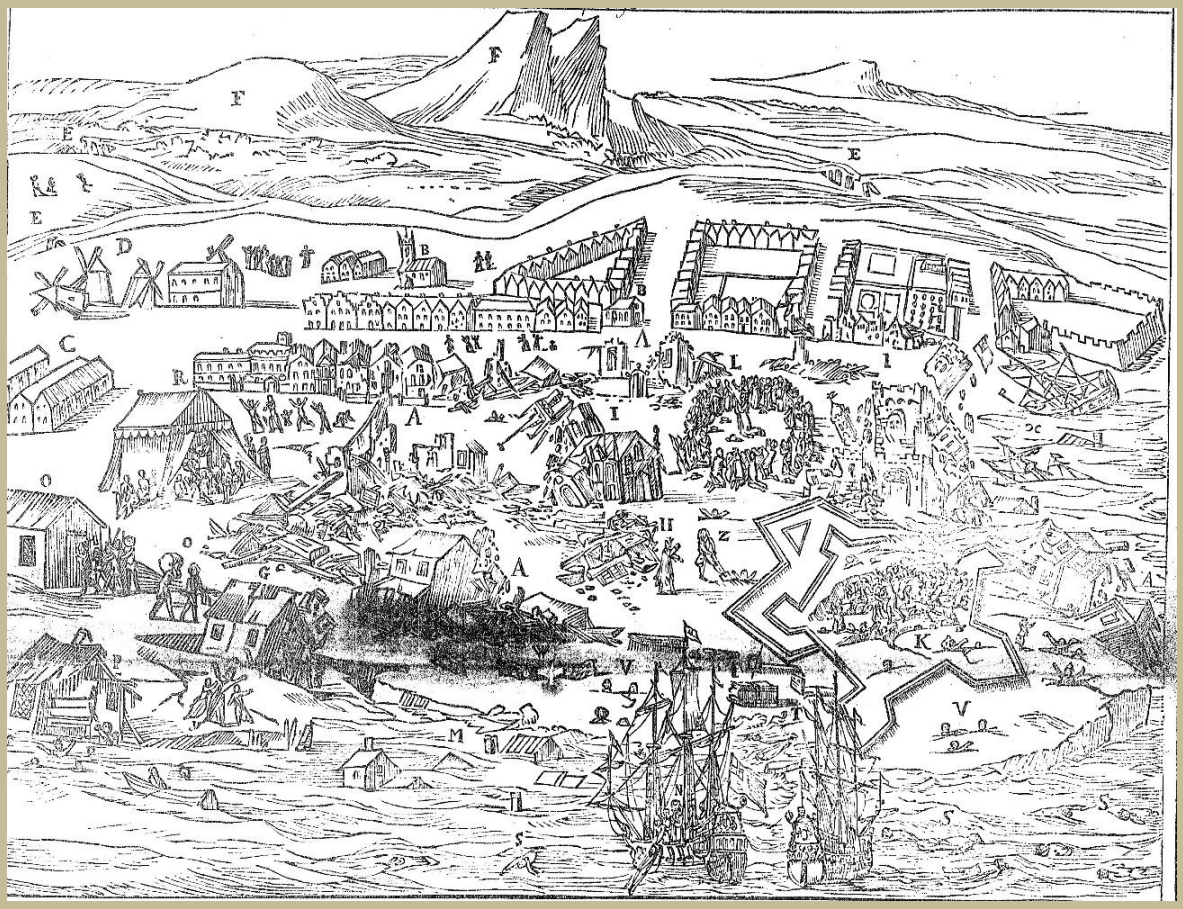
Diligent observers of natural things

Lay observations and the natural philosophy of earthquakes in the Royal Society of London 1665-1755



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Abstract

Eighteenth-century seismology primarily relied on lay observers to provide empirical evidence. This methodological commitment did not come out of nowhere. Since the mid-seventeenth century, the testimonies of contemporary earthquake observers became increasingly prominent sources of knowledge for natural philosophers. Their observations, as well as the specific lay-expert relation that formed as the result of this interaction formed the building blocks of eighteenth- and nineteenth-century seismology. The aims of this thesis are twofold. First, to tell the untold stories of these observers and evaluate their contribution to early modern earthquake science, taking the early *Royal Society* as a focus point. This historical argument serves to explain how and why seismology emerged in the eighteenth century with the specific epistemological and theoretical commitments that it had. Secondly, to develop an analytical method informed by the 'history of knowledge' that integrates different epistemologies, social relations and scientific theories. This method is geared to explain how the interactions between specific actors and practices shaped new knowledge about earthquakes in ways that transcend modern disciplinary classifications.

Acknowledgements

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I dedicate this to my mother.

Clarifications

Note on dating and names.

In Great Britain the Julian calendar remained in use until 1752. In the text I have kept the dating consistent with the sources. This means that English references generally refer to Julian or Old Style dates, while continental or post-1752 dates refer to the Gregorian or New Style dates. In cases where this may lead to confusion, I have explicitly noted both calendar dates.

I have maintained proper names according to contemporary spelling. In some cases where geographical names have changed I have added the modern name between brackets.

Commonly used abbreviations

F.R.S.- Fellow of the Royal Society

R.S.A.- Royal Society Archives

Phil. Trans.- Philosophical Transactions of the Royal Society

Terminology

While the words will occasionally rear their heads in the text, I will generally refrain from relying too much on the terms 'science' and 'scientific revolution', since these are rather anachronistic when applied to the late seventeenth and early eighteenth century. Instead, I will follow contemporary convention and refer to *natural philosophy* (roughly analogous to 'theoretical') and *natural history* (analogous to 'empirical').[1] The practitioners of this form of inquiry will be referred to as *natural philosophers*, *naturalists* and *earthquake theorists* to reflect both their general and specific interests. Doing so partly negates the risk of imposing nineteenth or twenty-first century classifications of science upon an enterprise that appears similar but had fundamental differences.

On the other hand, I will deliberately use the similarly anachronistic terms 'lay', 'expert', 'gender', 'race' and 'class' as analytic tools to trace patterns and connections that would not have been obvious to historical actors (or in different terms) but are of interest to modern historians.

[1] Daston, Lorraine & Park, Katherine - 'Introduction: The Age of the New', in : Daston, Lorraine; Park, Katharine (Eds.)- The Cambridge history of science. Vol. 3, Early modern science (Cambridge University Press, 2006), 1-20. p. 4, also 12-16.

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Introduction

In 1666, the first earthquake was reported to the recently established *Royal Society of London for Improving Natural Knowledge*.[1] It was a minor shock that hit a major center of English learning: the university town of Oxford. The reporters were two eminent members of the Royal Society itself: the mathematician John Wallis and the chemist Robert Boyle. Wallis's account was arguably the first recorded observation of an earthquake that included instrumental data (he noted the state of his thermometer and barometer), while Boyle wrote his account to provide independent confirmation of Wallis' observations.[2] A straightforward case of the improvement of natural knowledge, you might say. Yet upon closer reading, something strange comes to light: *neither of these two men actually observed the earthquake*. Although Wallis noticed 'some kind of odde shaking or heaving', he apparently thought nothing of it.[3] Only after hearing others speak of an earthquake did he consult his daily meteorological notes 'to see if any alternations considerable had then happened'. Robert Boyle confessed that he had been 'busied enough on other matters', and that he would not have known of the earthquake at all if 'one, that you know, whose hand is employed in this paper, and begins to be a diligent observer of natural things, had not advertis'd me of it; as being taken notice of by him and the rest of the people of the house'.[4] In both cases, the actual observations of the earthquakes were made by the unnamed and barely acknowledged apprentices and servants in the households of these two gentleman scientists. It is their contributions that this thesis investigates.

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[1] *Phil. Trans., Vol. 1, no. 10*, pp. 166-171; *Phil. Trans., Vol. 1, no. 11*, pp. 179-183. According to the old style calendar the earthquake occurred on 19 January 1665. This corresponds to 29 January 1666 in the Gregorian calendar.

[2] See: Roger Musson, 'A history of British seismology', in: *Bull Earthquake Eng., Vol. 11, 715–861* (2013), p. 729: 'They also mark what is almost certainly the first ever attempt to gather instrumental data in an earthquake investigation'. See also: Oldenburg to Boyle 1 february 1667/8, asking Boyle to confirm Wallis' observation, in: Alfred Rupert Hall & Marie Boas Hall (Eds.), *The Correspondence of Henry Oldenburg* (Taylor and Francis, 1986), Vol. III, p. 48, Vol. IV, p. 170.

[3] Wallis, *Phil. Trans., Vol. 1, no. 10*, p. 166.

[4] Boyle, *Phil. Trans., Vol. 1, no. 11*, p. 180.

Little more than a year later, on 27 June 1667, another (former) apprentice of Boyle named Robert Hooke proposed an ambitious research agenda to the members of the Royal Society: the systematic study of earthquakes. 'The subject is large', Hooke wrote, 'and doth look very like an impossibility to be undertaken even by the whole world, to be gone through within an age, much less to be undertaken by any particular society, or a small number of men.'[5] In opposition to the highly speculative theories on the shape and history of the earth that were floating around in mid seventeenth-century Europe, Hooke's proposed methodology was explicitly empirical. The central problem of this endeavor was a lack of quality observations. Recognizing this fact, Hooke not only argued that those interested in earthquakes should employ a wide range of methods to gather evidence; he also postulated that this was chiefly an age to collect observations (guided by working hypotheses), so that future generations may use their cumulative accounts as an empirical basis for their theories:

> The number of Natural Histories, Observations, Experiments, Calculations, Comparisons, Deductions and Demonstrations necessary thereunto, seeming to be incomprehensive and numberless: And therefore a vain Attempt, and not to be thought of till after some Ages past in making collections of Materials for so great a Building, and the employing a vast number of Hands in making this Preparation.[6]

Although Hooke's own theory on the nature of earthquakes did not have a strong impact on his contemporaries, his methodological reflections seem to have been widely shared.[7] In the following decades, natural philosophers increasingly shared letters, newspaper clippings and eye-witness reports of earthquakes. By the 1750s this trend had reached its zenith. Following the 'year of earthquakes' in 1750, and the destruction of Lisbon in 1755, several large histories of earthquakes and compilations of eye-witness accounts rolled out of London print shops.[8] The in-house publication of the Royal Society, the *Philosophical Transactions*, even released two 'special issues' dedicated to earthquakes.[9] The reports contained within were truly the product of 'a vast number of hands' from a

[5] Robert Hooke, 'Lectures and discourses of earthquakes, and subterraneous eruptions', in: Samuel Smith, Benjamin Walford (eds.) , *The Posthumous Works of Robert Hooke 1668-1700*. (London, 1705), p. 279. For the dating of this lecture see: Rhoda Rappaport , 'Hooke on Earthquakes: Lectures, Strategy and Audience', in: *The British Journal for the History of Science, Vol. 19 (2), 129-146* (1986), p. 144.
[6] Hooke, 'discourses of earthquakes', p. 279.
[7] Rappaport, 'Hooke on Earthquakes', p. 129. Hans Sloane for instance prefaced his collection of accounts of the Port Royal earthquake in 1692 with the following words: 'considering they give account of different earthquakes, or that they contain differing observations of the same earthquake; and that we cannot have too many of the phenomena, or matters of fact accompanying them recorded, I think it will be best they be all preserved for future use'. Sloane, *Phil. Trans. Vol. 18, no. 209*, p.78.
[8] See for instance: Grey, *Chronological and Historical Account Of the most memorable earthquakes* (London, 1750); John Bevis (Ed.), *The History and Philosophy of Earthquakes* (London, 1760);
[9] These were: *Phil. Trans., Vol. 46, no. 497* (1750); *Phil. Trans., Vol. 49* (1756) [by 1756 the Philosophical Transactions appeared annually so there are only volume numbers].

wide variety of social backgrounds. Unlike the accounts of Wallis and Boyle, they included statements not only from English gentlemen philosophers, but also from their wives, their children, servants, neighbors and passers-by in the streets. Next to these we also find observations made by an Indonesian nobleman, Turkish merchants, enslaved Africans in the Americas and many other voices that are not generally included in the history of European science in the seventeenth and eighteenth centuries.[10]

These developments raise a number of important questions. First, what drove this widely shared commitment to such an unprecedented empirical enterprise in the study of earthquakes, and why did these naturalists come to rely principally on the observations of 'lay persons'? Second, how did early earthquake theorists evaluate and use the observations made by people who, due to preconceptions about their gender, race or class were frequently denied the status of credible observers, and how did these observers assert their own credibility? Third, what was the impact of these observations on the scientific theories concerning earthquakes in the seventeenth and eighteenth centuries? And fourth, how did the interaction between observers and theorists both challenge and re-establish the technical and social boundaries between the expert and the lay observer? In sum, the central question of this thesis is: *how did the changing relation between lay observers and earthquake philosophers shape the development of earthquake philosophy in the Royal Society between 1665 and 1755?* In this introduction I will discuss, in order, how this question engages with the historiography of early seismology, expertise, the early Royal Society, and the histories of science and knowledge.

Lay observations and the history of seismology

Given that the technological ability to reliably measure earthquakes has been a relatively recent development, most historical investigation of earthquakes has relied to varying degrees on human observers.[11] To date, the only systematic investigation into the role of lay

[10] Lorraine Daston , 'The History of Science and the History of Knowledge', in: *KNOW, Vol. 1*, no. 1 (2017), pp. 141-143; Pamela Smith, 'Science on the Move: Recent Trends in the History of Early Modern Science', in: *Renaissance Quarterly, Vol. 62* (2009), pp. 357-372; Fa-ti Fan, 'Science in Cultural Borderlands: Methodological Reflections on the Study of Science, European Imperialism, and Cultural Encounter', in: *East Asian Science, Technology and Society: an International Journal, Vol. 1*, (2007), p. 214-215; Suman Seth, 'Putting knowledge in its place: science, colonialism, and the postcolonial', in*: Postcolonial Studies, Vol. 12, no. 4* (2009), pp. 373-384.

[11] Deborah Coen , 'Introduction: Witness to Disaster: Comparative Histories of Earthquake Science and Response', in: *Science in Context, Vol. 25, no. 1* (2012). In addition: the 'seismoscope' was first invented by the Chinese polymath Zhang Heng in 132 BC. This device was incredibly accurate at registering earthquakes, but gave no additional information. The technology was presumed lost by the thirteenth century. In the eighteenth century, the Frenchman Jean de Hautefeuille fashioned another basic seismometer, although seismographs only really took off only in the nineteenth century. Zhang Heng's seismograph was only successfully reconstructed in the twentieth century.

observers in early seismology is Deborah Coen's *The Earthquake Observers*.[12] Several important themes from this work have helped me shape my analysis: a close attention to epistemological debates on the status of eye-witnesses, the processes of collection, the scientific impact of these observations and the social relations that underlay this knowledge making. Coen describes how early seismology tried to reconcile its Kantian program of providing objective knowledge on earthquakes with its reliance on the inherently subjective accounts of reporters.[13] The story of the *Earthquake Observers* is the story of the practices employed to make these observations *scientific*: standardized questionnaires, increasing numbers of data points, error margins, intensity scales and isoseismal maps. Seismologists collected and interpreted the accounts, and oversaw a grand army of observers. As a result, in the early decades of the nineteenth century the earthquake observers were at their height: 'Likely in no other field is the researcher so completely dependent on the help of the non-geologist', wrote the famous English seismologist Robert Mallet, reflecting both the optimism and anxiety about eye-witnesses, 'and nowhere is the observation of each individual of such high value as with earthquakes'.[14] Yet the story of *The Earthquake Observers* is also the story of how the earthquake observer gradually lost its status as a privileged source of knowledge on earthquakes, and was replaced with more readily quantifiable information amassed through observational instruments. The endpoint of this story is the development of the Richter scale, which reassured the field of seismology of having a non-subjective measure for the intensity of earthquakes.[15]

Yet as the book's subtitle (from Lisbon to Richter) indicates, Coen is concerned with the practice of earthquake observations only after the Lisbon earthquake of 1755. In doing so, Coen echoes the mythology of the field of seismology that the 'serious, scientific study' of earthquakes began only in 1755, when the destruction of Lisbon caused a major part of the established enlightenment canon (Voltaire, Leibniz, Rousseau, and most influentially, Kant) to pick up their pens and start writing about earthquakes.[16] According to Charles Davison, this was the first time theorists 'drew their illustrations from contemporary records and no

[12] Deborah Coen, *The Earthquake Observers. Disaster Science from Lisbon to Richter* (University of Chicago Press, 2013).
[13] Coen, *The Earthquake Observers* pp. 7-9.
[14] Robert Mallet, *The First Principles of Observational, Volume 1* (Seismology London: chapman and hall, 1862), P. 7. Quoted in: Coen, *The Earthquake Observers*, p. 3.
[15] Although Coen convincingly argues that 'the original purpose of Richter's scale was to avoid "misinterpretation" by the public of the comparative "importance" of earthquakes; only later did the goal emerge of achieving an "objective and instrumentally-founded" measure of relative intensity.', in: *The Earthquake Observers*, p. 260.
[16] See for instance: Duncan Agnew, 'History of Seismology', in: William Lee, Paul Jennings, Carl Kisslinger, Hiroo Kanamori (Eds.), *International Handbook of Earthquake & Engineering Seismology* (Elsevier, 2003), pp. 3-4; David Oldroyd et al., 'The study of earthquakes in the hundred years following the Lisbon earthquake of 1755', in: *Earth sciences history: journal of the History of the Earth Sciences Society* (2007); Coen, *The Earthquake Observers*, pp. 7-12.

longer from the writings of Aristotle, Seneca or Pliny'.[17] This idea was first proposed in the late 1750s, when the famous naturalist John Michell (the first to convincingly dismiss any meteorological origin of earthquakes) legitimized his empirical approach by stating that 'we are now the better enabled to do [this research], as the late dreadful earthquake of the 1st of November supplies us with more facts, and those better related than any other earthquake of which we have an account'.[18] Though this might seem like a closed case, Michell in fact only used a few observations of the Lisbon earthquake, and relied just as much on observations of earlier events, which he found in the *Philosophical Transactions.* Like other naturalists, Michell made use of the observations that had been collected since the late seventeenth century.[19] And if the practice of collecting observations preceded the events of 1755, then the old image of the birth of seismology is unhelpful in examining the rise of the earthquake observer and its scientific, social and epistemological implications.

This image has nonetheless shaped the historiography of earthquake studies, and despite a wealth of source material, eye-witness reports of earthquakes in the seventeenth and eighteenth centuries have remained a blind spot within this tradition. Most of the studies on pre-1755 earthquake scholarship have principally focused on individual theories as anomalies, or treated earthquakes as a relatively minor part in the development of geology as a whole.[20] While these are legitimate inquiries, they generally do not ask the same questions about method and epistemology that have been investigated after 1755. And while it is indisputable that the quantity of reports rose dramatically throughout the eighteenth and nineteenth centuries, these statements fail to explain *why* earthquake theorists would bother with such an epistemologically problematic category as eye-witness observations, and *why at that specific point in time?* Retelling the story of earthquake science from the rise of the earthquake observer reveals that rather than marking a radical break with past practices, the

[17] Charles Davison in *The Founders of Seismology* (Cambridge University Press, 2014. Original from 1927), p. 1.
[18] John Michell, *Conjectures concerning the cause and observations upon the phaenomena of Earthquakes* (London, 1760) pp. 3-4.
[19] See: Michell, *Conjectures*, pp. 4-7.
[20] In the first category, see: Frances Willmoth, 'John Flamsteed's letter concerning the natural causes of earthquakes', in: *Annals of Science, Vol.44, 23-70* (1987); Rhoda Rappaport, 'Hooke on Earthquakes: Lectures, Strategy and Audience', in: *The British Journal for the History of Science, Vol. 19 (2), 129-146* (1986). In the second category, see: Rhoda Rappaport, *When Geologists were Historians 1665-1750* (Cornell University Press, 1997); Gary Rosenberg (Ed.), *The Revolution in Geology from the Renaissance to the Enlightenment* (Geological Society of America, 2009); Walter Alvarez; Henrique Leitão, 'The neglected early history of geology: The Copernican Revolution as a major advance in understanding the Earth', in: *Geology, Vol.38 (3): 231-234* (2010). A notable exception is: Roger Musson, 'A history of British seismology', in: *Bull Earthquake Eng., Vol. 11, 715–861* (2013), pp. 727-742. This account discusses seventeenth century earthquake theories at length but is mostly concerned with detailing the development of their content rather than charting their methods of data collection. Another is: Jamie Rae Bluestone, *Why the Earth Shakes: Pre-Modern Understandings and Modern Earthquake Science* (Phd Thesis, University of Minnesota, 2010), which also does not pay any attention to the role of observers. Of course, the emergence of seismology was paired with other methodological innovations, such as an increasing reliance on visual representations. See: Susanne Keller, 'Sections and Views: visual representations in eighteenth-century earthquake studies', in: *The British Journal for the History of Science, Vol. 31* (1998), particularly pp. 129-141.

reports of the 1755 earthquakes were drawn up, collected and examined according to patterns that had been well established over the course of the preceding century.

Yet retelling the chronology of earthquake studies is not the primary goal of this thesis, much less than investigating *how, why* and *by whom* this natural philosophical enterprise was set up. From Coen's description of observer-based seismology we can distill a variety of processes that determined its particular nature. There were **theoretical factors,** such as looking for observations in relatively remote mountainous areas and along fault lines, and assumptions about the frequency, extent and intensity of earthquakes which directed seismologists where to look and which observations to trust. Secondly, there were **epistemological factors**, such as the post-Kantian philosophy which began to think in terms of subjective and objective knowledge, and the mathematicalization and visualization of empirical data. Lastly, there were **social factors**. Coen notes that 'seismic testimony typically came from common folk who were, in the eyes of the scientific elite, effectively anonymous'.[21] The implications are twofold. The use of standardized surveys equalized the testimonies of different observers before the eyes of seismologists, and social status was not a great barrier to some forms of scientific participation. On the other hand, this replaced what Coen has characterized as an 'inclusive conversation' with a hierarchy between accredited scientists and anonymous observers.[22]

Matters lay somewhat different by the late seventeenth century. Contemporary theories suggested entirely different places to look, and different accounts to trust.[23] Concerning epistemology, philosophers talked not of objective and subjective knowledge, but employed different dualities: sense versus reason, particulars versus universals, objects versus events, or certainty versus probability.[24] The individual circumstances of observers were also considered to be more important, and perhaps even defining, for the credibility of their account. A good earthquake theorist needed to be both a judge of seismology and a judge of character to distinguish between useful and useless observations.[25] Arguably then, earthquake observers in the seventeenth century were categorically different from their nineteenth-century successors. The story which needs to be told is the story of how these three factors (theory, epistemology, social relations) transformed the earthquake observer

[21] Coen, *The Earthquake Observers*, p. 41.
[22] Coen, *The Earthquake Observers*, p. 15.
[23] See: Musson, pp. 727-742. A more detailed analysis of this point can be found in chapters 2 and 3 of this thesis.
[24] Lorraine Daston & Peter Gallisonr, *Objectivity* (Third Edition, Zone Books, 2015), pp. 29-35; Kirsten Walsh, 'Newton: from certainty to probability', in: *Philosophy of Science, Vol. 84* (2017), pp. 866-867; More generally: Barbara Shapiro, *Probability and Certainty in Seventeenth-Century England: A Study of the Relationship between Natural Science, Religion, History, Law, and Literature* (Princeton University Press, 1983); Barbara Shapiro, *A Culture of Fact. England 1550-1720* (Cornell University Press, 2000), pp. 53-56; Harold Cook, *Matters of Exchange. Commerce, Medicine, and Science in the Dutch Golden Age* (Yale University Press, 2007), pp. 378-409.
[25] Steven Shapin, *A Social History of Truth: Science and Civility in seventeenth-Century England* (University of Chicago Press, 1994), pp. 243-309.

and the field of early earthquake studies in general. Telling this story reveals both how social and epistemological factors influence the production of scientific knowledge, and how the process of scientific inquiry shaped epistemological thought and social relations.

Expertise, authority and trust

The image of the seismologist looking down over a mass of observers has a history. The lens through which I look at this history will be the construction of expert-lay relations. These relations have become a pressing concern for historians, sociologists and philosophers of knowledge living in an age which has seen a sharp decline in the trust accorded to scientific authority.[26] One strand of recent scholarship has focused on the ways in which scientific authority and scientific knowledge have been received by societies. This is a particular relationship between an expert community and their lay *audience*. These studies focus for instance on science communication and on public perceptions of scientific discourses, practices and institutions. Their proposed solutions to the 'crisis of credibility' generally concern the popular (self)presentation of science.[27] Another group of studies focuses on the ways in which lay persons and scientists work together to create new knowledge. This includes groups who are essential to the modern institution of science but whose work has not generally been recognized as 'scientific', but also comprises broader calls for 'citizen science' or 'participatory science' as a democratic remedy against the supposed gap between scientific experts and the general population.[28]

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[26] Harry Collins, *Are We All Scientific Experts Now?* (Polity Press, 2014) speaks of a 'growing crisis of expertise', pp. 1-16; Philip Kitcher, *Science in a Democratic Society* (Prometheus, 2011) is concerned with the 'erosion of scientific authority', pp. 15-40. These works reflect a wider worry of a 'post-truth society', visible for instance in the British MP Michael Gove's infamous exclamation: 'the people in this country have had enough of experts'. Michael Gove, Sky News interview with Faisal Islam on 3 June 2016.

[27] See for instance: Augusti Nieto-Galan -*Science in the Public Sphere. A History of Lay Knowledge and Expertise* (Routledge, 2016), especially pp. 1-20; Lynda Walsh, *Scientists as Prophets: a rhetorical genealogy* (Oxford University Press, 2013); Willemijn Ruberg, 'Expertise en de moderne samenleving , Een ambivalente relatie', in: *Tijdschrift Voor Geschiedenis, Vol. 127, no.1* (2014), pp. 164-165; Joris Vandendriessche, Evert Peeters, Kaat Wils, *Scientists' Expertise As Performance : Between State and Society, 1860 – 1960* (Chatto & Pickering, 2015). In 2014 an official UK report 'Public Attitudes to Science' reported that while people generally trusted the ability of scientists, 'four-in-ten (40%) say scientists are poor at communicating and three-in-ten (28%) think this about engineers. Five-in-ten (50%) consider scientists to be secretive, while three-in-ten (31%) say this about engineers.' Moreover, 'there is a low level of trust in mainstream science reporting' Economic & Social Research Council, *Public Attitudes to Science 2014. Main Report* (March 2014), pp. 2, 167.

[28] Citizen Science is distinct from for 'participatory community research' as practiced in anthropology in sociology, where the participant is also the object of study. Citizen science generally involves relatively rudimentary but essential observational or computational tasks. See : Bruno Strasser; Jerôme Baudry; Dana Mahr; Gabriela Sanchez; Elise Tancoigne, '"Citizen Science"? Rethinking Science and Public Participation', in: *Science and Technology Studies, Vol. 30* (2018), which proposes the following typology of citizen contributions: 'sensing, computing, analyzing, self-reporting and making', pp. 5-6. See also: Sean Johnston, Benjamin Franks & Sandy Whitelaw, 'Crowd-Sourced Science. Societal engagement, scientific authority and ethical practice', in: *Journal of*

The model of citizen science is predicated on the modern view that the sphere of citizenship and the sphere of science are fundamentally detached from one another. It argues that despite this distance, there can be meaningful interaction between the two. While this *concept* is a relatively novel development, the *practices* of 'citizen science' are not novel. In the early modern period as well as now, science has never been the sole creation of those who have been recognized as 'scientists'. Steven Shapin already stressed the reliance of seventeenth-century scientists on the manual labor and observational work of apprentices and servants.[29] The role of women, particularly as observers and practitioners in early modern botany and medicine but also beyond, too has received appreciation in recent scholarship.[30] Other studies have pointed at the impact of colonialism and the material wealth of natural specimens that were brought to Europe for study, but also to the reliance of European naturalists on local classifications of plants and minerals, and the dependence of surveyors and cartographers on local knowledge of the land itself.[31] Less innocuous, the often unacknowledged labor of enslaved Africans and native populations in the collection of natural specimens and the observation of (often more dangerous) natural phenomena and experiments form an important part of this history, too.[32]

Needless to say, none of these early modern examples gave rise to any democratic form of 'citizen science'. Nor, however, did they take place in a discursive context with strongly pronounced distinctions between lay and expert, which only arose by the

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Information Ethics, Vol. 26, no.1 (2017), pp. 49-65. For an historiographical overview of early lay participation see: Jeremy Vetter, 'Introduction: Lay Participation in the History of Scientific Observation', in: Science in Context, Vol. 24, no. 2 (2011), pp. 172-141.

[29] Shapin, *A Social History of Truth*, pp. 335-408. See also: Peter Dear, 'Totius in verba. Rhetoric and Authority in the Early Royal Society', in: Peter Dear (Ed.), The Scientific Enterprise in early modern Europe (University of Chicago Press, 1997), pp. 262-263.

[30] This is but a brief selection of a vast historiography: Florike Egmond, *Observing Nature. The Correspondence Network of Carolus Clusius (1526–1609)*, in: Dirk van Miert, *Communicating Observations in Early Modern Letters (1500–1675): Epistolography and Epistemology in the Age of the Scientific Revolution* (Warburg institute, 2013); Anu Korhonen: ''the several hours of the day had variety of employments assigned to them': Women's Timekeeping in Early Modern England', in: *Journal of Early Modern Studies, Vol. 6, 61-85* (2017); Mary Lindemann, *Medicine and Society in Early Modern Europe* (Cambridge University Press, 1999); Londa Schiebinger, 'Maria Winckelmann at the Berlin Academy. A turning Point for women in science', in: Peter Dear (Ed.), *The Scientific Enterprise in early modern Europe* (University of Chicago Press, 1997), particularly pp. 305-307; Alisha Rankin, 'Becoming an Expert Practitioner: Court Experimentalism and the Medical Skills of Anna of Saxony (1532–1585)', in: *Isis Vol. 98, no. 1*, pp. 2*3–53* (2007).

[31] For instance: Londa Schiebinger and Claudia Swan (Eds.), *Colonial Botany: Science, Commerce, and Politics in the Early Modern World* (University of Pennsylvania Press, 2005); Fa-ti Fan, *British Naturalists in Qing China: Science, Empire, and Cultural Encounter* (Harvard University Press, 2004); Daniela Bleichmar, *Visible Empire: Botanical Expeditions and Visual Culture in the Hispanic Enlightenment* (University of Chicago Press, 2012); Daniela Bleichmar, Paula De Vos, Kristin Huffine, and Kevin Sheehan (Eds.), *Science in the Spanish and Portugese Empires, 1500-1800* (Stanford University Press, 2009); James Delbourgo and Nicholas Dew (Eds.), *Science and Empire in the Atlantic World* (Routledge, 2008); Sarah Irving, *Natural Science and the Origins of the British Empire* (Pickering and Chatto, 2008);

[32] Pablo Gómzes , *The Experiential Caribbean: Creating Knowledge and Healing in the Early Modern Atlantic* (University of North Carolina Press, 2017); Neil Safier, *Measuring the New World: enlightenment science and South America* (University of Chicago Press, 2008), pp. 63-64, 267-274; Andrew Curran, *The Anatomy of Blackness: science and slavery in an age of enlightenment* (John Hopkins University Press, 2011).

nineteenth century.[33] The case will be made that this distinction was largely a response that originated in attempts to safeguard social and epistemic boundaries in the context of such proximate cooperation. This has important implications for the prospect of citizen science. Simply arguing for more democratic participation is not enough: we need to analyze the ways in which a participatory science project challenges existing relations between its participants, transforms them, forms new ones or re-establishes old patterns. Hence, this thesis looks at what may be termed an early example of 'citizen science' on the eve of the rise of the expert.

By the nineteenth century, the idea of expertise came to be increasingly associated with other distinctions, such professionalism over amateurism, and being a specialized scientist over a general natural philosopher. Hence by nineteenth-century standards, many of the earthquake philosophers discussed in this thesis would not qualify as experts.[34] The greatest 'specialist' among them was probably John Flamsteed, and he was specialized in astronomy, not seismology. Nor was natural philosophy a profession. Only the very wealthy, like Robert Boyle for instance, could afford to spend most of their time on natural inquiry. Yet the structure of knowledge production was hierarchical in the seventeenth century too. This hierarchy was informed by another crisis of authority. Although never wholly discarded, the authority of the Ancients and the Bible gradually lost their privileged epistemological status in the seventeenth century. The new catchphrases were experiment, sensation and observation.[35] This invited accusations of relativism: anyone can observe, but how can we trust their observations? As research by Shapin and others has shown, 'making knowledge' involved not only observing phenomena, but also the making of a truth-claim about these observations and getting this claim *acknowledged* within a community of legitimate truth makers.[36] Importantly, this legitimacy and trust arises in a social context and is predicated upon contemporary norms of class, race and gender. The Royal Society of London allowed members of the nobility to join their ranks even without any scientific credentials. When the natural philosopher (and Duchess) Margaret Cavendish planned to visit the Society however, it resulted in a major controversy.[37] Following the collapse of 'ancient authority', the 'gentleman

[33] Vetter, 'Lay Participation in the history of scientific observation', p. 137.
[34] Vetter, 'Lay participation in the history of scientific observation', pp. 128-131
[35]On the primacy of experience, see: Dear, 'Totius in verba', p. 268: 'The Royal society's empiricism was rooted in the authority of the individual reporter as the actor in a well-defined, particular experience'. See also: Peter Dear, *Discipline and Experience. The Mathematical way in the Scientific Revolution* (University of Chicago Press, 1995), especially pp. 63-92; Cook, *Matters of Exchange*, pp. 1-41.
[36] Shapin, *A Social History of Truth*, pp. 3-41; see also: Alexandra Shepard, *Accounting for Oneself Worth, Status, and the Social Order in Early Modern England* (Oxford University Press, 2015).
[37] Londa Schiebinger, 'Women of Natural Knowledge', in: Lorraine Daston, Lorraine; Katherine Park (Eds.), *The Cambridge history of science. Vol. 3, Early modern science* (Cambridge University Press, 2006), p. 197; Bruce Moran, 'Courts and Academies', in Ibid., p. 255 also speaks of a 'tidal wave of controversy'. The visit did take

scientist' had emerged in the seventeenth century as the free economic and political agent who could put the weight of his social standing and reputation behind a claim to the truth of his observations.[38]

Within this framework it becomes easy to see why the acknowledgement of 'lay' participants in early modern science was problematic. Observers needed recourse to forms of authority to make their accounts credible and valuable. Yet extending the markers of scientific authority meant challenging the social basis of the community of trust on which early modern natural philosophy was predicated. These distinctions were not entirely rigid however. One of the Royal Society's founding members, Thomas Sprat, wrote that it was impossible for natural philosophy to depend only on 'perfect philosophers', for these were a rare breed. Given this lack, Sprat argued:

> It suffices if many of them be plain, diligent, and laborious observers: such, who, though they bring not much knowledge, yet bring their hands, and their eyes uncorrupted: such as have not their brains infected by false images; and can honestly assist in the examining, and registering what the others present to their view.[39]

Honesty, diligence and physical ability could thus prove substitutes for knowledge and statues as guarantors of experience. Hence Sprat noted that the *Philosophical Transactions* were filled with contributions from the 'shops of mechanicks; from the voyages of merchants; from the ploughs of husbandmen; from the sports, the ponds, the parks, the gardens of gentlemen.'[40] Yet on other occasions in the same text, he lamented that some philosophers were 'forced to trust' the observations of others and warns against the 'treacherousness of servants'.[41] Clearly not all observers were created equal, and to understand the changing relations between experts and lay persons it is important to trace the different ways in which observational authority was constructed in practice as well as in theory.[42] For this purpose I do not aim to provide a definition of early modern expertise, but merely to present an analytically useful list of qualities on which an observer could call to claim scientific authority in an early modern context.[43] These

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place in the end however, yet as Schiebinger notes, the society did not grant full membership to any women until 1945.

[38] Shapin, *A Social History of Truth*, pp. 65-125.

[39] Thomas Sprat, *The History of the Royal Society of London, for the Improving of Natural Knowledge* (London, 1667), pp. 72-73.

[40] Sprat, *The History of the Royal Society*, p. 72

[41] Sprat, *The History of the Royal Society*, pp. 74, 83.

[42] For some of the difficulties involved in this enterprise, see: Brita Brenna, 'Clergymen Abiding in the Fields: The Making of the Naturalist Observer in Eighteenth-Century Norwegian Natural History', in: *Science in Context 24(2)* (2011), pp. 143-146, 159-160. Most importantly, Brenna draws our attention to the fact that 'a "lay-expert divide" was articulated in relation to social and intellectual criteria that would develop later on, and it was transformed when new social practices were introduced.', p. 145.

[43] See also: Dear, 'Totius in verba', pp. 270-272 for a discussion of authority and experience.

qualities referred to different types of knowledge and the skills required to make this knowledge become recognizable, credible and authoritative. These qualities thus connect the epistemological and sociological dimensions of authority.[44]

	Open	Partially restricted	Restricted
Knowledge type	Experiential knowledge	Theoretical knowledge	Intelligence
Skill type	Communicative skill	Technical skill	Character/certification

Figure 1. Markers of scientific authority in early modern earthquake reports.[45]

Not all of these qualities were equally attainable. As social position remained an important determinant for authority of any kind, the closely associated qualities of intelligence and moral character were limitedly available as a discursive strategy to claim credibility. Only a few non-upper-class observers were noted to be intelligent or 'curious', often accompanied by the fact that they were exceptionally or remarkably so.[46] Similarly, the ability to make one's expertise recognizable through some form of institutional certification (such as membership of the Royal Society) was highly restricted of course. Theoretical knowledge of earthquakes and the technical skill of using this knowledge to observe *the right things* were largely depended on one's learning, and hence linked to social position as well. While these qualities were more easily attributed to observers they formed an ambiguous class. On the one hand the lack of theoretical knowledge was considered a boon because it made observations less likely to be influenced by hypotheses: they did not have 'their brains infected by false images'.[47] On the other hand, in the case of an elusive phenomena like earthquakes, it was also the point on which observers were criticized the most. Finally, knowledge from experiencing an earthquake and the ability to relate this experience were most readily attributed to, and claimed by, lay observers. These were the most essential markers to ensure the validity of an observation.[48]

[44] See: Pavol Hardoš, 'Who Exactly is an Expert? On the Problem of Defining and Recognizing Expertise', in: *Sociológia, Vol. 50, no. 3* (2018), pp. 277-280, 284-285. For a short exposition of the idea of 'social epistemology', see: Steve Fuller, 'Social Epistemology', in: Bruhn Jensen, Klaus; Craig, Robert (Eds.), *The International Encyclopedia of Communication Theory and Philosophy* (John Wiley & Sons, 2016), pp. 1-8. My own thinking has been influenced by: Sandra Harding, 'Rethinking Standpoint Epistemology: what is "strong objectivity"?', in: Alcoff, Linda & Potter, Elizabeth (Eds.), *Feminist Epistemologies* (Routledge, 1993), pp. 49-82. In particular, the idea that 'standpoint epistemology sets the relationship between knowledge and politics at the center of its account in the sense that it tries to provide causal accounts- to explain- the effects that different kinds of politics have on the production of knowledge' forms the analytical starting point for chapter 2.

[45] Author's own illustration.

[46] For instance: Doddridge, *Phil. Trans., Vol. 46, no. 497*, pp. 718-719.

[47] Dear, 'totius in verba', p. 267; Sprat, *The History of the Royal Society*,p. 72.

[48] The points discussed in this paragraph are discussed in more detail in chapter 2 of this thesis.

At the heart of this story lies a paradox: eye-witness reports were included in the natural philosophy of earthquakes because of the epistemic primacy of experiential knowledge in the late seventeenth century, albeit reluctantly. Yet through the interactions between naturalists and observers this experiential knowledge lost its status in favor of the theoretical knowledge of the naturalists who interpreted the observations. To understand this development, we must consider the concept of meta-expertise: the ability to tell an expert from a lay person.[49] In situations where these distinctions are not yet rigidly defined, meta-expertise involves also the discursive power to define the social meaning of expertise. The development of such a meta-expertise is necessary: there need to be clear epistemological standards by which to judge the production of knowledge. Yet this power is not equally distributed. While lay observers employed a wide range of tactics to bolster the credibility of their statements, the ultimate decision on what counted as a scientific observation was made by the natural philosophers during the processes of selecting, communicating, publishing and commenting on the various observations. While the process of producing new knowledge may have been participatory, the debate on the underlying epistemological assumptions was not. If the inclusion of acknowledged observers challenged the social and epistemological order, the development of an expert-discourse was a means to stabilize this tension and to assign 'proper' roles to each participant.

All of this is not to discredit the production of scientific knowledge of earthquakes in the early modern period. It is rather the recognition of the fact that, as the philosopher Elizabeth Potter puts it, 'contextual values, that is, moral, social, and political values, influence the work of natural and social scientists not only when the work is considered by scientists to be poor science, but also when it is considered to be good science'.[50] Following this insight, I analyze the formation of an expert discourse as a stabilization of the epistemological, scientific and social challenges an observer-based earthquake philosophy created. I do this because I am interested in how these processes of destabilization and re-stabilization produced new knowledge.

[49] Collinson, *Are we all scientific experts now?*, pp. 74-79. Kitcher, *Science in a democratic society*, pp. 147-153, discusses the idea in different terms, referring to the transparency and order of certification.
[50] Potter, p. 235.

The Royal Society for the improvement of natural knowledge

The earth had no shortage of seismic shocks between 1665 and 1755: no fewer than 301 major earthquakes were recorded globally, next to countless minor ones.[51] The three largest hotbeds of seismic activity were the Americas, East Asia and the Mediterranean. Several of the earthquakes which struck these regions became major news events in Western Europe, such as the earthquake in Smyrna in 1687, those near Beijing in 1679, the earthquakes which destroyed Lima in 1688 and 1745, and the one levelling Port Royal in 1692. While western Europe saw no major earthquakes except two in Southern Italy in 1688 and 1693 and the famous Lisbon earthquake of 1755, there was certainly no lack of writing on earthquakes either far away or close at home in England. Minor shocks, with such strength as to *almost* shake a book out of one's hands, were widely reported and interpreted.[52] These interpretations were made within a varied and rapidly changing constellation of religious, popular and natural philosophical ideas.

While new theories and observations of earthquakes were being produced all over the world during the seventeenth and eighteenth centuries, the accounts collected in the *Philosophical Transactions of the Royal Society* (published from 1665 onwards) form the largest single repository of earthquake accounts. The Royal Society was formed in 1660 with the express purpose to promote and facilitate the new experimental philosophy that had been advocated by philosophers such as Francis Bacon. In an important sense, it marked the institutionalization of scientific knowledge in Britain, and dominated the discourse on what counted as legitimate scientific knowledge during the upcoming centuries. In part, this was due to the publication of the *Philosophical Transactions*, one of the earliest scientific journals, from 1665 onwards.[53] The *Transactions* were initially based on the wide correspondence of its first editor, Henry Oldenburg, but soon included letters sent to the Society as well as lectures and treatises of its members. They were filled with observations of various strange natural phenomena, ranging from fascinating medical cases to intricate experiments and inexplicable weather phenomena. Hence the earthquake accounts printed in the *Transactions* were only a small part of wider practices of observation.

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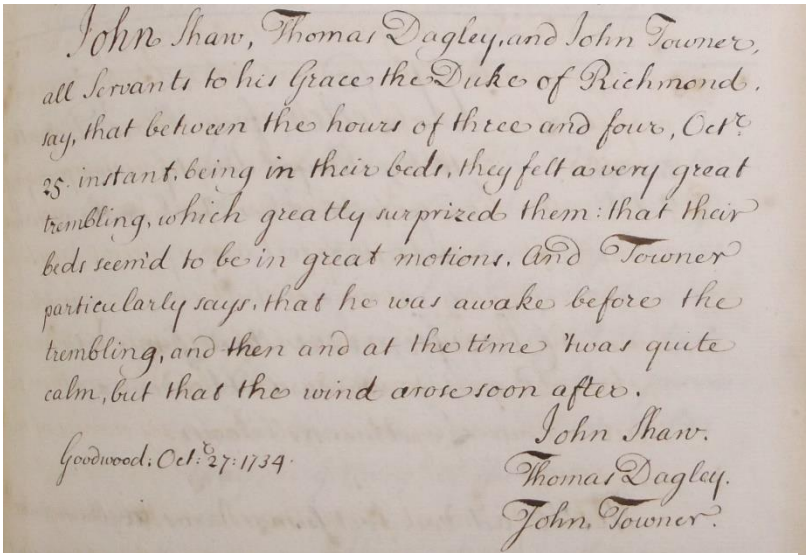
[51] According to the *Significant Earthquake Database* of the National Centers for Environmental Information. The database defines significant earthquakes as: 'Moderate damage (approximately $1 million or more), 10 or more deaths, Magnitude 7.5 or greater, Modified Mercalli Intensity X or greater, or the earthquake generated a tsunami.'https://www.ngdc.noaa.gov/nndc/struts/form?t=101650&s=1&d=1. Accessed 9-2-2019. For an overview of the earthquake accounts reported to and published by the Royal Society, see appendix **

[52] Anonymous, *A brief account, and seasonable improvement of the late earthquake in Northampton-shire, Jan. 4, 1675/6*, (London, 1676), p. 3.

[53] Save for a brief break between 1677 and 1679, the Philosophical Transactions were continuously published, a feat that not many seventeenth century journals could boast.

Focusing on the Royal Society has a number of advantages. It provides us with a limited group of natural philosophers who shared information, discussed their theories among each other and explicitly debated epistemological and methodological questions.[54] In 1696 for instance, the philosopher John Woodward shared his treatise titled *Brief Instructions for making Observations in all parts of the World and sending over Natural Things'*. The section on earthquakes contained a list of notable phenomena and questions to ask witnesses, some or all of which appeared in every earthquake report printed by the Royal Society. Developments such as this allow us to chart a relatively demarcated institutional discourse. At the same time, the Royal Society formed a hub within a much wider 'republic of letters', and the accounts contained within the archives do not only come from Britain (although given the relative absence of seismic activity there, the British isles are over-represented), but originate from all over the world.[55] The printed *Philosophical Transactions* themselves also travelled across the globe to inform and inspire a growing and global network of natural philosophers. This allows to also speak of broader developments in the history of earthquake studies. Lastly, the institutional continuity of the Royal Society means that bedsides the printed accounts, we also have access to a rich archive which contains many manuscripts that served as templates for the printed relations, and letters that did not appear in print at all.

There are also limitations to the source material however. The printed accounts comprise roughly 300 pages of eye-witness reports, and observations for a single earthquake ranged from a handful to several dozens, numbers which by the turn of the twentieth century were considered 'far too small for drawing the isoseismal lines of a strong


John Shaw, Thomas Dagley, and John Towner, all Servants to her Grace the Duke of Richmond, say, that between the hours of three and four, Oct.r 25. instant, being in their beds, they felt a very great trembling, which greatly surprized them: that their beds seem'd to be in great motions. And Towner particularly says, that he was awake before the trembling, and then and at the time 'twas quite calm, but that the wind arose soon after.

Goodwood. Oct.r 27. 1734.

John Shaw.
Thomas Dagley.
John Towner.

Figure 2. A 'certificate' containing the testimony of the servants of the duke of Richmond, 1734.

[54] See: Dwight Atkinson, *Scientific Discourse in Sociohistorical Context: The Philosophical Transactions of the Royal Society of London, 1675-1975* (Routledge, 1998); Peter Dear, 'Totius in verba', p. 257.
[55] See for instance: Dirk van Miert (Ed.), *Communicating Observations in Early Modern Letters (1500-1675). Epistolography and epistemology in the age of the scientific revolution* (The Warburg Institute, 2013), particularly pp. 199-222.

earthquake'.[56] Though these methods of collection were not systematic by nineteenth-century and later standards, they lived up to seventeenth and eighteenth-century standards of empiricism.[57] However, this does raise concerns about the empirical power of this present study. Because of the (relatively) small number of reports over a large period of time, statistical anomalies can easily skew any apparent trend. Hence, I have principally relied on qualitative methods of close-reading (more details below) instead of a quantitative approach.

Another element is more disconcerting: a cursory glance at the earthquake accounts reveals that they have nearly all been written and submitted by members of the Royal Society. This is an obvious problem for a study that aims to include a history 'from below'. Yet this surface-layer information hides much. Most of these accounts were no single observations, but composite reports that included up to several dozen witnesses. The language of these accounts varied from vague references to 'the whole town' feeling an earthquake to specific names and circumstances. Unlike the questionnaires filled out by nineteenth-century observers, these reports were not standardized. Sometimes the account would include a transcription of a conversation, excerpts from letters or 'certificates' signed by observers containing their testimony. These parameters of the sources inform the various 'reading strategies' that I have employed to analyze the earthquake reports.

1. The composite authorship of the earthquake reports poses a problem because it distorts or silences the voices of many observers. Yet, these distortions and silences are also interesting starting points to investigate the changing relations between naturalists and observers. Hence we need to pay close attention to the narrative voice in the accounts: whose voice is authoritative? Who represents whom? Why are some observers only mentioned while other are paraphrased or even quoted directly?
2. Because of these various stages in communicating the observations, many reports show traces of selection, interpretation and occasional commentary on the epistemological value of observations. These traces are informative for the way expert-lay relations were formed in the process of making, communicating and interpreting scientific observations.
3. I will also look at how these observer reports were used in natural philosophical treatises on earthquakes. Did they form the backbone of theoretical arguments or were they merely additional information? Were the accounts anonymized? Were

[56] Charles Davison, 'On Scales of Seismic Intensity and on the Construction and Use of Isoseismal Lines', in: *BSSA 11* (1921), p. 121. Quoted in: Coen, *The Earthquake Observers*, p. 284 (ftn. 17).
[57] J. Andrew Mendelsohn, 'The World on a page: making a general observation in the eighteenth century', in: Lorraine Daston & Elizabeth Lunbeck (Eds.), *Histories of Scientific Observations*, (University of Chicago Press, 2011).

they represented in full or in part? Which discursive strategies were used to establish or discredit these observations as credible information? And, again, are there any traces of selection, interpretation and commentary?

4. In those cases where the voices of actual observers are represented in the texts, we can look at the tactics and strategies they used to make their accounts more credible. Were there expectations regarding what a proper earthquake observation should look like, and to what extent did successful observers adhere to the rules of this genre to bolster their credibility?

These reading strategies rely on an interpretative balance between 'actor categories' and investigating the early modern social and epistemological frameworks with the benefit of hindsight.[58] This implies judging particular instances according to general patterns. To this end, the largest part of each chapter will examine the practices of knowledge making on the basis of a large number of reports and additional contemporary (natural) philosophical texts to contextualize interpretations. In addition, each chapter will also provide a close reading of a particular case to demonstrate the connections between the different points argued in the preceding sections.

An overview of the argument

So far we have discussed how scientific theory, epistemology and social relations shaped the construction of scientific knowledge through shifting lay-expert relations. In my analysis, I have sought to combine all these elements. There are several possible approaches to this task, varying from *internalist* to *externalist* approaches.[59]

1. First of all, one could tell this history as a *history of science* by tracing the different theories on earthquakes and their reception. The starting point of such a history would be the different theoretical frameworks in which earthquake science operated in the seventeenth and eighteenth centuries; its endpoint the impact of the observational data on these theories.
2. Secondly, one could tell a *history of knowledge*. The history of knowledge is interested in explaining the shifting boundaries between different disciplines and the

[58] Ed Jonker, 'Van relativisme naar oordeelsvorming. Recente tendensen in de wetenschapsgeschiedschrijving', in: *Studium, Vol. 1* (2011), pp. 7-8, 13-14.
[59] Daston, 'The History of Science and the History of Knowledge', pp. 132-142; Jonker, 'Van relativisme naar oordeelsvorming', pp. 2-3.

underlying assumptions about the nature of knowledge. Generally speaking, it is a history of practical epistemology. Within this approach, we could ask how the status of experiential knowledge changed, what tricks observers and naturalists used to discern the truth from contradicting observations, and what the act of observing even meant to contemporary observers.

3. A third approach can be called a *sociocultural history*. This approach would focus on how earthquake observations are embedded in a cultural context (for instance through the religious connotations of earthquakes) and a social setting (for instance analyzing the social relations between observers and naturalists). How did such arrangements influence the making and reporting of observations?

The central theoretical assumption of this thesis is that these three modes of analysis should not be separated. Rather, each of the following chapters analyzes the intricate connections between these ways of examining the phenomenon of earthquake observations. Each chapter is focused on the tensions in the theoretical, epistemological and social fields, with a particular focus on the construction of expert authority as a ways to stabilize these tensions.

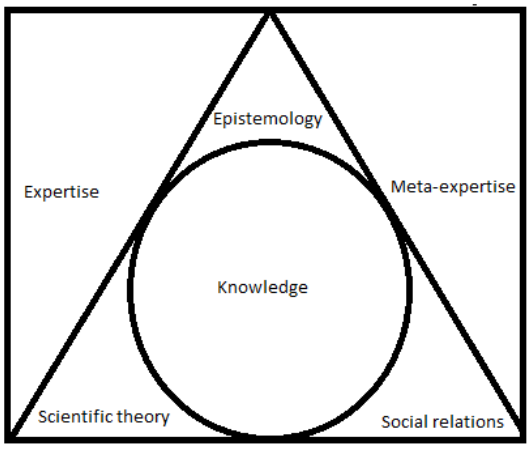


Figure 3. The theoretical model.

The first chapter explains the rise of the earthquake observer by examining the connection between changing epistemological assumptions about empirical knowledge and the scientific debate on the causes of earthquakes. The first half of the chapter will contextualize the various popular and natural philosophic ideas on earthquakes between 1665 and 1755, while the second half focusses on the epistemological commitments of these new theories, and map the sources of knowledge they employed. I argue that earthquakes took up a unique and interesting place in the discourse and practice of (lay)observation in the seventeenth century. The cause of earthquakes was much disputed: some philosophers placed their origins deep down in the earth, whereas other pointed to the sky and the similarities of earthquakes and lightning. These theoretical assumptions held important implications regarding *what* to

observe, and even whether the phenomenon *could* be observed. Moreover, in contrasts to other areas of natural philosophical inquiry, earthquakes can be characterized as *events* rather than *objects*. The ephemerality, irreproducibility and unpredictability of *events* posed new problems for those who were committed to an empirical epistemology. On the one hand, theorists tried to 'objectify' earthquakes by turning towards fossils, rocks, chemicals and the air as object-evidence. On the other hand, they increasingly relied on lay observations.

The second chapter analyzes how social relations and epistemological assumptions framed the ways in which earthquakes were reported. While it was known that earthquakes were both less intense and less frequent in Europe, European earthquakes dominated the scientific discourse because of prejudice against non-European observers and the idea that less intense earthquakes provided better conditions for observation. This choice gave rise to another concern however: how could lay observers be trusted to accurately report an earthquake which left little to no physical damage? Considering the many preconceptions about the observational skill, knowledge and intelligence of lay observers (and in particular of women and servants), this chapter investigates how reporters and naturalists emphasized experience and communicative ability as the primary ways to construct the credibility of lay observations.

The third chapter zooms in on the processes of translation which turned the experiential knowledge of eye-witnesses into legitimate natural philosophy through the expert's theoretical knowledge. I argue that the new observations were crucial in challenging some of the core assumptions of early modern earthquake philosophy. The chapter's two halves distinguish between the *aggregated* and *individuated* use of earthquake observations to chart the extent, timing and direction of an earthquake. In the first case, naturalists relied on a bird's-eye view over a larger number of observations to draw conclusions on the aforementioned elements. In the second case, they examined observations of several witnesses in more detail, and used their comments to indicate new possible phenomena to investigate. Both methods are informative of the ways in which the hierarchical expertise of the earthquake philosopher was constructed: they had superior knowledge because they alone had an overview of the all the different reports, and while some observers had more influence over the translation and interpretation of their observation than others, these acts were generally performed by naturalists. This chapter thus focusses on the scientific impact of earthquake observations and how scientific theory changed by challenging and reaffirming social orders.

The first three chapters each bracketed one aspect of our model. The fourth chapter aims to combine all three approaches in order to explain how earthquake observers turned into 'lay observers'. While theorists still made eager use of the experiential knowledge of observers, doubt about the veracity of such explicitly subjective and embodied reports

persisted. Echoing the methodological objectification of events through the study of fossils and chemicals discussed in the first chapter, I argue that the natural philosophy of earthquakes developed forms of inquiry in which the bodies and minds of earthquake observers themselves became sites of knowledge. The physiological and psychological effects of earthquakes played a serious role in natural philosophical theories, not only to interpret the findings of observers but also as characteristics of the earthquake event itself. This new form of knowledge was produced in tandem between the experiential knowledge of observers and the theoretical knowledge of naturalists, but reproduced a social hierarchy wherein the one was an object, and the other the creator of knowledge. This pathological knowledge was only a part of a wider discourse in which observers were encouraged to learn truths about themselves as observers and to reflect on their ability to convey their experience according to natural philosophical standards. As such, they became increasingly defined as an essentially lacking subject in relation to the expert naturalist: a lay observer.

Each of the elements described in these chapters (the increasing use of eye-witnesses; the methods of verification and constructing truth; the specific theoretical assumptions suggested, confirmed or discarded through observer testimonies; the hierarchical relation between theorists and observers; and the discourse of observers as lacking or lay subjects) are the building blocks from which eighteenth century was built. They were not formed in the isolated minds of a few gentleman scientists, but arose through a series of complex interactions between various actors who negotiated natural philosophical, epistemological and social questions. In order to better understand how this new discipline came to be, we will now recount the forgotten story of the seventeenth and eighteenth century earthquake observers.

1. The Rise of the Earthquake Observer

How did the epistemic challenges confronting the New Natural Philosophy give rise to the use of eye-witness reports?

Dark clouds were gathering in the skies above Livorno on the 16th of January 1742. Then, during the following night, a strange white hue suddenly appeared around the edges of these clouds. The bells of the city called all the residents out of their beds and onto the streets, among whom the learned cleric Pasqual Ranieri Pedini. As everyone was gazing up at the sky, many turned to Pedini for an explanation of the event. The citizens of Livorno were especially wary of this remarkable phenomenon because the previous evening had seen several minor shocks of the ground; this strange weather was clearly no coincidence to them. After a while Pedini managed to persuade them 'that it was nothing out of the common way, and did not portend any future evil, as they thought'. Pedini, however, was mistaken. Returning to his house, he was suddenly surprised by a violent shock 'far superior to the two former', which nearly caused him to fall down the stairs of his house. Still somewhat shaken, and perhaps a little sullen because of his public flop, he reflected in a letter on:

> the present calamities, the reasons of which are unseen by mortal eyes; for where shall we find those telescopes through which our sight may reach the subterraneous receptacles of that matter, which whether burned or fermented, makes the whole earth start, and terrify man? I look upon the foresight of these accidents, as an undertaking impossible to accomplish, and the prophetic fixing them to a certain time, much more so'.[1]

Pedini's thoughts echo the main themes of this chapter: the different theories of earthquakes in the seventeenth and eighteenth centuries, the epistemology of observation, and the difficult encounters between the two. Treating these themes serves two purposes. First, this chapter is written to provide some context on early modern natural philosophy and the study

[1] Pedini, *Phil. Trans., Vol. 42, no. 463*, pp. 77-90.

of earthquakes in particular. Secondly, I will argue how the tensions of new scientific theories and new epistemic commitments resulted in the rise of the earthquake observer around the turn of the eighteenth century. The first half of this chapter focusses on the popular and scholarly orthodoxies on earthquakes that were displayed on the streets of Livorno. It will trace the most prolific ideas in the middle of the seventeenth century, and show how these were challenged by two waves of new theories between 1665 and 1755. The second half of the chapter will shift focus from the content of these theories to the epistemological and methodological problems that students of earthquakes faced. As Pedini's remarks on subterranean telescopes indicate, there was no reliable way to measure or predict earthquakes. Nor was it possible to contain earthquakes within a laboratory, or to recreate the phenomenon as a controllable experiment. The epistemological demands of the 'new natural philosophy' developed in the seventeenth century necessitated some form of empirical observation however. This chapter will trace how the observations of lay persons came to be perceived as a necessary, though imperfect, solution to these problems.

Popular European ideas about earthquakes

Earthquakes were generally regarded as bad news.[2] Not only could they be extremely lethal, the damage done to buildings and livestock could cripple a local economy and make life arduous for those left without a roof over their head. In the aftermath of earthquakes and other natural disasters, civil and ecclesiastic powers were pressed to reestablish their authority in places where, according to the political imagination, both the houses and the minds of people lay in ruins. Fearful reports of looting, killing and raping among the rubble highlighted the sinful nature of the stricken city's inhabitants, and showed the dangers of a chaotic, un-overseeable polity.[3] These sensationalistic accounts of the post-earthquake city were balanced out by another narrative: that of a general sense of awe at God's omnipresent power, and the redemptive introspection and penitence that followed it. In the Catholic tradition, frequent public processions belonged to the repertoire of techniques employed to keep the population literally in-line, and to foster a sense of community and communal

[2] This short introduction mostly covers the 'bad' part of this statement. For the 'news' element in seventeenth century Europe, see: Carlos H. Caracciolo, 'Natural Disasters and the European Printed News Network', in: Joad Raymond & Noah Moxham (Eds.), *News Networks in Early Modern Europe* (Brill, 2016), pp. 756-778.

[3] Stephen Tobriner, 'safety and reconstruction of Noto after the Sicilian Earhtquake of 1693, the eighteenth-century context', in: Alessa Johns (Ed.), *Dreadful Visitations. Confronting natural catastrophe in the age of enlightenment* (Routledge, 1999), pp. 49-77; Francoise Lavocat, 'Narratives of Catastrophe in the Early Modern Period: Awareness of Historicity and Emergence of Interpretative Viewpoints', in: *Poetics Today, Vol.33, No.3-4* (2012), pp. 263-267; Charles Walker, *Shaky Colonialism: the 1746 Earthquake-Tsunami in Lima, Peru, and Its Long Aftermath* (Duke University Press, 2008), specifically pp. 74-89.

agency.[4] In the Protestant world, a similar function was fulfilled by public sermons, which were sometimes held for several weeks after the event.[5]

The suggestion of recent scholarship that earthquakes can be understood as social, cultural and moral events as well as natural phenomena would not have sounded strange to early modern ears. An important assumption underlying the seventeenth century understanding of earthquakes, one which made a slight tremor of the ground underneath Wallingford, Oxfordshire in 1683 as problematic as the destruction of Lima four years later, was that earthquakes were related to divine providence.[6] In the event of an earthquake, authorities did not only have to worry about the material breakdown of their power, but also about the loss of legitimacy suffered as a consequence of God's personal intervention in human affairs. Similarly, people did not only have cause to worry about the state of their bodies and their material goods, but were also concerned with the implications for their soul. Under such circumstances, moral significance could be attributed to the fact that earthquakes were more common and more severe in some place than in others. In September 1692, for instance, London was shaken by a small earthquake with no lasting damage. Keen moralists were quick to point out that this had been a mere reminder of the fate of Port Royal, the Jamaican city famous as a hotbed for buccaneers and the 'most debauched and ungodly People', which was almost entirely destroyed by an earthquake earlier that year.[7] Severe earthquakes were seen as punishments, lesser ones as omens.

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[4] José Mouthaan, 'Early modern perceptions of natural disasters: the eruption of the Vesuvius in 1631', in: *Traverse : Zeitschrift für Geschichte, Vol. 10* (2003), pp. 53-54.

[5] M. van de Wetering, 'Moralizing in Puritan Natural Science: Mysteriousness in Earthquake Sermons', in: *Journal of the History of Ideas, Vol.43* (July–September 1982), pp. 417-438; William Andrews, 'The Literature of the 1727 New England Earthquake', in: *Early American Literature, Vol.7* (Winter 1973), pp. 287-290; Matthew Mulcahy, 'The Port Royal Earthquake and the World of Wonders in Seventeenth-Century Jamaica', in: *Early American Studies, Vol.6, no. 2* (2008), pp. 410-415.

[6] Anon, *Strange News from Oxfordshire. Being a true and faithful account of the Wonderful and Dreadful Earthquake that happened in those* parts (London, 1683); Van de Wetering, 'Moralizing in Puritan Natural Science' pp. 417-420; M. Stuber, 'Divine Punishment or Object of Research? The Resonance of Earthquakes, Floods, Epidemics and Famine in the Correspondence Network of Albrecht von Haller', in: *Environment and History, Vol.9, no.2* (2003), pp. 174-185; Augustín Udías, 'Earthquakes as God's punishment in 17th, and 18th-century Spain', in: M. Köbln-Ebert, *Geology and Religion: a History of Harmony and Hostility* (The Geological Society of London, 2009), pp. 41-48.

[7] Thomas Doolittle, *Earthquakes Explained and Practically Improved* (London, 1693), p. 117: 'But compare your Mercies (for there are many in its preservation) with the Desolations brought upon many Cities in former Ages, and upon *Port-Royal* in *Jamaica*, by Earthquakes, and acknowledge God's mercy'; Rev. E. Heath, *A Full Account of the Late Dreadful Earthquake at Port Royal in Jamaica; Written in two Letters from the Minister of that Place* (London, 1692), p. 1. See also: Grey, *Chronological and Historical Account Of the most memorable earthquakes* (London, 1750), pp. iii-iv.

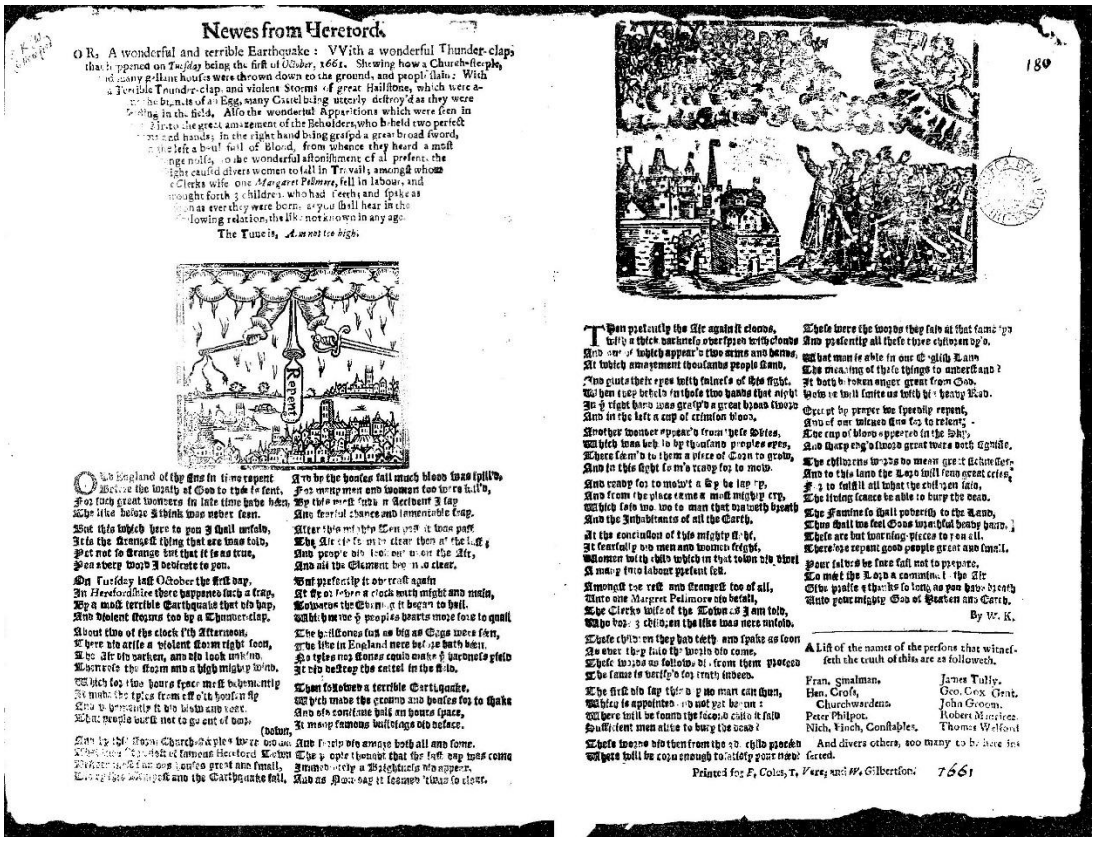

Figure 4. 'Newes from Hereford, or: a wonderful and terrible Earthquake' (London, 1661).

Earthquakes were also a part of a wider constellation of signs and wonders, which included meteors, eclipses, strange meteorological phenomena, monstrous births, apparitions, and those tiny creatures visible only through a microscope.[8] The language of wonders and marvels reflected the ontological status of these phenomena as a 'breached boundary' between established natural and moral categories and between the sacred and the profane. It also reflected the passions of bewilderment and curiosity that were aroused at the periphery of knowledge.[9] The idea of wonders resonated with seventeenth century natural philosophers, who considered the particular and the odd rather than the universal law to be the best point of entry into deciphering the 'Book of Nature'.[10] It also gave a vocabulary to popular expressions about earthquakes. Where we today are accustomed to deploy the vocabulary of 'disaster', popular culture in the middle of the seventeenth century resorted to the subcategory of wonders called 'horrors'.[11] These horrors seldom visited alone. The experience of fright doubtless inspired many additional fancies, but also required other

[8] See: Lorraine Daston & Katherine Park, *Wonders and the order of nature, 1150-1750* (Zone Books, 2001), pp. 14-17. See also: Christoph Carter, 'Meteors, Prodigies, and Signs: The Interpretation of the Unusual in Sixteenth-Century England', in: *Parergon, Vol. 29, no. 1* (2012), pp. 107-133.
[9] Daston & Park, *Wonders and the order of Nature*, p. 14-17.
[10] Eric Jorink, *Het 'Boeck der Nature'. Nederlandse geleerden en de wonderen van Gods schepping 1575-1715* (Primavera, 2006), p. 30, 37-43.
[11] Daston, Park, *Wonders and the order of nature*, p. 17.

similar phenomena in order to 'signify' the event: to make sense of it and to make the account believable.[12] In one exceptionally dramatic narrative of an earthquake in Hereford in 1661 (one of which the truth was attested by nine individuals mentioned by name, and 'diverse others, too many to be here inserted') the author drew on nearly the entire range of available horrors, from dying cattle and rains of hailstone and blood to divine apparitions and monstrous births:

> With a terrible thunder-clap, and violent storms of great Hailstone, which were a- […] of an Egg, many cattel being utterly destroy'd as they were [her]ding in the field. Also the wonderful Apparitions which were seen in […] the great amazement of the Beholders, who beheld two perfect [ar]ms and hands; in the right hand being graspd a great broad sword, [i]n the left a vial full of Blood, from whence they heard a most [stra]nge noise [t]o the wonderful astonishment of al present, the [fr]ight caused divers women to fall in Travail; amongst whom [th]e Clerks wife one Margaret Pellmore, fell in labour, and [b]rought forth 3 children who had teeth; and spake as soon as ever they were born, as you shall hear in the following relation, the like not known in any age.[13]

In parts of the world where earthquakes are more common, somewhat more benign (or at least less overtly moralistic) myths sought to explain their origin.[14] As Europeans ventured out in the 'age of exploration' and colonial conquest, they doubtless came in contact with these alternative explanations. Moreover, colonial settlers in places with high seismic activity soon adapted to new frameworks of what was natural. As earthquakes became part of life in the Caribbean, English settlers soon attributed little moral and religious significance to all but the most severe earthquakes.[15] When such damaging earthquakes did strike, they were also keener to observe their natural causes and consequences in addition to its religious grounds.[16] More generally, the greater and faster diffusion of earthquake accounts from across the globe meant that also in Europe earthquakes lost a part of their wondrous nature by the early eighteenth century. While their origins were still mysterious, they had lost another important quality of wonders: they were no longer rarities.[17]

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[12] Julie Sievers, 'Literatures of Wonder in Early Modern England and America', in: *Literature Compass 4/3, 766–783* (2007), p. 771.

[13] Anon. – *Newes from Hereford, or: a wonderful and terrible Earthquake* (London, 1661) In the account that followed, the triplet prophesized the end times.

[14] Severn, Roy, 'Understanding earthquakes: from myth to science', in: *Bull Earthquake Eng., Vol.10* (2102), pp. 351-366.

[15] Mulcahy, 'The Port Royal Earthquake', pp. 391, 420-421. Yet Mulcahy asserts they did continue to live in a 'world of wonders', as also noted by David Hall, *Worlds of Wonder, Days of Judgment: Popular Religious Belief in Early New England* (Harvard University Press, 1990), pp. 114-115.

[16] Mulcahy, 'The Port Royal Earthquake', pp. 391, 420-421.

[17] Daston & Park, *Wonders and the order of nature,* pp. 49, 240, 314-324. In 1750, for instance, the reverend Paul Dodridge remarked that: 'I was surprised to see how little the inhabitants of Northampton were impressed with this awful (tho' by no means supernatural) Event: the sound of such a shock was, in a manner, grown familiar to

This is not to say that there was a 'triumph of science over religion'.[18] To virtually all contemporary natural philosophers such a victory, or even the dichotomy itself, would be highly absurd. Earthquakes were seen as essentially super-natural phenomenon, in the sense that God was always the prime mover. The aim of natural philosophical earthquake theories was to show how God brought these earthquakes about *through natural means*.[19] The shift of earthquakes from divine wonders and horrors to natural phenomena reflected not so much a shift from irrational faith to rational science, but a broadly changing view of God: from a mystical entity speaking in tongues to a rational deity whose ways could be known through the careful study of Scripture and the 'Book of Nature'. Morality and nature, and science and religion, were not yet separate spheres. And while some may have had their reservations, many ordained ministers played an important role in developing new earthquake theories or popularizing them through their sermons.[20] So what exactly were they popularizing?

The scholarly view on earthquakes by 1650

The period preceding the 'scientific revolution' saw no shortage of, nor indeed any problem with, natural explanations for earthquakes. At the start of our period, these explanations were mostly inherited from ancient Greek philosophy, and particularly from Aristotle's *Meteorologica*.[21] Earlier writers such as Anaximenes of Miletus, his pupil Anaxagoras of Clazomenae, and the more famous Democritus of Abdera, had proposed that water and drought were the driving forces behind earthquakes, making the earth contract and expand. Aristotle proposed that the efficient cause of earthquakes was *wind* instead. In Aristotle's conception, the essence of the earth was dry and hot. Due to rainfall the soil could become wet and cold, which was balanced out by the natural heat of both the sun and the earth itself through evaporation. This process was considered the cause of winds and vapours. These winds could travel one of two ways depending on the relative strength of these two sources of heat. Either they became 'regular' winds blowing over the surface of the earth, or they

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their Ears […] in a very few hours it seem'd to have affected them no more than a shower of rain'. Doddrige, *Phil. Trans., Vol. 46, no. 497*, pp. 712-721.

[18] For a good overview of these debates, see: Rivka Feldhay, 'Religion', in: Daston, Lorraine; Park, Katharine (Eds.), *The Cambridge history of science. Vol. 3, Early modern science* , 727-755 (Cambridge University Press, 2006).

[19] Van de Wetering, 'Moralizing in Puritan Natural Science', pp. 422-424; Andrews, 'the literature of the 1727 New England earthquake', pp. 282-287. Writers such as William Stukeley and Stephen Hales were also explicit in pointing out that their natural philosophical investigation represented a different mode of inquiry, but stood in the service of divine matters. See: William Stukeley, *On the Causes of Earthquakes*, in: Phil. Trans., Vol. Vol. 46, no. 497, pp. 641-642, 645-646; William Stukeley, *The Philosophy of Earthquakes*, in: Phil. Trans., Vol. Vol. 46, no. 497, p. 750; Stephen Hales, *Some Considerations on the Causes of Earthquakes*, in: Phil. Trans., Vol. Vol. 46, no. 497, pp. 669-670.

[20] Ibid., pp. 424-427.

[21] Aristotle, *Metereology* (E. W. Webster Translation), Book B, sections 8-9.

were directed into the earth, which Aristotle conceived of as having a porous and cavernous structure. Hence Aristotle stated that 'we must suppose the action of the wind in the earth to be analogous to the tremors and throbbings caused in us by the force of the wind contained in our bodies'.[22] Storms and earthquakes were thus merely epiphenomena of the same process taking place above or below the surface. This also explained why earthquakes were generally observed before dawn and in spring and fall. The calm weather associated with these times was merely an indication that the wind was blowing *inside* the earth rather than outside. Finally, the friction created by these winds inside the hot and dry inner earth could produce fire, analogous to the manifestation of thunder and lightning in storms aboveground. These ignitions explained the correlations between earthquakes and volcanoes.[23]

Many of Aristotle's core assumptions and observations continued to exert a strong influence on the observation of and thinking about earthquakes all throughout the seventeenth- and eighteenth centuries. In newspapers, hurricanes and earthquakes were often reported side by side, and observers frequently mistook one for the other, or compared them.[24] In 1692, a popular pamphlet discussing the Port Royal earthquake gave an essentially Aristotelian explanation of the event, drawing attention to the 'exhaltation of hot and cold that is enclosed in the caves of the earth, strugling there to find passage and break forth'.[25] Around the same time, the natural philosopher Christiaan Huygens wondered 'whether by the distance, over which the succession of quakes is stretched, something could be said about the depth of the holes and the vapors'.[26] Descriptions taken verbatim from Aristotle, such as the hazy appearance of the sun, calm winds, a light, long-drawn cloud in the sky and the noise of wind made regular appearances in earthquake testimonies, and in 1750 the reverend Roger Pickering cautiously confirmed the Aristotelian observation that earthquakes were more frequent in spring and autumn.[27] The Aristotelian position remained the intellectual bedrock upon which new earthquake theories would be built throughout the seventeenth and eighteenth centuries. However, the gradual sedimentation of new ideas was starting to change the surface of what earthquakes looked like.

In 1665 Athanasius Kircher published his most popular work, *Mundus Subterraneus*: an overview of widely held contemporary ideas about the rich and interesting world beneath the

[22] Aristotle, *Metereology* , section 9.
[23] Aristotle, *Metereology* , section 10.
[24] For instance in the The *Oprechte Haerlemsche Courant* of 15-01-1728, an earthquake near Boston was initially reported as a 'loud rumbling, followed immediately by a hurricane' (*een groot gedruys hoorde, waer op immediatelijck een Orcaen volgde*). See also chapter 2 of this thesis on the descriptions of earthquakes and other phenomena such as lightning.
[25] Anon, *A Sad and terrible relation of the dreadful earth-quake that happened at Jamaco* (London, 1692), pp. 12-13.
[26] Huygens, *Oeuvres complètes*, p. 311. *'An ex spatio quo se extendit haec successio, aliquid de profunditate cavernarum ac vaporum conjici possit?'* .
[27] *Phil. Trans. Vol. 46, No. 497.* XV. Rev. Roger Pickering, F.R.S., pp. 622-625.

surface.[28] The two volumes of the work span a wide range of subjects that attest to the broad knowledge of their polyglot author: mathematics, geology, medicine, zoology (including classifications of subterranean dragons, humans and demons), alchemy, smithing and agriculture. It was, by all seventeenth century standards, an absolute bestseller.[29] In spite of its broad range, it offered a concise view of earthquakes and volcanoes. Like Aristotle, Kircher started with the image of a porous earth, marked by a multitude of subterraneous cavities and connecting vents. Following Descartes however, Kircher argued that some of these spaces were filled with an eternal fire that arose from the earth's centre.[30] These subterraneous vents of fire created a vast network which connected all known volcanoes aboveground. Earthquakes, Kircher explained, most frequently hit places located above such veins and near volcanoes. They were caused when incoming cold water from the surface came in contact with subterraneous substances such as saltpetre, sulphur and quicksilver. This caused these substances to release flammable vapours. When these vapours were ignited by the internal fire, they created an explosion which collapsed part of the earth's inner structure and caused the earth to cave in. This, Kircher supposed, was simply common knowledge.[31]

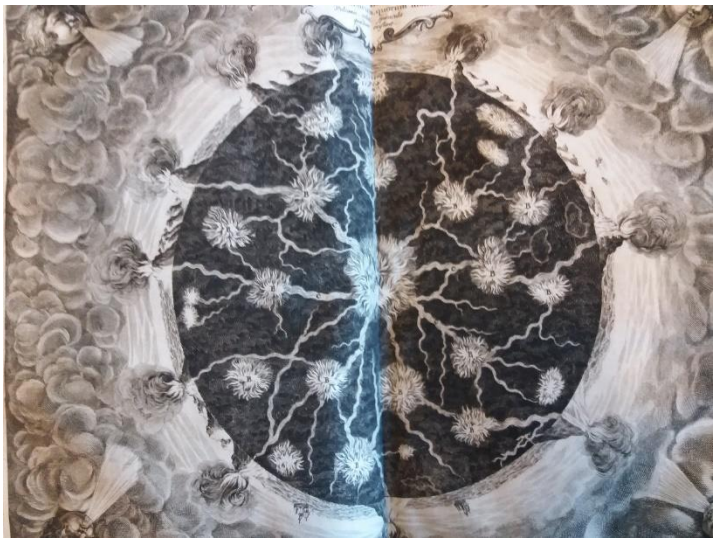

Figure 5. Kircher's view of the earth as printed in the Mundus Subterraneus (1665).

Although this mid seventeenth-century common sense view of earthquakes that Kircher described differs in some ways from the traditional Aristotelian account (the subterraneous fire ducts, the inclusion of elements unknown to the ancients, and a slightly different account of the origin of winds), the overall structure of the argument remained the same. Wind continued to be the driving force, though aided by sulphur. The remarks on earthquakes

[28] Athansius Kircher, *Mundus Subterraneus* (Amsterdam, 1665).
[29] William Parcell, 'Signs and symbols in Kircher's Mundus Subterraneus', in: Gary Rosenberg (Ed.), *The Revolution in Geology from the Renaissance to the Enlightenment* (Geological Society of America, 2009), P. 52.
[30] Kircher, *Mundus Subterraneus*, Vol. 1, p. 219.
[31] Ibid., p. 223.

within the *Mundus Subterraneus* are chiefly found within a chapter on the origins of wind (*De Ventorum Origine*).[32] Moreover, Kircher still considered earthquakes to be 'subterranean lightning', and also understood the violence of earthquakes to be the collapsing of part of the interior of the earth.[33] In the decades following the publication of the *Mundus Subterraneus*, these elements would become the focus of discussions on earthquakes.

New earthquake theories 1665-1700

The first of the big Aristotelian assumptions to be put to the test was the idea that earthquakes always caused the collapse of some part of the earth's cavernous structure, and led to the sinking of the ground above. This conception was challenged first by the Danish-turned-Florentine naturalist Nicolaus Steno (Niels Stensen) and the English Fellow of the Royal Society Robert Hooke.[34] Both men found themselves defending a seemingly unrelated and much-criticized position: that fossils were the remnants of historical organisms and not mere tricks of nature.[35] This view raised the tough question how ostensibly marine fossils could be found far out from the sea. Hypotheses ranged from dried up lakes and retreating shorelines to evidence of the biblical deluge.[36] Steno's observations on the stratification of rocks and fossils in his dissertation *De Solido intra Solidum* (more particularly his comment that marine deposits only explained horizontal, and not slanted layers), opened the path to a geological history of the earth and a more productive view on how geological processes shaped the earth's surface.[37] Steno remained reluctant to discard the flood-hypothesis however, and argued that while the fossil's exact position was determined by the shape of the earth as the consequence of earthquakes, all fossils were antediluvian in nature.[38]

In his lectures on earthquakes from 1667, Robert Hooke plainly discarded the deluge hypothesis on the grounds that the timespan of the flood stated in the Bible would not have been enough to produce such large fossils.[39] Instead, he proposed that geological processes had pushed up mountains and islands from the earth and thus displaced the fossils. And

[32] Ibid., pp. 219-223.
[33] Ibid., p. 223.
[34] Robert Hooke, 'Lectures and discourses of earthquakes, and subterraneous eruptions', in: Samuel Smith, Benjamin Walford (eds.) , *The Posthumous Works of Robert Hooke 1668-1700*. (London, 1705), pp. 277-450.
[35] Note that the term 'fossil' referred to any kind of deposit.
[36] Toshihiro Yamada, 'Hooke–Steno relations reconsidered: Reassessing the roles of Ole Borch and Robert Boyle', in : Gary Rosenberg (Ed.), *The Revolution in Geology from the Renaissance to the Enlightenment* (Geological Society of America, 2009), p. 107-108.
[37] Jens Morten Hanse, 'On the origin of natural history: Steno's modern, but forgotten philosophy of science', in : Gary Rosenberg (Ed.), *The Revolution in Geology from the Renaissance to the Enlightenment* (Geological Society of America, 2009), p. 175.
[38] Alan Cutler, 'Nicolaus Steno and the problem of deep time', in : Gary Rosenberg (Ed.), *The Revolution in Geology from the Renaissance to the Enlightenment* (Geological Society of America, 2009), pp. 143-148.
[39] Hooke, 'Lectures and discourses', pp. 314, 341, 412.

rather than arguing that earthquakes always indicated a collapsing ground, Hooke proposed four different sorts of effects of earthquakes. The first *genus* of effects identified by Hook was the 'raising of superficial parts of the earth above their former level'. This raising could be further classified into four *species*: the raising of an entire country, the raising of part of the sea bed to form islands, the raising of mountains out of flat land, and the deposition of rubble to elevate terrain. The second genus of effects consisted of these same four mechanisms working in the opposite direction. The third genus included the 'subversions, conversions, and transpositions of the earth', and the final genus the miscellaneous effects of vapours, liquids and heat arising.[40]

In order to explain how these earthquakes came about, Hooke made use of old and new ideas. Initially, he fully agreed with his contemporaries that earthquakes were the result of underground explosions, much like gunpowder and lightning. However, a major point of contention with Descartes and Kircher was that Hooke did not belief the fire inside the earth to be eternal. He wondered how this subterranean fuel could sustain itself if it continuously combusted in the process of earthquakes, and although Hooke was not entirely opposed to the theory of a degenerative and ageing earth, he nevertheless set out to find other explanations. In his lectures from the late 1680s he argued that geological phenomena could be explained by changes in the earth's centre of gravity and its axis of rotation. Such changes, Hooke hypothesized, would cause the landmasses to move about and create earthquakes.[41] These ideas seemed unlikely to his fellow members of the Royal Society, who could find no evidence of such changes occurring and accused Hooke of having 'turned the World upside down for the sake of a Shell'.[42] By the 1690s Hooke had, somewhat reluctantly, returned to the theory of 'subterraneous eruptions of fire'.[43] The other major problem of Hooke's theory was that it was based on the, at the time unquestionable, assumption that the earth was merely a few thousand years old. In order to reconcile his theory with this limited timespan, Hooke had to argue that earthquakes could produce mountains and islands over the course of but a few years. Until the view of geological time started to expand massively by the late eighteenth century, this generative view of earthquakes was understandably hard to swallow.[44]

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[40] Hooke, 'Lectures and discourses', pp. 298-318.
[41] Rhoda Rappaport, 'Hooke on Earthquakes: Lectures, Strategy and Audience', in: *The British Journal for the History of Science, Vol. 19, no. 2* (1986), pp. 129-146; Hooke, 'Lectures and discourses', pp. 346-362.
[42] Hooke, *Discourse on Earthquakes* p. 411.
[43] Rappaport-'Hooke on Earthquakes', p. 137; Hooke, p. 372.
[44] Rappaport-'Hooke on Earthquakes', pp. 129-130, 142-3; Ezio Vaccari, 'European views on terrestrial chronology from Descartes to the mid-eighteenth century', in: C. L. Lewis E. & S.J. Knell (Eds.), *The Age of the Earth: from 4004Bc to AD 2002*. (Geological Society of London, 2001), pp. 25-34.

One eminent critic of Hooke, Martin Lister, proposed his own theory on earthquakes in the *Philosophical Relations of the Royal Society* in 1684.[45] Aside from arguing that fossils did not originate from living organisms, Lister doubled down on the theory of explosive vapours and the relation of earthquakes to lightning. Where Kircher had identified a broad range of subterraneous substances and proposed a vague idea of how these substances caused explosions, Lister argued that 'Pyrites alone of all the known Minerals, yeilds [sic.] this inflamable vapour'.[46] Their sulphurous nature could cause the vapours emanating from them to combust 'spontaneously' by the natural heat of the earth or sun.[47] If these vapours were out in the open, they created lightning. If they were trapped in the earth, they resulted in earthquakes. While Lister maintained that the earth was probably still 'more or less hollow', his theory proposed an alternative to the vast underground system of caves that connected all volcanoes and earthquakes. The simple fact that Italy knew many pyrite deposits while England had but few already accounted for the higher frequency of earthquakes, volcanoes and lightning in the former.[48]

The first Astronomer Royal John Flamsteed was even more radical in his rejection of a hollow and cavernous earth, insisting that it was wholly solid except for some shallow caves at the surface level.[49] As yet another variation on the theme of connecting earthquakes and meteorological phenomena, Flamsteed argued in a letter from 1693 that the shaking of the earth was a mere illusory effect produced by explosions of nitrous and sulphurous vapours in the sky. The shockwaves of the explosion would cause buildings to 'bend' or 'expand' to one side, and then return to their initial position. As these waves were thus pushed back and forth, this created the impression of several large shocks.[50] Flamsteed also recognized the occurrence of earthquakes as part of volcanic eruptions, and agreed with some contemporaries that the impact of a large rock might send waves through the earth that could be registered as earthquakes. Despite allowing for

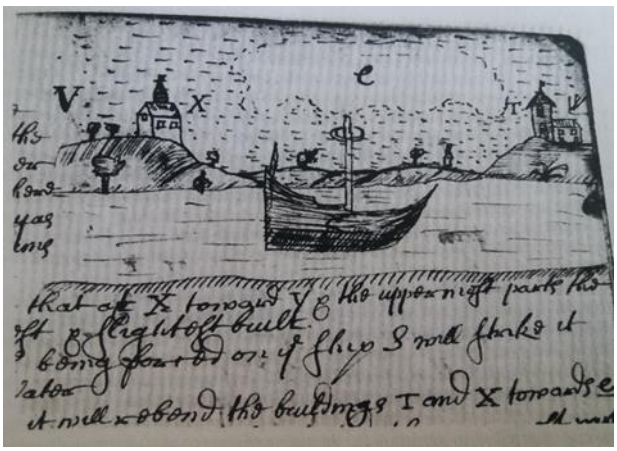

Figure 6. John Flamsteed's drawing of an 'air-quake', 1692.

[45] Martin Lister, 'Of the Nature of Earth-Quakes; more particularly of the Origine of the matter of them, from the Pyrites alone', in: *The Philosophical Transactions of the Royal Society, Vol. 14, No.157*, pp. 512-515.
[46] Lister, *Of the nature of Earthquakes,* p. 513.
[47] Though the name pyrite is now exclusively given to one particular iron sulphide, in it was used at the time as a general name for all sulphides. See: David Rickard, *Pyrite. A Natural History of Fool's Gold* (Oxford University Press, 2015), p. 76.
[48] Lister, *Of the nature of Earthquakes,* p. 514.
[49] Frances Willmoth, 'John Flamsteed's letter concerning the natural causes of earthquakes', in: Annals of Science, Vol.44, 23-70 (1987); Flamsteed, *letter concerning earthquakes* (Printed : London, 1750).
[50] Flamsteed, *Letter Concerning Earthquakes*, pp. 14-17.

multiple types of earthquakes, a close study of successive drafts of his letter on earthquakes has revealed that over time Flamsteed came to prefer his theory of 'air-quakes' as the primary explanation for earthquakes.[51] His contemporaries, however, did not. Flamsteed was very much aware of the real and possible objections to his theory and refrained from publishing his thoughts. His letter eventually appeared in print only in 1750, 31 years after his death.

New earthquake theories 1700-1755

Though the finer points of the various theories kept being discussed between 1700 and 1740, these decades saw few theoretical innovations. The theories of Lister and Flamsteed made a brief appearance in the work of Isaac Newton and were worked out by the famous French chemist Nicholas Lemery, who also popularized the subterraneous explosion theory with a broader audience in France, England and the Dutch Republic.[52] The nonconformist reverend Thomas Doolittle combined theological and natural philosophical accounts of earthquakes in his sermons, which influenced a rich tradition of puritan writings on earthquakes in England and the Americas.[53] In 1726, John Woodward was still defending the view that earthquakes were caused by vapours that got trapped when the earth's pores were clogged.[54] A new paradigm was being developed from the 1720s onwards, owing to the recently discovered connections between lightning and electricity, which took any kind of shockwave to be electrical in nature. However, it would take several minor earthquakes in England in 1750 for these new theories to see the inside of a print shop.

In an appendix to the *Philosophical Transactions* for the year 1750, the Royal Society published three papers of their Fellow William Stukeley.[55] When approaching the topic of

[51] Willmoth-'John Flamsteed's letter', pp. 50-66.
[52] See : Isaac Newton, Opticks: Or, A Treatise of the Reflections, Refractions, Inflections and Colours of Light (Fourth Edition, 1730), p. 354; Nicolas Lémery, 'Explication physique et chymique des Feux souterrains, des Tremblemens de Terre, des Ouragans, des Eclairs & du Tonnerre', in: Histoire de l'Académie Royale des Sciences, Année M. DCC, avec les Mémoires de Mathématiques et de Physique pour la même année. (Amsterdam: Gerard Kuyper, 1700) pp. 131-142; Lemery, Nicolas, *Cours de chymie : contenant la maniere de faire les operations qui sont en usage dans la medecine, par un methode facile; avec des raisonnements sur chaque operation, pour l'instruction de ceux qui veulent s'appliquer a cette science*. (Paris, 1687); Manuel García Cruz, 'Nicolas Lémery (1645-1715) y su Teoría Físico-Química Sobre Diversos Fenómenos de Interés para las Ciencias de la Tierra', in: International Commission on the History of the Geological Sciences, Cuadernos dieciochistas, 16 (2015), pp. 311-337.
[53] Van de Wetering, 'Moralizing in Puritan Natural Science', p. 421.
[54] John Woodward, *An Essay toward a Natural History of the Earth and Terrestrial Bodies illustrated, inlarged, and defended* (London, 1726). The first edition of this work had been published in 1695.
[55] Stukeley, 'On the Causes of Earthquakes'; William Stukeley, 'Concerning the Causes of Earthquakes'; Stukeley, 'The Philosophy of Earthquakes'. All in : *Philosophical Transactions of the Royal Society, Vol. 46, No. 497* (1750). These three lectures given to the Royal Society were published within the same volume of the *Philosophical Transactions* and argue the exact same argument.

earthquakes, Stukeley noted, he ´did not enter into the common notion of struggles between subterraneous winds, or fires, vapours, or waters, that heav'd up the ground, like animal convulsions; but I always thought it was an electrical Shock'.[56] Stukeley had found three reasons to discard these old, 'vulgar opinions'. While he was willing to admit that some minor earthquakes were caused by volcanic eruptions, and that these had some relation to subterranean vapours, he considered them separate from ´actual earthquakes´ on the basis that the latter occurred much more frequently and independent of any volcanic activity.[57] Secondly, the theory of subterraneous fire could be discarded because the existence of underground wells and streams, as well as the occurrence of earthquakes at sea were incompatible with it.[58] Lastly, given that the earth was solid except at the surface, a theory that posed an efficient cause deep underground would not be able to explain such violence at the surface. Earthquakes, Stukeley argued, were a surface phenomenon.[59]

Stukeley's alternative hypothesis worked as follows. Lighting is caused when warm and cold airs meet and produce electricity in the air. Similarly, a meeting of warm and cold weather also electrifies the earth. When a non-electrical body, such as rainfall, subsequently comes in contact with it, it discharges.[60] Stukeley found evidence of his theory in the fact that earthquakes were more prevalent in the Southern Hemisphere where there was more warm air to produce electricity, and in the observation that the heaviest earthquakes always struck coastal cities, where the presence of a large body of water to conduct the electricity created larger shocks.[61] The major problem of his theory was that it had trouble accounting for the relatively low occurrence of earthquakes in comparison to thunder and lighting. To this, too, Stukeley had devised an answer: 'It may be said, that, if this were the case, Earthquakes would happened much oftener than we find them. It may be answer'd, That they probably do, much oftener than observ'd: but *slight* ones; because of the Earth's being *slightly* electrified'.[62]

A later lecture of Stukeley's reveals that his theory was not immediately accepted, and he clearly felt the need to elaborate on it since 'some worthy members had not fully enter'd into my way of reasoning'.[63] Yet not everyone was hostile to his new way of explaining the well-known relationship between earthquakes and lightning. One fellow trying to reconcile Stukeley's theory with more orthodox explanations of earthquakes was the reverend Stephen

[56] Stukeley, 'On the Causes of Earthquakes', p. 642.
[57] Stukeley, 'Concerning the Causes of Earthquakes', p. 658.
[58]Stukeley, 'Concerning the Causes of Earthquakes', pp. 658-660.
[59] Stukeley, 'Concerning the Causes of Earthquakes', pp. 660-661.
[60] Stukeley, 'Concerning the Causes of Earthquakes', pp. 663-664.
[61] Stukeley, 'Concerning the Causes of Earthquakes', pp. 668 and 644, 665.
[62] Stukeley, 'On the Causes of Earthquakes', p. 644.
[63] Stukeley, 'Concerning the Causes of Earthquakes', p. 657.

Hales.[64] According to Hales, the calm and clear air often reported before an earthquake indicated the presence of sulphur in the atmosphere. Through an experiment with the air pump designed by Robert Hooke and Robert Boyle in the 1660s, Hales confirmed that the mixture of sulphurous and 'fresh' air created an explosive reaction that burned up parts of both. When the two airs mixed, the friction between them caused the air to warm up, and the discharge cooled the air down again. To Hales, this explained the heat before a thunder storm and the cooler air afterwards. The burning of the 'elastic' air created a vacuum, which caused new wind to rush in to fill it. Hence, the relation between lightning and storms could be explained. The discharge through lightning and the influx of new winds were usually enough to dispel the sulphurous vapours. Yet when this was not enough, for instance in places with much sulphur or little wind, the electricity would be taken up by the earth instead and strike as 'earth-lightning', or in other words: an earthquake. In this way, Hales managed to reconcile the theory of earthquakes as electric phenomena with the common view that earthquakes were linked to combustible sulphurous vapours.[65]

Although the various theories of the earth shared some common ground, no real consensus arose. Following the earthquake of 1755 an overview of the *History and Philosophy of Earthquakes* simply presented all the various theories from Hooke to Hales and left the reader to make up their own mind.[66] Nonetheless, some general developments can be discerned. Between 1665 and 1755, the source of earthquakes had gradually been raised from the deepest depths of the earth to its surface layer, and even up into the air. The same basic elements of wind, fire, vapours and lightning were used in different configurations to support divergent theories on the origins of earthquakes. What had changed? First of all, new intuitions about the nature of air, following the experimental use of the air-pump by Boyle and Hooke from 1667 onwards, had made the air a likely candidate for the 'missing link' in the theories of the earth. The discovery of electricity in lightning by the mid-eighteenth century provided a similar impulse to ascribe all sorts of elusive phenomena to this new theoretical insight. Secondly, the comments of William Stukeley are illustrative for a certain contemporary philosophical spirit with a strong predilection for observable causes in favour of unverifiable mechanisms hidden far beneath the surface. It is clear that by elevating the cause of earthquakes, late seventeenth and eighteenth-century

[64] Hales, Stephen, 'Some Considerations on the Causes of Earthquakes', in : *Philosophical Transactions of the Royal Society, Vol. 46, No. 497, pp. 669-681* (1750).
[65] Hales, *Some Considerations*, pp. 674-677.
[66] John Bevis (Ed.), *The History and Philosophy of Earthquakes* (London, 1760), preface. Bevis notes that the author 'has retained entirely the facts, arguments and conclusions of the authors from whence he has extracted his collections, and that almost in their own words; without ever presuming to criticise any hypothesis, much less to obtrude one of his own'. Another publication from the same decade similarly remarked the wide range of current explanations and argued that all of them had their respective merits and flaws: Grey, *Chronological and Historical Account Of the most memorable earthquakes* (London, 1750), pp. 1-3.

theories provided the possibility for growing empirical observations. In the next sections I will argue that the development of these theories was predicated on the belief that such observations *should* form the bedrock of earthquake science.

True knowledge and its sources

Not only Aristotle's particular account of the cause of earthquakes was under pressure by the mid seventeenth century. Rather, the whole Aristotelian system of metaphysics and epistemology which had dominated Christian and Islamic natural philosophy for the last centuries was being assailed from different sides. The scholastic approach to nature through the examination of its four causes (material, formal, efficient and final) was challenged by a revival of epicureanism and stoicism, which in their seventeenth century guise set out to explain causation by looking for nature's smallest component parts and the 'natural laws' that governed them.[67] Aristotelian notions about which kinds of thought counted as knowledge, and to which spheres of nature this knowledge belonged were being reconsidered, and boundaries between formerly separate disciplines and methodical practices were redrawn.[68] The most essential of these transformations for the purpose of the rest of my argument here was the junction of *natural history* (the realm of particular instances) and *natural philosophy* (the realm of universal truths).

It may strike modern readers as quaint that scholastic natural philosophy followed the mantra 'there is nothing in the mind which was not first in the senses', while Francis Bacon simultaneously complained that Aristotle and the scholastics 'did not properly consult experience'.[69] One explanation is that Bacon primarily accused Aristotle of using sensory experience only as evidence to confirm his preconceived theories, rather than as a guiding principle of inductive reasoning. As Peter Dear has argued however, the accusation was also informed by a fundamentally different understanding of the concept of experience.[70] For Aristotle, true experiential knowledge referred solely to universal, or common experience: that which was plain for all to see. Hence the fact that the sun rises in the east or,

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[67] Lynn Joy, 'scientific explanation from formal cause to laws of nature', in : Daston, Lorraine; Park, Katharine (Eds.), *The Cambridge history of science. Vol. 3, Early modern science* (Cambridge University Press, 2006), 70-105. pp, 71-79.

[68] Katherine Park and Lorraine Daston, 'Introduction: The Age of the New', in : Daston, Lorraine; Park, Katharine (Eds.), *The Cambridge history of science. Vol. 3, Early modern science* (Cambridge University Press, 2006), 1-20. pp. 11-12.

[69] 'Nihil in intellectu nisi prius in sensu', this quote was derived from Thomas Aquinas. See: Blackburn, Simon, *The Oxford Dictionary of Philosophy (2 rev. ed.)* (Oxford University Presss, 2008). For Bacon's quote, see: Francis Bacon, The New Organon (Ed. Lisa Jardine and Michael Silverthorne, Cambridge University Press, 2000), p. 52.

[70] Peter Dear, 'The Meanings of Experience', in: Daston, Lorraine; Park, Katharine (Eds.), *The Cambridge history of science. Vol. 3, Early modern science* (Cambridge University Press, 2006), 106-131. pp. 106-109. See also : Peter Dear, *Discipline and Experience. The Mathematical way in the Scientific Revolution* (University of Chicago Press, 1995), pp. 11-26.

characteristically, that *all men are mortal*, was said to be derived from experience. Since every single person (in theory at least) shared this experience, its truth could be guaranteed. This type of 'true experience' was given the status of *natural philosophy*. Experiences of particulars, an earthquake for instance, were not universally shared, and hence could not be reliable counted as absolute philosophical truth. While such accounts were valuable, they were placed within the more epistemologically uncertain realm of *natural history*. Bacon's radical proposition was that the truth claims of natural-historical experience were equal to those of natural-philosophical experience.[71]

Underlying this profound epistemological difference was an altogether different concept of nature. According to Aristotelian thought, every primary substance behaved according to its 'final cause': it did what it was naturally supposed to do. Smoke rose towards its naturally designated place in the atmosphere, while the final cause of trees was to grow. Physical deviations were part of the Aristotelian framework, but these were simply not 'natural'.[72] In contrast, Bacon identified three distinct spheres of nature. The first, 'nature in its true course', corresponded to the narrow Aristotelian concept. The second, 'nature erring and varying', greatly expanded the realm of nature by allowing for natural anomalies. For instance, a tree that would not grow could now also be explained as the outcome of a natural process. The third, 'nature altered and wrought', reflected the idea that the underlying principles of nature could be understood by purposefully altering a phenomenon and observing the different outcomes: *experience* from conducting *experiments*. For Aristotle, such an operation could not count as natural philosophical knowledge; first, because the result was not 'natural' (because tampered with), and second because the result was not 'philosophical' (because only valid within an experimental setting and hence not plain for all to verify). It thus took the Baconian revision of experience to start imagining experiments and natural history as valid practices capable of establishing natural philosophical truths.

The experimental 'New Philosophy' was eagerly taken up by the early Royal Society of London, who heralded Bacon as their chief inspiration and modeled themselves after Salamon's House, the scientific academy in Bacon's utopian novel *New Atlantis*. The promise of deriving philosophical truths from particular observations was tempting, precisely because recent inventions such as the microscope had proven that there were natural worlds that lay outside the common-experience. Yet this new commitment also created a major epistemological problem: what was the meaning of truth? For committed Aristotelians, strange phenomena and absurd observations (outliers, we might say) were easily denied the

[71] Although they remained separate categories in Bacon's thinking. See: Stephen Gaukroger, *Francis Bacon and the Transformation of Early-Modern Philosophy* (Cambridge University Press, 2004), pp. 91-95.
[72] Dear, *Discipline and Experience*, PP. 26-27; P. Harrison, 'Natural History', in : P. Harrison, R. Numbers, M. Shank (Eds.), *Wrestling with Nature. From Omens to Science* (University of Chicago Press, 2011), pp. 118-125.

status of philosophical truth on the basis of their particularity. The New Philosophy had to craft its own framework to make a distinction between fanciful and genuine truth-claims. Its inspiration came from the world of commerce, with its epistemological emphasis on the sensibility of material goods and the passions they aroused, as well as its notion of credit and credibility.[73] It was equally influenced by the virtues of honesty and civility, which were part and parcel of the expected gentlemanly behavior within the social circles from which these natural philosophers invariably came.[74] In short, the solution to the grand epistemological problem was to replace truth on the basis of common-sense with truth on the basis of trusted/good-sense.

The best sense was, of course, one's own. The frequency with which late seventeenth century natural philosophers stressed that they made certain observations 'with their own eyes' instead of relying on mere 'hearsay' reveals not only a shifting methodology, but also the rising myth of *epistemological individualism*.[75] On the one hand, these philosophers preferred to rely on their own senses, and they spend considerably less time peering through their reading spectacles in favor of other types of lenses. On the other hand, they also relied on vast networks of technicians, toolmakers, amanuenses, servants and colleagues to produce their observations. Even if it was consistently and purposefully characterized as otherwise, seventeenth century natural philosophy was an inherently social endeavor, based on an economy of trust. The worth of any particular observations was consequently judged partly, if not chiefly, by the trustworthiness of the person who made them. This belief fed into the exclusionary practices of early modern science, as established gentleman scientists would claim the observations of their subordinates in order to make them scientifically presentable, credible truth claims. Hence, the economy of trust functioned on a system of credit: some people had it, and could lend it to the observations of others who did not.

Observing objects and events

The word 'observation' can be understood to be both the physical act of observing, as well as the narrative account of it.[76] This latter sense of the word could sometimes be eliminated

[73] See: Harold Cook, *Matters of Exchange. Commerce, Medicine, and Science in the Dutch Golden Age* (Yale University Press, 2007), pp. 1-41 explores the ideas of sensibility, pp. 42-81 explores the 'information economy'.
[74] See: Shapin, Steven, *A Social History of Truth. Civility and Science in Seventeenth-Century England* (University of Chicago Press, 1994), pp. 42-64 discusses the idea of the 'gentleman scientist'.
[75] Ibid, pp. 3-41, discusses the notion of epistemological individualism and the necessity of trust as a basis for knowledge.
[76] Dirk van Miert, *Communicating Observations in Early Modern Letters (1500–1675): Epistolography and Epistemology in the Age of the Scientific Revolution* (Warburg institute, 2013), 'Introduction', pp. 2-3; ''Philology and Empiricism: Observation and Description in the Correspondence of Joseph Scaliger (1540–1609)', pp. 89-93; 'Concluding Observations on Communicating Observations', pp. 223-247.

because the scientific economy of trust, like any other economy, was also based on material exchanges.[77] Collections of botanical specimens, fossils and other curiosities were frequently gifted or traded, or collected at the request of an acquaintance. Such transactions did not only generate goodwill, but also conferred credibility as the observations that depended on these objects could now be verified by others. In this way, many natural philosophers presented and donated their collections of various specimens to the Royal Society. When such transactions were not possible, drawings or narrative descriptions made by credible members of the society could function as substitutes. The various cultural and social meanings of observation have been well studied in recent publications, as have the techniques of relating observation in drawings or narrative. Although it sometimes seems to have tried its best to promote itself as such, the seventeenth century method of observation was not an entirely naïve epistemology, and the problems of creating, guiding and relating observations were widely discussed.[78]

A general characteristic of scientific observation in the seventeenth century was that it was concerned with objects. Following Descartes' dualistic model of material and spiritual substances, the material and the tangible rather than the invisible became the proper sphere of natural inquiry. Comets, plants, bodies, rocks, and at some point even the air all qualified as visible, tangible objects capable of being observed. Earthquakes on the other hand, though their effects are very material and tangible, were instances of a more epistemic ally complicated category: *events*. The event-ness of earthquakes posed significant conceptual and methodological problems to seventeenth century natural philosophers, due to their unpredictability, ephemerality, singularity, and 'objectlessness'.[79] These problems are key to explain why, more than in other areas of study, earthquake theorists were reliant on observations from outside the relatively closed economy of trust of gentleman scientists and their households.

The unpredictability of earthquakes and the fact that they often occurred far away from the traditional European centers of learning, meant that natural philosophers had very limited chances to experience an earthquake for themselves. And even when they did, it proved nearly impossible to capture the experience within an experimental observation. To illustrate this point it is useful to examine some attempts to observe earthquakes experimentally. In 1755, Vitaliano Donati made a pendulum from a spiral wire and a leaden ball and used it to

[77] Cook, *Matters of Exchange*; Daston, Lorraine (Ed.), *Biographies of Scientific Objects* (University of Chicago Press, 2000), pp. 1-14.
[78]Gianna Pomata, 'observation rising: birth of an epistemic genre', in: Lorraine Daston & Elizabeth Lunbeck (Eds.), *Histories of Scientific Observation* (University of Chicago Press, 2011), pp. 45-69; Van Miert, 'Concluding Observations', pp. 242-243.
[79]See the Stanford Encyclopedia entry for 'events', which also explicitly contrasts it with objects: https://plato.stanford.edu/entries/events/ . Last accessed 2 June 2019. See also: Keller, 'Sections and Views', p. 130.

record the intensity of some aftershocks by recording the swinging of the weight. The problem with this experiment was the unpredictability of earthquakes: he had to be near the device to record the swinging.[80] One pastor from Maastricht tried to solve this problem by suspending the contraption over his billiard table and to use the displacement of the billiard balls to derive 'the direction, and, to a certain point, the degree of the force'. An acquaintance of his tried something similar with a bucket of water. Both attempts failed for the same two reasons: 'my balls did not move; nor did I make the experiment till after the great shocks were passed.' Once the experimental set up was made ready, the chance for observation had already passed.[81]

The ephemerality of earthquake events posed a second set of problems. First of all, there was only a limited time span in which to observe them. Secondly, earthquake philosophers were also aware that earthquakes could not be reproduced through experiment because the observed effects were in large part dependent on the conditions of the place where the earthquake took place. It was simply impossible (not to mention undesirable) to recreate the 1692 sinking of Port Royal in London to confirm the reports. Because every earthquake was, in principle, a different event, there was much discussion whether people were even talking about the same phenomenon when they were talking about earthquakes. This confusion meant that it was almost impossible to confirm or reject a possible hypothesis for the cause of earthquakes, simply because it was not certain whether the observed phenomenon (or the lack of a certain phenomenon) was essential to all earthquakes, or only to some. The perceived solution to these problems was to gather larger quantities of observations to facilitate comparison.

As object-less events, earthquakes could also not be spatially contained within a laboratory or observatory, which had been established as a legitimate site of knowledge making, and where only authorized eyes had access to the phenomenon. The lack of an object also made it hard to direct observation. It already proved difficult enough for several individuals to provide comparable observations on the same aspects of many clear-cut objects, and the event-nature of earthquakes meant that many observations were downright unsuited for comparison. Where one observation made notes of the weather in the sky, the other looked at the ground. Where one smelled vapors, another reported on the sound the earthquake made. Once again, the obvious solution seemed to gather enough observations so that at least some of the accounts from the same event could be compared.

These factors also explain why otherwise 'incredible' observers were accredited with their own observations. No credible gentleman scientist could claim to have experienced an

[80] Donati, *Phil. Trans., Vol. 49*, p. 615.
[81] Vernede, *Phil. Trans., Vol. 49*, p. 665.

earthquake from far away, or have experienced the same earthquake multiple times from different positions. Hence there was a limited possibility of making observations credible by subsuming them under the name of a trustworthy authority.

Rebuilding the house of knowledge

It is clear that seventeenth century earthquakes not only demolished the buildings of towns and cities, but also shook the foundations of the house of knowledge. A new natural philosophy determined to use observations of particulars to derive universal truths, a specific economy of scientific credibility, and a natural phenomenon whose event-nature challenged existing practices of observation formed the principal epistemological challenges facing early modern earthquake theorists. Considering the struggles faced by Robert Hooke in formulating his own theory of earthquakes, it is not surprising that his discourses on the subject were filled with methodological reflections on these issues.[82] Rebuilding the house of knowledge was one of Hooke's favored metaphors for explaining how he went to work. As he saw it, his task was to survey the grounds and provide several hypotheses to guide observation. As a surveyor, Hooke proposed what kinds of knowledge would make suitable material to build with, so that future builders working on the foundations and walls of the structure would not have to rely on a mere 'heap of unpolished materials', as he himself had.[83] As such, Hooke did not make many observations of his own, and firmly denied the possibility of presenting an adequate theory of earthquakes within the current state of natural philosophical knowledge. Among the 'vast number of hands' required to compile and compose the building materials, Hooke counted:

> Readers of History, Criticks, Rangers and Namesetters of Things, Observers and Watchers of several Appearances, and progressions of Natural Operations and Perfections, Collectors of curious Productions, Experimenters and Examiners of Things by several Means and several Methods and Instruments'.[84]

This was a motley crew of truth-makers, reflecting Hooke's concern with establishing a broad foundation of empirical knowledge. Within this large group of observers, we can make a general distinction between experimental and historical observers. In formulating his own ideas on earthquakes Hooke made use of both kinds of sources, though he reflected more

[82] See: D.R. Oldroyd, 'Robert Hooke's Methodology of Science as exemplified in his 'Discourse of Earthquakes'', in: *The British Journal for the History of Science, Vol. 6, No.2* (1972).
[83] Hooke, *Discourses on Earthquakes*, p. 329.
[84] Hooke, *Discourses on Earthquakes*, p. 279.

extensively on the latter group, likely because he felt this type of knowledge required more justification. The concerns that Hooke formulated were echoed up until and beyond 1760, and provide us with an excellent focal point for discussing the problems and solutions of his contemporaries.[85] I propose that the different methods discussed by these philosophers to tackle the study of earthquakes can be classified into two categories. The first concerns ways of objectifying the earthquake event. This should be taken in the literal sense that, in lieu of analyzing the elusive event-nature of earthquakes, natural philosophers were seeking ways to study earthquakes through examining tangible objects such as rocks and gasses. The second category of solutions was to expand the range of incoming information by examining different forms of eye-witness accounts, from ancient history to contemporary newspapers and letters.

Examining objects often involved a newly developed type of method: the experiment. In the *Discourses on Earthquakes*, Hooke performed and reported on several experiments. He used an experiment evaporating sandy and salty water to argue that rocks and minerals could be formed out of liquids.[86] He mentioned an experiment that involved breaking a loadstone in order to show how the magnetic axis of the earth might have changed.[87] Three experiments involving a bubble of glass, a dish of water and a pendulum clock respectively, were elaborately explained in order to show that the earth and the surface of the seas are oval shaped.[88] Also included in the *Discourses* is a lecture on the various experiments conducted with mercury in the air-pump together with Robert Boyle in the 1660s.[89] An experiment with two slabs of marble and some dark colored oil endeavored to prove that some, if not all, representations of forms in rocks could indeed be caused by chance rather than by fossilized plants or animals.[90] None of these experiments were directly related to examining the immediate causes of earthquakes, but were instead meant to demonstrate the underlying physical principles on which Hooke's theory was founded.

Other philosophers did make more central use of experiments in their theoretical expositions on earthquakes. Martin Lister conducted several experiments demonstrating the capacity of pyrite vapors to combust 'spontaneously' and cited similar experiments conducted by a Dr. Power.[91] Nicholas Lemery's work on earthquakes was solely expounded

[85] For instance in the preface to the 1760 *History and Philosophy of Earthquakes* (un-paginated).
[86] Hooke, *Discourses on Earthquakes*, p. 294-295.
[87] Hooke, *Discourses on Earthquakes*, p. 328.
[88] Hooke, *Discourses on Earthquakes*, pp. 350-353.
[89] Hooke, *Discourses on Earthquakes*, pp. 365-370. Likely more an addition of the editor on the basis that the lecture from 1671 was redelivered at the Royal Society in 1684. In the text, Hooke does not relate this lecture to the general problems of earthquakes he was investigating at the time.
[90] Hooke, *Discourses on Earthquakes*, p. 436.
[91] Lister, *Of the nature of Earthquakes*, p. 516.

through descriptions of his chemical experiments.[92] Stephen Hales conducted some experiments on the 'mixture of pure and sulphurous air' and based part of his theory on an experiment by Denis Papin on the velocity of air.[93] John Flamsteed proposed a thought-experiment, accompanied with a diagram, considering the effect of an explosion in the air on the surrounding buildings and vessels.[94] Seeing the effects of these experiments amounted, at least rhetorically, to 'full proof', and served to reduce the complicated causes and effects of earthquakes to containable, manageable objects.[95] Martin Lister was not keen to immediately accept the various prodigious tales on the relation between earthquakes and lightning, such as the falling of iron-stones and the magnetic quality of lightning, even though they accorded with his own theory, and even though they were reported by such credible sources as Josephus Justus Scaliger, Gerolamo Cardano and Henry Oldenburg.[96] Such relations served as useful pointers to interesting phenomena, but the status of hard evidence was clearly restricted to objects for Lister: '*This I am sure of*, I have a petrified piece of ash, which is magnetick, that is, the pyrites in Succo; which makes it probably it may be magnetick also in vapour'.[97]

The ability to examine phenomena for oneself was celebrated by more authors. Stephen Hales both opened and closed his treatise with a lengthy account of his own experience during the 1750 earthquakes.[98] William Stukeley recalled his trips down several caves and mines, noting the absence of a great subterranean fire or anything else that could explain earthquakes.[99] Robert Hooke's discourses were filled with elaborate descriptions of fossils and rock strata.[100] At times Hooke claimed to be able to produce evidence from a 'multitude of authorities', but for the sake of brevity he often related only his own observations and sometimes those of one 'a worthy member of this Society'.[101] In these sections, Hooke seemed to be at his most confident drawing conclusions:

Figure 7. Drawings of fossils in Hooke's iscourses on Earthquakes.

[92] See: Nicolas Lémery, 'Explication physique et chymique des Feux souterrains, des Tremblemens de Terre, des Ouragans, des Eclairs & du Tonnerre', in: *Histoire de l'Académie Royale des Sciences, Année M. DCC, avec les Mémoires de Mathématiques et de Physique pour la même année.* (Amsterdam: Gerard Kuyper, 1700).
[93] Hales, *Some Considerations*, pp. 672-3, 675.
[94] Flamsteed, p. 15. See figure 6.
[95] Hales, *Some Considerations*, p. 674.
[96] Lister, *Of the nature of Earthquakes*, pp. 518-519.
[97] Lister, *Of the nature of Earthquakes*, p. 519. Emphasis mine.
[98] Hales, *Some Considerations*, pp. 671-2, 680.
[99] Stukeley, *Concerning the Causes of Earthquakes*, p. 658.
[100] Hooke, *Discourses on Earthquakes*, for instance on pp. 281-298, 311-312, 318-328.
[101] Hooke, *Discourses on Earthquakes*, p. 334.

> 'And which seems to confirm this Conjecture *much more than any of the former Arguments*, I had this last Summer an Opportunity to observe upon the South-part of England, in a Clift whose Bottom the Sea wash'd, that at a good height in the Clift above the Surface of the Water, there was a Layer, as I may call it, or Vein of Shells, which was extended in length for some Miles'.[102]

In rest of this passage, Hooke continues to 'digg out', 'examine', 'find', 'crush' and 'break' many hundreds of shell fish fossils. The physical observations and manipulations clearly take center stage in the narrative and lend credibility to the endeavor. Yet unlike Lister and Lemery, Hooke did not present such physical objects and the operations performed on them as notably superior evidence to other types of evidence. In fact, seeing that Hooke's primary goal in examining earthquakes was to provide a plausible alternative to flood theories of fossil displacement, he faced a considerable challenge in proving that fossils could serve as meaningful evidence for a theory on earthquakes. To establish fossils as legitimate evidence, Hooke compared them to coins and urns from classical times, the favored source material of antiquarians and civil historians of the time.[103] While sticking to the principle of examining objects, Hooke's analogy did invert the relation between physical and historical evidence. For the purposes of Hooke's argument both were important, and he stressed that what he had 'indeavoured to shew by Experiment and Inspection' was not 'destitute of good Authority, proved from very eminent Authors both Antient (sic.) and Modern, to make out the Truth and Certainty thereof'.[104] To arrive at 'truth and certainty', Hooke paradoxically turned towards the source of knowledge that was commonly derided as most untruthful and uncertain: historical and contemporary eye-witnesses.

History and geology

For most earthquake theorists, historical accounts and natural objects continued to serve as necessary and complementary strands of evidence. Experiments done with pyrites and lightning proved useful in creating hypotheses, but few would argue that these hypotheses did not need to be affirmed by observations made within actual earthquakes. Experiencing an earthquake yielded a privileged form of knowledge, and hence the philosopher William Stukeley was glad to have experienced one himself in 1750, stating that 'we could not have

[102] Hooke, *Discourses on Earthquakes*, p. 292.
[103] Hooke, *Discourses on Earthquakes*, pp. 319, 321, 335. See also: Francis Haskell, *History and its lmages. Art and the Interpretation of the Past* (Yale University Press, 1993), pp. 9-172.
(1993). William Stukeley continued the analogy and made use of a roman coin depicting an earthquake as geological evidence. Stukeley, *Concerning the Causes of Earhtuakes*, p. 361.
[104] Hooke, *Discourses on Earthquakes*, p. 407.

form'd a proper Idea [of 'so extraordinary a Motion'], had we not repeatedly both seen and felt it'.[105] Because earthquakes were not an everyday occurrence, theorists had to resort to both recent and historical descriptions. Many accounts relied upon eye-witness, though often without mentioning them. Phrases such as 'from Ancient history we know', 'it is well observed that' and 'In Italy it is reported', were among the most common stock phrases of earthquake scholars.[106] Other times, references to authorities (who themselves made vague allusions to accounts from sailors and mineworkers) stood in for actual observations.[107] However, it has been argued that some form of historical methodology was developing within geological natural philosophy in the seventeenth century, in reference to the idea that the timescale of geological change was very short.[108] In what follows, I argue that the rise of a 'historical geology' in the seventeenth century was quickly superseded by attempts, in the spirit of Hooke, to compile better accounts of *recent* earthquakes that were more helpful than the limited historical accounts available.

Where Aristotle was the traditional ancient authority for the theoretical side of earthquakes philosophy, the most cited (both by name and under the header 'ancients') historical accounts were those of Pliny the Elder, Seneca, and to a lesser extent Herodotus.[109] Although by the middle of the eighteenth century earthquake theorists were generally dismissive about the quality of ancient accounts of earthquakes, the bare 'matters of fact' of these accounts kept being tossed around relatively uncritically by most. One of the few natural philosophers to not rely on ancient testimony was John Flamsteed, though it has been suggested this was the result of his very limited knowledge of the classics.[110] Between two versions of his draft letter on earthquakes, Flamsteed did insert a disingenuously general 'with the consent of Aristotle and Pliny' into a sentence however: a clear indication that the voice of the ancients still held some persuasive power. As useful as the classical authors could be, only Robert Hooke went as far to accredit any meaning to the tales of Atlantis in Plato's *Timmaeus*, the explorations of Hanno the Carthaginian, and the *Metamorphoses* of Ovid. Although he clearly regarded the accounts as fictional, Hooke's reliance on massive

[105] Stukeley, 'Concerning the Causes of Earthquakes', p. 657. Emphasis mine.
[106] Robert Hooke could produce the testimony of 'a great many others yet more modern, besides the testimony and judgement of diverse others, who have themselves declared their Judgment by word of Mouth.', p. 409, see also pp. 421, 429-431. William Stukeley refers to 'ancient history' (pp. 645, 661, 665, 746), and the testimony of 'many people' (pp. 644, 659, 663-4, 667). Martin Lister also mentions some 'ancients' as well as some 'chemists' (*Of the nature of Earthquakes* pp. 514, 518). These are just a selection, such vague references are about as common as more precise citations.
[107] Hales, *Some Considerations*, pp. 676-679.
[108] Most prominently: Rhoda Rappaport, *When Geologists were Historians 1665-1750* (Cornell University Press, 1997); David Oldroyd, 'Historicism and the rise of Historical Geology, part 1', in: *History of Science, xvii* (1979); Cecil Schneer, 'The Rise of Historical Geology in the Seventeenth Century', in: *Isis Vol. 45, No. 3,* (1954), pp. 256-268.
[109] Willmoth, 'John Flamsteed's letter', pp. 37. For instance, in Hooke these particular ancient authorities are mentioned on pages 299-302, 307, 310-11, 314, 407-8, 441).
[110] Willmoth, 'John Flamsteed's letter', pp. 38.

earthquake events in the far past to explain his theory motivated him to read these works as containing some truth about enormous historical earthquakes that turned into myths.[111]

The Bible was another authoritative account on earthquakes. Hooke discarded the flood hypothesis of fossil displacement, but took care to note that he discarded it precisely because it did not accord with biblical evidence.[112] A rich tradition of scholarship after 1692 tried to find similarities between biblical descriptions of earthquakes and the events that happened in Jamaica, England and beyond in recent years.[113] Part of the reason that the reverend Stephen Hales sought to reconcile Stukeley's conjectures on the electrical nature of earthquake with older theories was that discarding the latter meant discarding the biblical description stating that 'fire and hail, snow and vapour, and stormy wind fulfil his word'.[114] Though the word of God commanded authority, the relatively sparse descriptions of earthquakes and related events ultimately meant that biblical statements could make up only a very limited part of the available evidence. Biblical authority was reserved to explain the moral significance of earthquakes, and not their natural cause.

More recent histories were also gladly consulted. Hooke extensively quoted from the chronicles of Thomas of Childrey (c1350-1407), as did William Stukeley. Hooke also cited the sometimes rather dubious travel accounts collected in Samuel Purchas' *Pilgrimages*, as well as those noted down by José de Acosta in the *Natura Novi Orbis* (1596) and by Jan Huygen van Linschoten in the translated V*oyages into ye Easte and West Indies* (1598, Dutch original 1596).[115] Stephen Hales closely studied the observations made by Georges-Louis Leclerc, Comte de Buffon in the first part of his *Histoire Naturelle, Générale et Particulière* (1749).[116] Other accounts, either made or recounted by moderns such Athanasius Kircher, Bartholomäus Keckermann, Bernhardus Varenius, Nicolaes Witsen, Georgius Agricola, Conrad Gesner and Simon Stevin, could naturally also not go unmentioned by any respectable earthquake scholar.[117] Yet, there were considerable problems with this historical method. Robert Hooke noted as early as 1697 that many authors were wont to simply copy already known accounts from one another.[118] Conveniently for his own theory, Hooke also explained that the most severe earthquakes might not have been recorded at all because their destructive capacity left nobody alive to report them.[119] Similarly, most earthquake accounts came from cities simply because there were more

[111] Hooke, *Discourses on Earthquakes*, pp. 372-413.
[112] Hooke, *Discourses on Earthquakes*, pp. 314, 341, 412.
[113] See particularly: Thomas Doolittle, *Earthquakes Explained and Practically Improved* (London, 1693); Van de Wetering, 'Moralizing in Puritan Natural Science', p. 421.
[114] Hales, *Some Considerations*, pp. 670-671.
[115] Hooke, *Discourses on Earthquakes*, pp. 300-315.
[116] Hales, *Some Considerations*, pp. 676-679.
[117] Hooke, *Discourses on Earthquakes*, pp. 301-3, 409; Flamsteed, *Letter Concerning Earthquakes* p. 2.
[118] Hooke, *Discourses on Earthquakes*, pp. 444-445, also 439.
[119] Hooke, *Discourses on Earthquakes*, pp. 310-311.

people there to record it, while the most interesting earthquakes might have happened far from civilization.[120] All in all, the available historical record was deemed too small to function as a reliable depository of knowledge.

When original historical accounts were available, it proved difficult to ascertain whether an account was indeed an observation made by eye-witnesses or drawn up later, sometimes many years after the event.[121] Besides the issue of memory, there was also a concern that historical observations were simply misguided. Hooke had already postulated that observations needed to be guided by theory to be useful, and William Stukeley too had proposed that both Ancient and Modern accounts were unreliable because they were simply not looking at the right phenomena.[122] Hooke therefore proposed to temporarily bracket his historical investigations:

> Since many of those occurencies having been long since produced, and the relations of them made by such as were not eye-witnesses, many of the particular matters of Fact have been doubted or disputed; I shall therefore take notice of some particular instances which have happened within our own Memory[123]

Despite this intention, only very few of the observations cited in Hooke's *Discourses* came from such recent accounts. They consisted of a handful of letters obtained by either the Royal Society or Hooke himself, and a slightly larger number of printed pamphlets and newspapers. Early newspapers proved an especially worthwhile resource for Hooke, who remarked that many earthquakes were 'probably never recorded to Posterity, if the Gazett and News Papers had not taken notice of it'.[124] Their rather dry matter-of-fact relations of the time and place of earthquakes, sometimes combined with more elaborate descriptions, allowed Hooke to make a long analysis of an earthquake in the Leeward Islands in 1690, although he kept expressing a wish for more elaborate accounts.[125] Other writers on earthquakes also explicitly referred to printed relations as useful sources of information. Along with better observational instruments, this new medium was one of the ways that Hooke envisioned the house of knowledge would be rebuilt: the general argument made by Hooke was not that contemporary accounts could already provide a sufficient source of information on earthquakes. Rather, in stressing the differences between historical and

[120]Hooke, *Discourses on Earthquakes*, p. 430. Compare this with Stukeley's interpretation, who argued that the prevalence of earthquakes near cities was a sign that they were directed by God specifically for humans. Stukeley, 'On the Causes of Earthquakes', p. 645.
[121] Hooke, *Discourses on Earthquakes*, pp. 444-445, 334.
[122] Hooke, *Discourses on Earthquakes*, pp. 280, 329-330; Stukeley, 'Concerning the Causes of Earthquakes', pp. 657-658.
[123] Hooke, *Discourses on Earthquakes*, p. 292.
[124] Hooke, *Discourses on Earthquakes*, p. 446.
[125] Hooke, *Discourses on Earthquakes*, pp. 416-417.

contemporary accounts, Hooke was hoping to ensure that future 'historical accounts' would be better suited to natural philosophical inquiry.

Stukeley's remarks on the discrepancy between history (both ancient and modern) and contemporary observation, were starker: 'It would be childish to make a long recital of Particulars from History; for had we no other sort of Notices of Earthquakes?'[126] Stukeley's use of 'other notices' was also more elaborate than Hooke's. He widely cited the observations of shepherds and gardeners, newspapers and private correspondents, acquaintances and strangers. Writing more than half a century after Hooke, Stukeley could rely on a wealth of observations noted down for posterity in the *Philosophical Transactions* of the Royal Society. These observations were mostly made in the last few years, and came with their own problems of reliability. Yet they testified to a concerted effort between 1665 and 1755 to systematically provide the empirical building blocks required to rebuild the 'house of knowledge'.

Conclusion

The increasing proliferation of earthquake testimonies in both print and manuscript, and their extensive use by earthquake theorists as they became available, was the result of a long methodological and conceptual struggle. Rethinking Aristotle's particular views on earthquakes had yielded a wide field of competing theories on the nature of the disaster. The epistemological demands of the New Philosophy, on the other hand, contributed to two distinct and complementary methodological approaches. One approach sought to supply credible hypotheses, studying the processes of earthquakes by way of analogy in conceptually easy to grasp objects. The other approach set out to confirm these hypotheses with information from actual earthquakes. Historical accounts were a temporarily useful source, but were quickly recognized to provide neither the quantity nor the quality needed to settle the disputes between the various earthquake theories. For geology to establish itself as legitimate natural philosophy, future observations needed to be more numerous, directed, and precise. Because of the event-nature of earthquakes, it was nearly impossible for natural philosophers to contribute such observations themselves. Hence, they came to rely on sources that were perceived as inherently unreliable: the testimonies of lay people. In the rest of this thesis, I will explore how these sources of information could be incorporated into a system of knowledge-making that saw them as inherently problematic, and chart how these lay observers shaped the field of eighteenth century earthquake philosophy.

[126] Stukeley, 'Concerning the Causes of Earthquakes', p. 668.

2. How to Recognize an Earthquake

How were credibility and truth constructed in the accounts of earthquake observers?

On February 8 1750, Henry Baker experienced an earthquake in London. As he was walking through Chancery Lane, Baker noticed that many people came running onto the street and started speaking of a strange shaking of their houses, saying that it seemed like some large quantity of timber had fallen down somewhere. Continuing on his way, Baker walked along Holbourn, where he found people 'under the same consternation, and expressing themselves nearly in the same manner'. At Gray's Inn a crowd had gathered on the square, listening to a lamp lighter who felt like someone had been shaking the ladder he was standing on. Inside the Inn's library, a gentleman reckoned a box or heap of books had fallen down on the floor above. Despite the general confusion, Baker was in no apparent rush to return to his family, and he stopped to talk to many people in the streets of London before arriving at his home in Catherine Street. There he talked with his wife, his son and his neighbors, who had all felt the earthquake. To use his own words, Baker was clearly 'pretty curious to inquire of people in different places, to judge the better from their several reports', and noted: 'what therefore so many people, in different streets, at great distances from each other, have been surprised at, cannot be only fancy, but must be owing to some real cause.'[1]

Henry Baker's account is illustrative for the themes treated in this chapter, which focuses on the ways in which both the earthquake event and the credibility of its observers were constructed. In just a few pages, Baker acquaints us with a wide network of observers that include esteemed barristers at the Gray's Inn, his own household and a variety of people on the streets of London. In later reports to the Royal Society (of which he was a fellow), Baker would make good on his promise to deliver more observations in order to compare 'the

[1] Baker, *Phil. Trans. Vol. 46, no. 497*, pp. 601-603.

accounts of people in different situations'.[2] Yet Baker's narrative also points at an inherent difficulty arising from the use of eye-witness reports. One the one hand, the fact that so many individuals reported the same sensation confirmed that the earthquake had not just been mere fancy. On the other hand, Baker clearly noted with some surprise that all these people expressed themselves 'nearly in the same manner'.

Figure 8. Henry Baker's route through London as described in his letter.

Baker was aware that as people went out into the streets and talked with one another they began to use the same set of metaphors to describe the experience, forming a consensus in their stories and descriptions.[3] In fact, the expressions used by the inhabitants of London were part of a relatively stable repertoire of expressions used to describe earthquakes. These metaphors were not only a convenient shorthand for the more complex reality of earthquakes, the ability to employ this vocabulary made one's story relatable and credible. The reverend John Nixon for instance knew that he could trust an observation that was given to him because the observer 'very properly' likened the sound of the earthquake 'to the explosion of a cannon at a distance'.[4]

Yet these descriptions, like the explosion of a cannon, the rattling of a coach wagon, a hollow wind or some heavy object falling overhead, were not always metaphors. More often

[2] Ibidem.
[3] See also: *R.S.A., LBO/21/43*, pp. 159-160. In this account, three sisters sleeping in the same room recount discussing the various possible interpretations of the sensation, before settling together on the hypothesis of an earthquake.
[4] Smith, *Phil. Trans., Vol. 46, no. 497*, p. 730.

than not, there was at first a considerable confusion about what people had actually experienced. Signor Pascal Perdini remembered having a discussion about a sudden shock when sitting in a friend's house, since 'some of us thought it proceeded from walking in another chamber, [while] others thought it was a shock of an earthquake'.[5] Elizabeth Slade, after feeling her bed and curtains shake in Portsmouth, first thought it was the wind ('which she then heard blow strong'), but when she went up to close the window she 'found the sashes intirely close'.[6] Henry Baker took care to mention in his letter that he received no report of a powder mill exploding near London, because he knew that the two phenomena were often compared and confused.[7] A close correspondent of Baker, the reverend Henry Miles, was more cautious, writing at first that he was not yet able to say 'whether it was caused by the blowing up of a powder mill, or by an earthquake'.[8] Several days later Miles was also writing unambiguously about an earthquake, now *comparing* the noise to that of a gunpowder explosion.[9]

An even larger problem was that many who were asked to give an account of a known earthquake were forced to admit that they had felt nothing at all. Even Henry Baker confessed that his experience of the event came solely through observations made by others, as he himself felt nothing while walking through the streets of London.[10] In some rooms, half the people felt the earth shake while the other half felt nothing.[11] One of the watchmen overseeing the grounds of the Calico cotton printers in London testified that he had not felt anything at all, while another remarked that he had heard an unusual noise, but: 'not so remarkable as to have caused him to take notice of it to any one, had he not afterwards been told there was an earthquake'.[12] In fact, many 'observers' (including John Wallis and Robert Boyle) confirmed that they had not thought of an earthquake until they heard other people refer to the event as such.[13]

Clearly, observers often needed to convince both themselves and others that had felt an earthquake. The reason for this was simple. Roughly two-thirds of the earthquake accounts in the *Philosophical Transactions* concerned earthquakes in England, which was not exactly home to frequent or violent earthquakes. The fact that early earthquake science in the Royal

[5] Pedini, *Phil. Trans., Vol. 42, no. 463*, p. 79.
[6] Barlow, *Phil. Trans., Vol. 46, no. 497*, p. 694.
[7] Baker, *Phil. Trans. Vol. 46, no. 497*, p. 602. For other such comments see for instance: Freeman, *Phil. Trans., Vol. 46, no. 497*, p. 605; Martyn, *Phil. Trans., Vol. 46, no. 497*, p. 610. Bowman, *Phil. Trans., Vol. 46, no. 497*, p. 685, remarks with some sense of pride that he obviously did not confuse the earthquake for a powder-mill explosion because they don't work such large quantities there to cause such a rumble.
[8] Miles, *Phil. Trans., Vol. 46, no. 497*, p. 619.
[9] Miles, *Phil. Trans., Vol. 46, no. 497*, p. 628.
[10] Baker, *Phil. Trans. Vol. 46, no. 497*, p. 603. For another 'main relator' who did not observe the earthquake himself, see: Warren, *Phil. Trans., Vol. 49*, pp. 579-581.
[11] Flamsteed, *Letter Concerning Earthquakes*, P. 7
[12] Miles, Phil. Trans., Vol. 46, no. 497, p. 640.
[13] For instance Mr. Barber's mother, Dr. St. Legerm, and Rev. Mr. Bunce in Warren, *Phil. Trans., Vol. 49*, p. 580.

Society focused on these specific events means it dealt with a specific type of minor earthquakes of which it was not always clear *whether they were earthquakes at all*. As a result, the first major question of an earthquake science based on lay observations was how to convincingly relate that one had felt an earthquake. This was a highly social form of truth-making, allowing us to employ our second mode of analysis: the interaction between social and epistemological tensions. The first section briefly explores just why these small English earthquakes received such theoretical and empirical importance. The three subsequent sections investigate how prior experience with earthquakes, the language used to describe them, and verification through additional observations were employed to produce credibility. Although naturalists harbored suspicions against the observations made by lay people, the increasing prominence of servants, women and non-Europeans (in this order) as observers necessitated more detailed methods of discerning credibility. In short, there was a growing discourse of meta-expertise, discerning the elements which would make one a qualified observer. This was a discourse *about* observers rather than *with* observers, and was as much concerned with maintaining social orders as it was concerned with producing natural philosophical truths. The final section examines these processes up close in a small case study of an earthquake observed by one Elizabeth Cornwallis in Suffolk in 1750.

An Englishman abroad

Why did those interested in earthquakes not consider the more impactful and clear-cut disasters that happened in abundance all across the world? The geographic spread of observations is a clear case of how theory and observation informed one another. Where modern day seismologists would look at known tectonic fault lines as obvious places to find earthquakes, seventeenth and eighteenth-century earthquake theorists formulated their own reasons to look at European earthquakes. Robert Hook remarked that before studying earthquakes around the world, it would be wise:

> to be first of all a little acquainted with what we have at home, that thereby we may the better be able to observe and judge of what those remote parts may present us with, whether they be like our own or not, in what they agree, and in what they differ, that these we know at home may be the standards and touch-stones of all the rest we meet withal abroad'.[14]

[14] Hooke, *Discourses*, p. 371.

Hooke's statement reflects a growing belief in universally valid laws of nature.[15] It posited that the underlying causes of those earthquakes in England would be the exactly same around the world. The statement also betrays the entrenched belief that European nature was the most 'natural' of all, the 'standard and touch-stone' for scientific inquiry, and that observations made elsewhere were interesting only as deviations from this norm.[16] It was commonly acknowledged that earthquakes in the Americas were more powerful than those in Europe, but this discrepancy was generally explained by stating that climatic conditions in other continents were more 'degenerate', leading to more destructive natural events.[17] In part then, a discourse of European centrality and supremacy informed the focus on English earthquakes.

At the same time, this discourse largely functioned to legitimize *post-factum* several collective decisions that had been made for more mundane reasons. The practical realities that informed which accounts were gathered and trusted and which were not, decisively informed the outlook of earthquake theorists. The first of these realities was the limited range of correspondence networks. When the London legal reporter James Burrow suspected that a letter reporting an earthquake in Surrey was based only on 'hearsay and report', he ventured out to the parish of Lingfield to investigate. He talked with several parishioners, and assured himself that 'the earthquake was 'certainly and undoubtedly felt and observed by some persons'.[18] From the perspective of others within the Royal Society, Burrow's report was as much based on 'hearsay' as the letter that was sent to him, yet the possibility of venturing out (Lingfield is only 25 miles from central London) and skipping one step in the communication process added to the relative trustworthiness of the account. This was obviously less likely to happen further away from home.

It is not surprising that the second and third largest concentrations of earthquake accounts came from places that had close connections to England: East coast America (as an English colony) and Italy (as both a hotbed of seismic activity and a major European center of learning).[19] In other places, natural philosophers with an interest in earthquakes seemed to have a much harder time reaching correspondents. A case in point were the destructive earthquakes in Lima in 1687 and 1746. Although printed news of these events arrived in England, the exclusionary nature of Spanish imperial politics meant that English natural

[15] On Hooke's views relating to this, see: Joy, 'scientific explanation from formal causes to laws of nature', pp. 90-91.
[16] K. Kupperman, 'The Puzzle of the American Climate in the Early Colonial Period', in: *American Historical Review, `Vol. 87* (1982), pp. 1262-1289; Sam White, 'Unpuzzling American Climate: New World Experience and the Foundations of a New Science', in: *Isis, Vol. 106, No.3* (2015), pp. 544-566.
[17] Daniela Bleichmar, Paula De Vos, Kristin Huffine, and Kevin Sheehan (Eds.), *Science in the Spanish and Portugese Empires, 1500-1800* (Stanford University Press, 2009), pp. 25, 155, 288.
[18] Burrow, *Phil. Trans., Vol. 50*, pp. 614-617.
[19] There were 9 nine reports from Italy, and 8 from the Americas.

philosophers had few to no contacts in the city to approach for more detailed accounts. As a result, no eye-witness reports of these highly significant events were included in the *Philosophical Transactions*.[20]

There was also a clear distrust of non-European observers. Several accounts from Constantinople and Aleppo in the 1750s were all written by Englishmen residing there, and were overall much less likely to include observations made by other locals than reports from England. Murdock Mackenzie, a physician working in Constantinople in 1754, wrote for instance that he would only write about 'what I have seen'.[21] While this was not necessarily true (he for instance relied on anonymous sources to recount that some Janissaries on guard duty were killed during the earthquake), Mackenzie played into a rhetorical expectation that privileged his own observation as an Englishman over the inhabitants of Constantinople, even if the latter were more familiar with earthquakes. Patrick Russell, a Scottish naturalist and surgeon writing from Aleppo in 1759, more explicitly attributed his unwillingness to collect local observations to cultural prejudice, complaining that 'little accuracy can be expected, and the eastern disposition to exaggeration reigns, at present, universally.'[22] James Porter, also writing from Constantinople, preferred to communicate 'only those [shocks], which for the most part I felt', although 'there may be two or three founded on such reports, as I could fully depend on'. Porter thus established himself as the arbiter of reliability in his letter. Regarding an earthquake which was reported in Sivas (some 550 miles away from Constantinople), he noted that he could not 'be informed of the hour or precise time when they felt it there: I heard that it was on the same day. These people are not sufficiently observing to remember to an hour.'[23]

A report from Sumatra, written by one Mr. Perry, similarly stressed that Perry had felt the shocks himself and had not relied on other accounts. While he noted some of the visible effects of the earthquake around the British Fort Malborough, Perry concluded that 'these are all the ill effects, that have come to our knowledge, but it is reasonable to suppose, not all the damage that has happened upon the island'.[24] Limited colonial control over the land and the people living there clearly formed barriers to gathering information, in contrast to the more accessible and more socially controlled English landscape. Hence, while the cause of the 1692 Jamaica earthquake was thought to lie in the island's central mountains, no conclusive

[20] Of the 1687 earthquake there is a very small piece sent in by Hans Sloane, this however is an excerpt from a printed pamphlet: Sloane, *Phil. Trans., Vol. 18, no. 209*, p. 81.
[21] Mackenzie, *Phil. Trans. Vol. 48*, pp 319-321. Another report from Mackenzie, concerning an earthquake in Smyrna in 1739 likewise only recounted Mackenzie's own observations and not those of others. See: Mackenzie, *Phil. Trans. Vol. 46, no. 497*, pp. 700-701.
[22] Russell, *Phil. Trans. Vol. 52*, p. 532.
[23] Porter, *Phil. Trans., Vol. 49*, pp. 121, 123. On the difficulties of timing observations, see chapter 3 of this thesis.
[24] Perry, *Phil. Trans., Vol. 50*, pp. 491-492.

observations could be made there because, as one relator observed: 'those wild Desert places, being very rarely or never visited by any body, not by Negro's themselves, we are yet ignorant of what happened there'.[25] Even if a community of maroons was holding up in the mountains at the time of the earthquake they were not going to come share their observations with white English settlers, widespread colonial fantasies of runaway slaves returning to their masters out of sheer fright notwithstanding.[26] In general, testimonies from black slaves and freedmen and women are conspicuously missing from the Port Royal relations, possibly reflecting the widespread fear among the white population of a slave revolt in the wake of the earthquake.[27]

A final reason why the relatively unexciting English earthquakes received such methodological primacy was precisely their unexciting nature. As we will see, many observers tried to establish their reliability by stressing how they managed to keep their wits and watch the events unfold, something which was much more difficult in situations that were actually life-threatening. This also explains why many of the non-English earthquakes that were included in the *Philosophical Transactions* were most often also minor events. Of the more destructive earthquakes that occurred between 1665 and 1755, only the Port Royal earthquake of 1692 and the Lisbon earthquake of 1755 produced more than one account in the *Philosophical Transactions*. When we look closer at the Lisbon accounts moreover, we find that only three of the observations were made within Lisbon itself. Others were made from nearby villages such as Colares, and even more accounts came from nearby

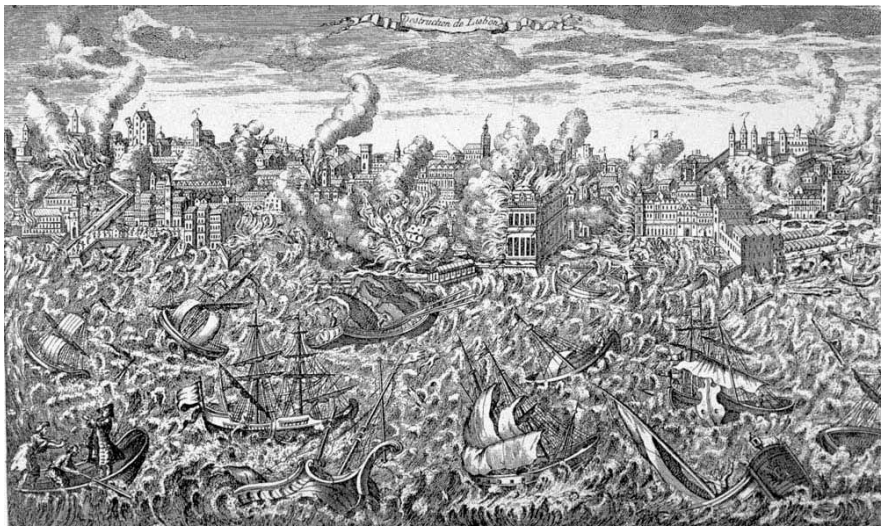

Figure 9. The destruction of Lisbon depicted in a copper engraving, 1755.

[25] Sloane, *Phil. Trans., Vol. 18, no. 209*, p. 95.
[26] Sloane, *Phil. Trans., Vol. 18, no. 209*, p. 87.
[27] See for instance: Anon., *Account of the Late Earthquake in Jamaica* (London, 1692), pp. 11-12.

cities such as Porto and Cadiz, where the earthquake had been far less destructive. The bulk of the 1755 reports however, concerned minor shocks felt along both sides of the Atlantic Ocean, and in mainland Europe.[28]

The report of an English merchant named Richard Wolfall, who was present in Lisbon at the time of the earthquake, serves to illustrate the point. Wolfall recounts how everyone rushed to open spaces in a frenzy after the first shocks, seemingly forgetting all about friends and loved ones, and caring even less for stopping in the street to observe the chaos from a natural philosophical point of view. Of the thirty-eight inhabitants of the house where he was lodging, only he and three others survived. This not only points at the great mortality of the earthquake, but also at how Wolfall's personal network of contacts (and hence, other possible eye-witnesses) was considerably reduced. Although Wolfall found the clarity of mind to compose a letter to the Royal Society, he begged them to 'please dress it up in different language' before presenting it (a request that was apparently not honoured), and he hoped his future correspondence would be 'more clear and correct than this confused letter I send at present'. To stress his point, Wolfall relates that he wrote his letter on a garden wall, using paper he had acquired only 'by accident'. Clearly, disasters such as the Lisbon earthquake did not provide the ideal conditions for deliberate observations and calm reflection.[29]

Character, skill and experience

Connections, prejudice and convenience played a limiting factor in the geographical spread of earthquake reports, and consequently also in the type of earthquakes that were reported. Unlike the destructive Lisbon earthquake, the trembles felt across England were not obvious earthquakes. Given learned prejudices against the character and observational skills of lay observers, the first challenge for earthquake observers was to prove that their account was not based on mere fancy. Hence, one relator felt the need to defend his network of lay observers with the following disclaimer:

> It is proper to observe that the following relations are not made by mean, ignorant, or fanciful people, but by persons of good sense, whose veracity is unquestionable, and whose judgement in this case is, I think, rational and just.[30]

[28] 3 came from Lisbon itself, 2 from nearby villages, 5 from Opporto, 5 from other nearby cities. 30 reports were drawn up from various other places, nowhere near Lisbon.
[29] Wolfall, *Phil. Trans., Vol. 49*, pp. 402-407.
[30] Barlow, *Phil. Trans., Vol. 46, no. 497*, pp. 692-693.

In this way, the authoritative word of a gentleman naturalist could guarantee the credibility of others. Every account that was thus conveyed was implicitly deemed trustworthy to some degree, and less credible reports were accompanied with explicit remarks. Although these accounts were used to great benefit, it remained proper to express awareness of their shortcomings in order to signal methodological sophistication: 'I find that the men of some towns speak dubiously', one correspondent to the Royal Society wrote in 1683, and 'one knows not how to believe the countrymen that are oftentime unobserving'.[31] This statement accurately, though paradoxically, reflects the two charges that were commonly levelled against lay observers. The first charge was that they observed too much, and invented additional phenomena that could not be verified by others. Hence, relators of earthquake accounts often stressed that they had filtered out the 'additional stories that are commonly raised on such occasions'.[32] By nature of this practice we know little of what these 'additional stories' exactly were. However, some clues remain scattered across various texts. The reverend John Nixon, writing about an earthquake in Sutton, received a report from a local observer about a meteor, which he 'rank'd among the other mirabilia usually invented upon these occasions to amuse the vulgar'.[33] Other reports that were later discredited included sightings of fire balls in the sky and intense lightning.[34] Yet the line between proper observation and fancy was difficult to draw. The reverend Paul Doddrige remarked that the report of a ball of fire seen over Northampton following an earthquake was not at all certain, but, like several others, dedicated part of his letter to describe the Aurora Borealis which was sighted around the same time.[35] Other letters included accounts of remarkably intense lightning and curious behaviour of animals, or were suggestively grouped together with reports on strange magnetic effects.[36]

The second charge was that lay observers reported too little, because they were 'unobserving'. The phrase *unobserving* could mean that they did not feel the earthquake at all, but also that when they did, they were unable to make the proper natural philosophical observations demanded by earthquake theorists. For this reason, one correspondent wished

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[31] Pigot, *Phil. Trans. Vol. 13, no. 151*, p. 316.
[32] Pedini, *Phil. Trans., Vol. 42, no. 463*, p. 88. See also: Cave, *Phil. Trans., Vol. 46, no. 497*, p. 705: 'I am cautious of transmitting any accounts, but such as I think may be depended upon'.
[33] Nixon, *Phil. Trans., Vol. 46, no. 497*, p. 712.
[34] In Sloane, *Phil. Trans., Vol. 18, no. 209*, p. 87, for instance, Hans Sloane's anonymous correspondent writes: 'as to the Fire-balls which you heard was seen in the Air; it was a great Falshood: for I neither saw nor heard (during our Months stay after the Earthquake any such thing'. Also see: Bonajutus, *Phil. Trans., Vol. 18, no. 207*, pp. 3-4; Smith, *Phil. Trans., Vol. 46, no. 497*, p. 728;; Trembley, *Phil. Trans., Vol. 46, no. 497*, pp. 610-611; Martyn, *Phil. Trans., Vol. 46, no. 497*, p. 631; Forster, *Phil. Trans., Vol. 45, no. 488*, p. 400; Lethieullier, *Phil. Trans., Vol. 46, no. 497*, pp. 611-613. Similarly, the following accounts thought it important to explicitly mention the lack of these phenomena: Miles, Phil. Trans., *Vol. 46, no. 497*, p. 630; Burrat, Phil. Trans., Vol. 46, no. 497, p. 681; Arderon, *Phil. Trans., Vol. 46, no. 497*, p. 698.
[35] Doddrige, *Phil. Trans., Vol. 46, no. 497*, pp. 718-721; Johnson, *Phil. Trans., Vol. 46, no. 497*, pp. 725-726.
[36] *Phil. Trans., Vol. 39, no. 437*, p. 74. For more detail on the attentiveness to the effects on animals, see chapter 4 of this thesis.

that in the future 'learned men especially' would strive to make a greater effort in communicating their observations.[37] Similarly, relators often stressed that they had gathered their information from 'naturalists', 'gentlemen of probity', and other 'intelligent' and 'creditable' persons.[38] This second charge is most clearly related to the peculiar nature of English earthquakes. First of all, the relative scarcity of earthquakes in England was often stressed. In 'proper observers', such unfamiliarity would instil *curiosity*, an important concept within early modern epistemology.[39] The reverend Roger Pickering, in a letter packed with narrative devices to enhance the credibility of his account, went to lengths to attest that during the earthquake: 'the sensation I felt […] was rather solemn than terrifying; so that I patiently lay to observe the following circumstances.'[40] With other observers, however, it was noted that unfamiliarity bred *surprize* rather than curiosity, which 'imposes upon their judgement'.[41] Secondly, and in partial contradiction of the first point, the fact that earthquakes were not very intense led to the suspicion that small earthquakes were in fact rather frequent, but often went unobserved and unreported. Henry Baker mused that, except for those of 1750, he only received earthquake accounts by 'mere accident'.[42] For instance, sometimes the subject came up in conversation, and someone remarked they had also once felt an earthquake. To Baker the reason that earthquake accounts were not more common was that: 'in country places, people are so little attentive to such matters, that, unless some considerable mischief be done, they mind them very little at the time, and, as soon over, think no more about them.'[43]

Good observations thus depended on the recognition of earthquakes, first of all *as earthquakes*, and secondly as *natural events worth observing*. Credibility and observational skill were thus explicitly linked to theoretical knowledge and perceived intelligence. Within this framework, one important circumstance could redeem the testimony of a non-naturalist: a previous experience with earthquakes. William Fauquier suspected the shakes he felt in 1750 to be an earthquake, because he believed he had once sensed something similar in Italy.[44] As a popular destination for English gentry, Italy was indeed the place where the more affluent earthquake observers had received some prior experience with the

[37]Pigot, *Phil. Trans., Vol. 13, no. 151*, p. 318.
[38] For example: Pedini, *Phil. Trans., Vol. 42, no. 463*, p. 89; Seddon, *Phil. Trans., Vol. 46, no. 497*, p. 695; Dudley, Phil. Trans., Vol. 39, no. 437, p. 63; Smith, *Phil. Trans., Vol. 46, no. 497*, pp. 728-729; Boyfield, *Phil. Trans., Vol. 46, no. 497*, p. 637; Bowman, *Phil. Trans., Vol. 46, no. 497*, p. 684; Bayley, *Phil. Trans., Vol. 39, no. 444*, p. 362; Miles, *Phil. Trans., Vol. 46, no. 497*, p. 629.
[39] Barbara Benedict, *Curiosity: A Cultural History of Early Modern Inquiry* (University of Chicago Press, 2002), pp. 1-23;Cook, *Matters of Exchange*, p. 16.
[40] Pickering, *Phil. Trans., Vol. 46, no. 497*, pp. 622-625.
[41] Pigot, *Phil. Trans., Vol. 13, no. 151*, p. 312.
[42] Baker, *Phil. Trans., Vol. 46, no. 497*, p. 691.
[43] Ibid., p. 691.
[44] Fauquier, *Phil. Trans., Vol. 46, no. 497*, p. 606.

phenomenon.[45] Fauquier's suspicion was only confirmed however when his wife 'declar'd it was an Earthquake, she having felt one in the West-Indies'.[46] Considering the general confusion surrounding this particular event, this testimony should not be read as stating the obvious. It was included precisely because it was an authoritative and decisive evaluation of the situation, one which could establish the event as a legitimate earthquake. In turn, another observer used his experience of the London earthquakes in 1750 to bolster his claim that he had felt another such shock in York in 1754.[47] While describing the 1692 shocks felt in London, John Flamsteed included an observation made by one of his servants, who had witnessed an earthquake six years before in her parental home in Endon, Staffordshire. Her prior experience ensured that she definitively 'knew it to be an earthquake', and heightened her credibility in the eyes of Flamsteed and his readers.[48]

The account given by this maidservant was clearly important to Flamsteed, because he worked hard to keep it in his treatise. In an early draft, the reference to the servant was gendered. It read: 'because *she* had experienced one some years before'.[49] In the final version of the letter, her gender was hidden under a layer of passive voice: 'my servant, having heard the like noise'.[50] It is likely that this covering up was an attempt to make her account more credible in the eyes of potential readers, rather than a stylistic choice. The testimonies of women, and especially lower class women, were considered less valuable than those of men. Other changes between these two drafts moreover indicate that Flamsteed was indeed purposefully editing to make his somewhat dubious theory of 'airquakes' seem more acceptable.[51] Flamsteed also had more general doubts about using the testimonies of those he considered 'vulgar'. In his *Letter Concerning Earthquakes* he commented: 'the nature and causes of things are not to be derived from the expressions of the common people, for they express themselves hastily, and as well as they can, in things they understand not at all'.[52] Despite all these perceived problems of credibility, Flamsteed resolved to retain the account. It is likely that he very much relied on it, because it was one of the few testimonies that could prove that the London earthquake felt like other earthquakes.

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[45] See Frances Willmoth, 'Rumblings in the Air: Understanding Earthquakes in the 1690s', in: *Endeavour, Vol. 31, no. 1* (2007), p. 26. See for instance: Bowman, *Phil. Trans., Vol. 46, no. 497*, p. 684.
[46] Fauquier, *Phil. Trans., Vol. 46, no. 497*, p. 606.
[47] Bakers, *Phil. Trans., Vol. 48*, p. 564.
[48] *Flamsteed's correspondence Vol. 2*, Letter 647: 27 March 1693, To Glen. pp. 478-483.
[49] *Flamsteed's correspondence Vol. 2*, Letter 647: 27 March 1693, To Glen. pp. 478-483.
[50] 'My Servant, who sat alone in the Kitchin, heard it plainly; and feeling the Shocks attending it, knew it to be an Earthquake, having heard the like Noise in an Earthquake that happened at Endon in Staffordshire, and in many Places of Cheshire, six or seven Years before.' Flamsteed, *Letter Concerning Earthquakes*, p. 3.
[51] For instance, a reference to the work of Bartholomeus Keckermann was substituted in the final draft with the remark that Keckermann backed his views 'with the Consent of Aristotle and Pliny'. Flamsteed, *Letter Concerning Earthuakes*, p. 3.
[52] Flamsteed, *Letter Concerning Earthuakes*, pp. 11-12.

Previous experiences with earthquake thus proved authoritative, and were essential in establishing the status of 'earthquake' for subsequent events. This is also borne out by the language used by relators to describe these observations. Whereas many people 'supposed' or 'apprehended' one cause or another, phrases related to unambiguous recognition and knowing were largely reserved for those accounts which based their authority on previous experience.[53] Similarly, the adverb 'immediately' was used to suggest that the knowledge of the cause was instant, and hence clear.[54] If a naturalist could get their hands on an account of someone who had experienced an earthquake before, this account would certainly be highlighted in the text.[55] Of course, most English earthquake observers had either no or very limited experience with earthquakes. However, in general experience as the basis for knowledge was less contested than perceived intelligence, theoretical knowledge of earthquakes or observational skill. In short, in the practice of observing and relating earthquakes, naturalists and observers came to stress some epistemological categories and de-emphasize other in order to produce credibility.

How to talk about an earthquake

Another important element of credibility was the skill of communicating the event in familiar terms. Earthquakes could be easily recognized if they conformed to the expectations of observers and interpreters. After the shocks in London in 1750, Margaret Sloper stated that she 'verily believed it to be an earthquake'. Like the Fauquiers, she drew on her own experience, having lived in Italy for some years. Unlike them, Margaret had never actually experienced an earthquake there. Instead, she argued that the shaking motion of the house had felt 'exactly like what she had often heard *describ'd* when she was in Italy'.[56] Similar to Grace Fauquier's comments, Margaret Sloper's judgement was presented as an authoritative and necessary insurance that the event was an actual earthquake. Her conclusion was based on a descriptive language that was likely far removed from the jargon of earthquake theorists, and hence proved problematic in many ways as a useful basis for philosophical

[53]Parsons, *Phil. Trans., Vol. 46, no. 497*, pp. 634; Winthrop, *Phil. Trans., Vol. 50*, p. 2; Doddrige, *Phil. Trans., Vol. 46, no. 497*, p. 717.

[54] Baker, *Phil. Trans., Vol. 48*, p. 564; Fauquier, *Phil. Trans., Vol. 46, no. 497*, p. 606; Martyn, *Phil. Trans., Vol. 46, no. 497*, p. 610; Folkes, *Phil. Trans., Vol. 46, no. 497*, p. 613; Barlow, *Phil. Trans., Vol. 46, no. 497*, p. 693; Anon., *Phil. Trans., Vol. 49*, p. 439: '*the cause whereof I immediately concluded could be nothing but an earthquake, having experienced one before.'*

[55] Among others: Colman, *Phil. Trans., Vol. 36, no. 409*, p. 125; Seddon, Phil. Trans., Vol. 46, no. 497, p. 696; Huxham, *Phil. Trans., Vol. 50*, p. 428.

[56] Lethieullier, *Phil. Trans., Vol. 46, no. 497*, p. 613. Emphasis mine. Similarly, for Philipp Vanbrugh a strong shock 'immediately brought into his mind the earthquake in Jamaica in 1692. Which several years ago had been, at Jamaica, particularly described to him, by a person who was in the island at the time of the earthquake'. Barlow, *Phil. Trans., Vol. 46, no. 497*, p. 693.

inquiry. Indeed, one of the reasons John Flamsteed was loath to use the testimonies of 'the common people' was that he did not trust the language they used: they expressed themselves hastily and they drew on 'expressions'. Of course, Flamsteed had a particular interest in proving the unreliability of common expressions. In order to prove his theory that the sensation of earthquakes was caused by explosions in the air rather than deep inside the earth, he endeavored to show that whenever people talked about feeling the ground shake, they actually meant to say that their houses were shaking instead. Nonetheless, Flamsteed pointedly observed that many people described earthquakes in more or less the same ways, conforming to existing metaphors and expectations. As he expressed it himself: 'use gives passports to words'.[57] Certain descriptions and metaphors made the event *relatable* in two distinct senses of the word. First, they captured some of the experience of the event in terms that other people could relate to. Second, using recognized descriptions that confirmed to prior expectations made accounts credible, and hence worthy of being related.

So how did lay observers describe earthquakes? Lacking any clear visual evidence, common metaphors referred most often to the sound and motion of the quake. In general, the use of metaphors was more common in describing sounds than in describing motions, which, as we will see in the next chapter, were usually described in different ways. Yet there was still a wide variety of possible metaphorical expressions. One observer wrote that he was rocked back and forth 'like a ship that is under sail', and noted that he was using 'an old expression'.[58] Another compared it to the motion of 'a boat on a river, the current of which is very slow'.[59] Many others thought the motion resembled that of being rocked in a coach or a cradle or, more rarely, described it as being pushed by a strong wind.[60] More sensible earthquakes were commonly referred to as 'a violent heaving' and generated a very different set of metaphors. One such description was the feeling that a large dog had gotten under the bed or chair a person happened to be on and was trying stand up.[61] Similarly, a certain Mr. Hadley of Portsmouth, who was lying in bed with a cold, was momentarily under the apprehension that 'somebody got under his bed out of wantonness and was trying to lift it up'.[62] One of the most common tropes was that of the gentlewoman who was (nearly) shaken

[57] Flamsteed, *Letter Concerning Earthquakes*, p. 11.
[58] Pigot, *Phil. Trans., Vol. 13, no. 151*, p. 314.
[59] Trembley, *Phil. Trans., Vol. 49*, p. 894. This was contrasted to another motion of the ground, which felt more 'like a pulse'.
[60] Folkes, *Phil. Trans., Vol. 46, no. 497*, p. 612; Parsons, *Phil. Trans., Vol. 46, no. 497*, p. 635; Mortimer, *Phil. Trans., Vol. 46, no. 497*, pp. 638-639; Burrow, *Phil. Trans., Vol. 46, no. 497*, p. 656; Baker, *Phil. Trans., Vol. 48*, p. 564; Trembley, *Phil. Trans., Vol. 49*, p. 617; Winthrop, *Phil. Trans., Vol. 50*, p. 12.
[61] For instance. Banks, *Phil. Trans., Vol. 24, no. 289*, pp. 1555-1757; Burrow, *Phil. Trans., Vol. 46, no. 497*, pp. 702-703.
[62] Newcome, *Phil. Trans., Vol. 46, no. 497*, p. 654.

out of her chair whilst reading.[63] More originally, one relator described the shaking of his house as 'a spaniel just come out of water', and a more severe shock as the 'shudder of a horse after swimming, more strong than that of a dog'.[64] He also wrote that it felt as if someone was actively shaking a bed on casters. While individual observers could employ different metaphors, the reports in which these observations were included were entirely predictable, often including at least one reference to someone falling out of their chair, someone describing the shake and the sound, and other related occurrences such as roof tiles falling and china-ware clattering. The recurring combination of these elements made it easy to recognize the text as the description of an earthquake.

The sound earthquakes produced was a more puzzling aspect. People examined the 'evidence of their ears' and had them grow familiar to the sound, but still differed widely in their descriptions.[65] Over time however, a relatively stable set of recurring expressions can be identified. The noise of an earthquake was generally described as a low rumble, growing louder before waning again. Paul Dudley, writing from Boston in 1727, recounted some of the sounds that the earthquake was observed to produce:

> Some of our people took this noise to be thunder; others compared it to the ratling of coaches and carts upon pavements, or frozen ground. One of my neighbours liken'd it to the shooting out of a load of stones from a cart under his window.[66]

The sound of thunder was an unsurprising association. We have seen in the previous chapter that there was a close theoretical connection between the two phenomena. More specifically, many accounts that referred to the sound of thunder noted that it sounded like 'thunder at a distance'.[67] This testifies to the fact that the sound was often perceived as a low rumbling. This was also the general point of other metaphors, such as those of empty carriages over cobblestone streets and the 'murmuring of a hollow wind'.[68] [69] Like with motion, there was a

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[63] Not only gentlewomen, but they are overly represented. See: *Banks, Phil. Trans., Vol. 24, no. 289*, pp. 1555-1558 (mentions five separate instances); Forster, *Phil. Trans., Vol. 45, no. 488*, p. 399; Bowman, Phil. Trans., Vol. 46, no. 497, p. 686; Smith, *Phil. Trans., Vol. 46, no. 497*, p. 730; Baker, *Phil. Trans., Vol. 48*, p. 565; Trembley, *Phil. Trans., Vol. 49*, p. 438.

[64] Bowman, *Phil. Trans., Vol. 46, no. 497*, pp. 685-686.

[65] Pickering, *Phil. Trans., Vol. 46, no. 497*, p. 623; Doddridge, *Phil. Trans., Vol. 46, no. 497*, p. 717.

[66] Dudley, *Phil. Trans., Vol. 39, no. 437*, p. 68.

[67] See for instance: Pigot, *Phil. Trans., Vol. 13, no. 151*, p. 314; Forster, *Phil. Trans., Vol. 45, no. 488*, p. 398; Clare, *Phil. Trans., Vol. 46, no. 497*, p. 620; Miles, *Phil. Trans., Vol. 46, no. 497*, p. 628; Burrat, *Phil. Trans., Vol. 46, no. 497*, pp. 681-682; Seddon, *Phil. Trans., Vol. 46, no. 497*, p. 697; Anon., *Phil. Trans., Vol. 46, no. 497*, p. 722; Smith, *Phil. Trans., Vol. 46, no. 497*, p. 729; Heberden, *Phil. Trans., Vol. 49*, p. 433; Winthrop, *Phil. Trans., Vol. 50*, p. 2.
(which refer to distant thunder specifically). See also: Sloane, *Phil. Trans., Vol. 18, no. 209*, p. 89, 98; Probo Nata, *Phil. Trans., Vol. 22, no. 264*, p. 597; Baker, *Phil. Trans., Vol. 46, no. 497*, p. 617; Boyfield, *Phil. Trans., Vol. 46, no. 497*, p. 637; Pennant, *Phil. Trans., Vol. 46, no. 497*, p. 687; Cave, *Phil. Trans., Vol. 46, no. 497*, p. 706; Anon., *Phil. Trans., Vol. 49*, p. 418; Borlase, *Phil. Trans., Vol. 50*, pp. 500-501. (which do not, but do refer to thunder)

[68] For references to the sound like carriages: Pigot, *Phil. Trans., Vol. 13, no. 151*, p. 312; Banks, *Phil. Trans., Vol. 24, no. 289*, p. 1557; Dudley, *Phil. Trans., Vol. 39, no. 437*, p. 68; Mortimer, *Phil. Trans., Vol. 46, no. 497*, p.

separate set of metaphors reserved for more intense descriptions. These expressions referred to an 'explosion', a sudden 'crack', thunder overhead or the discharge of one or multiple cannon.[70] [71]

In general, describing the sound of an earthquake was rather problematic. One observer noted that the earthquake sounded 'as if a coach or waggon had passed near us over an uneven pavement', but noted at the same time that 'the noise was as loud in the beginning and at the end, as in the middle; which neither the sound of thunder, or of carriages, ever is'.[72] Another observer similarly noted that while he described the noise as the rattling of a coach on the pavement, there was something 'very different in the sound' which made him rethink his earlier judgement.[73] Indeed, Paul Dudley, after summing up the different descriptions he had received from his Boston neighbours, commented that: 'in truth, the noise that accompanies an earthquake seems to be *sonus sui generis*, and there is no describing it.'[74] One observer from Jamaica made an attempt regardless, giving instructions on how to reproduce the sound most accurately:

> it is also accompanied with a Noise, which may be pretty well imitated, by putting the Tongue to the Roof of the Mouth, and in a whispering hollow Tone loudly pronouncing *hur r r r r*, whereby it is easily distinguished from either Wind or Thunder.[75]

Despite the recognition that it was hard to capture the sound of an earthquake in words, the use of a relatively stable set of metaphors proliferated throughout the seventeenth and eighteenth centuries. In order to explain this, it is important to recognize that experiencing an earthquake was to a large degree a social event. The physician and antiquarian James

638; Cooper, *Phil. Trans., Vol. 46, no. 497*, p. 648; Cave, *Phil. Trans., Vol. 46, no. 497*, p. 707; Doddridge, *Phil. Trans., Vol. 46, no. 497*, pp. 712-717; Anon., *Phil. Trans., Vol. 46, no. 497*, p. 722; Goodrich, *Phil. Trans., Vol. 46, no. 497*, p. 726; Smith, *Phil. Trans., Vol. 46, no. 497*, pp. 728-729; Baker, *Phil. Trans., Vol. 48*, p. 564; Sotqueler, *Phil. Trans., Vol. 49*, p. 414; Anon., *Phil. Trans., Vol. 49*, p. 418; Heberden, *Phil. Trans., Vol. 49*, p. 432; Colden, *Phil. Trans., Vol. 49*, p. 443; Borlase, *Phil. Trans., Vol. 50*, p. 500.

[69] For references to the sound like wind: Pigot, *Phil. Trans., Vol. 13, no. 151*, p. 312; Banks, *Phil. Trans., Vol. 24, no. 289*, p. 1557; Baker, *Phil. Trans., Vol. 46, no. 497*, pp. 617-618; Miles, *Phil. Trans., Vol. 46, no. 497*, p. 628; Russel, *Phil. Trans., Vol. 46, no. 497*, p. 632; Burrow, *Phil. Trans., Vol. 46, no. 497*, p. 656; Barlow, *Phil. Trans., Vol. 46, no. 497*, p. 694; Seddon, *Phil. Trans., Vol. 46, no. 497*, p. 697; Anon., *Phil. Trans., Vol. 46, no. 497*, p. 707; Nixon, *Phil. Trans., Vol. 46, no. 497*, p. 711; Anon., *Phil. Trans., Vol. 49*, p. 444; Warren, *Phil. Trans., Vol. 49*, p. 580; Burrow, *Phil. Trans., Vol. 50*, p. 615.

[70] For references to heavy objects falling: Pigot, *Phil. Trans., Vol. 13, no. 151*, p. 312; Banks, *Phil. Trans., Vol. 24, no. 289*, p. 1557; Bayley, *Phil. Trans., Vol. 39, no. 444*, p. 366; Baker, *Phil. Trans., Vol. 46, no. 497*, p. 601; Mortimer, *Phil. Trans., Vol. 46, no. 497*, p. 638; Burrat, *Phil. Trans., Vol. 46, no. 497*, pp. 681-682; Arderon, *Phil. Trans., Vol. 46, no. 497*, p. 699; Doddridge, *Phil. Trans., Vol. 46, no. 497*, p. 715; Green, *Phil. Trans., Vol. 46, no. 497*, p. 723; Goodrich, *Phil. Trans., Vol. 46, no. 497*, p. 726.

[71] For references to explosions and cannon: Lewis, *Phil. Trans., Vol. 38, no. 429*, p. 120; Pedini, *Phil. Trans., Vol. 42, no. 463*, pp. 87-88; Knight, *Phil. Trans., Vol. 46, no. 497*, p. 604; Layard, *Phil. Trans., Vol. 46, no. 497*, p. 621; Boyfield, *Phil. Trans., Vol. 46, no. 497*, p. 637; Anon., *Phil. Trans., Vol. 46, no. 497*, p. 683; Bowman, *Phil. Trans., Vol. 46, no. 497*, p. 685; Arderon, *Phil. Trans., Vol. 46, no. 497*, p. 698; Smith, *Phil. Trans., Vol. 46, no. 497*, p. 729; Sotqueler, *Phil. Trans., Vol. 49*, p. 414; Colden, *Phil. Trans., Vol. 49*, p. 443.

[72] Borlase, *Phil. Trans., Vol. 50*, p. 500.

[73] Goodrich, *Phil. Trans., Vol. 46, no. 497*, p. 726.

[74] Dudley, *Phil. Trans., Vol. 39, no. 437*, p. 68.

[75] Sloane, *Phil. Trans., Vol. 18, no. 209*, p. 98.

Parsons described how, following the shocks in London in March 1750, he went unto the square next to his house, where a large crowd was gathering to discuss what had happened.[76] Henry Baker similarly commented multiple times on the way that people came together to assess the damages and confirm with one another that they had felt the same thing.[77] During such conversations, people discussed previous earthquakes in other places, derived their interpretation from descriptions of these events in public prints or by word of mouth and influenced one another's observations. In 1692 a letter from Jamaica stated that 'the ground heaved and swelled like a rolling swelling sea'. The writer clearly had some doubts about this description, calling it a 'strange comparison'. However, since everyone there was using the expression, he would 'venture to do likewise'. Similarly, he noted that people often compared the colour of the sky at the time of the earthquake to a 'red hot oven', a description which he also used in his relation.[78] William Smith, a registrar in Peterborough, recounted that after an earthquake many people were:

> very much alarmed and ran into the street, where a great number of people instantly appeared; some to see if any coaches or carriages were coming, others to get away, expecting their houses were tumbling; and others finding somewhat extraordinary had happened, but at that instant did not know what, and came to see.[79]

Initially, the people of Peterborough had ran onto the streets with very different ideas about what was happening. By the time of Smith's investigation they all recounted the event as an earthquake. It shows that recognizing an earthquake and finding a common vocabulary to express the experience was a largely collective effort and depended to a large degree on the trust put in the descriptions of other observers. Moreover, because these particular metaphors were so common, an earthquake account that did not mention any of these phrases would be suspect. Hence, even many observers who had not heard any noise, specifically mentioned that they did *not hear a noise like thunder or carriages*.[80] This showcased that the observer was well aware of what they were supposed to have heard, adding to their credibility. Recognizing and recalling an earthquake, in short, were strongly embedded within a social and linguistic context.

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[76] Parsons, *Phil. Trans., Vol. 46, no. 497*, p. 634.
[77] Baker, *Phil. Trans., Vol. 46, no. 497*, pp. 601-603; Baker, *Phil. Trans., Vol. 46, no. 497*, pp. 617-618.
[78] Sloane, *Phil. Trans., Vol. 18, no. 209*, p. 98.
[79] Smith, *Phil. Trans., Vol. 46, no. 497*, p. 730.
[80] See for instance: Anon., *Phil. Trans., Vol. 49*, p. 444: 'I heard no noise like that of a rushing wind, which was heard in many places; but felt a gentle shaking of my bed, in such a manner as convinced me what it was'.

Verifying and explaining variations

We have discussed how earthquake observers emphasized their experience and communicative ability over other markers of scientific credibility which were less quickly accorded to lay observers. What remains to be asked is how these tactics were received, and what strategies naturalists employed to evaluate or reconcile possibly contradicting observations. This was a ubiquitous problem The use of particular metaphors helped people recognize and present the event as an earthquake. Yet to those who kept an overview over all the accounts it remained painfully clear that there was little agreement over what *exactly* was heard and felt. Metaphors used to express more or less intense shocks and noises were used by different observers in relation to the same event. A single individual earthquake was just as easily described as producing a loud explosion as it was said to be accompanied by a 'low rumbling sound'.[81] While the use of metaphors could function to lend credibility to an individual account, taken together they posed a problem. One relator complained for 'one fancied it to be the falling of something about his house; another the tumbling of wood; a third the rattling of a cart, one one thing, and another another'.[82] Some differences in description could be explained by looking at the backgrounds of individual relators. A sailor who referred to the motion as that of a ship at sea and to the sound as that of ship guns was quite obviously drawing from his own experiences to find suitable expressions.[83] Several people who had often witnessed the firing of a cannon used this particular form of authority to give weight to their description, and those who were in a house amongst others were more likely to describe the experience as if someone was walking or dancing overhead than those who were alone.[84]

More problematic was the fact that nearly no earthquake was felt 'universally'. As one observer put it, somewhat exasperated: 'many very odd instance we have of it. Some heard the noise, and felt not the shock; others felt it, and did not hear the noise'.[85] Despite the widespread preconceptions about lay observers outlined above, early modern collectors of earthquake accounts recognized that these variations were not merely the result of lacking observations. When Thomas Nixon noted that he received very different accounts about the intensity of the noise of an earthquake in Sutton, he chose not to fundamentally question the

[81] See for instance: Knight, *Phil. Trans., Vol. 46, no. 497*, pp. 603-604, relating the shock in London in February 1750.
[82] Pigot, *Phil. Trans., Vol. 13, no. 151*, p. 312.
[83] Lewis, *Phil. Trans., Vol. 38, no. 429*, p. 120; Boyfield, *Phil. Trans., Vol. 46, no. 497*, p. 637; Anon., *Phil. Trans., Vol. 46, no. 497*, p. 700.
[84] Bayley, *Phil. Trans., Vol. 39, no. 444*, p. 367.
[85] Smith, *Phil. Trans., Vol. 46, no. 497*, p. 731.

veracity of his correspondents but instead opted to reconcile them 'by supposing, that the explosion might have been heard abroad in such places where it was more violent; and not in others where it was less so'. [86]

Aside from the varying intensity of the earthquake itself, there were other external factors that could interfere with the observation of the earthquake and lead to differing accounts. One circumstance in relation to the noise was, rather unsurprisingly, the volume of other sounds nearby. One Mr. Banks from Hull noted that many people did not notice the earthquake there in 1703 because they were drinking ale in a tavern that was 'pretty full of company that were merry'.[87] Henry Baker remarked that many inhabitants of Norwhich heard an earthquake despite 'the continual noise and hurry there', whereas John Flamsteed noticed that the London earthquake of 1692 was heard especially by those 'in the out streets and alleys of London, remote from the noise and tumults of the greater Streets.'[88] There were clearly better and worse spaces for the observation of earthquake sounds. Hence, many reports also included accounts of those who were 'out in the fields', such as travellers but also milkmaids.[89] Interestingly, these ideal sites for observation were often the spaces of the 'common folk': the back alleys and fields.

The city streets did not only mask the sound of earthquakes, they could also distort it. In 1683 Thomas Pigot, a fellow of the Royal Society who was well known for his work on acoustics, argued that the shape of the buildings on the surface and the caverns underground influenced the way the earthquake sounded.[90] He related that some heard a *murmur* where others described a *rumbling*, and that some perceived the sound to move closer or farther away, whereas he himself was sure the sound remained fixed to one place. The sound was perceived more strongly in open fields, but also heard loudly indoors. According to Pigot the laboratory was also an excellent place for observation, 'for it is certain that all other sounds have a great advantage there', but he remained alone in this conviction.[91] By the mid-eighteenth century it was a commonly attested truth that 'people differently situated judged differently what the sound was'.[92]

Nearly every report also kept close track of which observers felt the shocks and which did not. As a consequence, few reports failed to mention that those who were on the upper floors of buildings perceived the motion more intensely, whereas those on the ground

[86] Nixon, *Phil. Trans., Vol. 46, no. 497*, p. 712.
[87] Banks, *Phil. Trans., Vol. 24, no. 289*, p. 1556.
[88] Flamsteed, *Letter Concerning Earthquakes*, p. 3. See also: Banks, *Phil. Trans., Vol. 24, no. 289*, p. 1555.
[89] Pigot, *Phil. Trans., Vol. 13, no. 151*, p. 315; Bonajutus, *Phil. Trans., Vol. 18, no. 207*, p. 3; Forster, *Phil. Trans., Vol. 45, no. 488*, pp. 399-400; Baker, *Phil. Trans., Vol. 46, no. 497*, p. 618; Layard, *Phil. Trans., Vol. 46, no. 497*, p. 621; Trembley, *Phil. Trans., Vol. 49*, p. 439; Trembley, *Phil. Trans., Vol. 49*, p. 618.
[90] Pigot, *Phil. Trans., Vol. 13, no. 151*, pp. 314-315.
[91] Ibidem.
[92] Smith, *Phil. Trans., Vol. 46, no. 497*, p. 731. See also: Winthrop, *Phil. Trans., Vol. 50*, p. 7.

floor often felt little or nothing.[93] Those who were sitting or lying down were also more likely to experience the lighter shakes than those who were on their feet.[94] This was part of the overarching observation that those who happened to be in motion were less likely to discern the shocks than those who were standing still.[95] Miners working underground also reported that the earthquake was more violent there than on the surface.[96] It was also long known that the shape, height and material of buildings made them more or less prone to being shaken by an earthquake; a fact that was also employed to account for different observations.[97] The knowledge of these general observations led Henry Miles to write with great surprise that his neighbours had felt an earthquake in February 1750. After all, nobody in his own house had, even though by all accounts they should have 'considering the parts they were in, and their being chiefly in a sitting posture', not to mention the fact that his house was 'very susceptible' to be shaken. He therefore concluded that the earthquake could not have been very considerable.[98]

The major problem caused by the use of eye-witness observations, namely the differences in observations, were solved by collecting *more* observations in order to derive general patterns influencing the description of earthquakes. Recognizing such patterns allowed earthquake theorists to explain differences between observers without undermining the credibility of their most important source. This was a crucial step, because it was required to legitimately claim that they were talking about earthquakes at all. It also led to new discoveries or confirmations of theories. As we will explore in more depth in the next chapter, the idea of an (epi)centre of an earthquake seems to be present in the accounts of multiple relators, who noted for instance that 'upon the whole I find, the higher one was, as

[93] For instance: Boyle, Phil. Trans., Vol. 1, no. 11, p. 180; Fauquier, *Phil. Trans., Vol. 46, no. 497*, p. 606; Miles, *Phil. Trans., Vol. 46, no. 497*, p. 607; Martyn, *Phil. Trans., Vol. 46, no. 497*, pp. 609-610; Folkes, *Phil. Trans., Vol. 46, no. 497*, pp. 614-615; Miles, *Phil. Trans., Vol. 46, no. 497*, p. 629; Newcome, *Phil. Trans., Vol. 46, no. 497*, pp. 653-654; Burrow, *Phil. Trans., Vol. 46, no. 497*, p. 655; De Reaumur, *Phil. Trans., Vol. 46, no. 497*, p. 692; Barlow, *Phil. Trans., Vol. 46, no. 497*, pp. 693-695; Doddridge, *Phil. Trans., Vol. 46, no. 497*, p. 714; Smith, *Phil. Trans., Vol. 46, no. 497*, p. 731; Trembley, *Phil. Trans., Vol. 49*, p. 439; Vernede, *Phil. Trans., Vol. 49*, p. 665.

[94] For instance: Miles, *Phil. Trans., Vol. 46, no. 497*, p. 607; Burrow, *Phil. Trans., Vol. 46, no. 497*, p. 655; Bowman, *Phil. Trans., Vol. 46, no. 497*, pp 685-686; De Reaumur, *Phil. Trans., Vol. 46, no. 497*, p. 692; Seddon, *Phil. Trans., Vol. 46, no. 497*, p. 696; Burrow, *Phil. Trans., Vol. 46, no. 497*, p. 703; Doddridge, *Phil. Trans., Vol. 46, no. 497*, pp. 715-716; Smith, *Phil. Trans., Vol. 46, no. 497*, pp. 730-731; Anon., *Phil. Trans., Vol. 49*, p. 423; Trembley, *Phil. Trans., Vol. 49*, p. 439; Warren, *Phil. Trans., Vol. 49*, pp. 580-581; Trembley, *Phil. Trans., Vol. 49*, p. 894.

[95] For instance Miles, *Phil. Trans., Vol. 46, no. 497*, p. 607; Parsons, *Phil. Trans., Vol. 46, no. 497*, p. 634 (in this case, it is mentioned that some who were walking were 'shaken very much'; Bowman, *Phil. Trans., Vol. 46, no. 497*, p. 686; Seddon, *Phil. Trans., Vol. 46, no. 497*, p. 696; Doddridge, *Phil. Trans., Vol. 46, no. 497*, pp. 713-714; Anon., *Phil. Trans., Vol. 49*, p. 423; Trembley, *Phil. Trans., Vol. 49*, p. 439.

[96] Bullock, *Phil. Trans., Vol. 49*, pp. 399-402; Vernede, *Phil. Trans., Vol. 49*, p. 667; Borlase, *Phil. Trans., Vol. 50*, pp. 499-505.

[97] For instance it was noted that after severe earthquakes in the west-indies and the middle-east, houses were rebuild lower in order to be more earthquake-resistant. Others noted that their houses were more or less susceptible to be shaken by winds or earthquakes: Miles, *Phil. Trans., Vol. 46, no. 497*, p. 607; Barlow, *Phil. Trans., Vol. 46, no. 497*, p. 695; D'Ulloa, *Phil. Trans., Vol. 49*, p. 427.

[98] Miles, *Phil. Trans., Vol. 46, no. 497*, pp. 607-609.

farther from the centre, the more the shock was felt'.[99] And while people feeling the earthquake more intensely on the upper floors of buildings was compatible with nearly every earthquake theory from underground cave-ins to electricity and explosions in the air, the observations made by miners strongly suggested that earthquakes had a cause that was internal to the earth, and which was not necessarily the collapse of an internal cavern. The epistemological question whether people really felt an earthquake thus exerted a crucial influence over the kinds of observations that were communicated and incorporated into theories.[100]

Moreover, this use of eye-witness reports *necessitated* the inclusion of accounts from various kinds of people. Servants, who often slept in the garret of the house, were a commonly recurring witness to the idea that the earthquake was felt stronger higher up in the house. Gardeners, milkmaids and journeymen provided crucial testimonies about the conditions of an earthquake in the fields. Rural miners were among the few people able to provide descriptions of the effects of an earthquake underground, and fishermen and ferrymen were more knowledgeable of the effects on the waters. This challenged some epistemological notions about who made credible observations. To note the various sensations of an earthquake throughout a single house (because it was recognized that different houses could produce different effects), the testimonies of all individuals of the household needed to be accredited equal weight. Observing earthquakes, in short, potentially challenged both natural philosophy and the social order. As we will see, the process of determining which observations were credible and useful functioned to incorporate both these challenges in an acceptable framework.

[99] Smith, *Phil. Trans., Vol. 46, no. 497*, p. 731.

[100] These various insight were 'rediscovered' and affirmed in the nineteenth century, and in the early twentieth century Charles Davison remarked that: 'An observer's estimate of the intensity of a shock depends on his position and occupation at the time. If he is outside and walking, the apparent intensity is much less than if he is indoors and at rest. There are, moreover, fewer objects by the movement of which the intensity can be roughly measured Speaking generally, the experience of a person who is out of doors is of little value in the construction of isoseismal lines.'. Davison, 'On Scales of Seismic Intensity', p. 121.

The earthquake testimony of Lady Elizabeth Cornwallis

In the previous sections of this chapter, we encountered the various epistemological and social problems that surrounded the first important question of English earthquake philosophy: how to recognize an earthquake? Starting from preconceptions about the quality of observations made by foreigners, commoners and women, we investigated the experience, language, and methods of verification that were narratively foregrounded to make these accounts credible (and hence, useful). In this final section, we will see in more detail how these elements were employed by observers and naturalists to construct a credible account of an earthquake in Culford, on 30 September 1750.[101] The report as printed in the *Philosophical Transactions* was written by James Burrow, a legal reporter from London, and focused on the account made by a close acquaintance of his: the countess Elizabeth Cornwallis.[102] Not much biographical information is available about Cornwallis, except that she was born Elizabeth Townshend in 1698 as the eldest child of Elizabeth Pelham and Charles Townshend, the 2nd viscount Townshend of Raynham.[103] Through her father, who was a member of the Royal Society, Elizabeth gained connections with members of the Royal Society such as James Burrow and Martin Folkes, and she was generally considered well versed in natural philosophy. In 1722 she married Charles, the 1st Earl Cornwallis. Elizabeth passed away in 1785.

Figure 10. Portrait of Elizabeth Cornwallis by Charles Jervas (1675-1739). Date unknown.

When James Burrow visited the Cornwallis's house in early October 1750, the conversation had apparently drifted to the topic of earthquakes. This was not surprising. The year 1750 would become known colloquially as the 'year of earthquakes' on account of the

[101] Burrow, *Phil. Trans., Vol. 46, no. 497*, p. 702-705.
[102] For a transcription see appendix A, i.
[103] 'Elizabeth Townshend (1698-1785)', entry in the online database 'Ancestry': https://www.ancestry.com/genealogy/records/elizabeth-townshend_10622964 (consulted 24-03-2019).

two tremors that had shaken up England on February 8 and March 8.[104] Smaller shocks had been reported throughout the summer, and a final large tremor had been felt in the North of England just a few days prior, on September 30. This conversation occasioned lady Elizabeth to convey that she too had felt an earthquake on the 30th. Around one o'clock she had been sitting in her dressing room and reading a book when she 'suddenly felt and saw her chair and person move backwards and forwards'. Lady Elizabeth described the feeling in familiar terms to us, recalling that she had actually gotten up to investigate whether the dog had moved under the chair, or whether any person had entered the room unperceived. When she found herself alone in the room, she resumed her previous posture, 'laying her hand or elbow upon the table', and tried to repeat the shaking sensation or 'any thing like it'. This proved impossible however, which was a clear indication for Elizabeth that something outside of her own perception had caused her to move.

Despite her originality in investigating the phenomenon, Lady Elizabeth's account raised some epistemological concerns. First of all, she admitted that she had found herself 'a good deal surprised', and had not made any observations *during* the earthquake itself. Secondly, she in fact had not thought that she had experienced an earthquake at all, until she came down for dinner and her daughter Charlotte asked her "whether she had not felt the Earthquake?" Thirdly, besides Elizabeth, Charlotte, and Charlotte's unnamed maidservant, nobody else in the house had perceived any shock. Lastly, James Burrow noted that while shocks had been felt throughout England during the previous year, no reports had ever come in from Suffolk, which would make this earthquake an especially rare event. In order to rank among the 'careful and exact' reports of earthquakes to which Burrow wanted to contribute, some assurances about the reliability of the report needed to be established.

Some of these assurances were introduced by Burrow. In the opening lines of the letter we are told that he considered lady Cornwallis's 'judgement and accuracy [..] superior to all doubt or exception, and her veracity still more so'. Moreover, Burrow noted that the Cornwallis manor was located about 4 miles from Bury St. Edmund's. To this, he added: 'which, I suppose I need not tell you'. The fact that he did nonetheless tell his correspondent (and by noting this drew extra attention to it) suggests that the phrase served an additional purpose besides adding geographical specificity. Indeed, the sentence reinforced a degree of familiarity which evidently existed between Lady Cornwallis and Martin Folkes, the president of the Royal Society and the addressee of Burrow's letter. These two phrases clearly served to underline Elizabeth Cornwallis's social status as a noble lady and someone with a known history of making natural observations. Yet, Elizabeth's account could still not

[104] William Stukeley for instance referred to the year as such in his lecture to the Royal Society on December 6, 1750: Stukeley, *Philosophy of Earthquakes*, pp. 731-732.

stand on its own. Burrow complemented the report with a letter from a certain Mr. Metcalfe, 'a clergyman of reputation, sense and fortune', who mentioned an earthquake near Leicester, and mentioned reports of an earthquake near Northampton in one of the public newspapers. Only after adding these additional observations did Burrow consider that: 'no doubt can remain of the shock which lady Cornwallis perceived at Culford, having been a real earthquake'.

This instance of verification was gendered. Although there was a general sense that accounts needed to be backed up by other observers, these practices were both more actively pursued and explicitly mentioned when the observations of women were involved. Earlier that year, one Mary Lethieullier had similarly experienced an earthquake while sitting and reading in her family home in Sheen.[105] Running downstairs in a hurry, she did not manage to convince her father and a visitor to the family that she had felt an earthquake, and afterwards 'neither said or thought any more of it'. Only later, when Mr. Lethieullier and the visitor read reports in the newspaper of a nearby earthquake, they 'began to doubt whether the young lady's apprehension was not founded upon somewhat more than mere fancy or imagination'. Lethieullier's account was also drawn up by James Burrow, who was interested in it because it confirmed an earlier relation from a certain Mr. Newcome. Yet Burrow noted that Mary Lethieullier's account 'depends indeed upon the perception of a single person; whereas his is verified by the sensations of six different ones.' Indeed, Burrow indicated that he was not entirely sure whether to believe the account or not. To make up for this lack, Burrow noted that the account nicely supported the idea that earthquakes were more sensible upstairs than downstairs. Moreover, by the end of the letter, Mr. Newcome's relation ends up supporting the observations of Mary Lethieullier, rather than the other way around, as was originally intended: 'Mr. Newcome's account seems to render it probably that she felt a real motion'.

In marked contrast to all this careful verification, one Walter Bowman from East Molesey could boldly proclaim that:

> I have not met with, nor heard of, any person, who felt this second shake which I have described. But, if any memorials are to be preserved of these several shocks, all which I have felt most distinctly, I think this ought not to be forgot; because I do not apprehend it to have been strong enough to have waked any person, nor to alarm even any one wake in Bed. And as for those who were up, and on foot, I do not think they could have perceived it, if I may judge by such a one,

[105] Burrow, *Phil. Trans., Vol. 46, no. 497*, pp. 655-656.

> which I once felt by a single start of my chair, without knowing what it was, till I compar'd notes with my more experience'd neigbours.[106]

Despite no one else having felt anything, Bowman was confident that his account would be considered both credible and valuable. In fact, he tried to bolster his claim of credibility by diminishing the observational skills of his neighbours, who likely had not been attentive enough or were simply incapable of judging correctly what it was, lacking the experience that Bowman obviously had. Although somewhat misplaced, Bowman's confidence appears justified, for his account was printed without problem in the *Philosophical Transactions*. The critical commentary provided by the reverend Stephen Hales, who communicated the account to the Royal Society, went no further than stressing that Bowman was a neighbour 'whose veracity and abilities to make the proper observations, I can depend on', verifying one small detail of the redness of the sky, and noting that the earthquake Bowman had once felt in his chair had likely been in Italy, where he had lived.

Back in Culford, Elizabeth Cornwallis had developed her own arguments in defense of her account and those of Charlotte and her maidservant. She told Burrow that her and Charlotte's rooms were both on the same side of the house, but that that of Charlotte's maidservant 'was in a different part of the house, and distant from either of the ladies apartments'. In short, the motion had not been a localized peculiarity but was felt throughout the house. They had also all felt it at exactly the same time. To explain the fact that nobody besides the three women had felt the earthquake, Lady Cornwallis argued that the three of them had been in a sitting posture, while as far as she could learn everybody else had been standing on their feet. She had also been upstairs during the event, and her daughter had been even one floor higher. These elements provided a very reasonable explanation and at the same time demonstrated her knowledge of the peculiarities of earthquakes. Despite all this, James Burrow noted with some dismay that Lady Cornwallis had neglected to make any inquiry with the inhabitants of Culford. After all, this would have been a clear strategy to confirm her account. Lady Cornwallis' reasons for not doing so once again demonstrate a different method, and an awareness of the need to stress the reliability of her own account rather than those of others: she had declined to make such investigations, she said, 'for fear of alarming them with apprehensions of danger, of which they would be very susceptible from the name of an earthquake; and partly from the little hopes she could have of procuring any tolerably accurate account of the fact from such reporters'. Her first argument explicitly contrasted her own calm handling of the situation with the fright that might be aroused in the minds of the villagers, while the second argument stressed her ability to tell a 'tolerably

[106] Bowman, *Phil. Trans., Vol. 46, no. 497*, p. 686.

accurate account' from one that was not. In a move somewhat resembling Walter Bowman's, Elizabeth Cornwallis carefully separated herself from 'such reporters' in order to bolster her own claims to credibility.

The case of Elizabeth Cornwallis clearly shows how various practices of collecting and verifying were employed to validate the disputed occurrence of an earthquake. First, the earthquake was established as a fact between the three women inside the Cornwallis manor. We know that Elizabeth Cornwallis only considered the possibility of an earthquake after it was mentioned by her daughter, and it seems likely that it was first suggested by Charlotte's maidservant, who was the only person described as 'alarmed' and immediately rushed to check up on her mistress. We do not know exactly how Charlotte and her maidservant convinced Elizabeth that she had experienced an earthquake however. What we also do not know is whether Elizabeth really thought the sensation was caused by a dog under her chair (at the first instant at least). The fact that she told this amusing but seemingly trivial detail to James Burrow, and the fact that Burrow put it in his report to the Royal Society, suggest that, while possibly true, the anecdote served an additional purpose. It invoked a familiar imagery (a lady sitting by the fireside) and a sensory metaphor (the heaving of the chair) to those who were acquainted with other earthquake accounts. Invoking this story served to render the account recognizable as an earthquake report. The investigative and rhetorical strategies employed by both Cornwallis and Burrow show how authority and observational skill were negotiated on various levels. Elizabeth Cornwallis had to convince James Burrow of the veracity of her account. Burrow, in turn, had to defend Cornwallis's observations against others within the Royal Society, and he clearly felt the need to provide additional, perhaps more compelling evidence of the earthquake. Finally, authority was distributed among the various (possible) observers. Cornwallis and Burrow tried to establish Charlotte and her maid as additional reliable observers. The villagers of Culford were discarded as possible observers in an effort to highlight Elizabeth Cornwallis's credibility however. Tactical decisions such as these varied from report to report. They show that the natural philosophical investigation into earthquakes depended on epistemological practices that were constrained by social hierarchies, though not entirely inflexible. These epistemological practices were key in answering the first fundamental question of observer-based earthquake science: how to recognize an earthquake.

Conclusion

Prejudice and practicalities constrained the extent to which earthquake reports were gathered. Most reports used by the Royal Society came from England, were earthquakes were neither common nor intense. In order to convince naturalists that an earthquake had

taken place, and that it had been observed with care, observers made use of a variety of tactics. These included using other accounts to verify observations, using reasonable arguments to explain diverging observations, stressing one's skill, character or experience with earthquakes, and using language that conformed to expectations. The ways in which these tactics were combined often reflected the social relations in which the practices of collecting and interpreting earthquake accounts were embedded. This chapter has focused on the challenges to established epistemologies of observation and social orders that were produced by observer-based earthquake philosophy. Opening the door to more observers did not only result in more available empirical data. In addition, it instigated a rethinking of old and new ideas of what a good observation meant and what practices could be developed to verify the credibility and veracity of these observations. These questions were not answered or set in stone at the beginning of the natural philosophical project, but emerged through the interaction between observers and naturalists. The next chapters will investigate what kind of earthquake philosophy emerged from this social and epistemological framework.

3. Earthquakes on the Move

How were the contributions of earthquake observers turned into Natural Philosophy?

In 1755, the Harvard professor John Winthrop had a stroke of luck. When an earthquake hit Boston on the morning of 18 November (likely an aftershock of the Lisbon earthquake), Winthrop tried to reach for his pocket watch in order to time the event precisely. Sensing the violence of the shock, Winthrop thought better of it, noting that it would be 'difficult, if not impracticable, to go from the bed to the chimney, without being thrown down'.[1] Fortunately, the first tremor had thrown over a glass tube that Winthrop had stored inside his pendulum clock 'for security'. The tube lodged itself against the pendulum, and stopped the clockwork from moving at the precise moment the earthquake started. When the shocks abated, Winthrop moved from his bed to the chimney to pick up his watch and concluded based on the difference between the two clocks that the shocks must have lasted four minutes in total. Despite stressing that he had set both his watch and the clock to the same time just the night before, Winthrop acknowledged that his method was still not entirely exact. Yet it was the best he could hope to achieve. With this information, Winthrop knew he could make a valuable contribution to the study of earthquakes, since 'by comparing it with the like accounts from distant places, we might be able to judge, with a good degree of exactness, of the course of this earthquake, the place of its origin, and the velocity of its progress'.[2]

Like Henry Baker in the introduction to the previous chapter, Winthrop was quick to compare his observations with those of others. Also like Baker, Winthrop encountered epistemological challenges. Some observers had noted a very different timespan ranging from one to seven minutes, and most accounts were 'so very lax, that no just conclusions can be drawn from them'.[3] Winthrop's response to these problems should be hardly surprising by now: carefully sifting the 'uncertain guesses of persons who had no rule to guess by' from

[1] Winthrop, *Phil. Trans., Vol. 50*, pp. 4-5.
[2] Ibid., p. 6.
[3] Ibid., p. 6.

those who had had a watch to aid their judgement, and highlighting the many observations that were in the same ballpark as his own, he established a reasonable margin of error for the timing of the earthquake. Unlike Baker, Winthrop was more explicit about *why getting these circumstances right actually mattered*. Winthrop did not only gather other witnesses to verify that an earthquake had occurred, he also aimed to use their observations to determine in which direction and with what speed the earthquake had moved. This was by no means a novel approach. Since the earliest days of the new earthquake philosophy, the use of earthquake testimonies was largely motivated by their potential, when aggregated, to answer some thorny questions about the extent to which earthquakes could be felt.[4] To establish the fact that an earthquake had occurred, one reliable testimony might (in principle) have been sufficient. In order to determine the extent of the earthquake, multiple accounts from different places were required by necessity.

This chapter considers *what* natural philosophical contributions were made by lay observers, and *how* these were made. In this analysis I identify two approaches towards the use of lay observations: 1) as aggregate data and 2) as individual observations. These two methods did not only generate different natural philosophical insights, they also highlighted different aspects of the developing expert-lay relations. After a brief discussion of the significance of weather reports in earthquake accounts, the following three sections focus on how aggregated earthquake reports were used to determine the *extent*, *time*, and *intensity* of an earthquake. Investigating these matters provided a partial answer to a more fundamental question about the nature of earthquakes: were they static events that simply affected a large area, or did they 'travel' from place to place? Answering this question could in turn provide meaningful clues about either the subterranean or meteorological origin of earthquakes, and was high on the research agenda of contemporary naturalists. Although the scale on which these questions were investigated would prove far too small for later seismologists, the basic categories and methodologies for gathering and synthesizing information were put in place in the period 1665-1755.[5] It was the most direct manifestation of the expert overlooking a growing number of observers 'from above'.

Earthquake reports were not only useful when aggregated however. Observers often noted the different kinds of shocks they felt, and provided additional details about the direction in which they perceived the earthquake to move. Deborah Coen has argued that the invention of observer-based earthquake science was a process in which lay observers needed

[4] See for instance the comments of Hooke on the possible extent of earthquakes: Hooke, pp. 421-431.
[5] Coen, *The Earthquake Observers*, p. 284, ftn. 17. In a footnote, Coen writes that 'Davison remarked that Milne "during his whole career investigated only one earthquake, that of February 22, 1880 in Japan" and "received only 120 replies to his circular, a number far too small for drawing the isoseismic lines of a strong earthquake.' In the seventeenth and early eighteenth century 120 observations would have been considered much. By the twentieth century observations of a single seismic event could number thousands.

to learn how to talk to scientists, while scientists needed to learn how to talk to lay observers.[6] The second half of this chapter looks at how early forms of this conversations took place. For instance, back in 1755 one of Winthrop's neighbors had sensed the upcoming earthquake and decided to stand stock still. He reckoned that he stood like that for at least two minutes before the earth actually began to tremble. Winthrop was skeptical of this assertion, and decided to test the man's account: 'I counted several numbers to him as slowly as a clock beats seconds; and then he said, he believed he could have counted half an hundred, at that rate, before the noise and shake came up to him'. Similarly, Winthrop carefully elucidates how he established the credibility of his neighbor's account of the intensity of the shock:

> The tops of two trees close by him, one of which is 25, the other 30 feet high, he thinks waved at least ten feet (and I depend on his judgment in this particular, because he judged right of the height of the trees, as I found by actual mensuration); and there were two of these great wavings, succeeded by one, which was smaller.[7]

Because other observations (the height of the trees) accurately corresponded to measurable facts, the veracity of the neighbor's statement on the intensity of the quake could be established. The example reflects that once the fact of an earthquake had been established, finding out the specifics of extent, timing and intensity (as well as other miscellaneous circumstances) required more specific methods of interrogation. Many descriptions, like the waving of the trees, needed to be carefully translated into comparable indications of the force of the earthquake. This is a different manifestation of the expert standing 'above' the observer, given the primacy of theoretical over experiential knowledge.

The final two sections of this chapter focus on these methods of translation by looking at the type of knowledge that could be gleaned from individual accounts, and by applying these insights in a case study of some particularly interesting observers deep down in the mines of Cornwall and Derbyshire. These sections not only show how naturalists interrogated and translated the words of earthquake observers, but also stress the agency of observers. They developed new methods and sites of observation, and employed their special knowledge to frame and specify the ways in which their observations could and should be used. Lay observers were the first to use the language of a 'center' and the first to confidently claim that the source of earthquakes lay within the ground itself. In short, they contributed more than mere data to the discussion over the origins and behavior of earthquakes.

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[6] Coen, *The Earthquake Observers*, pp. 82-85.
[7] Winthrop, *Phil. Trans., Vol. 50*, pp. 2-3.

A word about the weather

Seventeenth and eighteenth-century earthquake observers invariably commented on the weather conditions in which earthquakes took place. It is easy to write these observations off as products of soon outdated beliefs about the meteorological origins of earthquakes, and as proof that early earthquake science failed to look at the right things until John Michell's work in 1760 finally burned down the link between the weather and earthquakes and allowed seismology to rise from its ashes.[8] Yet Michell's confident statement 'that these concussions should owe their origin to something in the air, as it has sometimes been imagined, seems very ill to correspond with the phaenomena' was less of a ground-breaking idea than the conclusion of many years of debate and careful observation.[9] Michell himself cited observations from 1693, 1727 and 1728, taken from the *Philosophical Transactions*, to support his argument.[10] And these observations were not just bare information waiting to be interpreted. 'We have heard the rumbles in all weathers […] indifferently', one observer commented on the aftershocks of the 1727 Boston earthquake. Other observers had already drawn similar conclusions long before 1760.[11]

Instead of interpreting weather observations as clear signs of a fundamental belief in the meteorological nature of earthquakes, we should see the gathering of these observations as a sign that the paradigm was being questioned. Moreover, weather records were easy to keep and systematize over long periods of time. Keeping such records was a widespread custom of many upper, and middleclass men in the seventeenth and eighteenth centuries regardless of their interest in earthquakes.[12] The information concerning longer stretches of weather before an earthquake would have been very easy to come by. So if the data was there, why not use it to investigate the theory? After the 1727 earthquake the reverend Paul Dudley had compiled a list of weather records in New England from January that year up until the earthquake in November. John Winthrop compiled a similar list after the 1755 shocks and noted his findings in a column next to Dudley's observations so that 'a comparison may more easily be made between these two years'. The two accounts showed very large

[8] For such interpretations see: Russell McCormach, *Weighing the World. The reverend John Michell of Thornhill* (Springer, 2012), pp. 78-93; Christina Jungnickel & Russell McCormach, *Cavendish: The Experimental Life* (Bucknell University Press, 1999), pp. 182-185.
[9] Michell, *Conjectures*, p. 4.
[10] Michell, *Conjectures*, pp. 4-5.
[11] See for instance: Porter, Phil. Trans., Vol. 49, p. 122 : 'It doth not appear, that there are any fixed or probable prognostics of earthquakes; but that they come on us indiscriminately in the midst of high winds and calms, heat and cold, rain, snow, and fair weather'; Vernede, Phil. Trans., Vol. 49, p. 666.
[12] J. Andrew Mendelsohn, 'The World on a page: making a general observation in the eighteenth century', in: Lorraine Daston & Elizabeth Lunbeck (Eds.), *Histories of Scientific Observations*, (University of Chicago Press, 2011), pp. 396-420; Judith Pollmann, 'Archiving the Present and Chronicling for the Future in Early Modern Europe', in: *Past and Present, Supplement 11* (2016), pp. 246-247.

divergences.[13] Following the ideal of civility in gentlemanly natural philosophy, Winthrop refrained from spelling out the obvious conclusion:

> 'I shall not pretend to make a comparison between the weather of the two fore-mentioned years, nor inquire how far Mr. Dudley's conjecture as to the influence of the weather in producing the earthquake of 1727 might be affected by such a comparison. I choose to leave this to you, sir, and to the other gentlemen of the royal society, who I know are much better able to make a proper judgement in this matter'.[14]

The implication was clear however: the meteorological explanation of earthquakes fell short in the face of empirical observations. Yet the interpretation of such data was more varied than merely rejecting any hypothesis that posited a cause in the skies. By the mid-eighteenth century, different theories linking thunder and earthquakes could explain both the presence of lightning (as a direct cause) as well as its absence (taking earthquakes and lightning as alternative discharges of the same energy) as a sign for earthquakes.[15] On the whole, however, growing doubt over the relation between the weather and earthquakes in the decades preceding 1760 pointed natural philosophers towards the ground itself as a candidate for the cause of earthquakes.

Place

One of the problems that arose from questioning the meteorological paradigm was how to explain the wide extent of some earthquakes. Many seventeenth and eighteenth-century theories on the cause of earthquakes implied that the phenomenon was a highly local one. If an earthquake was felt in multiple places, it could be argued that earthquakes themselves did not move but that a front of bad weather caused earthquakes in a number of different places. On the other hand, those who argued that earthquakes found their cause underground maintained that local deposits of pyrites or other flammable substances determined the extent of the quakes.[16] Robert Boyle's 1682 letter for instance highlighted that the earthquake was strongly felt around a hill, which was 'very well stor'd with Mineral substances of several kinds'.[17]

[13] See: Winthrop, *Phil. Trans., Vol. 50*, pp. 15-17.
[14] Winthrop, *Phil. Trans., Vol. 50*,p. 18.
[15] Stukeley, 'Concerning the Causes of Earthquakes', pp. 663-668; Hales, 'Some Considerations on the Causes of Earthquakes', pp. 674-677.
[16] Christiaan Huygens, *Oeuvres complètes. Tome XIX. Mécanique théorique et physique 1666-1695*, P. 31
[17] Boyle, Phil. Trans., Vol. 1, no. 11, p. 181.

In September 1692 London was hit by a minor earthquake that was also felt in mainland Europe. This was remarkable according to contemporary observers, because the two places were 'separated by a wide and deep sea'.[18] One publication informed its readers that 'the philosophers, wont to give natural explanations for everything, managed to explain it by saying that the cause of this earthquake was very deep, far beneath the deepest bottoms of the sea'.[19] Many philosophers speculated just how far the effects of an earthquake could reach. Robert Hooke for instance wondered 'whether it may be reasonable to conceive, that there could be any communication subterraneous between these places of those eruptions in Naples and Lima; or whether it were superterraneous through the air and aether?'[20] To prove his point about the 'contemporaness' of earthquakes and other disasters around the globe, Hooke compiled long lists of extraordinary natural phenomena throughout Asia, Europe and the Americas in 1672 and 1680.[21] Various earthquakes and floods around the world in 1755-6 were similarly ascribed to the same circumstances that produced the great Lisbon earthquake.[22] For the smaller earthquakes that were observed in England, the extent of their effects was not reckoned to be global. Yet it was obvious that earthquakes were not merely felt in one particular place but extended over a larger geographical area: they remained remarkable events, and news of them spread quickly.

Shortly after the earthquake of September 1750, William Folkes travelled from his home in Newton to Yorkshire. He used the opportunity to stop in several villages along the way and inquired after the recent shocks.[23] Most naturalists, however, did not go out of their way to collect earthquake accounts. They principally relied on written correspondence, newspapers and word of mouth. Servants also played an important role in the dissemination of earthquake accounts. Directly following the February 1750 shocks in London, Gowin Knight recalled that he was visited by a servant of the Duke of Newcastle, sent to report the shaking of the latter's house and inquire whether the Knight family had felt it too.[24] Many other relators wrote how they or others sent out their servants 'on purpose to know'.[25]

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[18] *Europische Mercurius, Vol.3, Pt. 3*, p. 20.

[19] Ibidem: 'daar zich men het meest over moet verwonderen, want gelijk dit Koningryk door een wyde en diepe zee van al het overige van Europa afgescheiden is, schynt het, dat d'Aardbeevingen, welken het gevoelt, byzonder aan het zelve behoorden te weezen; en dat de geenen, die zich elders doen gevoelen, niet daar toe moesten doordringen. De Philosoophen, gewoon van alle dingen natuurlijke redenen te willen geeven, wisten 't goed te maaken met te zeggen, dat d'oorsaak van deze Aardbeeving zeer diep was geweest, en ver beneden de diepste gronden der Zee, welke Groot Brittanje van al het overige van Europa afscheid'.

[20] Hooke, P. 428.

[21] Hooke, pp. 429-431. On pages 309-310 Hooke also notes on an earthquake in 1582 that ''tis not a little observable, that at the same time that these changes happened in America, the like also happened in England'

[22] *Philosophical Transactions, Vol. 49*, pp. 351-444. See also: Koopmans, Joop, 'The 1755 Lisbon Earthquake and Tsunami in Dutch News Sources. The functioning of early modern news dissemination', in: S. Davies, P. Flechter (eds.), *News in Early Modern Europe. Currents and Connections* (Brill, 2014).

[23] Folkes, *Phil. Trans., Vol. 46, no. 497*, pp. 701-702.

[24] Knight, *Phil. Trans., Vol. 46, no. 497*, pp. 603-604.

[25] Parsons, *Phil. Trans., Vol. 46, no. 497*, p. 636 : 'I was also informed by Mr. Sherwood of this Society, that it was not felt at Horn-Church, a Gentleman having sent his Servant on purpose to know'. See also: Folkes, *Phil.*

Sometimes, as was the case with the servant of the Duke of Newcastle, they were given explicit instructions about whom to visit and make inquiries from. Other times it seems that they travelled past all nearby places and collected whatever they could. Despite all this effort, these servants were not credited by name. Yet their example does highlight once more the importance of an economy of trust within the framework of earthquake investigation. These anonymous servants were trusted to accurately recount the observations of others. When inquiring in villages and boroughs, they must also have made their own independent choices about where to ask, and whom to trust. Although no written record of these encounters remain, it is safe to say that the collection of testimonies by servants constituted a major part of the field work necessary to establish the extent of an earthquake.

After an earthquake in Sussex on 25 October 1734 Charles Lennox, the Duke of Richmond, wrote to the Royal Society to relate that while the earthquake was not very perceivable on his estate at Goodwood House, it had been reported in Chichester, Shoreham, Goreing, Tarring, Findon, Arundel Castle, Merston, and Braglesham Bay.[26] While we do not know exactly how these accounts were collected, many of the observers signed 'certificates' which contained their observations.[27] With this information Lennox concluded that the earthquake had been a coastal phenomenon, and travelled east to west. Lacking any further observations, however, we do not know whether the earthquake was felt any further east or west, but we do know that it had not been felt north of the 'Downs', a hill range running through Sussex.[28] Included with Lennox's letter came another account from Northamptonshire, referencing an earthquake that had happened there two weeks earlier, on October 10.[29] Using observations from various villages, the letter confirmed the east-west range of the earthquake, although the geographical spread of the Northampton observations seems far less clear-cut. Most of the observations were made in the villages surrounding the hamlet of Aynho, where the author of the letter worked as a rector.[30] Observations concerning the spread of an earthquake clearly happened at a very local level, but were compared over larger distances.

Trans., Vol. 46, no. 497, pp. 615; Russell, *Phil. Trans., Vol. 46, no. 497*, pp. 631-633. Robert Boyle also instructed his servants to keep his meteorological notes while he was away: Boyle, *Phil. Trans., Vol. 1, no. 11*, pp. 181-185.

[26] Fig. 11.

[27] *R.S.A., LBO/21/43.*

[28] Lennox, *Phil. Trans., Vol. 39, no. 444*, pp. 361-366.

[29] Ibid., p. 367.

[30] Fig. 12.

Figure 11. Observations from the earthquake in Sussex, 1734. Filled dots represent the places where the earthquake was felt [not the quantity of observations]; empty circles the places where it was reported not to be felt. The lines represent comments on the limit of the earthquake.

Figure 12. Observations from the earthquake in Northampton, 1734.

The earthquake which shook London in February 1750 mostly yielded accounts from within the city itself.[31] The lack of earthquake reports from outside the city could either mean that the shock was a highly local one, or that naturalists had simply failed to make use of their networks to gather information. This seems to have been a contemporary concern as well. To dispel confusion about why reports were not coming in from particular areas, and to get a more precise view of the trajectory of the earthquake, it was equally important to note where the earthquake had *not* been perceived. It was known that the February earthquake had not been felt as a far to the northeast as Ingatstone, though it had been felt on the coasts of France.[32] Another observer noted that the shock was not felt in Enfield-Chase, 'nor nearer to there (as I can learn) than Edmonton'.[33] In the other direction, no one west of the Kensington Turnpike had felt the shocks.[34]

[31] Fig. 13.
[32] Trembley, *Phil. Trans., Vol. 46, no. 497*, p. 610; Martyn, *Phil. Trans., Vol. 46, no. 497*, p. 610.
[33] Pickering, *Phil. Trans., Vol. 46, no. 497*, pp. 624.
[34] Martyn, *Phil. Trans., Vol. 46, no. 497*, p. 610.

Figure 13. Observations from the London earthquake, February 1750.

In March, observers noted that the earthquake was not felt in Watford to the west of London, nor any further than Ilford to the east.[35] This earthquake likely extended more towards the south, where reports came in from as far as Epsom. Importantly, the relator of this observation noted that 'how much farther, my information does not say', confirming that he was looking for the 'edge' of the affected area but had not found it yet.[36] William Stukeley, writing one month later, supposed the earthquake to have had a diameter of 30 miles.[37]

Figure 14. Observations from the London earthquake, March 1750.

[35] Parsons, *Phil. Trans., Vol. 46, no. 497*, p. 636.
[36] Miles, *Phil. Trans., Vol. 46, no. 497*, p. 629.
[37] Stukeley, pp. 659-660.

The final earthquake of 1750, in September, mostly affected Northamptonshire and Leicestershire.[38] Several reports confirmed that the quake had not been felt south of Towcester, nor west of Warwick. The many references to 'all the towns in between' make it difficult to establish exactly where the earthquake was perceived and where not, however. One observer extended the earthquake further to the north and south than any of the other observations could confirm:

> It is probable it began in Derbyshire, or some of the counties to the west of that (for I am informed it was felt as much at Derby as here, and at all places between); and passed off the Island thro' Lincolnshire, and part of Cambridgeshire. The breadth from North to South I imagine to be 40 or 50 miles; of which much the greatest part lay north of this place.[39]

Though it was difficult to establish exactly where the earthquake had been felt, all these accounts were detailed enough to arrive at approximately the same conclusion: the earthquake had started in the (north)west and travelled towards the (south)east. Aggregated observations showed patterns that no single observation, no matter the trustworthiness of the observer, could hope to discern.

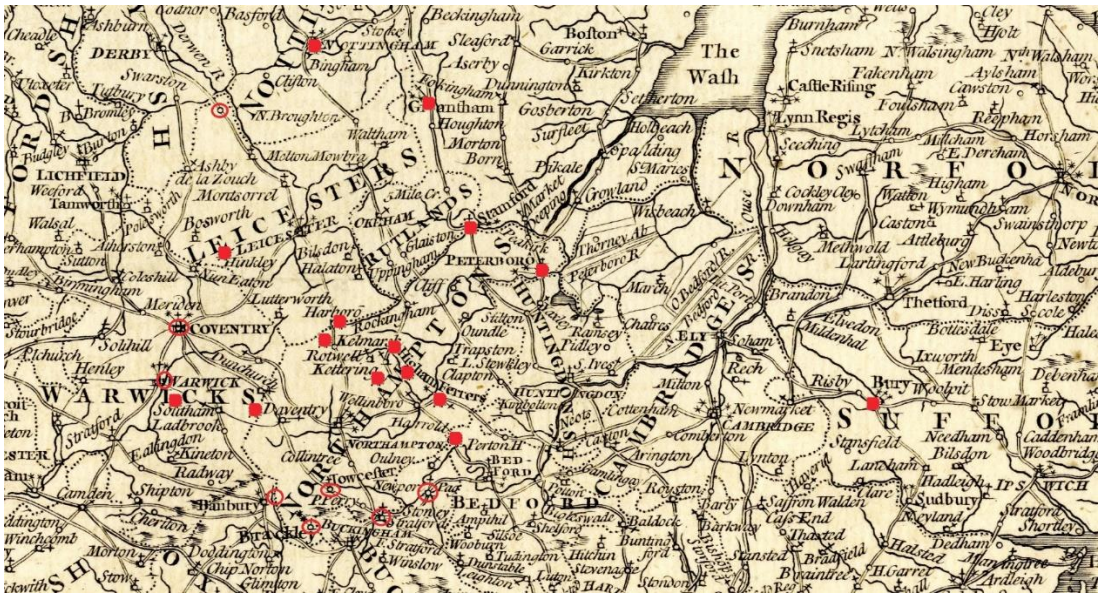

Figure 15. Observations from the Northamptonshire/Lincolnshire earthquake, September 1750.

In 1665, noting the extent of an earthquake was not yet considered an important aspect. Robert Boyle noted that though he had 'been inform'd by others, that this Earth-quake reach'd a good many miles', he had 'neither leasure, nor inclination to entertain you with

[38] Fig. 15.
[39] Anon, *Phil. Trans., Vol. 46, no. 497*, p. 722.

uncertain reports of the extent and other circumstances'.[40] He did relate that the earthquake was felt much more heavily at a house several miles out, but did not use this information to say something about the direction of the earthquake. In fact, this house stood on that hill 'well stor'd with mineral substances', which we encountered earlier. For Boyle, the occurrence and intensity of an earthquake still depended on highly local circumstances, such as the presence of sulphurous minerals, and did not indicate a larger, passing phenomenon. Only with the increase of lay observations in the decades to come did this conception of earthquakes change, so that later theorists like William Stukeley spent several pages recounting and comparing all the various places where the earthquakes had been perceived.[41]

Time

The idea that earthquakes were neither static nor local answered some questions, and raised new ones. One major problem faced by naturalists and observers alike was the way in which earthquakes moved. Some held the effects of an earthquake to manifest themselves instantaneously throughout the entire affected area, whereas others argued that the shocks travelled along a trajectory of sorts.[42] The language used in most accounts indicates that the interpretation of traveling earthquakes rapidly gained currency in the eighteenth century. For instance, descriptions such as a noise like that of an *oncoming* carriage supported the idea that earthquakes had a direction. Yet as late as 1750, William Stukeley remarked that observations from different places usually timed the earthquake at the same instant. This was an observation Stukeley was keen to make, for it agreed with his theory on the electrical nature of earthquakes: it could not be explained 'by any natural power, but that of an electrical vibration; which, we know, acts instantaneously'.[43] In his eagerness to present findings that supported his theory, Stukeley overlooked some of the fundamental difficulties in accurately determining the relative timing, and hence the possible direction, of an earthquake. Seismic shocks travel fast, at approximately 6 to 8 kilometers per second.[44] And while eighteenth century clocks could be very precise, there was no guarantee that two

[40] Boyle, Phil. Trans., Vol. 1, no. 11, p. 181.
[41] Stukeley, *Philosophy of Earthquakes*, pp. 735-736.
[42] The reverend Stephen Hales noted for instance that an earthquake in Smyrna in 1688 was perceived to move eastwards, while one in London moved westwards 'because the first kindling probably began on the western side; and in the earthquake at London on the eastern side'. Hales, p. 678.
[43] Stukeley, *Philosophy of Earthquakes*, p. 738, see also p. 736. It was preceded by some words of caution: 'as far as we can possibly learn, where no one can be prepar'd at different places'.
[44] This refers specifically to P-waves, primary or longitudinal waves. The velocity of p-waves is depended on a myriad of different factors, most principally the material it moves through and their depth. See: William Lee, Hiroo Kanamori, Paul Jennings, Paul Kisslinger, *International Handbook of Earthquake & Engineering Seismology, Part B* (Elsevier, 2003), pp. 1828, 1833.

clocks at several miles distances were set to exactly the same second (or even to the same minute, for that matter).

Mechanical clocks, including rudimentary watches, had been around since the late middle ages, but significant improvements had been made by the end of the seventeenth century.[45] Clocks were often set according to a 'common time', usually the clock of a church's bell tower, to ensure that everyone within the same town or village maintained the same standard of time. Earthquakes could interfere with the fragile mechanisms of early modern clocks, as witnessed by the breaking of Winthrop's clock, and by many other watches seemingly slowing down or stopping altogether.[46] More significantly, many observers did not have clocks or watches to adjust or repair. Hence, many of the observed times were preceded by a disclaimer along the lines of: 'as far as I could guess'. Other indications of the duration were expressed in phrases such as 'the time of an ave maria', 'two pater nosters' or 'as quick as a thought itself'.[47] This uncertainty did not go unnoticed. Already in the 1680s, Thomas Pigot argued that:

> 'The time of the day, at which the earthquake happened, passes for the same in every place, that felt it; all say about seven a clock: but I dare make no inference from hence, that the shaking really was in all places at the same time, unless the time had been exactly observed, to a minute at least, in several places'.[48]

Another observer likewise noted that 'whether progressive or instantaneous in the several places where it was felt is uncertain, for want of accurately determining the precise point of time in distant places', explicitly linking the epistemological point to its theoretical implications.[49] Despite all this, earthquake observers incessantly noted down the time an earthquake had started, as well as its duration.[50] What was the point of getting the time right if it could hardly be compared with other places? In part, they were looking for patterns (do earthquakes happen more towards dawn or dusk?). In part, they were using time as a means to make their account seem more detailed, professional, and hence more reliable. And finally, in many cases observers did somehow conclude that an earthquake had been felt by

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[45] Gerhard Dohrn van Rossum, *History of the Hour. Clocks and Modern Temporal Orders* (University of Chicago Press, 1996), p. 316.
[46] Winthrop, *Phil. Trans., Vol. 50*, p. 5, also mentions that several other clocks in the city had stopped. Phil. Trans., Vol. 38, no. 429, p. 120 mentions that at the house of one Mr. Chew, the earthquake 'stopp'd the Pendulum of his Clock', and Cadwaller Colden mentioned that 'my watch, for some time before it, went unusually slow': Colden, *Phil. Trans., Vol. 49*, p. 443.
[47] Pedini, *Phil. Trans., Vol. 42, no. 463*, p. 80; Trembley, *Phil. Trans., Vol. 49,* p. 617; Warren, *Phil. Trans., Vol. 49*, p. 580; Burrow, *Phil. Trans., Vol. 46, no. 497*, p. 627.
[48] Pigot, *Phil. Trans., Vol. 13, no. 151*, p. 318.
[49] Borlase, Phil. Trans., Vol. 50, p. 500.
[50] When one observer forgot, he sent another letter mentioning : 'I omitted a very material circumstance, which was to acquaint you of the time that it lasted, which was between five and seven minutes.' Wolfall, *Phil. Trans., Vol. 49*, p. 408.

some first, and by others later. After the previously mentioned shocks of 1692, the Dutch philosopher Christiaan noted that the earthquake was reported in Luik *fifteen minutes* before he had sensed it in his home near The Hague. He used this information to ponder 'whether by the distance, over which the succession of quakes is stretched, something could be said about the depth of the holes and the vapors'.[51] Without resorting to measurement, others also mentioned that the earthquake had been felt earlier by some relative to others.[52] This seems highly unlikely due to the speed with which earthquakes travel, and it is more probable that some perceived either fore, or aftershocks at different moments.

Unsurprisingly then, noting down aftershocks was another important reason to keep track of the time of different events.[53] It could yield potential information about the frequency and proximity of shocks, although this was not commented on explicitly. It also ensured that, in the case of multiple shocks, people were talking about the same event. This was of clear natural philosophical importance in its own right, but also served to establish authority and credibility among different observers. Regulating and standardizing time, after all, was a social affair. A personal watch could convey a sense of exactness and credibility, but getting one's story straight about the time of an event meant seeking a general agreement between different observers with access to the same common information. Because of this, and because of the many practical difficulties of time keeping discussed above, one observer made sure to mention both the time on his watch (eighteen minutes to six) and the time according to the sun (half past five), which was plain for all to judge.[54] The timing of an earthquake thus clearly shows the connections between social credibility and theoretical questions.

In his account of the 1727 Boston earthquake, Paul Dudley noted that the newspapers reported the earthquake at 'forty Minutes after Ten o clock at Night', while his own watch told him it had been five minutes earlier. Yet, Dudley noted that 'the clocks of the town [on which the newspapers had based their timing] might be truest.'[55] James Burrow, relating accounts of an earthquake in late 1750 in Suffolk and Leicestershire, referred to a newspaper in Northampton to prove that the earthquake had been felt around one in the afternoon.[56] Newspapers were authoritative as a form of common observation regarding the time and

[51] Christiaan Huygens, *Oeuvres complètes. Tome XIX. Mécanique théorique et physique 1666-1695*, p. 31 : 'An ex spatio quo se extendit haec successio, aliquid de profunditate cavernarum ac vaporum conjici possit?'
[52] For instance, one doctor Holder recounted that the earthquake was 'observed by those in the furthest part of the Garden, some very discernible time before it was observed by those in the house'. Wallis, *Phil. Trans., Vol. 1, no. 10*, p. 169.
[53] It was done by virtually all correspondents, and in great detail. For some especially minute accounts see: Plant, *Phil. Trans., Vol. 42, no. 462*, pp. 33-42, who noted down the aftershocks of the 1727 Boston earthquakes for years, 'precisely at every time I heard it'; Trembley, *Phil. Trans., Vol. 49,* pp. 616-622; Vernede, *Phil. Trans., Vol. 49*, pp. 663-668.
[54] Birch, *Phil. Trans., Vol. 46, no. 497*, p. 615.
[55] Dudley, *Phil. Trans., Vol. 39, no. 437*, p. 70.
[56] Burrow, *Phil Trans. Vol. 46, no.* 497, p. 705.

place of the event, but they were generally suspect of exaggerating their accounts in order to sell better.[57] The reverend Paul Doddrige, writing from Northampton about the same newspaper as Burrow, noted that the earthquake had been 'magnified far beyond the truth' in the papers:

> The earthquake […] happened on Sunday the 30th of September, about 20 minutes after 12 at noon. Our mercury strangely fixed it at a quarter before one; which is so palpable a mistake, and contrary to the certain knowledge of so many hundreds of people, that I could not but be surprised to see it'.[58]

A single diverging observation such as Dudley's watch could not challenge what was known 'in common' and spread in the news. Yet the shared knowledge of an entire town was important enough to defend, and credible enough to defend itself. So while there were clear reasons to note the time of an earthquake, they did little to determine its course. In spite of this, many observers kept talking about earthquakes as if they were travelling phenomena. Different factors still seemed to confirm this hypothesis, even if the most obvious indicator (it being observed earlier in some places than others) proved unreliable.

Intensity

An alternative measure for determining the trajectory of an earthquake was to chart the intensity of the shocks as they were perceived in different places. The scientific idea of an epicenter or hypocenter was not developed until the nineteenth century, but it was clear enough to seventeenth and eighteenth-century observers that earthquakes were felt more intensely in some places than in others. Most observers related this knowledge to the idea of a traveling earthquake. They reckoned that the subterraneous vapors which propelled the earthquake would burn up in the process, and that lacking fuel, the earthquake would slow down and lose intensity. One observer determined for instance that the march 1750 earthquake was felt more sensibly towards the south than in the north, which allowed him to the deduce 'that the force of the vapour was spent before it reached that place'.[59] Some naturalists also suggested that the shakes were the result of shocks that travelled through the very substance of the earth itself. John Michell held that earthquakes resulted from a combination of travelling vapors and shock waves through the earth itself, arguing that

[57] Andrew Pettegree, *The Invention of News. How the World Came to Know About Itself* (Yale University Press, 2014), pp. 251-268.
[58] Doddrige, *Phil Trans. Vol. 46, no. 497*, pp. 712-713.
[59] Cooper, *Phil. Trans., Vol. 46, no. 497*, p. 648.

vapors moved between different strata, pushing the earth back and forth like waves. He explained this idea by making reference to well-known descriptions: 'the compressibility and elasticity of the earth may be collected, in some measure from the vibration of the walls of houses, occasioned by the passing of carriages in the streets next to them.'[60] William Stukeley reckoned that earthquakes travelled along rivers and coastlines. The water in these bodies would conduct the earthquake's electricity and propel it further. Regarding an earthquake which was felt around the parish of Spalding, Stukeley remarked:

> 'particularly of [its] effects being mostly spread to the North and South, and especially felt on the sea coast. we may observe that such is the direction of Spalding river, which both conducts and strengthens the electric vibration; conveying it along the sea-shore, thence up Boston channel, and so up Boston River to Lincoln; as we discern, by casting our eye upon a map'.[61]

Again these observations interacted with a wide field of competing theories. Whether caused by travelling vapors, elastic waves or electrical shocks, various reporters made reference to the idea that earthquakes travelled from place to place, leaving behind a trail of destruction. Yet the limited intensity of English earthquakes overall meant that on this account, too, observations were not always particularly insightful.[62] One naturalist noted for instance that:

> 'most of the accounts concur in this particular, that the chairs, wainscot, doors, chests of drawers and other moveables, were heard rattling; and one, that a Bell rung of itself just before they felt the heaving of their Beds […][63]

Descriptions like these remain in use even today to establish some indication of the severity of an earthquake.[64] They were clearly useful: it is easy to distinguish this harmless shock from an actually damaging earthquake event. Yet in order to determine the path of an earthquake through the countryside, more was needed: the observations quoted in the account above were all made in various villages in Sussex, yet they all observed the earthquake in the same degree (they all 'concur in this particular'). Between these places, there was no way of telling where the earthquake had hit harder, and hence where it had originated. With other earthquakes, accounts did specifically mention that some places were more affected than others however. It was only because the intensity of earthquakes was carefully noted in general, that some useful observations could be retained.

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[60] Michell, p. 598. See also p. 599.
[61] Stukeley, p. 743.
[62] Coen, *The Earthquake Observers*, pp. 82-87.
[63] Bayley, *Phil. Trans., Vol. 39, no. 444*, p. 366.
[64] See for instance the Mercalli scale. Similarly, the Richter scale was originally developed as an aid to making eye-witness testimonies more reliable, instead of a truly 'objective' measurement for intensity. Coen, *The Earthquake Observers*, pp. 272-275, 263-266.

Sometimes general categories could be distinguished in the language used to describe the shocks. In the lowest category, we find remarks that few if any people were able to feel the earth shake. One step up, we find observers talking about the rattling of glass and porcelain. Seeing and/or feeling the furniture move, and sensing doors slamming shut or going ajar was another general category. Next came the falling of roof tiles and bricks (particularly from chimneys), in order words: semi-permanent damage and considerable risk for those outside. Even heavier concussions were observed to cause cracks in walls, or even the partial collapse of some structures. Here we are already far away from English standards. After this category, the intensity of the shocks was chiefly measured in the amount and type of structures that were overthrown.

These categories were not necessarily mutually exclusive. Shocks that were causing chimneys to fall down also created a rattling sound arising from glasses, cutlery and china-ware. Because observers were generally keen to note down the strongest effect of the shock, these categories provided rough, though relatively reliable ways to gauge the intensity of an earthquake. One observer for instance remarked that his watering pot had been overturned by an earthquake. In an addendum to the report, he remarked that he had since learned that the watering pot had not been overturned directly, but had been hit by a falling brick. This implied a heavier shake than was initially assumed, and reveals the potentially useful information behind the rather awkward anecdote.[65]

The particular categories of intensity varied from report to report. One observer writing from Cornwall, the reverend William Borlase, used the impressions the earthquake left behind on its observers rather than its material damages to classify the intensity of the shocks.[66] On St. Mary, the largest of the Scilly islands, the shocks were described as 'violent'. In the parish of Shenan, the houses were perceived to shake, 'and the brass pans and pewter rattled one against another *in several houses*'. In the parish of St. Just there was a 'strong unusual agitation of the sea'. Several masons at Trevailer [Trewellard] perceived both the scaffolding and the wall they were building to move, and one observer likened the shocks to those he had felt in London in 1750. Somewhat further to the east, the shocks were invariably described with more intense language. In Marazion, people 'ran out into the streets, lest the houses should fall upon them', and in St. Ives the shock was described as 'very violent'. One observer even went as far as to say that the only heavier earthquake he had ever felt was the one which had destroyed Lisbon in 1755.[67] In Penzance, the shocks were 'every-where perceived more or less, according as people's attention was engaged'.

[65] Clare, *Phil. Trans., Vol. 46, no. 497*, p. 620.
[66] Borlase, *Phil. Trans., Vol. 50*, pp. 499-505. Fig. 15.
[67] Though of course, we do not know if this anonymous observer ever felt more earthquakes at all.

They were strong enough to not only *rattle* the pewter dishes, but also to slide them from one end of a shelf to the other.

Going even further east, the intensity seems to have decreased again. In Tehidy, the rooms and ground were still observed to move, and from Redruth to Camelford the earthquake was 'sensibly felt'. It was perceived as far as Lostwythel, but 10 miles east of that, at Liskerd, 'it was but faintly perceived, and that by a few persons'. In Loo and Plymouth the earthquake was 'scarcely sufficient to excite curiosity or fear'. William Borlase clearly recognized the west-east gradient of the earthquake. At the time of the shocks, he was visiting a friend on his estate east of Marazion. Rather than writing from his own perpective and starting the account with observations from Merazion, Borlase summed up the earthquake observations from west to east, with the intention to 'trace [the earthquake] according to the best informations I could procure'. Although he drew no explicit conclusions from this, he managed to show that the earthquake had a clear trajectory on the basis that some places were more affected than others.

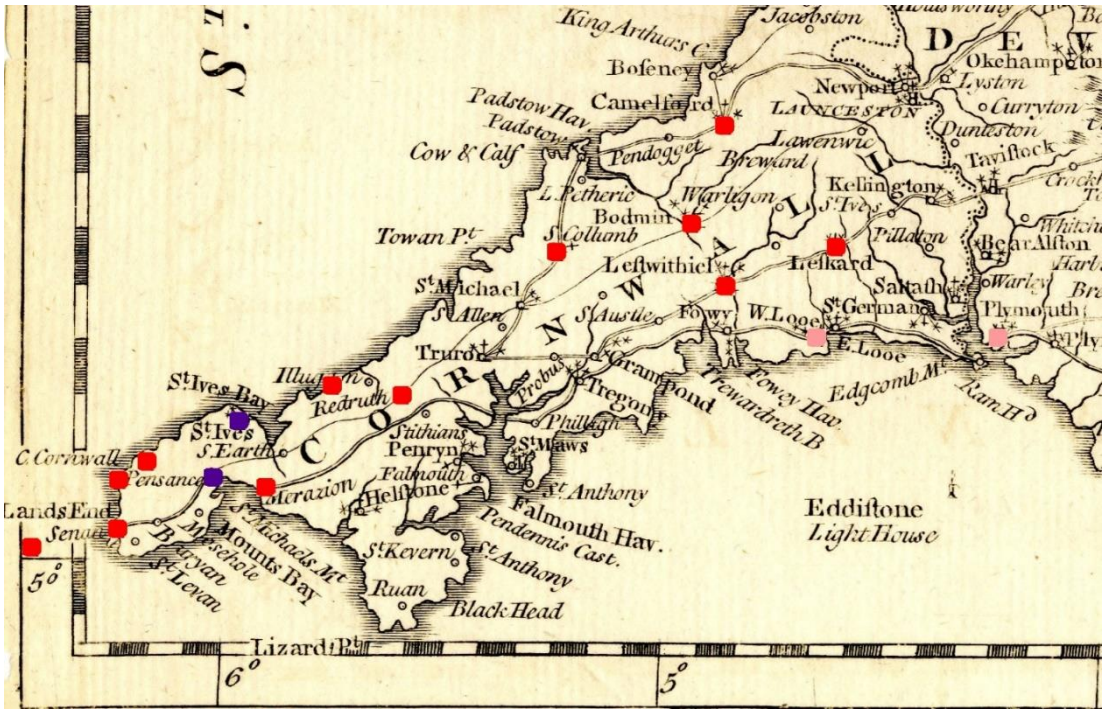

Figure 16. Observations from the Cornwall earthquake, 1757. The color gradient represents the intensity of the reported shocks.

The idea of a trajectory also implied an origin. William Borlase reckoned the earthquake to originate off the coast of Cornwall, moving east to north-eastwards over the land.[68] This idea of a origin was not akin to the modern understanding of an epicenter; eighteenth-century

[68] Contemporary geologists reckon that the most likely location for the epicenter of the earthquake was east of Penzance, which places it between the three villages where it was felt most heavily: Penzance, Marazion and St. Ives.

naturalist did not picture waves emanating in concentric circles from the earthquake's center. However, both concepts involve a point where the earthquake is felt strongest. And sometimes observations did hint towards the earthquake having a clear center. After the 1692 Jamaica earthquake, observers agreed that the earthquake had originated from the central mountains.[69] During the 1727 Boston earthquake, the center of the event was reckoned to be in the town of Newbury, precisely because the reports coming from there were considerably more severe.[70] After the Lisbon earthquake of 1755, observers were unsure whether to locate the center within Lisbon itself (because of the high damage) or off the coast, since it produced a tidal wave and was also heavily felt on the island of Madeira as well as along the Atlantic coast of Morocco.[71] One observer of the September 1750 shocks, noted that the earthquake 'reached 30 or 40 miles from us each way; and I fansy we were not far from the center of it'.[72]

The natural philosophical discussion on the instantaneousness of earthquakes, and consequently over their origin or center, was characterized chiefly by the use of eye-witness reports. Finding out the extent, time, and intensity meant comparing many different observations, and mapping them out. Although no 'earthquake maps' remain, the textual reports and reflections in more theoretical pieces definitely indicate that naturalists compiled observations and used them as aggregated data.[73] These developments had clear implications for the developing theories on earthquakes: the increasing use of observations promoted a conception of traveling earthquakes that could be measured by their intensity, and were hypothesized to have some form of center or origin. At the same time these developments also held implications for the development of lay-expert relations. The special position of naturalists in the network of knowledge-producers began to manifest itself as an hierarchical one: standing above the various observations they took a 'bird's eye view' over all the information. This position was not dictated primarily by any difference in skill or knowledge, but resulted from the availability of collection and correspondence networks.

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[69] 'But in the Mountains are said to be the most violent Shakes of all; and 'tis a generally receiv'd Opinion, that the hearer to the Mountains, the greater the Shake', Sloane, *Phil. Trans., Vol. 18, no. 209*, p. 94.

[70] 'The Town of Newbury, at the Mouth of Merrimack River, about forty Miles from Boston, is the Place that seems to have been the Center of the Shock and Shakes felt by us', Colman, *Phil. Trans., Vol. 36, no. 409*, p. 125.

[71] 'I am almost positive, that the earthquake was progressive, and that this place [Lisbon] was the center of it', Sacchetti, *Phil. Trans., Vol. 49*, p. 410. Other accounts placed the source in the ocean: 'It is possible that the cause of all these misfortunes came from under the western ocean, for I have just been in conversation with a captain of a ship, who seems a very sensible man, who tells me, that he was fifty leagues off at sea; that the shock was there so violent as greatly to injure the deck of his ship.' Wolfall, *Phil. Trans., Vol. 49*, p. 407. See also: Heberden, *Phil. Trans., Vol. 49*, pp. 432-434; Chambers, *Phil. Trans., Vol. 49*, pp. 435-436 and Fowke, *Phil. Trans., Vol. 49*, pp. 428-432 for accounts of the extent in Madeira and the Moroccan coast.

[72] Green, *Phil. Trans., Vol. 46, no. 497*, p. 724.

[73] Recall for instance Stukeley's remark on the progress of earthquakes, which could be discerned 'by casting our eye upon a map'. Stukeley, p. 743. The use of visual representations in earthquake studies was a development of the late eighteenth century, see: Keller, 'Sections and Views', pp. 129-141.

‘A very sensible Scotchman’: using particular accounts

The accumulation of earthquake reports provided insights that could not be gleaned from individual observations. In spite of this, retrieving information about the trajectory of an earthquake remained a difficult task. Other causes could explain why an earthquake was more or less intensely felt by different observers, as we saw in the previous chapter. Nor did aggregated reports say much about the types of shocks that were perceived, or other phenomena that could help to classify and understand the earthquake event. Individual reports still provided useful repositories of such knowledge, however. Many observers for instance commented on the direction of the shocks they perceived. Others classified them into different kinds. Some of these descriptions were elicited by naturalists asking further questions, others seems to have arisen from the initiative of observers themselves.

At first glance, such observations could settle the score on the direction of earthquakes. Yet this proved more difficult than expected. Although the shocks of march 8 1750 were most often described as moving from the northwest and southeast and rocking houses and people in both directions, it was also described as moving northeast-southwest and back, east to west, west to east, and south to north.[74] One observer noted more cautiously that, due to his great surprise, he was entirely unable to tell what the direction of the shakes was.[75] The September earthquake was generally observed to move from west to east, ‘as was very easily discerned by every body that was out of door’.[76] Yet another observer could ‘plainly distinguish’ it’s course as Northeast to Southwest.[77] Another observer agreed, noting that a lady who was facing Southwest at the time, was thrown forward to the tip of her toes, and was in danger of falling forwards.[78] The account also noted that the eastern parts of Northampton were more affected than those in the west, indicating that the earthquake was losing force as it moved westwards. Yet other observations within the same account complicated this view. The noise that accompanied the earthquake was perceived to travel from the southwest to the northeast for instance, and ‘the tremulation of the ground extended itself at least 60 miles in Length from South to North, and from west to east about 5, or at most 30’, seemingly indicating a more northwards passage.[79]

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[74] For the first: Folkes, *Phil. Trans., Vol. 46, no. 497*, pp. 613-615; Mortimer, *Phil. Trans., Vol. 46, no. 497*, p. 638; Anon, *Phil. Trans., Vol. 46, no. 497*, pp. 647. For the others, see: Layard, *Phil. Trans., Vol. 46, no. 497*, pp. 621; Pickering, *Phil. Trans., Vol. 46, no. 497*, p. 623; Burrow, *Phil. Trans., Vol. 46, no. 497*, p. 626; Miles, *Phil. Trans., Vol. 46, no. 497*, p. 629; Russell, Phil. Trans., Vol. 46, no. 497, p. 632.
[75] Parsons, *Phil. Trans., Vol. 46, no. 497*, p. 633.
[76] Anon, *Phil. Trans., Vol. 46, no. 497*, p. 722.
[77] Smith, *Phil. Trans., Vol. 46, no. 497*, p. 729.
[78] Doddrige, *Phil. Trans., Vol. 46, no. 497*, p. 713.
[79] Doddrige, *Phil. Trans., Vol. 46, no. 497*, p. 715.

The discrepancies were noted by theorists. Stephen Hales, in his *Consideration on the Causes of Earthquakes*, recalled that the French naturalist Comte de Buffon had written 'that the vibrations of the earth, in earthquakes, have commonly been from North to South'. Yet the London shocks, according to Hales, were perceived to move from east to west, and another earthquake in Smyrna, treated by de Buffon himself, had moved from west to east. Hales reconciled the remarks as follows:

> tho' the progress of the earthquake as Smyrna was from west to east, yet the vibrations of the earth might be from north to south; and thereby occasion the falling of the castle walls, which run from east to west, but not those which run from north to south. A probable argument, that, as the freest passage, so the greatest explosions were made in the clefts of the earth which runs east and west; which would make the vibrations north and south.[80]

If the earthquake moved from east to west, its vibrations would be sensed as north-south. This was one way of reconciling the many different observations on the way in which earthquakes moved. Yet it also meant that any observation of the motion could be made to fit any theory of the earthquake's passage. Hence, William Stukeley, in his paper on the causes of earthquakes simply followed the majority of observation and, though carefully, acknowledged a west-east earthquake to be possible.[81]

Particular accounts could also reveal the different types of shocks that observers experienced. First, there was a broad distinction between shocks that were observed to move horizontally and those that moved vertically. The vocabulary used to describe these shocks varied widely. Sharp and sudden blows were referred to as *shocks* or *concussions*, or otherwise as a *crash* or *shove.* If the sensation lasted longer, people resorted to descriptions that included a *trembling*, *tremor, quake*, *pulse* or *vibration*. More vertically inclined sensations were described as a *wave* or *undulation*. These kinds of descriptions were often compared, and put in contrast to one another. One observer described an earthquake 'Not at all like a quaking or tremulous motion, but like a forcible shoving backwards and forewards' , while another stated that 'the first thing perceived was a shock, like the sudden stop of a body in motion, a kind of jarring. This was succeeded immediately by a gentle motion, nearly in the direction between east and west; which made 3 or 4 slow and deliberate vibrations'.[82] While terms like *undulation, pulse* and *vibration* might come across as rather specialist language, they were in fact introduced into the seismological vocabulary through

[80] Hales, pp. 678-679.
[81] Stukeley, *Philosophy of Earthquakes*, p. 737.
[82] Mortimer, *Phil. Trans., Vol. 46, no. 497*, p. 638; Taylor, *Phil. Trans., Vol. 46, no. 497*, p. 650.

observer's reports and by the efforts of naturalists to establish a common language of communication with their observers.[83]

The making of such observations had an interesting history. As noted before, the minor earthquakes that manifested in England were principally noticed by people sitting or lying down. For these observers it was often easy to recollect what position one had been in when the earthquake began, and hence to recall what direction the motion went. Beds especially, because of their relatively fixed position within a room, proved to be oddly ideal sites for earthquake observations. This went combined with the fact that many shocks occurred in the morning, when many people were still in bed. Hence, it is not strange to find both many references to beds as well as detailed descriptions of the positions and orientation of these beds in many earthquake reports.[84] The first mention of this practice comes from an observer in the 1680s, who mentioned that he 'found by a great many enquirys that [those who felt the shocks] had various impressions according to the position of their beds to a wall on this or that side'.[85] In 1734, a certain Dr. Bayley from Havant explicitly used the descriptions of beds to derive the motion of the earthquake, and his correspondent at the Royal Society found that his method of noting 'the different Motions of the beds, according to the different situations they were in, is very well worth observing.[86] In 1750, a 'sensible young man' told the reverend Henry Miles that 'considering the situation his bed was placed in, and the motion he had felt from one side to another, he concluded the shock proceeded from the west'.[87] The young man had also talked with others who had, independently, made similar observations from the comfort their beds and taken care to determine the position and direction of their bed.[88] Mr Bird, the 'eminent Mathematical instrument maker' well known to most Royal Society members, also perceived his bed to move, and was sure it had also been lifted from the ground.[89] The practice of bed observations thus answered to the various questions about type and direction. It is worth observing that this practice originated with lay

[83] Coen, *The Earthquake Observers*, pp. 82-85.
[84] Consider for instance Pickering, Phil. Trans., Vol. 46, no. 497, pp. 622-623: 'The attention this occasion'd led me very sensibly to perceive myself rais'd in my bed (which stood N. and S.) and to observe, that the motion, as I lay upon my Back, began on my right Side, and from Head to Foot inclined me towards the left'. For more examples see for instance: Knight, *Phil. Trans., Vol. 46, no. 497*, p. 604; Folkes, *Phil. Trans., Vol. 46, no. 497*, pp. 613-614; Baker, *Phil. Trans., Vol. 46, no. 497*, p. 617; Miles, *Phil. Trans., Vol. 46, no. 497*, p. 629; Parsons, *Phil. Trans., Vol. 46, no. 497*, p. 633; Folkes, *Phil. Trans., Vol. 46, no. 497*, p. 682; Bowman, *Phil. Trans., Vol. 46, no. 497*, pp. 685-686; Pennant, *Phil. Trans., Vol. 46, no. 497*, p. 687; Barlow, Phil. Trans., Vol. 46, no. 497, pp. 642-695; *Colden, Phil. Trans., Vol. 49*, p. 443; Allemond, *Phil. Trans., Vol. 49*, p. 545; Warren, *Phil. Trans., Vol. 49*, pp. 580-581; Burrow, *Phil. Trans., Vol. 50*, p. 616.
[85] Pigot, *Phil. Trans., Vol. 13, no. 151*, p. 321.
[86] Lennox, *Phil. Trans., Vol. 39, no. 444*, pp. 362-363. Bayley's own bed shook from side to side, but that of a 'learned and ingenious gentleman in this town' behaved like the tolling of a Vessel when it crosses over a Wave, the Head and Feet thereof riling and falling alternately several times.'
[87] Miles, *Phil. Trans., Vol. 46, no. 497*, pp. 629.
[88] Contradictions could still arise from these different accounts. For instance, James Parsons 'inquired particularly of such as were thus rocked, about the situation of their beds, and observed, that, tho' all described the motion to be from side to side, their beds were in all directions.' Parsons, Phil. Trans., Vol. 46, no. 497, p. 635.
[89] Burrat, *Phil. Trans., Vol. 46, no. 497*, pp. 682-683

observers themselves, had been noticed by naturalists in the decades preceding the shocks of 1750, and were now used to great extent.

Lay observers did not always take such initiative in making observations. In such circumstances, naturalists needed to make more specific inquiries, and often held longer conversations with observers to get to the bottom of things. On march 9, 1750, just one day after the second earthquake of the year had hit London, James Burrow interrogated Robert Shaw, a 'very sensible Scotchman' working as a gardener in the Inner Temple Gardens.[90] Unlike many other earthquake reports, the details of this conversation between the semi-expert naturalist and the lay observer are relatively well preserved. The account is drawn up in Burrow's hand, yet he took care not to paraphrase too much and focused on the ways in which Shaw expressed himself. As he immediately wrote down the content of the conversation as soon as it was over, he argued that it 'contains a tolerably exact account of what he related to me'.[91]

On the morning of the earthquake Robert Shaw had just arrived at the Temple Gardens when he heard a noise, 'louder, he thought, than ANY noise he ever heard'. Shaw described the noise as coming from behind the buildings he was facing, and later described it as running from the [Thames] waterside towards Temple-Bar. From this information it could be concluded that Shaw was facing south. This was important, because when asked 'what perception he had of his own *personal motion*, and that of the particular *ground whereon he then stood*', Shaw was not able to give an answer. He did remark that he 'saw the whole building *move upwards*, then *incline forwards* towards him (so that he thought it would fall upon him); then *recline backwards*, and then *settle*.'[92] James Burrow was interested in the force of the shock, and inquired after the declination of the building. When Shaw mentioned that he did not dare answer that question with any certainty, Burrow resorted to alternative means:

> In order to form some notion of it, I made a comparative inclination of the garden key, which I held perpendicular to my hand, and moved backwards and forewards, till he should judge it to approach nearest to what he could recollect of the heeling of the buildings (which indeed was but an inaccurate method of coming at the truth of a thing professedly uncertain even to the relator).[93]

The declination was several degrees. This was credible, according to Burrow, because it was 'agreeable to [Shaw's] apprehension that the building would fall upon him'. Although the

[90] Burrow, Phil. Trans., Vol. 46, no. 497, pp. 626-628.
[91] Ibid., p. 628.
[92] Ibid., p. 626. Emphasis in the original.
[93] Ibid., p. 627.

account was by no means perfect, Burrow was keen to use it to gauge both the direction (northwards) and the intensity (enough to cause a declination of several degrees) of the earthquake.

Shaw's account was guided to answer one specific question. Yet it was also mentioned explicitly in one of William Stukeley's theoretical papers on earthquakes, this time in order to prove that the sound of an earthquake precedes the motion, something which supposedly indicated its electrical nature.[94] Even though this individuated mode of using observations for natural philosophy was more conversational than the use of aggregated observations, the interpretational worth of the observation was clearly not determined in the process of communicating observations, but by the theorists who employed them. This process of translation also re-enforced social hierarchies. Another observer, who was also 'remarkably curious for a man in his sphere of life', similarly recounted his experience of an earthquake. Despite this admission, relator of the account used the translation of his words to imply that the two of them did not share the same way of thinking or speaking, by inserting such phrases as 'if I understand him correctly', much like Burrow did with the account of Shaw.[95] The raw observations of witnesses were clearly no natural philosophy yet: the making of knowledge was becoming defined as the work of the theoretical knowledge of the expert naturalist.

'This very intelligent captain of the mine': natural philosophy from below

In the previous sections of this chapter, it has been argued that lay observers played an important role in deriving the extent, force, and direction of earthquakes. These observations were analyzed on an aggregated level, but also focused on the specific sensations earthquakes caused with individuals. Ultimately, the point of determining these matters was to provide proof about the cause of earthquakes. Did they move primarily past rivers and shorelines because of their electrical nature, as William Stukeley would have it? Or did they move along caverns and passages filled with flammable vapors? Although the omnipresence of both caverns and bodies of water throughout England meant that no hard conclusions could be drawn (or, rather, that everyone could find some evidence in favor of their preferred conclusion), the use of lay observations did generate new ways of thinking about earthquakes, for instance by having to incorporate remarks by lay observers concerning some

[94] Stukeley, 'Concerning the Causes of Earthquakes', p. 666.
[95] Doddridge, *Phil. Trans., Vol. 46, no. 497*, pp. 718-719.

perceived 'center' of the shocks and by adopting a vocabulary about the different types of shakes.

In this battle over the question where the source of earthquakes lay, one particular group of observers gained prominence towards the end of our period. These were the miners who felt the earthquakes *below* rather than aboveground and were thus in a special position to comment on the super, or subterraneous nature of the phenomenon. Miners' testimonies had played a noticeable role in the earthquake philosophy practiced by members of the Royal Society since its earliest days. Most of these references to the 'experience of miners' were largely hypothetical, however. Martin Lister supported his theory of pyrites as the cause of earthquakes by observing that 'these subterraneous cavities are at certain times and in certain seasons full of inflamable vapours; the damps in our mines sufficiently witness' and 'that damps naturally fire of themselves, we have the general testimony of miners.'[96] Several decades later, William Stukeley employed similarly vague references to argue that 'we never hear, from the many hundreds of thousands of workmen in this kind, of the cavernous state of the earth', and that 'the workmen in coal-mines, and the like, never fail to meet with the veins of springs everywhere […] a circumstance not very favourable to subterraneous fires.'[97] Rather than examining their accounts of specific earthquakes in detail, these writers invoked the experience of miners as a form of common knowledge about the inner structure of the earth. As laborers, miners were often regarded unfit for particular scientific observation. Only their general knowledge of a mine was reliable.

Nonetheless, the testimonies of miners were increasingly recognized as valuable towards the middle of the eighteenth century. One correspondent to the Royal Society remarked that following a small earthquake his father in law had immediately expressed curiosity about the situation in the nearby mines at Houille [Hoei, in present-day Belgium].[98] After the 1757 earthquake in Cornwall William Borlase, whose extensive cataloguing of the damages we discussed earlier, also made enquiry at the many tin, and copper mines in the area.[99] The most particular account came from Huel-rith mine, a tin mine on the slopes of Trink hill, south of the heavily affected parish of St. Ives, and likely near the epicenter of the earthquake.[100] Here, Borlase interviewed the overseer of the mine, a certain Mr. J Nantcarrow. The overseer recounted how he had felt 'the earth move under me with a prodigious swift, and apparently horizontal tremor: its continuance was but for a few seconds of time'. The noise was 'not like thunder, but rather a dull rumbling even sound, like deads

[96] Lister, pp. 513, 516.
[97] Stukeley, pp. 658-660.
[98] Vernede, *Phil. Trans., Vol. 49*, p. 667.
[99] Borlase, Phil. Trans., *Vol. 50*, pp. 503-505. For a transcription see appendix A.ii.
[100] http://www.cornwallinfocus.co.uk/mining/reeth.php, accessed 11-4-2019.

coming under ground'.[101] Borlase inferred from this that the noise came from below, and also noted that several miners who were 60 fathoms (about 110 meters) underground had found the earth to move 'about them', as did others working in the adjoining mines.

Mr. Nantcarrow, in the meantime, wanted to ensure that the right conclusions were being drawn from his observations. He either had prior knowledge of the natural philosophical theories that placed the cause of earthquakes in the atmosphere, or he learned of this interpretation while conversing with William Borlase. In any case, he was adamant to prove that the source of the noise and shaking 'proceeded from below, and not from any concussion in the atmosphere above'.[102] He noted that no thunderstorm had ever before affected the air as far deep as 60 fathoms, and that the various objects and the noise of the ongoing work between the surface and the depths of the mine would certainly 'contribute to break the vibrations of the air as they descend'. William Borlase found himself likely to agree with Mr. Nantcarrow, concluding that 'this therefore could be no other than a real tremor of the earth', and referring to his interlocutor with language that shows both admiration and some measure of surprise: 'this very intelligent captain of the mine'.[103] Mr. Nantcarrow and the other miners employed their special authority as knowers of the underground not only to bolster the reliability of their observations, but also to interpret what their observations meant.

Let's also consider another case. After the Lisbon earthquake of 1755 it was observed by many that the waters in England were very strongly agitated.[104] While it was generally agreed that this was an effect of the earthquake, there was some discussion about what this could mean, because no one had perceived any tremors of the ground in England itself. No one, that is, except for those working underground. Several months after the event, the reverend William Bullock travelled to the lead mines at Eyam-Edge in Derbyshire, some seven miles from his home in Ashford. Here he made a 'strict inquiry […] and can assure you, that the circumstances related may safely be relied upon as matter of fact'.[105] He first talked to the overseer of the mine, one Francis Mason. Sitting in a small room aboveground, Mason had felt one single shock and observed that several pieces of plasters had come down from the walls. This made him fear that one of the mineshaft had collapsed, but 'contrary to his expectation, [he] found the shaft open, and all things about the spot in their proper order'.[106] Later that day, as Mason was walking home from the mine, he noticed a cleft 'about one foot deep, and six inches in diameter; its continuation from one end to the other,

[101] 'Deads' refers to discarded rocks and other rubble in the mineshafts.
[102] Borlase, Phil. Trans., *Vol. 50*, p. 504.
[103] Ibid., p. 504.
[104] For all these various accounts, see: *Philosophical Transactions, Vol. 49*, pp. 351-398.
[105] Bullock, *Phil. Trans., Vol. 49*, p. 398.
[106] Ibid., p. 399.

was near 150 yards, being parallel to the range of the vein on the north side'. When William Bullock went to examine the cleft, it had shrunk to no more than 60 yards long, eight or nine inches deep, and four inches in diameter. Despite these diverging observations, Bullock was willing to believe the overseer's account, noting that there were some traces that indicated a previously larger cleft. The more noticeable accounts however, came from within the mines. Bullock noted that:

> Though my inquiry was of every one in particular, that was there employed about the mine, the concurrence of whose testimonies might seem more strongly to confirm the account; yet I look upon it as unnecessary to trouble you with every man's story, which would be only a repetition of, or something similar to, what has been before related.[107]

The testimonies of two miners stood out in particular. These were William Hallom and John Howson.[108] At the time of the earthquake they were about 120 yards below ground, and employed in drawing the mined ore along a 50 yards long drift so that it could be raised up along the shafts. William Hallom described how he had just loaded his cart at the end of the drift, when he was 'suddenly surprised by a shock'. Hallom ran towards his partner, who was at the western end of the drift. Not daring to climb up the shaft, the two men deliberated where to take refuge. They were soon startled by another shock, this one 'much more violent than the former […] so great, that it caused the rocks to grind one upon another'. In a bout of fright, they ran towards the eastern end of the drift. Another (anonymous) miner, working in a cavity some 12 yards below, shouted that they should climb down the shaft, because his cavity was 'encompassed with solid rock' and less likely to cave in than the mine shafts. Once they arrived here, the three men shared their experiences of the event with each other, noting that none of them were harmed. As they were talking they were interrupted by a third shake, followed by two more at four to five minute intervals. Each of these shakes was less intense than the second shock. They heard a low rumbling noise after every shock, 'which continued for about half a minute, gradually decreasing, or appearing at a greater distance'.[109] The final remarks of the report neatly demonstrate the attention to the timing, intensity and extent of the shocks:

> They imagined, that the whole space of time, from the first shock to the last, was about twenty minutes […] as they went along the drifts, they observed, that several pieces of minerals were dropped from the sides and roof, but all the shafts remained intire, without the

[107] Ibid., p. 400.
[108] For a transcription see appendix A, iii.
[109] Ibid., pp. 400-401.

> least discomposure. The space of ground at the aforesaid mines, wherein it was felt, was 960 yards'.[110]

What could be learned from these accounts? First of all, it was clear that the shocks were more severe below the ground than above. Where the overseer Francis Mason only felt one shock, the miners in the drifts all agreed that there were five. Mason had likely only sensed the second, most severe shock. Further away from the mines, not even this shock had been felt. In the Reeth mines in Cornwall the miners down below too felt the earthquake more severely and particularly than those above. Where Mr. Nantcarrow described the motion as a 'prodigious swift, and apparently horizontal motion', the workmen agreed that it was indeed quick at first, but then became a 'slower wavy tremor'.[111] Moroever, the earth moved 'about' them, and not only beneath. The miners Howson and Hallom also testified that it had been the earth and rocks around them that had been moving, grinding 'one upon the other'. All this established the earthquake as a subterraneous phenomenon, though it was still ambiguous whether this grinding was the cause or the effect of the shocks. Their description of the sound was careful, noting that it was 'gradually decreasing' while allowing for the possibility that this was merely because it was 'appearing at a greater distance'. They did not identify the general direction of the shocks however.

The miners of Huel-Rith and Eyam-edge made observations about the nature of earthquakes that strongly favored a subterranean cause for earthquakes. Unbeknown to any at the time, observations such as these would eventually inform the idea that seismic waves traveling through the earth itself, rather than any vapors or electricity, were the cause of earthquakes. The reasons that these testimonies were collected and recorded in the first place were twofold. First, over the preceding decades cumulative epistemological challenges concerning who was deemed fit to make earthquake observations had made even the remote mining areas of Derbyshire and Cornwall legitimate sites for producing natural philosophical knowledge. Secondly, the wish to make a comprehensive overview of the varying intensity of earthquakes (both inspiring of and driven by the availability of eye-witness accounts) drew attention to the stories of miners who claimed to have felt the earthquake very violently. The conclusion that this was indeed the case was by no means set in stone beforehand. Even though William Borlase noted that the earthquake was 'particularly alarming in our mines', his first explanation for this was simply that there was 'less refuge, and consequently a greater dread from the tremors'.[112] The benefit of hindsight makes it seem obvious that mines would be an ideal site for earthquake observations. Yet in the

[110] Ibid., pp. 401-402.
[111] Borlase, Phil. Trans., *Vol. 50*, p. 503.
[112] Borlase, Phil. Trans., *Vol. 50*, p. 503.

eighteenth century it was the growing practice of employing lay observations, and the kinds of questions these observations were directed to answer, that gradually drew the attention of naturalists from the hillsides to the mineshafts.[113]

Conclusion

This chapter has focused on the contributions of observers to the natural philosophy of earthquakes. While they were considered indispensable to establish the extent, time and relative intensity of earthquakes, the conclusions about these elements could only be drawn by expert naturalists because only they had access to all the aggregated data. Lay observers also suggested new interesting spaces and ways of observing, and were the first to suggest the language of 'moving' earthquakes, a 'center' and of waves traveling through the earth itself. Such examples show the kind of agency some observers sometimes had over the making and framing of their observations. Yet most of the time, the value of 'natural philosophical' work was considered to lie in the process of interpretation and theoretical translation performed by naturalists rather than in the making of observations. The practices of observing earthquakes generated new natural philosophical insights that laid the foundations for late-eighteenth century seismology. They also established another condition for early seismology: a more clearly defined hierarchy between observers who provided the raw materials and the naturalists whose theoretical knowledge and oversight over the observations transformed these materials into natural philosophy.

[113] Consider also this quote from John Michell, which is very close to explaining earthquakes in relation to fault displacement, based on the observations of miners: 'Besides the raising of the strata in a ridge, there is another very remarkable appearance in the structure of the earth, though a very common one; and this is what is usually called by miners, the trapping down of the strata; that is, the whole set of strata on one side of a cleft are sunk down below the level of the corresponding strata on the other side. If, in some cases, this difference in the level of the strata, on the different sides of the cleft, should be very considerable, it may have a great effect in producing some of the singularities of particular earthquakes.' Michell, pp. 24-25; Musson, Roger, 'A history of British seismology', in: *Bull Earthquake Eng., Vol. 11, 715–861* (2013), p. 741.

4. Subject to Shaking

How did earthquake observers become lay subjects?

The shipwrecked sailor was all alone when the earthquake struck. Years later, he would recount that 'the ground I stood on shook three times at about eight minutes distance, with three such shocks, as would have overturn'd the strongest building that could be suppos'd to have stood on the earth, and a great piece of the top of a rock, which stood about half a mile from me next the sea, fell down with such a terrible noise, as I never heard in all my life. I perceiv'd also, the very sea was put into violent motion by it; and I believe the shocks were stronger under the water than on the island.' The earthquake did not affect the castaway for long. As soon as the first fright was over, the 'impression it had made went off also'. Yet this first fright had been considerable: 'I was so amaz'd with the thing it self, having never felt the like, or discours'd with any one that had, that I was like one dead or stupify'd; and the motion of the earth made my stomach sick like one that was toss'd at sea'.

Although the sailor's account ticks all the boxes of a credible earthquake report, it was entirely fictional. In fact, the sailor is none other than Robinson Crusoe, the protagonist of Daniel Defoe's eponymous novel published in 1719. It is still a matter of debate which contemporary and historical sources Defoe used for inspiration, and hence whether Crusoe's description of the earthquake was based on any real testimony. What is certain, however, is that Daniel Defoe had close contacts with several members of the Royal Society and that he was well acquainted with contemporary standards of gathering and presenting eye-witness accounts.[1] For his own publication on 'The Storm' which hit England in 1703, Defoe had made extensive use of lay observations, as well as printed accounts from the *Philosophical Transactions*.[2] When it came to earthquakes, Defoe was clearly aware of the kind of language and observations that would make the account realistic, and diligently included details on the

[1] Ilse Vickers, *Defoe and the New Sciences* (Cambridge University Press, 2006), pp. 99-138. See also pp. 29, 152-153 for the influence of Robert Hooke's *Discourses* and John Woodward's *On Observation.*
[2] Daniel Defoe, *The Storm* (London, 1704)*;* Vickers, p. 66.

place, time and intensity of the earthquake, as well as its effects on the weather. Aside from all these matters, Defoe was also attentive to the kinds of sensations and passions the earthquake would likely arouse within the observing protagonist. Crusoe was 'frighted', 'amazed' and 'stupified', and he began to feel sea-sick.[3]

Charles Davison, a nineteenth century seismologist with an interest in the effects of earthquakes on human beings, approved of these characterizations, but stressed that 'the ground would not have ceased from trembling, slight as a rule, but broken every now and then by violent shocks that should have filled Robinson Crusoe with fresh terror. He certainly should not have felt composed for several days.'[4] By the early twentieth century, a body of scientific knowledge (seismopathology, as it was called by one exponent) had emerged that prescribed how one *should* naturally feel after an earthquake.[5] Around 1700, English naturalists and observers were developing a similar discourse about the effects of earthquakes on human beings and animals. Daniel Defoe was well aware of this. Despite his claim to be only a little affected, Crusoe later remarks to still be 'terrify'd and dejected'. For days, he 'never slept in quiet', and even months after the event he dreams of the trembling earth spawning a horrible fiery apparition.[6] With the words 'terrified and dejected', Defoe captured an essential element of the eighteenth-century pathology of earthquakes: it employed the prevalent humoral theory of the body to link together the physical, mental, moral and epistemological aspects of experiencing an earthquake. 'Dejected' referred to the common diagnosis of melancholia, or an excess of black bile. 'Fright', as we will see, was both an epistemic category and a passion which could affect the body by causing humoral imbalances.

The connections between early eighteenth-century medical theory and earthquake philosophy are interesting in their own right, but they are not the central concern of this chapter. Rather, I am interested in what this highly personal form of knowledge can tell us about the impact of early earthquake science on the observers themselves. What did it mean to be an earthquake observer? To make this analysis, I employ the concept of the *subject*, in its Foucauldian sense. An explanation of this somewhat technical term is in order here. Since the introduction of the subject as a philosophical concept in post-Kantian philosophy, it was held to be a free agent and *producer of knowledge* of the external world.[7] This tradition uses

[3] On pages 9-10 of Defoe, *Robinson Crusoe* (Oxford University Press, 2007. Ed. Thomas Keymer. Original 1719) Crusoe's 'rite of passage' in becoming a sailor is overcoming a terrible sea-sickness. In the later storm which shipwrecks Crusoe, there is no mention of him being sick even though it is a more violent storm than any of the sailors ever experienced. Crusoe has clearly overcome his sea-sickness. Hence, this is a powerful metaphor to describe the intensity of the shakes.

[4] Quoted in Coen, *The Earthquake Observers*, p. 37: Davison, "Robinson Crusoe's Earthquake: The Realism of Defoe." Times (London), 19 March. 1934, p. 17.

[5] Coen, *The Earthquake Observers*, p. 132. See also, Ibid. pp. 125-140.

[6] Defoe, *Robinson Crusoe*, pp. 70-71, 75.

[7] As Daston and Gallison remind us, the words objective and subjective themselves have a long and convoluted history. In the seventeenth century '"Objective" referred to things as they are presented to consciousness, whereas

the word subject merely as a technical term for *any person* in the general sense (I.E. 'the subject has consciousness' refers to an essential human characteristic). For Foucault, however, the subject is as much a *product of knowledge* as it is a producer of knowledge.[8] It is not an a-historical 'person' but always a 'certain kind of person' that changes through time and in different cultural and institutional contexts.[9] The Foucauldian subject thus refers more to the kind of person someone is perceived to be by others and by themselves. Their subjectivity is the knowledge they have about themselves: their capacities, desires, responsibilities, the expectations about their behavior, etc. An individual person encounters the world not as a blank slate, but through its many subjectivities. They are a political subject (am I a loyal citizen or a dissident?), an economic subject (am I an ethical consumer?), a medical subject (am I a fit person?), a gendered subject, a racialized subject, a religious subject and so on. All these forms of self-understanding shape the course of one's actions.[10] Even though these modes of subjectivity arise through discourses that are outside of any individual subject, they only exist insofar as people think of themselves in terms of these discourses.

According to Foucault, there are three main ways in which a person acquires a certain subjectivity. To make matters less confusing, he called these processes of producing subjects 'modes of objectification'.[11] The first of these modes is through scientific inquiry: the subject is established and studied as the object of a certain kind of knowledge. For instance: 'the swing voter' is an object of study in political science, as is 'the chronic patient' in medical science or 'the laborer' in economic science. At the same time, new sciences like economics and psychology gave us new vocabularies for thinking and talking about ourselves as such subjects. The second mode of objectification is through 'dividing practices'. The modern age's penchant for actively separating 'the mad and the sane, the sick and the healthy, the criminals and the "good boys"' spatially distinguishes one subject from another, and places some

"subjective" referred to things in themselves'. In the works of Kant subjective referred to empirical sensation, and objective validity to the preconditions of this experience: time, space and causality. Post-Kantian philosophy established the now common connotations of objectivity and subjectivity by coupling this idea with other early-modern dualities such as sense versus reason, particulars versus universals, objects versus events, and certainty versus probability. See: Lorraine Daston & Peter Gallison, *Objectivity* (Zone Books, 2010), pp. 29-30; Udo Thiel, *The Early Modern Subject. Self-consciousness and personal identity from Descartes to Hume* (Oxford University Press, 2011), pp. 432-437. Foucault's reaction was targeted to the conception of the subject in the dominant strands of early twentieth century continental philosophy: existentialism and phenomenology. See: Clare O'Farell, 'Michel Foucault: the unconscious of history', in: Nancy Partner, & Sarah Foot (Eds), *The Sage Handbook of Historical Theory* (Sage, 2013), p. 176; D. Trombadori, 'Interview with Michel Foucault', in: James Faubion (Ed.), *The Essential Works of Michel Foucault*: *Power* (The New Press, 2000), pp. 246-247.

[8] Michel Foucault, '*The Subject and* Power', in: Paul Rabinow (Ed.), *The Essential Works of Michel Foucault*: *Ethics, Subjectivity and Truth* (The New Press, 1997), pp. 777-778, 781-783.

[9] Cressida Heyes , 'Subjectivity and Power', in: Diana Taylor (Ed.), Michel Foucault. Key concepts. (Acumen Publishing, 2013), p. 159; Foucualt famously argued that 'mankind' is itself such a subject that arose out of the 'human sciences' and is 'a recent invention' within a restricted chronological and geographical area. Michel Foucault, *The Order of Things: an archeology of the human sciences* (Routledge, 2005. Original *Les Mots et les Choses*, 1966). See the famous conclusion on pp. 421-422.

[10] The ability to shape one's actions is integral to Foucault's understanding of power, which he refers to as a *conducting* of a persons *conduct*. Foucault, '*The Subject and* Power', p. 789-790.

[11] Foucault, '*The Subject and* Power', p. 777.

subjects in institutional contexts where different rules apply from the rest of society and where they are constantly observed. In this mode, self-knowledge arises from the realization that your subjectivity is governed by a different and explicit set of rules. The third mode of objectification relates to 'the way a human being turns himself into a subject.'[12] Examples of this include how people 'learn to recognize' themselves as sexual, moral or rational subjects. Where the first two modes of objectification rely on subjects being observed by others, this final form engages with how subjects learn to observe themselves.

With this short theoretical exposition in mind, I wish to investigate how early modern earthquake philosophy shaped lay observers as subjects through the first and third mode of objectification specifically. These modes were strongly intertwined, but for the sake of argumentative clarity I will discuss them relatively independently. The first mode of objectification came through the medical discourse associated with earthquakes. After briefly revisiting the distinction between the knowledge of objects and the experience of events formulated in the first chapter, the subsequent two sections will argue that lay observers not only participated as observers of external objects and events but that, as they examined the state of their own bodies and minds, they also became objects of earthquake-related knowledge. The fourth section examines how earthquake observers were also understood as subjects within a growing discourse of 'moral philosophy'. Notwithstanding Cartesian dualism, there still were close connections between the early modern body and the mind, and between medical and moral knowledge.[13] Although earthquakes were beginning to lose some of their religious significance, they remained moral events that affected the behavior of those who experienced one. It is important to note that these forms of discursive objectification also include an element of coming to understand one-self within this naturalistic and moral discourse. This third mode of objectification will be explored further in the final section of this chapter. It argues that the very acts of observing and telling truth carried moral connotations. The epistemological question *are you reliable?* also implies a moral judgement. Earthquake observers thus learned to recognize themselves as moral subjects in relation to the social practices of natural philosophy. In both forms of objectification, observing earthquakes meant observing oneself; knowledge was tied to self-knowledge.[14]

This type of analysis offers the opportunity to study more in-depth the many ways in which natural philosophy, epistemology and social forces were intricately tied up in the practical realities of earthquake observations, a theme which we have already touched upon in the

[12] Foucault, '*The Subject and* Power', p. 778.
[13] Mary Lindemann , *Medicine and Society in Early Modern Europe* (Cambridge University Press, 1999), p. 11; Angus Gowland, 'The problem of early modern Melancholy', in: *Past and Present, Vol. 191, no. 1* (2006), pp. 84, 99-101; Kevin Siena , 'Pliable Bodies: the moral biology of health and disease', in: Carole Reeves (Ed.), *A Cultural History of the Human Body in the Enlightenment* (Berg, 2010), p. 41.
[14] In Foucault's more complicated terminology, the idea of the observers a both a knower and a known is termed the 'Empirico-transcendental doublet', see: Foucault, *The Order of Things*, p. 347.

previous chapters. The two previous chapters highlighted the potential for observer-based earthquake philosophy to challenge social norms about who is a reliable observer and interpreter, and analyzed the ways in which these challenges generated new natural philosophical insights. This chapter examines how the practices of observation also *reproduced* social hierarchies. For one, it shows how observers were not in control of the way their accounts were used. The epistemic practices which turned lay observers into objects of knowledge clearly reflected unequal power relations deciding on the nature and value of knowledge, and how this knowledge was to be employed. Secondly, it shows how earthquake observers were led to observe and evaluate themselves according to the metrics provided by earthquake philosophy. As shown in the previous chapter, the attempts of earthquake philosophers to stand above those whose accounts they used also produced new knowledge about earthquakes. At the same time they also laid the foundations for another body of knowledge: the subjectivity of the lay observer.

Object evidence reconsidered

In the first chapter of this thesis we analyzed how earthquake philosophy wrestled with an essential dichotomy between objects and events. From this dichotomy, we distilled two types of evidence used by naturalists to study earthquakes as events. On the one hand, there were attempts to 'objectify' the earthquake. On the other, there was experiential evidence provided by eye-witnesses. So far we have focused primarily on these experiences, and on the ways in which they were translated for and by naturalists. We found that, far from conforming to ideal-type scientific observations, most lay observers consistently commented on how they *felt* the earthquake. While these insights were essential in challenging old theories and suggesting new approaches, it was also painfully clear that observers were unable to deliver the epistemological certainty that naturalists desired. Descriptions detailing how an observer felt the earthquake were on principle impervious to external verification. To be sure, one account could be compared to another, but there were many different factors that could explain any discrepancy between two accounts.[15] Even if the statement 'I felt' could be considered true beyond any doubt, there was a growing anxiety that this personal feeling was no longer part of the world of common knowledge that was open for all to experience. Such solipsistic anxiety could have caused a wholesale rejection of the idea of eye-witnesses as a reliable basis

[15] For instance the varying intensity of the earthquake, the different terrain, the different positions of the observers, their differing constitutions and general susceptibility to earthquakes, and finally also the different skills of observing.

for natural philosophy. Instead, eighteenth-century naturalists and observers alike engaged in practices aiming to translate the unobservable.

The strict distinction between object-evidence and experiential evidence was a useful analytical tool, but at this point in the argument it has lost its usefulness. We have already encountered some of the ways in which knowledge through direct sensation and knowledge through objects complemented one another while making and presenting earthquake observations. In a way, every earthquake observation was also an indirect observation. Even those observers who commented how the earthquake directly affected them, and how they felt *it* pass by did not, technically, describe any essence of the event itself. They described the parameters of the event: its time, place, direction and intensity. In turn, these things could only be observed using tools and proxies. When one observed an earthquake, one was not *looking* at an earthquake. Rather, one's gaze was directed at a watch, a map, a bed and possibly some fallen roof tiles. The elusive earthquake was translated into a collection of material objects that could be examined, exchanged and compared. The detailed descriptions of an earthquake's effect mundane objects did not merely derive from a desire to record any observation indiscriminately; they were among the most relatable kinds of observation.

Better still were observations of natural objects that were produced or transformed by an earthquake. Wells opening up, running dry or turning foul, and clefts opening in the ground were among such things.[16] They were not only mentioned when they occurred, but sometimes also when they failed to manifest after an earthquake.[17] A major factor determining both the credibility and usefulness of such observations was that naturalists could travel to such sites themselves and witness the phenomenon with their own eyes. In many cases these wells had since already been restored to their former state, and the chasms had shrunken or disappeared by the time the naturalist arrived on the scene (as we saw in William Borlase's account in the previous chapter).[18] This mattered little, as the limited observations made at these sites indicate that the fact that there had been a cleft was more important than the specifics of the site itself.[19]

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[16] See: Woodward, *Brief Instructions*, pp. 7-8: ' whether they spue not forth water: whether the water of the wells, springs, and rivers thereabouts do not become warm, turbid, or send forth more water than usual, at the time of the Earthquake: whether the neighbouring therma, or hot-springs, if any, become not more hot, and muddy, than before'.

[17] See for instance: Bonajutus, *Phil. Trans., Vol. 18, no. 207*, p. 5; Sloane, *Phil. Trans., Vol. 18, no. 209*, p. 88; Plant, *Phil. Trans., Vol. 42, no. 462*, p. 33; Pedini, *Phil. Trans., Vol. 42, no. 463*, pp. 79-81, 86; Porter, *Phil. Trans., Vol. 49*, p. 118; Latham, *Phil. Trans., Vol. 49*, pp. 411-413; Sotqueler, *Phil. Trans., Vol. 49*, p. 416; De Vautravers, *Phil. Trans., Vol. 49*, p. 438; Pye, *Phil. Trans., Vol. 49*, p. 459; Bonnet, *Phil. Trans., Vol. 49*, pp. 511-512; Trembley, *Phil. Trans., Vol. 49*, p. 618. That there was a demand for object-evidence that could not always be met is clear in the following remark: 'as for any thing (I presume you meant lambent flame, vapour, &c.) being perceiv'd on the surface of the ground, before or during the earthquake, nothing of this kind has as yet been mention'd to me from any quarter', Nixon, *Phil. Trans., Vol. 46, no. 497*, p. 712. See also: Vernede, Phil. Trans., Vol. 49, p. 668.

[18] Borlase, *Phil. Trans., Vol. 50*, pp. 499-502.

[19] The closest inspections seem to have related to the depth and width of the clefts. A common formulation of such inspection is formulated in: Latham, *Phil. Trans., Vol. 49*, pp. 411-413: 'the ground was opened; in some places you might put your hand down broad-ways, and not feel the bottom with a long stick.' For similar descriptions see: Bonajutus, *Phil. Trans., Vol. 18, no. 207*, p. 5; Bonnet, *Phil. Trans., Vol. 49*, pp. 511-512.

Similarly, a German diplomat writing after the 1755 Lisbon earthquake mentioned that he 'was informed, that there was some some bituminous matter, but could find none. Indeed I once picked up a stone split through the middle, whose edges seemed to me to have sulphur lodged on them; but I was then in a hurry, and never could find the place where I had taken it up.'[20] Here, as in other cases, eye-witness reports were instrumental in directing an interested observer towards a tangible object produced by the earthquake. While reflecting the high status of object-evidence for naturalists, it also shows the limitations of this approach: the bituminous matter itself was never found, and the site of the sulphurous rock was lost.

Animals, too, were observed with interest. In fact, the questionable ability of various animals to sense an upcoming earthquake is still a subject of research today.[21] In the early eighteenth century there were no references yet to such predictive capacities, but the deviation of their behavior from the norm was commonly attested to. Horses and dogs, the few animals still living among the wealthier and urban earthquake observers, feature most frequently.[22] Shepherds and milkmaids also noticed the effects of an earthquake on their flocks and herds.[23] One woman from Northampton observed that the birds she kept 'drooped remarkably, and hid their heads under their wings: a circumstance which is often observed in Italy, and other places where these phaenomena are frequent'.[24] A London fisherman relied on the testimony of his crew to relate that the fish had started jumping out of the water when the earthquake struck in 1750.[25] Of course, William Stukeley used these observations to argue that the earthquake was electric: 'like as the experiment of electrifying the fishes; it makes them sick'.[26] Such observations were also used to determine that the effects of earthquakes were not only limited to the land but were also felt underwater. More importantly, it showed that earthquakes had clear effects on living creatures.

All these observations made reference to a shared world of knowledge. In the eighteenth century, when even in a city like London humans and animals lived in much closer proximity than now, knowledge of animals natural behavior, and their responses to danger constituted such a type of common knowledge. Those familiar with the surrounding landscape could recognize changes in the surrounding land caused by the earthquake. These sites were in

[20] Sotqueler, *Phil. Trans., Vol. 49*, pp. 417-418.
[21] Heiko Woith, Gesa Petersen, Sebastian Hainzl, Torsten Dahm, 'Review: Can Animals Predict Earthquakes?', in: *Bulletin of the Seismological Society of America, Vol. 108, No. 3A* (2018), pp. 1031–1045.
[22] For instance: Dudley, *Phil. Trans., Vol. 39, no. 437*, p. 69; Cooke, *Phil. Trans., Vol. 46, no. 497*, p. 651; Porter, *Phil. Trans., Vol. 49*, p. 118; Bonajutus, *Phil. Trans., Vol. 18, no. 207*, pp. 3-4; Bayley, *Phil. Trans., Vol. 39, no. 444*, p. 366; Miles, *Phil. Trans., Vol. 46, no. 497*, p. 641.
[23] Layard, *Phil. Trans., Vol. 46, no. 497*, p. 621; Burrat, *Phil. Trans., Vol. 46, no. 497*, p. 682.
[24] Doddridge, *Phil. Trans., Vol. 46, no. 497*, p. 719.
[25] Boyfield, *Phil. Trans., Vol. 46, no. 497*, pp. 637-638. See also: Pigot, *Phil. Trans., Vol. 13, no. 151*, p. 317; Parsons, *Phil. Trans., Vol. 46, no. 497*, p. 636, mentions three independent of fish leaping out of several ponds during the earthquake of March 1750.
[26] Stukeley, *Philosophy of Earthquakes*, p. 744.

principle accessible to most people who wanted to see it for themselves. Finally, the observations of furniture and household items fulfilled a similar function: their size and weight was generally known, which made the experience relatable and facilitated comparison among accounts. Earthquake observers, in short, relied on their own sensations as well as on the objects around them to communicate their experiences. In this way, an uneasy balance between the necessity of lay observations and the epistemologically more reassuring objects continued to shape the natural philosophy of earthquakes.[27]

The elements of earthquake pathology

The final objects of knowledge were the bodies and minds of the observers themselves. The epistemic tensions which earthquake philosophers had identified and struggled with since the conception of observer-based earthquake philosophy led them to use the available accounts in new, innovative ways that combined the observer as *knower* and as *known.* The observer's knowledge regarding their own bodies and state of mind was seen as more reliable than their knowledge of the external world, and hence an acceptable compromise between object evidence and experiential evidence. What kind of knowledge was produced by this epistemological commitment?

The effects of earthquakes on human health were already a subject of inquiry in Woodward's 1696 guide to natural observations, which urged observers to note 'whether fevers, and other distempers do not then invade inhabitants of those parts'.[28] Around the same time, John Flamsteed mentioned that people found themselves to be sick in the stomach and their head to turn dizzy after an earthquake.[29] Headaches, back pains and a feeling of sea-sickness indeed seem to have been among the most common complaints.[30] Usually such effects would only last a few hours, but there could also be more severe consequences. Flamsteed recounted the story of a London surgeon named Kesterne, who was prone to fits of apoplexy.[31] During the September 1692 shocks in London Kesterne felt himself 'very affected' and resolved to place his elbows in the window frame and put his head between his hands. After the earthquake was over he had his blood let as a preventive measure against another stroke.

[27] Of course not every object was equally 'reassuring': a broken vase was less ambiguous evidence than a vase that had been displaced and would soon be put back in its original position. The point of this section is not to catalogue these differences however, but to argue that objects played an important role in experience-based observations.
[28] Woodward, *Brief Instructions*, p. 8.
[29] Flamsteed, *Letter Concerning Earthquakes*, p. 6.
[30] Lewis, *Phil. Trans., Vol. 38, no. 429*, p. 120; Temple, *Phil. Trans., Vol. 41, no. 456*, p. 341; Parsons, *Phil. Trans., Vol. 46, no. 497*, p. 634; Trembley, *Phil. Trans., Vol. 49*, pp. 438-439.
[31] Flamsteed, *Letter Concerning Earthquakes*, p. 6. Apoplexy is now used as a general term for internal bleeding. In early modern medical discourse it referred almost exclusively to hemorrhagic strokes.

Another account related the story of a man who suffered from convulsive fits but had been cured of them. Yet after experiencing an earthquake in 1732 he was 'immediately seized with them again'.[32] Although these cases were somewhat extreme, it was generally known that 'there never happen'd one shock amongst us, but what occasion'd some alteration at that time in every person's countenance and constitutions'.[33] One observer gave an overview of the various other afflictions caused by an earthquake:

> The effects it has had on humane bodies (although I do not believe they have all immediately been caused by the earthquake) have (yet) been various: such as foolishness (but not to any great degree), madness, dullness, sottishness, and stolidity everywhere. Hypochondriack, melancholic and cholerick distempers. Every day fevers have been common, with many continual and tertian. Malignant, moral and dangerous ones in a great number, with deliria and lethargies. Where there has been any infection caused by the natural malignity of the air, infinite mortality has followed. The small-pox has made great destruction amongst children. And in short, there has been no state or condition which has not had its share in so universal a calamity.[34]

There is a lot to unpack in these observations. First of all, the author did not believe that all these symptoms were *immediate* effects of the earthquake, but could not indicate which effects were immediate and which were secondary. This general uncertainty is also reflected in the wide variety of symptoms. Other accounts can help us here. Among the effects likely to be recognized as secondary would be the fevers and smallpox. Following the 1692 Port Royal earthquake, several observers noted that the 'want of dry houses, warm lodging, proper medicine and other conveniences' were detrimental to the health of the survivors, causing such illnesses.[35] Melancholy, too, could be a secondary effect resulting from the misery and destruction following an earthquake, and in one case the realization that earthquakes were becoming more common in England 'furnished room for melancholy reflections' in the mind of an observer.[36] Yet it was not always secondary. In another case, several witnesses described how their minds were struck 'with the melancholy presage of the approaching earthquake', suggesting that the earthquake itself, rather than the devastation it caused, directly influenced people's mental state.[37]

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[32] Temple, *Phil. Trans., Vol. 41, no. 456*, p. 341. See also: Birch, *Phil. Trans., Vol. 46, no. 497*, p. 615-616: 'another gentleman describ'd to me the sensation, upon being awaken'd by the motion, to be like that of falling into a fit'.
[33] Plant, *Phil. Trans., Vol. 42, no. 462*, p. 41.
[34] Bonajutus, *Phil. Trans., Vol. 18, no. 207*, p. 8.
[35] Anon., *Phil. Trans., Vol. 18, no. 209*, p. 83. See also: Anon., *Phil. Trans., Vol. 18, no. 209*, p. 100.
[36] Folkes, Phil. Trans., Vol. 46, no. 497, pp. 701-702. See also: Anon., *Phil. Trans., Vol. 18, no. 209*, p. 93; Pedini, Phil. Trans., Vol. 42, no. 463, p. 85.
[37] Bonajutus, Phil. Trans., Vol. 18, no. 207, p. 4.

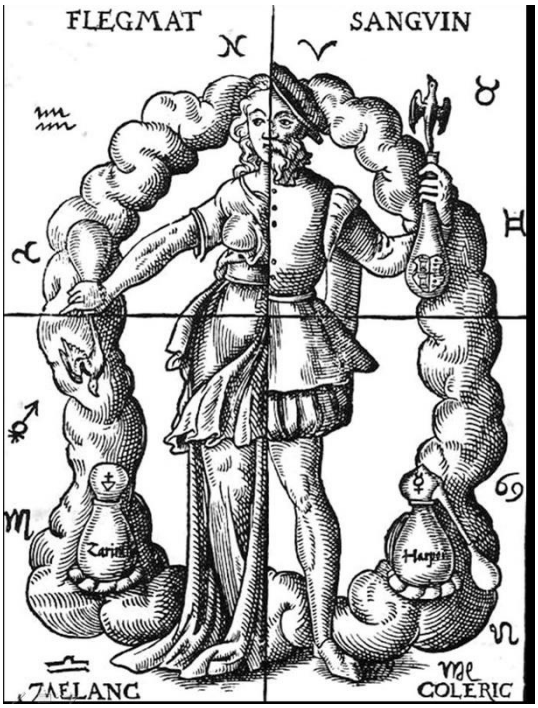

Figure 17. The four humors depicted in Leonhard Thurneysser's Quinta Essentia (1574).

This seemingly confused picture of cause and effect becomes clearer when we take a closer look at the place these various afflictions occupied within the prevailing system of medical knowledge. Although under siege from many different sides, the humoral theory of Hippocrates and Galen exerted a strong influence over the ways in which the body and disease were perceived in the early eighteenth century. According to this theory, the body contained four essential liquids: blood, yellow bile, black bile and phlegm. If these humors were balanced, the body was healthy. An excess or deficiency of one of these humors would cause disease.[38] The different humors were also connected to different mental states and natural elements, and were mapped onto a matrix of hot, cold, wet and dry. For instance, an excess of blood (warm and wet) made a person more sanguine, but also more prone to hemorrhages and fevers. If the humoral imbalance became too large, bloodletting and purges could be prescribed to restore it. In light of this theory, the 'melancholic and cholerick distempers' caused by the earthquake were the result of an excess of black and yellow bile. These were respectively, and not coincidentally, related to the elements of earth and fire: the proper cause of earthquakes. The other effects could be explained within these categories. The cold and dry black bile made people dull, stolid and lethargic.[39] The warm and dry yellow bile was associated with foolishness, madness, 'sottishness', and deliria.[40]

Several effects were also attributed to the 'natural malignity of the air'. Air was the element associated with blood (warm and wet). This connection helped contemporaries to explain the effects of earthquakes on people suffering from strokes, which was a disease of the blood. The air was also responsible for fevers in two ways. First, we find observers noting that the 'hurtful vapours' that 'belched from the many openings of the earth' were responsible for the great sickness that was generally experienced after an earthquake.[41] Secondly, hot and humid winds were said to produce more violent earthquakes and other natural disasters, and were generally

[38] Lindemann , *Medicine and Society in Early Modern Europe*, pp. 9-15; Marius Engelbrecht , *De Onttovering van de Waanzin* (Athaneum, 2013), pp. 31-35.
[39] Although these qualities could easily be ascribed to the phlegmatic, too.
[40] Engelbrecht, *De Onttovering van de Waanzin*, pp. 31-32.
[41] Anon., *Phil. Trans., Vol. 18, no. 209*, p. 100. See also: Plant, *Phil. Trans., Vol. 42, no. 462*, p. 41; Parsons, *Phil. Trans., Vol. 46, no. 497*, p. 634; Goodrich, *Phil. Trans., Vol. 46, no. 497*, pp. 726-727.

used to explain the relative lack of earthquakes in England.[42] Following an earthquake during a particularly warm summer, one observer noted that 'an abundance of people have suffered very severely from these excessive heats: putrid, bilious, petechial, nervous fevers, are exceedingly common every-where. Dysenteries haemorrhages, most profuse sweats, affect not only those in fevers, but a vast many others'.[43] All these afflictions were related to heat and blood. William Stukeley also saw possibilities for further research in describing how electricity was the cause for both earthquakes and the heat of the blood, asserting that:

> all motion, voluntary and involuntary, generation, even life itself, all the operations of the vegetable kingdom, and an Infinity more of nature's works, are owing to the activity of this electric fire; the very soul of the material world. And, in my opinion, it is this alone that solves the famous Question, so much agitated with the writers in medicine, about the heat of the blood. How these, how earthquakes, are begun and propagated, we are yet to seek.[44]

Earth, wind and fire were the essential elements for explaining the cause of earthquakes, and for understanding the diseases caused by them. This constellation of associations shows how interconnected the early eighteenth-century view of the natural world still was. The observer was not external to the elemental forces that produced earthquakes, but deeply affected by them. We have already seen that some experienced the melancholy of an earthquake before the event took place. Similarly, several observers claimed that 'for a few minutes before a shock of it came, they could foretell it by an alteration in their stomachs'.[45] These comments followed the 1727 Boston earthquake, whose aftershocks were recorded by the reverend Matthias Plant until as late as 1741. It seems that the inhabitants of Boston had learned to observe and read their bodies in order to construct knowledge about earthquakes. This knowledge was first of all predictive: if successful, earthquakes were no longer a mysterious force that could strike seemingly at random, but operated according to rationally discernible laws. Secondly, it said something about the nature of earthquakes. The alterations in people's stomachs, Plant supposed, were occasioned 'by an alteration in the air'.[46]Observations of people thus got to the very heart of a long debate on the nature and cause of the phenomenon.

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[42] This theory was proposed by Juan de Cárdenas in his *Problemas y Secretos Marvaillosos de las Indias* (1591). Quoted in: Jorge Cañizares-Esguerra, *Nature, Empire, and Nation: Explorations of the History of Science in the Iberian World* (Stanford University Press, 2006), p. 69. Jan Golinski, 'American Climate and the Civilization of Nature', in: James Delbourgo and Nicholas Dew (Eds.), Science and Empire in the Atlantic World (Routledge, 2008), pp. 153-156, 158.

[43] Huxham, *Phil. Trans., Vol. 50*, pp. 428-429. Putrid fevers closely correspond with the present-day 'epidemic typhus', which mostly spreads easily after humane and natural disasters. Bilous fever also invokes the hot and dry yellow bile, is inflammatory. Dysentery and hemorrhages both have 'bloody' symptoms. The account reflects contemporary fears over a possible change in England's temperature climate.

[44] Stukeley, *Philosophy of Earthquakes*, p. 748.

[45] Plant, *Phil. Trans., Vol. 42, no. 462*, p. 41. The point was an important one, seeing that the reverend Matthias Plant (who communicated it to the Royal Society) sought to back it up with the testimony of 'sundry persons' as well as 'attest to the truth of the thing by my own experience'.

[46] Plant, *Phil. Trans., Vol. 42, no. 462*, p. 41.

Seen within the frame of humoral medical theory, the wide ranging observations of earthquake-related afflictions reveal themselves to be relatively systematic and closely tied to different interpretations of the natural causes of earthquakes. There were limitations, too. Humoral theory could account for virtually any disease by positing a certain imbalance between multiple humors. If both choleric and melancholic distempers were observed, the earthquake clearly caused an imbalance in yellow and black bile. Yet it had trouble reasoning the other way around, and could not predict the effects of any given cause. Why did earthquakes produce both melancholic and choleric distempers in different persons? The logical answer was that earthquakes, like any external factor, altered and exacerbated the existing humoral misbalances within a person.[47] Hence the comment that 'there has been no state nor condition which has not had its share in so universal a calamity'.[48] A person affected with a melancholic distemper after an earthquake was likely more susceptible to melancholia due to an already existing excess of black bile. The earthquake merely brought out what was inside. As a result, not only did observers come to understand earthquakes by looking at their bodies, they came to understand themselves through earthquakes.

Fright and fancy

In our discussion of the pathology of earthquakes, we have so far bracketed two small but important statements. The first is that besides producing melancholic and choleric distempers, the earthquake also caused 'hypochondriack' distempers.[49] The second is that the earthquake inflicted 'nervous fevers'.[50] Both statements reflect the common view that the fear and excitement accompanying an earthquake could strongly influence the body through the imagination and the nervous system.[51] Such views were also expressed by natural philosophers who argued that earthquakes particularly affected 'those of weak nerves, or that have nervous complaints' and were generally 'obnoxious to hysterics'.[52] In early modern medical thought, the nervous system arose as an intermediary between the mind and the body that could imitate physical complaints and diseases through the imagination.[53] Yet it could not be said that such

[47] Siena, 'Pliable Bodies', pp 37-42; See also: Engelbrecht, *De Onttovering van de Waanzin*, p. 34.
[48] Bonajutus, *Phil. Trans., Vol. 18, no. 207*, p. 8.
[49] Bonajutus, *Phil. Trans., Vol. 18, no. 207*, p. 8.
[50] Huxham, *Phil. Trans., Vol. 50*, pp. 428-429.
[51] For a good contemporary overview of these views, see: William Falconer, A *dissertation on the influence of the passions upon disorders of the body* (London, 1696), for the matters discussed in this chapter, see particularly pp. 12-22 (on the basics of the passions), 48-52 (on the working of fear), 97-107 (in relation to apoplexy and the nervous system of women in labour), 128-176 (in relation to mental health).
[52]Stukeley, *Philosophy of Earthquakes*, p. 744.
[53] See: Elizabeth Green Musselman, *Nervous Conditions. Science and the Body Politic in Early Industrial Britain* (State University of New York Press, 2006), pp. 14-17; Séverine Pilloud & Micheline Louis-Courvoisier, 'The intimate experience of the body in the eighteenth century : between interiority and exteriority', in: *Medical History,*

nervous diseases were merely in one's head. Rather, exciting passions such as anger and fright were reckoned to cause real physical effects. As Barbara Duden puts it: 'anger, fright, impressions, delusions and imagined things were the prime causes of illness. With all these phenomena there occurred an exchange between outside and inside, and the inner body mediated what happened to it in the outside world'.[54] Hence to fully understand the way earthquakes were thought to affect human bodies we need to look at the pathology of fright that was associated with them.[55]

Fright was one of the many 'passions' that formed an important part of the complex medical system of the eighteenth century.[56] Unlike the modern emotions, which are imagined chiefly in the mind and affect the body only secondarily, the passions were thought to affect both the mind and body simultaneously through humoral imbalances.[57] According to the medical wisdom of Robert Burton in his famous *Anatomy of Melancholy*, the impression of fear influenced the temperature of the body. In accordance with Aristotelian tradition fear cooled a person down, which caused the physical manifestations of shivering and paleness, and allowed the dry and cold black bile to become dominant and cause melancholic anxiety.[58] Fright, in short, was not seen merely as a temporary response, but could produce a lasting medical impact. The language that accompanied the words 'fear' and 'fright' in the earthquake relations also reveals that there was more to this passion than just shock. The effects of fright were described as 'damaging', and some persons were 'sadly' frightened, implying some longer lasting effect.[59] One clerk was so affected by the shakes that he was 'forc'd for a while to give over his work'.[60] Several observers argued that the various illnesses which manifested directly after the earthquake had been caused directly by fright, and one even went as far as to state that 'some, through mere fear, have died'.[61] Especially those with 'weaker constitutions'

Vol. 47 (2003), pp. 460-464; Wendy Churchill, *Female Patients in Early Modern Britain. Gender, Diagnosis, and Treatment* (Routledge, 2012), pp. 179-223; David Gentilcore, 'The fear of disease and the disease of fear', in: William Naphy & Penny Roberts, *Fear in Early Modern Society* (Manchester University Press, 1997), pp. 184-186, 194-196; Siena, 'Pliable Bodies', pp. 35-36.

[54] Barbara Duden, *The Woman beneath the Skin. A Doctor's Patients in Eighteenth-Century Germany* (Harvard University Press, trans. Thomas Dunlap, 1998), p. 142. See also: p. 149: 'fright penetrated, drove the blood from the limbs to the heart, caused the heart to tighten, to suffocate under the abundance of blood'.

[55] This association was already made in Burton's *The Anatomy of Melancholy* (1621), where earthquakes are described in Section II, member IV, subsection III, 'Terrors and Affrights, Causes of Melancholy'.

[56] Harold Cook, *Matters of Exchange. Commerce, Medicine, and Science in the Dutch Golden Age* (Yale University Press, 2007), pp. 44-45, 252-259.

[57] Gail, Kern Paster, Katherine, Rowe, Mary, Floyd-Wilson, 'Introduction', in: Ibid. (Eds.), *Reading the Early Modern Passions: Essays in the Cultural History of Emotion* (University of Pennsylvania Press, 2004), pp. 16-18.

[58] Burton, *The Anatomy of Melancholy*, section I, member V, subsection I, 'continent, inward, antecedent, next causes, and how the body works on the mind', p. 257; Mary Ann Lund, 'Without a Cause: Fear in the Anatomy of Melancholy', in: Daniel McCann & Claire McKechnie-Mason , *Fear in the Medical and Literary Imagination, Medieval to Modern* (Palgrave Macmillan, 2018), pp. 41-44.

[59] Banks, *Phil. Trans., Vol. 24, no. 289*, pp. 1556-1557; Pennant, *Phil. Trans., Vol. 46, no. 497*, p. 687.

[60] Banks, *Phil. Trans., Vol. 24, no. 289*, p. 1556.

[61] Smith, *Phil. Trans., Vol. 46, no. 497*, p. 730; Anon., *Phil. Trans., Vol. 49*, p. 419-420.. Another noted that he had 'no doubt that the people in London are frighted at it, as the very mentioning of it seems to strike a terror into all sorts of people even at this distance'.

proved susceptible to these nervous afflictions. In the written earthquake reports, those with 'weaker constitutions' and those who were so 'sadly frightened' were rarely the authors, but usually appeared as objects of knowledge. It appears that a discourse on the fitness to experience an earthquake went hand in hand with a discourse on the fitness to observe and report an earthquake.

Nervous conditions such as fright were embedded in a strongly sexed discourse. Although not exclusively diagnosed in women, nervous diseases such as hysteria became known as feminine afflictions, as women's bodies were deemed more sensitive and nervous than men's bodies.[62] In eighteenth-century medical terms, women's bodies were considered to be more 'porous' and hence more easily affected by both the outside world and by the imagination.[63] It was a medical commonplace that the effects produced by the nerves were stronger on pregnant women, and that such effects could also influence the unborn child. The imagination of a pregnant woman was considered able to influence the skin color, as well as the physiognomy and temperament of the child, and exposure to horror or fright could deform it or cause a miscarriage.[64] This general framework of links between women, the imagination and fright is also visible in the earthquake observations. One observer mentioned hearing of a gentlewoman 'who was put into such a fright that she miscarried two days after'.[65] Several accounts pointed out that women specifically complained of headaches after an earthquake and were more prone to be shaken violently, as the trope of the gentlewoman launched from her chair discussed in chapter 2 also bears out.[66] Only one observer noted, with some surprise, that:

GB 133 Eng MS 19, W. Arderton to Henry Baker, *Correspondence of Henry Baker, Vol. IV*, fol. 256r.

[62] Churchill, pp. 209, 228; Green Musselman, pp. 43-47; Gentilcore, pp. 196-198.

[63] Lisa Forman Cody , 'The Body in Birth and Death', in: Carole Reeves (Ed.), *A Cultural History of the Human Body in the Enlightenment* (Berg, 2010), p. 24.

[64] Laura Gowing, 'Marked Bodies and Social Meaning', in: Carole Reeves (Ed.), *A Cultural History of the Human Body in the Enlightenment* (Berg, 2010), p. 145; Forman Cody, 'The Body in Birth and Death', pp. 24-25.In another case, unrelated to earthquakes, a report to the Royal Society recounted a pregnant woman who saw a small-poxed woman from a distance. The child was born with the smallpox. The report commented: 'it is very surprising and wonderful to consider the different manners, in which children, while in the mothers wombs, are affected by various accidents happening to the mothers. How the imagination only, affected by the disagreeableness of the sight, should convey the infection to this child in the case above recited, is, I own, what I am not able to account for; especially as there was no fright or suprize, and that the mother was under no apprehension of danger.' *Phil. Trans., Vol. 493*, p. 234.

[65] Banks, *Phil. Trans., Vol. 24, no. 289*, p. 1557. Another account noted that one Mrs. Alicock had died of an earthquake-related fright, several days after giving birth. While she was 'in a very fine way', she was 'so alarmed with the accident, that she expired within a few hours'. See: Doddrige, *Phil. Trans., Vol. 46, no. 497*, p. 717.

[66] For instance: Baker, Phil. Trans., Vol. 46, no. 497, p. 602.

> In one house, in the town of Uppingham, where two men and a woman were sitting, upon the approach of the sound (tho' they had no though of an earthquake) the men could hardly draw their breath in the house; but were immediately obliged to go out for fresh air; but the woman felt no disorder.[67]

Most comments on the different experiences and effects of an earthquake on men and women were less specific, but the language employed to describe their states does point towards some perceived differences. Whereas male observers were only occasionally described as frightened, female observers were almost invariably described as such. Similarly, servants were readily described as frightened than their masters. For instance, one observer noted that he was much 'surprized' and on his way through to the house encountered one maid servant 'running in a great fright' and a young gentleman whose mental state went unmentioned.[68] The general consistency with which such terms were used makes it likely that the distinction between 'surprized' and 'frightened' was a deliberate one. Only in really violent shocks, such as the 1692 Port Royal and 1755 Lisbon earthquakes, does the language of fright become applied more or less indiscriminately. For the lighter English earthquakes it seems that the fright of women was perceived as an expected response, whereas some male observers 'confessed' and 'owned' to the fact that they felt frightened.[69] These clearly gendered expectations surrounding the manifestation of fright were also expressed by one seasoned earthquake observer, who after experiencing several earthquakes in Italy complained that the 'dastardly' Londoners of 1750 were 'unmanned' by even a very slight shock. These 'fearful expectations' could be explained only by the fact that 'in all events, Englishmen are still after the fair'.[70]

The epistemological implications of this discourse were not always clear-cut. In the nineteenth century, for instance, several Swiss seismologists argued that women especially excelled at observing earthquakes by virtue of being more easily affected by them.[71] In the seventeenth and eighteenth centuries however, fright remained a problematic category in terms of observation. 'So strongly has the shock affected the minds of some', one observer wrote, 'that they imagine six or seven more have been felt since'. [72] This statement was both a warning against some questionable observations that were circulating, but also a piece of knowledge in

[67] Goodrich, *Phil. Trans., Vol. 46, no. 497*, p. 727.
[68] Newcome, Phil. Trans., Vol. 46, no. 497, pp. 653-654.
[69] Mortimer, Phil. Trans., Vol. 46, no. 497, p. 638. Anon., Phil. Trans., Vol. 49, p. 419: 'for my part, I must confess, I never was so much frightened in my life'. This latter account felt the need to substantiate the reason of the fright, fearing that his description of the event might not adequately explain his fright: 'the description I have given you of it, I can assure you, is not near so shocking as the thing really was itself'.
[70] Bowman, Phil. Trans., Vol. 46, no. 497, p. 685.
[71] Coen, *The Earthquake Observers* p. 93.
[72] Anon., Phil. Trans., Vol. 49, p. 420.

itself: the earthquake was able to produce shocks both in the ground and in the mind. The knowledge of these frightened observers was deemed inadequate when it came to their assessment of the external world, but the knowledge of their internal imagination was unquestionable.

After an earthquake in Naples in 1732, one observer noted that one he, as well as the company he was in and half the town for as far as he could learn, found themselves 'seized with a shaking, just as if we all had the palsy, our teeth chattering in our heads to such a degree that we could hardly speak'.[73] While he noted that the shocks had had 'a great effect upon the nerves', he set out to prove that this were not caused by fright. Most obviously, the main shock of the earthquake had been more 'terrifying' but had not produced the palsy-like effect. Hence, something peculiar in the motion of the aftershock must have triggered the nerves to respond in such a way. Secondly, even those who were not sensible of the earthquake, and could thus not be under any apprehension of danger, found themselves shaking. The third and fourth arguments demonstrated that the earthquake had caused other effects, such as fits and headaches. Here, again, we see an ambiguity towards the concept of nerves and fear in reporting an earthquake. The observer clearly felt that admitting to fright would undermine his credibility. On the other hand, the effects of the earthquake on the nervous system were clearly significant enough observations to be communicated to the royal society. In fact, they formed the most substantial part of the report.

In the earthquake observations, 'fright' appears as a clear medical category. Together with 'surprise', it had a epistemological component that, as we have seen earlier, 'imposes on judgements'.[74] Unlike surprise, fright was also a nervous condition that produced internal effects in those who experienced an earthquake, and most strongly in women. Like with other forms of damage caused by earthquakes demonstrated in tangible objects, these effects were observed, noted down, communicated to the Royal Society, and embedded within a system of natural philosophical knowledge about earthquakes. The epistemological and medical aspects of fright also intersected. Where the medical understanding depended on the identity between *those who were knowers* and *those who were known*, the epistemological aspect of fright suggested that those who were affected were less reliable knowers of the external world. Knowledge of fright was generated by the frightened, but access to this particular knowledge seemed to exclude being a good observer in other respects.

[73] Temple, Phil. Trans., Vol. 41, no. 456, pp. 340-341.
[74] Pigot, Phil. Trans., Vol. 13, no. 151, p. 312.

Moral subjectivity

Earthquake observers did not only become subjects through a medical discourse, but also through a moral discourse. After all, earthquakes remained events of moral significance. In mid eighteenth-century popular expressions, they were still seen as interventions or revelations of God, although this did not prevent observers from making relevant natural observations.[75] And although earthquake theorists maintained strict boundaries between the religious and natural philosophical aspects of their work, they all argued that, in the end, their theories contributed to some higher divine goal: describing the workings of God's creation.[76] 'The original meaning of the word Philosophy was rightly applied to moral wisdom', William Stukeley concluded his distinctly naturalistic treatise *On the Philosophy of Earthquakes.* 'We, who have improv'd both, should join them both together.'[77] As the phrase 'join them both together' suggests, the relationship between natural philosophical and moral wisdom was not strictly hierarchical. Rather, both forms of knowledge complemented and aided each other. The framework described in the previous sections, which joined the external and internal, and the physical and mental effects of earthquakes similarly hints towards a holistic view on the relation between morality and nature. It testifies to the drastic way in which the relation between natural and moral knowledge of earthquakes changed between 1660 and 1760.

Earthquakes can be called moral events because they made moral and immoral behavior visible. Within a religious framework, the earthquake as punishment or warning made visible the behavior which had occasioned such wrath. The location of the earthquake indicated a place of sin. In the aftermath of the earthquake, this place of sin could be transformed into a place of repentance, visibly demonstrated through processions and services, or persist in demonstrating its sinfulness through outburst of public violence in the ruined city. The underlying logic of earthquakes as divine omens subsumed nature to morality, and required observers to see themselves as being in some way implicated in the event. As a result, the response to an earthquake was a measure of one's godliness and willingness to repent. For this reason one observer of the 1692 Port Royal earthquake counted among the 'thing[s] worth observation' that after the violent shakes, when the local minister called for everyone to kneel and pray, even the Jewish population of Port Royal joined the congregation and were even

[75] See for instance the more moralistic treatise on the 1750 earthquake by John Wesley: *The Cause and Cure of Earthquakes*: 'Now, that God is himself the Author, and sin the moral cause, of earthquakes, (whatever the natural cause may be) cannot be denied by any who believe the Scriptures'.
[76] This was an important point for, among others, Hooke, Hales and Stukeley. Vickers, *Defoe and the New Sciences*, p. 29; Hales, *Considerations*, pp. 669-670; Stukeley, *Causes of Earthquakes*, pp. 645-646.
[77] Stukeley, *Philosophy of Earthquakes*, p. 750.

heard by one 'to call upon Jesus Christ'.[78] The (in part likely imagined) human response was motivated and explained by religious sentiment.

As we have seen in the previous chapter, the idea that earthquakes only struck one particular place gradually gave way to a conception of earthquakes as a more extended, and possibly travelling phenomenon. This undermined a basic premise of the religious interpretation of earthquakes: the earthquake no longer indicated one clear place of sin. Yet even in a strictly natural philosophical framework, earthquakes retained a capacity for making morality visible. First of all, through the acts of observing and relating itself. In chapter 2 we discussed the qualities of a good observer, which were classified under the general headers of skill and veracity (or *fides* and *dilligentia)*. Observational skill implied a soundness of mind, as well as an attentiveness to the surrounding world and an interest in the work of the creator. The moral implication of the observer was no longer as a cause of the earthquake, but hidden in the imperative to be aware of the wonders of God. Veracity also indicates a clear judgement on the character of the observer. Knowing nature meant knowing people, and inquiring after an earthquake meant inquiring after who could be a trustworthy observer. In short, it made these moral judgements visible.

Moreover, the previous sections of this chapter have shown that there were considerable moral implications attached to the way an earthquake was experienced both physically and mentally. Distempers to which individuals were prone could be accentuated by an earthquake. Henry Baker noted that the London shocks in February and March 1750 were followed by a mass panic, which affected people of 'all conditions' but especially the common folk.[79] Most left their houses, and many even went out of town, neglecting their duties. This scare was exacerbated by a local 'prophet' who predicted another earthquake would destroy Westminster Abbey come April. When no earthquake came, the false prophet was 'put into a mad-house and treated as a madman, as he ought to have been before'.[80] More directly, an observer of the 1755 Lisbon earthquake noted that 'the fear and consternation was so great, that the most resolute person durst not stay a moment to remove a few stones off the friend he loved most, though many might have been saved by so doing: but nothing was thought of but self-preservation'.[81] Another recounted how everyone had fled and became separated from one another: 'friends of their friends, fathers of their children, husbands of their wives; because every one fled away from their habitations, full of terror, confusion, and distraction.'[82] In 1683 a 'poor laboring man, a mean trasher' put his whole street in uproar after an earthquake because

[78] Anon, *Phil. Trans., Vol. 18, no. 209*, p. 86.
[79] GB 133 Eng MS 19 Henry Baker to Josephus Bruni, *Correspondence of Henry Baker*, Vol. IV, fol. 277r.
[80] Ibidem.
[81] Wolfall, *Phil. Trans., Vol. 49*, pp. 403-404.
[82] Sacchetti, *Phil. Trans., Vol. 49*, p. 410.

of his raving fright.[83] In these cases, the breakdown of order and morality in the aftermath of the earthquake was not explained by a general sinfulness that had occasioned the earthquake in the first place.[84] They were rather caused by the earthquake itself, as a natural effect on the bodies and minds of those who had experienced it.

This shift in interpretation is part of a wider trend. The questions of human sin and human nature were no longer solely a matter of religious authority, but became part of the domain of natural philosophical inquiry. In this new inquiry into human morality, the humors and their effects on conduct played an important role.[85] David Hume's *Treatise Concerning Human Nature*, published in 1739, perfectly captured this spirit. Hume argued that the philosophy of human nature had seen less advancement in the past century than natural philosophy, and believed that this could be remedied by 'introducing the experimental method [of natural philosophy] into moral subjects', as the treatise's subtitle suggests.[86] Moral, for Hume, referred to both human nature in general, as well as to the narrow concept of morality. According to Hume, these types of knowledge are essential for understanding the world: 'as the science of man is the only solid foundation for the other sciences, so the only solid foundation we can give to this science itself must be laid on experience and observation.'[87] The three books of the treatise reflect the general categories of producing subjectivity in natural philosophy. Book one, 'of the understanding', discusses the human faculties in relation to reason and the senses, and corresponds to how the subject makes knowledge of the external world. Book two, 'of the passions', discusses the various passions in relation to the mind and the body, and corresponds to how the subject is affected by the external world. Finally book three, 'of morals', reflects the close ties between subjective knowledge and the senses of morality which are associated with the passions. Earthquake philosophy's interest in the human and moral implications of its subject thus fit within a wider narrative of studying human morality within a natural philosophical framework. Large scale practices of observation (by contemporary standards) of earthquakes provided a strong infrastructure wherein people learned new ways of looking at the world, at themselves, and of being looked at by others.

[83] Anon, *Strange News from Oxfordshire* (London, 1683), p. 2.
[84] See for instance: Pedini, *Phil. Trans., Vol. 42, no. 463*, p. 88.
[85] Cook, *Matters of Exchange*, pp. 397-409. See also: Engelbrecht, *De Onttovering van de Waanzin*, pp. 14-16, 23-28.
[86] Hume, *A Treatise of Human Nature. Being an attempt to introduce the experimental method of reasoning into moral subjects* (1739; digitized print of 1896 edition by ISN primary resources in International Affairs).
[87] Hume, p. 6.

‘My mean low stile’: being an earthquake observer

We have seen how earthquake observers were objectified within medical and moral discourses on earthquakes. Their embodied experiences generated a new area of knowledge which revealed as much about people as about earthquakes. Moreover, through the epistemological implications of the medical notion of ‘fright’, this knowledge also kept in place gendered and classist preconceptions about who counted as a good observer. In this final section, another form of objectification and subject formation within early modern earthquake philosophy is examined. This form concentrates on how the observers came to think of themselves as observers with epistemological and moral responsibilities. Experiencing an earthquake, especially a severe one, could be a life-changing event. Writing and talking about the experience was a way of processing the event, and to make sense of it in relation to oneself. Yet as an earthquake observer, one became aware that there were better and worse ways of writing and talking about the event. How did this formulation of new subjectivities impact those who were involved in creating and being created by them? In other words: what was it like to be an earthquake observer? To answer this question, we must go beyond the printed earthquake relations in the *Philosophical Transactions* and examine the few more personal sources on which these were based. This is not a straightforward matter. As we have seen, most earthquake accounts were drawn up from spoken testimony and were referred to rather imprecisely. Few written letters from lay observers to naturalists remain, if they were ever there. Yet in some rare cases, we have a limited view into how some observers related their experiences to naturalists, and how they discussed themselves in relation to this natural philosophical project. One such case is the earthquake testimony of Henrietta Boston.[88]

Henrietta Boston was intimately connected to several figures we have already encountered. She was the fifth child of Daniel Defoe, and the sister in law of the natural philosopher Henry Baker (who married Defoe’s youngest daughter, Sophia). In 1735 Defoe married John Boston and moved with him to Wimborne, where John Boston was appointed as officer of the excise (a collector of goods and service tax).[89] Here, on 4 May 1749, Henrietta, her young child John, and her sister (likely Hannah Defoe) felt an earthquake. Her account of the event can be found in a letter she sent to Henry Baker, who edited it and published it in the *Philosophical Transactions* in 1750.[90] This letter has an interesting but elusive history. It was one of those

[88] This letter was found among the correspondence of Henry Baker: GB 133 Eng MS 19 Henrietta Boston to Henry Baker, *Correspondence of Henry Baker*, Vol. IV, fol. 276. For a transcription see appendix A, iv.
[89] Maximilian Novak, *Daniel Defoe. Master of fictions* (Oxford University Press, 2003), p. 602; Walter Wilson, *Memoirs of the life and times of Daniel Defoe* (Hurst, Chance, and Co., 1830), pp. 644-645.
[90] The account was printed in the *Philosophical Transactions*, Vol. 46, no. 497, pp. 689-691.

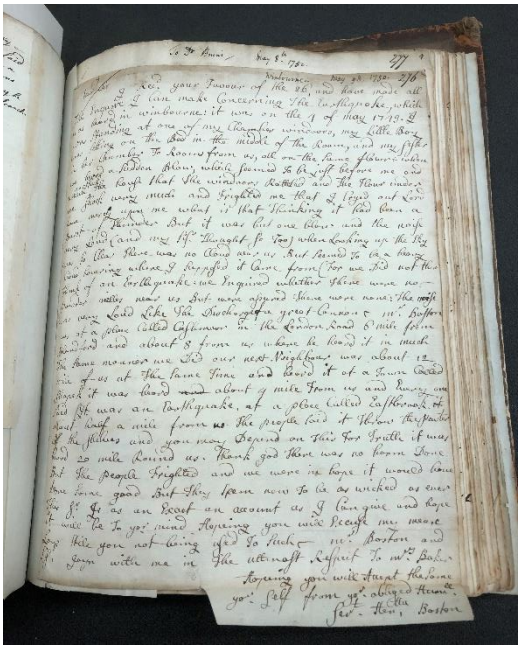

Figure 18. The original letter by Henrietta Boston, 1750. A transcription of this letter is included in appendix A, iv.

testimonies Baker had acquired 'by mere accident', as Henrietta had mentioned the earthquake in an offhand comment in an earlier letter. Baker had responded by 'desiring her to send me the best account she could collect'.[91] Neither Boston's first letter nor Bakers request for more specific information have survived among Bakers correspondence, so we do not know which specific instructions Baker imparted to Boston. What we do know is that Boston promised to have made 'all the enquire I can' and that her account consisted of a familiar mixture of ingredients.

To begin with, there were the epistemological strategies to safeguard the validity of her account: common descriptions (thunder, cannon, a powder mill), places of observations (the upper floor, a bed), and the verification of her observation through those of others (her sister, husband and neighbor, and the inhabitants of Shapwick and Eastbrook). There were both aggregated observations concerning the extent of the earthquake, concluding that 'you may depend on this for truth it was heard 20 mile round us'; as well as a more detailed account of the individual experience of Boston herself. Overall, the account paid close attention to the situation of the various observers: Mr. Boston was in Cashmoor, about eight miles from Wimborne, and their anonymous neighbor was twelve miles away, in Shapwick. Henrietta Boston was 'standing at one of my chamber windows' while her son was 'sitting on the bed in the middle of the room' and her sister was 'in her chamber to rooms from us, all on the same flowr'. Their experiences were clearly modified by, and relevant because of, their different positions. In short, many of the themes treated in this thesis coalesce in this account.

Boston's account was copied relatively faithfully by Henry Baker. Baker inserted one or two comments and paraphrased here and there, but generally took care to deploy the same words that Boston used.[92] Apart from the shift from first person to third person, there are three important alterations between Boston's original letter and the final printed version.[93] These

[91] Baker, Phil. Trans., Vol. 46, no. 497, p. 689.

[92] For instance, Henry Baker noted that Henrietta Boston had 'heard a sudden blow (so she expresses it)'. The parenthesized statement draws attention to the fact that he is purposefully adopting Boston's vocabulary.

[93] The shift from first person to third person is significant in itself. In other several cases, naturalists simply reproduced the original letter without describing it in the third person. It seems that in Boston's case, an authoritative male voice *describing* the woman's account was preferred over her own testimony in the first person.

alterations concern the evaluation of Boston's state of fright, and two omitted sentences in which Boston drew explicit connections between the experience of the earthquake, its moral implications, and her experience as an observer. Baker was clearly not interested in the moral implications of the earthquake, although they were considered relevant by Boston. He was also likely not keen to include Boston's evaluation of her own observational qualities because it might jeopardize her credibility as an observer, and hence the usefulness of her account. It is likely that, for similar reasons, similar remarks in other letters and conversations were also not included in the final reports printed in the *Philosophical Transactions*. Hence it is worthwhile to examine these remarks, short as they are, in more detail.

First, the fright. In her letter, Boston wrote that as the window suddenly began to rattle and the floor was shaking beneath her, she became frighted and cried out: 'Lord have mercy upon me, what is that'. Interestingly, her statement captured the pathological as well as the moral and epistemological connotations of fright. In Baker's rewrite of the story, the focus lies more strongly on the pathological element, through the addition of one small phrase. He writes that Boston was 'frighted *to such a degree*, that she cried out […]'. The words 'to such a degree' do not simply lend an air of rhetorical credibility to the statement. They place the statement in a presumed metric of levels of fright: more frightened than a simple shock, but not so much as to cause a lasting physical effect such as sickness or shaking. This minor differences testifies to the different way of looking at one particular experience between the observer and the naturalist. Where Boston is working through her own surprise and fright as a lived experience, to Baker the statement is only important as a (somewhat unspecific) technical indication.

Both Boston and Baker mention fright twice. The first mention, as we have seen, relates only to Boston's personal experience of the fright. The second mention refers to the fact that the earthquake did not do any harm except for frighten an unspecified number of people. In both Boston's and Baker's version, this second statement establishes that the fright did not affect everyone universally, and that the fright was considered as a form of 'damage' produced by the earthquake. Omitted from the printed version, however, is the continuation of Boston's remark that 'there was no harm done but the people frighted, *and we were in hope it would done some good but they seem now to be as wicked as ever*.' The 'seem to be' establishes the phrase as an observation, and the statement as a whole reflects the presumed connection between the natural event and the moral effects. The fright would hopefully have produced a positive moral effect but failed to do so. It is not hard to explain why Henrietta Boston attributed more weight to this observation than Henry Baker did. She and her husband were rational dissenters, deeply Christian though unaffiliated with the Anglican church, who sought to understand God and religion through rational and (natural) philosophical means. As such, Boston was clearly interested in the effects of nature on morality. In this statement on the

effects of fright, however, Boston places herself beyond the subjects she is observing: they remain wicked whereas she is not.

Boston's reflection on her own position within the practice of observing is also found in a short sentence that was not included in the printed version of the account. Before closing the letter, she notes that: 'this h[ere] is as an exact an account as I can give and hope it will be to your mind hopeing you will excuse my mean low stile you not being used to such.' Given that Boston was likely answering specific queries from Baker, or at least undertaking her investigation of the effects of the earthquake in nearby places under his instruction, Boston was aware of the specific power relation between her and Baker. She noted some defects in her own account (both she and her sister had mistaken the earthquake for a burst of thunder and an exploding gunpowder mill) and the epistemologically problematic state of fright she had been during the observation. The statement reflects that Boston, without ever having experienced an earthquake, was aware of a particular style of observing and relating such observations. This information likely reached her in the same ways that it had reached her father Daniel Defoe earlier, who had incorporated them into his narrative of *Robinson Crusoe*.

Why was this phrase left out of the version communicated by Henry Baker to the Royal Society? After all, such a reflection on the account's credibility would have been relevant information. The most likely answer is that Henry Baker tried to present the account as convincing as possible. And in a convincing account of a lay observer, there could be no room for doubt. In the context of the naturalist's theoretical knowledge, doubt was an epistemic *virtue*: it demonstrates a degree of critical reflection and a willingness to defy dogmatism. In the context of experiential knowledge however, doubt was an epistemic *vice*: it demonstrates the unreliableness of the observation, and because the moment of observation has already passed, the uncertainty can never be settled.

In a similar vein, the account of a Javanese nobleman drawn up in the first person for the Dutch colonial authorities in Batavia considered the possibility that the account might not have been complete or entirely accurate. The writer, the Tommagon Porbonata, implored the audience to notify him if they should find any mistakes in his reporting.[94] The account went through a number of iterations, taken up in letters sent from the colonial administrations to the VOC headquarters, from whence the Amsterdam secretary Jonas Witsen communicated the report to the Royal Society. When the report appeared in the *Philosophical Transactions*, it had followed a rewritten version in the third person, rather than the original in the first person, and all references to ambiguity had been left out.[95] Hans Sloane advertised the account by

[94] 'Belief te duyden wanneer in dit mijn geschrift iets mogt sijn dat niet wel is'. Nationaal Archief. *1.04.*02 (Verenigde *Oostindische Compagnie)*, 1626 (Veertiende Boek, Batavia's ingekomen brievenboek, deel V), katern Bantam 1, pp. 24-25.
[95] Porbonata, *Phil. Trans., Vol. 22, no. 264*, pp. 595-598.

stating that 'the exactness of it, and the extraordinary accident it related, made it extreamly welcome to all people of understanding'.[96] Because of the epistemologically problematic status of eye-witnesses, their accounts were often presented as impeccable: if it was decided that a witness was credible (or should be credible), their flaws were diminished in the printed report.

Yet while such flaws were somewhat covered up in the printed accounts, it emerges from the few remaining epistolary sources that observers were keenly aware they were being held to standards that were largely out of their control. They drive home the point that observers had the moral duty to be exact and reliable to a degree which was practically impossible to achieve.[97] Henrietta Boston clearly struggled with this imperative of being a 'good observer' and excused herself for it, exposing the moral and social implications of being asked 1) to know what being a good observer means; 2) to be a good observer; 3) to have a reliable memory and style of communication; and 4) to judge the credibility of other's experiences. In search of an epistemologically convincing earthquake philosophy, naturalists came to foster normative expectations about what the correct way of experiencing an earthquake was. This was not a spiritual or personal experience, but a way of experiencing that should be communicable, clear and true according to natural philosophical terms. In the interactions between naturalists and observers, the latter came to understand themselves according to the terms of this discourse, as either lacking, or essentially inadequate subjects of observation. If this was a kind of self-understanding fostered by being an earthquake observer, it was very much an understanding from the point of view of the earthquake philosopher who tried to maintain his epistemological and social superiority over the observing subjects on which he was reliant. As observers came to understand themselves as essentially lacking in these aspects, they acquired a sense of self in relation to the project of earthquake philosophy that was marked by their status as a lay person: they became a lay subject.

Conclusion

This chapter focused on the ways in which earthquake observers did not only generate new knowledge about earthquakes, but also about themselves. This question was not tangential to early earthquake philosophy, but encompasses all the theoretical, epistemological and social elements that shaped the study of earthquakes in the seventeenth and eighteenth centuries. Observers were seen as a confluence between experience-based knowledge and object-based

[96] Royal Society Archives: LBO/13/05 , Copy letter from Hans Sloane, London, to Mr J. Witsen.
[97] On moral and epistemic values and their connection to social epistemology, see: Fuller, 'Social Epistemology', pp. 6-7.

knowledge, and they were objectified in a pathological discourse that examined the effects of earthquakes on their bodies and minds. They were thus turned into observing subjects. This new knowledge was interesting from a natural philosophical point of view, but was also closely tied to discourses about the credibility of observers and their social position. Through the process of subjectification, observers came to understand themselves according to terms which we would now define as a ‘lay discourse’. In reconciling epistemological tensions between experiential knowledge and object-knowledge, and in generating new philosophical insights, the social configuration of expert naturalist and lay observers that formed the basis of later seismology was conjured into existence.

Conclusion

In my thesis I have examined the changing relations between earthquake observers and natural philosophers in the period preceding the rise of modern seismology. Instead of taking the 1755 Lisbon earthquake as the prime catalyst for this field to emerge, I examined the preconditions for this enterprise: how did earthquake theories and methods develop to the point where seismological investigation made sense, and how did the earthquake scientist come to look down over a mass of observers? I have argued that the main theoretical and methodological assumptions of early seismology developed in tandem and gave rise to a number of tensions related to theory (what causes earthquakes?), epistemology (how can we have true knowledge of these events?) and social relations (who counts as a credible observer?). I analyzed the development of early seismology and the emergence of expertise as driven by the various attempts to stabilize these tensions.

First, I considered the rise of the earthquake observer as the result of theoretical and epistemological tensions. Questioning the Aristotelian model of earthquakes led to a wide field of competing theories, which posited the causes of earthquakes as either subterranean, meteorological or some combination of the two. Attempts to settle this debate instigated a search for empirical evidence. Lacking what one naturalist termed ‘subterranean telescopes’, earthquake philosophers needed other ways to make the earthquakes knowable. I have analyzed two different modes of doing so. In the first mode, it was assumed that earthquakes could be studied by examining the qualities of objects related to them, such as pyrites and fossils. The applications of this analysis proved to be limited however. The second mode of analysis relied on the experience of earthquakes. At first, this information was derived from historical accounts, but these were soon overtaken by contemporary observations, which were both more numerous and more directed towards contemporary questions. Such experiences could not be predicted nor replicated in experimental settings, meaning that naturalists often had no personal access to them and instead relied on testimonies from eye-witnesses.

These eye-witnesses often included people who, due to preconceptions about their gender, race or class were frequently denied the status of credible observers. The second chapter has investigated these epistemic and social tensions produced by their inclusion in the natural philosophy of earthquakes. Ethnicity seems to have had the largest impact, seeing that few observations made by non-European observers were considered for communication in a natural philosophical context. Partially resulting from this, English earthquakes were overly represented in the earthquake observations, even if the country experienced fewer and less intense earthquakes than other parts of the world. These events were more ambiguous, and hence necessitated verification from multiple and various sources in different situations. Increasingly, this plurality included new spaces and actors: servants in the garret, milkmaids in the fields, workers in the mines, etc. I used these cases to investigate how authoritative knowledge was constructed according to the metric explained in the introduction. Preconceptions about *social position*, *character, intelligence* and *observational skill* were used by naturalists to distinguish between credible and incredible observers. These ideas were also challenged, chiefly through emphasizing experience and employing recognizable language to demonstrate their *communicative ability* and indicate some awareness of *theoretical knowledge*. In short, this chapter studied the construction of credibility: the prerequisite for natural philosophical knowledge.

In chapter three I investigated how the contributions of credible earthquake observers were translated into natural philosophy. This chapter focused most concretely on two aspects: first, how natural philosophical theories changed as the result of observations. I argued that observations regarding the place, time and intensity of the earthquakes transformed the image of earthquakes as locally specific phenomena to traveling phenomena, and gave rise to the idea of an (epi)center. These insights were used in the ongoing debate over the causes of earthquakes. Most observations tended to focus on the supposed meteorological side of earthquakes, because these were the most visible aspects. As a result it became increasingly clear that these theoretical assumptions could not be verified. At the same time, the testimonies of mine workers were the first accounts to provide experiential evidence for the subterranean nature of earthquakes not as underground explosions but as shocks of the earth itself. Secondly, this chapter focused on the construction of expertise through two processes of translation. In the first, naturalists became experts because only they could lay claim to knowledge that came from a top-down view over all the various observations. This aggregated knowledge came to be an essential prerequisite for turning observation into natural philosophy. In a similar move of constructing top-down relations, naturalists reserved the right to framing and interpreting the language and circumstances of eye-witnesses. In their reports, they performed the ‘interrogation’ of their witnesses, language which

reflected that the experience of observers was the raw material, and the theoretical knowledge of naturalists the expert procedure, out of which natural philosophy was created.

Finally, I discussed the coalescence of all these tensions in a case study of one particular body of knowledge: the human pathology of earthquakes. This case study examined how making knowledge of earthquakes went hand in hand with constructing knowledge of lay observers, using the Foucauldian subject as a central analytic concept. Through a growing medical discourse on the effects of earthquakes on humans, observers were themselves turned into objects of knowledge. This objectification was a response to the epistemological tensions about the reliability of subjective experiences, and replicated social inequalities in the model of knowledge production. Simultaneously, observers were encouraged to reflect on the moral implications of experiencing an earthquake. These interpretations relied on the religious connotations of earthquakes, but increasingly also on the medical relations between fright, the nerves and the passions. The observers were also invited to reflect on the moral responsibilities of observing truthfully, and their inadequacy to do so. While the credibility of observers was generally stressed in reports, the few available sources which can tell us something about the construction of these reports reveal that behind the scenes the discourse was dominated by the image of a lacking subject, who did not (and perhaps never could) live up to the ideal standards set by naturalists.

The central question of this thesis concerned the changing relations between lay observers and earthquake philosophers, and how this shaped the development of earthquake philosophy in the Royal Society between 1665 and 1755. I have shown how the inclusion of eye-witnesses created highly productive tensions by challenging old theories (the meteorological origins), proposing new paradigms (the traveling earthquake), and opening up new fields of knowledge (the pathology of earthquakes). This inclusion also constituted a challenge to social relations. At the same time I have demonstrated how equally productive attempts at stabilizing these tensions led to a revaluation of forms of knowledge (experiential and theoretical knowledge), ways of producing knowledge (techniques of verification and translation) and opening up more areas of knowledge (the moral philosophy of earthquakes and observation). Modern seismology did not arise out of nowhere in the late eighteenth century within a fixed constellation of expert-lay relations. It was rather the large-scale application of the theoretical, methodological and social principles that had gestated during the preceding century.

Although much has been explored in this thesis, equally many aspects have been left out. For one, I have been at pains to establish the uniqueness of earthquakes as epistemologically problematic events. While this focus has certainly helped to answer several specific questions about the practices of earthquake philosophy, it also means that I have refrained from analyzing many connected or epistemologically similar phenomena, such as volcanoes, tsunamis, and (following early modern classifications) lightning strikes and heavy storms. All of these phenomena were intensively studied and observed as well, though less systematically featured in the *Philosophical Transactions*.[1] If we want to examine the development of observation and lay-expert relations in other fields, much of the analysis concerning social relations and epistemological assumptions presented here will doubtless carry over. But if we also take seriously the idea that the scientific theories behind the phenomena were an equally determining part of this triangle of elements, some comparative analysis would be interesting. Earthquake philosophy and its related practices of observation were marked by a certain theoretical openness: everyone was in the dark about their true causes, and all but the staunchest proponents of one theory or another recognized this fact. In contrast we could look at storms or lightning, where there was a greater deal of scientific consensus, and see how this change impacted the development of expert-lay relations there.

Additionally, the focus on early modern earthquake philosophy has been limited to the rather Anglocentric discourse of the early Royal Society. Further study on this topic might want to investigate similar developments within institutions such as the *Académie Royale des Sciences* (from 1666) and the various scientific societies of early modern Italy, the seismic hotbed of Europe. More generally, giving the higher frequency and intensity of earthquakes in the Americas and east Asia (and the well-studied early modern interpretations of these events), the original setup of this project had anticipated an analysis of the connections between early modern earthquake science and colonialism. Instead, a surprising lack of accounts from non-European sources informed the central question of chapter 2: *how was credibility constructed in the context of minor English earthquakes, where it was not always clear whether they had been earthquakes at all?* In the larger story of credibility presented in this thesis, ethnicity thus played a paradoxical role. On the one hand it seems to have been by far the greatest barrier to being considered a credible observer by English naturalists. On the other hand, this fact meant that there was very little material through which to analyze the developments of these specific social relations in the context of discourses of expertise, like has been done somewhat more elaborately with regard to class and

[1] The comparatively elusive and ephemeral event of a thunder-storm, for instance, only has about a fifth of the entries of earthquakes in the same period 1665-1755. See: Paul Henry Maty- *A General Index to the Philosophical Transactions from the first to the end of the seventienth volume* (London, 1783), pp. 148-154, 466-468.

gender relations. Such an analysis would be a worthwhile one however, and could possibly be based on colonial archives rather than those of European scientific institutions.

In spite of these lacunae, I have been able to make a valuable contribution to the growing field of the 'history of knowledge'. This approach to the history of science is committed to placing scientific knowledge, epistemology and practices in a wider cultural context. Yet this is easier said than done. While recent decades have seen many publications that have elucidated the social position of early modern scientists themselves, and have stressed the importance of actors and sites not traditionally found in the history of science, the interactions between these domains e has remained largely elusive. A major problem is of course a lack of source material. It is difficult to trace often unaccredited contributions. For early modern participation in scientific projects, historians have most often looked at practices of collecting in botanical sciences, or to practices of (self)-diagnosis in medicine. I have shown that outside of these fields, too, a history of knowledge approach that takes into account cultural and social conditions as well as epistemological and scientific principles is possible, and yields highly useful insights. The earthquake reports of the *Royal Society* reveal some remarkable interactions between observers and naturalists, interactions that shine a new light on the ways in which the epistemic value of observation and various scientific theories were shaped by their social context. Credibility and truth were negotiated at many different levels, between various observers, intermediaries, local naturalists and members of the royal society. Earthquake reports moreover provide a link between the global correspondence network of the *Royal Society* and the local day-to-day networks of its members.

By studying these interactions, I have been able to show how the natural philosophy of earthquakes was only one specific form of knowledge that was built in the process of observing and communicating earthquakes. The job of the expert naturalist became to transform all kinds of different bodies of knowledge into a natural philosophy. These different knowledges included medical knowledge, social knowledge, moral knowledge, practical knowledge and everyday knowledge, and generally transcend modern disciplinary classifications. The rules of what counted as natural historical or natural philosophical knowledge shifted over time and also depended on social context - take for instance the rise and fall of observation as a epistemologically privileged form in earthquake studies. What I have tried to outline in this thesis is a general approach to analyzing such developments. The elements of epistemology, scientific theory and social relations should not be analyzed as (respectively) the foundational assumptions, the core stuff and the non-essential by-products of scientific inquiry. This model neither serves to explain the development of new scientific insights, nor to describe the impact of the scientific project on these fields. I argue

for an admittedly messier yet insightful approach: an analysis of the tensions between each of these components at every stage of the research process. I hope that even those who might disagree with the specific arguments brought forth in this thesis can find something useful in this general approach.

Lay observers made important contributions to the natural philosophical study of earthquakes in the seventeenth and eighteenth centuries. Their accounts provided the empirical basis that was necessary to pass judgement on a growing number of hypotheses and theories. Yet their role was not only passive: observers themselves suggested new phenomena to observe and factors to consider, based on their experiential knowledge. At the same time, their impact on early earthquake studies was also much wider than the mere contribution of data. The inclusion of lay observations considerably challenged existing methodologies and epistemological assumptions, forming the basis for the mid-eighteenth century rise of seismology. This field was predicated on theoretical hypotheses that had been formulated with the help of earthquake observers. It also depended on a specific hierarchic relation between seismologists and observers in order to assess the credibility and usefulness of observations. This particular relation, which has been our lens to examine early modern earthquake studies, also developed in the period 1665-1755. Like Robert Boyle's apprentice in the introduction, seventeenth and eighteenth- century earthquake observers were becoming 'diligent observers of natural things', challenging early modern social epistemologies. But this form of observation soon lost the epistemologically privileged status it had enjoyed in the seventeenth century, and came to be considered the simple 'groundwork' from which seismologists created natural philosophical truths. This process not only shaped early seismology, but also our historical understanding of it. While the accounts of earthquake observers were indispensable for the development of earthquake philosophy, their contributions have been nearly forgotten. Bringing these forgotten contributions back into the spotlight has been a highly rewarding endeavor.

Appendices

A.

Source Transcriptions

I.

The testimony of Elizabeth Cornwallis 1750.

Burrow, *Phil. Trans., Vol. 46, no. 497*, p. 702-705.

Dear Sir,

This Morning I have been making a visit at Lord Cornwallis's at Culford; which, I suppose, I need not tell you, is about four Miles from Bury in Suffolk. Lady Cornwallis (whose judgment and accuracy are superior to all doubt or exception, and her veracity still more so), assured me, that on Sunday last, about one o'clock, as she was sitting and reading in her dressing-room at Culford, she suddenly felt and saw her chair and person move backwards and forwards; so that she reached and examined whether any dog had got under her feet and chair, or any one entered her chamber unperceived; but found herself absolutely alone in the room: Whereupon she tried, whether, by laying her hand or elbow upon the table, she could repeat the same motion, or any thing like it; but could not. She added, that she felt herself a good deal surprised at this extraordinary sensation, at the instant of perceiving it: But neither then, not afterwards, had the least imagination about an earthquake; till, upon coming down to dinner, she was asked by miss Charlotte Cornwallis, her second daughter, a yonng [sic.] lady grown up, 'whether she had not felt the earthquake?" Miss charlotte agreed to the time; and was herself also sitting and reading in her own dressing-room, which was one pair of stairs higher than her ladyship's, yet on the same side of the house. However, it was also felt by miss Charlotte Cornwallis's maid-servant, whose chamber was in a different part of the house, and distant from either of the ladies apartments; and who was so alarmed at it, as to leave her room, and come into her young lady's, to see what was the matter. No one else in the house perceived it. But lady Cornwallis says, that, as far as she can learn, they were all upon their feet; none being sitting, except the three already mentioned. The house stands

alone in the park: And Lady Cornwallis had declined making any inquiry amongst the inhabitants of the adjacent village; partly, for fear of alarming them with apprehensions of danger, of which they would be very susceptible from the name of an earthquake; and partly from the little hopes she could have of procuring any tolerably accurate account of the fact from such reporters.

As you have been so careful and exact in collecting the several histories of those various shocks, which have been felt in diverse parts of the kingdom within these nine months last past, it will not be unacceptable, I hope, to furnish you with this supplement to them; especially as none of the former have (as far as I remember) affected these parts.

P.S.

On our return hither to Mr. Wollaston's, we found a letter from a worthy friend of Mr. Wollaston's and mine, Mr. Metcalfe, a clergyman of reputation, sense, and fortune; who resides at Leicester, and has two Livings near that place; one at Narborough, the other at Tilton: out of which I will transcribe a paragraph, which will serve to confirm lady Cornwallis's Relation. [….] Since the receipt of the above letter, I have read, in the public news-papers, an account of its having been also felt at Northampton about the same time.So that no doubt can remain of the shock which lady Cornwallis perceived at Culford, having been a real earthquake.

II.

The testimony of J. Nantcarrow, 1755.

Borlase, *Phil. Trans., Vol. 50,* pp. 503-505.

At Huel-rith mine, near Godolphin, the noise was seemingly underneath. I felt (says the director of the mine) the earth move under me with a prodigious swift, and apparently horizontal tremor: its continuance was but for a few seconds of time, not like thunder, but rather a dull rumbling even sound, like deads running under ground. In the smith's shop the window-leaves shook, and the slating of the house cracked. The whim-house shook so terribly, that a man there at work ran out of it, concluding it to be falling. Several persons then in the mine, working 60 fathom deep, thought they found the earth about them to move, and heard an uncommon noise: some heard the nosie, and felt no tremor; others, working in a mine adjoining called Huel-braeg, were so frightened, that they called to their companions above to be drawn up from the bottoms. Their moor-house was shaken, and the padlock of their candle-chest was heard to strike against the staples. To shew, that this noise proceeded from below, and not from any concussion the atmosphere above, this very intelligent captain

of the mine (Mr. J. Nantcarrow) observes, from his own experience, that thunder was never known to affect the air at 60 fathoms deep, even in a single shaft pierced into the hardest stone; much less could it continue the sound thro' such workings as there are in this mine, impeded in all parts with deads, great quantities of timer, various noises, such as the rattling of chains, friction of wheels and ropes, and dashing of waters; all which must contribute to break the vibrations of the air as they descend: and I intirely agree with this gentleman's conclusion, that thunder, or any other noises from above in the atmosphere, could not be heard at half the depth of this mine. This therefore could be no other than a real tremor of the earth, attended with a noise, owing to a current of air and vapour proceeding upwards from the earth.

III.

The testimonies of William Hallom and John Howson.

Bullock, *Phil. Trans., Vol. 49*, pp. 400-402.

William Hallom and Jo. Howson, miners, say, That at the aforesaid time they were employed in carting, or drawing along the drifts the ore and other minerals to be raised up the shafts. The drift, wherein they were working, is about 60 fathoms, or 120 yards deep, and the space of it from one end to the other 50 yards, or upwards. Hallom was the end of the drift, ahd just loaded his cart, and was drawing it along, but was suddenly surprised by a shock, which so terrified him, that he immediately quitted his employment, and ran to the west end of the drift to his partner, who was not less terrified than myself. They durst not attempt to climb the shaft, lest that should be running in upon them, but consulted what means to take for their safety. Whilst they were thinking of some place of refuge, they were alarmed by a shock much more violent than the former; which put them in such a consternation, that they both ran precipitately to the other end of the dirt. There was a miner working at the forfield, or east end of the vein, about six fathoms below their level, who called out to them, imagining they were in danger of being killed by the shafts running in upon them., which he supposed was the case; and told them, if by any means they could get down the shaft to him, they would be more secure, because the cavity, where he was working, was encompassed with solid rock. They went down the shaft to him, where, after observing they had neither of them received any misfortune, he told them, that the violence of the second shock was so great, that it caused the rocks to grind one upon another. His narration was interrupted by a third shock; and, after an interval of about four or five minutes, was succeeded by a fourth; and about the same space of time after, by a fifth; none of which were so violent as the second.

They heard after every shock a loud rumbling in the bowels of the earth, which continued for about half a minute, gradually decreasing, or appearing at a greater distance. They imagined, that they whole space of time, from the first shock to the last, was about twenty minutes; and they tarried about ten minutes in the mine after the last shock; when they thought it adviseable to examine the passages, and to get out of the mine, if possible. As they went along the drifts, they observed, that several pieces of minerals were dropped from the sides and roof, but all the shafts remained intire, without the least discomposure.

III.

The testimony of Henrietta Defoe-Boston, 1750.

GB 133 Eng MS 19, Correspondence of Henry Baker, Vol. IV, fol. 276.

Winbourne may 5th 1750.

I rec[eive]d your favour of the 26, and have made all the enquire I can make concerning the earthquake, which was heard in winbourne: it was on the 4 of may 1749. I was standing at one of my chamber windows, my little boy was sitting on the bed in the middle of the room, and my sister in her chamber to rooms from us, all on the same flowr: when we heard a sudden blow, which seemed to be just before me and which shock the house that the windows rattled and the flowr under me shock very much and frighted me that I cryed out Lord have mercy upon me what is that thinking it had been burst of thunder but it was but one blow and the noise very loud (and my sis[ter] thought so too) when looking up the sky was so clear there was no cloud near us but seemed to be a heavy cloud hovering where I supposed it came from (for we did not think of an earthquake: we enquired whether there were no powder milles near us but were assured there were none: the noise was very loud like the discharge of a great Cannon. Mr. Boston was at a place called Cashmoor in the London Road 6 mile from Blandford and about 8 from us where he heard in much the same manner we did our next neighbor was about 12 mile of us at the same time and heard it at a town called Shapeck it was heard about 4 mile from us and every one said it was an earthquake, at a place called Eastbrook it about half a mile from us the people said it throw the pewter of the shelves and you may depend on this for truth it was heard 20 mile round us. Thank god there was no harm done but the people frighted and we were in hope it would done some good but they seem now to be as wicked as ever. This H[ere] is as an exact an account as I can give and hope it will be to your mind hopeing you will excuse my mean low stile you not being used to such. Mr. Boston and Sis[ter] joyn with me in the utmost respect to Mr. Baker hopeing you will accept the same your self from your obliged humble sister Henrietta Boston.

B.

Primary sources

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