un rôle secondaire dans l'ordre politique, est devenu une puissance colossale. Or les Anglais, bien convaincus que sans astronomie on n'avait ni commerce, ni marine, ont fait des dépenses incroyables pour pousser cette science vers la perfection'; 'Rapport fait à la Convention [...] par le Représentant du peuple, Henri-Baptiste Grégoire, 25 June 1795', in *Lois, décréts, ordonnances, arrêtés et décisions concernant le Bureau des longitudes* (Paris/Imprimerie nationale, 1909), pp. 1–2.
19. See Martina Schiavon, 'The English Board of Longitude (1714–1828) ou comment le gouvernement anglais a promu les sciences', *Archives internationales d'histoire des sciences*, 62–168, (2012), 177–224 (p. 187); Howse, 'The British Board of Longitude' (unpublished manuscript, National Maritime Museum, Greenwich).
20. See Rebekah Higgitt and Richard Dunn, 'The Bureau and the Board: change and collaboration in the final decades of the British Board of longitude', in *Le Bureau des longitudes (1795–1930): Context national et international*, ed. by Martina Schiavon and Laurent Rollet (Nancy: PUN-éditions universitaires de Lorraine, forthcoming).
21. Between 1870 and 1949 the Bureau also published the *Annales du Bureau des longitudes et de l'Observatoire astronomique de Montsouris*, recording the work of

its laboratory and observatory in the parc Montsouris (Boistel, *L'observatoire de la Marine*).

22. Schiavon, *Itinéraires de la précision*, Chapters 1–2.
23. Waring has proposed that political authority in the Board might be represented by the Admiralty. While the Admiralty played an important role in the construction of a scientific community, she shows that the necessity of such political authority for decisions regarding the distribution of financial patronage was always discussed by members of the Board such as Thomas Young and John Herschel; Sophie Waring, 'The Board of Longitude and the Funding of Scientific Work: Negotiating Authority and Expertise in the Early Nineteenth Century', *Journal for Maritime Research*, 16 (2014), 55–71 (p. 62).
24. Science itself may be considered an institution when it acquires relative autonomy and develops its own rules: see, for instance, Roger Hahn, *L'anatomie d'une institution scientifique: l'Académie des sciences de Paris, 1666–1803* (Bruxelles: Editions des Archives contemporaines, 1993); Timothy Lenoir, *Instituting Science: The Cultural Production of Scientific Disciplines* (Stanford: Stanford University Press, 1997); Crosland, *Science under Control*. On the evolution of the concept of scientific institution and particularly its progressive involvement in teaching matters, see Laurent Rollet, 'Peut-on faire l'histoire des pôles scientifiques?', *Histoire de l'éducation*, 122 (2009), 93–113.
25. Titular members were officially nominated and paid by the relevant minister (see the nominations in AN, F/17/13569); Capitaine, 'Le Bureau des longitudes'. See also: *Lois, décrets, ordonnances, arrêtés et décisions concernant le Bureau des longitudes* (Paris/Imprimerie nationale, 1909), Bibliothèque nationale de France <http://catalogue.bnf.fr/ark:/12148/cb339707461> [accessed 15 January 2015].
26. A former pupil of the Ecole polytechnique, Faye had been 'pupil of astronomy' before being named Professeur de mathématiques pures et appliquées at the Faculty of Nancy from 1854 to 1857; he entered the Bureau in 1863 (see Boistel, Le Gars and Le Lay (eds), *Hervé Faye*).
27. For an overview, see Jacques Lévy, 'L'astronomie et les expéditions', in *La figure de la terre du XVIIIe siècle à l'ère spatiale*, ed. by Henri Lacombe and Pierre Costabel (Paris: Éditions Gauthier-Villars, 1988), pp. 151–59. See in particular: Lorelai Kury, 'Les instructions de voyage dans les expéditions scientifiques françaises (1750–1830)', *Revue d'histoire des sciences et de leur application*, 51 (1998), 65–91; Olivier Chapuis, *A la mer comme au ciel: Beautemps-Beaupré et la naissance de l'hydrographie moderne (1700–1850)* (Paris: Presses universitaires Paris-Sorbonne, 1999); Hélène Blais, 'Les voyages français dans le Pacifique. Pratique de l'espace, savoirs géographiques et expansion coloniale (1815–1845)' (unpublished PhD thesis, Ecole des hautes études en sciences sociales, 2000); Marie-Noëlle Bourguet, 'Landscape with Numbers. Natural History, Travel and Instruments in the Late Eighteenth and Early Nineteenth Centuries', in *Instruments, Travel and Science*, ed. by M. N. Bourguet, C. Licoppe and H. O. Sibum (London and New York: Routledge, 2002), pp. 96–125; Isabelle Surun, 'Géographies de l'exploration. La carte, le terrain, et le texte (Afrique occidentale, 1780–1880)' (unpublished PhD thesis, Ecole des hautes études en sciences sociales, 2003); Geraldine Barron, 'Entre tradition et innovation: itinéraire d'un marin, Edmond Pâris (1806–1893)' (unpublished PhD thesis, Université Paris Diderot Paris 7, 2015).

28. See Jacques Gapaillard, *Histoire de l'heure en France* (Paris: Vuibert-Adapt-Snes, 2011).
29. See for instance: Maurice Daumas, *Arago, 1786–1853, la jeunesse de la science*, new edition by Emmanuelle Grison (Paris: Belin, 1987); Pierre Costabel, Pierre Dugac and Michel Metiver, *Siméon-Denis Poisson et la science de son temps* (Palaiseau: Ecole polytechnique, 1981); Eugène Frankel, *Jean-Baptiste Biot: The Career of a Physicist in Early 19th Century France* (Princeton: Princeton University Press, 1972); Charles Gillispie, Robert Fox and Trevor Grattan-Guinness, *Pierre-Simon Laplace, 1749–1827. A Life in Exact Science* (Princeton: Princeton University Press, 1997); Paul Levert, Françoise Lamotte and Maurice Lantier, *Urbain Le Verrier. Savant universel, gloire nationale, personnalité cotentine* (Coutances: OCEP, 1977); Jesper Lütez, *Joseph Liouville (1809–1882): Master of Pure and Applied Mathematics* (New York: Springer Verlag, 1990); Jean Mascart, *La vie et les travaux du chevalier Jean-Charles de Borda (1733–1799)* (Paris: Presses de l'Université Paris Sorbonne, 2000); Jean-Claude Pecker, Raymond Chevallier and Alain Gros, 'Jérôme de Lalande, 1732–1807' (Bourg-en-Bresse: Éd. les nouvelles Annales de l'Ain, 1985); James Lequeux, *François Arago, un savant généreux. Physique et astronomie au XIXe siècle* (Paris: EDP sciences/Observatoire de Paris, 2008); Jeremy Gray, *Henri Poincaré: A Scientific Biography* (Princeton: Princeton University Press, 2012); Guy Boistel, 'Ernest Laugier', *Dictionnaire de biographie française*, CXIV (Paris: Letouzey et Âné, 2000), 444–45.
30. See Paolo Brenni, 'H.-P. Gambey (1787–1847)', *Bulletin of the Scientific Instrument Society*, 38 (1993), 11–13; Paolo Brenni, 'Soleil Duboscq-Pellin: A Dynasty of Scientific Instrument Makers', in *Proceedings of the 11th International Scientific Instrument Symposium*, ed. by G. Dragoni, A. McConnell and G. L'E. Turner (Bologna: Grafis Edizioni, 1994), pp. 107–11; Paolo Brenni, 'Soleil, Duboscq and their successors', *Bulletin of the Scientific Instrument Society*, 51 (1996), 7–16; Paolo Brenni, 'The Brünners and Paul Gautier', *Bulletin of the Scientific Instrument Society*, 49 (1996), 3–8; Paolo Brenni, 'Louis Clement François Bréguet and Antoine Louis Bréguet', *Bulletin of the Scientific Instrument Society*, 50 (1996), 19–24; Paolo Brenni, 'Jules Carpentier (1851–1921)', *Bulletin of the Scientific Instrument Society*, 43 (1997), 12–15.
31. See Schiavon, 'The English Board', p. 191, note 32.
32. The Glass Committee became a joint committee of the Royal Society and the Board of Longitude in 1820 and its responsibility was given to Michael Faraday (Schiavon, 'The English Board', p. 186). The Committee's goal was to produce lenses that could match or better Fraunhofer's; see Myles W. Jackson, *Spectrum of Belief: Joseph von Fraunhofer and the Craft of Precision Optics* (Cambridge, MA/London: MIT Press, 2000), Chapter 5.
33. Jackson, *Spectrum of Belief*, p. 88.
34. Ibid. pp. 91–95 (quote on p. 91).
35. Quoted in Jackson, *Spectrum of Belief*, p. 93.
36. Feurtet, 'Le Bureau', p. 81.
37. On Rochon, see Danielle Fauque, 'L'abbé Alexis-Marie de Rochon (1741–1817), astronome et opticien de la marine', *La Mer au siècle des Encyclopédistes*, ed. by Jean Balcou (Paris-Geneva: Champion-Slatkine, 1987), pp. 175–83.
38. See de Place, 'Le Bureau', 1988.
39. Lalande quoted by Feurtet, 'Le Bureau', p. 77.

40. Bernard Plongeron, 'Introduction', in Henri-Baptiste Grégoire, *L'abbé Grégoire et la République des savants* (Paris: Editions du CTHS, 2001), p. 54.
41. Feurtet, 'Le Bureau', p. 117.
42. While Lalande dreamed of building the biggest telescope in the world, Borda and Caroché experimented with achromatic lenses, which seemed more promising than mirrors. Borda was also interested in improving reflecting instruments for use at sea; see Feurtet, 'Le Bureau', p. 116–20.
43. On geodesy, see Schiavon, 'Geodesy', pp. 200–18; see also Kershaw's chapter in this volume.
44. On Laurent, see Brenni, 'Soleil, Duboscq', pp. 3–8.
45. Schiavon, *Itinéraires de la précision,* pp. 632–33.
46. French precision instrument makers generally preferred to maintain artisanal traditions until the First World War; see Schiavon, *Itinéraires de la précision*, Chapters 3–4; for a comparison with foreign makers, see also Bernward Joerges and Terry Shinn, *Instrumentation between State, Science and Industry* (Dordrecht: Kluwer Academic Publishers, 2001).
47. Firstly placed under the Comité d'Instruction publique, the Bureau was placed, until 1831, under the Ministère de l'Intérieur. From March 1831 to October 1832, it came under the Ministère du Commerce before being definitively transferred to the Ministère de l'Instruction publique (now Ministère de l'Enseignement Supérieure et de la Recherche).
48. For instance, the minutes from 1892 report negotiations with the representative of the Quito government in Ecuador, Antonio Flores Jijón. The purpose was to interest the French government in a new meridian arc measurement to reconstruct the arc of Peru with more precision and take charge of Quito Observatory. The minutes also detail aspects of the history of the meridian arc measurement in South America (1901–06) that are not found in: Lewis Pyenson, 'Ciencia pura y hegemonía política: investigadores franceses y alemanes en Latinoamérica', in *Nuevas tendencias en historia de la ciencia*, ed. by A. Lafuente and J.-J. Saldaña (Madrid: CSIS 1987), pp. 195–215; Martina Schiavon, 'Les officiers géodésiens du Service géographique de l'armée et la mesure de l'arc de méridien de Quito (1901–1906)', *Histoire & Mesure*, XXI-2 (2006), 55–94.
49. Going beyond the period studied in this book, a good example is Henri Poincaré. The minutes of the Bureau contain about 500 unedited manuscripts by him, mostly administrative, but also including studies submitted for comment from Bureau members (some are contained in Scott Walter, Ralf Krömer, Philippe Nabonnand and Martina Schiavon (eds), *La correspondence entre Henri Poincaré et les astronomes et géodésiens* (Basel: Springer-Birkhäuser, forthcoming)). While Poincaré is a well-known mathematician, these manuscripts offer another image, of a scientific administrator constantly and actively engaged with the applied sciences, something shown in his exchanges with Hervé Faye and Robert Emile Bourgeois concerning geodesy; see Martina Schiavon, 'Poincaré, membre du Bureau des longitudes, et la géodésie (1893–1912)', *Journée de commémoration du centenaire de la disparition de Henri Poincaré, 29 avril 1854–17 juillet 1912*, Paris, Institut d'Astrophysique de Paris, 9 July 2012 www.canal-u.tv/video/cerimes/poincare_membre_du_bureau_des_longitudes_Et_la_geodesie_1893_1912.10019> [accessed 5 May 2015].
50. See Higgitt and Dunn, 'The Bureau and the Board'.

51. In Britain, Airy did something analogous to Le Verrier and Bigourdan, classifying the papers of the Board of Longitude to better document his history of the Royal Observatory (Schiavon, 'The English Board', pp. 178–79).
52. See Simon Schaffer, 'Astronomers Mark Time: Discipline and the Personal Equation', *Science in Context*, 2 (1998), 115–45; Fabien Locher, 'L'empire de l'astronome: Urbain Le Verrier, l'ordre et le pouvoir', *Cahiers d'histoire critique*, 102 (2007), 33–48. On the institutionalization of astronomy at the beginning of the twentieth century, see Arnaud Saint-Martin, 'French Astronomy during the Belle Époque. The Professionalization of a Scientific Activity', *Sociologie du travail*, 54 (2012), e53–e72. On the organization of a network of French astronomical observatories under the Third French Republic (1870–1940), see Jérôme Lamy, *L'Observatoire de Toulouse aux XVIIIe et XIXe siècles. Archéologie d'un espace savant* (Rennes: PUR, 2007); Laëtitia Maison, 'La fondation et les premiers travaux de l'observatoire astronomique de Bordeaux (1871–1906): histoire d'une réorientation scientifique' (unpublished PhD thesis, Université de Bordeaux I, 2004).
53. George Airy, 'Remarks on the History and Position of the (now abolished) Board of Longitude', 18 May 1872, in Royal Commission on Scientific Instruction and the Advancement of Science, *Minutes of Evidence, Appendices, and Analyses of Evidence, Vol. II* (London: HMSO, 1874), Appendix VIII.
54. This Commission was organized on the 28th October 1853 by the Ministère de l'Instruction publique and was composed of: marshal Vaillant, Biot, Dumas, admiral Baudin, Binet, Le Verrier and Charles Fortoul. Feurtet studied the work of the Commission and showed that Airy was asked about the administration of the Royal Observatory (Feurtet, 'Le Bureau', p. 479).
55. 'Quant aux observatoires de province, suivant l'expréssion pittoresque de l'astronome de Zach, [le Bureau les a] complètement dégringolée'; Urbain Le Verrier, 'Historique de l'Observatoire de Paris' addressed to the minister in 1864, AN F/17/3721. On Franz-Xaver von Zach, astronomer of the Duke of Gotha, and the question of the Observatoire de Marseille, see Jean-Marie Feurtet, 'Une lente mise en orbite. L'Observatoire de Marseille et le Bureau des longitudes (1795–1822)', in *Le Bureau des longitudes*, ed. by Schiavon and Rollet.
56. Arago and Faye, for instance, played an important part in popularization (see Colette Le Lay, 'L'annuaire du Bureau des longitudes et la diffusion scientifique: enjeux et controverses (1795–1870)', *Romantisme*, 166 (2014), 21–31; Colette le Lay, 'Hervé Faye, diffuseur de l'astronomie', in *Hervé Faye*, ed. by Boistel, Le Gars and Le Lay, pp. 63–71.
57. On computation, see Boistel's chapter in this volume.
58. Schiavon, 'Hervé Faye'.

Part II

Longitude in Transnational Contexts

6

Patriotic and Cosmopolitan Patchworks: Following a Swedish Astronomer into London's Communities of Maritime Longitude, 1759–60

Jacob Orrje

On 15 July 1759, Bengt Ferrner was a long way from home. The Swedish astronomer was touring Europe and had just arrived in London. In his travel journal, he professed to being shocked and bewildered by the chaotic city life: the area around Leadenhall Street, where he arrived, was nothing like he had imagined the city. Ferrner – a man who repeatedly praised orderly and well-arranged structures in his journal – considered the streets crowded, intricate and hard to navigate. He likened the King's Arms tavern, where he disembarked, to a den of thieves.[1] Nonetheless, he would stay in Britain for over a year and soon took an interest in the city's scientific communities as well as its theatre, music and industry.

Ferrner – like any true stargazer – was very much an observer and spectator. In London, he frequented the theatre houses of Covent Garden and Drury Lane, listened to operas at Haymarket and attended concerts.[2] He also watched theatrical displays of science. He described the loud bangs and dramatic effects generated by the electrical experiments of Edward Nairne, as well as his disappointment at the display of a fire hose that did not meet the audience's expectations. He paid to see marvels too: a very tall woman and a living crocodile from the Nile.[3] But Ferrner was not primarily a curious spectator of the marvellous and dramatic. One of his main interests was naval science in a broad sense – conducted in the intersection between astronomical observatories and the maritime world of naval academies and dockyards. His journal contains extensive information about what was happening in this field.

When Ferrner visited Britain, Europe and its colonies were engulfed in the Seven Years' War (1756–63). The war was a struggle for control of the North American colonies, and largely took place on the seas. The British navy was fully mobilized and Ferrner had many opportunities to study the relationship between science and navy.[4] The Swedish astronomer also arrived in the midst of another conflict: that between proponents of different methods for determining longitude at sea. In the centre of these events were the British Admiralty and the Board of Longitude, created under the Longitude Act of 1714, which oversaw rewards up to £20,000 for a sufficiently accurate method. Apart from naval officers, the Board included the President of the Royal Society, the professors of astronomy at Oxford and Cambridge, the Astronomer Royal and the Speaker of the House of Commons. Maritime longitude was an issue of great concern to the political and commercial interests of the British state.[5]

The astronomers, mathematicians and instrument makers who dealt with the Board were hardly a homogeneous community, and there were different approaches to solving the longitude problem. Dunn and Higgitt identify a number of contending solutions: by rockets, magnetic variation and inclination, eclipses of Jupiter's satellites, the Moon's movements and by comparing a ship's local time, found by celestial observation, to the time at a place of known longitude kept by a precision timekeeper.[6] In 1759–60, John Dollond was perfecting the achromatic lens, which was central to exact astronomical observations. Meanwhile, astronomers were working on celestial solutions to the problem and John Harrison was preparing his sea watch ('H4') for its first sea trial. In London, Ferrner encountered these men working on celestial and mechanical solutions to maritime longitude, and repeatedly wrote of his encounters in his journal.

After his death in 1802, Ferrner was described as someone who 'had seen several kinds of people, courts and nations,' and who 'knew his way around in all sorts of social circles'.[7] These social skills are evident in Ferrner's journal. He met and discussed navigational issues with almost everyone who was anyone, noting their answers in his travelogue. His journal thus gives a stranger's perspective on a crucial time in naval science and astronomy in Britain. Ferrner moved relatively easily between taverns, workshops, observatories and naval bases. However, in order to enter these spaces, Ferrner needed to use a wide array of contacts and resources.

Bengt Ferrner was both a travelling spy and a man of science: he aspired to participate in the scientific communities of London and to learn of British technological and scientific developments for the benefit of the Swedish state. Similarly, the men he interacted with were correspondents of a Republic of Letters, yet also involved in matters of importance to the British state by virtue of the Longitude Act. Because of this double nature of Ferrner's visit and the research he observed, his journal makes an ideal case study of the frictions between patriotic and cosmopolitan ideals in eighteenth-century navigational sciences. How can one write a European history of research such as that on maritime longitude, which national interests drove to such a high degree? By studying how Ferrner simultaneously adopted national and cosmopolitan identities in order to make and circulate navigational knowledge, I aim to integrate national stories of longitude research into a larger transnational one.

Ferrner on naval power

Bengt Ferrner (Figure 6.1) was born in 1724, just a few years after the end of the Great Northern War (1700–21). In the aftermath of its military defeat and the death of Karl XII, Sweden had been reformed into a constitutional monarchy with a weak king and strong parliament. The

Figure 6.1 Bengt Ferrner (1724–1802), oil painting, by Jean Hugues Taraval, 1762. Photo: Erik Cornelius/Nationalmuseum Sweden

new government adopted a policy of economic betterment, according to which the loss of provinces during the war was to be compensated by improving Sweden's remaining possessions. Improvement, according to the cameralist logic of the time, came through a state-governed effort to improve domestic mining, agriculture and manufacture, and by maintaining a positive balance of trade. The natural sciences, including astronomy, played an important part in these efforts.[8]

As shown in other contributions in this book, a wide range of European states and empires took an interest in maritime longitude. Sweden's faltering Baltic empire was no exception. Whereas the Swedish state had no regular institution analogous to the British Board of Longitude or the French Bureau des longitudes, solutions to the problem of determining longitude at sea were discussed repeatedly in a number of forums. Already in 1710, an Uppsala dissertation presided over by Pehr Elvius discussed (unsuccessful) magnetic and astronomical solutions to determine maritime longitude, as well as the various European rewards for successful solutions. From this time, work on maritime longitude began to interest Swedish visitors to Britain, including Emanuel Swedenborg, who visited London in 1710–12.[9]

By the late 1750s, the Swedish state considered naval reform central to defence against a growing Russia, and mathematically educated shipbuilders such as Fredrik Henrik af Chapman were engaged at the naval centre of Karlskrona to construct vessels suitable for the Baltic.[10] Astronomy was an integrated part of this naval effort. For example, the astronomical teaching at Uppsala University in the 1750s contained a course in navigation, and when the parliament of 1755–56 discussed Swedish naval education, it was decided that the civil director at the new naval academy in Karlskrona should be a professor of astronomy and mathematics.[11]

Ferrner had studied astronomy and mathematics at Uppsala with Mårten Strömer and Samuel Klingenstierna, and he was part of the influential astronomical and mathematical networks that introduced Newtonian science into Sweden.[12] Consequently, he became a junior member (*ämnessven*) of the Swedish Royal Academy of Sciences in 1748, at the age of 24, and three years later became the practical astronomer at Uppsala.[13] Through the help of Strömer, Ferrner was appointed to the position at Karlskrona on 5 November 1756.[14] In order to attain this position, in the autumn of 1756 Ferrner and Strömer held two lectures at the Swedish Royal Academy of Sciences on the relation between astronomy, navigation and state power. First, Ferrner held an address on 'Naval power', in which he discussed the relationship between astronomy, navy and state using examples from great ancient and modern European empires. Ferrner argued that for most empires 'strength at sea' was 'the correct thermometer, by which one should judge their power'. He argued, therefore, that astronomy and mathematics, if applied to navigation, could restore Sweden to its former greatness.[15] In his response to Ferrner's address, Strömer again stressed the importance of the relation between the sciences and naval power. He also conveniently proposed that Ferrner was the appropriate man to cultivate this relationship.[16]

Two months later, Strömer held his own 'Address on the connection between astronomy and navigation'. Unlike Ferrner, Strömer argued for a reciprocal relationship between astronomy and navigation: astronomy was the key to developing navigation, and navigation was pivotal to the circulation of local astronomical observations, a process by which astronomical observations were made universally valid. For Strömer, this patriotic science did not stand in opposition to transnational astronomical projects; instead, he argued that national power depended on voyages and a transnational circulation of knowledge.[17] The fact that Ferrner and Strömer gained control over the naval academy shows that influential state officials saw merit in these arguments. It was in the public interest to promote astronomers and naval academies, as well as the circulation of instruments, men and knowledge.

Ferrner's travelogue

Ferrner never took up the professorship at Karlskrona. Instead, in 1758 he accepted an offer from the Swedish industrialist and exporter of copper Jean Henri Lefebure, to tutor his 22-year-old son Jean on a European tour. Ferrner kept notes of this tour and compiled them into a neatly handwritten travelogue when he returned to Sweden. Three of the original four parts are stored in the National Library of Sweden (the fourth, on Italy, has been lost) and were published in a Swedish edition of 547 pages in 1956. The 206 pages of this edition concerning Ferrner's visit to Britain are the basis of this chapter.

As with any form of travel writing, Ferrner's travelogue is not just passive description. His writing and editing constitute performances for a set of audiences, whose expectations also shaped the written text.[18] Therefore, one must think about the journal's intended audiences and look to other complementary sources to get a more accurate picture of his visit to London. While in Britain, Ferrner corresponded with several members of the Swedish scientific networks of which he was part. These letters form a second set of sources, against which the journal can be juxtaposed. Together, the journal and the letters show Ferrner's perspective on everyday life as a foreigner interacting in European astronomical and navigational research in the mid-eighteenth century.[19]

From the late seventeenth century, many Swedish scholars and civil servants toured Europe to advance their careers. On their travels, they collected technical, commercial and scientific information of interest to Swedish universities and the state bureaus in Stockholm.[20] Their reports were generally short and written in an impersonal, matter-of-fact style.[21] Historians have considered such Swedish travel journals, including Ferrner's, to be important sources on eighteenth-century British technology.[22] Written for the Swedish state administration, they describe industrial sites, processes and machines in detail. Ferrner's journal differs somewhat from these technical travelogues: it focuses on his social encounters as much as on technical details of scientific instruments and industrial sites. Its diverse contents reflect Ferrner's disparate interests and the diverse interests of his many audiences.

Of course, Lefebure's father, who had financed the voyage, was the most obvious reader of the journal. On his tour, young Lefebure was supposed not only to fraternize with European high society but also to visit industrial sites to learn about things that would help him take over his father's business. Consequently, Ferrner's journal mixes accounts of visits to European political centres with descriptions of provincial industrial sites. If he ever read Ferrner's journal, Jean Henri Lefebure would have been assured that he had gotten his money's worth and that his son's experiences had been appropriate for an heir to the family business. But Lefebure was hardly the only intended reader. As noted, Ferrner was a member of the Swedish Royal Academy of Sciences. Its astronomers and mathematicians were another audience interested in Ferrner's effortless and cordial interactions with prominent European men of science. Ferrner presented himself to this audience as a cosmopolitan member of a European Republic of Letters, and also a loyal ambassador of Swedish science who tirelessly promoted his Swedish colleagues.

The journal also reflected the military and naval interests of the Swedish state. Ferrner, who believed that he would be in charge of the education of naval cadets after his return, took an interest in new navigational techniques and education during his tour.[23] His seemingly cosmopolitan interactions with European science were therefore partly motivated by the Swedish navy's interest in the latest developments in navigation. Consequently, national and transnational narratives are intertwined in Ferrner's journal. Following these threads, one can delineate how Ferrner relied on both patriotic and cosmopolitan identities when he entered London's navigational communities.

Arriving in London

In July 1759, Ferrner and Lefebure stood confused outside the King's Arms in east London. The two did not stay there long: soon they met the manservants of Abraham Spalding and Gustavus Brander, two merchants of Swedish descent who were important players in the Anglo-Swedish iron trade. At Prince's Square – in the maritime suburb of Wapping, east of London – lay a Swedish Lutheran church.[24] The church, funded by the Swedish state, was a social space that glued together a Swedish migrant community in Wapping. In 1739, when the church's former minister Jacob Serenius applied for financial aid from the Swedish state, he described the patriotic role of the church. He argued that if sailors, artisans and merchants there could practise their Lutheran faith and the Swedish language, they would maintain ties to the home country.[25]

Spalding and Brander were important members of this congregation. Even so, they should *not* be considered Swedes in an absolute sense. To them, Swedishness was one national identity of many. While active in the church, they also displayed the British patriotism expected of the London elite. Brander, who was no stranger to London high society, was perhaps the clearest example of this double affiliation. He was a fellow of the Society of Antiquaries from 1749, of the Royal Society from 1754, a trustee of the British Museum and a Director of the Bank of England between 1762 and 1779.[26] He thus both performed the roles of Swede, London gentleman, collector, amateur man of science and middleman in the Anglo-Swedish metal trade. Brander is a reminder that we should not see eighteenth-century patriotisms and cosmopolitanism as incompatible. Displays of diverse patriotisms and cosmopolitanism seem to have been expected of a man in his position, who based his wealth on the transnational circulation of goods and people.

For Swedish visitors to London, this community became a bridge to other parts of the city. The natural historian Pehr Kalm, who visited London in 1748, described this clearly in his journal:

> Immediately upon my arrival I addressed myself, according to the instructions given me by the Royal Academy of Science of Sweden, to *Mr. Abraham Spalding*, a Swedish merchant in London, who afterwards, during the whole of my visit to England gave me every imaginable information, help, advice, and explanation of various things; recommended me, partly himself, partly through his friends, to all the places I had occasion to visit, or where there was anything remarkable

> to see; lent me all the money I required for the whole of my foreign travels, and besides that, showed me manifold kindness.[27]

In Wapping, visitors could speak their native language before learning to communicate in English, and could receive help from families who, for various reasons, wished to maintain a connection to Sweden. Spalding and Brander quickly arranged for Ferrner and Lefebure to stay with a retired schoolmaster in Walthamstow for two months from 20 July. There they learned English in a rural environment that was separate from both the Swedish-speaking community in Wapping and the chaotic life of London. During this time, they only made day-trips into the city. Only after his return from Walthamstow did Ferrner wholeheartedly start to engage with London's astronomers, mathematicians and instrument makers.[28]

The instrument makers' workshops

Ferrner's journal entries from September 1759 show how his passion for astronomy, mathematics and the maritime intersected in an interest in the latest developments in nautical instruments. During a day trip to London on 6 September, he dined on a Swedish ship with his hosts Spalding and Brander. On the eleventh, he visited the East India Company shipyard in Blackwall, where he watched large and small vessels sail by and observed how the tide filled and emptied the docks used to repair ships. On the sixteenth, he took the stagecoach to London to meet the instrument maker James Short and discuss astronomical and nautical instruments.[29]

Ferrner began visiting instrument-making workshops before returning permanently to London and before he had initiated contact with the mathematicians and astronomers of the city. In eighteenth-century London, the acquisition of a scientific instrument was often a lengthy process.[30] Ferrner made repeated visits to several makers, engaging in discussions, watching demonstrations and gaining promises of drawings of instruments. Ferrner used his Swedish network to approach these artisans: he forwarded greetings and letters from patrons such as Klingenstierna or the astronomer Pehr Wargentin. Sometimes his visits lasted entire days. For example, in his entry for 3 March 1760, Ferrner noted how he spent the whole day visiting Short, Dollond and Bird.[31] Spalding and Brander maintained contacts with London's instrument makers over an even longer period. In their letters to Wargentin, for instance, they itemized the costs of lenses, telescopes and microscopes that they sent to Stockholm.[32] Spalding also accompanied Ferrner to the workshops on some occasions.[33] The Swedish merchants thus facilitated Ferrner's contacts with the instrument makers.

In his interactions with instrument makers, Ferrner was both a fellow expert, curious about their work, and an agent of Swedish scientific networks. Thanks to his knowledge of astronomy and navigation, he could engage in conversations in a way Spalding and Brander could not. The instruments became a medium through which Ferrner and the makers could communicate using the skills and gestures of shared techniques. During his first visit to Short, he discussed 'the Reflexion [reflecting] Telescope with Dollond's micrometer for the Uppsala observatory that [Short] promised to complete by the end of the month'. The two also discussed a telescope with a 12-foot focus that Short had made for a Dr Stephens. On the thirtieth, Ferrner returned to examine the now completed reflecting telescope for Uppsala. He found it, 'as well as [he] could see', to be 'completely functional'.[34]

Although they might appear so in the journal, these interactions were not without friction. In describing a visit to John Dollond on 5 October, for example, Ferrner wrote that he presented Klingenstierna's compliments and that Dollond professed how Klingenstierna's demonstration had inspired his discovery of how to reduce chromatic aberration though 'the combination of green and white glass'. Dollond promised to send one of his prisms to Klingenstierna as a gift, and Ferrner returned on the thirteenth to collect the prism together with a letter in which Dollond acknowledged the role of Klingenstierna's demonstration in his invention. Although the journal describes the meeting with Dollond as polite, Ferrner's correspondence with Klingenstierna reveals underlying tensions, sparked by a priority dispute between Dollond and the Swedish mathematician. There, it is evident that Ferrner distrusted Dollond's mathematical skill and his ability fully to grasp Klingenstierna's theory: 'I have much trust in Mr Dollond's knowledge in optics, and it is not his fault if he does not have enough knowledge in mathematics to explain himself'. In Klingenstierna's correspondence with members of the Royal Society, he regularly mentioned Ferrner as a go-between who looked after Klingenstierna's interests in London.[35] The way Ferrner represented his encounters with British science was thus not completely consistent across different media, but varied depending on to whom he wrote and his reasons for writing.

On 5 October, Ferrner went to Jeremiah Sisson's workshop to see 'Esquire Irwin's Balance Chair, for sitting in when at sea'.[36] However, Sisson was not at home and Ferrner was unable to meet him until the twelfth. Ferrner described how Sisson had placed Christopher Irwin's chair 'on top of his house' and made a hole through the roof 'for the balance'. Irwin had put his chair forward as the solution to the problem of 'taking several necessary Observations [of the celestial bodies] on board, notwithstanding the Tossings of the Ship', but Ferrner was not convinced that it would fulfil this purpose.[37] Although the invention was 'polite and comfortable', he was concerned that the ship's movements would make observations inaccurate. Nevertheless, Sisson promised Ferrner a drawing of the chair and an estimate of its cost.[38] Irwin's book is included in a posthumous catalogue of Ferrner's library, so Ferrner must have taken subsequent interest in his method during his stay in London.[39]

Three days later, Ferrner visited the workshop of the clockmaker John Harrison to see his marine timekeepers. Ferrner noted no more of this first meeting than that he met Harrison and saw his invention. He did not meet Harrison again until 4 August 1760, when Harrison told him that he would go to sea with 'his sea watch' next April.[40] In the middle of the eighteenth century, Harrison's workshop was a known attraction, and some even paid to see his inventions. Most visitors were simply curious, but some also sought to duplicate Harrison's work.[41] Ferrner was one of the curious, lacking any substantial expertise in mechanics. However, on one of his provincial tours through Britain, he met and talked to a man who claimed to have duplicated Harrison's clock. During the summer of 1760 when travelling in Scotland, Ferrner met watchmaker and mechanic Alexander Cumming. He showed Ferrner a pendulum clock 'that he had made in imitation of some aspects of Harrison's sea clock after having seen it only a couple of times'.[42]

Overall, Ferrner's encounters with the instrument makers of London show one aspect of how the Anglo-Swedish circulation of scientific knowledge and instruments depended on national networks intertwined with transnational ties. In London's workshops, Ferrner was not only a representative of his Swedish patrons, but also a fellow expert who could engage in conversation fuelled by a common interest in instruments and techniques. The workshops were commercial spaces, and merchants such as Spalding and Brander could act as middlemen to a degree. But what was being traded in the workshops was not simply homogeneous merchandise. The expertise of a trained astronomer and mathematician like Ferrner was a way of guaranteeing the quality of the merchandise and of learning more about the construction of instruments.

Instrument-makers' workshops provide one model to understand the transnational circulation of scientific knowledge and objects between networks that were also integrated within their respective European states. In these spaces, scientific exchange was not much different from other commercial endeavours. There, the social practices of transnational trade formed a bridge by which a foreign expert such as Ferrner could acquire the navigational knowledge and instruments he coveted.

Socializing in London science

The correspondents of Ferrner's Swedish patrons gave him access to London's astronomical and mathematical communities. The most important of these contacts was the clockmaker John Ellicott, a member of the Swedish Academy and the Royal Society who maintained a frequent and cordial correspondence with Wargentin until his death. He also sent scientific instruments and new editions of the transactions of the Royal Society to Wargentin via the Swedish merchants in Wapping, thus acting as a middleman much like Ferrner. In return, Ellicott received astronomical data and publications from Wargentin, which he translated and submitted to the *Philosophical Transactions*.[43] Ellicott's name appears throughout Ferrner's journal, as well as in Ferrner's correspondence with Wargentin, and it was he who introduced Ferrner to the scientific communities of the city.[44]

It would be a mistake to see Ferrner's contacts with London astronomers as mediated by the Royal Society as an institution, although the social gatherings of its members gave Ferrner opportunities to enter the upper strata of London's scientific communities. On 4 October, Ferrner dined at 'the Royal Society Club' at the Mitre Tavern in Fleet Street. This private club – consisting of Fellows and their invited guests – met on Thursdays before Royal Society meetings just across the street. On his first visit, Ferrner socialized with Ellicott, Thomas Birch (Secretary of the Royal Society) and nine other Fellows.[45] Though Ferrner does not mention who invited him, it seems likely that it was Ellicott.

Ferrner found the company 'entertaining' and the dinner well worth the three shillings it cost. During his year in London, he travelled across London almost every Thursday to attend these gatherings, where he made new friends and contacts.[46] At one dinner, on 8 November 1759, Birch presented him to the President of the Royal Society, the Earl of Macclesfield, who welcomed Ferrner to Britain. Macclesfield had heard from Astronomer Royal James Bradley that Ferrner had asked about the astronomical observatory in Oxford. He invited Ferrner there and promised him that the professor of geometry, Nathanael Bliss, would show him Oxford, if he himself were absent. Bliss, who was also at the dinner, promised to introduce Ferrner to anyone he wished to meet in Oxford and show him 'astronomical observations, particularly of *Immersiones & Emersiones satellitum Jovis*'.[47]

The contacts Ferrner established at the Royal Society Club opened up his further passage into London's astronomical and mathematical communities. On 26 November, Ellicott invited Ferrner and his pupil Lefebure to another private society of 'scholars, traders and artisans', who met every Monday at the George and Vulture Tavern at the end of George Yard in east London. There, Ferrner met Benjamin Franklin and his son William, John Smeaton, Gowin Knight and the Cambridge astronomer John Michell.[48] After returning to America, Franklin wrote to Ellicott: 'I shall always remember with Pleasure the agreeable Hours I pass'd in that chearful [*sic*], sensible and intelligent Society. The Monday scarce comes round but I think of you and am present with you *in Spirit*'.[49] At Ferrner's first visit to the Monday club, Franklin and Ferrner discussed 'Linnaeus and Professor Kalm, the latter whom [Franklin] had met in America'.[50] Franklin does not discuss Ferrner explicitly in any of his letters, but on 11 June 1760, he mentions to Mary Stevenson 'a particular late Instance which I had from a Swedish Gentleman of Good Credit', most likely Ferrner.[51]

Visits to these clubs were another way for Ferrner to establish contacts with astronomers, mathematicians and artisans. Word also spread of the Swedish astronomy professor visiting the city. While Ferrner entered these clubs as a representative of a Swedish scientific network, as time went by he forged a personal network through repeated socializing. Soon these new contacts would invite Ferrner to their homes; for example, Franklin and Knight each invited Ferrner to watch electrical and magnetic experiments.[52] Having established such personal ties, Ferrner gradually came to be identified both as a Swede and as a fellow man of science.

In his journal, Ferrner thus paradoxically presented a complex transnational scientific network in which national identities were central. This complex network is perhaps most evident in Ferrner's encounter with the mathematician Thomas Simpson, amid growing tensions between Britain and France. Simpson – whom Ferrner noted had a 21-year-old son serving as a lieutenant in the British army in America – wondered whether Klingenstierna might publish a theory of the lunar movements, as he did not know anyone more capable of doing so. 'The way the French treat the subject is so diffuse and disorderly, especially d'Allembert's [*sic*], that one can await little use thereof', he wrote. Simpson did not want to hear any mention of Euler, while the integral calculus of the young mathematician Louis Antoine de Bougainville, who at that time was defending French Quebec from the British, was 'a new proof of the boastfulness and big words of the French'. According to Simpson, in his preface he made 'himself a censor and [did] the English nation a great wrong'. Any day, Simpson continued, 'Mr Maskelyne will refute him, when he has time to finish his remarks of his book'. Then, Simpson continued, 'Bougainville will need all his French talk to explain himself'.[53]

Although Ferrner may well have dramatized his conversation with Simpson to meet his readers' expectations, it shows that Ferrner, or his readers, were not alien to seeing European science as a complex interplay between national identities. Considering how European mathematicians and astronomers were deeply involved in naval and military matters, it is unsurprising to see patriotic sentiments sparked by conflict. These national identities were not simple opposites, but related to each other dynamically. According to Ferrner, Simpson appreciated Klingenstierna's work because he disliked French mathematicians working on similar scientific questions. This is not surprising: Simpson had taught mathematics at the Royal Military Academy in Woolwich since 1743. He had also been elected fellow of the Swedish Academy of Sciences in 1758, just a year before meeting Ferrner.[54] That Ferrner originated from a country less involved in the war facilitated his interactions with men such as Simpson and made it easier for him to mediate contacts between his Swedish and British contacts.

Observatories and naval bases

While instrument-makers' workshops and dinner clubs were entryways into the scientific communities of London, observatories and naval bases were warehouses of relevant information, where Ferrner could learn about British navigational techniques and instruments. One such place was Greenwich. On 26 October 1759, Ferrner visited the Royal Observatory, where he dined and spoke to James Bradley. Again, Ellicott introduced him. In his journal, Ferrner noted how Bradley 'had been in London and asked for [Ferrner], as Mr Ellicott had informed him that a Swedish professor of astronomy had arrived in London'.[55] Bradley invited Ferrner to dine with him and before dinner showed him the Observatory and its instruments. Ferrner compared them to the instruments he knew from Stockholm and Uppsala.[56] A three-foot quadrant by Bird was of the same kind as one in Stockholm Observatory, and the eight-foot tube had a micrometer made by Sisson that was similar to that at Uppsala. Similarly, the zenith sector was of the same kind as that in Uppsala, as was the regulator by Graham. The instruments made by London's makers were familiar not only to Ferrner and his contacts in London, but also to the Swedish readers of his journal. During dinner, Ferrner asked Bradley for his opinions on the problem of finding longitude at sea. Bradley answered that he considered tables of the Moon's motions to be the likeliest solution. The two then drank to Swedish men of science, including Klingenstierna and Wargentin, expressing the international bonds between astronomical communities and the patriotic nature of eighteenth-century networks.[57]

The dinner with Bradley was an opportunity to acquire astronomical information. Ferrner asked Bradley for observations of the satellites of Jupiter that corresponded to those made in Sweden. On 26 April, Ferrner returned to the Observatory to request these observations again, which 'Professor Strömer now needed for his map office', but Bradley was not at home and Ferrner met his assistant Charles Mason. On Ferrner's previous visit, Bradley had complimented Mason, who had been at the Observatory for three years, and now Ferrner flattered him by inviting him 'to dine [...] at a tavern by Greenwich Park'. His strategy was successful: having eaten, the two returned to the Observatory and Mason gave him access to the astronomical journals, to 'pick out whatever [he himself] wished'.[58]

Ferrner did not only take an interest in nautical research in London; he also made three provincial tours of England and Scotland, visiting industrial areas as well as the universities of Cambridge, Oxford and Edinburgh. In February 1760, he visited the naval dockyards at Plymouth and Portsmouth. In his entries from these visits, the interplay between patriotic sentiment, state interest and cosmopolitanism was just as evident as in his account of London science.

The sea was in turmoil when Ferrner reached Plymouth; onlookers had never seen waves as high. The next morning, news was spreading through the town that the storm had driven Admiral Boscawen back to Plymouth and that his largest ship, the *Ramillies*, was lost with most of its crew of 750. The commercial network of his hosts in Wapping facilitated Ferrner's access to the naval base. On arrival, he gave a letter of recommendation from Spalding and Brander to John Mignan, a 'French' merchant who brokered the contact between Ferrner and one of the commissioners at the dockyard.[59] The commissioner allowed Ferrner and his companion to see 'the dock, the yard, the storehouses, and everything [they] wished'. Ferrner took notes on the facilities around the dockyard, which he compared to installations at home. He noted how the workers were diligent and worked double shifts because of the war.

He was also told that, 'all the iron used here was Swedish and was valued […] as the best in the world' – a nod, perhaps, to the tour's financier.[60]

However, the help of the 'French' Mignan raised questions about Ferrner's identity. Ferrner had visited on payday and he described a town filled by a drunk and disorderly workforce. A worker attacked Ferrner and his guide, Mr Squire (Mignan's accountant), calling him 'French dog, Rascal &c'. The dockworkers, he noted, probably thought that he and Lefebure were 'French, or at least strangers'. Another shouted after them, 'God damn you West Country Souls'. Ferrner wrote that this man was from eastern England and did not like the people from western parts. He added that it was not strange that the English were so rude to strangers, when there was such an 'antipathy between the [English] provinces'.[61] As in London, the inhabitants of Plymouth identified Ferrner through many identities. To the workers, he was a stranger not primarily because he was Swedish, but because his origin was unclear. Whatever the basis of this perception – be it Frenchness or something else – it affected his ability to move about Plymouth.

Ferrner's description of Portsmouth was a complete contrast. He appreciated order and tidiness, and to him Portsmouth was the most 'beautiful and the most regular town in England, with broad and well-kept streets, which intersect at right angles'. He visited the Royal Naval Academy and its headmaster, the mathematician John Robertson.[62] Robertson, who had been an apprentice before becoming a mathematician, had been headmaster of the Academy since 1755. An expert in navigation and naval fortification, he had previously taught at the Royal Mathematical School at Christ's Hospital and in 1754 had published the popular *Elements of Navigation*. As head of the Academy, he was directly involved in work for the Board of Longitude.[63] In 1763, for example, Robertson was charged with determining the local time at Portsmouth and setting Harrison's sea watch as part of the Barbados trial of H4.[64] Robertson's career also shows how mathematics was part of the British navy in more ways than just the quest for longitude.[65] As Boistel has noted, educating naval officers in mathematics was a way to encourage the adoption of the latest navigational techniques; poor mathematical education, by contrast, became an obstacle to determining longitude at sea by astronomical methods.[66] As seen previously, Ferrner and the Swedish state also recognized the relevance of mathematics for navigation. In Portsmouth, Ferrner could witness mathematics and navigation taught together at a naval college, something that might be relevant to his position in Karlskrona.

Ferrner was able to enter Portsmouth because he was Robertson's fellow mathematician. Whereas Ferrner's identity in Plymouth was unclear, in Portsmouth Robertson readily identified him as a Swedish mathematician. This established a clear framework for their interaction: Ferrner's interactions with Roberson much resembled those with the astronomers and mathematicians in London. The two gossiped about, 'Mr Thomas Simpson in Woolwich', and 'about Greenwich'. They also discussed the mathematician William Emerson, who had recently published A *Treatise of Navigation* (1755) – and Robert Heath, who had edited *The Ladies' Diary* until 1753 when he was dismissed after a quarrel with Simpson. Robertson stated that, 'Simpson was undeniably the greatest mathematicus, gifted with an excellent genius, polite; but reserved and uncommunicative as well as difficult to have dealings with'. Emerson was 'surly, lives alone in the country-side off a small interest of £80 Ster[ling], is very indifferent to whether anyone visits him, fishes and hunts what he needs'. In spite of these eccentricities, Robertson considered him the second greatest mathematician in England. Heath on the other hand 'was not a bad mathematicus; but of such a bad character and disposition that he did not deserve anyone's company'. Nonetheless, Robertson considered the three mathematicians 'nothing less than his friends'.[67]

Such exchanges, in which British mathematicians ranked each other according to skill and morals, appear repeatedly in Ferrner's journal. While retaining a polite façade, the British mathematicians of Ferrner's journal revealed the secret of social relations within the community. Having revealed fierce competition backstage, they re-established the facade with the contradictory statement that, in spite of all that had been said, the mathematicians were good friends. This glimpse behind the scenes of British science shows that national networks of sciences were not monolithic but were a patchwork of networks and alliances. In his journal, Ferrner gave his readers a window onto the scientific networks of another nation, which were otherwise invisible to the arms-length relationships of the Republic of Letters.

Ferrner carefully described the organization of the Academy in Portsmouth, taking note of activities such as fencing and drawing, as well as courses in shipbuilding and mathematics. As at Plymouth, Ferrner gave detailed accounts of buildings and the dockyard.[68] His visits to Plymouth and Portsmouth show how closely integrated were Ferrner's roles as industrial spy and visiting astronomer and mathematician. He pragmatically used the resources available to him – networks of state, commerce and science, as well as patriotic and cosmopolitan social structures – to enter places that otherwise would be closed to a foreign national.

Conclusion

Ferrner's journal and letters challenge the traditional history of eighteenth-century European science as a frictionless international collaboration. Historians of science have identified a major change in the Republic of Letters in the late eighteenth century, when patriotic values began to be associated with the pursuit of knowledge.[69] Still, patriotism is ever-present in Ferrner's journal of 1759–60. It was important both in Sweden and in London: patriotism and national categories recur in Ferrner's descriptions of scientific conversations. His journal presented him as a man who used seemingly contradictory resources and identities, not unlike his hosts, the traders acting as middlemen in the Anglo-Swedish metal trade. National scientific networks were central to the funding, legitimization and knowledge making of sciences – such as astronomy and mathematics – which were tied to state and military interests. Furthermore, national belonging worked as an identifying marker, which facilitated transnational collaboration. Ferrner was greeted as a representative of his Swedish network, and it was because of his place in this network, based on Swedish patriotism, that London's communities of naval science research opened up to him.

As Daston points out, the Republic of Letters of the late eighteenth century embraced a cosmopolitanism that yearned for distance.[70] However, when Ferrner lived as a stranger in the midst of London science, he did not live in this cosmopolitan republic. Consequently, Ferrner's journal describes norms and values that are rarely found in the transnational correspondence of astronomers and mathematicians. Nonetheless, while Ferrner's interactions in London cannot be categorized as purely 'cosmopolitan', he did not present a European science divided along clear-cut national boundaries either. In his journal, patriotic and cosmopolitan ideals coexisted. His everyday interactions with London men of science were not arms-length relationships. Instead, through his encounters Ferrner wove a patchwork of networks that followed and crossed national boundaries. These networks contained both patriotic and cosmopolitan ideals, and were connected to commercial ventures as well as state projects.

Notes

1. Bengt Ferrner, *Resa i Europa. En astronom, industrispion och teaterhabitué genom Danmark, Tyskland, Holland, England, Frankrike och Italien 1758–1762* (Uppsala: Almquist & Wiksell, 1956), p. 133. This initially negative view can be contrasted with Ferrner's praise of Portsmouth's orderly streets; see below and Ferrner, *Resa i Europa*, p. 207ff. Göran Rydén, 'Viewing and Walking. Swedish Visitors to Eighteenth-Century London', *Journal of Urban History*, 39

(2013), 255–74, identifies a similar tendency in a number of eighteenth-century Swedish travel reports in London, which aim to establish a systematic 'general picture' of the city.

2. On Ferrner's interest in music, see Thomas Schönberg, 'Bengt Ferrner's Musical Tour in Europe. The Musical Scene of Mid 18th Century through the Eyes of a Swedish Music-Loving Astronomer' (Thesis (DMA), University of Hartford, 1993); Lars Berglund, 'Travelling and the Formation of Taste. The European Journey of Bengt Ferrner and Jean Lefebure 1758–1763', in *Sweden in the Eighteenth-Century World. Provincial Cosmopolitans*, ed. by Göran Rydén (Farnham: Ashgate, 2013), pp. 95–122.
3. Ferrner, *Resa i Europa*, pp. 154, 220, 234, 328. On electrical performance and dramatic effects, see Simon Schaffer, 'Natural Philosophy and Public Spectacle in the Eighteenth Century', *History of Science*, 21 (1983), 1–43 (p. 9). Ferrner saw the crocodile after a meeting at the Royal Society. See also P. Fontes da Costa, 'The Culture of Curiosity at The Royal Society in the First Half of the Eighteenth Century', *Notes and Records of the Royal Society*, 56 (2002), 147–66.
4. John Brewer, *The Sinews of Power. War, Money and the English State, 1688–1783* (London: Unwin Hyman, 1989), p. 175; Matt Schumann, 'International Rivalry and State Identity in the Seven Years War', in *Statehood Before and Beyond Ethnicity. Minor States in Northern and Eastern Europe, 1600–2000*, ed. by Linas Eriksonas and Leos Müller (Brussels: P.I.E.-Peter Lang, 2005), pp. 159–78 (p. 165).
5. Richard Dunn and Rebekah Higgitt, *Finding Longitude: How Ships, Clocks and Stars Helped Solve the Longitude Problem* (Glasgow: Collins, 2014), pp. 39–44.
6. Ibid., pp. 44–73.
7. Axel Gabriel Silverstolpe, *Tal öfver kansli-rådet Bengt Ferrner* (Stockholm: Carl Delén, 1802), p. 18. All translations from Swedish sources are my own, unless otherwise noted.
8. On Swedish and European astronomy and 'state utility', see Sven Widmalm, *Mellan kartan och verkligheten. Geodesi och kartläggning, 1695–1860* (Uppsala: Institutionen för idé- och lärdomshistoria, 1990), pp. 116–17. For an overview of Swedish political developments in this period, see Michael Roberts, *The Age of Liberty. Sweden 1719–1772* (Cambridge: Cambridge University Press, 1986), p. 17ff. For a discussion of the relationships between sciences, economy and politics, see Sven Widmalm, 'Instituting Science in Sweden', in *The Scientific Revolution in National Context*, ed. by Roy Porter and Mikuláš Teich (Cambridge: Cambridge University Press, 1992), pp. 240–62 (p. 248ff); Lisbet Koerner, 'Daedalus Hyperboreus. Baltic Natural History and Mineralogy in the Enlightenment', in *The Sciences in Enlightened Europe*, ed. by William Clark, Jan Golinski and Simon Schaffer (Chicago: University of Chicago Press, 1999), pp. 389–422 (p. 397); Lisbet Koerner, *Linnaeus. Nature and Nation* (Cambridge, MA: Harvard University Press, 1999), pp. 1–6.
9. [Per Elvius], *De Longitudine Geographica Dissertatio [...] pro Gradu [...] Andreas Duraeus* (Uppsala: Literis Wernerianis, 1710). On Swedenborg's visit to London and interest in maritime longitude, see Simon Schaffer, 'Swedenborg's Lunars', *Annals of Science*, 71 (2014), 2–26 (p. 8).
10. Daniel G. Harris, *F. H. Chapman. The First Naval Architect and His Work* (London: Conway Maritime, 1989), p. 25.

11. Sten G. Lindberg, 'Bengt Ferrner', *Svenskt biografiskt lexicon* (Stockholm, 1956), pp. 635–44; Wilhelm Sjöstrand, *Grunddragen av den militära undervisningens uppkomst- och utvecklingshistoria i Sverige till år 1792* (Uppsala: Lundequistska, 1941), p. 354ff. Swedish attempts to train seafarers scientifically mirror contemporary European discussions. See Guy Boistel, 'Training Seafarers in Astronomy. Methods, Naval Schools and Naval Observatories in 18th- and 19th-Century France', in *The Heavens on Earth. Observatories and Astronomy in Nineteenth-century Science and Culture*, ed. by David Aubin, Charlotte Bigg and Heinz Otto Sibum (Durham, NC: Duke University Press, 2010), pp. 148–73 (p. 159); W. F. Sedgwick, 'Robertson, John (1707–1776)', *Oxford Dictionary of National Biography* (Oxford: Oxford University Press, 2008) <http://www.oxforddnb.com/view/article/23802> [accessed 6 October 2014]).
12. Hjalmar Fors, 'Matematiker mot linneaner. Konkurrerande vetenskapliga nätverk kring Torbern Bergman', in *Vetenskapens sociala strukturer. Sju historiska fallstudier om konflikt, samverkan och makt*, ed. by Sven Widmalm (Lund: Nordic Academic Press, 2008), pp. 25–53.
13. Lindberg, 'Bengt Ferrner'. These meteorological observations were published in articles in the proceedings of the Swedish Royal Academy of Sciences between 1752 and 1758. For a discussion of how the position of practical astronomer in Uppsala was linked to a patriotic framework of science, see Sven Widmalm, 'Auroral Research and the Character of Astronomy in Enlightenment Sweden', *Acta Borealia*, 29 (2012), 137–56.
14. Lindberg, 'Bengt Ferrner'.
15. Bengt Ferrner, *Inträdes-tal, om sjö-magt, hållit för Kongl. Vetensk. akademien den 28. augusti 1756* (Stockholm: Lars Salvius, 1756), p. 4.
16. Ferrner, *Inträdes-Tal*, p. 25ff.
17. Mårten Strömer, *Tal om förbindelsen imellan astronomien och styrmans-konsten* (Stockholm: Lars Salvius, 1756).
18. Woolrich discusses this issue in the introduction to his English translation of part of the journal, which does not include Ferrner's stay in London; see Bengt Ferrner, *Ferrner's Journal 1759/1760. An Industrial Spy in Bath and Bristol* (Eindhoven: Archæologische Pers, 1987), p. 2ff.
19. Ferrner, *Resa i Europa*, pp. 131–336. The translation of original Swedish sources is my own. I have changed Ferrner's inconsistent spelling of English names into their common form.
20. Hanna Hodacs and Kenneth Nyberg, *Naturalhistoria på resande fot. Om att forska, undervisa och göra karriär i 1700-talets Sverige* (Lund: Nordic Academic Press, 2007); Hanna Hodacs, 'Linnaeans Outdoor. The Transformative Role of Studying Nature "on the Move" and Outside', *British Journal for the History of Science*, 44 (2011), 1–27.
21. These reports used in the education of young officials of the Swedish bureaus can be found in the Swedish National Archive.
22. See Michael W. Flinn, 'The Travel Diaries of Swedish Engineers of the Eighteenth Century as Sources of Technological History', *Transactions of the Newcomen Society*, 31 (1957), 95–109; A. P. Woolrich, *Mechanical Arts & Merchandise. Industrial Espionage and Travellers' Accounts as a Source for Technology Historians* (Eindhoven: Archaeologische Pers, 1989).
23. Ferrner never took up this position, becoming tutor to the crown prince Gustav after returning to Sweden; Lindberg, 'Bengt Ferrner', p. 642.

24. Sven Evander, *Svenska kyrkan i London 1710–2000. En historia i ord och bilder* (London: Svenska kyrkan, 2001); Derek B. Morris and Ken Cozens, *Wapping 1600–1800. A Social History of an Early Modern London Maritime Suburb* (London: East London History Society, 2009).
25. Jacob Serenius to the clerial estate of the Swedish parliament, 1739, Royal Library, Stockholm, A 1019:50.
26. Thompson Cooper, 'Brander, Gustavus (1719/20–1787)', *Oxford Dictionary of National Biography* (Oxford: Oxford University Press, 2008) <http://www.oxforddnb.com/view/article/3259> [accessed 3 October 2012].
27. Pehr Kalm, *Kalm's Account of His Visit to England on His Way to America in 1748*, trans. by Joseph Lucas (London: Macmillan & Co., 1892), p. 6.
28. Ferrner, *Resa i Europa*, pp. 134–44.
29. Ibid. p. 140ff.
30. Jim Bennett, 'Shopping for Instruments in Paris and London', in *Merchants and Marvels. Commerce, Science, and Art in Early Modern Europe*, ed. by Pamela H. Smith and Paula Findlen (New York and London: Routledge, 2002), pp. 370–95.
31. Ferrner, *Resa i Europa*, p. 216.
32. See Abraham Spalding to Pehr Wargentin, 24 October 1752, Wargentin Collection of Letters, Archive of the Swedish Royal Academy of Sciences (hereafter Wargentin Collection); Spalding to Wargentin, undated, Wargentin Collection; Spalding to Wargentin, 29 March 1768, Wargentin Collection.
33. Ferrner, *Resa i Europa*, p. 147.
34. Ibid. pp. 142–44.
35. Ibid. pp. 145,148; Ferrner to Samuel Klingenstierna, 22 April 1760, Uppsala University Library, A 9s. See also Klingenstierna's letters in A 9m. For an account of Ferrner's interactions with Dollond, see N. V. E. Nordenmark and Johan Nordström, 'Om uppfinningen av den akromatiska och aplanatiska linsen', *Lychnos*, 4 (1938), 1–52, and 5 (1939), 313–84. On the controversy between Dollond and Klingenstierna, see also Brian Gee, *Francis Watkins and the Dollond Telescope Patent Controversy* (Farnham: Ashgate, 2014), pp. 117–18, 135–40.
36. Ferrner, *Resa i Europa*, p. 145.
37. Christopher Irwin, *A Summary of the Principles and Scope of a Method, Humbly Proposed, for Finding the Longitude at Sea* (London: Printed for R. and J. Dodsley, 1760), p. 6.
38. Ferrner, *Resa i Europa*, p. 145. Ferrner's opinion of Sisson's instrument is consistent with Strömer's views of this method in his speech at the Academy three years earlier. Maskelyne later dismissed Irwin's chair for similar reasons; see Albert Van Helden, 'Longitude and the Satellites of Jupiter', in *The Quest for Longitude*, ed. by William J. H. Andrewes (Cambridge, MA: Collection of Historical Scientific Instruments, Harvard University, 1996), pp. 86–100.
39. *Förteckning öfver framl. herr cantzlie-rådet och riddaren Bengt Ferrners boksamling* (Stockholm, 1803); Irwin, *A Summary*, p. 6.
40. Ferrner, *Resa i Europa*, p. 330. On 18 August 1760, Ferrner returned to Harrison's to see his clock. This time he was joined by, among others, the natural historian Daniel Solander, who had just arrived in London; Ferrner, *Resa i Europa*, p. 333.

41. In 1757, for instance, Benjamin Franklin paid Harrison 10s. 6d. to see his sea clock; William J. H. Andrewes, 'Even Newton Could Be Wrong', in *The Quest for Longitude*, ed. by Andrewes, pp. 189–234 (pp. 207–8).
42. Ferrner, *Resa i Europa*, p. 288.
43. See for example Peter Wargentin, 'An Account of the Observations Made on the Same Transit in Sweden: In a Letter from Mr. Peter Wargentin, Secretary to the Royal Academy of Sciences in Sweden, and F. R. S. to Mr. John Ellicot, F. R. S. Translated from the French', *Philosophical Transactions*, 52 (1761–62), 213–16.
44. Ferrner's correspondence with Wargentin (70 letters between 1749 and 1781) is in the Wargentin Collection.
45. The other fellows were: 'Mr Akensid, Munckley, Hebbedine, Hadley, Knight, Hyde, Watson, Burrow [and] Colebrooke', Ferrner, *Resa i Europa*, p. 145.
46. Ferrner noted 14 dinners at the tavern (Ferrner, *Resa i Europa*, pp. 145, 150, 155ff, 160, 163, 164ff, 216–17, 219, 232, 326–28, 333.)
47. Ferrner, *Resa i Europa*, p. 155ff.
48. Ibid. p. 161. For the tavern's location, see Henry A. Harben, 'George and Vulture Tavern', *A Dictionary of London* (London: H. Jenkins, 1918).
49. Benjamin Franklin to John Ellicott, 13 April 1763, Benjamin Franklin Papers <http://franklinpapers.org/franklin/framedVolumes.jsp?vol=10&page=248a> [accessed 8 October 2014].
50. Ferrner, *Resa i Europa*, p. 161.
51. Benjamin Franklin to Mary Stevenson, 11 June 1760, Benjamin Franklin Papers <http://franklinpapers.org/franklin/framedVolumes.jsp?vol=9&page=119a> [accessed 8 October 2014].
52. Ferrner, *Resa i Europa*, pp. 219ff, 333.
53. Ibid. pp. 233–34.
54. Niccolò Guicciardini, 'Simpson, Thomas (1710–1761)', *Oxford Dictionary of National Biography* (Oxford: Oxford University Press, 2004) <http://www.oxforddnb.com/view/article/25594> [accessed 10 October 2014].
55. Ferrner, *Resa i Europa*, p. 150. Ferrner met John Ellicott about three weeks earlier on 2 October at the Mitre Tavern; Ferrner, *Resa i Europa*, p. 145.
56. Compare McConnell's discussion of how buyers of observatory apparatus 'referred to an existing piece, which [they] knew and admired'; Anita McConnell, 'From Craft Workshop to Big Business. The London Scientific Instrument Trade's Response to Increasing Demand, 1750–1820', *The London Journal*, 19 (1994), 36–53 (p. 49).
57. Ferrner, *Resa i Europa*, pp. 150–51.
58. Ibid. pp. 150ff, 233. Ferrner stayed in Greenwich until late in the evening, but he could not make any satisfactory observations.
59. The Plymouth merchant John Mignan was as French as Gustavus Brander was Swedish. Mignan was the son of George Mignan who was living in Plymouth by 1727; 'Will of George Mignan, Merchant of Plymouth, Devon', 1727, The National Archives, Kew, Prob 11/615/110; 'Will of John Mignan, Merchant of Plymouth, Devon', 1754, The National Archives, Kew, Prob 11/808/53.
60. Ferrner, *Resa i Europa*, pp. 198, 200. This agrees with the analysis of the eighteenth-century trade in Baltic iron in Göran Rydén, Chris Evans and Owen Jackson, 'Baltic Iron and the British Iron Industry in the Eighteenth Century', *Economic History Review*, 55 (2002), 645–65 (p. 650).

61. Ibid. pp. 200–01.
62. Ibid. p. 207ff.
63. Sedgwick, 'Robertson'.
64. Bennett, 'Shopping for instruments', pp. 76–77.
65. Brewer, *Sinews of Power*, pp. 221–30.
66. Boistel, 'Training Seafarers', p. 149.
67. Ferrner, *Resa i Europa*, p. 209.
68. Ibid. pp. 208–13. The education at Karlskrona came to resemble that at Portsmouth; see Sjöstrand, *Grunddragen*, pp. 354–61.
69. Lorraine Daston, 'Nationalism and Scientific Neutrality under Napoleon', in *Solomon's House Revisited: The Organization and Institutionalization of Science*, ed. by Tore Frängsmyr (Canton, MA: Science History Publications, 1990), pp. 95–115. For an overview of the following discussion, see the review article Geert J. Somsen, 'A History of Universalism. Conceptions of the Internationality of Science from the Enlightenment to the Cold War', *Minerva*, 46 (2008), 361–79.
70. Lorraine Daston, 'The Ideal and Reality of the Republic of Letters in the Enlightenment', *Science in Context*, 4 (1991), 367–86 (p. 383).

7

‘Perfectly Correct’: Russian Navigators and the Royal Navy

Simon Werrett

The era of the Board of Longitude’s existence, between 1714 and 1828, was also a remarkable period in the history of Russia’s navy. At the beginning of the eighteenth century, Tsar Peter I set about reforming the Russian military following disastrous campaigns in the Great Northern War with Sweden, and created a substantial Baltic fleet centred on the new capital, St Petersburg.[1] To provide expertise for training sailors on Russian ships, Peter turned west, and in particular to Britain. These efforts inaugurated a steady traffic of experts and students between Britain and Russia in the eighteenth and early nineteenth centuries that helped transform Russian navigation practices into a form resembling, and sometimes advancing on, those of Britain. This essay explores the British role in developing Russian navigation and makes three arguments. First, while the Russians evidently relied greatly on British expertise during this period, the traffic was not one way. Russian institutions provided theoretical expertise, practical experimental resources, and generous patronage that played a role in shaping British solutions to navigational problems including finding longitude at sea. Russians were not passive recipients of British expertise, and some techniques, at least, emerged from transnational co-operation and the circulation of knowledge.[2]

Second, an examination of the techniques used to navigate on Russian ships makes clear that officers did not rely on any single method, such as an accurate chronometer, but used several different approaches, choosing the one most appropriate to a given situation. This diversity and opportunism supports criticisms of a historiography of longitude that has presented John Harrison’s marine timekeepers as ‘the’ solution to the longitude in the eighteenth century.[3] Third, as Russian officers chose between many methods of navigation, so this process entailed complex relationships of trust in different instruments and personnel.

Navigation was not just a technical procedure but also an emotional, often unpredictable negotiation and judgments of trust were critical in making decisions. British personnel, instruments and techniques helped Russians make such adjudications and, while Britishness was no guarantee of navigational reliability, it was often taken into consideration in navigating decisions.

The rise of Russian navigation, 1700–60

In the seventeenth century, Muscovite Russia was more preoccupied with land than sea.[4] The Tsars pursued a land-based empire, annexing new territories and opening them to settlers. Prosperity depended on serfdom, and keeping the serfs fixed to their estates. Until the end of the eighteenth century, most Russian exploration focused on the land, charting new territories in the south and east and the vast regions of Siberia and Kamchatka. Mathematical and astronomical navigation (*korablevozhdenie*, *navigatsiia*, *moreplavanie*) at sea thus appear to have been virtually unknown in Russia before the end of the seventeenth century, though some compasses were manufactured in Kholmogory and may have been used in coastal areas.[5] A text known as 'The Starry Sky of the Archangel Sailors', surviving in six copies and dating from the seventeenth century, described the southing of stars, the compass rose, the means of finding true north, and the use of dividers. It also included the first known Russian star map. But since it contained errors, Ryan has proposed that it is unlikely to have been used at sea.[6]

This situation changed in the reign of Tsar Peter I (*c.*1698–1725), who encouraged navigational education in Russia as part of an effort to build up a new imperial navy with newfound access to the Baltic via St Petersburg, the new port capital founded in 1703. Peter was personally interested in western navigation and studied with the Dutch master Jan Albertusz van Dam during a visit to the Dutch Republic in 1697.[7] He trusted foreigners to improve navigation in Russia and often positioned navigation at the forefront of broader educational reforms. Russian students were sent abroad to Venice and Dalmatia in the 1690s to learn navigation.[8] In 1701, Peter opened a School of Navigation in Moscow, while Russia's first book on navigation, published the same year, derived from the sea-manuals of the Dutch writer Abraham de Graaf.[9] Despite this Dutch connection, Peter chose to hire Scots and English to run the new Moscow school, reflecting both the high reputation of British navigation and a tradition of hiring Scots to serve in the Russian court.[10] Heading the new school was the mathematician and astronomer Henry Farquharson of Marischal College, Aberdeen, together with two alumni of the Royal Mathematical School at Christ's Hospital – Stephen Gwyn and Richard Price. Ryan has traced the history of the Moscow School of Navigation and notes that it was one of the first institutions to teach geometry, trigonometry and astronomy in Russia, to some 200 students aged between 12 and 17.[11] An extant manuscript, probably dating to 1703 and perhaps authored by Farquharson, indicates that students learned geometry, course plotting and dead reckoning through worked examples, and found latitude by means of observations of the height of the Sun using a Davis quadrant (backstaff), methods typical of late seventeenth-century English practice.[12] Ryan suggests the likely English provenance of this text, which took London as the prime meridian and included measures in English feet.[13]

In 1715 the School transferred to St Petersburg and was renamed the Naval Academy. Farquharson taught there until his death in 1739. The Naval Academy had its own press, and helped to introduce basic western ideas of spatial literacy and navigation to Russia. Under Farquharson's direction, members of the Naval Academy published handbooks on mathematics and navigation in the 1730s, including the first navigation book published by a Russian author, the Baltic fleet officer Stepan Gavrilovich Malygin.[14] In 1752, the Naval Academy was reformed, becoming the Naval Cadet Corps.[15] Trust in British expertise endured. After Farquharson's death the Russian government was keen to find a British replacement, and candidates included Matthew Mitchell, captain of the *Pearl* on George Anson's expedition of 1740–44, and the astronomer Thomas Wright of Durham. These men proved too expensive to hire but the royal naval schoolmaster of the *Penzance*, Thomas Newberry, was appointed professor of mathematics and navigation at the Naval Cadet Corps in 1757 and remained there five years.[16] After his departure, the Cadet Corps continued as a centre for British influence, publishing the first English grammars for Russians and the first Anglo–Russian dictionaries.[17]

Just upriver from the Naval Cadet Corps on Vasilevskii Island in St Petersburg was the Imperial Academy of Sciences, another institution created by Peter I to enhance education in Russia.[18] Again, Peter and his assistants relied on imported expertise to staff the new Academy, which included among its members prominent foreign savants such as the French astronomer and geographer Joseph-Nicolas Delisle and the Swiss mathematician Leonhard Euler. From its opening in 1725 the Academy devoted much attention to geography, exploration and navigation. The very first public assembly held there in 1727 consisted of a lecture on the problem of discovering longitude, discussed by the mathematicians Georg Bernhard Bilfinger and Jacob Hermann.[19] Bilfinger explained to an audience of nobles and officials who were unfamiliar with the sciences that mathematics was 'excellent for Navigation' and went on to explain the difficulties of using eclipse observations and timekeepers to find longitude at sea. Bilfinger looked to the work of British astronomer Edmond Halley for an alternative and suggested that Halley's world chart based on multiple measures of magnetic declination (also known as magnetic variation) might offer a future solution. He suggested that careful and exact measures in Russia needed to be made: 'We may know in some years whether or not we can count on these measures, or whether they have to be abandoned'.[20]

As Raspopov and Meshcheryakov have shown, both before and after Bilfinger's speech numerous measurements of magnetic variation were made across the Russian empire. Peter I decreed that Russian vessels must measure declination off the Russian coasts and the academic adjunct Friedrich Christoph Mayer made measurements on the site of the Academy in 1726. From the 1690s, Russian nautical charts included magnetic declination and, from 1714, Peter inaugurated the translation and publication of numerous atlases of the Baltic Sea showing declination.[21] In the Russian Naval Regulations of 1720, Peter also ordered all new ships to be fitted with compasses, and in the following year established a compass manufactory in St Petersburg, overseen by the Admiralty Board.[22] Peter also sent Vitus Bering on an expedition to Kamchatka from 1725 to 1728, while the Academy organized a second Kamchatka expedition, again under Bering, in 1733 to 1743. Both voyages took many variation measurements.[23]

Ultimately, magnetic variation would not turn out to be a definitive longitude solution, but the Russians invested in other avenues of research. In 1732, perhaps in answer to Bilfinger's call, one P. I. Roquette, watchmaker to the Empress Anna Ivanovna, sent a longitude solution to the Royal Society, where it was translated and discussed by the mathematician and astronomer James Hodgson.[24] The proposal hinged on various cosmological assumptions and the idea that longitude might be found using a combination of portable clocks and tables of the variations they underwent in different seasons owing to changes in air pressure, which Roquette claimed to have discovered. Hodgson, who digested the method for the Royal Society, was unimpressed,

> What answer must be given to a Man who is so very ignorant of the first principles of Astronomy and Philosophy, who has asserted so many falsehoods and calls them Demonstrations, and is so vastly fond of his Performance I leave you, Gentlemen, to determine.[25]

A much more successful contributor to solving the longitude was the academy's mathematician Leonhard Euler. Euler had considered a naval career as a young man and his interests in navigation were significant. He firmly believed that mathematics would improve the art. In an essay on the utility of higher mathematics, he wrote,

> no one, I imagine, would dare to question the utility of higher mathematics [for navigation]. If we consider the journey of a boat on the ocean, we will think first of the loxodromic curve, the invention of which assuredly may not be attributed to elementary mathematics. This curve is used to solve most of the problems that present themselves to anyone who wants to study the art of setting the course of a ship. The complete theory of navigation [...] is so arduous, demanding such a deep knowledge of hydrostatics and mechanics that the help of higher analysis is of prime necessity.[26]

Euler reckoned mathematics was also essential to understand the ideal shapes for ships' hulls, the effect of cargo on a ship's equilibrium, and the art of arranging sails and steering in a contrary wind. He promoted the work of Johann Bernoulli on these questions and addressed some of them himself in *Scientia navalis*, completed in St Petersburg in 1738 and published in 1749. The treatise laid out the principles of hydrostatics and a scientific theory of shipbuilding that proved influential.[27] In England, the book was published in translation at the instigation of East India Company engineer Henry Watson in 1776, and Euler's ideas informed experiments to study ideal hull shapes made in Britain by Mark Beaufoy in the 1790s for the Society for the Improvement of Naval Architecture.[28]

Euler's first book of lunar theory, published in 1753, addressed the three-body problem and was important for navigation.[29] In 1755, at Euler's request, Tobias Mayer sent a set of lunar tables worked out using Euler's theory from Göttingen to the Admiralty, which referred them to the Board of Longitude in London as a submission for a reward.[30] In February 1765, after Mayer's death, the Board awarded £3000 to his widow for this contribution, with £300 awarded to Euler on the grounds that his calculations had been the basis of Mayer's tables.[31] Having tested Mayer's tables on a voyage to St Helena in 1761, Nevil Maskelyne published his *British Mariner's Guide* and, as Astronomer Royal, the first *Nautical Almanac*.[32] Euler participated in the election of Maskelyne to the St Petersburg Academy of Sciences in 1776.[33]

A member of the Russian Academy thus played a significant role in shaping British navigational practice. Euler also helped to spread news of the longitude reward and its applicants to Russian and German readers via his *Letters to a German Princess*, published in 1768 and addressed to a lay audience unfamiliar with the sciences. The book, consisting of a series of letters sent to the 15-year-old Sophie Charlotte of Brandenburg-Schwedt, included a long discussion of navigation techniques.[34] After describing dead reckoning, Euler explained how a timekeeper could be used to find longitude. Although he approved the method, Euler regretted that a clock of sufficient accuracy would never be created, since even John Harrison's experiments had failed:

> About ten years ago [...] an English artist pretended that he had constructed a timepiece proof against the motion of a ship at sea [...] on which the inventor claimed and received part of the parliamentary reward proposed for the discovery of the longitude [...] But since that time we have heard no more of it; from which it is to be assumed that this attempt has failed, like many others which had the same object in view.[35]

Euler went on to advocate, not surprisingly, astronomical methods of longitude determination, and measures based on the Moon's motions, via Mayer's tables in particular. He explained how the 'English nation, generously disposed to engage genius and ability' offered 'three prizes, for ascertaining the longitude' and made clear his view that 'Mr. Mayer is at this moment claiming the highest, and I think he is entitled to it'.[36] Discussions of navigation were thus not restricted to technical literature in eighteenth-century Russia, and protagonists in the search for longitude helped convey news of the British competition to new audiences. Euler also had a significant impact on British navigating techniques, reminding us that navigational knowledge did not travel in only one direction between Britain and Russia in the eighteenth century. Other Russian academicians sought to contribute to the longitude. The Academy's professor of chemistry Mikhail Vasil'evich Lomonosov wrote a dissertation on navigation in May 1759. Lomonosov proposed a form of marine chair for keeping a telescopic observer steady on board a ship, perhaps inspired by the marine chair of Christopher Irwin, patented in March 1759.[37] Although the consequences of Lomonosov's plans are unknown, another St Petersburg academician, Wolfgang Ludwig Krafft, devised new procedures for reducing lunar distances, which he sent to Maskelyne in 1794.[38]

Britain and the reform of the Russian Navy, 1760–1800

In 1770, the Empress Catherine II lamented that ‘up to the year 1762 the navy has fallen little by little into annihilation’.[39] Ships were poorly constructed and badly supplied with artillery; and the organization of shipyards and naval administration needed reform. Catherine was determined to improve matters, leading to a renewed exchange between Britain and Russia’s navies. While the Russian government preferred German and French academics, when it came to navigation they continued to employ Scottish and English experts to bring about improvements. Beginning in 1768, the Admiralty ordered ships’ cannon and a steam-pump from the Carron Company of Falkirk.[40] Several Scots officers were taken into Russian service in 1764, including Lieutenant, later Admiral Samuel Greig, who distinguished himself in action against the Turks.[41] English officers were also hired. From 1770 to 1774, Charles Knowles served as an admiral of the Russian fleet, while Samuel Bentham, subsequently Inspector General of the Naval Works at Portsmouth, built ships for the Black Sea fleet for Empress Catherine’s favourite Prince Grigorii Potemkin between 1780 and 1791.[42] Captain James Cook’s former midshipman James Trevenan also joined the Russian Navy, serving from 1787 until his death in action in 1790.

Knowles set about transforming the Russian Navy along English lines, overseeing a series of reforms based on comparing the state of Russian ships and naval administration to English practice. Knowles included concerns about navigation in his recommendations to Catherine II:

> The vessels of Your Imperial Majesty are also extremely lacking in disciplined subaltern officers and a number of good Boatswain’s mates to arrange and dispose the sailors to their respective duties, as much in the navigation as in the manoeuvring of vessels, in which I am well informed they are very defective, particularly in darkness and in bad weather.[43]

Knowles had experience with navigational improvement. He had recommended the Scots navigator and later natural philosopher John Robison to the Board of Longitude as the keeper of John Harrison’s timekeeper on its trial to the West Indies in 1762. When Knowles went to Russia in 1770, he engaged Robison as his private secretary, and Robison was subsequently appointed inspector general of the marine cadets at the Russian naval base in Kronstadt with the rank of lieutenant colonel, where he remained until 1774.[44] Despite his concerns over navigation, Knowles's main preoccupation was improving shipbuilding in Russia and most of his advice concerned timber and hemp supply and the organization of shipyards and shipbuilding.

Russia's circumnavigations, 1800–30

Knowles, Greig and Bentham were engaged primarily to provide immediate leadership and assistance in wartime, helping to build up the Russian fleet to fight the Turks and the Swedes.[45] They were not explicitly hired to train Russians in British navigation methods, and their focus was on improving shipbuilding and construction, as had been Euler's in *Scientia navalis*. But subsequent Russian efforts did focus on training students in British naval and navigation techniques. In 1785, no doubt inspired by Cook's circumnavigations, the Russian government hired Joseph Billings, able seaman and astronomer's assistant to William Bayly on Cook's third voyage, to lead an overland expedition to Kamchatka and the Aleutian islands to investigate the fur trade. Billings was to train students from one of a number of regional navigation schools which had opened across the empire by that time. His instructions included the order to take 'five or six of the best scholars of the Navigation School' at Irkutsk, and 'to employ them [...] in surveying and drawing charts'.[46]

Russian interest in the fur trade led to further naval developments in the early nineteenth century. Between 1803 and 1850, 36 expeditions set out from St Petersburg to sail around the world to provision the Russian American Company's fur-trading posts in Alaska and open trade to China and Japan. Baltic German and Russian naval officers headed these circumnavigations, including Adam Johann von Krusenstern and Yuri Lisianskii on *Nadezhda* and *Neva* (1803–07), Vasilii Mikhailovich Golovnin on *Diana* (1807–09), Otto von Kotzebue on *Rurik* (1815–18) and Feodor Petrovich Litke on *Seniavin* and *Moller* (1826–29). As Vinkovetsky has shown, the voyages marked a shift in Russian imperial policy, giving a new role to the navy and maritime colonies in place of a traditional emphasis on land-based territorial acquisition.[47] The British overseas empire was part of the inspiration for this change, and the Russian circumnavigations marked a highpoint in British interaction with Russian navigation.

The Academy of Sciences supported these voyages. From 1787, academic astronomer Petr Borisovich Inokhodtsev lectured to naval officers on navigational science. Inokhodtsev had continued the Academy's programme of measures of magnetic variation in the 1770s, noting that Kursk's measures were anomalous, explained by very large deposits of iron ore in the region.[48] By 1803 another academic astronomer, Friedrich Theodor Schubert, began training officers and published a textbook on the determination of latitude and longitude by astronomical methods.[49] He also developed new instruments for the circumnavigators including a sextant, pocket chronometer and achromatic telescope. From 1813, he published the *Morskoi mesiatseslov* (Maritime Calendar), equivalent to the British *Nautical Almanac*. After the Krusenstern-Lisianskii voyage, another academician, Platon Iakovlevich Gamaleia, published *Teoriia i praktika korablevozhdeniia* (The Theory and Practice of Navigation) (St Petersburg, 1806–08), which used Krusenstern's experiences to formulate navigating methods for subsequent voyages.[50]

The Academy took the problem of longitude seriously in these years and may have served as an alternative source of support for longitude schemes to the Board of Longitude. In 1803, for instance, the Pennsylvania surveyor John Churchman visited the Academy. He planned to solve the longitude problem by magnetic variation, a method of longstanding interest to the Russians.[51] Churchman was elected to the St Petersburg Academy and proposed his method to the Russian Admiralty, although it is not clear what became of it. In English proposals for his method, Churchman used his status and connections in Russia to lend credit to his ideas.[52]

The main source of expertise sought by the Russians in navigation, however, continued to be Britain. British navigators and naval expertise continued to enjoy a high reputation, particularly after Cook's voyages became known in Russia.[53] In 1793, the Russian ambassador in London, Semen Romanovich Vorontsov, arranged for 14 young naval officers to travel to Britain to spend four years in the Royal Navy. Twelve more followed in 1797.[54] Half were dispatched to the Mediterranean while the other half served in the West Indies. When they returned to Russia in 1799 they were ardent anglophiles, prompting some to fear their loyalties might be divided:

> They spoke the language, and had a good deal the manners and appearance of British seamen [...] They spoke openly in favour of England, and refused to throw aside their blue jackets and trousers, notwithstanding the emperor had issued two orders to that effect.[55]

Many of the circumnavigators came from this contingent. The first was Adam Johann von Krusenstern, a Baltic German native of Estonia, who like many Baltic Germans under Russian rule served in the Russian Navy. In May 1794, Krusenstern sailed to America in the British ship

Thetis under the command of Captain Alexander Cochrane. Another Russian in the same squadron, sailing on *L'Oiseau*, was Iurii Lisianskii, a graduate of the Russian Naval Cadet Corps and a veteran of Russia's war with Sweden of 1788–90. Lisianskii fought with Rear-Admiral George Murray against American ships provisioning France, sailed to Halifax and the West Indies, then travelled across the United States, before joining Krusenstern on the *Reasonable*, captained by Charles Boyle and bound for the Cape of Good Hope. Krusenstern went on to India and China while Lisianskii travelled in South Africa.

This extensive experience and training led Tsar Alexander I to appoint Krusenstern and Lisianskii to lead the first Russian circumnavigation from 1803 to 1806, on the *Nadezhda* and *Neva*, both originally constructed in Britain. Other officers trained by the Royal Navy, or trained by officers who had trained in Britain, commanded subsequent circumnavigations. Vasilii Mikhailovich Golovnin, who served in the Royal Navy in the 1790s, took his ship *Diana* to North America in 1807–09, and then took the *Kamchatka* in 1817–19. Two of Krusenstern's officers, Otto von Kotzebue and Thaddeus von Bellingshausen, commanded voyages to the Pacific between 1815 and 1826. Golovnin's officers Ferdinand von Wrangell and Fedor Petrovich Litke sailed on *Krotkii* and *Seniavin* to North America in the late 1820s.

British navigational expertise was not exclusive to the Royal Navy, however. Another navigator on the Krusenstern voyage was a veteran of the East India Company.[56] As a youth, Hermann Ludwig von Löwenstern, another Baltic German, had tried to learn navigation with a Russian pilot. 'I went to a lot of trouble with it,' he wrote in his diary, 'but did not get very far'.[57] Instead, Löwenstern joined the East India Company and spent five months learning English and navigation. Although he quit because he found life on a Company ship unbearable (full of 'wrangling, strife, envy, hate, deceit, cheating, egoism, uncharitableness, lies, and laziness'), Löwenstern nevertheless retained a great admiration for all things British.[58] He always spoke of distances in 'English miles' and admired English instruments. On board Krusenstern's ship *Nadezhda* when it landed in England on its outbound voyage, Löwenstern was delighted when the captain purchased him a sextant and chronometer from Robert Pennington.[59]

The Russian officers' enthusiasm for Britain and the Royal Navy thus included an admiration of British navigational instruments. This was a common attitude among the Russians. Even before the first Russian circumnavigation began, Lisianskii travelled to London to purchase instruments and two ships, which became the *Nadezhda* and *Neva*. The instruments included a reflecting circle, 12 inches in diameter, a 10-inch sextant, a three-foot transit instrument by Troughton and an 18-inch diameter astronomical quadrant by Adams. Lisianskii also bought four Arnold and two Pennington chronometers. All of these were shipped to the St Petersburg Academy of Sciences, where the astronomer Schubert tested and prepared them for the voyage. In the end, three of the chronometers were taken – an Arnold box chronometer, which Krusenstern reckoned was the best of the three, an Arnold pocket chronometer, which stopped for a time during the voyage, and a Pennington pocket chronometer. Krusenstern also used Mayer's lunar tables, as revised by the astronomers Tobias Bürg and Charles Mason, and charts by the English cartographer Aaron Arrowsmith. When he left St Petersburg in *Nadezhda* in 1803, Krusenstern and his astronomer Johann Caspar Horner insisted on stopping in London to buy more instruments, which Löwenstern thought were 'very nice'.[60]

Such arrangements became formalized for subsequent voyages, with a letter being sent to the Russian ambassador in London (Count Christopher Lieven from 1812 to 1834), to order instruments ahead of a voyage before an inevitable stop during the voyage to buy more and meet English makers.[61] Typically, Russian ships were equipped with logs and sounding machines by Edward Massey, telescopes by Tully, Dollond or Troughton, and chronometers by Arnold and Barraud. Maps were by Aaron Arrowsmith and John Purdy.[62]

Britain's longstanding reputation for navigational and manufacturing expertise and Royal Navy training thus ensured Russians' continuing use of British skills and hardware into the nineteenth century. Again, though, it would be wrong to see this as one-way. Russian patronage and Russian voyages helped test and secure British innovations in navigation. Between 1819 and 1824, Peter Barlow, professor of mathematics at the Royal Military Academy, Woolwich, devised a method of correcting compass needles from local deviations due to the increasing quantities of iron used in ship construction. The technique, which involved placing a small disc of soft iron near the compass to offset deviations, received patronage from the Board of Longitude, who granted Barlow £500 to make experiments. Working with Barlow, the instrument-makers Gilbert devised a novel azimuth compass made from new brass, after it became clear that recast brass became magnetic.[63]

The Tsar of Russia, Alexander I, also supported Barlow, rewarding him with a gold watch and dress-chain, sent via the Russian ambassador Lieven, when the Russian Navy adopted his method.[64] Russian Imperial patronage could thus help establish new techniques in Britain and encourage their use in Russia. Krusenstern played an important role in this exchange, experimenting with Barlow's technique using the Gilbert compass at the Russian port of Kronstadt, and disseminating Barlow's work to Admiral Greig commanding the Russian marine station on the Black Sea. In 1824, Krusenstern's results were published in the *Philosophical Magazine* in Britain and no doubt his enthusiastic endorsement helped establish credit for Barlow's method.[65]

Two years later in 1826, another Russian circumnavigator, F. P. Litke, collaborated with Barlow under the guidance of John Barrow, Secretary of the Admiralty. Litke and Barlow, together with Captain William Parry and Edward Sabine, fitted Litke's ship *Seniavin* with an 'invariable clock', which they tested at the Royal Observatory in Greenwich after Litke had arrived in Portsmouth on the usual stopover to collect instruments.[66] Litke also used Sabine's invariable pendulum on the voyage to demonstrate that the flatness of the Earth was greater than had earlier been derived from lunar inequalities.[67] Developing instruments and techniques thus came to benefit from Anglo–Russian co-operation.

Meanwhile, British instruments played a salient role in Russian navigating techniques on the circumnavigations. Once Russian ships embarked from Portsmouth, bound for the Atlantic, they proceeded using a variety of navigating methods. Much of the time, officers were keenly aware of weather, winds, coastlines, landmarks, lighthouses, birds, reefs, swells and other features of which they could take advantage to navigate near land. Navigators were opportunistic about these methods, using them when an appropriate situation arose. 'At this time of year,' wrote Litke at Portsmouth in November 1826, 'a favorable wind seawards is such a precious thing that one has to take the utmost advantage when one does blow'.[68]

Out on the ocean, technical methods became more urgent, demanding systematic and disciplined operations. The instructions for Bellingshausen's voyage of 1819–21 were explicit about how the ships should navigate. Every 24 hours, dead reckoning and observed position had to be 'determined by bearing and by the distance from some known point, wherever possible one whose latitude and longitude have been accurately determined'. If there was a discrepancy between the dead reckoning and observed position, it needed to be investigated by using charts on which the reckoning was plotted, and by astronomical observations 'made as frequently as possible'. Latitude should be determined not only by observations of the altitude of the Sun at noon, but also the meridian altitude of twilight stars and ex-meridian altitudes of the Sun. 'For the longitude,' the instructions continued, 'lunar distances should be taken whenever circumstances permit, and the results of these observations should be compared with those given by chronometers'. Whenever the ships approached a point of known longitude, the chronometers should be re-rated. All observations were to be recorded in a log.[69] In sum, the normal way of navigating was by dead reckoning, checked against astronomical observations, which in turn were checked against the average reading of two or three chronometers, themselves periodically set right in places of known longitude. Bearings were taken from a compass and speed, as Löwenstern wrote, with a log and line, or 'leash that can become longer or shorter' and an 'hourglass' or 'sand clock'.[70]

In practice, these acts of navigation were never simple matters of procedure or reliance on instruments. Löwenstern noted:

> In Kronstadt in 1803, after experimenting, we found that the second glass took 14 seconds to run. Krusenstern ordered the distance between knots to be made 46 fuss [feet] long [...] On Russian ships of war, the second glass runs at 29 seconds, and the log line is set to 30 seconds; that is 50 fuss 11 zoll [inches] English from one another. That shows how imprecise the ship's calculations are.[71]

Navigating was a highly charged affair because mistakes could lead to catastrophe, and officers often recorded the fear, anxiety, danger and excitement that accompanied navigation. Litke learned lessons after he navigated a hazardous passage off the coast of Unalaska. He recalled,

> In the six years which have passed since then, I often go over the events of that day in my mind, and each time I reproach myself for exposing the ship and its crew [...] to such danger. Extremes are met in all conditions in human life. Often misplaced prudence will lead to an unwise decision but, on the contrary, sometimes one must needs be bold to be prudent.[72]

Navigating was also frequently out of the hands of officers, as strong winds, storms and periods of calm dictated their ability to move about. When navigators did have the opportunity to make observations, find positions or plot courses, these acts demanded negotiation. Instruments and individuals were often deemed inadequate to determine the right action to take. On his voyage of 1785, Joseph Billings had a timekeeper on board his ship but did not consider it reliable. Off the coast of Kamchatka, 'The ship's reckoning still differing so materially from that of the time-keeper, induced Captain Billings to reject this method of ascertaining the longitude'.[73]

British instruments proved troublesome despite their reputation, and the marriage of academic theory and navigational practice might not be successful. Löwenstern repeatedly lamented the inaccuracy of reckonings and chronometers on the Krusenstern-Lisianskii circumnavigation:

> Without observations we would be lost. Enough effort has been put into devising Logs [...] that are supposed to determine a ship's course, but a lot of what seems clear on paper is impossible in practice or at least very defective. None of them meet expectations, etc. Navigation owes its thanks to astronomy that it has reached its present perfection.[74]

Both Billings and Löwenstern noted the social component of these measures. Since measures were underdetermined, navigators had to assess each other's reliability to make navigating decisions. Löwenstern reckoned that the only trustworthy navigator on his ship was himself, writing in his diary that, 'It seems to me as if I were hired to ferret out the mistakes in the ship's reckonings'.[75] He lamented the excuses made by other officers for discrepancies between their reckonings and position, and how the excuse would be altered if it were found unconvincing: 'we find then immediately some other excuse. Then it must be the fault of the high seas, the waves, the drift'.[76] For Löwenstern, only astronomical observations provided closure for disputed measures. 'Seldom can we determine our position with certainty on the map without having made an observation'.[77]

Löwenstern placed his faith in himself and in astronomical technique over his fellows. Litke was equally reluctant to trust others. When he sailed from St Petersburg to Portsmouth in September 1826, he complained on reaching Elsinore that,

> Usually it is here that one takes aboard pilots for the North Sea, but very few of them really have the essential knowledge and experience to warrant their being any real help. On the contrary, it has happened more than once that because of the pigheadedness of these ships' pilots, navigators have found themselves in difficulties. That is why we find it more agreeable to proceed on our own.[78]

On reaching the southern English coast, however, Litke became more open to allowing pilots to take over. Choosing which pilot to trust depended on personal experience and familiarity.

> The pilots were not slow in coming aboard and, by a strange fluke, among them was a former acquaintance of mine who, nine years previously, had piloted the corvette *Kamchatka* through the Spithead roads. It was only natural to give him preference over the others.[79]

Record keeping was another essential part of navigation and here too British practice played a role. Lisianskii noted how he 'scrupulously attended' to a journal of his chronometer readings every day. Other navigators reported this work as 'tedious'.[80] Stories of British navigators circulated indicating the dangers of improper record keeping. Löwenstern reported the tale of William Robert Broughton, whose journals and charts were torn up by monkeys when his back was turned at Port Jackson in Australia in 1795. Broughton replaced the lost charts with inferior versions, leading him to a disastrous shipwreck off the coast of Japan.[81]

Russian solutions to problems of navigational trust thus depended on judgments of nationality, personality and technique. Clearly, Russians reckoned British instruments and expertise would make for more reliable navigation than those of other nations, reflecting a longstanding tradition of trust in British navigational expertise. This trust was perhaps most manifest in a widespread admiration for James Cook, whose status among Russian naval officers in the early nineteenth century was very high, thanks in part to their British training.[82] Cook's stellar reputation with the Russians had a notable influence on navigation. Generally, the circumnavigators followed Cook's routes from Europe to Cape Horn and the Pacific, and Russian voyages often visited places prominent on Cook's voyages, such as the place where he had been killed in Hawai'i. Travelling in the Pacific in the early 1820s, Otto von Kotzebue noted that his trip to Matavai Bay, Tahiti, was because of the celebrity bestowed upon it by Cook.[83] He made sure, he wrote, to set up an observatory at Cape Venus on 'precisely the same spot where Cook's Observatory had formerly been erected'.[84]

Perhaps more significantly, Cook's measurements were taken as the standard against which to calibrate Russian instruments and measures. The Russians often referred their measurements to Cook's. When Krusenstern sounded Avacha Bay on the coast of Kamchatka he found 'the depths marked in Captain Cook's plan of Awatscha Bay perfectly correct. Indeed the whole plan of it [...] is drawn with an accuracy that cannot be exceeded'.[85] Cook was the limit of perfection, and so served as a standard against which to make judgments. The journals of Bellingshausen's voyage of 1819–21 also recorded that they 'accepted the latitude [...] as fixed by Captain Cook as true, correcting our own reading'.[86] On another voyage to the Pacific of 1815–18, Otto von Kotzebue wrote 'My calculation of the longitude of the Pallisers, agreed with that of Cook, within three minutes. Between our latitude and Cook's there was no difference; I therefore had no reason to complain of my time-keepers'.[87]

Conclusion

British training might raise fears of unpatriotic Russians, and British instruments might prove unreliable, but Captain Cook was 'perfectly correct'. National reputations played a role in Russian navigating decisions. This reflected an enduring interaction between the British and Russians, which evidently benefitted both sides. The Russians admired British navigators and relied on British expertise in the construction of the navy from the reign of Peter I onwards. Russians learned a diverse array of navigating methods from the British, which they put to work on circumnavigations in the early nineteenth century. No one method, such as the use of an accurate chronometer, predominated, a situation common across European navies, as other contributions to this volume show. Britishness also figured in the routines of navigating on Russian ships, part of a complex process of adjudicating between methods, instruments, measurements and personnel in the effort to navigate successfully. Cook's reputation helped make some of these decisions easy, and his measurements even served to calibrate instruments. Britishness was no guarantee for navigational reliability, however, and it was just one element in a series of judgments.

New ideas, methods, instruments and personnel also flowed from Russia to Britain in this period. The Academy of Sciences, the Russian court, and the Imperial Navy all provided theoretical and practical resources and patronage that helped transform and promote British innovations in navigation. The British, for their part, came to be much impressed by Russian navigators' contributions. In 1801, the barrister and political commentator William Hunter lamented that 'the Russians are far from being expert navigators'.[88] But the view was quite different by the 1840s. English translations of accounts of Russian circumnavigations proved popular, and one translator considered Kotzebue's voyage to be of 'great importance to geography and navigation'.[89] The English geographer Alexander Findlay reckoned Russian charts produced on the voyages were the best available for some regions, and Charles Darwin used the charts and accounts of Litke, Bellingshausen, Kotzebue and

Krusenstern to develop his theories on coral reefs.[90] Britain's knowledge of the oceans thus depended in significant ways on Russian expertise.

Notes

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2. See Roy MacLeod, 'On Visiting the Moving Metropolis: Reflections on the Architecture of Imperial Science', in *Scientific Colonialism: A Cross-Cultural Comparison*, ed. by Nathan Reingold and Marc Rothenberg (Washington DC: Smithsonian Institution, 1987), pp. 217–49; Michael H. Fisher, *Counterflows to Colonialism: Indian Travellers and Settlers in Britain, 1600–1857* (Delhi: Permanent Black, 2006); Kapil Raj, *Relocating Modern Science: Circulation and the Construction of Knowledge in South Asia and Europe, 1650–1900* (London: Palgrave Macmillan, 2010).
3. See J. A. Bennett, 'Science Lost and Longitude Found: The Tercentenary of John Harrison', *Journal of the History of Astronomy*, 24 (1993), 281–87; J. A. Bennett, 'The Travels and Trials of Mr Harrison's Timekeeper', in *Instruments, Travel and Science: Itineraries of Precision from the Seventeenth to the Twentieth Century*, ed. by Marie-Noelle Bourguet, Christian Licoppe and H. Otto Sibum (London: Routledge, 2002), pp. 75–95; Richard Dunn and Rebekah Higgitt, *Finding Longitude: How Ships, Clocks and Stars Helped Solve the Longitude Problem* (Glasgow: Collins, 2014); Katy Barrett, '"Explaining" Themselves: The Barrington Papers, the Board of Longitude, and the Fate of John Harrison', *Notes and Records of the Royal Society*, 65 (2011), 145–62.
4. Valerie Kivelson, *Cartographies of Tsardom: The Land and Its Meanings in Seventeenth-Century Russia* (Ithaca: Cornell University Press, 2006).
5. O. M. Raspopov and V. V. Meshcheryakov, 'Magnetic Declination Measurements over European Russia and Siberia in the 18th Century', *Geomagnetism and Aeronomy*, 51 (2011), 1146–54, (p. 1146).
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7. Ryan, 'Navigation and Modernisation', p. 91.
8. A. V. Solov'ev, 'Russkie navigatory sredi iuzhnykh slavian', in *Iubileinyi sbornik Russkogo arkheologicheskogo obshchestva v korolevste Iugoslavii* (Belgrade, 1936), pp. 291–301; A. Florovskii, 'Moskovskie navigatory v Venetsii v 1697–1698 gg. i rimskaia tserkov', in *Ost und West in der Geschichte des Denkens under der Kulturellen Beziehungen. Festschrift für Eduard Winter* (Berlin, 1966), pp. 195–99.
9. Il'ia Kopievskii, *Kniga, uchashchaia morskogo plavaniia* (Amsterdam, 1701); see Ryan, 'Navigation and Modernisation', p. 89.
10. Paul Dukes (ed.), *The Caledonian Phalanx: Scots in Russia* (Edinburgh: National Library of Scotland, 1987); Mark Cornwall and Murray Frame (eds), *Scotland*

and the Slavs: Cultures in Contact 1500–2000 (Newtonville, MA: Oriental Research Partners, 2001).

11. Ryan, 'Navigation and Modernisation'; see also Nicholas Hans, 'The Moscow School of Mathematics and Navigation (1701)', *Slavonic and East European Review*, 29 (1951), 532–36; Nicholas Hans, 'Henry Farquharson, Pioneer of Russian Education', *Aberdeen University Review*, 38 (1959), 26–29; D. Fedosov, 'A Scottish mathematician in Russia: Henry Farquharson in Russia (*c*.1675–1739)', in *The Universities of Aberdeen and Europe: The First Three Centuries*, ed. by Paul Dukes (Aberdeen, 1995), pp. 102–18; Robert Collis, *The Petrine Instauration: Religion, Esotericism and Science at the Court of Peter the Great, 1689–1725* (Leiden: Brill, 2012), pp. 70–74.
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13. Ryan, 'Navigation and Modernisation', p. 87.
14. Stepan Gavrilovich Malygin, *Sokrashchennaia navigatsiia po kartie de-rediuksion* (St Petersburg, 1733). Malygin was a graduate of the Moscow School of Navigation before serving in the Baltic fleet; another text was Fedor Ivanovich Soimonov, *Ekstrakt shturmanskago iskusstva iz nauk prinadlezhashchikh k moreplavaniiu sochinennyi* (St Petersburg, 1739). Soimonov was also a graduate of the Moscow School of Navigation, serving in campaigns against the Swedes and Turks. He made a hydrographic survey of the Caspian Sea between 1719 and 1727; Leonid Arkad'evich Gol'denberg, *Fedor Ivanovich Soimonov* (Moscow: Nauka, 1966).
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21. Raspopov and Meshcheryakov, 'Magnetic declination measurements', p. 1147.
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23. Ibid. pp. 1148–53; on the second expedition see J. L. Black, *G. F. Müller and the Imperial Russian Academy* (Kingston and Montreal: McGill-Queen's University Press, 1986); B. G. Ostrovskii, *Velikaia Severnaia ekspeditsiia 1733–1743 gg.* (Arkhangel'sk: Sevkraigiz, 1935).

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43. 'Il manque aussi extrement aux Vaissaux de Notre Majesté Imperiale des officiers Subalternes disciplinés, et un nombre de Bons Boatmans maats pour arranger et disposer les matelots a leurs devoirs respectifs, tant dans la Navigation que dans le manoeuvre des Vaisseaux, en quoi j'ai eté bien informé qu'ils sont fort defectueux, particulierement dans l'obscurité et dans le mauvais tems.' Correspondence of Catherine and Admiral Knowles, NMM LBK/80.
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80. Urey Lisyansky [Iurii Fedorovich Lisyanskii], *A Voyage Around The World in the Years 1803, 4, 5, & 6* (London, 1814), p. 22; Glynn Barrett, *The Russians and Australia* (Vancouver: University of British Colombia Press, 1988), p. 76.
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83. Otto von Kotzebue, *A New Voyage Round the World in the Years 1823, 24, 25, and 26*, 2 vols (London, 1830), I, 145.
84. Kotzebue, *Voyage of Discovery into the South Seas*, I, 176.

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86. Bellingshausen, *Voyage [...] to the Antarctic Seas*, I, 102.
87. Kotzebue, *Voyage of Discovery into the South* Seas, I, 155.
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8

A Different Kind of Longitude: The Metrology of Location by Geodesy

Michael Kershaw

> 'Tis in vain to talk of the Use of finding the Longitude at Sea, except that you know the true Longitude and Latitude of the Port for which you are designed.
>
> John Flamsteed to Samuel Pepys, 21 April 1697[1]

Histories of longitude often start with descriptions of maritime disasters that have been ascribed to the problem of finding longitude at sea. One much-cited example is that of the unfortunate Admiral Sir Cloudesley Shovell, who perished in 1707 along with at least 1,400 of his men. Several of his fleet's ships struck rocks off the Isles of Scilly on the way home, in a costly failure of navigation. But a historical emphasis on the difficulties of onboard navigation can distract our attention from uncertainties elsewhere. When navigating by dead reckoning, any positional error in the point of departure is carried throughout the voyage. In Shovell's case, we know that the stated longitude of his reference point – near Gibraltar – was located too far west. Even if he had subsequently been able to compute exactly where he was, there was considerable uncertainty as to the position of the islands that he was trying to avoid.[2] The Admiral's fate was an exemplar of the importance of positional accuracy both at sea *and* on land.

Until long after the time of Flamsteed, quoted above, the positions of major ports were determined astronomically. It was easier to observe the heavens from dry land than from the deck of a ship at sea. But the problem remained that the known astronomical methods of determining longitude were inherently imprecise. The new technique of surveying by triangulation, developed in France during the eighteenth century, offered potential improvement. Such surveys could fix the position of ports and coastlines, relative to the country's main observatory, with more precision than any astronomer. They could also be extended to islands at the limits of visibility from the mainland, and joined to other national triangulations.

Even across seas, the process simplified the problem to establishing the relative position of two national observatories, from which the configuration of their country's landmass could be determined by survey.

The use of triangulation surveys introduced, however, an entirely new difficulty: the need for precise standards of length. Astronomical measurement of latitude relied only upon measurement of angle, universally based on the division of a circle. Astronomical measurement of longitude relied only upon measurement of time, the units of which were defined by the movement of the heavens. Surveying, however, relied upon the measurement of length, the units of which were quite arbitrary. In the absence of some easily accessible universal standard of length, as one geodesist despairingly wrote, 'all we can do is construct bars of metal as standards and provide them with names'.[3] These standard bars (for which I will use the term *étalons*) were used to calibrate the rods that, in turn, were used to measure the baselines that defined the scale of every triangulation.[4] The measurement of a triangulation network is, however, insufficient on its own to deduce values of latitude and longitude. To translate measurements of distance into latitude and longitude, we need to know the size and shape of the Earth, and to specify the coordinates of a point of origin – such as Greenwich – thus defining the geodetic datum.

Finding the 'figure of the Earth' was one of the *grands projets* of eighteenth- and nineteenth-century science. It involved the survey of vast meridian arcs in Europe, Russia, India and the Americas. These geodetic measurements were the most demanding of all contemporary sciences in their need for precision in the determination of length. As a Nobel prize-winning metrologist of the early twentieth century made clear: 'often, in the last two centuries, geodesy has preceded metrology proper, or at the least has driven its progress'.[5] Metrological historiography, which concentrates on the metric system, only partly reflects that view. Here we learn of the definition of the metre as one ten-millionth of the distance from pole to equator through Paris, and its determination by measurement of the meridian arc of France in the 1790s. We also have an extensive history of the international adoption of the metric system from the mid-nineteenth century, in the face of an enduring 'battle of the standards' between metric and imperial norms. The metric system, however, had limited impact on geodesy until the twentieth century, and we must look elsewhere to understand geodetic metrology.[6]

This chapter examines the British and French metrologies that supported their early geodesy. From the later eighteenth century, British and French practitioners had reason to bring together their measurements. There was, however, no fixed numerical conversion ratio between British and French norms of length (and, indeed, there wasn't until well into the twentieth century).[7] How, then, could these two groups combine results? I will show that there was no battle of the standards here; metrological coherence was achieved between a remarkable variety of authoritative *étalons*, with different lengths, names and physical configurations, by means of extensive comparisons from *étalon* to *étalon*. This was possible because British and French geodesists formed networks of trust – of practitioners, instruments and techniques – that reached across the Channel. In other words, the suggestion in some of the literature that well-defined universal standards are somehow necessary to facilitate the practice of precision measurement is demonstrably a 'metrological fallacy'.[8]

This technique of inter-comparison of *étalons* was extended internationally. By the end of the nineteenth century, it had become an effective metrological basis for the establishment of the figure of the Earth, and a firm foundation for geodetic longitude measurement. Therefore, there were now two reliable operations for the measurement of the relative longitude of two places. The first was determination of the difference of astronomical *time* between them as a fraction of a day; the second was determination of the *distance* between their two meridians as a fraction of the circumference of the Earth.[9] It was axiomatic to practitioners of the eighteenth and nineteenth centuries that these two different *kinds* of longitude (using the term in an operationalist sense) were identical.[10] I will conclude by explaining why it was found in the twentieth century that they were wrong, and how we now use a different kind of longitude from that with which Flamsteed was familiar.

French and British metrology

French precision metrology had its roots in seventeenth-century geodetic measurement. There was strong control by the Académie des sciences; clear identity between geodetic and legal measure; and innovation was always combined with respect for historical standards. By contrast, British metrology had no basis in geodetic practice; it embraced a variety of institutional influences, legal definitions and competing *étalons*; and it suffered from significant discontinuities. An understanding of the relationship between French and British metrologies requires an appreciation of their different natures.

The metrology of the Académie

One of the great scientific questions of the late eighteenth century was the shape of the Earth – flattened at the poles or otherwise – a matter often presented as a conflict between the predictions of Newtonian and Cartesian natural philosophies. The issue could be settled only by measuring the length of a degree of arc at two different latitudes. Therefore, in the 1730s, the Académie mounted geodetic expeditions to Peru and Lapland. Consistent standards of length were essential for their work. The then legal standard in France was a bar of rudimentary construction, mounted outdoors for public use, known as the *toise du Châtelet*. For the expedition to Peru, an improved *étalon* known as the *toise du Pérou* was calibrated from it, constructed to new standards of precision. It was an iron bar with a rebate at each end, exposing the two parallel faces that defined its length.[11] A second, notionally identical bar was used on the expedition to Lapland, and the Académie made a small number of additional copies. In due course this new *toise de Pérou* replaced the *toise du Châtelet* as the legal standard.

The next development in French metrology was the creation of the metric system at the end of the eighteenth century. The benefits of a universal measure of length, drawn from nature, were much promoted and the choice was the metre, defined as one ten-millionth of the length of the meridian arc from pole to equator through Paris. A re-measurement of the Paris meridian followed, an extravagant project promoted with an emphasis on the highest possible precision. For the measurement of its baselines, a novel device made of platinum known as the Borda rule was used, calibrated to be exactly twice the length of the *toise du Pérou*.[12] The meridian arc was therefore measured in toises. The length of the metre was then calculated to be a particular fraction of a toise, and construction of a platinum bar, called the archive metre, followed. The metre was not, therefore, completely new, but derived from the metrology of the Académie.

By the start of the nineteenth century, therefore, the practice of precision measurement in France was based on two units of length that were linked in fixed ratio – the toise and the metre – and represented by three *étalons*. The first was the *toise de Pérou*, now disused but the basis for much historic data and the parent of many secondary *étalons*. The second was the Borda rule, notionally twice that length, and regarded by many as the truest *étalon* of geodetic length.[13] The third was the archive metre, representing a unit whose geodetic use was still limited. This system was vastly better ordered than that across the Channel.

The metrology of Britain and its empire

The legal foundation of British length standards was the Exchequer yard, a somewhat rough-and-ready brass bar dating back to the time of Elizabeth I.[14] Progress in the experimental sciences brought greater need for precision, which learned societies and instrument makers met. By the end of the eighteenth century, a number of scales (metal rules inscribed with length markings) were in common use amongst men of science as *de facto* standards, constructed by well-known makers such as Troughton, Bird, Aubert and Graham. As these scales were of higher precision than the primary standard (meaning they could be compared and reproduced with greater certainty), they became a material collective that effectively replaced it. That was achieved by a process of mutual grounding to establish their interrelationships, notwithstanding the inevitable difficulties presented by the lack of any firm point of reference.[15] This uncertain collective supported the geodesy of Britain and its empire around 1800. In the calibration of the few baselines that supported this early geodetic measurement, there is reference to as many as seven different scales.[16]

There was some prospect of order by the 1820s, as a parliamentary committee planned a new imperial system of weights and measures. Their starting point was to align the legal standard with that used in the country's geodetic operations, in particular the first baseline measured in the 1780s. Unfortunately, they found a sizeable difference between that particular yard and 'every other source of authority'.[17] The linkage of legal and geodetic standards was therefore abandoned. The committee fell back on an older *étalon* as legal standard, even though the points marking its length had become enlarged by the repeated application of beam compasses. In the face of this indeterminacy of the legal standard, users created their own. The Royal Society procured one, which defined a yard by the distance between two dots on gold discs set into a brass bar. The Astronomical Society followed, with a 5-foot tubular brass scale. In addition, the Ordnance Survey developed new baseline measuring apparatus in the late 1820s, built by the instrument makers Troughton and Simms, which used a 10-foot iron standard known as $O_{1.}$ To achieve coherence, all these standards were brought together in an extensive exercise in mutual grounding, involving dozens of physical inter-comparisons over two decades.[18]

Such shaky foundation as existed for this collective – the legal standard yard – was destroyed when the Houses of Parliament burnt down in 1834. Accordingly, a new standard had to be constructed *ab initio*, based on *étalons* that had been compared to that lost. This took some 20 years, during which there were significant technical advances in materials, comparison techniques and temperature control. [19] However, the imperial yard that resulted was explicitly a wholly new standard of length, becoming yet another member of a diverse material collective. It follows that in discussing British metrology and its coherence with other systems, we have to look behind the units to the *étalons* employed by particular users, at particular times, for their measurements.

Metrological coherence across the Channel

In 1742, a notice was read at a Royal Society meeting:

> Some curious Gentlemen both of the Royal Society of London, and of the Royal Academy of Sciences at Paris, thinking it might be of good Use, for the better comparing together the Success of Experiments made in England and in France, proposed some time since, that accurate Standards of the Measures and Weights of both Nations, carefully examined, and made to agree with each other, might be laid up and preserved in the Archives both of the Royal Society here, and of the Royal Academy of Sciences at Paris[20]

As a result, two brass rods were calibrated against a standard yard kept at the Tower of London. Both were sent to the Académie des sciences to be marked off against a Paris half-toise, and one bar was then returned to London. Here, the yard and half-toise were compared and found to be related to each other in the ratio 107 to 114. In Paris, repeating the comparison independently produced a near-identical result.[21] It was explicit that this comparison between particular *étalons* was only for the use of men of science, and emphatically not for the establishment of 'the just and legal Proportions between the Weights and Measures of both Nations'.[22]

We do not know what experiments the members of the respective academies had in mind, and a quarter of a century passed before the comparison of French and British units was referred to again. The cause was the first British arc measurement, carried out by Charles Mason and Jeremiah Dixon, two English astronomer-surveyors, in the 1760s. While surveying a boundary between the colonies of Pennsylvania and Maryland, the availability of an easily measured north-south line prompted them to seek the Royal Society's support in carrying out an arc measurement. The interest in the result, the length of a degree of arc at that latitude, was in its comparison to data from other arcs. That data was mainly French, and therefore expressed in toises. In considering the conversion of the British result from feet to toises, the Astronomer Royal expressed some disquiet as to the accuracy of the previously established ratio. He therefore procured a trusted copy of the *toise de Pérou* and commissioned a British instrument maker to make a new comparison against the Royal Society scale. The new result was inevitably, and inexplicably, slightly different.[23] We see from this exercise that there was a geodetic need to allow British results to be made commensurable with French data, but we are unable to judge the success of the unilateral conversion. The proper test of coherence between metrologies is that comparable results must be obtained when the techniques and standards of different users are applied independently to measurement of the same phenomena. That test soon followed.

Metrologies meet by triangulation

The metrologies of Britain and France first met by geodetic triangulation across the Channel. During the eighteenth century, the observatories of Paris and Greenwich were the most important in the world, defining the two most-used reference meridians. They provided data for annual publication in the *Connaissance des temps* and *Nautical Almanac*, one or other of which was a *vade mecum* for navigators using astronomical techniques. These annuals contained astronomical tables for determining longitude, and the measured longitude of important locations – referred to a zero of longitude through Paris or Greenwich respectively. Both publications, however, relied in part upon data from the other country's observatory, necessarily adjusted for their difference in longitude. Therefore, if a re-measurement caused a notional movement of Greenwich relative to Paris, various far-flung parts of the British and French empires would do the same in sympathy. Their relative longitude was therefore a matter of importance, but also of considerable uncertainty.[24] Improved accuracy was promised by the possibility of forming a trigonometric junction between the two observatories.

The initiative was French, and followed the successful triangulation of that country in the mid-eighteenth century, led by the astronomer Cassini de Thury. He argued that an extension of the triangulation to Greenwich was uniquely important in allowing the determination of the relative positions of the two 'most celebrated' observatories in Europe within a distance of a few toises.[25] In 1783, the Académie presented a memorial proposing the extension and the following year Joseph Banks, President of the Royal Society, gave instructions for the project to proceed. Preparations commenced for the measurement of baselines in England, and for triangulations from Greenwich to Dover and across the Channel.[26] This last exercise has been portrayed as a competitive endeavour that, 'for a while turned the coast of the English Channel into an arena for technological rivalry between Britain and France'.[27] It was true that the British and French adopted different measurement techniques but, at the level of the practitioners, there was a spirit of co-operation. The French team, led by Jean-Dominique Cassini (son of Cassini de Thury), spent time at Dover Castle with General Roy, the British military engineer and surveyor, to plan the measurements. Roy wrote of the pleasure of the company and that everything was settled 'in the most amicable manner possible'.[28]

The cross-channel observations measured only the angles of the triangles joining the chosen stations in England and France (Figure 8.1). For

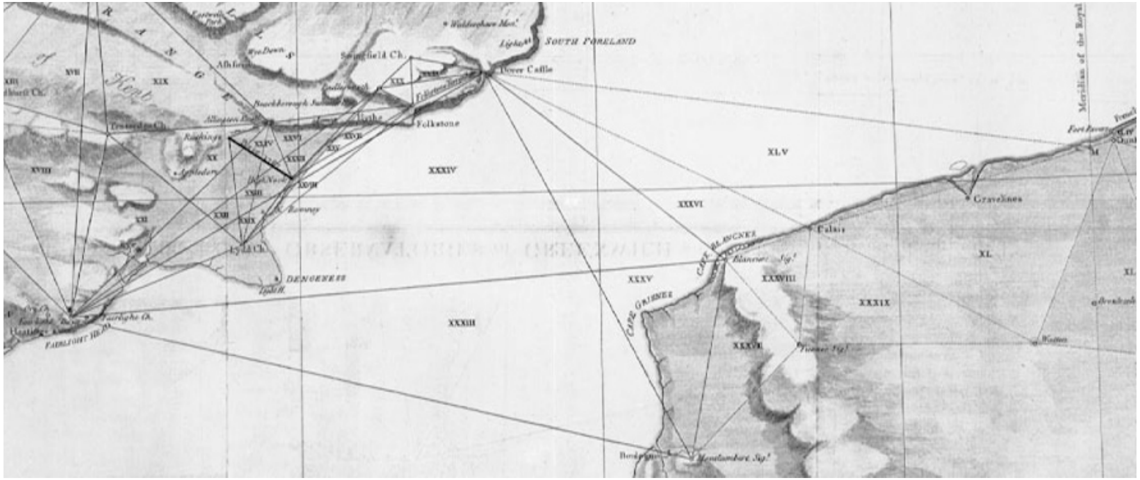

Figure 8.1 The cross-channel triangulation, from the 'Plan of the Triangles whereby the Distance between the Royal Observatories of Greenwich and Paris has been determined' in William Roy, 'An Account of the Trigonometrical Operation', *Philosophical Transactions*, 80 (1790). The bold line near the English coast is the base of verification on Romney Marsh (The David Rumsey Map Collection www.davidrumsey.com)

the metrologies of Britain and France to meet, a scale of length had to be determined on each side of the Channel, by joining the triangles to a national baseline. In England, the triangulation was extended, via a base of verification on Romney Marsh, to the country's new primary base on Hounslow Heath (now Heathrow Airport). Both bases were measured, using new techniques of glass rods and metal chains, in feet derived from the Royal Society's scale.[29] In France, the coastal triangulation was connected via the existing meridian triangulation to the country's primary base near Paris. That had been measured by Cassini de Thury in the 1740s, using new techniques of metal measuring rods, painstakingly calibrated against the *toise de Châtelet*.[30] Thus the metrologies of Britain and France met. The base at Hounslow Heath was connected by a series of nearly 70 British and French triangles to the base near Paris. Those bases were almost certainly the two most precisely measured distances in the world, but they were defined in different units.

Coherence was tested by computing the length of an intermediate base at Dunkirk from each national primary base and the intervening triangulation. Over a distance of about eight miles, the British and French results differed only by about one foot. Roy wrote of the 'wonderful degree of accuracy [of] operations of this sort', and the French were content too, even if not quite so self-confident. For them, the agreement was as 'surprising as it was satisfying'.[31] Yet the two accounts scarcely mention the translation between metrologies that was necessary to compare results. Roy's account is set out mainly in feet, while Cassini expressed his results almost entirely in toises. The conversion ratio is never explicit. In fact, to understand how conversions were made, we need to look at a plate at the end of Roy's account, which gives the ratio. Cassini does not even mention the subject, and we need to look elsewhere, at a later French translation of Roy's account, to find what we would now describe as a ready reckoner for conversion.[32] This was based on the comparison carried out 30 years earlier for the arc measurement of Mason and Dixon.

That comparison had linked not any norms of British and French length measurement, but specifically the two *étalons* that later were to support each side of cross-channel triangulation. On the English side, the bases were defined in terms of feet of the Royal Society's scale. On the French side, the Paris base was defined in terms of the *toise de Châtelet*. The conversion ratio had been derived from precisely these two *étalons*.[33] The ratio was a mutually understood fact, based on interaction between the two institutions. It was implicit in the way they mixed their observations without commentary, and in their conclusion that any constraints on accuracy were perceived to arise from sources other than the conversion between metrologies. For these users their metrologies were effectively coherent. Such coherence was the result of the physical comparisons between *étalons*, within a network of trust of practitioners, instruments and standards that brought experimental success.

Metrologies meet with the seconds pendulum

The next meeting of British and French metrology arose in a different way – through gravimetric experimentation, in which the length of a seconds pendulum (a pendulum with a period of oscillation of exactly one second) is determined at different latitudes. Although historians have paid less attention to pendulum experiments than to arc measurements, they were an important part of the science of geodesy and contributed significantly to the determination of the figure of the Earth.[34] Again, the French led the way. An expedition to French Guiana, mounted by the Académie in the 1670s, was the first to note that a pendulum clock beat slower at this latitude than at Paris, and subsequent expeditions to Lapland and Peru therefore travelled with pendulums. In the mid-eighteenth century, the mathematician Alexis Clairault provided a theoretical basis for computation of the Earth's ellipticity from local determinations of gravity and, by the early nineteenth century, the British too were very active in the field. This section will describe briefly the pendulum techniques that each country adopted, showing how their results were linked to local *étalons* of length and then brought together.

There is a close connection between pendulums and metrology. It is a product of Newtonian mechanics that the period of oscillation of a simple pendulum varies only with the square root of its length; it follows, in principle at least, that the seconds pendulum offers a way of defining a standard of length. It was occasionally proposed as a standard during the seventeenth and eighteenth centuries, though never adopted. Again, when the metric system was being conceived, the seconds pendulum was given serious consideration, even though a standard based on the size of the Earth prevailed. When the British created new imperial standards in the 1820s, they provided for recreation of the yard *étalon*, in the event of its loss, by definition of the length of a seconds pendulum beating in London.[35] Thus, while the seconds pendulum might be considered a perpetual runner-up as a standard of length, it was a fairly close one, and the identity between pendulum experiments and precision measurement of length was well established.

Pendulum experimentation is surprisingly complex, both in terms of the construction of the apparatus and the corrections needed to compensate for factors such as drag and lift from the atmosphere, elasticity of the suspension, the changing arc of the swing, and adjustments to sea level. The basic technique relied upon a pendulum swinging for several hours, at a slightly different rate to the pendulum of an astronomically regulated clock; observation of the infrequent but regular coincidences between the two allows the pendulum rate to be determined. A very large and sophisticated pendulum apparatus that Borda created, and calibrated against the Borda rule itself, was employed at the Observatoire de Paris in 1792.[36] Later, and in the field, French practitioners used shorter pendulums for convenience, calibrated against a standard metre.[37] British techniques were somewhat different and emerged from work by Henry Kater as a member of a pendulum committee established by the Royal Society; but again, calibration was diligent, this time using what was known as the scale of Shuckburgh.[38] Until the early nineteenth century the two groups of practitioners operated in different places, so their experimental results remained incommensurable. But, towards 1820, geodetic necessity caused their operations to overlap geographically, and we can assess the coherence of their metrologies.

The first places at which both French and British pendulums were swung were, somewhat curiously, Edinburgh and Unst, one of the northern Shetland Islands. A French expedition to Scotland was part of a project to take measurements along an extended meridian arc through France: although a southerly extension by triangulation through Spain to the Balearic isles had been achieved, it was not possible to the north. In part, that was because the triangulation of England was still work-in-progress, and, in part, because an English arc extension would necessarily be slightly to the west of the Paris meridian. It was thought, however, that pendulum determinations were less troubled by such small irregularities. Accordingly, Jean-Baptiste Biot, a French astronomer and mathematician, set out in 1817 to carry out pendulum measurements in England and Scotland, using the same apparatus that had served him earlier in France and Spain. He was followed one year later by Kater, who had been instructed to determine the length of a seconds pendulum in London and at the principal stations of the Ordnance Survey, again in connection with the processes instigated by Parliament to improve standards of weights and measures.

Biot's visit was a cooperative endeavour. He arrived in England laden with pendulums, repeating circle, astronomical clock and chronometers. Under the auspices of the influential Banks 'everything became easy'. Biot had support from the Ordnance Survey, whose officers accompanied him on his journey; from the Navy, who provided a brig to transport him and his equipment; from the Astronomer Royal, who offered him 'every facility imaginable' on a return trip via Greenwich; and from the Royal Society, which allowed use of their platinum metre *étalon,* which Biot himself had previously verified in Paris, to check the calibration of his pendulum after use in the field. It was slow, disagreeable and difficult fieldwork. His pendulums were swung in an empty cow-shed, and his portable observatory set up in a garden nearby; all this on an island that Biot described as 'foggy, rocky, without roads and without trees [...] a kingdom of rain, wind and tempest [...] the atmosphere always cold and wet'.[39] Such rigours notwithstanding, Kater followed in Biot's footsteps (Figure 8.2).

Kater published first, and could only make preliminary, though encouraging comparisons with Biot's work.[40] Biot published second, and could do the job properly. The results showed that they differed as to the length of a seconds pendulum at Edinburgh and Unst by less than 0.002 millimetres, or about 2 parts per million.[41] These comparisons relied on the conversion of Kater's data into metres. The parliamentary instructions for Kater's pendulum experiments had required him not only to determine the length of pendulums in London and at the stations of the Ordnance Survey in terms of the British standard of length, but also to compare that standard with the new continental standard.[42] As a result, he had undertaken exhaustive comparisons of two metre *étalons* calibrated in Paris, and had published a conversion factor: 1 metre was equal to 39.37079 inches of the Shuckburgh scale.[43] It was by using this conversion ratio that Biot's and Kater's results agreed so closely. Biot concluded that the French and British pendulum apparatuses were equally good, giving 'exactly the same results when used in the same location and with equal care'.[44] That conclusion rested on coherence between English and French experimental technique and metrology.

Just as striking was the confidence in the accuracy of the conversion between length standards, illustrated by the way in which inconsistencies in experimental results were approached. There were by now three available values for the length of a seconds pendulum in Paris. Two had been measured directly, and a third could be derived from Kater's determination in London, adjusted for the relative gravitational fields and converted from inches to millimetres. The discrepancy between highest and lowest results, as much as 40 parts per million, was significant. However, suspicion did *not* fall on the metrology, rather on possible imperfections of the measurement processes.[45] That suspicion was actually correct: resolution was obtained a few years later by Britain's most prominent practitioner of pendulum experiments, Edward Sabine, who re-measured the relative gravitational fields at Greenwich and Paris. Using this revised determination, but with the same conversion ratio between Shuckburgh feet and metres, the discrepancies were almost completely eliminated.[46]

It is therefore clear that for the purposes of gravimetric experimentation in the early nineteenth century, the metrologies of Britain and France were coherent. They were linked by a single ratio between two *étalons*, determined from comparisons of the Shuckburgh foot scale to a metre

Figure 8.2 Memorial to Biot and Kater, erected by Thomas Edmondson, host to both scientific visitors © RCAHMS. Licensor www.rcahms.gov.uk

étalon of irreproachable provenance. This ratio was incommensurable with the ratio recently used by geodetic practitioners to link the triangulations of England and France, which was based on the Royal Society scale and the *toise du Châtelet*. Yet it was just as much a mutually understood fact, only for a different group of users with different techniques. These practitioners of gravimetric experimentation formed a network of trust capable of coherently assembling data based on different standards of length, in just the same way – based on comparisons from *étalon* to *étalon* – as earlier geodesists were able to join triangulations.

Further meetings by triangulation

Ever more precise determination of the relative positions of the observatories of Greenwich and Paris was a continuing theme. The Board of Longitude and the Bureau des longitudes carried out a second cross-channel triangulation in the 1820s, although the results were never published in full, while a third was completed in the 1860s.[47] As will be shown, effective metrological coherence was achieved once again, despite this being at a time when, the literature tells us, the 'battle of the standards' was fully engaged.[48] The reasons for another triangulation were twofold. The first was to refine further the coherence of astronomical and navigational data published by the Paris and Greenwich observatories. The second was to refine knowledge of the figure of the Earth, taking advantage of new telegraphic techniques for measuring longitude. The triangulation of Great Britain had recently been completed and it had been proposed that by linking this with the triangulations of France, Belgium, Prussia and Russia, a vast arc of parallel, subtending 75 degrees from the Atlantic to near the Caspian Sea, could be measured.[49] The use of the electric telegraph to determine the relative longitudes of the ends of the arc would then provide a 'crucial test' of the figure of the Earth.[50] The precision of the existing cross-channel linkage, which formed part of the arc, was considered inadequate, so it had to be re-measured.

There were, as with the first cross-channel triangulation, issues of competition. British geodesy was in the ascendant. The Principal Triangulation of Great Britain, undertaken in the first half of the nineteenth century, set new standards of instrumental and mathematical technique. In contrast, post-Napoleonic French geodesy had not regained its former prominence. This partly reflected a perception that the measurement of the metric meridian had exhausted the subject, as well as a shift to military surveying of comparatively low precision.[51] So when a British proposal for a new cross-channel triangulation arrived in 1860, there were few French officers left with relevant experience, and any instruments they had were dilapidated. The French accepted the challenge, but the team was a reflection of their limited capabilities: it was led by a veteran who retired during the fieldwork, assisted by two young captains, neither of whom had undertaken geodetic work since leaving their staff college. It was, for the French, a baptism of a new generation of officers.[52] The British, by contrast, fielded a full and experienced team, including the renowned Alexander Ross Clarke, head of the Trigonometrical Department of the Ordnance Survey and recently responsible for the intricate computation of the results of the Principal Triangulation.

The project's execution was another highly collaborative exercise. The Ordnance Survey officers went to Boulogne to meet their French counterparts and make arrangements. The French and British teams made all cross-channel observations in duplicate and constructed the coastal triangulation stations at shared expense. The British account of the project includes correspondence involving the British Ambassador in Paris, the French Ministers of War and Foreign Affairs and even the French and British customs, as evidence of the depth of the collaboration.[53] The French officers wrote 'from the heart' of the excellent relations with the British engineers, described from the British side as a 'most perfect accord'.[54] The British account is matter-of-fact, though it clearly required some endurance to live under canvas in severe winter weather, nonetheless thought better than the 'miserable hovels' in that part of France.[55] It was, however, routine for the British – even the long measurements across water, because the Principal Triangulation had been extended to Ireland and the Shetlands. The French account, by contrast, is a sorry tale of difficulties with instrumentation and delays through rain, fog and gales; they struggled on but their fieldwork took months longer than that of the British.

On the British side, the coastal triangles were linked to the Principal Triangulation. Its scale was determined by two bases, in Northern Ireland and on Salisbury Plain, which had been measured with the Ordnance Survey's base apparatus. On the French side, the triangles were joined to those of the metric meridian near Dunkirk, and thence to its base near Paris. As had been the case in the 1780s, the cross-channel triangulation provided a link between what were probably the most precisely measured distances in the world, and the results were satisfactory. The difference in length of the sides of the cross-channel triangles, as computed by the British and the French, was only a few parts per million. The French wrote that, even if the excellent agreement might be due in part to the compensating errors that occur in all triangulations, the result 'might inspire some confidence'.[56] The British, probably reflecting their limited faith in the capabilities of the French, called the agreement 'truly surprising'.[57]

The accounts, however, gloss over the different metrologies, with the British bases having been measured in Ordnance feet and the French base in toises of the Borda rule. The British account gives no more than the logarithm of an unexplained conversion ratio, while the French makes no mention at all. That, I suggest, is because again the issue was so well understood. The mutual grounding of the various British *étalons*, both amongst themselves and with French ones, had been continuous during the first half of the nineteenth century, and gave the precise answer – albeit somewhat inelegantly, since the relationship between Ordnance Survey bar O_1 and the Borda rule had only been established via a series of intermediary *étalons*. [58] But the physical comparisons had been carried out with the utmost diligence, repeated at closely controlled temperatures, by multiple observers, using the best contemporary comparator technology. Once again, French and British metrology was made coherent by comparison from *étalon* to *étalon* – different *étalon*s to those described earlier, with a numerical ratio that was different to either previous example. And, once again, the group of users collaborated in a network of trust.

The figure of the Earth

Geodetic measurement in the nineteenth century was an increasingly international, rather than Anglo–French, affair and one of its principal purposes remained the determination of the figure of the Earth. One aspect was the project to measure the arc of the 52nd parallel, of which the third cross-channel triangulation had formed part. This was the catalyst for yet wider inter-comparison of geodetic *étalons* and refinement of geodetic metrology. As Alexander Ross Clarke of the Ordnance Survey wrote, until the relative lengths of those standards used along the arc were known, 'it would be impossible accurately to express the length of the arc of parallel in terms of any one of the standards.'[59] What followed was an invitation by the British Government to various countries to send their standards for comparison. A new comparator apparatus of remarkable sophistication and precision was installed in a purpose-built room at the Ordnance Survey in Southampton. Here, *étalons* compared in the 1860s included various bars from Britain and its empire, the standard toises of Belgium and Prussia, and the Russian double-toise; many others followed.[60]

During the nineteenth century the figure of the Earth was continually refined, as additional arc measurements were made and the metrological coherence of data was improved. For example, when the Astronomer Royal George Airy published the dimensions of what is known as the Airy 1830 spheroid, he had data from arcs together subtending about 45 degrees, and used a single conversion ratio between British and continental measures.[61] Likewise, the Clarke 1866 spheroid drew on data from arcs subtending nearly twice that distance, while Clarke's comparisons at Southampton made the relationship between the various British, European and Russian *étalons* much more secure. That said, any nineteenth-century figure of the Earth remained a somewhat curious metrological hybrid. Of the total amplitude of the arcs Clarke used, less than half was measured using any foot standards; other contributions were from arcs using the Russian toise and *sazhen*, German toise, the Borda rule, the *toise de Pérou* and others. In addition, although most of this data was incorporated on the basis of diligent comparisons *étalon* to *étalon*, there were always loose ends.[62] Therefore, we can never be sure of the exact the length of the foot or toise used in any published figure of the Earth in the nineteenth century. However, the process was good enough for its purpose. Towards the end of the century, as successive determinations converged, the work was deemed complete. Major John Herschel, a respected British military geodesist, expressed that position in the 1880s: '*we do actually know* the mean figure of the earth *as well as we can know it*'.[63] There was, at last, an apparently secure basis for the geodetic mapping of the world.

Conclusion

In this chapter I have emphasized the importance of metrological coherence to geodesy, first between Britain and France and then internationally. I have shown that metrological coherence was achieved not at the level of any norms, but at the level of the *étalon*. If it were necessary for metrologies to meet, comparisons were made between the particular *étalons* that supported the relevant experimental work. Between Britain and France the Royal Society's scale met the *toise de Châtelet*, the Shuckburgh scale met the archive metre, and the Ordnance foot met the Borda rule. Each of these numerically different, effectively incommensurable, conversions was fit for its particular purpose.[64] The same was true across Europe, Russia and the British Empire.

Historians have paid little attention to metrological coherence. To the extent that they do address the relationship between British and French practice, it is generally through the 'battle of the standards'. There is, however, no evidence of metrological conflict amongst men of science. Rather, as Gavin de Beer and others have argued, the sciences of Britain and France were highly collaborative during the eighteenth and nineteenth centuries.[65] In observing how British and French practitioners brought their geodetic and gravimetric measurements together, I have explored this collaboration in new detail. Looking at the techniques and *étalons* actually used by practitioners of different nations has shown how they were able effectively to share and combine data, despite their different and developing metrologies. Practitioners formed networks of trust, evidenced by the mutual grounding of their experimental results, institutional interactions and face-to-face encounters. Within each of these networks, multiple *étalons* could be embraced, with metrological coherence maintained by inter-comparisons of those trusted artefacts. As Airy wrote, when rejecting the need for any legal metric standards for use in Great Britain:

> I do not think that the question of a certified standard needs to be raised; not because a scientific standard is unimportant, but because men of science require moral and not legal evidence, and will be satisfied with the authority of any standard of which the derivation and subsequent custody are known from communications such as are usual between learned societies and men of science.[66]

In those words, he neatly summarizes the practices of nineteenth-century metrology at the frontier of precision.

This metrological system supported a vast endeavour of geodetic surveying, one that during the later nineteenth century covered all of Europe and much of North America in a network of triangles. At the same time new astronomical techniques of longitude determination, employing the electric telegraph, facilitated the establishment of a global web of longitude measurements. These two operations of measurement were complementary and of broadly similar precision. Uncertainties were of the order of a few metres for the determination of geodetic distances across countries, or of hundredths of a second for the determination of astronomical longitude by telegraph (the two linked mathematically by the figure of the Earth). For practitioners there was, in any event, only one kind of longitude. Accordingly, minor differences between geodetic and astronomical operations of measurement simply provided more data for refining the figure of the Earth, and its local physical and gravitational anomalies, to make those results coherent.[67]

The search for what Flamsteed called the 'true longitude and latitude' by astronomical means was, however, chimerical because – as a timekeeper – there is something wrong with the Earth. Its speed of rotation is gradually slowing and shows seasonal variations; the position of its rotational axis moves unpredictably; and changes deep within it alter the direction of the local gravitational field. All this became apparent during the twentieth century as photographic techniques of astronomical observation, and new electric and atomic clocks, offered ever-improving precision.[68] It had been implicit in the adoption of the Earth's rotation as the basis for our standard of time that it provided a linear flow – what we call Newtonian time – and that was now demonstrably not the case. Therefore, the unspoken assumption that the two operationally distinct kinds of longitude, as measured by *astronomical time* and by *distance*, were identical turned out to be incorrect. We are thus invited to choose between the two operations. Because it is only measurement of distance that gives us the desired invariability of outcome, we use it today – facilitated, of course, by modern methods of electromagnetic distance measurement, on land and from satellites, supported by atomic standards of time and length.[69] The observation by Flamsteed that introduced this chapter remains, however, as valid as it was three centuries ago.

Notes

1. Quoted in W. E. May, *A History of Marine Navigation* (Henley-on-Thames: Foulis, 1973), p. 29.
2. May, *A History of Marine Navigation*, p. 28.
3. M. Hotine, 'The Re-triangulation of Great Britain: IV – Base Measurement', *Empire Survey Review*, 34 (1939), 211–25 (p. 217).
4. I use the French term *étalon* to describe physical standards. It avoids an ambiguity in English by distinguishing between 'standards' as norms – regulatory or customary constructs, such as the imperial or metric systems – and as the practical representations of those norms.
5. Charles-Édouard Guillaume, *La convention du mètre et le Bureau international des poids et mesures* (Paris: Gauthier-Villars, 1902), p. 21.
6. Ken Alder, *The Measure of All Things* (London: Little, Brown, 2002), introduces the literature on the metric system. See also Michael Kershaw, 'The "Nec Plus Ultra" of Precision Measurement: Geodesy and the Forgotten Purpose of the Metre Convention', *Studies in the History and Philosophy of Science*, 43 (2012), 563–76, on the geodetic role of the metre.
7. When in 1960 the metre was re-defined in terms of the wavelength of light, the yard was defined in terms of the metre, and it ceased to have independent existence.
8. The terms 'network of trust' and 'metrological fallacy' are from Graeme Gooday, *The Morals of Measurement: Accuracy, Irony and Trust in Late Victorian Electrical Practice* (Cambridge: Cambridge University Press, 2004).
9. The measurement of the difference in astronomical time between two locations requires the establishment of simultaneity, by, for example, the observation of astronomical events, the carriage of timekeepers or the electric telegraph.
10. See Percy Bridgman, *The Logic of Modern Physics* (New York: MacMillan, 1927), on the concept of operationalism, which proposes that scientific concepts are defined by their operations of measurement and observation.
11. Louis Marquet, 'Le pendule à secondes et les étalons de longeur utilisés par l'expédition à l'Équateur: la Toise de Pérou', in *La figure de la terre du XVIIIe siècle a l'ère spatiale*, ed. by Henri Lacombe and Pierre Costabel (Paris: Gauthier-Villars Marquet, 1988), pp. 191–207.

12. Named after Jean-Charles de Borda – engineer, astronomer and Académicien. See Jean-Baptiste Delambre, *Base du système métrique décimal* (Paris: Baudouin, 1806–43), III, 311–36, for a full description.
13. See Harold Winterbotham, 'An Old File and a New Arc', *Empire Survey Review*, 7 (1933), 7–12, on the importance of the Borda rule.
14. See Ronald Zupko, *Revolution in Measurement: Western European Weights and Measures since the Age of Science* (Philadelphia: American Philosophical Society, 1990), pp. 58–104, on early British metrology.
15. George Shuckbergh Evelyn, 'An Account of Some Endeavours to Ascertain a Standard of Weight and Measure', *Philosophical Transactions*, 88 (1798), 133–82, and Henry Kater, 'An Account of the Comparison of Various British Standards of Linear Measure', *Philosophical Transactions*, 111 (1821), 75–94, describe such comparisons.
16. These are the scales of the Royal Society, General Roy, and the instrument makers Bird, Ramsden (in two forms), Cary and Aubert; see Kater, 'An Account of the Comparison'.
17. Kater, 'An Account of the Comparison', p. 92.
18. See Henry Kater, 'An Account of the Construction and Verification of a Copy of the Imperial Standard Yard Made for the Royal Society', *Philosophical Transactions*, 121 (1831), 345–47; Francis Baily, 'Report on the new Standard Scale of this Society', *Memoirs of the Royal Astronomical Society*, 9 (1836), 35–184; and William Yolland, *An Account of the Measurement of the Lough Foyle Base in Ireland* (London: Longman & Co., 1847), for descriptions of the standards and their multiple inter-comparisons.
19. G. B. Airy, 'Account of the Construction of the New National Standard of Length, and of Its Principal Copies', *Philosophical Transactions*, 147 (1857), 621–702.
20. Anon., 'An Account of the Proportions of the English and French Measures and Weights, from the Standards of the Same, Kept at the Royal Society', *Philosophical Transactions*, 42 (1742–43), 185–88 (p. 185).
21. Matthew Raper, 'An Enquiry into the Measure of the Roman Foot', *Philosophical Transactions*, 51 (1759–60), 774–823 (p. 778).
22. 'An Account of a Comparison Lately Made by Some Gentlemen of the Royal Society, of the Standard of a Yard, and the Several Weights Lately Made for Their Use; With the Original Standards', *Philosophical Transactions*, 42 (1742–43), 541–56 (p. 542).
23. Charles Mason and Jeremiah Dixon, 'Observations for Determining the Length of a Degree of Latitude in the Provinces of Maryland and Pennsylvania, in North America', *Philosophical Transactions*, 58 (1768), 274–328, describes the arc measurement and comparison of standards. It shows a new ratio between yard and half-toise of 1 to 1.06575.
24. See James Short, 'The Difference of Longitude between the Royal Observatories of Greenwich and Paris, Determined by the Observations of the Transits of Mercury over the Sun in the Years 1723, 1736, 1743, and 1753', *Philosophical Transactions*, 53 (1763), 158–69.
25. Jean-Dominique Cassini, Pierre Méchain and Adrien Marie Legendre, *Exposé des opérations faites en France en 1787 pour la jonction des observatoires de Paris et de Greenwich* (Paris: Imprimerie de l'Institution des Sourds-Muets, 1789), p. xiii.

26. See Jean-Pierre Martin and Anita McConnell, 'Joining the Observatories of Greenwich and Paris', *Notes and Records of the Royal Society*, 62 (1990), 355–72, on the project's genesis.
27. Sven Widmalm, 'Accuracy, Rhetoric and Technology: The Paris-Greenwich Triangulation, 1784–88', in *The Quantifying Spirit in the Eighteenth Century,* ed. by Tore Frangsmyr, J. L. Heilbron and Robin E. Rider (Berkeley: University of California Press, 1990), 179–207 (p. 193).
28. William Roy, 'An Account of the Trigonometrical Operation whereby the Distance between the Meridians of the Royal Observatories of Greenwich and Paris has been determined', *Philosophical Transactions*, 80 (1790), 111–270 (p. 113).
29. William Roy, 'An Account of the Measurement of a Base on Hounslow-Heath', *Philosophical Transactions*, 75 (1785), 385–480, and William Roy, 'An Account of the Trigonometrical Operation', describe the base measurement apparatus, its calibration and use.
30. Cassini de Thury, 'La Méridienne de l'Observatoire de Paris, Vérifiée dans toute l'étendue du Royaume par de nouvelles Observations', *Suite des mémoires de l'Académie royale des sciences, année 1740* (Paris: Guerin, 1744), 33.
31. Roy, 'An Account of the Trigonometrical Operation', p. 183; Cassini, 'La Meridienne de l'Observatoire de Paris', p. xvi.
32. Roy, 'An Account of the Measurement of a Base', Plate XI and R. de Prony, *Description des moyens employés pour mésurer la base de Hounslow-Heath dans la province de Middlesex, publiée dans le vol. LXXXV des Transactions Philosophiques [...] Traduit de l'Anglois par M. de Prony* (Paris: Didot, 1787).
33. Mason and Dixon, 'Observations for Determining the Length of a Degree of Latitude', postscript, explains that the toise obtained from Paris for comparison with the standard foot was 'exactly adjusted' to the length of the *toise de Pérou*, which was recently deduced directly from the *toise du Chatelet*.
34. See Victor Lenzen and Robert Multhauf, 'Paper 44: Development of Gravity Pendulums in the 19th Century', *United States National Museum Bulletin*, 240 (1964), 303–52.
35. Alder, *The Measure of All Things*, pp. 93–95; Airy, 'Account of the Construction of the New National Standard', p. 626. When the imperial yard *étalon* was destroyed, it proved impossible to recreate it with sufficient certainty using the pendulum definition.
36. The apparatus and its calibration are described in Delambre, *Base du système métrique décimal*, III, 337–401.
37. Jean-Baptiste Biot and Francois Arago, *Recueil d'observations géodésiques, astronomiques et physiques exécutees par ordre du Bureau des longitudes de France, en Espagne, en France, en Angleterre et en Écosse* (Paris: Courcier, 1821), pp. 441–49, describes the field pendulum apparatus and its calibration against a metre *étalon*.
38. Henry Kater, 'An Account of Experiments for Determining the Length of the Pendulum Vibrating Seconds in the Latitude of London', *Philosophical Transactions*, 108 (1818), 33–102, describes his techniques. He first established the absolute strength of gravity at London using a device known as the reversible compound pendulum. He then established relative strength of gravity between the field stations and London with a device known as an invariable pendulum, measuring its period (not length) at each location.

39. Biot and Arago, *Recueil d'observations géodésiques*, pp. 528, 539, 535.
40. Henry Kater, 'An Account of Experiments for Determining the Variation in the Length of the Pendulum Vibrating Seconds, at the Principal Stations of the Trigonometrical Survey of Great Britain', *Philosophical Transactions*,109 (1819), 337–508 (p. 426).
41. Biot and Arago, *Recueil d'observations géodésiques*, pp. 576, 581.
42. Kater, 'An Account of Experiments for Determining the Variation', p. 338.
43. Henry Kater, 'On the Length of the French Metre Estimated in Parts of the English Standard', *Philosophical Transactions*, 108 (1818), 103–09.
44. Biot and Arago, *Recueil d'observations géodésiques*, p. 582.
45. The two direct measurements were by Borda in 1792, and a later one by Biot and others. Data for the relative gravitational fields came from work by Biot at Greenwich in 1817 on his way back from Scotland, using a pendulum then also used in Paris. It was said that various experimental difficulties were 'beyond all doubt' responsible for discrepancies between observations. See Biot and Arago, *Recueil d'observations géodésiques*, pp. 585–88.
46. Edward Sabine, 'Experiments to Determine the Difference in the Length of the Seconds Pendulum in London and in Paris', *Philosophical Transactions*, 118 (1828), 35–77. Elimination of the discrepancies also needed a correction for Borda's result reflecting the fact that the measurements were made in the basement of the Observatory, two storeys below Biot's.
47. See Rebekah Higgitt and Richard Dunn, 'The Bureau and the Board – Change and Collaboration in the Final Decades of the British Board of Longitude', in *Le Bureau des longitudes (1795–1930): Context national et international*, ed. by Martina Schiavon and Laurent Rollet (Nancy: PUN-éditions universitaires de Lorraine, forthcoming), on this as a collaborative project.
48. See Ronald Zupko, *Revolution in Measurement: Western European Weights and Measures since the Age of Science*, (Philadelphia: The American Philosophical Society, 1990), Chapter 7, on the 'battle of the standards'.
49. Henry James, *Extension of the Triangulation of the Ordnance Survey into France and Belgium* (London: Ordnance Survey, 1863), describes the project.
50. A. R. Clarke, *Comparison of the Standards of Length of England, France, Belgium, Prussia, Russia, India and Australia* (London: HMSO, 1867), p. vi.
51. See Jean-Jacques Levallois, *Mesurer la terre: 300 ans de géodésie française* (Paris: Presses de l'école nationale des ponts et chaussées, 1988), p. 127, on the decline of French geodesy.
52. H. Levret, 'Jonction géodésique des triangulations de la France avec l'Angleterre et détermination de la différence en longitude entre les observatoires de Paris et de Greenwich', *Mémorial du dépôt général de la guerre IX, supplement* (Paris: Imprimerie impériale [1865]), is the French account
53. James, *Extension of the Triangulation*, Appendix.
54. Levret, 'Jonction géodésique des triangulations', p. 76; T. Pilkington White, *The Ordnance Survey of the United Kingdom* (Edinburgh and London: William Blackwood and Sons, 1886), p. 139.
55. James, *Extension of the Triangulation*, p. 55.
56. Levret, 'Jonction géodésique des triangulations', p. 57.
57. A. R. Clarke and Henry James, 'Abstract of the Results of the Comparisons of the Standards of Length of England, France, Belgium, Prussia, Russia, India,

Australia, Made at the Ordnance Survey Office, Southampton', *Philosophical Transactions*, 157 (1867), 161–80 (p. 161).

58. The indirect process of inter-comparison involved Ordnance Survey, Royal Astronomical Society, Royal Society and other standards. The sequence is set out in Henry James, 'On the Figure, Dimensions, and Mean Specific Gravity of the Earth, as Derived from the Ordnance Trigonometrical Survey of Great Britain and Ireland', *Philosophical Transactions*, 146 (1856), 607–26 (pp. 611–12). The result given equates one metre to 3.2808746 Ordnance feet, the log of which is consistent with the value of 0.515989635 in the British account of the cross-channel triangulation.
59. Clarke, *Comparison of the Standards of Length,* p. vii.
60. A. R. Clarke and Henry James, 'Results of the Comparisons of the Standards of Length of England, Austria, Spain, United States, Cape of Good Hope, and of a Second Russian Standard, Made at the Ordnance Survey Office, Southampton', *Philosophical Transactions*, 163 (1873), 445–69, covers the later comparisons.
61. See G. B. Airy, 'Figure of the Earth', *Encylopedia Metropolitana*, 5 (1845), 165–240 (p. 217).
62. A significant weakness was that the French did not permit Clarke to include the Borda rule in his comparisons; it was later found that there was a systematic error between the French toise and that used elsewhere in Europe, largely based on inaccurate copies made in the 1820s.
63. John Herschel, Letter to Prof. J. E. Hilgard, in 'Appendix 22. Report of a Conference on gravity determinations, held at Washington, D.C. in May 1882', *Report of the Superintendent of the U.S. Coast and Geodetic Survey 1882* (Washington: Government Printing Office, 1883), pp. 504–05.
64. The variations are significant, the smallest difference from the modern foot–metre conversion ratio being a few parts per million and the largest being nearly 20 parts per million.
65. See Gavin de Beer, *The Sciences Were Never at War* (London: Thomas Nelson and Sons, 1960); Robert Fox and Bernard Joly (eds), *Échanges franco-britanniques entre savants depuis le XVIIe siècle* (London: College Publications, 2010).
66. *Second Report of the Commissioners Appointed to Enquire into the Condition of the Exchequer (now Board of Trade) Standards. On the question of the Introduction of the Metric System of Weights and Measures into the United Kingdom* (London: HMSO, 1869), Appendix 1.
67. The *Comptes-rendus* of the *Conférence Générale de l'Association Géodésique Internationale* set out the growth of the geodetic and astronomical networks over time. A good example of the quantification and treatment of errors is Charles A. Schott, *Geodesy: The Transcontinental Triangulation and the American Arc of Parallel* (Washington: Government Office of Printing, 1900).
68. Tony Jones, *Splitting the Second* (Bristol: IOP, 2000), pp. 11–17, introduces what is wrong with the Earth as a timekeeper.
69. See Michael Kershaw, '20th century Longitude' (unpublished).

Part III

Voyages as Test Sites

9

Testing Longitude Methods in Mid-Eighteenth Century France

Danielle M. E. Fauque

Between 1767 and 1772, four voyages took place that tested the different methods for finding longitude at sea. Pierre Le Roy's watches were tested on the *Aurore* (1767) and *Enjouée* (1769), and Ferdinand Berthoud's clocks on the *Isis* in the same period (1768–69). On the *Flore* (1771–72), Berthoud's and Le Roy's timekeepers were tested concurrently. During these same voyages, octants and sextants revealed their qualities as the instruments best suited to the lunar distance method. The trials on the *Flore* effectively closed the quest for longitude at sea in France. The four expeditions were also representative of the period, and each showed progress regarding methods of control. From the amateurism exhibited on the *Aurore* to the rigorous methods practised with a team spirit on the *Flore*, the different elements of scientific expeditions and a learned navy came into place in France at the end of the eighteenth century.

The longitude question at the Académie royale des sciences

Although secondary literature on longitude at sea often focuses on the British Board of Longitude, the question was equally important in France from the second half of the seventeenth century, in particular through Christiaan Huygens's work on the use of pendulums and spiral springs for clocks. The results at sea, however, had been disappointing.[1] In Britain, the Longitude Act of 1714 gave the matter renewed prominence. This was true in France too, probably encouraged by the size of the British rewards. One initially promising idea was a prize of 100,000 livres proposed by the regent Philippe d'Orléans, largely in response to the many proposals received by the Conseil de Marine.[2] In the event, however, the money did not materialize. A more successful scheme arose from the comte Rouillé de Meslay's bequest to the Académie to create a biennial prize for contributions towards advances in knowledge, with special reference to navigation. Prizes for the determination of time at sea were awarded in 1720 (for a method using pendulum clocks), 1725 (for an essay on water clocks and hourglasses) and 1745, postponed until 1747 (for determination of time by astronomical observations). Timekeeping was not set again as a subject until 1765, however, although prizes were awarded for other innovations.[3]

French efforts in the first quarter of the eighteenth century focused on finding a mechanical solution for determining longitude. In 1716, Henry Sully, an English clockmaker working in France, submitted a clock that the Académie thought excellent.[4] Seven years later, he presented a clock that was tested at sea and found promising, and published an account in 1726.[5] Appointed *horloger* to the duc d'Orléans, he had worked with the celebrated clockmaker Julien Le Roy, whose ideas were taken up and continued by Berthoud and Pierre Le Roy, Julien's son. By the time he died in 1728, however, Sully's innovations had not been properly tested.[6]

The Académie received many other proposals, some independently of the Meslay prize, and these became more numerous after 1748. The academicians deputed to examine them were the astronomer Pierre Bouguer, an authority on navigation, Henri-Louis Duhamel de Monceau, a general inspector of the navy, the mathematician Alexis Clairaut, and Alexandre-Guy Pingré, an astronomer and navigator.[7] Through his work as an assessor, Bouguer was therefore responsible for examining all longitude proposals. Yet he rejected mechanical proposals, in particular clocks, because, he said, no perfect timekeeper yet existed and there was little prospect that one would ever be built. In his eyes, only astronomical methods might allow longitude to be determined precisely. His critical reports must have impeded French horological efforts.[8] The lunar method was also favoured by some naval officers in the 1750s, notably Jean-Baptiste d'Après de Mannevillette.[9] Yet the calculations were long and tedious. Nicolas-Louis de Lacaille, astronomer and friend of Bouguer, therefore devised a method that would be 'usable by the ordinary sailor', believing that lunar observations offered the only hope of a longitude method.[10]

French reactions to John Harrison's invention (1763–67)

Although some individuals in France had been working on the longitude problem, wider interest came with news of the first trials of John Harrison's sea watch (H4). In 1763, the French Ambassador in London wrote to the Ministre des Affaires étrangères, César-Gabriel de Choiseul, duc de Praslin, that H4 had been tested between Britain and Jamaica and that it was to be publicly examined to determine whether it deserved the 'prize' of '100,000 livres'.[11] Clockmakers and members of the scientific community wishing to attend were invited to apply to the Board of Longitude; the Ambassador recommended sending a savant and a skilled clockmaker.[12] The letter was passed to the Ministre de la Guerre et de la Marine and to the Académie royale des sciences, which decided that the mathematician Charles Camus should go to London with clockmaker Ferdinand Berthoud (1727–1807).[13] The Treaty of Paris also having brought the Seven Years' War to an end, news of H4 was spreading. The astronomer Jérôme de Lalande immediately left for London, where he was able to see H4, thanks to the intervention of Harrison's friend, James Short, and meet Nevil Maskelyne.[14] When Camus and Berthoud reached London some weeks later, however, Harrison refused to show them the sea watch, even though Lalande was with them.[15]

The Académie's choice of Berthoud acknowledged his status as one of France's leading clockmakers, with a known interest in marine horology, demonstrated in his *Essai sur l'horlogerie*.[16] Berthoud had lodged sealed letters concerning a marine clock with the Académie as early as 1754, then two in 1760.[17] These were opened formally in 1763, after Berthoud's return from London.[18] By 1764, he had finished two new clocks, designated numbers 2 and 3, the designs of which showed the influence of Harrison's ideas. Number 3 was tested off Brest.[19] Berthoud also recognized the commercial advantages of making links with the navy, and he was already working to develop good relations with the Ministre de la Marine.

Over the same period, another celebrated clockmaker, Pierre Le Roy (1712–85), was working on marine timepieces. Coming from a family well connected to the Académie, Le Roy had inherited his father's title of *Horloger du Roi*. Like Berthoud, he first declared an interest in marine timekeepers with a sealed letter to the Académie in 1754, opened in 1763.[20] The same year, he read a memorial on the marine watch that he deposited with the Académie in August 1764.[21] The Académie therefore decided that a committee appointed to examine Berthoud's instruments should report on Le Roy's as well.[22]

Thus, longitude was high on the Académie's agenda by 1765, and further news came from Britain on 23 February, when the astronomer Pierre-Charles Le Monnier described H4's Barbados trial.[23] Within two months, the Académie announced that its next Meslay prize would be, 'On the best method of determining time at sea', with 2,000 livres to be awarded after sea trials.[24] Towards the end of 1765, the Ministère de la Marine authorized Berthoud to make a second visit to London.[25] Back in Paris in mid-March, he reported that an unnamed commissioner (Thomas Mudge) had described Harrison's mechanism in enough detail to understand its intricacies, although making a copy would be challenging, given the lack of skills in France. Berthoud therefore asked for a pension of 3,000 livres to devote himself to marine clocks.[26] He later modified his request and in March 1766 submitted a plan of work to Choiseul, which he later sent to the duc de Praslin, by then Ministre de la Marine. The king promised 9,600 livres for two clocks, to be paid as work progressed and subject to trials.[27] Numbered 6 and 8, these were tested on the voyages described below. At the time, it seems, the judgement of the Académie counted for less than Berthoud's contract with the Ministre de la Marine.

Meanwhile, Le Roy was working on a marine watch for the same competition.[28] With entrants required to submit their timekeepers and documents before the Académie's annual closure on 5 September, Fouchy was sent to discover Berthoud's intentions.[29] Berthoud felt, however, that he was not ready.[30] Meanwhile, Le Monnier reported that Le Roy had submitted a watch suited to extreme climates. 'When the prizes are announced at the public session after Easter,' he wrote, 'we shall express the praise that the marine watch of the son of the late M. Julien Le Roy merits. But we cannot award the prize for this excellent watch, since we do not know how it will function at sea'.[31] He added that the watch should be tested in all conditions, as the Marquis de Courtanvaux intended. In the light of Maskelyne's criticisms of H4 following further tests at the Royal Observatory, and the resulting disputes, French observers also concluded that H4's sea trials had been in undemanding climates.[32] Theirs should take place in more extreme conditions.

Berthoud and Le Roy had been in close competition, therefore, from the moment they almost simultaneously submitted memorials and drawings. Henceforth their paths diverged. Berthoud courted the Ministère de la Marine; Le Roy felt he could count on the academic community of which he was an insider, something Le Monnier recognized in stressing Le Roy's family background over his horological standing. Tellingly, Berthoud had struggled for acceptance into the Parisian corporation of clockmakers since his arrival from Switzerland 20 years earlier.[33] Other significant figures also appeared on the scene in 1766 and 1767. New instruments for the lunar distance method were presented: a binocular telescope by the abbé Alexis de Rochon; and the '*mégamètre*' devised by the chevalier Charles-François de Charnières.[34] Another key figure would be the naval ensign Charles Claret d'Eveux de Fleurieu, who was familiar with Berthoud's clocks.

Four test voyages

Between 1767 and 1772, four voyages tested the timekeepers alongside other methods for finding longitude at sea, and were sources of recurring tensions between the Ministère and the Académie.[35] Le Roy's sea clocks were tested on the *Aurore* (1767) and *Enjouée* (1768), each at the Académie's request for Meslay prizes, while Berthoud's clocks underwent trial on the *Isis* (1768–69) under the supervision of the Ministère de la Marine. The fourth voyage, on the *Flore*, was different. Frustrated by Le Roy's and Berthoud's competing claims, the king (through his minister) decreed that the rival timekeepers should be tested simultaneously.[36] The Ministère de la Marine and the Académie therefore mounted the *Flore* voyage jointly.

The *Aurore* (1767)

By September 1766, of the clocks and other instruments put forward to compete, only Le Roy's watch had proved satisfactory, following tests on land and a river vessel. The conditions of the Meslay prize required a sea trial, however, and the marquis de Courtanvaux, François César Le Tellier de Louvois, an honorary academician, offered to fit out a ship at his own expense. As he observed of H4, its error was measured only at the end of its sea trial, with no account of variations during the crossing. It was conceivable that it was the net result of irregularities that in part had compensated for one another. Courtanvaux argued, therefore, that the watch's performance should be checked at intermediate intervals. To that end, he paid for the construction in Le Havre of a light, fast vessel with a shallow draft that could enter small ports. Designed by Nicolas-Marie Ozanne, the luxurious 66-foot (22.4m) corvette with a crew of 22 was named *Aurore* (Figure 9.1) and accorded the status of royal frigate.

Courtanvaux was accompanied by Pingré, Le Roy and the astronomer Charles Messier.[37] Le Roy left for Le Havre with two watches: the one demonstrated before the king on 5 August 1766 (later designated 'A') and a new watch ('S'), although only watch A was considered for the Meslay prize. Instruments for testing the watches (transit instrument, quadrant, astronomical pendulum and telescope) were also taken, as well as compasses, an octant, barometers and Charnières's *mégamètre*. Observations began after Le Roy handed over his watch on 15 May 1767, and the *Aurore* left Le Havre on 25 May, intending to sail to St Petersburg via Amsterdam, with intermediate stops to check the watch's performance on land. In the event, however, poor conditions meant that they did not reach Amsterdam until 11 July, too late for a voyage to the Baltic

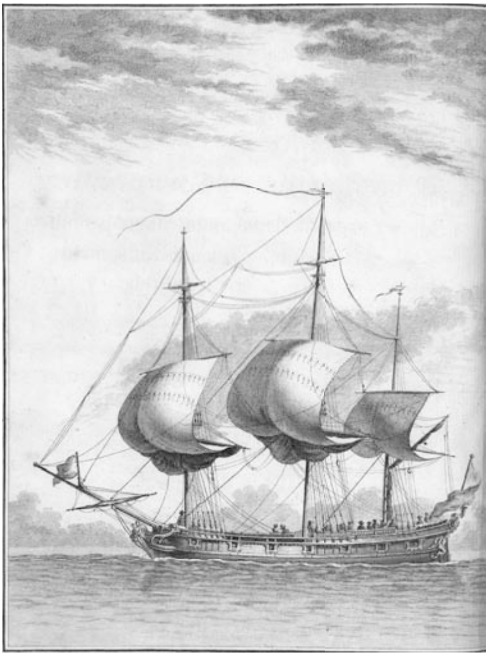

Figure 9.1 *L'Aurore*, from François César Le Tellier, marquis de Courtanvaux, *Journal du voyage de M. le Marquis de Courtanvaux*, ed. by Alexandre Guy Pingré (Paris: Imprimerie royale, 1768) © National Maritime Museum, Greenwich

that could return before winter. The *Aurore* left Amsterdam on 22 July and arrived back in Le Havre on 28 August.

At each stop, A was found to be fast. Le Roy attributed this to the lengthening of a metal harpsichord string used in the movement, which had broken before reaching Le Havre and been repaired as best Le Roy was able. Nevertheless, the watch settled and the acceleration diminished. S worked better, maintaining a satisfactory rate. The *Aurore*'s lightness meant that it rolled badly, disturbing the watches' performances. The results at the end of each leg showed, however, that the overall error for the 98-day journey had nothing to do with compensating errors, as had been suggested of H4. Still, the error of 51 seconds for watch A was too large for the prize to be awarded, although Le Roy considered the voyage a valuable opportunity for improving his watches. Tests of the *mégamètre* yielded no significant results.

Courtanvaux told the Ministre de la Marine that a longer voyage would be necessary for the 1769 Meslay prize. In December 1767, he submitted a plan, which was discussed and sent to the Ministre with the Académie's endorsement.[38] The plan was to leave Le Havre in early May for the Canaries, stopping in Cádiz. Competitors would be required to present their instruments for prior examination by 16 January. The *Aurore* voyage was already facing criticism, however, for being too short and the tests of the watches insufficiently rigorous. Stays had been lengthy wherever the *Aurore* anchored, giving leading citizens and the idly curious the opportunity of visiting the ship, while Courtanvaux usually went ashore. Having disembarked at Rotterdam, he had even made his way separately by canal to Amsterdam, where he re-joined the ship, leaving Le Roy and Pingré on board to continue their tests. The expedition, it seemed, was more a tour punctuated by social engagements than a scientific expedition.[39] The second expedition was organized without Courtanvaux.[40]

The *Enjouée* (1768)

Although Courtanvaux took his plan for a second voyage no further, the Ministre de la Marine did pursue the matter. Praslin proposed that Le Roy's and Berthoud's timepieces be tested on the same vessel, and instructed the Académie to plan accordingly.[41] But relations between the clockmakers deteriorated and neither was willing to accept the proposal.[42] The Académie noted that no clock had yet been lodged with it, the closing date for the receipt of instruments being 6 September 1768.[43] On 30 April 1768, Duhamel repeated the case for systematic trials. His letter to the Ministre reiterated that Le Roy's and Berthoud's timekeepers should be tested together on the same vessel, but also noted Berthoud's refusal to deposit his clocks with the Académie and the mistrust this revealed, despite assurances.[44]

Meanwhile, an unexpected opportunity to test Le Roy's watches presented itself. A newly built frigate, the *Enjouée*, was being outfitted in Le Havre on the Ministre's orders for a voyage to the island of Saint-Pierre.[45] Its commander, Jean-Baptiste Lollivier de Tronjoly, had been on several missions to Newfoundland since the end of the Seven Years' War. Following the 1763 Treaty of Paris, Britain had allowed French fishermen to use the island group of Saint-Pierre et Miquelon, and Canadians who refused to swear allegiance to the British crown had taken refuge there. Tronjoly, well known for his diplomatic skills, was given responsibility for conveying the Canadians to France. He was also to help end the ongoing skirmishes between British and French fishermen, in discussion with the British governor of Newfoundland. The trial of two of Le Roy's marine watches was now designated the expedition's official purpose, although it had clear diplomatic and commercial objectives. It was also to test other new inventions: Pierre Isaac Poissonnier's *cucurbite* for desalinating seawater; tablets for broth to be served to the sick; and a new speed log by a former merchant seaman named Le Valois.[46] The *Enjouée* was to sail for Saint-Pierre at the end of the month, then go directly to Sala in Morocco, which had just signed a peace treaty with France, and on to Cádiz and Lisbon to purchase gold and silver, before returning to Brest.[47]

On 18 May, Jean-Dominique Cassini (Cassini *fils*) described a plan for testing the watches to the Académie; it was sent to the Ministre de la Marine the following day.[48] Cassini *fils* was still not 20 and yet to be elected to the Académie, but he was well known to the academicians. Johann Wilhelm Wallot, a young German astronomer, assisted him on the voyage. Poissonnier, Le Roy, and César François Cassini de Thury, with his wife, niece and son, arrived in Le Havre on 23 May. Observations began that day under the supervision of Cassini de Thury, who made a number of recommendations, having been assured that the ship's officers would assist his son.[49] On 30 May, the watches were taken on board, labelled and installed in a cupboard. A report was signed and keys were entrusted to Cassini *fils* and Tronjoly.[50]

At this stage, the winds became unfavourable, and the frigate could not sail until 13 June. Once at sea, the crossing proceeded without incident to Saint-Pierre. Believing that something was wrong with one of his clocks, Le Roy asked to open it. The clock appeared to be functioning normally.[51] Moreover, since the longitude of Saint-Pierre had not been precisely determined, no definitive check of the watches was possible.

The vessel left Saint-Pierre on 3 August, reaching Sala in Morocco on 26 August. There the French were well received, but they were not allowed to disembark to make observations. They prepared to leave again quickly, but further unfavourable winds forced them to anchor until 10 September. By 13 September, they were in Cádiz, where they took the watches to the naval observatory to check their performance. The time lost meant that they could not go to Lisbon, so they sailed directly to Brest, arriving on 30 October. The watches were immediately taken ashore for further tests.

Cassini *fils* had designated the watches as A ('ancienne') and S ('seconde') and checked their rates daily.[52] At Le Havre, it was observed that A was running slow, S fast. At Saint-Pierre, S was running even faster, while A was less slow. By Cádiz, both were running ahead of mean time. After several more days, characterized by unusually high temperatures, both were abnormally fast, although this became less marked. In the course of the voyage, the ship anchored three times (at Saint-Pierre, Sala, and Cádiz), but a comparison between the watches and an astronomical pendulum was only possible at Le Havre, Cádiz and Brest, ports whose exact longitude was known. The watches had endured five months at sea; they had been taken off the ship twice and were subjected to damp and extremes of cold and heat. When compared with local time in Brest, they were found to be out by 9½ seconds (A) and 1 minute 35 and 55 sixtieths of a second (S). Both watches had fulfilled the prize conditions, although the irregularities at Cádiz showed that imperfections remained.[53] The Académie gave its endorsement and awarded Le Roy a double prize (combining those for 1767 and 1769) of 4000 livres, adding that the voyage had allowed him a better understanding of the irregularities and that he would be in a position to correct them.

In January 1769, Cassini delivered his report to the Académie with papers on Le Valois's log and the best method of determining time at sea for checking watches. The chevalier Gabriel de Bory (a naval officer) and the astronomer Jean-Sylvain Bailly read a report on the second paper on 4 March. Cassini had used the hour-angle method to determine the true time and hence the longitude. Recognizing that the method was beyond ordinary seamen, he began to draw up tables for all latitudes and every degree of solar declination. The tables, which were published in his voyage account in 1770, gave the hour angle corresponding to the Sun's observed altitude.[54] The account also contained the memorial Le Roy submitted for the competition.[55] Drawing on what he had learned on the voyage, Le Roy continued to work and present his results to the Académie.

As Tronjoly's report reveals, he was less enthusiastic. Writing from Cádiz on 16 September 1768, he commented that while using the watches required only calculations that anyone could master, one watch had not worked properly and he feared the other might malfunction. By contrast, he found the dried stock tablets and the *cucurbite* quite successful; the crew preferred the desalinated water, as did Cassini. Le Valois's log, however, had broken and could not be repaired.

Tronjoly's report also reveals the extent to which testing the watches served as a cover for political and commercial objectives. The diplomatic purpose was to check that the terms of specific treaties were being observed; the commercial aim was to purchase South American gold and silver in Cádiz. Contacts with the British in Newfoundland had gone well. In Sala, the governor supplied them with food, and the terms and spirit of the treaty had been fulfilled. In Cádiz, there was time to load gold and silver destined for Paris. Tronjoly added that traders generally preferred doing business with the British than with the French.[56] Thus, the *Enjouée* followed a route that had more to do with politics and commerce than with scientific requirements. This was not the case for two subsequent voyages, which were conceived explicitly as scientific expeditions.

The *Isis* (1768–69)

In 1766 the duc de Praslin had visited Brest and made it a focus for improvements, ordering expeditions to correct hydrographic charts and test marine timekeepers. This was part of a drive to compete with Britain, as were other innovatory policies including the resurrection of the Académie de marine, which received the title 'royale'.[57] Even so, Praslin needed some convincing of the benefits of seconding the 30-year-old chevalier de Fleurieu, then serving as an officer at Toulon, to work with Berthoud on clocks 6 and 8. Indeed, Fleurieu had to remind Praslin that his predecessor as Ministre de la Marine had authorized just such an appointment.[58]

Before his election as an honorary academician in 1769, Praslin had close, though strained, relations with the Académie. As the official source of expertise, the Académie was crucial for anyone seeking approval for an invention or other scientific project. It was natural, therefore, that Praslin should approach the Académie to assess memorials he received. Correspondence between Praslin and the Académie reveals how difficult these consultations could be, particularly as disagreements between the Berthoud and Le Roy camps escalated. Fleurieu, a champion for Berthoud, added to the tension, no doubt explaining the delays and poor personal relations during the preparations for sea trials of Berthoud's clocks.[59]

Fleurieu submitted a plan to the Ministre de la Marine in July 1767, which was sent to the Académie for comment on 8 August. After a reminder, the Académie replied that dealing with the lengthy document would take time.[60] On 15 November, following a request from the Académie's committee, Fleurieu added a supplement describing the procedure for testing the clocks, which was approved on 9 December. He intended, he said, to go beyond the original plan; he now hoped to examine procedures for determining longitude both at sea and at the ports where the ship anchored. With that in mind, he asked for a Charnières *mégamètre* and a Rochon binocular telescope. He also planned to make observations of magnetic variation with an azimuth compass and of the ship's speed with a log designed by Bouguer, with the time being measured by a Berthoud pendulum clock instead of a sandglass.[61] Dead reckoning was so approximate, and often wrong, that it could not be relied upon; yet all astronomical practices involved its use to some degree. Mechanical methods might therefore be preferable for the levels of accuracy sailors needed. Finally, he intended to improve existing charts by surveying the coastline and recording currents. What Fleurieu was proposing was a scientific expedition in the Enlightenment manner.

Fleurieu intended to use lunar distances, although he acknowledged that the calculations would be lengthy. Hence, he suggested, it would be desirable for the *Connaissance des temps* to publish tables of the distance between the Moon and zodiacal stars at three-hour intervals, as Nevil Maskelyne had in the *Nautical Almanac*. As Fleurieu observed, on his voyage to St Helena Maskelyne had used a method originally suggested by Lacaille. Fleurieu invoked the authority of savants such as Bouguer and Lacaille, and had no hesitation in mentioning a British authority, Maskelyne. Finally, he insisted that all officers were to take part in the experiments. Thus, the *Isis* expedition was to be undertaken with goals extending beyond testing timekeepers, which were rather to be used routinely as instruments whose efficacy was not in question.

Once the plan was approved, Praslin asked the Académie to appoint an astronomer. Lalande declined, but Pingré accepted: 'he likes travelling', as Lalande put it.[62] But problems soon arose. Fleurieu's published response to a memorial by Le Roy was so fierce that the Ministre de la Marine asked him to destroy it.[63] Then Praslin refused to authorize the purchase of new instruments. Charnières added to the difficulties by asking to be put in charge of the expedition, even though the command had been promised to Fleurieu, who could not accept this humiliation and threatened to resign from the navy. This would have put the venture at risk, since Charnières had no experience of clocks. After weeks of discussion, Fleurieu was reinstated as commander.[64]

The vessel originally intended for the voyage was a corvette, the *Ambition*. But this was considered too small and was replaced with a frigate, the *Isis*, which reached Rochefort in mid-November.[65] The adjustment of clocks 6 and 8, which arrived on 3 November, was delayed by bad weather, and it was not until 7 December that they were ready to be taken on board. Fleurieu received his official orders at the end of November. These were brief. He was free to plan the voyage as he thought best, and was assured of the king's and thus the Ministre's complete confidence.[66] In addition to the clocks, Charnières's *mégamètre* and Rochon's telescope, the scientific equipment comprised a 6-inch achromatic telescope by Ramsden, octants (some British) and instruments for terrestrial observations (including an astronomical clock and quadrant). The *Isis* set sail the following day but was obliged to remain in harbour off the Île d'Aix until February because of bad weather, although it was possible to practise the procedures for testing the clocks.

They were able to depart for Cádiz on 12 February 1769 and the voyage proceeded smoothly. The plan was to sail to the Île d'Aix, then on, notably, to Cádiz, the Cape Verde Islands, Saint-Domingue, Newfoundland, the Azores, the Canary Isles, Cádiz again, and finally back to the Île d'Aix on 31 October 1769. On 3 November, the final observations with the clocks were completed, and Fleurieu reported to the Ministre de la Marine on the admirable conduct of the officers and cadets, all of whom had shown enthusiasm and competence in making observations.[67]

The clocks had performed impressively between 10 November 1768 and 21 November 1769, often giving longitudes to within half a degree. They had kept good time, especially number 8, which had performed exceptionally. The clocks were never moved and their performance was invariably checked by two observers working independently, with the officer of the watch as witness. For trials at ports, contact was established by signal between ship and observatory on land, with a signed entry in the register each time. Fourteen such checks were made, allowing the compensating errors and rate of each watch to be precisely determined. An added advantage of the clocks was that they had allowed d'Après de Mannevillette's hydrographic charts to be checked and corrected. Fleurieu insisted that the octant was the only instrument a sailor needed for daily altitude measurements. Indeed, octant observations had generally been accurate enough for testing the clocks as well, making it unnecessary to fall back on more complex procedures using an astronomical quadrant. The *mégamètre* and Rochon's telescope, however, had not lived up to expectations.

On 21 February 1770, after several weeks of hesitation and internal disagreement, the commissioners appointed by the Académie approved Fleurieu's thorough report, and Pingré gave a lecture on the expedition to the Académie on 25 April.[68] In Berthoud's eyes, the expedition had been an unqualified success, and on 1 April, Louis XV appointed him clockmaker (*horloger mécanicien*) to the king and the navy with responsibility for inspecting timepieces in all French ports. On 14 August, Berthoud undertook to supply 20 marine clocks.[69] On the Ministre de la Marine's orders, he passed number 3 to the marquis Joseph-Bernard de Chabert for his hydrographic expedition to the Mediterranean, and number 6 to Rochon, who was about to embark with Kerguelen in search of a faster route to the East Indies. Number 8 was installed on the *Flore* the following year.

Fleurieu's report, which he wrote primarily for young naval officers and with the obvious purpose of avoiding potential criticism, was published in 1773 as a comprehensive treatise of navigation, not simply a voyage account.[70] A lengthy introduction outlined the plan, methods and results. In the first section, 'Journal des horloges marines', Fleurieu described the expedition's main purpose. He followed this with a 'Journal de navigation', which emphasized the value of marine clocks for navigation and geography. The final part of the first volume contained Fleurieu's criticisms of existing charts and proposed new charts for the Atlantic. The second volume was devoted to a 'Recueil des observations astronomiques' and related tables. Finally, there was an appendix of 'Instructions' on the use of marine timekeepers for determining longitude, along with illustrative calculations, ways of finding latitude and tables. These would prove useful on later hydrographic expeditions and voyages of discovery.[71] The expedition marked the beginning of a new age of navigational techniques, one that called for better-trained seamen.[72] Over just two years, there had been a transition from a traditional sea voyage, with the *Aurore*, to a scientific one, with the *Isis*. On a broader front, the voyage paved the way for the modernization of the navy that would take place after 1774.

The *Flore* (1771–72)

It was agreed that a new expedition would set out in the autumn of 1770. Reporting on Fleurieu's memorial, the Académie committee suggested rigorous testing of Charnières's *mégamètre* and Rochon's telescope on the new expedition.[73] On 7 March 1770, academicians Jean-Charles de

Borda, Bory and Pingré drew up a programme for the voyage, which was approved on 14 and 24 March.[74] On 15 August, the Ministre de la Marine put back the expedition's departure to the spring of 1771.[75] Praslin and his cousin Choiseul had, however, been removed from their posts on 24 December 1770, after a disagreement with the king over foreign policy. On 29 January, Fouchy requested that on the new voyage, the acting minister and general controller of finance, the abbé Joseph-Marie Terray, should arrange tests to compare all methods for determining longitude.[76] But preparations for such tests inevitably took longer than originally planned, and the departure was again postponed, this time to the autumn.[77]

The commander of the *Flore*, an experienced seaman named Jean René de Verdun de la Crenne, was to work alongside two academicians appointed to check the clocks: Borda, an engineer trained at the École du génie in Mézières, and Pingré, for whom this would be the third such voyage. Pingré was to be assisted by Jean-Michel Tabary, known as Mersais, a pupil of Lalande.[78] The team also included a draughtsman, Pierre Ozanne.[79] The expedition had two distinct objectives. The first was to complete the work requested by the Académie to check the instruments being considered for the Meslay prize. These included Le Roy's watches A and S; a watch by a maker called Arsandeaux, which proved to work very irregularly; a pendulum clock by Joannes Biesta that was broken from the start of the expedition; and a marine chair by Fyot that turned out to be unusable. Le Roy also sent a new pocket watch (*petite ronde*), although not for competition.[80] The second objective was conveyed in the Ministre's request that 'all aspects of the great problem of longitude' should be considered. He ordered the committee to compare all the recently proposed instruments, as well as the methods navigators were already using. Every officer on board was to take part. The instruments taken included Berthoud's clock number 8, the *mégamètre*, Rochon's telescope (which performed disappointingly), octants and sextants, and instruments for land-based observations (two seconds pendulums, three astronomical quadrants, a transit instrument and several telescopes).

The king's instructions allowed the committee to dictate the mission's specifics. The expedition left Brest for Cádiz, sailed – among other places – to Cape Verde and Martinique, before reaching Saint-Domingue and Newfoundland, then returned to Brest via Denmark. In Antigua, the vessel narrowly avoided running aground, and during the resulting repairs (in Martinique), watch A was damaged while entering dry dock. The clocks were never moved, even during this operation. Otherwise, the voyage proceeded uneventfully. An interim report was sent to the Ministre de la Marine at each stopping point. On 29 October 1772, Verdun de la Crenne submitted an account of the whole mission.[81]

On behalf of the committee, Pingré read a memorial on the trials at the public meeting of the Académie on 21 April 1773.[82] Before hearing the report, the Académie presented the double prize to Le Roy, with an award also going to Arsandeaux as the *accessit*.[83] The procedures for checking the watches were simpler but more rigorous than on the *Isis*. The ear was more important than eye on the *Flore*, as Jean Mascart has put it.[84] Communication was by pistol shot. The observer overseeing the watches on the ship recorded the time of each shot, while an observer on land recorded the time of the flash seen through a telescope, with the mean calculated from five signals. The timekeepers' performance was measured over six-week intervals, following the method Maskelyne had employed in testing H4 at the Royal Observatory. Timekeepers 8, A and S gave the longitude more precisely than was expected, often to within than half a degree. The results with the *mégamètre*, however, were not precise, due to a fault in the instrument, although it still seemed sufficiently promising to justify further work.

The octant and sextant emerged as the most reliable observing instruments. They met the astronomical needs of navigators for latitude, and served well for determining local time and longitude by lunar distance. The main instrument used for lunar distances belonged to an officer named Laub: a 15-inch, English brass sextant, incorporating telescope and screw adjustment for the vernier. Another English sextant, in wood, and two octants were also used, with altitude readings communicated to Borda in confidence. Generally, these readings agreed to within one minute of arc. For longitude, three observers took simultaneous readings of the altitude of the Moon and Sun or appropriate star, and the distance between the Moon and the Sun or star. The vessel's longitude was calculated according to a procedure devised by Borda, which was easier and more direct than that of the *Nautical Almanac*: officers, pilots and helmsmen were said to be able to master it without undue difficulty.[85]

Verdun de La Crenne, Borda and Pingré drew a simple conclusion: the octant and sextant were the only instruments to be used for astronomical observations at sea, and only the lunar distance method was practicable. The time at the port of departure, however, could only be determined with accurate marine watches, usually two of them. In this way, daily observations (mechanical and astronomical) could be collated to determine longitude. By comparison, dead reckoning with log and compass gave at best a rough measure. Of the remaining possibilities, magnetic variation was too irregular to be dependable, the eclipses of Jupiter's satellites too difficult to observe at sea, and other eclipses too infrequent to be useful. Although longitude determination with a watch was the quickest and simplest method, the watch's mechanism was too easily disturbed. As they concluded, 'A good sextant will occasion no such fears', and it could be used at any time: 'L'usage du sextant est de tous les jours'.[86]

The use of marine timekeepers for preparing hydrographic charts, the other aspect of the expedition's work, received unqualified endorsement. For mapping coastlines, watches and sextants had to be used together. Headlands and key coastal features passed during the voyage were surveyed, with the *Flore* commissioned to undertake a special cruise for this purpose in the West Indies. For such hydrographical work, observations were taken every day and as frequently as possible, with a marked gain in efficiency. It took years of hard work to make use of all the information gathered on the *Flore*, particularly for producing definitive charts. The fruits were finally published in 1778, along with two revised maps of the Atlantic and 148 coastal profiles.[87]

Conclusion

Following the Seven Years' War, French work on the determination of longitude took place in a world marked by the success of H4. In France two clockmakers of distinction emerged, Pierre Le Roy and Ferdinand Berthoud. But another consequence of these voyages was the establishment of a new approach to marine surveying, characterized by on-board tests by well-trained observers following rigorous procedures. In France, the use of watches for hydrographic surveys went hand in hand with the increasing use of the octant and sextant, the instruments employed often being of British manufacture. This also marked the end of the use of the backstaff.[88] On the *Flore*, Borda, an engineer by training who was new to navigation, pioneered a simpler method for determining longitude by lunar distances. On the same voyage, he had the idea of improving the accuracy of the octant and sextant by incorporating the principle of Tobias Mayer's repeating circle.[89] His reflecting circle was ready for use by 1775 and was tried successfully at sea the following year.

The voyage of the *Flore* also marked the end of the dominance of the Académie des sciences over the Ministère de la Marine with regard to memorials and inventions in navigation. The Académie did not set a specifically maritime topic for a prize competition again, and Pingré's account of the voyage of the *Flore* in the Académie's annual publication for 1777 seems to have been the last communication on the subject. The

Ministère, however, continued to receive suggestions regarding longitude. In some respects, the Académie royale de marine filled the gap in expertise by evaluating proposals that came its way.[90]

In French maritime affairs, the subsequent few years of peace marked a turning point in the passage from a traditional navy to one based more heavily on science, a process that began with Choiseul's reforms in 1765 and accelerated from 1774 under Louis XVI. The innovative *Isis* voyage and the emblematic *Flore* voyage have therefore to be seen as first steps towards something of a golden age in French scientific navigation, which was to find its greatest expression in the voyage of Jean-François de La Pérouse.

Notes

1. John H. Leopold, 'The Longitude Timekeepers of Christiaan Huygens', in *The Quest for Longitude,* ed. by William J. H. Andrewes (Cambridge, MA: Harvard University, 1996), pp. 101–14; Fabien Chareix, 'Vaincre la houle: les horloges marines de Christiaan Huygens', in *Le Calcul des longitudes*, ed. by Vincent Jullien, (Rennes: Presses Universitaires de Rennes, 2002), pp. 169–202.
2. Registre manuscrit de l'Académie royale des sciences (hereafter RMARS), 21 March 1716, p. 99, regent's letter (15 March) to abbé Jean-Paul Bignon, President of the Académie; quoted in Ernest Maindron (ed.), *Les Fondations de prix à l'Académie des sciences. Les lauréats de l'Académie, 1714–1880* (Paris: Gauthier-Villars, 1881), p. 23. The Conseil de marine was created on Louis XIV's death in 1715 since Louis XV was a minor. It ceased in 1723, when a Secrétaire d'État à la Marine, called the Ministre de la Marine, was appointed.
3. Maindron (ed.), *Les Fondations*, pp. 17–22.
4. RMARS, 20 May 1716, p. 157; 13 June, pp. 185^{v}–6, report.
5. RMARS, 17 April 1723, p. 71. RMARS, 11 March 1724, p. 99, report: 'cette horloge mérite qu'on en fasse des essais à la mer'.
6. Henry Sully, *Description abrégée d'un horloge d'une nouvelle invention, pour la juste mesure du temps sur mer* (Paris: Briasson, 1726).
7. Danielle Fauque, 'Introduction: Pierre Bouguer, figure emblématique ou savant singulier?' in *Pierre Bouguer (1698–1758), un savant et la marine dans la première moitié du XVIII^{e} siècle* (=*Revue d'histoire des sciences*, 63 (2010)), 5–21.
8. Guy Boistel, 'Pierre Bouguer, commissaire pour la marine et expert pour les longitudes', in *Pierre Bouguer*, pp. 121–59 (pp. 141, 149–51); Guy Boistel, 'L'astronomie nautique au XVIIIe siècle en France: tables de la Lune et longitudes en mer' (Thèse de doctorat, Université de Nantes, 2001), pp. 322–29.
9. D. Fauque, 'The Introduction of the Octant in Eighteenth-Century France', in *Koersvast. Vijf eeuwen navigatie op zee*, ed. by Remmelt Daalder, Frits Loomeijer, Diederick Wildeman and Leo Akveld (Zaltbommel: Aprilis, 2005), pp. 95–104 (pp. 99–101).
10. Jérôme Lalande, *Connaissance des temps pour 1760* (Paris: Imprimerie royale, 1759), pp. 205–11; see also Guy Boistel's chapter in this volume.
11. The maximum reward under the 1714 Longitude Act was £20,000.

12. Archives nationales de France, fonds marine (hereafter AN, FM), G98, fol. 4, Duc de Nivernois to the duc de Praslin, London, 21 March 1763.
13. AN, FM, G98, fol. 5, Duc de Praslin to the duc de Choiseul, 28 March 1763; AN, FM, G98, fol. 6, Jean-Paul Grandjean de Fouchy, permanent secretary (secrétaire perpétuel) of the Académie des sciences (hereafter Académie) to the Ministre de la Marine (hereafter Ministre), 4 April 1763.
14. Jérôme Lalande, *Journal d'un voyage en Angleterre 1763* (Oxford: Voltaire Foundation, 1980).
15. Danielle Fauque, 'La correspondance Jérôme Lalande et Nevil Maskelyne: un exemple de collaboration internationale au XVIII^{e} siècle', in *Jérôme Lalande (1732–1807). Une trajectoire scientifique*, ed. by Guy Boistel, Jérôme Lamy and Colette Lelay (Rennes: PUR, 2010), pp. 109–28 (pp. 110–12).
16. Ferdinand Berthoud, *Essai sur l'horlogerie, dans lequel on traite de cet art relativement à l'usage civil, à l'astronomie et à la navigation, en établissant des principes confirmés par l'expérience* (Paris: Jombert, 1763).
17. The Académie's president opened the letter in 1976. See Catherine Cardinal, 'Biographie', in *Ferdinand Berthoud 1727–1807, horloger mécanicien du roi et de la marine*, ed. by C. Cardinal (La Chaux de fonds, Suisse: Musée international d'horlogerie, 1984), pp. 19–55 (p. 24).
18. RMARS, 28 February 1761, p. 53^{v}; 20 June 1764, pp. 221^{v}–27^{v}, report on Berthoud's clock.
19. RMARS, 8 August 1764, p. 325^{v}; 11 August 1764, pp. 326^{v}–27; 14 August 1764, p. 328; 18 August 1764, p. 328^{v}; 29 August 1764, pp. 347–49^{v}; Cardinal, 'Biographie', p. 26.
20. RMARS, 28 June 1763, p. 253.
21. RMARS, 7 December 1763, p. 365; 10 December 1763, p. 365^{v}; 14 August 1764, p. 327^{v}; 18 August 1764, p. 328^{v}. Le Roy used the word 'montre' (watch) for his timekeepers, Berthoud the word 'horloge' (clock). These conventions have been followed.
22. RMARS, 18 August 1764, p. 329.
23. RMARS, 23 February 1765, p. 115^{v}; see also AN, FM, G98, 9 August 1764, fol. 8.
24. RMARS, 17 April 1765, pp. 147^{v}–48.
25. AN, FM, G98, fol. 9, Berthoud to Ministre, 26 December 1765, with reply from Ministre, 24 January 1766; AN, FM, G98, 11 January 1766, fol. 10, authorization for travel.
26. AN, FM, G98, London, 7 February 1766, fol. 11; Paris, 14 March 1766, fol. 12. Compare Anthony Turner, 'L'Angleterre, la France et la navigation: le contexte historique de l'œuvre chronométrique de Ferdinand Berthoud', in *Berthoud*, ed. by Cardinal, pp. 143–63 (pp. 157–8); see also Cardinal, 'Biographie', p. 28.
27. AN, FM, G97, 24 July 1766, fol. 10, extract from the king's decision; see also Cardinal, 'Biographie', pp. 29–31.
28. C. Cardinal, 'Ferdinand Berthoud and Pierre Le Roy: Judgement in the Twentieth Century of a Quarrel Dating from the Eighteenth Century', in *The Quest for Longitude*, ed. by Andrewes, pp. 282–92.
29. At this final meeting before the Académie's closure, Camus, Le Monnier, d'Alembert, Étienne Bézout and César-François Cassini de Thury were appointed judges.

30. RMARS, 30 August 1766, p. 291^{v}; 5 September, pp. 302^{r-v}. Members of the Académie's committee went to Berthoud's workshop to check that the clocks were identical to those described in the memorials and drawings. The documents were then sealed again and lodged with the Académie. The 'Assemblée' did not see Berthoud's clocks.
31. AN, FM, G98, 15 April 1767, fol. 14, Le Monnier to Ministre.
32. See also Richard Dunn and Rebekah Higgitt, *Finding Longitude: How Ships, Clocks and Stars Helped Solve the Longitude Problem* (Glasgow: Collins, 2014), pp. 99–103.
33. See Cardinal, 'Biographie'.
34. D. Fauque, 'Alexis-Marie Rochon (1741–1817), savant astronome et opticien', in *Revue d'histoire des sciences*, 38 (1985), 3–36. D. Fauque, 'Le mégamètre de Charles-François de Charnières (1766–1774)', in *Le Calcul des longitudes*, ed. by Jullien, pp. 61–82.
35. Berthoud's clocks and Charnières's *mégamètre* were trialled on other voyages including Joseph-Bernard de Chabert's in the Mediterranean, Jean-Baptiste Chappe d'Auteroche's to observe the transit of Venus, and Yves-Joseph de Kerguelen de Trémarec's to the southern Indian Ocean.
36. See the illustrations of Le Roy's and Berthoud's timekeepers in Dunn and Higgitt, *Finding Longitude,* pp. 117–18.
37. Pingré had been well known since his voyage in 1761 to Rodrigues island (now Republic of Mauritius).
38. RMARS, 2 December 1767, p. 257; 9 December 1767, p. 278^{v}; AN, FM, G98, 2 January 1768, Fouchy to Ministre de la Marine; 16 December 1767, fol. 99, with the report of 9 December, fols. 100–01.
39. See François César Le Tellier, Marquis de Courtanvaux, *Journal du voyage de M. le Marquis de Courtanvaux*, ed. by Alexandre Guy Pingré (Paris: Imprimerie royale, 1768); see also *Histoire de l'Académie royale des sciences* (hereafter *HARS*) *pour l'année 1767 avec les Mémoires* (Paris: Imprimerie royale, 1770), *Histoire*, pp. 120–36.
40. It is uncertain why Courtanvaux declined to join the second expedition; see Jean Le Bot, 'La frégate *L'Aurore* et les montres marines de Pierre Le Roy', *Neptunia*, 201 (1999), 3–12.
41. RMARS, 3 February 1768, p. 21, Praslin to the Académie.
42. Cardinal, 'Quarrel'; see also: Pierre Le Roy, *Exposé succinct des travaux de Mrs Harrison et Le Roy dans la recherche des longitudes en mer et des épreuves faites de leurs ouvrages* (Paris: Jombert; Nyon: Prault Père, 1768), in which Le Roy's watch was judged superior to Harrison's. Le Roy presented this book to the Académie on 6 February 1768. With thanks to Marie Jacob for information on this quarrel.
43. RMARS, 16 March 1768, p. 52^{v}.
44. AN, FM, G98, 30 April 1768, fol. 102.
45. AN, FM, B3/577, p. 53, Jean Louis Roch Mistral, commissioner for the Navy at Le Havre to Ministre, 2 May 1768, Ministre's instructions, 23 April 1768.
46. Pierre Isaac Poissonnier, general inspector for naval hospitals, presented a paper on his invention to the Académie on 22 May 1765; see RMARS 1765, pp. 244^{v}–45.
47. AN, FM, B3/577, p. 55, Mistral to Ministre, 6 May 1768; AN, FM, B2/387, pp. 553–54, instructions to Tronjoly, 12 May 1768. Tronjoly was injured

during the bombardment of Sala on 1765. A peace treaty with Morocco was signed in June 1767.

48. RMARS 1768, 18 May, p. 90; AN, FM, G 98, fols 107, 111.
49. AN, FM, B3/577, p. 63, Mistral to Ministre, 23 May 1768.
50. The key procedure was similar on the other two voyages.
51. AN, FM, G98, fols 117–20, 124–25. Cassini, Le Roy and Tronjoly wrote independently to the Ministre about this episode.
52. Cassini *fils*, *Voyage fait par ordre du roi en 1768, pour éprouver les montres marines inventées par M. Le Roy* (Paris: C.A. Jombert, 1770), p. 51.
53. Ibid. p. 113.
54. See *HARS pour 1769* (Paris: Imprimerie royale, 1772), *Histoire*, pp. 102–106. A complimentary report on Cassini's book by Bailly and fellow-astronomer Giovanni Domenico Maraldi was read before the Académie on 23 December 1769.
55. Le Roy l'aîné, 'Mémoire sur la meilleure méthode de mesurer le tems en mer [...] contenant la description de la montre à longitudes présentée à S.M. le 5 aoust 1766', in Cassini *fils*, *Voyage*.
56. AN, FM, G98, fols 122–23, Tronjoly to Ministre, 16 September 1768.
57. Alfred Doneaud du Plan, *Histoire de l'Académie de marine* (Paris: Berger-Levrault, 1878).
58. AN, FM, C7/106, Fleurieu's personal file, 13 January 1766, and 2 August 1766; AM, FM, G97, fols 17–18, Fleurieu to Choiseul, 31 July 1766; fol. 14, Choiseul to Praslin, 14 August 1766.
59. Cardinal, 'Quarrel', p. 286.
60. RMARS, 2 September 1767, p. 224.
61. Pierre Bouguer, *Nouveau traité de navigation* (Paris: Jombert, 1753), pp. 105–106, figs 51–52.
62. AN, FM, G97, Lalande to Ministre, 21 February 1768; RMARS, p. 60^{v}, Praslin to Fouchy, 16 April 1768.
63. AN, FM, G97, fols 158–59, concerning the *Examen critique d'un mémoire publié par M. Le Roy sur l'épreuve des horloges propres à déterminer les longitudes en mer*, 14 April 1768.
64. AN, FM, G97, 6 and 12 June 1768, fols 166–72.
65. AN, FM, B2/388, pp. 205–06.
66. AN, FM, B2/388, p. 496.
67. AN, FM, G97, 5 December 1769, fol. 209.
68. RMARS 1770, 21 February, pp. 45–70^{v}. The commissioners were Cassini de Thury, Maraldi, Chabert, Du Séjour, Bory, Borda and Bailly. *HARS pour 1770* (1773), *Histoire*, pp. 97–102; A.-G. Pingré, 'Précis d'un voyage en Amérique', in *HARS pour 1770* (1773), *Mémoires*, pp. 487–513.
69. AN, FM, G97, fols 14–15, Berthoud to Ministre, 14 August 1770.
70. Charles d'Eveux de Fleurieu, *Voyage fait par ordre du roi en 1768 et 1769, à différentes parties du monde, pour éprouver sur mer les horloges marines inventées par M. Ferdinand Berthoud* (Paris: Imprimerie royale, 1773).
71. For Fleurieu's career, see Ulane Bonnel (ed.), *Fleurieu et la marine de son temps* (Paris: Economa, 1992).
72. Training of naval cadets was restructured several times between 1765, under Choiseul, and 1787, under maréchal de Castries.

73. RMARS 1770, 21 February, p. 70^{v}. The commissioners were Cassini de Thury, Maraldi, Bory.
74. RMARS 1770, 7 March, p. 81, and noted 14 March, at 24 March meeting, p. 102^{v}.
75. AN, FM, G98, fol. 154; RMARS, 18 August 1770, p. 235.
76. AN, FM, G98, fol. 151, Fouchy to Ministre, 29 January 1771.
77. AN, FM, G98, fol. 153, undated but probably following Fouchy's last letter.
78. AN, FM, G98, fol. 166, Lalande to Ministre, 30 July 1771.
79. AN, FM, G98, fol. 172, Verdun de La Crenne to Ministre, 18 September 1771. Ozanne was appointed drawing master with responsibility for instructing naval cadets.
80. RMARS 1771, 6 February, pp. 44–45.
81. AN, FM, G98, fols 221–29, Verdun de la Crenne to Ministre, 29 October 1772.
82. RMARS 1773, public meeting, p. 90; AN, FM, G98, fols 205–15, 'Opérations faites sur mer pour la détermination des longitudes et autres objets concernant la navigation', read on 21 April.
83. AN, FM, G98, fols 248–301, the first version was sent to the Ministre on 8 May 1773. Published in *HARS pour 1773* (1777), pp. 64–9; J.-C. de Borda, A.-G. Pingré and J.-R. de Verdun, 'Opérations faites tant à bord de la frégate du roi La Flore, qu'en différens ports ou rades d'Europe, d'Afrique et d'Amérique; Pour la vérification des instrumens et des méthodes relatives à la determination des longitudes sur mer, & à d'autres objets concernant la navigation', in *HARS pour 1773* (1777), Mémoires, pp. 258–322. The manuscript was deposited with the Académie on 27 November 1776.
84. Jean Mascart, *La Vie et les travaux du chevalier Jean-Charles de Borda, 1733–1799* (Paris: Presses Universitaires Paris-Sorbonne, 2000), p. 343.
85. Borda et al., 'Opérations', p. 263.
86. Ibid., p. 306.
87. J.-R. de Verdun de La Crenne, J.-C. de Borda, A.-G. Pingré, *Voyage fait par ordre du roi en 1771 et 1772, en diverses parties de l'Europe, de l'Afrique et de l'Amérique; pour vérifier l'utilité de plusieurs méthodes et instrumens servant à déterminer la latitude et la longitude, tant du vaisseau que des côtes, isles et écueils qu'on reconnaît. Suivi de recherches pour rectifier les cartes hydrographiques*, 2 vols (Paris: Imprimerie. royale, 1778).
88. See Willem F. J. Mörzer Bruyns, *Sextants at Greenwich* (Oxford: Oxford University Press, 2009); D. Fauque, 'Revue critique. De l'art de naviguer à la science nautique au Siècle des lumières', *Revue d'histoire des sciences*, 63 (2010), 189–219 (pp. 203–08).
89. Verdun de La Crenne et al., *Voyage*, p. 10.
90. Philippe Henwood, 'L'Académie de marine à Brest au XVIII^{e} siècle', in *La mer au siècle des encyclopédies,* ed. by Jean Balcou (Paris-Genève: Champion-Slatkine, 1987), pp. 125–34.

10

Navigating the Pacific from Bougainville to Dumont d'Urville: French Approaches to Determining Longitude, 1766–1840

John Gascoigne

Defeated in the Atlantic during the Seven Years' War, the French turned to the Pacific with the hope of finding new lands and markets that would redress the balance of power so grievously disturbed by the expansionist energies of perfidious Albion. France, however, faced the same problem as its rival in venturing into what was, from a European perspective, largely a new quarter of the globe. Navigating the Pacific magnified across a third of the Earth's surface the problem of locating one's position with exactitude; in particular, it required determining longitude at sea. The means to do so had been an increasing preoccupation of both the British and French states and their associated scientific establishments. As Danielle Fauque and Guy Boistel show in this volume, various French techniques for solving this problem had been recorded before the deployment of John Harrison's epochal invention, his sea watch 'H4', in 1761. The conclusion of the Seven Years' War in 1763 was, however, to lead to a fruitful interaction between both nations' attempts to solve 'the longitude problem'.

The Peace of Paris of 1763 brought with it a greater determination by both France and Britain to venture into the relatively unknown Pacific. This conjunction of the growing engagement with the problem of determining longitude and French exploration of the Pacific is the theme of this chapter. By tracing the ways in which the French explorers of the Pacific calculated longitude through the major periods of the eventful late eighteenth and early nineteenth centuries, the Old Regime, the Revolution and the Restoration and July Monarchy, I will illustrate the continuity that existed in the use of such techniques – even though the use of the chronometer eventually did come to predominate. For the difficulties of navigating across the vastness of the Pacific were particularly conducive to pragmatism and pluralism. The quest might be for total accuracy, which the maps based on their records would reflect, but there was an awareness that some element of approximation was inevitable. The lack of ready access to ports where instruments could be checked meant that in the Pacific one had to work with the best approximation available. This was often arrived at by comparing the results established by different methods and, if necessary, averaging them. One theme that emerges from any study of the French voyages of exploration into the Pacific is that there was no clear polarization between the determination of longitude by astronomical methods and the use of timekeepers. Popular histories and the films derived from them might portray the emergence of modern methods of determining longitude as a battle between the proponents of these two methods, with Nevil Maskelyne as the representative of the former wearing a metaphorical black hat and John Harrison a white one. The French practice in the Pacific was, however, to make the two methods complementary.

The Old Regime

Appropriately, the first major French voyage into the Pacific – that of Louis Bougainville in 1766–69 – was led by one of those involved in the epochal defeat at Quebec who hoped to redress French fortunes in a new corner of the globe. Although himself an army captain, Bougainville was open to navigational innovations relying heavily on the astronomical techniques employed by Pierre-Antoine Véron – the presence of this astronomer on board being an early instance of the close interweaving of scientific objectives with French Pacific exploration. Like all navigators to that point, Bougainville had to rely for his determination of longitude primarily on dead reckoning. The inaccuracies of this method when used in the Pacific had become notorious, given the long distances travelled away from land and well-established points of reference.

When Europeans first ventured into the South Seas in the sixteenth and seventeenth centuries, such errors in the Pacific had led to islands apparently shifting location, as early sightings could not be confirmed because of the inaccuracy of co-ordinates for relocating them. Poor charting could combine with wishful thinking to conjure up unknown lands. With some asperity, Bougainville referred to such lack of precision when he passed through the area around Vanuatu, in which the Spanish visionary, Pedro de Quirós, had claimed in 1606 that Terra Australis – a land comparable in size and wealth to America – was located. ‘Now Quiros’s longitude and latitude are left behind’, wrote Bougainville, but then added rather caustically, ‘Where then is his great land?’[1]

This early history of navigational vagueness still coloured the account of the Pacific voyage of Étienne Marchand in 1790–92, and he focused particularly on the problems of relying on readings of longitude. ‘[W]hen two islands have not been discovered by the same navigator and in the same voyage’, he considered, ‘we can depend only on the latitude assigned to each island’. By contrast, the longitude reading was so uncertain that he thought the best course was to approach the island ‘two or three hundred leagues astern of the place where the chart fixes their position’. When, however, Charles Fleurieu, the sometime Ministre de la Marine et des Colonies from 1790–91, published his edition of the voyage in 1798–1800 he enthusiastically added a note praising the recent improvements in determining longitude – something which reflects Fleurieu’s membership of the recently-founded Bureau des longitudes, of which he became a member in 1795. Marchand, he asserted, had been describing the old practices where longitude had virtually been determined ‘by chance’. By contrast, ‘the moderns can employ [...] means that give to those who know how to employ similar ones, the assurance of finding with facility the places which they wish to touch’.[2]

Such methods and confident assertions, however, lay in the future on Bougainville’s voyage, the first major French incursion into the South Seas. Determined to avoid navigational phantoms, Bougainville turned, when possible, to the astronomical calculations of Véron to check his own dead reckoning calculations. He was gratified indeed, when, after departing from the River Plate, he found that his and Véron’s results nearly coincided. Véron had taken as his fixed point the longitude at Montevideo and then used the Sun’s position to determine the time where the ship was now located. To determine the time elapsed since they left Montevideo, ‘a timekeeper graduated in seconds checked on land’ was employed, a prefiguring of the use of the chronometer (despite the fact that the clock used by Véron would not have kept as accurate time). Véron’s results and Bougainville’s differed only by one sixtieth of a degree which, Bougainville proudly observed, was ‘an impressively close concordance between these two methods’.[3] It was an early instance of the pluralism in methods of calculating longitude which long continued to be characteristic in the Pacific.

Generally, Véron’s calculations of longitude were more conventionally based on the lunar distance method. A variety of stars served as fixed points to establish the Moon's position though, on one occasion, Véron also employed observations of the position of Venus. Véron did not employ observation of the satellites of Jupiter, the major alternative astronomical method. Indeed, French navigators rarely mention it since it required accurate telescopic observations not easily performed at sea. When tabulating his readings of lunar distances, Véron followed what was to be the standard practice of averaging them. Reluctant to rely on a single reading, given the difficulties of observation, Véron, for example, made six observations from the Moon to Venus. The next step was to use this data to establish the equivalent time at Paris, which then could be compared with local time, established from the position of the Sun. Such comparison required reference to tables published in the *Connaissance des temps* since 1679. But the tables Véron used in 1768 would have required much greater computational dexterity than those which came into use after 1774, when Joseph-Jérôme de Lalande included in the *Connaissance* lunar distance tables copied directly from the British *Nautical Almanac*, though with French headings and instructions. A nationally dissonant note was struck, since these British-derived tables made Greenwich the point of reference while all other tables in the *Connaissance* were based around Paris. This anomaly, however, was put right after 1790.[4]

Bougainville's Pacific expedition had considerable support in high places but was not officially state-sponsored. The first nationally endorsed French voyage into the southern oceans was the abortive one of Yves-Joseph de Kerguelen-Trémarec in 1771–72, which did not get beyond the southern Atlantic. An equally fruitless second voyage followed in 1773–74. So disappointing, indeed, were the results that Kerguelen was imprisoned for defrauding the state with his inflated claims of the benefits of his voyages. One of those who played a role in sending him to prison was Charles-François de Charnières, who accompanied Kerguelen on the first voyage as far as Mauritius. His early departure was occasioned by friction with Kerguelen, probably linked with Charnières's chief reason for being on board: his high hopes for trying out a new device for determining longitude, which he termed his *mégamètre*. It was not a success, however, being outmoded by the octant and sextant.[5]

It was, however, characteristic of voyages destined for the Pacific to be employed to test out new devices. Kerguelen also had on board timekeepers produced by one of France's leading clockmakers, Ferdinand Berthoud. Born in Switzerland, Berthoud became effectively timekeeper-maker to the French navy. Using techniques at first independent of Harrison's innovations, Berthoud produced his first timekeeper in 1763, though the results were disappointing.[6] The later Berthoud timekeepers that Kerguelen had on both his expeditions proved inaccurate and the calculations of longitude had to rely chiefly on the astronomers on board using the lunar distance method. Part of the difficulty was that Kerguelen did not bring tables that enabled correction of the timekeepers according to changing temperatures.[7] Such difficulties led the influential duc de Croy, an ardent advocate of Pacific exploration, to reflect in a memorial to the Naval Academy in 1774 on the possibilities of timekeepers. These he considered a promising innovation, thanks particularly to Berthoud's devices, but not sufficiently accurate to be used without checking them against the results from astronomical methods.[8]

It took some time for the French state once again to sponsor a major voyage to the Pacific after the fiasco of Kerguelen's voyages. When, however, the La Pérouse voyage was dispatched in 1785, it was equipped with five timekeepers by Berthoud, which, despite their mixed record on Kerguelen's voyages, had by then achieved sufficient accuracy to be more commonly used.[9] Interestingly, La Pérouse also took with him an (unspecified) English timekeeper and, later, the d'Entrecasteaux expedition borrowed an Arnold chronometer at Cape Town to replace a broken Berthoud one.[10] By contrast, the English appear to have made little use of French timekeepers. Their standards may have been high, but the English evidently believed that they did not surpass those produced in England.

La Pérouse also took smaller timekeepers, the size of watches, though with disappointing results.[11] Improvement came from Berthoud's nephew, Louis Berthoud. It was Louis, indeed, who was to win a prize from the Académie des sciences, on the eve of its abolition in 1793, for a pocket watch to determine longitude at sea.[12] Such pocket watches were useful companion pieces to the larger and more delicate timekeepers since they enabled shore-based checking of the time and, if necessary, correction of the main timekeepers without moving them.[13] Louis Berthoud's marine watch was particularly valued since such devices were fiendishly difficult to make or, at least, to make so that they kept accurate time. Experiments by Nicholas-Antoine Nouet at the Observatoire de Paris confirmed its accuracy and the report by the Académie gloated a little on its merits as against those of the English Harrison.[14] By 1815 these were so commonly used that they were made standard for all French naval vessels.[15]

The La Pérouse expedition, like others of the period, established longitude by complementary use of the timekeeping and lunar distance methods. The instructions to La Pérouse (largely framed by Fleurieu) entrusted oversight of the chronometers to the astronomers, together with the requirement that they 'seize every favourable opportunity to check while on land whether they have remained regular'. Moreover, the instructions continued: 'As often as the conditions of the sky shall allow' the astronomers on both ships of the expedition should 'order measurement of the distance between the moon and the sun or the stars to be taken by means of instruments provided' – the results to be compared with those given by the chronometers. The impossibility of any one observation, or indeed any one timekeeper, providing completely accurate results was acknowledged, hence it was recommended that 'the average result of different operations' be used to 'give a more precise determination'.[16]

To the astronomers, too, fell the duty of recording how much each timekeeper lost and gained, while duly correcting such readings 'for the variations caused by the temperature as measured by the thermometer and the swings of the pendulum'. Such corrections for temperature worked well until they reached the cold climates around Cape Horn. There, commented La Pérouse, Berthoud's 'table of temperatures [...] was not exact'. On the other hand, when in the north-west Pacific, La Pérouse considered the close conformity between the figures for longitude from the clocks and lunar distances 'a proof of the excellence of Mr. Berthoud's chronometers'.[17]

What stands out from La Pérouse's comments is the belief that measures of longitude derived from lunar distances, rather than from timekeepers, were the more accurate. Nonetheless, by the time of this voyage, the utility of timekeepers was firmly established, as La Pérouse himself commented. For proof of their value La Pérouse pointed to the way in which, 18 months into the voyage, two of Berthoud's timekeepers 'gave results that were as satisfactory as at the time we sailed'. Indeed, he considered that 'Mr Berthoud has exceeded himself'. But timekeepers, like all instruments, were subject to human error – he distrusted the readings from one timepiece since 'we had forgotten to wind up this chronometer for twenty-four hours'.[18]

The Revolutionary period

While La Pérouse's voyage was the (ill-fated) fruit of contributions from the top echelons of the French state and its scientific establishment, the Pacific voyage of Étienne Marchand (1790–92) was simply a private trading voyage. Yet, as the enthusiastic editing of Fleurieu emphasized, it had considerable success in fixing longitude at sea. Like most merchant vessels of this period, Marchand's *Solide* did not have the luxury of expensive chronometers though he took considerable care when it came to calculating longitudes. Like Bougainville in the pre-timekeeper period, his methods were dead reckoning and lunar distances, with about 70 observations to establish the latter. It was as well he did so, since calculations based on dead reckoning proved predictably unreliable on occasions. Soon after setting off, for example, it was discovered that 'the currents had driven the ship, by an imperceptible movement, 2° 9′ beyond her progress to the westward indicated by the dead reckoning'. Later in the voyage, the need for checking calculations based on dead reckoning was again driven home. Not only did the current carry 'the ship out of her apparent course 1° 46″', but also 'the false measure of time, indicated by the half-minute glass' led to accumulating errors in longitude from dead reckoning. Such errors were corrected not only by lunar observations but also by 'the daily observation of the latitude'. Marchand had the additional advantage of being able to refer to James Cook's published navigational records and, when possible, used these as a check on his own.[19] Thanks to this and frequent checking using the lunar distance method with the improved tables, Marchand kept reasonably accurate records. Fleurieu calculated that, at the end of the voyage, his error was about eight degrees, or 850 km.[20] It was possible, then, to manage reasonably well without chronometers – even in the vast expense of the Pacific – as many merchant vessels continued to do.

Marchand may have had to be self-sufficient when it came to arranging navigational aids but the French state once again lavished its considerable scientific resources on the voyage of Antoine Bruni d'Entrecasteaux (1791–94). Such a parallel with the voyage of La Pérouse was appropriate indeed, given that the object of d'Entrecasteaux's voyage was to search for the missing La Pérouse voyage. The search was in vain and, after d'Entrecasteaux died in 1793, the voyage broke up the following year in post-Revolutionary political disharmony on arrival at Surabaya, as news reached the divided crew that the monarchy had been abolished and replaced by a French republic. La Pérouse's fate, however, was not established until 1826 when, by chance, relics of the voyage were discovered on Vanikoro (in the Solomon Islands).

The d'Entrecasteaux voyage was largely initiated by individuals linked to the Académie des sciences and the Jardin du roi. The number of scientific personnel on board and the equipment allocated to the expedition reflected its scientific character. Devices for calculating longitude included chronometers made by Ferdinand Berthoud, as well as watches by him and his nephew, Louis. Copies of the *Connaissance des temps* and the *Nautical Almanac* were also included.[21] Overall, Élisabeth Paul de Rossel, one of d'Entrecasteaux's officers who published the official account of the voyage, took a very favourable view of the accuracy of the clocks devised by the two Berthouds. Given, however, that there appeared to be some unknown source of slight inaccuracy, he strongly recommended checking chronometers against the lunar distance method.[22]

Like other major French expeditions into the Pacific, that of d'Entrecasteaux did check the accuracy of its timekeepers against astronomically derived results. The vessel was equipped with a telescope specifically intended for the observation of Jupiter's satellites on land but, in the event, this method proved something of a disappointment. This explains why it is only infrequently referred to in the records of French Pacific exploration. When on land in Tasmania, the astronomers on the d'Entrecasteaux expedition were only able to 'observe one eclipse of the first satellite of Jupiter' and, soon afterwards, in the Tongan islands, the astronomers tried a number of times to observe Jupiter's satellites but poor weather defeated them. They fell back on the tried and true lunar distance method.[23]

Also on board was a reflecting circle devised in 1775 by Jean-Charles de Borda, military engineer, mariner and, from 1764, member of the Académie. This was an expensive item and, well into the nineteenth century, it was reserved for major expeditions, such as the state-sponsored Pacific voyages.[24] Borda had been closely involved with the early testing of devices to calculate longitude and had made manifest, for example, the inadequacy of Charnières's *mégamètre*.[25] He had also devised more effective ways to calculate lunar distances. Appropriately, then, he was among the first members of the Bureau des longitudes. He was involved in the preparations for the astronomical contributions of the La Pérouse and the d'Entrecasteux voyages, both of which took his reflecting circle.[26] This device was a refined version of that devised in 1752 by the German astronomer Tobias Mayer. who had provided the lunar theory on which the calculations embodied in Maskelyne's *Nautical Almanac* were based.

The main merit of Borda's improved and widely used device was that it allowed ready viewing of dual images of the object through an attached telescope. This enabled calculation of the angle through which the Earth would need to turn to bring the meridian of longitude directly under the Sun; thus, the hour angle at the solar noon is zero degrees. This, then, was a measure of the movement of the Earth on its daily rotation and, as such, could be used to determine time. Though this was not as accurate as the more elaborate measurement of lunar distances, it was useful for routine recording of longitude. [27] On the d'Entrecasteaux voyage, for example, it was used to determine the Sun's meridian altitude. This, in turn, was used to regulate one of Louis Berthoud's timepieces.[28] In any case, like a sextant, the Borda circle could also be used to measure lunar distances.

The Borda circle became a distinctive feature of French expeditions. Since it was a circle, it could encompass a full 360 degrees (and, indeed, it was graduated to allow observations around two circles). By contrast, the sextant could only measure 120 degrees but the British generally considered that enough for their purposes. Though there were English equivalents of the Borda circle (such as that devised by Edward Troughton at the end of the eighteenth century), these were of a simpler design and, in any case, were not generally preferred by the English to the sextant.[29] This national divide was reflected in the way in which the copiously equipped La Pérouse voyage took with it four Borda circles (though it also took three English-made sextants, perhaps because French ones were not available or were considered inferior).[30]

The formation of the Bureau des longitudes in 1795 embodied the strongly astronomical and scientific approach of the French to determining longitude, as Martina Schiavon's chapter in this volume describes. The Bureau's responsibilities attested to its astronomical character, since it was responsible for both the Observatoire de Paris and the École militaire. It also supervised the publication of the *Connaissance des temps*, which was revised in 1796 to reflect the introduction of the Revolutionary calendar. In time, too, the Bureau oversaw observatories in Marseille, Toulouse and Montauban.[31] In effect, it presided over the French astronomical establishment.[32] Rivalry with Britain, with which France was at war from 1792, does much to explain why the French founded this parallel body, indeed the proposal by abbé Grégoire, which led to its establishment, spoke of the need to 'compete with the English'.[33]

In 1797 Fleurieu, one of the Bureau's early members, pointedly compared the way in which, in contrast to the French, 'there is not a single English captain, employed in a long voyage, who does not at this day, make use of the new methods for determining the longitude of his ship'. This, he added bitterly, explains why 'the navigation of our enemies boldly embraces the two hemispheres'. Even two years after the Bureau's foundation, bemoaned Fleurieu, fewer than a hundred Frenchmen could make use of the tables produced by the Bureau or other useful devices that the French had produced, such as Borda's circle. This instrument Fleurieu regarded as a Gallic triumph, which 'the smallness of its bulk renders as portable, as convenient for use, as the excellence of its principles renders it certain and exact in its results'.[34] Fleurieu, however, may have painted an excessively rosy picture of the expertise on British ships to create a sense of urgency about the need for a French response. Certainly, the chapters by Jane Wess and David Philip Miller in this collection would suggest British practice was less state-of-the-art than Fleurieu claimed.

Part of the difficulty to which Fleurieu referred with such feeling was that the calculation of longitude on French vessels voyaging into the Pacific had often been the responsibility of specialist civilian astronomers. By contrast, the British were less inclined to take men of science on board and tended to leave the task of ascertaining longitude more to the naval officers. Conflicts between scientific and naval personnel, along with political tensions, were a feature of French Pacific voyages of the Revolutionary period. In particular, they poisoned relations on board the expedition under Nicolas Baudin (1800–04), the strongly scientific character of which reflected its Napoleonic patronage. Indeed, on board his two ships the 22 men of science outnumbered the senior officers.[35] Civilian astronomers played a major role in ascertaining longitude: the astronomer, Pierre-François Bernier, for example, kept a record of the variations between chronometers on *Le Géographe*.[36] Nonetheless, by then naval officers were playing more of a role in routine recordings of longitude. Fleurieu would have been pleased to hear that this owed something to the growing use of the Borda circle. Lieutenant Pierre-Guillaume Gicquel of *Le Géographe*, for example, used this method soon after the expedition set off in 1800 to establish the time, which he recorded alongside the chronometer readings. Lieutenant Gicquel claimed to be the only one on board able to use the Borda circle but others, no doubt, learned during the voyage.[37] There were also those on the accompanying *Le Naturaliste*, such as Sub-Lieutenant François Antoine Hérisson, who recorded calculating longitude from readings of the hour angle.[38] The records of the Baudin expedition include frequent reference to establishing longitude by lunar methods but also refer to 'estimated longitude', presumably from dead reckoning. [39]

Use of the Borda circle to calculate longitude became a regular practice on French expeditions and dual tables of both the hour angle and the readings from the chronometers were maintained, for example on Dumont d'Urville's expedition of 1837–40, indicating a lingering suspicion of the accuracy of chronometers.[40] There was, however, no serious debate about their utility: hence, French naval vessels were routinely equipped with chronometers from 1815 (their use in the French and British navies became standard, then, around much the same time).[41] By about the time of Dumont d'Urville's return in 1840, however, the superior accuracy of ascertaining longitude by chronometers, rather than astronomical methods, was generally acknowledged. This meant less attention to utilizing other methods such as lunar distances, which had complemented the use of chronometers since their invention. But the conditions of Pacific voyaging, with vast distances between points where chronometers could be reliably checked, meant that the need for astronomical methods lingered longer than in other parts of the world. Merchant vessels, which often could not afford chronometers, kept such astronomical skills alive. Furthermore, naval officers continued to be trained in astronomical methods as a backup to the use of chronometers.[42]

The Restoration (1814–30) and July Monarchy (1830–48)

This incorporation of the calculation of longitude into the regular duties of naval officers became well established in the remarkable and little-known series of French voyages of exploration and scientific enquiry in the period 1817–40. Having been defeated in the titanic struggle engendered by the Revolution, a depleted France nonetheless found the resources to undertake such voyages with scientific and imperial goals in mind. The list of voyages is considerable, exceeding British exploration in the Pacific. They included Louis de Freycinet's *Uranie* expedition of 1817–20, Isidore Duperry's *Coquille* expedition of 1822–25, Hyacinthe de Bougainville's *Thétis* expedition of 1824–26, Louis de Tromelin's *Bayonnaise* expedition of 1826–29, Cyrille Pierre Théodore Laplace's *La Favorite* expedition of 1829–32 and *Artémise* expedition of 1837–40, Auguste-Nicholas Vaillant's *Bonite* voyage of 1836–37, Abel Aubert Du Petit-Thouars's *Vénus* expedition of 1836–39, Jean Baptiste Cécille's *Héroine* expedition of 1837–39 and, most significantly, Dumont d'Urville's *Astrolabe* expeditions of 1826–29 and 1837–40.

The scientific interest in the first of these, under Freycinet, was considerable, since it was the first major European expedition since the Baudin expedition returned in 1804 (a year after Baudin died) – a long hiatus explained by the Napoleonic Wars. Consequently, there was a report by a star-studded Académie committee, which included Alexander von Humboldt, Georges Cuvier, Joseph Louis Gay-Lussac, Louis Thenard, Jean-Baptiste Biot (a member of the Bureau des longitudes) and Elisabeth de Rossel from d'Entrecasteaux's voyage. The report focused on the degree to which Freycinet's voyage had achieved what they termed its chief goal: research on the shape of the Earth and terrestrial magnetism. Determination of longitude played a part, though the report expressed continuing reservations about the accuracy of the chronometer, remarking that determinations of longitude based only on one chronometer were not of much value in promoting the science of geography.[43] The Freycinet expedition, however, carried five chronometers: four by Louis Berthoud and one by Abraham-Louis Bréguet.[44] Like Ferdinand Berthoud (Louis's uncle and founder of the firm), Bréguet was Swiss by origin but established himself in Paris. There he made chronometers and other scientific instruments, becoming a member of the Bureau des longitudes in 1814 and the Académie in 1816. The firm of Bréguet et fils (which continues) devoted particular attention to the manufacture of chronometers with Louis Bréguet, the grandson, becoming designer-manufacturer to the Bureau in the mid-nineteenth century.[45]

Having experienced firsthand the tensions caused on the Baudin expedition by the clashes between civilian experts and naval officers, Freycinet allocated the scientific work, including the determination of longitude, to the officers.[46] This became characteristic of the Restoration voyages, with even the Académie commenting on 'the sad example' that Baudin's voyage offered of attempting to incorporate a large number of men of science into the routines of naval life.[47] Under Freycinet's command, Lieutenant Laborde, for example, was responsible for astronomical and magnetic observations, including the calculation of lunar distances, which, wrote Laborde, were of 'very high importance for the determination of absolute longitudes'.[48] Indeed, the five chronometers on board all had anomalies, so checking against lunar calculations remained essential.

Others of the Restoration French Pacific voyages also took the precaution of taking on board a number of chronometers for comparison. Hyacinthe de Bougainville's *Thétis* expedition of 1826–29, for example, took four chronometers by Louis Berthoud.[49] Comparing their readings and keeping registers of the details seems to have become part of the regular duties of the officers. Although a mere ensign, Charles Jacquinot was entrusted with the responsibility of overseeing the chronometers on the *Coquille* expedition of 1822–25. The report of his captain, Louis Isidore Duperrey, to the Académie commended Jacquinot for undertaking 'this exacting task with a zeal and an accuracy worthy of the praises of the Académie'.[50] Such work now fell to the naval officers whereas previously on long Pacific voyages it had often been the province of civilian men of science.

Such comparison between chronometers continued to be combined with checks against lunar readings. The Académie strongly endorsed this practice in a report on the navigation of the first *Astrolabe* voyage of 1826–29, under Jules Dumont d'Urville. Its spokesman was de Rossel, who could speak with authority, having served on the d'Entrecasteaux voyage and edited the voyage account. He urged continuation of the practice of using both chronometers and 'observation of the distances from the moon to the sun and to the stars', arguing for the 'excellence of these two methods of determining longitude'. Indeed, he went so far as to argue that 'The great precision of the astronomical tables and that of the instruments means that the problem of [establishing] longitude at sea is resolved'.[51] Such a continuing commitment to using both systems of establishing longitude was evident, too, in the instructions issued to the *La Favorite* voyage under Cyrille Pierre Laplace (1829–32). Laplace was urged 'to obtain differences of longitude, by means of chronometers' but then to check these against 'terrestrial points, the longitudes of which have been ascertained before by correct Astronomical observations'.[52]

The Académie, in its 1840 report on the *Vénus* expedition under Abel du Petit-Thouars (1836–39), again used its prestige to assert the importance of using the lunar distance method as well as chronometers in determining exact longitudes. Among the commissioners was France's most celebrated cartographer, Charles-François Beautemps-Beaupré; the mathematician and astronomer, François Arago, was the rapporteur. Against the rising tide of what it termed 'superficial spirits' who maintained the sufficiency of measuring longitude by chronometers, it pointed out that the six chronometers on the voyage had varied – hence the need to check them regularly by astronomical methods. For all the utility of chronometers, the report concluded, 'the celestial sphere is still the most direct, the surest, the most exact of the instruments of longitude'.[53] The defensive tone suggests, however, that those who claimed they could rely upon chronometers without astronomical cross-checking increasingly questioned the need for such methods. However, the great distances in the Pacific between major ports where longitude could be checked challenged confidence in chronometers. Hence, captains continued to use astronomical methods to confirm longitudes where in other parts of the globe they were less necessary. On the epic voyage of the *Astrolabe* and *Zélée* (1837–40), Dumont d'Urville, when he first made his way across the Pacific from Valparaiso to Manga-Reva (and then to Tahiti), turned to the lunar distance method to check his chronometers in July 1838. Significantly, he gave the lunar readings greater credence than those of the chronometers, arguing that the astronomical calculations 'show that our chronometers place us nearly 30 minutes to the west'. This made him concerned about the accuracy of his chronometers since, 'This difference is a little too much for an interval of fifty days from the time they were regulated'.[54]

Though the vast expanses of the Pacific made it difficult, French voyagers were urged, where possible, to use the facilities of those ports they did encounter to check their chronometers. When the *La Favorite* expedition under Cyrille Pierre Laplace reached Manila, his instructions required him to use the opportunity to 'verify with the greatest exactitude the regulation of his chronometers', especially as 'the longitude of Manila must be taken as a point of departure to fix the longitude of all the points of the Chinese Sea which he shall have visited'. Similarly, when he reached the Atlantic and the port of Rio de Janiero, the instructions continued, 'He will assure himself at that place, the longitude of which is well ascertained, whether his chronometers have been steady'. Even when chronometers were not being used to determine the route, he was admonished to continue taking accurate readings of 'these precious instruments'. [55]

Better still was checking chronometers in official observatories, though there were only limited opportunities to do so. The first of the major Restoration voyages – the *Uranie* under Louis de Freycinet (1817–20) – took advantage of a visit to the observatory at Mauritius in May 1818 to perform scientific observations including determining longitude.[56] Before setting off on the *Coquille* expedition under Isidore Duperrey (1822–25), the five chronometers were given a rigorous inspection, being 'compared, daily, with the pendulum at the Paris observatory, and their rating was regulated by M. Barral, the acting director, and our own officers, by altitudes of the sun taken with an astronomical repeating circle [probably that of Borda]'.[57]

Such precision gave the *Coquille*'s officers, such as ensign Jacquinot, the confidence to take it on themselves to establish exact readings of longitude in areas of the globe such as New Zealand, where the charts of earlier voyages had differed among themselves. In that case, they were assured of the accuracy of their timekeepers because they had been regulated before leaving Sydney (presumably at the observatory). Reassuringly, when the *Coquille* reached New Zealand, Jacquinot reported that the watches 'stayed in almost perfect agreement and the daily rate we took at the Bay of Islands differed by only a few tenths of a second from that calculated at our point of departure'.[58] Once the longitude of a Pacific location had been established with care, the French navy could build on it in future voyages. The instructions for *La Favorite*, for example, referred to the way in which the longitude had been determined securely by the d'Entrecasteaux expedition. Consequently, it 'may be taken as the point of departure towards those which will be observed when leaving that port'.[59]

Compilation of such detailed records had a long history in the French navy, reflecting the instinctive centralization of information since (as in Britain) logs were deposited in state repositories on return. This French bureaucratic liking for uniformity and clarity was apparent in the decision in 1772 to adopt printed forms for shipboard records (earlier than the Royal Navy). Such records promoted a more quantitative approach to recording the details of a voyage, whether the basic coordinates of latitude or longitude or other measurements such as magnetic variation.[60] As methods for determining longitude became more sophisticated so, too, did the records tallying precise information. On the d'Entrecasteux expedition, for example, there were journals kept by the civilian astronomer, Ambrose Pierson (who also served as a chaplain), recording the readings from the chronometers along with astronomical calculations to determine longitude and workbooks comparing the two.[61] Among the many detailed records kept by the Baudin expedition were longitudes measured both by timekeeper and by lunar distances.[62]

Steadily, the amount of information recorded about the Pacific expanded, the French tradition of bureaucratic system marrying well with the scientific impulse to collect as much data as possible.[63] Measuring longitude was one of the most potent forms of encompassing the globe so that it could be mapped, travelled across and controlled. The French experience of making the Pacific accessible by means of the grid of longitude and latitude illustrated the extent to which the goals of both science and of a modernizing and expansionist state could work together with a common purpose.

Notes

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Part IV

The Practice of Navigation

11

Navigation and Mathematics: A Match Made in the Heavens?

Jane Wess

> [W]e may be said to receive from the *Mathematics* [...] Increase of Fortune, and conveniences of Labour [...] we have safe Traffick through the deceitful Billows, pass in a direct Road through the tractless Ways of the Sea
>
> Isaac Barrow, 1734[1]

This chapter explores the extent to which elite astronomy and mathematics influenced the practice of navigation in Britain in the period some historians celebrate as the birth of scientific navigation.[2] Although some writers, particularly those closest to the practice, consider navigation to be an art, most tell of the 'science' of navigation and most put the start of scientific navigation with the advent of the *Nautical Almanac*.[3] Obviously these developments, from intuition to reliance on a mathematically based procedure, were not instantaneous or without problems.

The research was stimulated by an interest in the extent to which mathematics could deliver effective results. Because the lunar distance method of finding longitude depended on a much higher level of mathematics than any other lay mathematical practice of this period, it was a crucial case study. This chapter argues that, contrary to expectations and the implications of some authors, the lunar distance method featured very little in actual practice right up until the end of the eighteenth century and beyond. The golden years, which Howse identified as 1780–1840, before chronometers became sufficiently reliable and affordable for widespread use, appear to have been very few, if they existed at all.[4]

This argument challenges the applicability of Wigner's concept of the 'unreasonable effectiveness of mathematics' to real situations in this period.[5] This concept was the underlying assumption behind Isaac

Newton's approach: in an excerpt from his 'Optical Lectures', Newton stated that 'Astronomy, geography, navigation, optics and mechanics are truly considered mathematical sciences, even if they deal with physical things [...]. Indeed, I hope to show, as it were by example, how valuable mathematics is in natural philosophy'.[6] In particular, the anonymous author of the preface to Newton's 'Theory of the Moon's Motion', purports to show that the new Newtonian philosophy has a practical as well as theoretical significance.[7] This concept has gained credibility and been steadfastly adhered to by many writers since.[8] While Wigner cites the inverse square law of planetary motion and the mathematical description of the helium atom, he infers the upholding of our understanding of the entire physical world through the use of mathematical description. He does not question the transfer of this theoretical understanding to lay practice, where the significance can be frustrated. According to authors in *Is Mathematics Inevitable?,* for example, 'The practical value of mathematics cannot be over-emphasised'.[9] However, although believed by many to be humanity's 'most powerful weapon in the conquest of nature', mathematics not only failed to deliver in this case, but also, it will be argued, was the major impediment to the success of the lunar distance method.[10]

The issue of longitude loomed large in the expansionist endeavours of European nations, in particular as represented by the Royal Society in Britain during the eighteenth century. It stimulated the pursuit of natural philosophy and the production of instruments.[11] We can connect the mathematization of navigation, in the desire to find longitude, with the development of Britain as a nation state. While Williams can reasonably argue that, 'Only in modern times has navigation been central to warfare and defence', global trade, colonial expansion, and national competition for dominance over far-distant lands all played a central role in establishing power.[12] Gascoigne has documented the increasing role of the Royal Society in government, and the influence of commercial interests, specifically in navigation, in England during the century from 1670. This is apparent in the foundation of the Royal Observatory and the Royal Mathematical School at Christ's Hospital, the Smith Prize, the passing of the Longitude Act and the multiple purposes of James Cook's voyages.[13] At this time, according to Lambert, Britain began to benefit from a Tudor decision to become a maritime nation.[14] Besides the national interest, the vast British East India Company (EIC), with tax-collecting rights and a large army, was sending between ten and 15 ships a year to India by 1710. In the following 30 years, 'warehouses filled, ships sailed, dividends steadied, fortunes accrued'.[15] The EIC was commissioning ships at a rate of 30 per year in the mid-eighteenth century, and did not want to lose them or their precious cargos. Ten thousand tons of shipping passed through Calcutta each year between 1715 and 1725.[16]

The triumphalist tale of John Harrison has been told many times, even before Sobel's 'true story of a lone genius', galvanizing historians into counterbalancing presentations of the lunar distance method.[17] The unfortunate bipolarity of the debate has contributed to an unwillingness to investigate critically the efficacy of the lunar distance method. Bennett's unassuming paragraph at the start of his chapter 'Longitude Found' in *The Divided Circle*, to the effect that the widespread use of both methods had to await the nineteenth century, requires a wider airing backed up by detailed research.[18] This chapter goes some way towards that, arguing that dead reckoning was the principal method of finding longitude until at least 1800, that the emphasis on more sophisticated methods has overshadowed its prevalence and that the impact of the lunar distance method in this period has been over-stated. Recently, more balanced views have seen the practices as complementary, with no single method being taken as 'read'.[19] However, before the work of the Board of Longitude project, some authors overlooked dead reckoning, not even acknowledging it as a method of finding longitude.[20]

The input of 'Mathematicians of the First Character'

Between the late seventeenth and mid-eighteenth centuries, leading mathematicians and astronomers devoted considerable resources to understanding the motion of the Moon. These were 'Mathematicians of the First Character', to use a phrase from the preface of Newton's *A Treatise on the Method of Fluxions and Infinite Series*.[21] There is a long list of major players, including Newton, Edmond Halley, Roger Cotes, John Flamsteed, Jean le Rond d'Alembert, Alexis Clairaut, Nicolas-Louis de Lacaille, Leonhard Euler, James Bradley and Nevil Maskelyne.

The beginning of this particular episode in the 'harrassment of the Moon', as William Whiston charmingly put it in 1728, was a visit by Newton to Greenwich in September 1694, when he was shown Flamsteed's lunar data.[22] Newton had asked for 'the Right Ascensions and apparent meridional altitudes of ye Moon as you have found them in yr observations without allowing for the refraction and parallax. I will take care of all the rest'.[23] Curtis Wilson relates that Newton received 250 lunar observations, but that the errors averaged eight arc minutes, when observations to within two arc minutes were necessary to find longitude to one degree.[24] There is some uncertainty as to what exactly Flamsteed sent Newton, but it may have been corrected results when Newton required raw data.[25] The implication from earlier historians that Flamsteed withheld data appears untrue, a ruse later put about by Newton to disguise his lack of progress. At that time Newton felt there was just a possibility that the Moon's motion could be understood in a way which would solve the longitude problem, Gregory relating that Newton felt 'the theory of the Moon is within his grasp'.[26] However, with the theory not comprehensively mastered, four years later Newton wanted to keep his efforts from public gaze, claiming that Flamsteed's disclosure of his work on the Moon would show that he was 'trifling his time away with mathematical things'.[27] In computations not made public at the time, Newton had written that it was 'better to omit' them, 'as being too complicated and impeded by approximations, and insufficiently accurate'.[28] Westfall describes this episode as the 'lunar debacle'.[29]

Whiteside, Westfall and Iliffe have recounted Newton's struggle with this problem.[30] Although other historians have argued that it can only be seen as a failure if you discount the following 50 years, the fact that Newton saw it as a failure is revealing.[31] He is reported to have said that his 'head never ached but with his studies on the Moon', and confided to De Moivre that he would like to have had another go at it if he were a younger man.[32]

Newton's *Theory of the Moon's Motion* appeared in a pamphlet in 1702, consisting of a seven-step recipe in tortuous and opaque text for applying equations to solve the problem of finding longitude by the lunar method. It was included in a slightly different form in the second edition of the *Principia* in 1713, when, according to Cohen, a reviewer called attention to the longitude problem.[33] Lunar theory appeared in the 1726 English edition but without a specific reference to its use. The statement that 'The mean motion of the moon and its apogee are not yet known with sufficient accuracy', made in the third edition of the *Principia*, begs the question 'for what?', but it is reasonable to assume that it refers to finding longitude.[34] Newton's statement to the parliamentary commission on longitude in 1714 supports this, listing the various methods before denigrating them: 'A Third [method] is, by the Place of the Moon: But her Theory is not yet exact enough for this Purpose: It is exact enough to determine her Longitude within Two or Three Degrees, but not within a Degree'.[35] In 1727 James Bradley, the then Astronomer Royal, detected and explained the aberration of light.[36] What is particularly relevant here is that he had been working on the problem of lunar parallax.

A 1789 portrait of Joseph Banks, long-standing President of the Royal Society, shows him with an engraving of the Moon by John Russell.[37] Tobias Mayer also saw the potential of the Moon for ascertaining the precise latitudes and longitudes of places for map-making.[38] In his 1745 *Mathematischer Atlas* he produced detailed illustrations of the Moon's surface.[39] More importantly for the manifestation of the lunar distance method, he produced a lunar theory sufficiently accurate for use by Maskelyne in tables for navigation. The Moon was the implicit, and sometimes explicit, object with potential. The end product of this endeavour, spanning three generations and including French work on lunar distances discussed in the chapter by Guy Boistel, was the *Nautical Almanac*, which arrived in 1767, and the accompanying *Tables Requisite*, published in 1766, 1781 and 1801, containing data that did not need recalculating on an annual basis.[40] The fact that there was a tangible product at this point is largely due to Maskelyne's efforts.[41] The *Nautical Almanac*, so it was claimed, enabled seamen to use the lunar distance method

A new emphasis on general practice

Some long-held views of this episode, promulgated by a series of historians, should now be challenged. The first is that the *Nautical Almanac* changed everything. Taylor claimed that in 1767 'The pre-scientific age of navigation was brought to a close'.[42] Much later Howse claimed that 'a very high proportion of the world's deep sea navigators' began to use the *Nautical Almanac* from 1767.[43] However, while 10,000 copies of the *Tables Requisite* were printed by 1781, this is no guarantee of use: Howse states that 6,992 remained unsold by 1784.[44] A survey of navigators' textbooks, instruments and ships' logs in the 1770s has found little evidence of new methods being introduced as an immediate result of the publication of the *Nautical Almanac*.

The second misconception is that the lunar distance method was easy, an error made by taking too literally the word of the person who was promoting it. Maskelyne's assertion that it demanded 'only care in the computer, and [...] no particular knowledge of spherical trigonometry' is technically true but misleading.[45] Taylor and Richey's statement that the tables 'made a long known theoretical method practicable' was not borne out in the immediate aftermath.[46] Likewise, Howse claimed that 'any competent mariner' could now measure longitude 'quickly and comparatively easily'.[47]

The third misconception is that the new tables did away with the most time-consuming and difficult parts, reducing the calculations from four hours to 30 minutes.[48] It is likely that this statement held true for Maskelyne, James Cook and John Campbell, who were among the small number of people capable of performing the calculations at this time, but otherwise seamen were not using the lunar distance method. Again, Howse: 'officers of the East India Company and of the Royal Navy showed the way. Some of the former had already begun to find their longitude the hard way, [...] now they turned thankfully to the new almanac'.[49] However, the impression that the *Nautical Almanac* changed standard practice is misleading.

The fourth misconception is that seamen were backward. It may be that practitioners take up new methods slowly but, as Thomas Haselden put it in 1722, 'Navigation is no trifling knowledge'.[50] Dead reckoning demanded a level of mathematics not strictly elementary. Using 'plain' trigonometry, sometimes with the Mercator chart, following through the individual problems to form a traverse table to produce the day's work, relates approximately to highest school level today. There was considerable skill in navigation, which became increasingly mathematical in this earlier period, not through the use of lunar distances but through gradually improved teaching via schools, lectures and texts explaining the more routine, but nevertheless demanding, methods. The texts indicate an increasing use of the Gunter rule during the eighteenth century, a mathematical tool for navigation based on the work of Edmund Gunter in the early seventeenth century.[51] There is a perennial concern that the use of instruments leads to a de-skilling, but I have elsewhere argued that there was an increase in the mathematical skill of lay practitioners during the period, and an increase in the use of an instrument indicates a wider take-up, not a resort to rules by those already familiar with the unaided process.[52] However, the computation of lunar distances was tedious, complicated and opaque compared to calculations based on dead reckoning, and without appropriate training remained beyond the capabilities of ordinary navigators.

Evidence from texts, instruments and ships' logs

The lunar distance method supposedly was in its prime during the last third of the eighteenth century.[53] However, this section will show, first, that there are few references to the method and the *Nautical Almanac* in navigation texts in the period immediately following its first publication. Second, it will explore the puzzling development of and relationships between instruments, in particular the Hadley quadrant (octant) and the sextant. Third, evidence from a selection of ships' written logs is examined.

I examined the following practical navigation texts for content regarding use of the lunar distance method:

William Mountaine, *A Description of the Lines on a Gunter Scale*, 1768
Alexander Ewing, *A Synopsis of Practical Mathematics*, 1771 and 1779
Benjamin Donn, *Description and Use of the Navigation Scale*, 1772
John Robertson, *The Elements of Navigation*, 1772
William Puddicombe, *The Mariner's Instructor*, 1773
Murdoch Makenzie, *Treatise on Maritime Surveying*, 1774
Thomas Haselden, *Seaman's Daily Assistant*, 1774
Benjamin Donn, *The British Mariner's Assistant*, 1774 and 1785
John Hamilton Moore, *Practical Navigator*, 1776 and 1786
Henry Wilson, *Navigation New Modelled*, 1777
Andrew Wakely, *The Mariner's Compass Rectified*, 1779
Benjamin Martin, *The Mariner's Mirror*, 1782
John Bettesworth, *The Seaman's Sure Guide*, 1783
William Nicholson, *The Navigator's Assistant*, 1784
John Ritchie, *Directions for Sailing in the Northern Part of the Bay of Bengal*, 1785
Samuel Dunn, *The Lunar Method Shortened in Calculation and Improved*, 1788
John Hamilton Moore, *The New Practical Navigator*, 1794
Andrew MacKay, *Description and Use of a Sliding Gunter*, 1802
Andrew MacKay, *The Complete Navigator*, 1804

The texts published in the 1770s included no references to the *Nautical Almanac* or lunar distances. William Puddicombe wrote in 1773:

> The manner of keeping a reckoning by our English navigators is by the log line and half minute glass. Longitude 'made' being what is always kept in the HM Navy, and the longitude IN being more generally kept on Merchant ships. Meridian is where you take your departure from.[54]

In 1777, Henry Wilson wrote, 'Nor is it possible to conclude, that Navigation is arrived at its *ne plus ultra* so long as the Longitude remains to be such a puzzling Subject to our best Mathematicians'.[55] Ewing recommended taking the Lizard as the reference meridian in 1779, which would make use of the *Nautical Almanac* impossible.[56]

In the 1780s, a more complex situation arises. Benjamin Martin's preface talks of a reform in navigation, but by this he means the discovery of the true figure of the Earth. He gives a table of meridional parts to take account of the ellipsoid nature of the globe, indicating the use of dead reckoning. His preface to part 2, dated 1782, clearly states: 'The Method of finding the Longitude by the Distance of the Moon from a Star is well known to be, if not impossible, yet so difficult and dubious as to be of little or no Use to the Mariner', citing the authority of the long-dead Halley.[57] He then begins a suspicious trend: devoting three pages out of 100 or so to the lunar distance method, which is considerably more difficult than anything else in the book. John Bettesworth devotes seven pages out of 161, William Nicholson 13 out of 223. Nicholson was honest enough to say that, 'A book of the nature of the present might be thought essentially improper, if it did not contain instructions for the solution of this problem [Longitude]. A Chapter is therefore added', thus making it clear that he was only including it for form's sake.[58] In 1785, Benjamin Donn gave instructions for taking the latitude by observation but not the longitude. There are no lunar distances in Ritchie in 1785 or in John Robertson in 1786. In 1788, Samuel Dunn gave rules for lunar distances that are considerably simpler than Newton's or Maskelyne's, the first meaningful reference found in a book of this type intended for practising navigators.[59]

Fewer works were available for study from the 1790s, possibly indicating reduced production in Britain. May notes that the first text to mention a chronometer was by John Malham, in 1790.[60] John Hamilton Moore does mention the sextant, the *Nautical Almanac* and *Tables Requisite*, giving more examples than previous works, but still only devoting an extremely small proportion to lunar distances. In 1802 and 1804, Andrew MacKay gives a detailed description of the observation, recommending a Hadley quadrant for the altitude measurements and a sextant for the lunar distance.[61] However, the calculations still only occupy 20 of 268 pages, less than 10 per cent. It appears that information on taking lunars was available in texts by the end of the century, but it was not what sold the books.

Second, if we turn to the development of instruments, there is a considerable literature. However, there is a tendency to focus on early, beautiful and special instruments rather than those produced in large numbers, possibly because of the influence of the scientific-instrument trade and a tendency among instrument historians to focus on the moment of invention rather than general take-up.

The basic instruments for dead reckoning were the sandglass, log and line, and compass. Unless there is a specific reference to a 'watch', 'timekeeper' or, later, 'chronometer', time was measured by sandglass.

East Indiamen and Navy ships recorded speed and heading every hour, merchantmen every two hours.[62] If the lunar distance method were as successful as previously claimed, one would expect less emphasis on log readings in this period.[63] Essentially, the log and line gives the ship's speed, which, combined with intervals of time, gives its distance and, with direction or 'course', the position relative to where the ship started. A day's work refers to the combination of the ship's various tacks in 24 hours. Basic trigonometry, sometimes adjusted for latitude by using meridional parts, gives a deduced change in latitude and longitude. The considerable development of the log and line in this period, making them more 'scientific', is unexpected if the *Nautical Almanac* had the effect claimed. One in the Science Museum collection by Foxon was patented in 1772. It uses a spiral mechanism, the turns of which can be counted to give a more accurate reading.[64] The other essential piece of kit was the compass.

The standard Gunter scale also appeared to be developing during this period.[65] Benjamin Donn proposed one, an example of which is in the Science Museum, accompanied by a booklet, in about 1775.[66] The problems described were all to do with dead reckoning; that is, with plane trigonometry. The 'canons', or basic rules, are also set out on the instrument. The first is 'As the Radius is to Distance so is the sine of the course of departure, and so is the sine complement of the course to the difference of latitude'. Departure was the change in longitude. This is followed by three similar canons – for finding latitude, mid-latitude sailing and using the Mercator chart – involving the table of meridional parts. In 1778, William Mountaine advocated a sliding Gunter, which does not appear to have been as successful as the basic rule.[67]

Another instrument that continued unabated was the octant. Mörzer Bruyns's *Sextants at Greenwich* shows examples from the 1750s almost identical to those of a century later.[68] Some writers have given the impression, perhaps inadvertently, that the sextant superseded the octant. This may be an interpretation of readers accustomed to linear developments of instruments and the ideas that accompany them. Wynter and Turner describe the sextant as an improvement on the octant; Andrewes states, 'When the new theory that made the lunar distance method viable was advanced in the 1750s, it took little time to develop the octant into a more precise angle-measuring instrument'.[69]

The longevity of the octant therefore requires explanation. For example, Turner says that the sextant superseded the octant in the 1770s, but he goes on to say the latter remained until about 1900 because of its low cost.[70] Mörzer Bruyns is clear: 'While the octant remained in use to measure altitudes, the sextant was primarily used for lunar distances'.[71] May also describes how two assistant observers would use octants during a lunar, an interesting parallel to trigonometric surveying practice at this time, where the highest-ranking person would have the most expensive instrument.[72] Bennett says the octant took on the role of the workaday instrument, while the sextant was more special.[73] Hewson gave a compelling answer to the question of longevity back in 1951: 'The reason for the popularity of the Hadley's quadrant in spite of the superior precision of the sextant, was that probably for observations of latitude it served well enough'.[74] Given the lack of evidence for lunars, which I will discuss, and the extraordinary cost of sextants, at four and a half times the price of octants, the vexing question is why people were buying sextants at all.[75] Without further research, the answer would be guesswork, but the beauty of the contemporary instruments is striking, in line with the 'special' nature described by Bennett.

An important factor is the introduction of mechanical scale division. Chapman argues that the expense and slow production of hand-graduated instruments limited the full potential of the lunar method. He differentiates the work of instrument makers Jesse Ramsden and Edward Troughton, both operating with the new dividing engines in the period under consideration, from that of earlier followers of the classical tradition: George Graham, John Bird and Jeremiah Sisson. He notes that Ramsden and Troughton were part of the movement that introduced the industrial revolution.[76] Producing sextants in considerable quantities was required for widespread use of the lunar distance method. 'Machine graduation offered the prospect not only of cheaper, more plentiful instruments, but of easily attested accuracy that was free from personal errors'.[77] Ramsden's second engine, specifically for nautical instruments, came into use in 1775.

McConnell relates Bird's approval of Ramsden's engine and his assessment that it was accurate to two thousandths of an inch. She relates that after the introduction of this second engine, hand dividing quickly gave way to engine division. Ramsden took in instruments from other makers, producing over 1200 by 1794, if the serial numbers are an accurate reflection. McConnell gives a figure of 1450 by 1800, and claims Troughton produced at a similar rate. The most detailed analysis of the number of sextants produced comes from Stimson's 1975 survey.[78] It is not necessary to go outside London, indeed outside Greenwich, for tangible evidence of the successful production of these exquisite instruments from the Ramsden and Troughton stables. After 1775, the slow production of sextants cannot stand up as an impediment to the adoption of the lunar distance method.

Thirdly, there is evidence from ships' logs. May made a survey of logs from the point of view of the introduction of the chronometer in 1976.[79] Similar research is required for the use of the lunar distance method. About 50 Royal Navy logs picked at random between 1767 and 1800 have been studied, also a set plucked from 1807/8, two logs from ships of exploration, ten EIC logs picked at random from the period 1780–1800, and about 20 'Memoirs of Charts' published for the EIC by Alexander Dalrymple between 1785 and 1797.[80] The latter are part of a set of 50 drawn up by Dalrymple following his appointment by the EIC in 1779. Previously, May had looked at a small number of logs, mostly from the EIC.[81] A range of routes was included because these ships were selected at random, and, over a period of years, they often sailed to several different places.

There was very little difference in Royal Navy logs between 1767 and 1800, whatever the size of vessel or route. Captains' logs were preruled, with date, day, wind, course, distance, latitude in, longitude made and bearings in columns from left to right on the left-hand page.[82] The right-hand was a written log, always describing the weather, often rations and, sometimes, punishments. There were no attempts to take numerical measurements within a certain distance from any shore; that distance could be up to 30 leagues or 90 miles. While it may appear that longitude was measured immediately upon leaving this area, these measurements were taken in all conditions, even when the weather is described as squally with thunder. The process is clearly dead reckoning, with latitude and longitude, the latter from the point of departure at the start of the day, deduced from a combination of the various courses and distances.

In about 1775/6, longitude made (that is, from the start of any particular journey) was replaced by longitude in, from a fixed meridian. This was a step towards a nominal zero line, but that was not always Greenwich, as the Lizard was frequently used. Latitude in and longitude made, or in, were marked every day, whatever the weather, also indicating no use of astronomical measurement. Masters' logs were rougher but more detailed, with speed in knots and depth in fathoms marked each hour.[83] A gradual move towards the use of numbers in describing the course (or compass direction) in degrees rather than compass points is detectable but not consistent.

In about 1795, there is a break in the pattern. There are regular daily latitude measurements without the accompanying course, distance and longitude in, for places within reach of land where previously nothing would have been recorded. These are not dead reckoning results, which rely on course and distance. They appear to be latitude results by observing altitude with an octant or possibly a sextant. Unfortunately, no reference to instruments appeared in the written logs, but EIC ships in the 1780s and 1790s frequently took latitude, but not longitude, by observation, so this would not be unexpected. This trend appears not to continue and there is a reversion to the previous manner of working later in the decade. More ships' logs need to be studied, but a random selection of Royal Navy ships sailing in the first decade of the nineteenth century revealed no change.

In order to be sure lunar distances were not missed, a voyage of exploration was added to the list of Navy ships, although these relatively few and sophisticated projects were not the focus of this study.[84] Evidence of lunars was immediately found, having a very different appearance to dead reckoning. The first of these was the voyage of the *Discovery*, which, with the *Chatham*, sailed under George Vancouver to Australia and New Zealand across the Pacific and then up the west side of North America between 1791 and 1795. In 1793, Lieutenant Puget was acting commander and kept the log.[85] He was using two 'watches', one running on Nootka rate and one on Monterey rate. From 2 February 1794, he also began to take lunars or 'longitude by Sun and Moon'. The log includes calculations of differences resulting from the various methods employed. Vancouver had taken 12 sextants, according to McConnell, the majority by Ramsden.[86] It appears that these instruments, and particularly the chronometers, were objects of study as much as providers of reliable data.

Another voyage of exploration was that of the *King George*, which sailed to the Sandwich Islands in 1786–87. The logbook is ruled to give spaces for longitude by account (dead reckoning), by observation (astronomically), and by the 'watch' or 'time piece', the first being taken regularly and the latter two sporadically. On 27 March 1787, all three were taken with results 50 minutes of arc apart.[87] These pioneering vessels were experimenting with the new methods 20 years after the introduction of the *Nautical Almanac*.

Previous research has indicated that the EIC was ahead of the Navy in taking up new methods of navigation, and Dalrymple's *Memoirs of Charts* bear this out.[88] On 8 April 1779 Dalrymple urged navigators to supply him with their observations 'either by the Moon or timekeeper', promising to provide templates for logs, 'properly ruled with all the columns which I could *wish filled up*'.[89] The aim was to produce reliable charts using as many methods as possible to find latitude and longitude: namely old charts, existing ships' logs, bearings, lunars, chronometers and dead reckoning. The ambition was to reduce the need for future

East Indiamen, the regular merchant ships, to have to perform ocean navigation.[90]

One of the longer memoirs, written between December 1786 and February 1787, concerns the production of a chart from Mozambique to India.[91] Dalrymple compared his own use of lunars with a timekeeper on the sloop *Swallow* in 1776, which were productive until the timekeeper 'stopt'.[92] In 1785, both Captain Huddart and Captain Cumming made chronometer observations through the Straits of Malacca.[93] 'Had Capt Cumming's chronometer shewn the exact longitude on his arrival upon the Coast of China, we should, from His observations, have very satisfactory data [...] but this was not the case'.[94] Dalrymple frequently had to take the mean of two or more very different results to construct his charts.

Another Memoir recounts the passage of the *Atlas* under Captain Allen Cooper in 1785, eastward of Banka, Indonesia.[95] Cooper had an Arnold chronometer, which he compared with lunar distances. Dalrymple, while acknowledging his gratitude to Cooper, stated, 'Capt Cooper's observations, do not give, with competent precision, the Longitudes in this quarter'. Cooper routinely corrected his chronometer from bearings taken from the shore. The following year another *Memoir* concerned the Straits of Sunda and Banka. The problem treated was the difference of longitude between Krakatoa and Lusipara. Dalrymple used as evidence the timekeeper deployed on the EIC ship *Resolution*, without corrections by lunar distance, 'which I think reduces the difference of longitude to an impossible quantity', indicating he did not believe the results.[96] These are warts-and-all accounts and frequently Dalrymple expresses frustration: having discovered that the *Sulivan*, *Ponsborne* and *Hawke* had sailed to the east of Banka in the preceding few years; 'I cannot learn that any sketch was made [...] and I despair of doing it from the Journals'.[97]

John McCluer, first lieutenant of the East India Company Marine at Bombay, was taking the time by chronometer every hour and altitudes with an octant in 1786. He complained: 'The EIC sent for this survey a box and 2 Arnold chronometers which were more trouble than any real use', although Dalrymple thought that McCluer's expectations were too high.[98] On a later voyage to Malabar in 1789–90, McCluer took lunars as well as using a chronometer, but put little faith in his results.[99] However, at Tellicherry and Anjengo he was pleased with the similarity of his chronometer and the astronomical results he made on land.[100] On 20 and 26 April 1788, Captain John Pascal Larkins took sophisticated lunar distances each side of Antares with a sextant and a quintant, with a discrepancy of half a degree. However, the astronomical observations are sporadic, whereas course and distance, leading to latitude and longitude made, are a constant feature of the log.[101] By 1793, James Horsburgh in the *Anna* was frequently taking longitude from stars both sides of the Moon in the most sophisticated observations found.[102] Horsburgh, an accomplished mathematician, was introduced to Joseph Banks in 1796 and he became a Fellow of the Royal Society in 1806, so he arguably is unrepresentative of ships' captains in general.[103]

Dalrymple expressed his view in 1784: 'Lunar Observations at Sea are incompetent to fix reciprocal Longitudes of *Places* so near to each other; and the Observations do not agree together, so that we must have patience 'till The exact Longitude can be obtained by Chronometer'.[104] Even for one of the few who had taken lunar distances successfully, the 'golden age of the lunar distance method' is a misleading phrase. At about the same time he wrote:

> The more recent Improvements in NAVIGATION, which leaves the *Lunar Observations* as far behind, as *they* did the *Variation*, is by CHRONOMETER. [...] the degree of precision to which *they* are now brought, by *Arnold*, would not, ten years ago, have been believed *within* the *bounds* of *possibility*.[105]

Dalrymple's close association with the chronometer maker John Arnold, documented by Cook, needs to be taken into account, but his comments add weight to the view that the lunar distance method was not generally accepted as a satisfactory solution for finding longitude.[106] Dalrymple did concede that lunars were useful for long voyages. The divide between voyages of exploration and the rest was not as marked in the EIC as it was in the Royal Navy. However, neither are the voyages that produced Dalrymple's *Memoirs* truly representative of the routine East Indiamen's practice, for as Dalrymple comments; 'there is rarely time for surveying on the side'.[107] For information concerning the range of situations, ten ship's logs were chosen at random for the period 1780–1800.[108]

The *Lively*, whose log was kept by the chief mate Robert Neve, did not measure latitude or longitude other than by dead reckoning in 1780–81 on its way to and from Madras and Bengal.[109] The *Tartar* under Captain Edward Ffiot measured latitude astronomically, as well as magnetic variation, on its way to and from Madras and Bengal in 1781–83.[110] However, there was no measure of longitude other than by dead reckoning. The *Hawke*'s log, mentioned previously, shows that while the vessel was travelling to and from China and Madras, latitude was observed astronomically from 1781, but longitude only by dead reckoning until 1790, when a chronometer was introduced.[111] The *Queen* under Captain Peter

Douglas, travelling between Madras, China and St Helena, measured latitude astronomically from 1783, and introduced lunars occasionally from 1787.[112] The *Royal Admiral*, under Joseph Huddart, a navigator of renown, used a chronometer for longitude from 1787.[113] The *Swallow* under Captain George Curtis only used dead reckoning on its trips between England, Madras and Bengal in 1790 and 1791.[114] The *Taunton Castle*, under Captain James Urmston, introduced lunars and a chronometer from 1791 when travelling between Bombay and China.[115] The *Ocean* under Captain Patton, sailing to and from St Helena, Madras and China, used lunars from 1792.[116] The *Isabella* under Captain George Wilkinson only observed latitude astronomically in 1798.[117] Similarly, the *Castle Edin* under Alexander Cumming observed latitude astronomically but not longitude until 1802, when lunars and a chronometer were introduced.[118] A further two logs from EIC ships in 1802–03 indicate that astronomical and chronometric methods were introduced at about this time, but they were not routine procedures.[119] Dead reckoning continued after the introduction of the new method in every case.[120]

It appears from this cursory survey that earlier writers were correct to suggest that the EIC was ahead of the Navy in adopting new methods of navigation: take-up varied much more than in naval ships, indicating that individual captains had some discretion. However, the majority of ships appear not to have been using lunars before 1790, a generation after the introduction of the *Nautical Almanac*. The earlier use of chronometers in the EIC as compared with the Navy would also serve to foreshorten the claimed 'golden age of the lunar distance method'.

Conclusion

A consideration of texts, instruments and most particularly ships' logs, suggests that the Royal Navy did not use the lunar distance method except on elite and specific voyages until the first decade of the nineteenth century at the earliest. In the East India Company, a brief study suggests that only ships of exploration were using them until the 1790s, and then it was just as likely a chronometer would be preferred, or neither. Therefore, the 'Golden Age' of the lunar distance method did not start until the nineteenth century, after which lunar distances were used tentatively and sporadically, often alongside chronometers, and accompanied by dead reckoning. It is possible to say that whatever frustrated this 'Golden Age', it was not the availability of instruments. People must have bought sextants intending to use them, or perhaps simply liked them because they were beautiful objects. Many latitude observations were taken, so it may well be that in the more well-resourced ships the sextant did indeed replace the octant, and the linear history of instruments will be vindicated.

The problem was not intrinsically the weather or the convenient position of the Moon, although these must have had some bearing on long-term take-up. Although it is a nuisance not to be able to take lunars every day, even on clear calm days there was no evidence of attempts to take them. The problem was not, certainly by 1800, an absence of texts, although they were tellingly slow in coming. The problem was not that chronometers were more reliable or accurate. In this period they were not, as they were still considered objects of enquiry rather than simply as tools. The problem has to involve the mathematics. This is not beautiful maths that would appeal to a top mathematician; it is tedious, complex, unintuitive and non-visual mathematics.

Although this chapter has questioned the writings of Howse in particular, he, paradoxically, exposed the reality of the situation to a considerable extent. He related that ships' masters were supposed to get a certificate from the Portsmouth Royal Naval Academy, the idea being that when the ship docked there they took a course on the lunar distance method. However, older masters were refusing to learn, and by 1771, four years after the publication of the *Nautical Almanac*, only 14 had been instructed.[121]

Going back to Newton, Kollerstrom described the 'anguished sense of failure which haunted Newton over this endeavour'.[122] For so long the mathematical elite had seen the Moon's motion as the solution to one of the most important problems of the day. Sadly, mathematics – 'Man's finest creation for the investigation of nature'[123] – rather than delivering Barrow's promise of 'safe Traffick through the deceitful Billows',[124] proved to be the downfall of astronomical navigation in this period.

Notes

1. Isaac Barrow, *The Usefulness of Mathematical Learning Explained and Demonstrated*, (London: Stephen Austen, 1734), p. xxviii–xxix.
2. Eric G. Forbes, 'The Bicentenary of the Nautical Almanac (1767)', *British Journal for the History of Science*, 3 (1967), 393–94; Eric G. Forbes, *The Birth of Scientific Navigation: The Solving in the Eighteenth Century of the Problem of Finding Longitude at Sea* (London: National Maritime Museum, 1974).
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12

Longitude Networks on Land and Sea: The East India Company and Longitude Measurement ‘in the Wild’, 1770–1840

David Philip Miller

> McCluer had a lot of trouble with the box chronometer [...] until [he] found that by holding the key still and turning the chronometer around it he could keep it going, and when he adopted this trick it subsequently kept much better time
>
> W. E. May, drawing on John McCluer, 1806[1]

> More in fact is to be gained in Hydrography [...] by establishing the true place and bearing of a few fixed Observatories on terra firma, simply as starting points, than from a thousand unconnected or disputed points of departure [...] they constitute, in fine, a sort of half-way house between the earth and the heavens, to which any phenomena may be referred
>
> Thomas B. Jervis, 1840[2]

The story of John Harrison and his marine timekeepers, engagingly told by Dava Sobel, has promulgated the misleading notion that accurate clocks solved the problem of determining longitude at sea.[3] Neither Harrison’s timekeepers nor the cheaper, more seaworthy, chronometers of Arnold and Earnshaw immediately transformed the longitude problem, partly because there were alternative, well-supported methods, notably that employing lunar distances, promoted by Nevil Maskelyne, Astronomer Royal, the villain of Sobel’s piece.[4]

However, to advocate either of these 'solutions' misses the central point that I want to make – that we need to understand the longitude problem, and its solution, in a broader sense. Neither technological determinism identifying an instrument as *the* solution, nor singular method determinism, captures how longitude was established in practice.[5] Its solution could be universalized in theory but individual determinations of longitude at sea were contingent acts reliant upon hardware (instruments, ships), software (methods, procedures, logs, charts, tables, outputs of land-based observatories), and wetware (the embodied skills, abilities, judgments and goals, of sailors, officers, hydrographers and their masters).

We are accustomed to a tidy view of navigation and survey from the centre. An alternative takes the perspective of the practitioner 'in the field, or rather on board ship, working with his eyes and hands to make a record of the voyage'.[6] But the views from the centre and the field 'are not mutually exclusive, and it is important to consider the ways in which they formed part of a network through which images made on the spot were transformed into authoritative knowledge'.[7] In the determination of longitude, the networks linking the navigator in the field with the centre need to be delineated if the diverse processes constituting knowledge about longitude are to be properly understood. Besides this macroscopic aspect, there is also a micro-dimension: the mutual dependencies of different instruments, inscriptions and skills in on-the-spot, goal-directed determinations of longitude. Edwin Hutchins has depicted this compellingly in his study of modern navigational cognition 'in the wild':

> In attempting to understand the history of navigation from a cognitive perspective, it is important to consider the whole suite of instruments that are used together in doing the task. The tools of navigation share with one another a rich network of mutual computational and representational dependencies.[8]

To illustrate the nature of these longitude networks I discuss the longitude determination practices of East India Company ships in the late eighteenth and early nineteenth centuries. This focus allows depiction of the interdependent hardware, software and wetware involved in those practices. It also allows us, because of the varying objectives of Company voyages, to examine how longitude solutions and networks varied with purpose and circumstance.

East India Company voyages and their evolving longitude networks

Hardware

East India Company ships (East Indiamen) in the late eighteenth century were early adopters of the use of chronometers in longitude determination. The Company was a 'front-row spectator of the longitude experiments of the eighteenth century'.[9] We know this because of records and survivals of the instruments used, the claims of their makers and the evidence of ships' logs. The inclusion, in 1791, of a column in the logbooks used by East Indiamen for longitude by chronometer seems a significant moment in the story of longitude determination in practice.[10]

In the late eighteenth and early nineteenth centuries, the heyday of its merchant fleet, the Company operated more than 70 ships, some built and owned by the Company, most hired from private owners. However, the Company carefully regulated the specifications of ships, the rates paid for freight, and the appointment of commanders, officers and crew. The provision of supplies and equipment, including chronometers, was similarly controlled.[11]

Arnott identifies about 100 numbered chronometers carried on East Indiamen between 1800 and 1833.[12] Of these, the Company owned about half, the hired officers the rest. Given that the standard logs that all Company ships had to fill included a column for longitude by chronometer from the early 1790s, there were probably enough chronometers for all ships to carry at least one. Arnott finds examples of multiple chronometers being carried on one ship, some belonging to the Company, some to the officers. The point is reinforced by evidence that Company chronometers were occasionally lent to Royal Navy captains, a generosity belying a chronometer shortage. Chronometers were first carried on East Indiamen in the 1770s and 1780s, though, as May showed by examining the logs of ships with timekeepers aboard, they were not always used.[13] When they were, understandably it was at first experimentally and with caution, but with growing confidence.

Using a chronometer was not a simple matter of reading the time. Chronometer-centric histories of navigation neglect other aspects of the hardware crucial to chronometric longitude determination. The latter involved the comparison of local time with the reference time carried by the chronometer. The usual method of determining local time depended on measuring solar altitudes, so the hardware of octant and sextant is a standard facet of our longitude network. Chronometric and astronomical methods interacted and were complementary in other ways. Various timekeepers aided the 'double altitude' method for finding local time and latitude and were used to time intervals between longitude determinations by lunar distance, so that a number of results could be averaged and greater accuracy ensured.[14] Measuring altitudes by octant or sextant requires, of course, that the position of the horizon be determined, which is difficult on a pitching ship, or in poor atmospheric or meteorological conditions. The taking of solar altitudes on *terra firma* was thus important, as was the use of another piece of hardware – the artificial horizon. In certain conditions and situations, the artificial horizon was an indispensable aid to the observations needed to get full benefit from a chronometer.[15] We begin to see in these interdependencies something of navigation in the wild.

Other hardware was required for lunar distances. Although the lunar distance method of longitude determination is often discussed as an alternative to determination by chronometer, in practice (certainly in the period dealt with here) the methods were often used in conjunction. While 'lunars' could be taken at sea routinely when weather and the position of the Moon allowed, the opportunity during a voyage to set up the rudiments of an observatory on land enabled more accurate lunar measurements which, after a fairly complex process of calculation involving the mathematical skills of the 'wetware' and the use of software in the form of tables, provided a check on the determinations of longitude being made by chronometer in conjunction with local time measurement. Where more than one chronometer was carried, lunar distance observations could be used to determine the timekeepers' accuracy, and hence reliability. The constancy of the rate of a chronometer (or otherwise) could be checked whenever a ship spent several days in a harbour of known latitude and longitude.[16]

We will see that although chronometers and lunars could be used in conjunction, there is some doubt about the extent to which they were so used during regular voyages of East Indiamen (as opposed to surveying voyages) in the late eighteenth century. More generally, the use of chronometers to determine longitude was a practice hedged about by contingent judgments. These concerned the condition of chronometers themselves, the success (or otherwise) with which they had been cared for at sea, the constancy of their rates, the reliability of other longitude measures, determinations of local time and, crucially, how much time and effort it was worth investing in potentially superfluous precision.[17]

The hardware of temporary land-based observatories could also be used to deploy methods that were impracticable at sea, notably the measurement of eclipses and transits. These could offer more accurate longitude determinations. Similar services were provided by the hardware of permanent observatories. Chronometers were usually calibrated and adjusted in London, but there were moves in the very late eighteenth and early nineteenth century to establish Indian observatories to perform these tasks. Much depended on contrasting views about the advisability of moving chronometers once installed on board ship. Observatory time signals to ships in harbour (by flash of a gun or drop of a time-ball), or 'running' time by watch between observatory and shipboard chronometer, provided an alternative check of chronometers left *in situ*.[18]

Ships themselves were part of the hardware of longitude determination.[19] As Sorrenson puts it: 'On a map, the ship's track is a representation of the probing course of the instrument through the sea, whereas the coastal outlines are the mark of that instrument's interaction with the coasts under investigation'.[20] More prosaically, the ship, together with log lines and compasses, was also involved in the time-honoured practice of determining longitude 'by account', that is by dead reckoning. In the mid-eighteenth century, in light of Harrison's timekeeping ambitions, some were unimpressed and felt that established precepts in the use of latitude, log, lead and lookout were sufficient to find a ship's position *accurately enough*. Such practices retained a central place even after astronomical and chronometric methods became established.[21] The practice of dead reckoning was itself replete with tacit and contingent aspects. For example, the use of reduced knot spacing on the log line ensured 'that speed and distance run was overestimated, and the vessel prudently kept behind her dead-reckoning position'.[22] Conservative practices compensated for large margins of error. When currents were understood, dead reckoning could be relied upon to an extent in everyday navigation, for example in latitude sailing. Where currents were not understood, the determination of longitude by chronometer and comparison with the figure from dead reckoning could indicate current direction and strength. The chronometer was thus variously the means and object of inquiry.

As already noted, through this period ships would often use chronometers, lunar distances and dead reckoning as checks upon each other. The evidence of textbooks on navigation suggests some reluctance to trust timekeepers until surprisingly late. Andrew Mackay's text of 1807, for example, advised against relying on them too much, and considered that lunars would remain the most trusted method: 'The same degree of confidence cannot be placed in time-keepers, as their rate of going is so liable to be altered from the least accidental injury'.[23] However, Mackay endorsed using chronometers to enable multiple lunar distances to be taken which could then be averaged to obtain a more accurate result.

Within the larger narrative of chronometric measurement as bringing greater precision to navigation, we should not underestimate the 'unruliness' of technology in use.[24] As Scott argues, navigators were 'accustomed to coping with imprecision at sea'. Even at the end of our period, there were still poorly charted areas, information on tides and ocean currents was limited, the magnetic compass posed many difficulties and astro-navigation remained a demanding art. Mariners evolved 'wrinkles' in their practices, as Scott terms them, to cope with these problems.[25] The chronometer was no exception, as the image of Lieutenant McCluer turning a box chronometer around a stationary key reminds us (see initial epigram). The trend, however, was obviously towards greater trust in the chronometric method. By the 1840s, chronometers were ubiquitous, generally considered more reliable, more easily checkable and hence more relied upon. But some navigators were, nevertheless, concerned that increased confidence in them was creating a dangerous situation. Commander William Walker, R.N., pointed to a larger problem – the decline of old, but still necessary, skills:

> The general use of chronometers, the correctness of our logarithmic tables for practical astronomical purposes; the accuracy of our astronomical and mathematical instruments used for naval purposes, have left us little to desire for all the purposes of practical navigation, so long as *the state of the weather* will allow us to make astronomical observations [...] the necessary care and attention, under ordinary circumstances, to the *helm*, *log*, *lead* and *lookout* have been sadly neglected by the majority of seamen [...] worse dead reckonings are now kept than were ever kept before.[26]

Therefore, as chronometers became ever more reliable, appearing 'wrinkle free', they still could not stand alone in longitude determination, and previous generations of techniques, and skills, had to be maintained to cover all contingencies and situations. Walker was not alone in this view. In 1840, Henry Raper advocated the possession of chronometers by 'every vessel beyond a mere coaster', but warned that 'too firm a reliance on such an instrument would lead to the dangerous error of relaxing that vigilance which the known uncertainty of the dead reckoning keeps perpetually alive'.[27]

We conclude that the chronometer was just part of a battery of hardware and techniques used in a complex of mutually interdependent practices that were themselves contingent upon purpose, location and conditions. It is also important to note that, while I am making an analytical distinction between hardware, software and wetware, at all points these were operating together as skills, transcriptions and instruments conjoined to produce measurements.

Software

The software involved in longitude determinations by East Indiamen consisted of inscriptions of many sorts: those made by navigators during their voyages; the ships' logs that were kept and then accumulated back in London; the charts constructed in London and other centres and then carried by the commanders of subsequent voyages; the *Nautical Almanac,* which, from its first publication in 1767, provided information needed for the lunar and other methods; and the outputs of other observatories.

The information provided by charts was vital. Accurate determination of longitude at sea is worth little if the chart depicting the location of coastlines, islands and shoals is deficient. Improvements of longitude determination at sea were thus only of value if they moved in step with the determinations of longitude on, and of, land that were carried at sea in the form of charts. Even with those improvements, a wise captain would still deploy lookout, lead and other indicators such as birdlife and the colour of the sea when he considered his ship was approaching land or shallows.

This software was contained and produced by institutionalized practices of navigation and hydrography. From 1779 a key institution through which much of the software passed, and where it was combined and consolidated, was the office of the Hydrographer to the East India Company.[28] Two men, Alexander Dalrymple (1737–1808) and James Horsburgh (1762–1836) held that office during our period, and apart from their work through the Hydrographic Office, they published a good deal about East India voyages, routes and navigation, further increasing the software available.

The numerous charts produced by these hydrographers drew upon the accumulated navigational experience of East Indiamen and upon the more sustained work of maritime surveyors. Such charts obviously gave officers greater confidence in their knowledge of the lie of the sea and the land. They clearly also made it increasingly possible to trust that determinations of position at sea would enable accurate, timely voyages that took advantage of favourable conditions and avoided unfavourable ones.

When Dalrymple began making charts for the Company in the early 1770s, the practice of determining longitude for navigational purposes often diverged from that ideally required for chart-making. To remedy this, Dalrymple not only championed the use of chronometers, especially those made by his friend and associate John Arnold, but also sought to shape the routine practices of the navigators. From 1779, he issued leaflets on the use of chronometers and forms for chronometric and other observations, trying to harness the regular, routine voyages of Company ships to accurate chart-making. The software issued in accord with the chart-maker's goals shaped the hardware, and how it was used.[29] Importantly, however, 'scientific' charts were not always what officers of East Indiamen wanted. Private producers of so-called 'bluebacks' dominated the chart trade. Some features of bluebacks that made them technically less perfect, such as heavily engraved coastlines and over-scale renditions of important hazards, in fact catered to the needs of users. So did their robustness and compactness.[30]

Other crucial pieces of software were the determinations of their longitude by local Indian observatories (local meridians), sets of 'corresponding observations' undertaken by them, for example of eclipses of Jupiter's satellites, and, to an extent, charts produced by them. The accurate determination of a local meridian and observations made at it, thus contributed to longitude determination for Company ships, especially those engaged in surveying the Indian coast and exploring and trading in the seas beyond India. On the strength of accurate knowledge of their own position, such observatories could also more reliably check the rating of chronometers than commanders of ships could. I discuss these important nodes in the networks of longitude determination under 'software' since that was their output, but I could equally have discussed them under 'hardware'. The distinction is heuristic not real.

The earliest and most successful of these observatories originated in the Madras Government's urgent desire in the 1780s to survey the Coromandel Coast because of the numerous ships being lost on its shores.[31] In 1787, a marine officer, Michael Topping, ran a 300-mile set of triangles from Madras to Palk Strait (between southeast India and modern day Sri Lanka).[32] He knew that the longitude determinations made by astronomical means during the survey had little value unless comparable with corresponding observations made at stations of known longitude. William Petrie, a member of the Madras Council and keen amateur astronomer, lent his private observatory to assist with Topping's survey. The astronomer in Petrie's observatory, John Goldingham, undertook observations of Jupiter's satellites at Madras corresponding with Topping's field observations.

In 1789, Petrie offered his instruments to the Madras Government. Topping was placed in charge of the planned observatory and a site was purchased. In 1791, the instruments were installed there, including timekeepers sent by the Court of Directors. The navigational focus was clear as Topping concentrated on 'correspondent astronomical observations, as the only sure and practicable method of finding the relative position of distant transmarine situations'. He hoped 'by the help of these observations, and the use of Chronometers', to accurately chart the eastern seas.[33] Topping's successor, John Goldingham, continued observations of the eclipses of Jupiter's satellites, developing sophisticated methods for weighting different kinds of longitude observations to construct an 'accurate' figure.[34] Such weighting of observations from different sources was something that was also done in simpler, often informal, ways, by navigators and chart-makers.[35]

In February 1805, John Warren became acting astronomer at Madras. He continued Goldingham's observations and further emphasized the Observatory's navigational support role by promoting its services for rating chronometers. He insisted that chronometers could, without detriment, be carried from ship to observatory to have their rates checked with a rigour impossible on board ship. Warren conducted experiments supporting this contention and the proposed role for the observatory.[36]

As of 1811, two Brahmin assistants at Madras Observatory made daily observations of the Sun's transit at noon, regular observations of the eclipses of Jupiter's satellites and transits of a number of fixed stars with a view to regulating the astronomical clock. In addition, Warren advised, they 'make out the rate of the numerous time-keepers which are sent to the Observatory by Captains of ships'.[37] Warren claimed that Commanders of East Indiamen had taken up this service from the earliest days of the Madras Observatory.[38] He had enjoyed less success among Royal Navy captains who, like Whidbey and Owen, considered any disturbance of the chronometer likely to derange it.

Madras Observatory became a key institution for navigation, for coastal surveys, and for what became the Great Trigonometrical Survey of India, the original baseline of which was referenced to the Observatory's longitude. Smaller observatories were also established at Bombay and Calcutta, which likewise engaged in observations and time measurements supportive of navigation.[39] The practical focus of Company-supported observatories is sometimes lamented as a colonialist devaluation of astronomical science proper in India. A positive valuation of navigational and geographical practice must needs celebrate rather than lament that focus. The observatories, especially Madras, were important to longitude determination on land and sea. Routine navigation and scientific survey became dependent upon them, their determined position and their observations.[40]

Wetware

To understand the navigational practices of East Indiamen we need to inquire into the skills of the commanders and other officers and their acquisition. This task is difficult and a comprehensive treatment is beyond the scope of this chapter.[41] However, I will offer some new evidence in relation to the use of chronometric and lunar distance methods by Company officers.

We know the identities of commanders and other officers of East Indiamen and have records of their experience at sea.[42] In the later eighteenth century, such commands were lucrative and prestigious.[43] They were open to purchase and commands could pass to relatively inexperienced individuals. However, there were also dynasties (like the Larkins and the Wordsworths) that combined wealth with extensive experience and expertise. Their scions often went to sea as midshipmen at an early age. Developments in the internal arrangements of the Company in the late eighteenth century opened commands somewhat to able chief mates, whose progress had previously been difficult.[44] The degree to which these officers were educated in navigation at institutions designed for the purpose, including the Royal Mathematical School at Christ's Hospital, is an important question.[45] For most, the Company's elaborate system of qualification for officers ensured a fairly rigorous training, though that was provided through a complex network of teachers and examiners, and through training aboard ship, rather than by a single institution.

Surviving records of the Company's Committee of Shipping provide some details. That Committee required the proposed commander and mates (chief, second, third and fourth) for all voyages to appear before it and provide certain information. First there was evidence of age (referred to as 'Age Certificates'), usually attestations by curates regarding baptism. Second, there was evidence of experience of voyages undertaken and years at sea.[46] Third, and of most interest here, was the requirement for evidence of specific navigational competencies. On 7 October 1768, the Court of Directors resolved that all candidates for officer and mate on East Indiamen must produce a certificate stating that they were qualified in determining longitude by lunar observations.

This certificate was required before they were examined for the position of mate.[47] In consequence, in the early records examined (from 1771) each entry for first to fourth mate bears an annotation of the form 'X's Certificate Produced'.[48] After 1787, the procedure was modified and each entry was marked 'A No. xx' and 'L No. xx', indicating the number of the Age [A] Certificate and the Longitude [L] Certificate for that individual. Clearly, the Company was by then keeping a central register of certificates. Unfortunately, the only Longitude Certificates surviving in the archive date from 1820 onwards. Therefore, from 1787 until 1820 we lack the names of the certifiers. It is clear, however, that throughout our period the Company had a working system.

The designation 'Longitude Certificates' indicates that quickly after the Court's order of 1768, the Company paid regular attention to competence in longitude determination. The precise form of early certificates is unclear. The post-1820 certificates are printed forms, with the candidate's name entered and the signature of the examiner attesting to qualification in 'the Lunar method of finding the Longitude at Sea', 'working the Time for correction of Chronometers', and 'measuring Angular Distances, taking Altitudes with an Artificial Horizon, and working them'.[49] We can presume that early certificates would not have dealt with working the time for correction of chronometers, but exactly when that requirement was added is unknown. Perhaps it was added when the new logbook requiring an entry for longitude by chronometer was introduced in the early 1790s.

The Committee's records, at least up to 1787, give other important information, notably the identities of certifiers. It is clear that the Company relied upon a relatively small group of teachers of mathematics and navigation. A number, and those relied on most heavily, were successive Masters of the Royal Mathematical School at Christ's Hospital. Also active were teachers from the Royal Naval Academy at Portsmouth. But proprietors of private academies also served as certifiers, the most prominent during our period being Robert Bishop, the most frequent certifier through much of the 1770s. Bishop had worked for Maskelyne in the early days of the *Nautical Almanac*, and developed forms to assist in the computation of longitude by lunars using the *Almanac*. He had been appointed by the Board of Longitude to instruct masters of HM ships in the use of the *Almanac* and quadrant, and to issue certificates of competence. It seems likely that Company practice mimicked these early efforts in the Navy. We know that Bishop had a close relationship with the Company because he used journals from their voyages to compile his *East India Navigator's Daily Assistant*.[50]

Samuel Dunn, the next most prominent certifier, had also previously been nominated as an instructor and examiner for masters of HM ships. As already noted, however, the Company's certification system seems to have been more consistently applied than the Navy's, which faced significant resistance.[51]

Many of the certifiers published texts on navigation, and often supplied instruments. Officers of East Indiamen, and those aspiring to be such, would have been important customers. Notable texts included John Robertson's *Elements of Navigation* (1754), which reached its seventh, and last, edition in 1805. The fourth edition (1786) was revised by William Wales, another certifier. *Elements* was written for young students but no doubt mature seamen also used it, though some considered it too theoretical. John Hamilton Moore's *Practical Navigator* (1772) was intended to be more accessible, having 'arranged, digested, simplified and rendered navigation attainable to the most common capacity'. The work went through 20 editions to 1828.[52] Of particular interest is the mock examination that Moore included, since it indicates the range of knowledge and technique, besides determination of latitude and longitude, expected of the budding seaman. Although competence in longitude determination was separately certified, other aspects of seamanship were evidently tested by oral examination at the Committee of Shipping.[53] Another source, already mentioned, bearing on the issue of the education of officers are the publications Dalrymple produced to instruct them in the conduct of navigational measurements.[54] It is obviously hard to gauge the extent to which education and instruction gained in these ways was used, or to determine precisely how. But such sources suggest a range of expectations.

It is perhaps odd, given this tradition of education and certification, that in the late eighteenth and early nineteenth centuries there was concern about neglect of the lunar method. Jane Wess's chapter in this book questions the common assumption that East Indiamen in the late eighteenth century employed chronometric and lunar distance methods of longitude determination. She argues that the lunar distance method was rarely used in regular navigation because of the extensive and 'ugly' mathematical calculations it involved. In that sense, the relative neglect of lunars may reflect the mathematical capacities (and time budgets) of our 'wetware'.

Serious concerns about the use of the lunar distance method on East Indiamen are evident from the Court of Directors' instruction of 24 January 1804 concerning the requisite qualifications of Commanders and Mates of ships, which stated:

> That such of the officers as have not been already instructed in the method of finding the longitude of a ship at sea, by lunar observations, do immediately perfect themselves under Mr Lawrence Gwynne, at Christ's Hospital, previous to their attending the Committee to be examined for their respective stations; and that they do produce to the Committee a certificate from that gentleman of their being qualified in the method.[55]

Why might there be a significant lack of facility in the lunar method among Company officers? Perhaps the method was too involved to be readily taught through the normal processes of apprenticeship or learned through private study. Perhaps it did require formal instruction of the sort that officers were encouraged to obtain from Mr Gwynne. It appears, however, that such instruction had been regularly taken for some years under a variety of teachers and certifiers, so any lack of facility is puzzling. The other possibility is that officers did not find the method useful or managed without it.

Another set of instructions to Commanders issued in 1810 referred repeatedly to lunars. First, Commanders were advised, as before, that officers deficient in the technique should see Gwynne. Second, Commanders and their officers were ordered to 'embrace every favourable opportunity [...] to take lunar observations'. It was insisted also that 'Your own, Chief and Second Mate's journals, are to be kept in the fullest and most explicit manner, and not, as is too often the case, a diary only of courses, winds and weather'. Finally, the point was rammed home:

> Such charts as you may receive from this House are to be returned, at the end of the voyage, with your journals; and the graduated charts for the ship's tract, with variations, longitude by observation and chronometer, you are particularly directed to render as complete as possible; and you are strictly to enjoin your senior officers and encourage your junior officers, at all times when practicable, to use the lunar observations, to observe the variation of the compass by azimuths and amplitudes, finding the latitude by double altitudes, and every other branch of navigation.[56]

The need to promulgate these instructions suggests that by this time the officers of East Indiamen preferred to use some combination of the oldest and the newest methods of longitude determination – dead reckoning by compass and log on the one hand, chronometric determination on the other. The enthusiasm for chronometers may have caused the apparent neglect of lunars, with officers judging that they could navigate safely without much attention to them. The Court of Directors, perhaps influenced by the Hydrographer, took a different view. Perhaps they were concerned that too much trust was being placed in chronometers.[57] It is also possible that the Hydrographer's charting activity was the impetus. Even if the lack of lunars as a check on chronometric measurements did not worry the practical voyager too much (we will see below that the use of *any* method other than dead reckoning was necessary only at key points in a voyage), it would have serious consequences for the Hydrographer's use of the information gathered to produce charts. Perhaps the 'neglect' of lunars was a construct generated by divergence between interests within the Company.

Longitude networks, contingency and the navigational practices of East Indiamen

Bringing together hardware, software and wetware on Company ships occurred within a framework of varied purposes. One of the most obvious was the safe and timely conveyance of a valuable ship, crew and cargo. Chronometer use during such voyages was complex. It was not a matter of simply reading the time off the chronometer, determining local time and hence deriving the longitude. More than one chronometer was usually involved (though depending on knowledge of their condition and rate, the decision might be made not to rely upon them at all), as was careful judgement, combined with information about longitude from other sources and hence about the rates of the chronometers. When the decision was made to trust a particular chronometer, it might be relied upon implicitly for monitoring longitude over relatively short distances. Generally speaking, however, chronometer reliability declined with the length of the voyage because of accumulated errors and so lunar distances, less precise but more constant, might be relied upon more in the later stages of a voyage.[58]

The extent of reliance upon longitude determination by chronometer would have differed in the case of ships whose purpose was to explore and chart new routes. We should also be open to the possibility of variation in practice according to the route in question: the use of longitude determination by chronometer in the charting of variations to routes between British and Indian ports might differ from the charting of routes in explorations further east launched from those Indian ports.

Getting to India (or not)

There were a number of passages to India from the Cape of Good Hope. The Inner Passage, through the Mozambique Channel, offered the shortest route, but was dangerous because of the variable winds and strong adverse current. The *Winterton* was wrecked in 1792 in these waters. When the Commander estimated that his ship was 80 miles off the southwestern shore of Madagascar, he was relying on a chronometer that had served him well to that point and on lunars taken four days before.[59] Perhaps his charts deceived him. Nevertheless, he had wisely entertained the possibility that his determined position and/or his charts might let him down, since he was careful to set a night course that reduced the ship's eastward progress. His one omission was in not taking soundings – 'no leadsman's voice sang out the fathoms'. The ship ran aground.[60]

Despite the dangers, the vast majority of ships cleared the Mozambique Channel. The preferred course brought them to the latitudes that could be sailed through the Maldives, notably the 8-degree and 9-degree channels. During such latitude sailing, determining longitude was not a major concern, indeed the technique of latitude sailing had been developed long before to cope with inaccuracies in dead reckoning. Having passed the Maldives and sighted the coast, navigation by landmarks then became possible.

The Outer Passage, sailing east from the Cape until well east of Madagascar and the Chagos Islands before turning north into the Bay of Bengal, could be used at any time of year, but was longer. It also had its dangers if the longitude at which the northward passage was made was uncertain. Before longitude determination by lunars or chronometers became available, the Inner Passage was generally preferred for that reason.[61] A Middle Passage might be an attractive route to Bombay, sailing east of Madagascar but west of the Chagos group, though this was only viable if a ship left the Cape after September. It was not much used.

Examination of logbooks is key to understanding the realities of the navigational practices of East Indiamen.[62] Even commanders deeply interested in promoting longitude determination, such as Joseph Huddart, whose journals of a voyage in 1778–79 in the *Royal Admiral* were among those examined, were sparing in their determinations of longitude by observation.[63] Long stretches of voyages would be guided only by dead reckoning and, importantly, by awareness of the likely imprecision of the method. Occasional observations might be made to correct the longitude from dead reckoning, but errors of 4 or 5 degrees (several hundred miles, depending on latitude) were often accumulated. Of course, arriving at a place of known longitude also enabled the dead reckoning to start anew. Before the 1780s, when the usual route to India was via the Mozambique Channel, ships often stopped at the Comoro Islands, where they fixed their position. Those islands were commonly used as a reference meridian for the balance of the voyage. After a long spell in open sea, it was important to know when to 'get serious' about longitude again, and when that point came there was in Huddart's case, as in others, a flurry of astronomical observations to determine longitude until land was sighted. From the 1780s onwards, voyages increasingly eschewed the Mozambique Channel and instead headed into the southern Indian Ocean, exploiting prevailing winds. On this route, getting serious again about longitude came in deciding when to turn north for the east Indian coast or the Malacca Straits. When East Indiamen began to carry chronometers, they might use them in a similar tactical fashion, though often alongside lunar distance determinations as a check.

Currents could create serious problems with dead reckoning and, if this was known or expected, lunars might be used more regularly. For example, the *General Coote*, in 1783, under Commander William Harrington and on its way to Madras, had a passage of its voyage during which lunars were taken daily and diverged considerably from the longitude by account. Presumably, Harrington knew that currents would make dead reckoning more uncertain in this area. By contrast, in the subsequent voyage from Madras to China, lunars were rarely or never used. Longitude determinations were made by dead reckoning and the journal records regular sightings of landmarks as a check upon it. [64]

So, on a ship comfortably in open sea, or in regular sight of charted landmarks, whatever techniques were available, the longitude need not be, and was not, pursued with precision. To follow the journal of a trading ship closely, to travel vicariously with her crew, passengers and cargo, is to enter a world in which judgment and skill guided the *selective* pursuit of precision. That ships' officers only became serious about longitude (and other) observations at key junctures is confirmed by recent systematic studies of observations of all types from East Indiamen logs. These show that observations were most commonly made when turning east after the long swing west in the Atlantic, when approaching and leaving the Cape of Good Hope and when approaching the Indian coast or parts further east.[65]

Company surveying activities and longitude

Getting to India by sea involved more than selection of a general route. Potentially hazardous shorelines and shoals along that route had to be avoided. A voyage might also involve finding safe harbour, procuring land-based resources and even, in certain periods, avoiding or confronting enemy shipping. Thus, the charting and measurement of longitude acquired strategic foci. One such focus was the 1,500-mile chain of islands and atolls making up the Laccadives, Maldives, and Chagos Archipelago, known in the Arab world as the 'eleven thousand islands'.

An early Company survey activated both by concern for secure navigation and imperial competition was that of Chagos Island (called Diego Garcia by the French) and its archipelago, conducted in 1786 by Lieutenant Archibald Blair.[66] Blair established a meridian at Flag-Staff Point on Diego Garcia, from which he measured longitude by chronometer as he sailed between the archipelago's islands. Navigation was conducted by setting courses by compass, by dead reckoning and by measuring latitudes. Occasionally Blair took opportunity in rare episodes of clear weather to make observations of Jupiter's satellites to determine longitude on Diego Garcia and other islands.[67] We see from this that Company surveyors early settled on the basic strategy of chronometer use in maritime survey. As if to demonstrate the importance of accurate determination of longitudes, the East Indiaman *Atlas* (with the young James Horsburgh as first mate) was wrecked on Chagos Island just in time for Blair's surveying party to rescue the crew. The navigators on the *Atlas*, working by dead reckoning, had thought that they were 4 degrees further east.[68]

In other early surveying activity, sometime before 1787 Joseph Huddart, commanding the *Royal Admiral*, carried a set of chronometers down the coast from Bombay to Anjengo and back.[69] From 1787 to 1790 Lieutenant John McCluer (alternatively McClure), under orders from the Directors of the Company, surveyed the Indian coast southwards from Bombay. The plan was for a detailed chronometer survey, and the instructions required that he take 'altitudes for determining the time by chronometer every hour'.[70] During the survey, in the *Experiment*, McCluer found the timepieces supplied by the Company – a box chronometer and two watches by Arnold – unsatisfactory and he determined longitudes by lunars.[71] The Company also required McCluer to survey the Laccadive Islands and the banks between them and the Malabar Coast, and to establish the relative positions of the Laccadives, Malicoy and the Head of the Maldives.[72] The Company was making a significant investment in accurate surveys of the coast and its approaches, and showing considerable enthusiasm for, and faith in, chronometers.

Another focus of Company surveying was the eastern seas beyond India in relation to the China trade. There is little point in detailing that surveying. Suffice it to quote Captain Basil Hall who offered this retrospective estimation:

> The East India Company have the sole merit [...] of having originated the splendid idea of surveying in a scientific manner, not only the vast seas and coast of China, but all the straits, bays and islands in the Indian ocean and Malay Archipelago. This work, perhaps the most useful, and certainly the greatest, of its kind that any nation ever undertook, has been steadily carried on at an enormous expense for many years[73]

Conclusions

Instrument-centric accounts of the history of the determination of longitude at sea offer a shorthand approach in recounting the development of navigation, and are tempting fodder for heroic accounts of technological change. But they are profoundly misleading. I have demonstrated some of the ways they mislead us as far as longitude determination by East Indiamen is concerned. This was a situated activity dependent upon complex networks linking hardware, software and wetware at a given place and time. Throughout, making seaborne *and land-based* astronomical observations was a crucial part of the process of determining longitude 'in the wild'. Survey on land and sea were crucially linked by the reference of position by East Indiamen and maritime survey ships to the longitudes of Indian observatories, notably Madras and Bombay. The rating of chronometers by those same observatories strengthened that link. A later generation of scientific marine surveyors saw their predecessors' promiscuous mixing of opportunistic astronomical observations and relative meridian distances as an obstacle to truly scientific longitude determination by chronometer.[74] We might judge it differently, as a necessary, and productive, outcome of the then current and complex state of the longitude networks on land and sea in which East India Company venturers invested, and to which they entrusted, so much.

I have also emphasized the point that exactly how those networks were drawn upon in a given longitude determination depended upon on-the-spot judgments of what was necessary and possible given immediate purpose and conditions. East Indiamen pioneered the regular use of chronometers as part of their armoury for longitude observation.

Dalrymple and Horsburgh, successive Hydrographers to the Company, sought to shape the practice of everyday navigation of the Company's fleet by promoting the use of chronometers and record keeping in ways that enabled the production of more accurate charts. However, whether longitude by chronometer was taken, whether it was relied upon and to what extent, whether it was part of a process of short runs from a longitude established astronomically, and so on, was a contingent matter. Those parts of the longitude networks that built and maintained the skills of East Indiamen officers were also a serious concern of the Company. The Court of Directors conscientiously tried to ensure that officers were well schooled in methods of longitude determination and certified as such. However, the navigator employed techniques that were practicable and that sufficed rather than those that were ideal from a theoretical, hydrographic or administrative viewpoint. What techniques to employ, and how to employ them were, in short, practical choices 'in the wild'.[75]

Notes

Thanks to Rebekah Higgitt, Richard Dunn and Simon Schaffer for the opportunity to develop this essay, the organizers of and contributors to the Huntington Library conference 'Oceanic Enterprises' for helping me clarify my ideas and Simon Werrett for 'the wild'.

1. W. E. May, 'How the Chronometer Went to Sea', *Antiquarian Horology*, 9 (1976), 638–63 (p. 643), drawing on John McCluer, *Description of the Coast of India* (London: William Ballintine, 1806), pp. iii–iv.
2. Thomas B. Jervis, speech to Section C of the British Association for the Advancement of Science, in *Transactions of the Bombay Geographical Society*, 4 (1840), 158–89 (p. 168).
3. Dava Sobel, *Longitude: The True Story of a Lone Genius who Solved the Greatest Scientific Problem of His Time* (New York: Walker, 1995); David Philip Miller, 'The Sobel Effect', *Metascience*, 11 (2002), 185–200. Jim Bennett, 'The Travels and Trials of Mr Harrison's Timekeeper', in *Instruments Travel and Science: Itineraries of Precision from the Seventeenth to the Twentieth Century*, ed. by Marie-Noëlle Bourguet, Christian Licoppe and H. Otto Sibum (London and New York: Routledge, 2002), pp. 75–95, shows how Harrison's critics maintained skepticism that he had 'discovered' the longitude.
4. Jonathan Betts, 'Arnold and Earnshaw: The Practicable Solution', in *The Quest for Longitude*, ed. by William J. H. Andrewes (Cambridge, MA: Harvard University Collection of Historical Scientific Instruments, 1996), pp. 311–28. As an antidote to the characterization of Maskelyne as villain, see Rebekah Higgitt (ed.), *Maskelyne: Astronomer Royal* (London: Robert Hale, 2014); Derek Howse, *Nevil Maskelyne: The Seaman's Astronomer* (Cambridge: Cambridge University Press, 1989).

5. The value of the solution of the longitude problem has been questioned on the grounds that the paramount safety problem was that ships were at the mercy of wind and weather. From this perspective, mechanical propulsion systems were more important than marine chronometers. See William E. Carter and Merri S. Carter, 'The Age of Sail: A Time when the Fortunes of Nations and Lives of Seamen Literally Turned with the Winds Their Ships Encountered at Sea', *Journal of Navigation*, 63 (2010), 717–31. A more moderate treatment is D. F. H. Grocott, 'Shipwrecks in the Revolutionary and Napoleonic Eras, 1793–1815. Causal Factors and Comments', *Journal of Navigation*, 52 (1999), 149–62.
6. Felix Driver and Luciana Martins, 'Visual Histories: John Septimus Roe and the Art of Navigation, *c.*1815–1830', *History Workshop Journal*, 54 (2002), 144–61 (pp. 145–46). An overview of the technicalities involved in 'traversing the trackless oceans' is in A. R. T. Jonkers, *Earth's Magnetism in the Age of Sail* (Baltimore: Johns Hopkins University Press, 2003), pp. 132–46.
7. Driver and Martins, 'Visual Histories', p. 146.
8. Edwin Hutchins, *Cognition in the Wild* (Cambridge, MA: MIT Press, 1995), pp. 114–15.
9. Andrew S. Cook, 'Alexander Dalrymple and John Arnold: Chronometers and the Representation of Longitude on East India Company Charts', *Vistas in Astronomy*, 28 (1985), 189–95 (p. 189).
10. W. E. May, 'The Log-books used by Ships of the East India Company', *Journal of Navigation*, 27 (1974), 116–18.
11. Jean Sutton, *Lords of the East: The East India Company and its Ships (1600–1874)*, 2nd edition (London: Conway Maritime Press, 2000), pp. 87–93.
12. Phillip Arnott, 'Chronometers on East India Company Ships 1800 to 1833', *Antiquarian Horology*, 30 (2007), 481–500; May, 'How the Chronometer Went to Sea', pp. 641–43.
13. May, 'How the Chronometer went to Sea'.
14. J. A. Bennett, *The Divided Circle. A History of Instruments for Astronomy, Navigation and Surveying* (Oxford: Phaidon/Christie's, 1987), p. 179.
15. Richard Owen, 'Essay on Chronometers', prefixed to W. F. W. Owen, *Tables of Latitudes and Longitudes by Chronometer of Places in the Atlantic and Indian Oceans [...] Resulting from the Observations of HMS Leven and Barracouta in the years 1820 to 1826* (London: Hydrographical Office, the Admiralty, 1827), p. 2. James Horsburgh made a similar point in *Memoirs: Comprising the Navigation to and from China, by the China Sea* (London: Privately printed, 1805), pp. v–vi.
16. Susanna Nockolds, 'Early Timekeepers at Sea', *Antiquarian Horology*, 4 (1963), 110–13, 148–52 (p. 150 describes the procedure).
17. The complexities of chronometer care, management and use can be seen in Owen, 'Essay on Chronometers'.
18. Nockolds, 'Early Timekeepers at Sea', p. 149. For the case for least disturbance of chronometers see Joseph Whidbey, 'Remarks on Timekeepers, the Compass &c', *Naval Chronicle*, 2 (1799), 505–12.
19. Richard Sorrenson, 'The Ship as a Scientific Instrument in the Eighteenth Century', *Osiris*, 11 (1996), 221–36.
20. Ibid., p. 229, drawing on Bruno Latour, whose conception was that scientific explorers aboard a ship turn it 'into the inked needle of an instrument of

enormous proportions that scribbles the shape of [countries] [...] in London, Paris or Den Hagen'; Bruno Latour, 'The Force and Reason of Experiment', in *Experimental Inquiries*, ed. by Homer LeGrand (Dordrecht: Kluwer, 1990), pp. 49–80 (p. 56).

21. John Robertson, *Elements of Navigation* (London: 1754), as discussed in J. B. Hewson, *A History of the Practice of Navigation* (Glasgow: Brown, Son & Ferguson, 1951), pp. 242–43.
22. Frank Scott, 'Speed, Navigational Accuracy and the "Ship Log"', *Mariner's Mirror*, 92 (2006), 477–81 (p. 478).
23. See Jane Wess's essay in this volume; Andrew Mackay, *The Complete Navigator*, US edition (Philadelphia: B. B. Hopkins & Co., 1807), pp. 169–70. See also Nockolds, 'Early Timekeepers at Sea', p. 149. Texts divided on this question depending upon their stance on the reliability of chronometric methods. William Wales, for instance, was more positive in *The Method of Finding the Longitude at Sea by Time-Keepers* (London: Privately printed, 1794).
24. For this view of technology, see Brian Wynne, 'Unruly Technology: Practical Rules, Impractical Discourses and Public Understanding', *Social Studies of Science*, 18 (1988), 147–67.
25. Scott, 'Speed', p. 478.
26. William Walker, *The Magnetism of Ships, and the Mariner's Compass* (London: Piper Brothers & Co, 1853), pp. 87–88.
27. Henry Raper, *The Practice of Navigation and Nautical Astronomy*, 2nd edition (London: R. B. Bate, 1842), p. xi.
28. Howard T. Fry, *Alexander Dalrymple (1737–1808) and the Expansion of British Trade* (London: Frank Cass & Co Ltd., 1970); Andrew S. Cook, 'Alexander Dalrymple (1737–1808), Hydrographer to the East India Company and the Admiralty, as Publisher: A Catalogue of Books and Charts', 3 vols (unpublished doctoral thesis, University of St Andrews, 1993).
29. Cook, 'Alexander Dalrymple', I, 71, 86, 110, 241. Discovering the actual behavior of navigators of East Indiamen relies on a systematic examination of the logs, and even then involves interpretation. On the divergence between theory and practice in East Indiamen navigation, see Henry Harries, 'Pre-Greenwich Sea Longitudes', *The Observatory*, 50 (1927), 315–19.
30. Susanna Fisher, *The Makers of the Blueback Charts. A History of Imray Laurie Norie & Wilson Ltd* (St Ives: Imray Laurie Norie & Wilson, 2001), p. 12.
31. The following account relies on: R. H. Phillimore, *Historical Records of the Survey of India,* 5 vols (Dehra Dun: Survey Printing Group, 1945–62), I, 5–6, 101–102, 171–74; R. K. Kochhar, 'Madras Observatory: The Beginning', *Bulletin of the Astronomical Society of India*, 13 (1985), 162–68; R. K. Kochhar, 'Growth of Modern Astronomy in India 1651–1960', *Vistas in Astronomy*, 34 (1991), 69–105; S. M. Razaullah Ansari, 'Early Modern observatories in India 1792–1900', in *Science and Modern India: An Institutional History c.1784–1947*, ed. by Uma Dasgupta (Delhi: Pearson Education India, 2010), pp. 349–80.
32. Topping (*c.*1747–96) was sent to India at Dalrymple's initiative; Susan Hots, 'Topping, Michael', in *A Biographical Dictionary of Civil Engineers in Great Britain and Ireland*, ed. by A. W. Skempton et al. (London: Thomas Telford, 2002), pp. 711–12.
33. Quoted in Phillimore, *Historical Records*, I, 173.

34. Matthew H. Edney, *Mapping an Empire: The Geographical Construction of British India* (Chicago: University of Chicago Press, 1997), pp. 88–90.
35. See, for example, James Horsburgh, *Memoirs: Comprising the Navigation to and from China by the China Sea, and through the Various Straits and Channels in the Indian Archipelago* (London: Privately printed, 1805).
36. Nockolds, 'Early Timekeepers at Sea', pp. 151–52; John Warren, *An Account of Some Experiments made on Three Chronometers to Ascertain how far Motion [...] Does Affect their Rate of Going* (Madras, 1807).
37. Phillimore, *Historical Records*, II, 196.
38. May, 'How the Chronometer Went to Sea', p. 643, notices that Dalrymple quotes a letter from George Robertson, first mate of the *General Coote*, reporting that the watches 'performed wonderfully' during a voyage of 1787–89. One of them was landed at Madras Observatory and M. Petré [sic] declared it the best he had ever seen.
39. On the early Bombay Observatory, see Simon Schaffer, 'The Bombay Case: Astronomers, Instrument Makers and the East India Company', *Journal of the History of Astronomy*, 43 (2012), 1–30; Clements R. Markham, *A Memoir of the Indian Surveys*, 2nd edition (London: W. H. Allen & Co, 1878), p. 23; Ansari, 'Early Modern Observatories', p. 357.
40. On imperial significances of astronomical ventures in India, South Africa and St Helena, see John McAleer, '"Stargazers at the world's end": Telescopes, Observatories and "views" of Empire in the Nineteenth-Century British Empire', *British Journal for the History of Science*, 46 (2013), 389–413.
41. I have not seen an important account of the training of Company surveyors: Andrew S. Cook, 'The Training of East India Company Surveyors in the Early 19th Century', presented to the 10th International Conference on the History of Cartography, 29 August–2 September 1983. On navigational training generally, see Edwin Charles Millington, *Seamen in the Making. A Short History of Nautical Training* (London: J. D. Potter, 1935), pp. 48–60. On the Royal Navy, see Harry W. Dickinson, *Educating the Royal Navy: Eighteenth- and Nineteenth-Century Education for Officers* (London: Routledge, 2007), especially pp. 9–56.
42. See Anthony Farrington, *A Biographical Index of East India Company Maritime Service Officers, 1600–1834* (London: The British Library, 1999).
43. The following is based on 'The Commanders', in Sutton, *Lords of the East*. A detailed account of the education and certification of officers of the Dutch East India Company in longitude determination strongly parallels the British case: J. R. Bruijn, *Commanders of Dutch East India Ships in the Eighteenth Century* (Woodbridge: Boydell Press, 2011), pp. 175, 284–85, 292–93.
44. Sutton, *Lords of the East*, pp. 62–63.
45. N. Plumley, 'The Royal Mathematical School within Christ's Hospital. The Early Years', *Vistas in Astronomy*, 20 (1976), 51–56; E. H. Pearce, *Annals of Christ's Hospital* (London: Methuen, 1901), Chapter 4. In our period, William Dawes and William Wales held the Mastership, both bringing considerable practical experience. On Wales, see Wayne Orchiston and Derek Howse, 'From Transit of Venus to Teaching Navigation: The Work of William Wales', *Journal of Navigation*, 53 (2000), 156–66.
46. A fourth mate had to be 20 and have completed a voyage of three years as a midshipman. A third mate had to have completed two voyages and be at least 21, while a second mate must have completed a voyage as third or

fourth mate and be at least 22. The minimum age for Commander was 25; Millington, *Seamen in the Making,* p. 90; Evan Cotton*, East Indiamen. The East India Company's Maritime Service* (London: Batchworth Press, 1949), pp. 23–24.

47. British Library (hereafter BL), India Office Records, L/MAR/C/1; Sutton, *The East India Company's Maritime Service*, pp. 122, 132 note 21.
48. 'Description of Commanders and Mates Examined by the Committee of Shipping', India Office Records, Marine Records Miscellaneous, BL IOR/L/MAR/C/652 (covers 1770–78); IOR/L/MAR/C/653 (1778–82); IOR/L/MAR/C/654 (1783–87); IOR/L/MAR/C/655 (1788–94); IOR/L/MAR/C/656 (1795–98).
49. Examples signed by Thomas Lynn can be seen at BL IOR L/MAR/C/671.
50. Robert Bishop, *The East India Navigator's Daily Assistant; with the New Method of Computing the Longitude* (London: Printed for the Author, 1773) was dedicated to the Directors of the Company. On Bishop, see Howse, *Nevil Maskelyne*, pp. 93–95, and the summary by Megan Barford in 'Board of Longitude' <cudl.lib.cam.uk/view/MS-RGO-00004–00155/1> [accessed 9 April 2015].
51. Howse, *Nevil Maskelyne*, p. 121, and Wess, this volume. Dickinson, *Educating the Royal Navy*, pp. 44–45, suggests that the guarding by Navy officers of their powers of patronage in the promotion of lower ranks may explain resistance to 'academic' land-based instruction and certification. We might add that, to the extent that complex mathematical methods lent themselves only to 'academic' instruction they would be a focus for resistance of this sort.
52. Charles H. Cotter, 'A Brief Historical Survey of British Navigation Manuals', *Journal of Navigation*, 36 (1983), 237–49 (pp. 244–45). On Moore, see Susanna Fisher, *Makers of the Blueback Charts*, pp. 19–27.
53. John Hamilton Moore, *The Practical Navigator*, 9th edition (London: 1791), pp. 272–95. Questions and model answers were designed for candidates to 'refresh their Memories, previous to that Examination which they must pass through, before they are appointed to a Commission in the Royal Navy, or an Officer in the East India Service' (p. 272).
54. Cook, 'Alexander Dalrymple', I, 239–45.
55. 'Qualifications of Commanders and Mates of Ships, established by the Honorable Court of Directors, 24th of January, 1804', appended to Charles Hardy and H. C. Hardy, *A Register of Ships Employed in the Service of the Honorable the United East India Company, from the Year 1760 to 1810 with an Appendix* (London: Black, Parry and Kingsbury, 1811), pp. 112–13 (p. 113).
56. Quotations taken from 'Extracts from the Instructions to a Commander; which particularly relate to the Officers, Surgeon, and Purser, and intended for their Information', appended to Hardy and Hardy, *A Register of Ships*, pp. 81–91 (pp. 82, 88, 89).
57. Controversy existed from the early 1790s about the length of time over which the rate of chronometers had to be checked to ensure reliable use on long voyages. The view that rates could be established quickly, making chronometers a reliable way of determining longitude, prevailed. William Wales cited the facility with which Company officers used chronometers and determined their rates as evidence for it: 'the business of finding the rate [...] has so little of what is really difficult in it, that some of the officers in the East India Company's service constantly found the rates of their time-keepers

themselves, without any thing having been written expressly concerning it'; Wales, *The Method of Finding the Longitude*, pp. iii–iv.

58. Bennett, *The Divided Circle*, p. 179.
59. *A Narrative of the Loss of the* Winterton *on her Passage to India* (London, 1795); Jean Hood, *Marked for Misfortune: An Epic Tale of Shipwreck, Human Endeavour and Survival in the Age of Sail* (London: Conway Maritime, 2003); Pierre Van Den Boogaerde, *Shipwrecks of Madagascar* (Strategic Book Publishing, 2008), pp. 97–106.
60. Hood, *Marked for Misfortune*, 3.
61. Nockolds, 'Early Timekeepers at Sea', pp. 110–11.
62. Cook, 'Alexander Dalrymple and John Arnold'; Andrew Cook, 'Establishing the sea-routes to India and China', in *The Worlds of the East India Company*, ed. by H. V. Bowen, Margarette Lincoln and Nigel Rigby (Woodbridge: Boydell, 2002), pp. 119–36.
63. Journal of the *Royal Admiral*, BL IOR L/MAR/B/338A.
64. Journal of the *General Coote*, BL IOR L/MAR/B/441/A-C.
65. Eric Freeman, Tom Ross, Philip Brohan and Clive Wilkinson, 'English East India Company Logbooks – Significant Contributions to History and Science' <ftp://ftp.wmo.int/Documents/PublicWeb/amp/mmop/documents/JCOMM-TR/J-TR-59-MARCDAT-III/ppts/E3-Freeman-EIC.pdf> [accessed 9 April 2015]. See also Clive Wilkinson, 'The Non-Climatic Research Potential of Ships' Logbooks and Journals', *Climatic Research*, 73 (2005), 155–67.
66. H. T. Fry, 'Early British Interest in the Chagos Archipelago and the Maldive Islands', *Mariner's Mirror*, 53 (1967), 344–49.
67. Archibald Blair, *Remarks and Observations in a Survey of the Chagos Archipelago by Lieutenant Archibald Blair, 1786 and 1787* (London: George Bigg, 1788).
68. Remarkably, the *Atlas* had strayed more than 1,000 miles from its course. Horsburgh later recounted: 'The charts on board were very erroneous in the delineation of the Chagos Islands and Banks; and the Commander trusting too much to dead reckoning [...] unfortunately, a cloud over Diego Garcia prevented the helmsman from discerning it (the officer of the watch being asleep), till we were on the reef close to the shore [...] We had been set 4° to the westward of account in the passage from Bencoolen of 20 days'; James Horsburgh, *The India Directory,* 6th edition (London: W. H. Allen, 1852), I, 188, note; Cook, 'Establishing the Sea Routes', p. 135.
69. Phillimore, *Historical Records*, I, 176.
70. Ibid., I, 124; Andrew C. F. David, 'McCluer, John (1759?–1795), *Oxford Dictionary of National Biography* (Oxford: Oxford University Press, 2004), online edition 2007 <http://www.oxforddnb.com/view/article/17389> [accessed 9 April 2015]; Andrew Dunlop, *Memoirs of a Bombay Mariner: Being the Story of Captain John McClure of the Bombay Marine* (Salisbury, Rhodesia: M. O. Collins, 1975).
71. Phillimore, *Historical Records*, I, 203.
72. Ibid. I, 124.
73. Quoted in L. S. Dawson, *Memoirs of Hydrography Including Brief Biographies of the Principal Officers who have Served in H.M. Naval Surveying Service between the Years 1750 and 1885. Part I: 1750–1830* (Eastbourne: Henry W. Keay, 1883), pp. 73–74. On surveying in the eastern seas, see Thomas Forrest, *A Voyage to New Guinea, and the Moluccas from Balambangan [...] Performed in the* Tartar *Galley, belonging to the Honourable East India Company, During the Years 1774,*

1775 and 1776, 2nd edition (London: G. Scott, 1780) and Dunlop, *Memoirs of a Bombay Mariner*. For the political and strategic context, see H. T. Fry, 'Alexander Dalrymple and New Guinea', *Journal of Pacific History*, 4 (1969), 83–104.

74. For example, Henry Raper, *The Practice of Navigation,* and Charles F. A. Shadwell, *Notes on the Management of Chronometers and the Measurement of Meridian Distances,* (London: J.D. Potter, 1855), pp. 2–3. Both single out Horsburgh's '*East India Directory*' as containing numerous instances of this defective mode of proceeding.
75. Patricia Fara makes this point well, in a different way, in *Sympathetic Attractions: Magnetic Practices, Beliefs and Symbolism in Eighteenth-Century England* (Princeton: Princeton University Press, 1996), pp. 69–77, which highlights the contingency of navigational practices and the tensions between theoretical, practical and pragmatic navigation. W. E. May, *A History of Marine Navigation* (Henley-on-Thames: G.T. Foulis & Co., 1973), pp. xiii, 19, concedes, in passing, the importance of the 'human factor', and our ignorance about how ships were actually navigated since manuals, written by mathematicians, indicated 'what *could* be done under ideal conditions rather than what it would have been convenient to do in practice'.

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