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A history of British seismology

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Abstract The work of John Milne, the centenary of whose death is marked in 2013, has had a large impact in the development in global seismology. On his return from Japan to England in 1895, he established for the first time a global earthquake recording network, centred on his observatory at Shide, Isle of Wight. His composite bulletins, the "Shide Circulars" developed, in the twentieth century, into the world earthquake bulletins of the International Seismological Summary and eventually the International Seismological Centre, which continues to publish the definitive earthquake parameters of world earthquakes on a monthly basis. In fact, seismology has a long tradition in Britain, stretching back to early investigations by members of the Royal Society after 1660. Investigations in Scotland in the early 1840s led to a number of firsts, including the first network of instruments, the first seismic bulletin, and indeed, the first use of the word "seismometer", from which words like "seismology" are a back-formation. This paper will present a chronological survey of the development of seismology in the British Isles, from the first written observations of local earthquakes in the seventh century, and the first theoretical writing on earthquakes in the twelfth century, up to the monitoring of earthquakes in Britain in the present day.

Keywords History of seismology · British Isles · John Milne · Robert Mallet · Seismic monitoring · Macroseismology

1 Preface

In choosing this title for the 14th Mallet-Milne lecture, I am of course calling to mind Charles Davison's "History of British Earthquakes" (Davison 1924), which for many years was the

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standard text on the seismicity of the UK. However, all three words of the title come with questions attached, and dealing with them at the outset will serve to outline the scope of this monograph.

History, according to the humorists Sellar and Yeatman, is what you can remember. I have suggested before now that all earthquakes are historical. By the time a seismologist hears about one, it's already over. A volcanologist hearing that Vesuvius is erupting has plenty of time to pack a bag, head for the airport, arrive in Naples and find the eruption is still continuing. An earthquake is over in a minute or two at most, and the best the seismologist can do is visit the ruins and perhaps record the aftershocks. The main event is already in the past.

For most people, though, it would be stretching definitions to describe yesterday's earthquakes as historical events. For the man in the street, history is something that happened a long time ago. It is the bane of archivists everywhere that while anyone who discovers a letter dated 1762 will immediately recognise it as being of historical interest, a letter dated 1962 will probably be consigned to the bin. This is a significant contributor to the poor survival of documents from all periods—they get discarded before they become sufficiently old to be considered interesting as historical documents. Only those of financial value: wills, title deeds, etc, survive.

So is there a date at which history ends and current affairs begin? In seismology there often is. It has been the practice in the UK to treat the start of modern instrumental monitoring as the end of history. Thus all British earthquakes before 1970 are classed as historical, those after as modern—although many twentieth century British earthquakes before 1970 are reasonably well instrumentally recorded. Similar distinctions probably exist in other countries, with different pivotal dates.

I will follow the same distinction in this study, but I will carry the history up to the present day, although the main focus will be on the period before John Milne's death in 1913, the centenary of which this lecture commemorates.

The word "British" has other problems. The distinction between England, Britain, Great Britain, the United Kingdom, and the British Isles is a constant source of difficulty to many, and not just to those who live in none of those divisions. Here I intend to use the term "British" in its widest geographical (and not political) sense, to encompass the whole British Isles. I am not one who sees the use of this term as loaded (and I speak as a citizen of the Irish Republic), and the alternative, "Atlantic Isles", would seem to include at least the Faeroes, if not Iceland and the Canaries as well.

Furthermore, for most of the period under discussion, Ireland and Great Britain were part of the same political entity, and to try and exclude Ireland would mean writing Robert Mallet out of the Mallet-Milne lecture, which would hardly do.

Seismology is the study of earthquakes, but one can differentiate between theoretical and practical seismology, by which I mean on the one hand, the attempt to understand what earthquakes are and why and how they occur, and on the other hand, the attempt to monitor and measure them. With respect to the present monograph, I focus to a large extent on observational seismology, and in particular, the observation of British earthquakes. Before the twentieth century, earthquake monitoring was restricted for the most part to documenting earthquakes through their felt effects, not always scientifically. Up to 1900, I will consider developments in theoretical seismology in Britain; over this period, the nature and cause of earthquakes was very much in doubt, and it is interesting to trace the evolution of thinking about them; it makes sense to write about what individual scientists thought about earthquake phenomena.

After the end of the nineteenth century, I will concentrate almost exclusively on the development of seismology as earthquake monitoring. From the twentieth century on, the development of scientific thinking about earthquakes has been such an international enterprise that it makes no sense to try and distinguish a purely “British seismology”. Regrettably, this means passing over much of the seismological research undertaken by universities in Britain in the twentieth century, which could occupy several chapters. As a result, some important figures like Sir Harold Jeffreys (1891–1989) are largely passed over. Similarly, the history of explosion monitoring is regrettably omitted.

One could question whether it was ever possible to isolate British theoretical seismology from that elsewhere. The earliest writers on earthquakes in Britain took their cue from classical authors such as Aristotle and Seneca. In the seventeenth century, the Belgian writer Fromondus (Libert Froidmont 1587–1653) was influential. The earthquake committee of the British Association for the Advancement of Science in the 1840s was informed about investigations in Italy. Robert Mallet was a friend of Alexis Perrey in France, and John Milne was influenced by Ernst von Rebeur-Paschwitz in Germany.

It is doubtful if it is possible to be so complete as to mention everyone who has ever written about earthquakes in Britain, so the account that follows is necessarily partial. I have left out, for instance, authors of short papers on earthquakes of a particular county, such as Edward Parfitt’s (1820–1893) account of earthquakes in Devon (Parfitt 1884); Parfitt was the Librarian of the Devon and Exeter Institute. Some writers of now forgotten theoretical pamphlets are also passed over; I have given more weight to those who actually were involved in investigations of British earthquakes. I have passed over rather briefly the work of British seismologists overseas, apart from brief notes on the work done by Milne and his contemporaries in Japan. Regrettably, this means passing over the work of Richard Dixon Oldham (1858–1936), the first Director of the Geological Survey of India, and the discoverer of the earth’s core.

Also, although this monograph is structured largely by devoting sections to particular individuals, there was in reality much overlap—as will be apparent.

2 Earliest British writings on earthquakes: to the end of Aristotelianism

The story of writings on earthquakes in Britain starts, perhaps surprisingly, in Iona (Fig. 1). This small island off the west of Scotland is today only reachable by two ferry crossings; a car ferry to Mull and a passenger-only ferry to Iona itself. But as Professor William Kirk, an inspirational figure to Irish geographers, used to remind undergraduates, ideas of centrality depend on where you put the spokes. Ireland and the west of Scotland may seem today like the extreme periphery of Europe, but there was a period when they were the intellectual hub of Europe, a centre of learning, and the base from which Christian missionaries were dispatched to turn back the tide of paganism across the continent (see, for instance, MacCulloch 2010).

2.1 Irish annals

The earliest written mention of earthquakes in the British Isles are notes of their occurrence in monastic annals, and the earliest of these refer to earthquakes in Ireland. These are mentioned in Irish annals, but the relationship between the early chronicles is complex and difficult to untangle. Early chroniclers copied from each other, sometimes badly, and some of the chronicles in the chain of transmission are now lost. Understanding the relationship between them therefore requires a good deal of detective work (Dumville 1984).

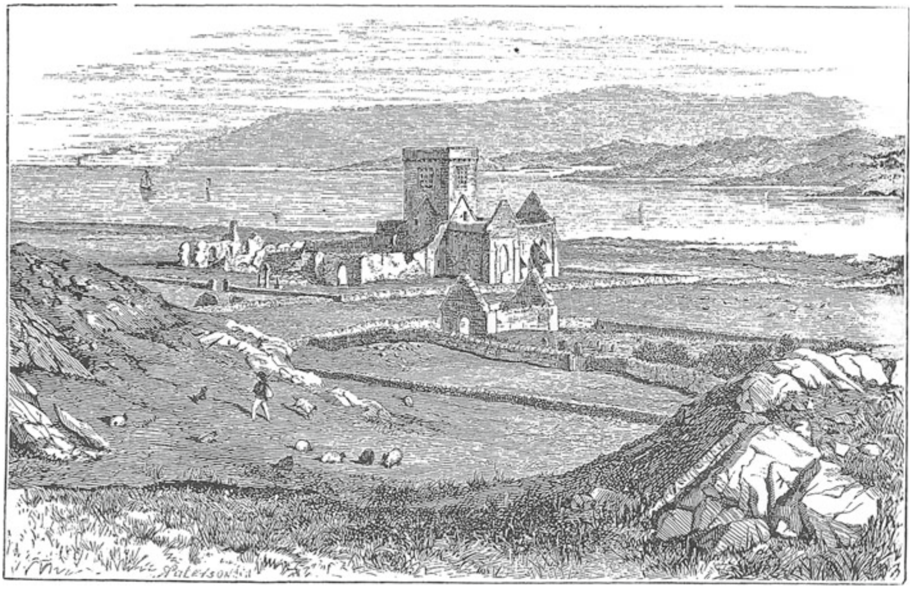


Fig. 1 Ruins of Iona Abbey—site of the first written records of earthquakes in the British Isles (Author's image)

All the extant Irish annals are believed to be continuations of the same original chronicle, which was compiled in Iona some time in the 6th century by St Columba, based on an earlier annalistic world history written by Rufinus of Aquileia at the beginning of the 5th century (McCarthy 2008). This chronicle continued to be added to by successive abbots of Iona until around 740, when the text was brought to Ireland and formed the basis of the various Irish chronicles.

The earliest contender for the title of "first recorded British earthquake" is an entry in the Annals of Ulster (Balé and Purcell 2003 is the most recent edition) for 601—"An earthquake in Bairche." Bairche (or Ui Bairrche) was a kingdom in the south-east of what is now County Laois. There is a general problem with all events of this type, that the word used for earthquake, "terra motus", was also used for landslides, landslips, rockfalls, bog-bursts and other similar phenomena. The fact that Ireland is so aseismic makes one particularly inclined to suspect that something other than a real earthquake may be being described. Principia (1982), using a Victorian edition of the Annals of Ulster, give the location as Mourne (County Down) and the year as 600.

Claims for an Irish earthquake in 448 (e.g. Cusack 1868) rest on a misinterpretation of a note in the Annals of Ulster referring to an earthquake in Constantinople.

Further events are noted in 664, 680, 684, December 707 (two events), October 721, and 8 February 730. It is uncertain whether any of these are genuine earthquakes, and the 680 event is probably a misdated reference to the 684 event, which occurred in the Isle of Man and may have been a rockfall. The fact that the 707 reference mentions two events suggests the possibility of a real earthquake and an aftershock, but could equally well be two landslides in a week of heavy winter rainfall. Finally, we have notice of an earthquake on 12 April 740 on Islay, and there the record stops. It is rather interesting that the last of these pre-millennial events from the Irish annals coincides with the approximate date at which the chronicle ceased to be kept in Iona.

2.2 Later British chronicles

The earliest earthquake report that can be reliably considered a real earthquake (because it is reported as felt over an area) comes in 974. It was described by the source, the chronicle commonly attributed to Florence of Worcester (Stevenson 1853–1856), as affecting all England. A main source for the chronicle for most of this period is the chronicle compiled at Fulda, Germany, by the Irish monk Marianus Scotus. The complete entry for 974 in the Worcester chronicle reads:

> This year there was a violent earthquake through the whole of England. Eberger, archbishop of Cologne, gave the abbey of St. Martin at Cologne to the Scots for ever. Minborin, a Scot, was the first abbot.

The second part of the entry is exactly as appears in Marianus Scotus (Scotus 1559), but not the first part, which appears to have been inserted by the chronicler from some other source; none of the extant versions of the Anglo-Saxon chronicle mention the event. If the chronicle draws on a German source like Scotus, it cannot be taken for granted that the interpolations are local to Worcester. The epicentre could have been anywhere in England.

According to Goutoulas (1653) this earthquake threw down houses and killed people; whether Goutoulas was citing a source now lost or made this detail up one cannot tell.

Reports of earthquakes continue in British monastic chronicles throughout the Middle Ages, with a marked decline after the thirteenth century. The record of seismicity in Britain in the fifteenth century is almost a complete blank. A complete account of the earthquakes of the period up to 1600 is given in Musson (2008).

It should be a principle of historical research not to read into any source document more than it can reliably bear. In particular, any source has to be taken together with its context. Reading a document in the light of the original author's intentions in compiling it, and not according to what the modern reader would like it to mean, is fundamental to correct procedures in historical research. While it would be nice to think of medieval monasteries as akin to a recording network for British earthquakes, this analogy cannot be sustained.

The reasons why this comparison is unreliable have been gone into by Musson (1998, 2004). Monastic chroniclers recorded things that they felt were of note, and what these things were, and why they were of note, varied. In the case of "prodigies" (remarkable occurrences) such as earthquakes, the importance of these to the medieval mind was that they were portents, either of God's wrath or coming political events. They were not seen as purely natural occurrences.

The corollary to this is that if an earthquake is memorable because it may be a portent, it is not all that important exactly where it occurs, or how severe it is. The important thing is the date of its occurrence, and this is probably one reason why many chronicles record the occurrence of earthquakes with absolutely no details beyond the date, while the date is recorded carefully, using more than one dating system to make sure the reader is in no doubt.

Consequently, if the chronicle of a particular abbey mentions any earthquake, this does not necessarily mean the earthquake was felt at that abbey, unless this is specifically stated or there is other internal evidence to suggest this.

On the other hand, the absence of mention of an earthquake in a chronicle is not necessarily evidence that the earthquake was not felt at that place. It may indicate that the writer himself did not feel the earthquake (for a variety of possible reasons) or that he did not consider it worth recording.

One can make comparisons with the reporting by medieval chroniclers of severe storms, which are more frequent, more damaging, and more widely observed in Britain than are earthquakes (Musson 2004). The lack of such reports on a regular basis shows the general lack of interest shown in such phenomena by monastic annalists.

2.3 Alexander Neckham

For the earliest British writing on earthquakes as a phenomenon, outside of mentions in chronicles, one turns to Alexander Neckham. Details of his life are fragmentary, but it is known that he was born in St Albans in September 1157 (Wright 1863). Educated first at St Albans, he moved to the University of Paris, where he achieved the post of professor some time around 1180, while he was still in his twenties. He returned to England around 6 years later, and became a monk in the Augustinian abbey at Cirencester, and was subsequently elected abbot in 1213. He died 4 years later, in 1217, at Kempsey, near Worcester, and is said to be buried in Worcester Cathedral (Wright 1863).

Neckham today is best known for his *De Naturis Rerum* (Of the Nature of Things), an account of scientific knowledge as it existed in England towards the end of the twelfth century. The exact date of composition is unknown, but indirect evidence suggests that it was a well-known text around the time of the accession of King John to the English throne in 1199.

Neckham was not a particularly original thinker, and where he does venture ideas of his own, they are often wrong. He is interesting today for preserving what passed as received knowledge at the period at which he lived, drawing largely on Solinus (early third century), Cassiodorus (sixth century), and ancient authors such as Pliny and Aristotle. To these he adds a number of observations and stories of his own, including what seems to be the first known reference to the “man in the moon”.

Chapter 48 of *De Naturis Rerum* is dedicated to the subject of earthquakes. It reads in full:

> Terra infimum est elementorum, et quasi mundi centrum. Unde cum ponderibus sit librata suis, mirum est unde motus terrae proveniat. Perhibent quod sub- terraneos meatus et cavernas venti violentia subintrat, qui cum libertatem non habeat exeundi, furit in se et multiplicatur. Erigitur igitur terrae superficies et movetur. Sciendum est enim impossibile esse totalem. De sancta terram moveri localiter. Per terram designatur sancta ecclesia, eo quod laboribus et pressuris teritur, vel a stabilitate sic dicitur. Licet igitur in multis tribulationes sustineat ecclesia, tamen semper aliqui in ecclesia tranquillitate gaudent. Tunditur Tyrus fluctibus, sed non submergitur. Volvitur Arcturus, sed non occidit. Sicut autem ventus interceptus et inclusus in cavernis terrae civitates dejicit, ita indignatio superba, diu in corde regnans, virtutum arcem prosternit. (Wright 1863)

This can be loosely translated as follows:

> The land is the lowest of the elements, and the centre of the world. Hence, since the weight is self-balanced, it is strange that earthquakes should occur. It is said that violent winds enter underground channels and caverns, which become trapped, and rage, and are redoubled in force. Then the surface of the earth is lifted up and moved. It should be noted, that it is impossible for this to be total and all lands to be moved. Land dedicated to the Holy Church by labour and afflictions is called stable. Therefore, although many are the afflictions sustained by the church, there is always some tranquillity to be enjoyed. Tyre was pounded by waves, but not drowned. Arcturus revolved, but was not killed. But as the winds caught and shut up in the caves of the earth throw down cities,

so too will anger against the proud, so long found in the hearts of rulers, cast them down like a castle.

The very limited scientific content in this is drawn from Aristotle: earthquakes are caused by the action of subterranean winds, trapped in a system of caves and passageways, and eventually violently escaping. For a commentary on Aristotle's theory of earthquakes and its transmission, see Oeser (1992). Here the point of interest is that Neckham is the first British expression of Aristotelian orthodoxy as regards earthquakes.

Later in life, Neckham produced a verse paraphrase of *De Naturis Rerum*, entitled *De Laudibus Divinæ Sapientiæ* (Of the Praises of Divine Wisdom). Much of the subject material in this poem is the same, with some additions, but the various folksy anecdotes are omitted. Earthquakes make an appearance in Book Five, and the ideas are much the same, though differently expressed. Both works are printed in Wright (1863).

While Neckham might be the first person in Britain to write about earthquakes in what might be called loosely a "scientific" context, he evidently had no particular interest in the subject. The inclusion of a chapter on the subject reflects rather that Neckham's work is something akin to an encyclopaedia, covering all topics. Indeed, up to perhaps the early nineteenth century, it was expected that a learned person should be learned about everything; it was not beyond expectation that an author could write an account of all knowledge of the world single-handedly. The last person to attempt this was Alexander Humboldt; after Humboldt the increase in human knowledge became too much for one man to master, until we reach the situation today, when normally a professional scientist will have full knowledge only of a narrow division within a discipline.

Similarly, it is worth pointing out that "scientist" is a relatively recent concept. Neckham would have regarded himself as a philosopher. Philosophy was the pursuit of knowledge, one branch of which was "natural philosophy", knowledge of the physical universe. This term, which we would now read as "science", continued to be used into the nineteenth century.

That said, the most famous figure in the history of medieval science in Britain, Roger Bacon (c. 1214–1294), was more of a specialist, being particularly interested in optics, astronomy, alchemy and mathematics. His works contain no mention of earthquakes.

2.4 The earthquake of 6 April 1580 and its aftermath

To a large extent, developments in the study of earthquakes in Britain have been driven by events. Notable earthquakes, and not just those affecting Britain, have often been the spur for particular individuals to take an interest in the subject, and produce written works—which were more easily distributed in the age of the printing press.

The first of these "inspirational" earthquakes was that of 6 April 1580, one of the larger British earthquakes, with an epicentre in the Dover Straits and a magnitude probably around 5.5 Mw, although higher values have been suggested (Roger et al. 2011).

Despite the numerous accounts preserved, there are still some difficulties in assessing intensities for this event. As is so often the case, accounts speak more about damage to special buildings—churches and castles—than to ordinary houses. To take examples only from Kent, the tower of the church of St Peter's, Broadstairs was cracked from top to bottom (Hasted 1800), St Peter's and St Mary's churches in Sandwich were both cracked (Boys 1792), and the church of St Peter and St Paul, Sutton (near Dover) was partly thrown down (Hasted 1800). Damage to ordinary houses is mentioned at none of these locations, but one suspects that this is more due to a sense that what happened to plebeian housing was not worth recording, than to an actual absence of damage. Significant damage to churches in

British earthquakes is unusual, and comparable effects are only to be found amongst a few later events. In the case of the 1884 Colchester earthquake, damage to churches occurred in an area over which the intensity was 7 or 8 EMS (European Macroseismic Scale). Thus the maximum intensity in England in 1580 should have been in this range.

Writers who do mention damage to houses are Fleming (1580), Churchyard (1580) and Wood (1796). Fleming (1580) reports householders in London complaining that £20, £30, not even £100 would be enough to meet the cost of repairs. Churchyard (1580) is more precise in describing the fall of chimneys and small pieces of stone and mortar from the tops of houses in London (particularly Shoreditch). Wood (1796), following an entry in the Annals of Merton College (Fletcher 1976) speaks of great damage to both the foundations and roofs of houses and churches in Kent.

This earthquake was also the first occasion in Britain on which we have specific information on earthquake fatalities (Musson 2003). Two child apprentices, Thomas Gray and Mabel Everite were struck by stones falling from the roof of Christ's Church, Newgate, in London. The boy was killed outright; the girl died of her injuries a few days later.

On the continent, the strongest effects are reported from Calais, where several houses are reported to have been thrown down; the town walls were damaged and part of the watch-tower collapsed (Bourquelot 1857; Bernard 1715; Bellart and Vion 1991). At Boulogne, damage was done to the church of Notre-Dame, and in houses even heavy furniture was displaced (d'Hauttefeuille and Bernard 1860; Bellart and Vion 1991).

The effects in London gave rise to such alarm that many printed pamphlets were rushed out, containing a mixture of journalism, doggerel, theology, and scientific discussion. Since all printed works had to be registered at the Stationer's Company, we know exactly what was published, even where no copies still survive. The first of these pamphlets, "A godly newe ballat moving us to repent by ye example of ye earthquake happened in London ye 6. of Aprill 1580" was issued the very next day after the earthquake, price fourpence (Arber 1875). The author is unknown, and no copy has survived, but Collier (1849) cites a MS ballad which he suggests is this one. Pamphlets of this sort were the journalism of the day.

Four of these pamphlets have survived: Churchyard (1580), Fleming (1580), Golding (1580) and Twyne (1580). Hitherto these have been examined for details of the effects of the earthquake (e.g. Neilson et al. 1984a) or for their social context (e.g. Walsham 1999), but here I want to consider what they tell about the state of knowledge on earthquakes in general.

2.4.1 Churchyard

Thomas Churchyard (c. 1520–1604), soldier and author, was quick off the mark, and his pamphlet was rushed out on 8 April, two days after the earthquake. Its full title reads "A Warning to the Wyse, a Feare to the Fond, a Bridle to the Lewde, and a Glasse to the Good; written of the late Earthquake chanced in London and other places, the 6th of April, 1580, for the Glory of God and benefit of men, that warely can walk, and wisely judge. Set forth in verse and prose, by Thomas Churchyard, gentleman". Churchyard makes it very clear what he thinks about earthquakes as natural phenomena:

> But perhaps some fine headed fellowes will wrest (by naturall argumentes) Gods doing and works, to a worldly or earthly operation, proceeding from a hidden cause in the body and bowels of ye earth … Let such fine wittes search out secretes, and sift what they can from the bottome of their senses. Yet those that feare God … will take the Earthquake to be of another kind of Nature: And beholding the myraculous manner of the same … will embrace Gods visitation … (Churchyard 1580)

So much for science; earthquakes are supernatural visitations from God, so far as Churchyard is concerned, and that is an end of it. Though, as Walsham (1999) makes clear, it was possible at this period (and earlier) to operate a sort of double-think, whereby, for instance, an eclipse could be seen as a divinely-sent portent of things to come, even though it was known that an eclipse had a natural cause.

A further point of interest regarding Churchyard is that, alone of any of the pamphlet authors of 1580, he makes a second appearance in the annals of British earthquakes. After the southern North Sea earthquake of 1602, he brought out another pamphlet, formerly thought to be lost. However, a single copy was found to survive in the Lambeth Palace Library. Sadly, this publication contains no information whatever about the earthquake beyond the reference to it in the title (Churchyard 1602).

2.4.2 Fleming

Abraham Fleming (c1550–1607) is rather more interesting. He graduated from Cambridge in 1582, after having interspersed his studies with frequent trips to London (Venn and Venn 1922). He was a prolific author with wide interests, especially translation from the Classics. Later he took orders as a Calvinist minister.

Fleming's (1580) tract is a rather more substantial treatise, the full title of which reads "A Bright Burning Beacon, forewarning all wise Virgins to trim their lampes against the comming of the Bridegroome. Conteining A generall doctrine of sundrie signs and wonders, specially Earthquakes both particular and generall: A discourse of the end of this world: A commemoration of our late Earthquake, the 6. of April, about 6. of the clocke in the evening 1580. And a praier for the appeasing of Gods wrath and indignation"

The emphasis of this work is religious in nature, and most of it is a translation of Book VII of Friedrich Nausea's (1532) *Libri mirabilium septem* with Fleming's interpolations. On the causes of earthquakes, he cites the traditional Aristotelian view, transmitted via Pliny, as given by Nausea:

> I think in like manner, that the earth is made to quake by the violence of winds shut up & kept close in the hollowe places of the same, which windes by their stirring doe stirre the earth, & so make an Earthquake … Now these winds thus shut up, seeking a vent here & there to breake out, and trieng by all meanes they can make to have passage, that breaking out of prison (as it were) they might be set at libertie, and blowe at large, whiles this is intended, the earth trembleth, rocketh, & reeleth as though it would fall …

But there is an alternative hypothesis:

> Hereupon say some, that trembling in the earth, is nothing else but that which thunder is in a cloude: and the gaping of the ground none other thing, than when as lightning bursteth forth with violence …

After translating these scientific speculations from Nausea, Fleming interpolates some text of his own, more or less agreeing with Churchyard:

> Notwithstanding these reasons carrie with them a countenance & shew of credite, and therefore may the lesse be gainsaid: yet least by seeking to become wise in the secret workes of God, and referring that to the course of naturall causes, which come to passe by the providence of his judgement, we fall into securitie, from securitie into incredulitie, from incredulitie into atheisme, from atheisme into open blasphemie: my

> counsell and advice is, that our eares tickle not to heare every vaine Philosophers fancie decanting upon matters of great importance, and thereby pull from God the cause of his justly conceived indignation against the wickedness of the world …

From Nausea, Fleming repeats some of the characteristics of earthquakes as gleaned from historical accounts. He states, for instance, that earthquakes are most common in places near the sea, and also in hilly districts. Also that they are more common in spring and autumn. He states that earthquakes happen more often by night than by day. A reader would also learn of the different effects of earthquakes (ultimately from Seneca):

> It is not unknowne to the learned, that in the kind of moving and shaking there is a great difference: for the earth may quake many waies. Now it is a dangerous and fearfull Earthquake, when as the earth is rowled to and fro like a wave of the Sea … Contrariwise it is not so perilous, when with quaking, the frames of houses and buildings cracke with shrinking … as also, when houses meeting together, ratle & knocke one against another, by reason of interchangeable moving, the one resisting and withstanding the other.

Fleming now chips in a contribution of his own. Considering the accounts of what happened in England, he concludes that the 1580 earthquake was of the less dangerous kind of shaking.

However, Fleming's Chapter 12 is entitled "A contemplation of wonderfull accidents, and principally of Earthquaks, as well particular as generall, which have happened in the realms of England, Ireland, & Scotland, from the time of K. William the Conquerour, to the reigne of our sovereigne Lady and gratious Queene Elizabeth, &c." This is Fleming's original work, and forms the first known catalogue of British earthquakes (Musson 2004). The catalogue is presented in the form of free text, moving from one event to another, rather than as any sort of table. Because of the interest of this very early catalogue, it may be as well to list the events in full. The earthquakes mentioned are as follows:

- "Reign of Eugenius"—London
- March 1077—all England
- 1084—unspecified
- 1165—Ely, Norfolk, Suffolk
- 1179—Oxenhall, near Darlington
- Monday the week before Easter 1185—all England; damage at Lincoln
- January-February 1199—Scotland
- 1222—Warwickshire
- 1247—London
- 1248—Bath and Wells
- 1250—St Albans
- 1266—Ireland
- 1274—various places in England
- 1275—all England, damage at Glastonbury
- 1382—two earthquakes in various places, especially Kent
- 1563—Lincoln and Northampton
- 6 April 1580—London

Of these, the first is probably mythical, and the 1179 event is not an earthquake. No sources are given, but it is clear that the entries are mostly derived from medieval chronicles, directly or indirectly. It is noteworthy that there is only one event between 1382 and 1580, when one might expect the author to be better informed about his own time than about the thirteenth century. Fleming's failure to mention a large English earthquake in 1575, only five years previously, is particularly surprising. His principal source would seem to be the general historical work of Holinshed (1577), hardly surprising when one finds Fleming listed as a contributor to the second edition of Holinshed et al. (1587). Holinshed is also more or less the only source (albeit secondary) known for the Scottish earthquakes of 1202, which Fleming misdates, and is the most accessible source for the Irish earthquake of 1266 (which is confirmed by the annals of Clyn and Dowling; Butler 2003).

2.4.3 Twyne

Golding (1580) we can pass over as another moraliser; of the various lost pamphlets, so far as can be judged from surviving details, these were either moralising tracts, or ballads. Far and away the most interesting of the 1580 writers is Thomas Twyne. His pamphlet (Twyne 1580) appeared a week after the earthquake, and was entitled "*A shorte and pithie Discourse, concerning the engendring, tokens, and effects of all Earthquakes in Generall: Particularly applied and conferred with that most strange and terrible worke of the Lord in shaking the Earth, not only within the Citie of London, but also in most partes of all Englande: Which hapned upon Wednesday in Easter weeke last past, which was the sixr day of April, almost at sixe a clocke in the evening, in the yeare of our Lord GOD. 1580.*"

Of the various pamphleteers, Twyne was the one with the nearest to a scientific background. Having obtained a fellowship at Oxford in 1564, he moved to Cambridge to study medicine, eventually becoming a successful physician (Ockenden 1936). Twyne, in contrast to his contemporaries, sets out at once in his title that his main objective is to consider the effects and causes of earthquakes in general, rather than to call the wicked to repentance.

Nevertheless, Twyne is completely orthodox in his views:

> … I must be fayne to borrowe from the Prophane wryters, who have most dilligently laboured in the search of naturall causes, whereunto they could not so clearly have atteyned without the finger of God, which hath led men as well into the true contemplation of these matters, as of any other knowledge. And therefore following Aristotle as cheefe in this behalfe …

Here is a succinct statement of the medieval view: Aristotle, backed by the Church, is a definitive guide, with the authority of Christian doctrine. Thus we have the usual explanation that earthquakes are due to subterraneous winds or vapours. For this reason, he believes, earthquakes are more likely in places with caverns or mines, such as Mendip (Somerset) or Newcastle—two places not actually associated with seismic activity. Also suitable are "hollow cliffes by the Sea side" such as Dover and Folkestone, and this is rather more reasonable, given the location of 1580 earthquake. As does Fleming, he mentions Aristotle's association of earthquakes with hilly districts, "so that (perhapps) the uplandishe people of *Wales* are better acquainted with such effectes than we are, as it standeth with good reason, and I have heard also some to report by tryall and knowledge."

Twyne continues with a description of the noise that may accompany an earthquake, followed by a discussion of "earthquake weather", which includes a description of earthquake clouds ("a long narrowe clowde stretched foorth, which is the forerunner of an Earthquake") a phenomenon which was apparently observed by some people before the 6 April 1580 event. There are still those today who support the idea of cloud precursors as a real phenomenon.

Twyne's attitude to earthquakes, and also astronomical events such as comets, is dualistic. The sun, planets and stars (i.e. natural forces) provide the efficient cause of such phenomena, through which God works. Earthquakes are no miracles, and proceed from natural causes, but nevertheless are ultimately subject to Divine will. Thus he gives thanks to God that the shaking lasted only a minute, whereas an earthquake that struck Constantinople once lasted a whole year (such reports refer to long aftershock sequences, which were often not realised to be a succession of separate events).

Twyne shows an enquiring mind, and he attempts to check if earthquake effects he has read about, occurred in the 1580 earthquake. Thus he finds and reports that the waters of the Thames were disturbed (seiche), but there were no great marine inundations. Nor was water in wells or springs rendered turbid.

He even tries to time the arrival of the earthquake at different places, supposing that it started in eastern Kent, reached Rochester at 17 h, London at almost 18 h, Windsor around 18 h 30 m, and then bent round from a westerly progression to a northerly one. Of course, in the absence of standard time, such figures are meaningless, but to even attempt the measurement is extremely advanced for the period. Twyne provides the first instance in Britain of what might be considered a scientific investigation of an earthquake.

2.4.4 Shakespeare

If William Shakespeare is mentioned in a seismological context, it is usually with regard to a line in Romeo and Juliet, Act I Scene 3, where the Nurse remarks:

> 'Tis since the earthquake now eleven years;
> And she was wean'd (I never shall forget it),
> Of all the days of the year, upon that day;

This has often been taken as a reference to the 1580 earthquake, which one may suppose Shakespeare felt, and as evidence that the play was written in 1591 (a credible date, from other evidence). Certainly, it is likely that Londoners of the time used the date of the earthquake as a reference in this way, and it is not to be supposed that "the earthquake" was intended to refer to an Italian earthquake, just because the play is nominally set in Italy.

However, far more interesting from a seismological perspective is Henry IV Part One. In Act III Scene 1, Owen Glendower (Owain Glyndŵr) is boasting of his wizardly powers, and that his birth was marked by supernatural portents, including an earthquake. Hotspur is not impressed, and replies:

> Diseased nature oftentimes breaks forth
> In strange eruptions; oft the teeming earth
> Is with a kind of colic pinch'd and vex'd
> By the imprisoning of unruly wind
> Within her womb, which, for enlargement striving,
> Shakes the old beldame earth and topples down
> Steeples and mossgrown towers. At your birth
> Our grandam earth, having this distemp'rature,
> In passion shook.

This is a nice summary of the Aristotelian theory of earthquakes. In this scene, Owen Glendower is being held up to ridicule, and the audience are expected to side with Hotspur. So the message is that earthquakes are a natural phenomenon, and not supernatural portents. Lomnitz (1995) suggests that the reference to intestinal gases was included for comic relief, and therefore indicates that Shakespeare was doubtful about Aristotle's theory. But the comparison with intestinal gas is already there in Aristotle, and it is only Glendower that is being ridiculed here, not Aristotle.

It would be interesting to know where Shakespeare got his knowledge from. Did he read any of the pamphlets by Twyne or his contemporaries? (This play was written about 1597, some time after the earthquake of 1580.) What is shown here is that Aristotle's explanation of earthquakes counted as general knowledge at the end of the sixteenth century.

3 British seismology in the age of enlightenment

In Britain, the seventeenth century was a period of revolution—quite literally, with the overthrow and regicide of Charles I, followed eventually by the restoration of his son, Charles II, in 1660. But it was also a period of scientific revolution in Europe. As we saw with Thomas Twyne, as late as 1580, Aristotle was still seen as an unimpeachable authority whose views were not to be questioned. Earthquakes were due to the violent escape of subterranean winds. Aristotle, Seneca and Pliny all said so, and doubtless God had guided their writings for the benefit of subsequent generations.

This was about to change.

The only way that the stranglehold of Aristotelian orthodoxy could be broken was for Aristotle to be found majorly wrong on something, in which case it would open up the rest of his work to dispute. That breakthrough was achieved by Galileo in his work on astronomy in the early seventeenth century. Once it was proved and accepted that the old Aristotelian cosmogony of the sun and stars revolving round the Earth was wrong, then the whole Aristotelian edifice was up for grabs, and it became possible to question other parts of it without running the risk of being tried by the Inquisition.

The word "Renaissance" here is appropriate. There are times in history, and the late nineteenth century was one of them, when it seemed that scientific knowledge was almost complete, and little remained to be discovered—only for this certainty to be shattered by a realisation of how much remains unknown. The seventeenth century "rebirth" saw everything that had been taken for granted as known, through the writings of Aristotle, Seneca and Pliny, suddenly revealed as unknown and uncertain. Everything needed to be reinvestigated.

The response to this need was the founding of scientific societies where like-minded scholars could meet and exchange ideas and findings, usually under a noble or royal patron. The earliest of these was in Italy, appropriately enough—the Accademia dei Lincei, founded in Rome as early as 1603, and which included Galileo himself as a member.

Further societies followed; although in Italy the "Academy of Lynxes" folded after the death of its patron, informal societies gathered in England and France in the first half of the century, which evolved into very eminent bodies in the 1660s: the Royal Society in London in 1660, and the Académie des Sciences in Paris in 1666. The first German learned society, the Collegium Naturae Curiosorum, was earlier, in 1652, but was largely dedicated to medicine.

These societies, besides offering scholars a chance to meet (the Royal Society met weekly) also provided an outlet for disseminating results through publication. The journal established by the Royal Society came out under the title "Philosophical Transactions of the Royal Society" (or Phil. Trans.), "philosophical" in its seventeenth century usage (from "love of wisdom") as the pursuit of knowledge—all knowledge.

3.1 Earthquake investigations in 1666

The first opportunity that members of the newly-founded Royal Society had to investigate an earthquake came in 1666. Of all the earthquakes to become historically important, that of 19 January 1666 must be one of the most unlikely. It was small (∼3 ML) and did

Fig. 2 John Wallis (Portrait by David Loggan © National Portrait Gallery, London)

no damage. But it was felt in Oxford, home to some of the finest scientific minds in the country.

The date of the event is variously given as 19 January 1665 according to the Julian calendar, and 29 January 1666 according to the Gregorian, which did not come into official use in Britain until 1752. The compromise followed here is to use the date according to the "old style" still in use, but with the modern practice of beginning the year on 1 January rather than 25 March, hence 19 January 1666. The epicentre was near the Buckinghamshire-Oxfordshire border, probably on the Buckingham side, and the highest assignable intensity is 4–5 EMS at Brill. It was felt over most of the two counties. At Oxford, the intensity was only 3 EMS, where it was felt by the mathematician John Wallis (Fig. 2), who noticed "some kind of odd shaking or heaving" in his study (Wallis 1666), which he took at first to be due to the passage of carts or coaches nearby.

3.1.1 Wallis

Wallis's (1666) paper on the earthquake consists largely of thermometric and barometric observations made around the time of the earthquake, and descriptions of the instruments used. This is frustrating to the modern seismologist, who is looking for information on the effects of the quake in order to reconstruct the macroseismic field, and has no interest in completely irrelevant meteorological detail. However, it is important to view things in context. Wallis himself had no idea what measurements were relevant and which were not. Thus his observations were a very natural attempt to collect potentially important information. They also mark what is almost certainly the first ever attempt to gather instrumental data in an earthquake investigation. The data may have been the wrong data, but the motivation was correct. Ho noted that his barometer (which he refers to as a "baroscope") had risen half an inch about the time of the earthquake, but confessed he was not watching it closely at the time, and has no conclusions to draw.

Wallis also attempted to determine the extent to which the earthquake was felt, collecting information from outlying villages, namely Blechington, Bostol, Horton, Stanton-St Johns and Whately. From this, he concluded that Oxford "was about the skirts" of the area affected (Wallis 1666).

3.1.2 Boyle

The second account of the earthquake to appear in Phil. Trans. was by another well-known name not normally associated today with earthquakes—Robert Boyle. Boyle was visiting friends outside Oxford at the time of the shock, and observed a "manifest trembling in the house" not so strong as to be recognised at once as an earthquake, but felt also by the other occupants of the house.

Boyle (1666) conjectured that the effects of the earthquake might be more pronounced on higher ground, so sent an enquiry to an acquaintance in Brill, who reported back that the effects were more considerable than in Oxford. Boyle's short text implies at the end that he intended to make more a extensive investigation of the extent of the shock; if he did, it was not published.

3.1.3 Hooke

Robert Hooke's "Lectures and discourses of earthquakes" followed 2 years after Wallis and Boyle's letters to the Royal Society, but were not published until the next century (Hooke 1705). Hooke has sometimes been credited as being the first contributor to the history of seismology, on account of his development of the theory of elasticity (e.g. Wallace et al. nd). However, Hooke's interest in elasticity and his interest in earthquakes were two quite separate things, and is not until the next century that anyone would make the connection between the two subjects.

Hooke's interest in earthquakes was actually not in the nature of seismic phenomena, but in their impact on geology. The particular problem that concerned Hooke was that of fossils. The existence of fossil sea-shells in strata far from the sea and at considerable elevations was problematic, given that at this period it was taken for granted that the Earth was less than 6,000 years old.

There were two possible explanations. If they were truly the remains of sea creatures in a mineralised form, then some agency must have been responsible for the dislocation. Alternatively, they were not the remains of sea creatures, but purely inorganic mineral formations that imitated living forms according to some inherent tendency of things towards certain shapes, a “plastick vertue” or “apish tricks of Nature”.

Hooke found this far-fetched. If a fossil looked like a shell in the finest detail, then it clearly was a shell, even if the process of fossilisation was, at that time, a mystery. Furthermore, if fossils were simply minerals following the form of living things, why were these so often sea shells, and never fruit or vegetables? It followed, then, that some great force had uplifted the former seabed, and for this, Hooke looked to earthquakes.

He proceeded to approach the subject after the manner of Francis Bacon, listing and attempting to classify the various types of earthquake (Oldroyd 1972). These he considered purely in terms of their uplift or downthrow effect, which is all that was relevant for explaining fossils. Thus he distinguishes between earthquakes that raise up land that was originally at or near sea level from those that raise up the seabed, or raise up mountains from level country. He does not seem to have been interested in the fact that many earthquakes that are felt, have no such effects. Thus the historical examples he gives are all of either: great earthquakes that did cause large land movements; volcanic eruptions (often accompanied by earthquakes); or landslides (giving some notable British examples of these).

Hooke does proceed towards a hypothesis to explain why earthquakes occur, which is ingenious and unlike any preceding. He was well aware that in historical times the position of the magnetic poles had changed. Now he speculated that the position of Earth’s axis of rotation and centre of gravity might also fluctuate also. This would set up instabilities that would cause the slipping and sliding of strata in response.

The chain of reasoning by which Hooke arrives at his conclusions is interesting from the perspective of the development of scientific method (Oldroyd 1972) even if his ideas seemed absurd to his contemporaries (Rappaport 1986). The idea of a gravitational cause of earthquakes is rather more sophisticated than blasts of underground winds, and had Hooke thought more about the mechanics of the displacement of strata that would be necessary, he might have deduced the role of faulting well ahead of anyone else.

3.2 Writings after the 1692 Verviers earthquake

Key British earthquakes in the development of seismological writing in Britain have tended to be those felt where the intelligentsia were; usually London, and exceptionally in the case of the 1666 event, Oxford. The 1580 quake is the first example. The second occurred on 8 September 1692. This earthquake, one of the largest in NW Europe, had an epicentre near Verviers, Belgium (Alexandre et al. 2010). It was very distinctly felt in London and SE England, and as far as Somerset (Melville et al. 1996).

This earthquake was one of a triad that made a large impact on the British public consciousness in the early 1690s, the other two being the Jamaican earthquake of 1692, that destroyed the colony town of Port Royal, and the 1693 Val di Noto earthquake, Sicily. The latter event is considered to have been the largest Italian earthquake in historical times (Boschi and Guidoboni 2001). It caused heavy destruction in the region of Catania and Noto in SE Sicily. The latter city was so completely ruined that it was rebuilt in a new location.

3.2.1 Hallywell

Little is known about Charles Hallywell. He was born in Sussex about 1672, graduated from Christ’s College Cambridge, and eventually became vicar of the parish of Seaford, Sussex, and died there in 1707 (Peile 1913). His “Philosophical discourse of earthquakes: Occasioned by the late earthquake, September the 8th 1692” was published under the initials “C.H.” only.

Attribution to Hallywell is provided by Peile (1913). Hallywell felt the 1692 earthquake in Sussex, which led him, as a clergyman, "to the consideration of the immediate Origine and Natural Causes of Earthquakes; not at all doubting, but that there is a higher Power which moderates and guides the blind impetus and force of such raging and ferocient Motions"—the orthodox view that God works through natural forces (Hallywell 1693).

In setting out his theoretical basis he cites his sources: Thomas Burnet's Theory of the Earth (Burnet 1691), Athanasius Kircher's Mundus Subterraneus (Kircher 1664) and Libertus Fromondus's Meteorologicorvm Libri Sex (Froidmont 1627) are referenced in particular.

Hallywell's main idea is that the earth is largely hollow, and that we live on a thin shell above a vast system of caverns. He gives various bits of evidence for this—rivers disappearing underground, for instance. However, he differs from the Aristotelians in favouring fire as the explanation for earthquakes. His idea is that exhalations from deep subterranean heat permeate the earth, leaving deposits of sulphurous, nitrous, bituminous and other potentially explosive minerals. Occasionally subterraneous fire penetrates into rocks containing these deposits, and the resulting explosions are felt at the surface as earthquakes.

He then gives five examples of earthquake effects and argues that these are compatible with underground explosions: thunderings heard underground, earthquakes felt over large areas almost simultaneously, cities being swallowed up, ejection of groundwater, and the formation of new islands.

While less scientific in his reasoning than his contemporary Flamsteed (see next section), his arguments are better expressed than some other writers, and at least progress away from underground winds. he concludes by remarking

> And if any one shall think that these Phaenomena being terrible and rare, that therefore they must needs signifie and prognosticate strange things; I must tell him, That I am not at all inclinable to Astrological Vanities: so I think that an earthquake may happen as well as many a blazing Star, without any ominous or direful Presages (Hallywell 1693).

3.2.2 Flamsteed

John Flamsteed (Fig. 3) was a prominent name in late seventeenth century science in Britain; he is best known today for having been the first Astronomer Royal (appointed in 1675). He was greatly impressed by the earthquakes of 1692–1693, which inspired him to apply himself to trying to solve the theoretical problem of the cause of earthquakes.

His essay (Flamsteed 1750), in the form of a letter to a correspondent in Turin, was posthumously published after the London earthquakes of 1750 (see Sect. 3.3.2). In the introduction, he states that he was refraining from printing it to avoid having to deal with criticisms—hence the fact that it was only published around 30 years after his death.

His sources, besides observations on the recent earthquakes, including a first-hand account of the Port Royal earthquake from a neighbour, one Captain Guy, consisted of conversations with some merchants acquainted with earthquakes from living long in Smyrna, and "a book of Keckerman's". The latter is presumed to be a work by Bartholomew Keckerman (1575–1609), a prolific author on many subjects; Flamsteed took from him an account of the Unterwalden earthquake (Switzerland) of 18 September 1601 (New Style).

He made seven main observations. First, he said, earthquakes always happen in calm seasons. This is supported by Keckerman (who notes it is consistent with Aristotle), and the reports of Captain Guy and the Smyrna merchants.

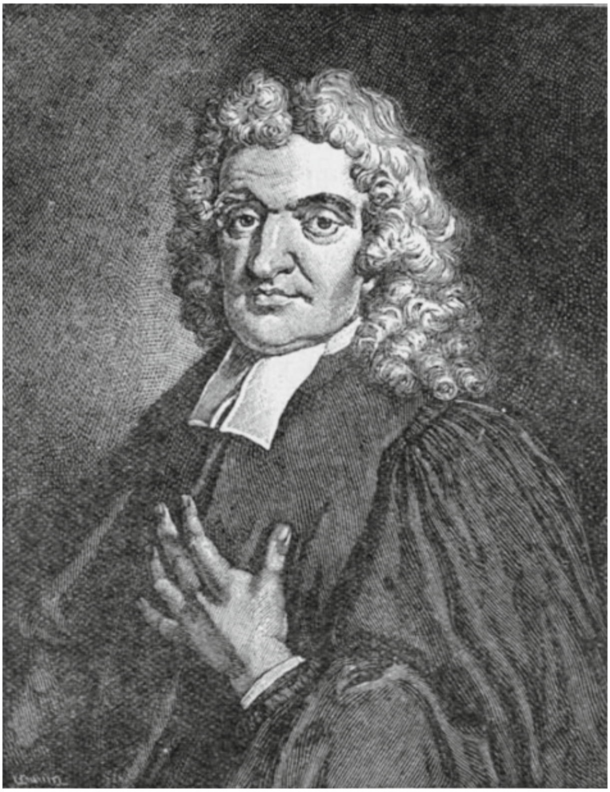


Fig. 3 John Flamsteed (Ball 1907)

Secondly, a noise in the air is always heard just before the shaking starts. This is supported by experience in London in 1692, and also from some British earthquakes earlier in the century.

Thirdly, earthquakes can be felt at sea. An earthquake at sea is experienced on board ship as if the vessel had run aground; evidence from the Smyrna merchants, and Hooke.

Fouthly, some earthquakes affect very large regions, while others are local. Flamsteed has an exaggerated idea of the extent of the 1601 Unterwalden earthquake from Keckerman: “felt in Part of Asia, all over Thrace, Hungary, Bohemia, Germany, Italy and France at the same time”. He knew that the 1692 Verviers earthquake was felt in France, Germany and the Netherlands as well as SE England, and also knew that it was not felt in the North of England or Scotland. This is contrasted with the limited extent of the 1666 event; on account of inaccurate time records, he had the mistaken idea that the shock took some time to travel a small distance.

His fifth observation is that “subterraneous noises, explosions, and eruptions, commonly precede or accompany such as are of any large extent; but not all the great ones.” This is derived chiefly from Keckerman.

His last two points come from London observations of 8 September 1692: it was felt much more in upper floors than at ground level, and it made some people feel nauseous.

These observations gave Flamsteed the evidence he needed to show that earthquakes could not be due to underground shocks or explosions. If they occurred underground, earthquakes should happen in stormy or rainy conditions just as much as in calm conditions. Secondly, a noise in the air means something is happening in the air, not in the ground. Thirdly, how could an underground shock cause a ship floating on the sea to shake, when the sea itself remained calm? Then, if an earthquake can be felt over a very large region, if it were caused by explosions in subterranean caverns, it follows that the cavern system must extend everywhere, which seems inherently improbable. Lastly and conclusively, a shock originating underground would be felt more strongly at ground level and least strongly in the upper floors of buildings, the reverse of what is observed.

Therefore earthquakes must be atmospheric explosions. "But because the common people have seen their houses shaken and overturned … they have therefore used to say, whenever their houses shake, that the earth shakes; and so to rest satisfied with this as a general reason of earthquakes" (Flamsteed 1750). Flamsteed continues to explore the implications of this, considering that some very fine particles of nitrous or sulphurous material in the atmosphere must be responsible for the explosions. He notes, for instance, that the weather was wet in the North of England and Scotland on 8 September 1692—which would wash these particles out of the atmosphere, explaining why the shock that was felt in London was not felt in the North.

Flamsteed has received little recognition for his account of earthquakes, largely because it has been dismissed as entirely wrong by those with the benefit of hindsight. Wrong it may be, but it is an excellent piece of reasoning from available evidence, given the information available to Flamsteed, and, as noted by Kennedy and Sarjeant (1982) he deserves full credit.

3.2.3 Crouch

Nathaniel Crouch is a very different figure. His interest today is less related to what he wrote and more in the influence he had. He was by profession a bookseller, and having identified a gap in the market for small, popular, easily-digested volumes, he set to fill it himself. His technique was to boil down standard texts into shorter, simplified works; the resulting works were readable and accessible, and became popular, remaining in print well after his death some time in the 1720s.

His works, which include many histories, were published under the pseudonym of Richard Burton, often abbreviated to R.B. In editions published after his death this was changed to Robert Burton.

His work on earthquakes (Crouch 1694) came out under the lengthy title "The General History of Earthquakes: Being an Account of the Most Remarkable and Tremendous Earthquakes That Have Happened in Divers Parts of the World, From the Creation to This Time; As They Are Recorded by Sacred and Common Authors; And Particularly Those Lately in Naples, Smyrna, Jamaica and Sicily. With a Description of the Famous Burning Mount, Aetna, in That Island; And the Relation of the Several Dreadful Conflagrations and Fiery Irruptions Thereof for Many Ages. Likewise the Natural and Material Causes of Earthquakes, With the Usual Signs and Prognosticks of Their Approach; And the Consequences and Effects That Have Followed Several of Them". As is evident from this title, Crouch lumped volcanoes in with earthquakes. It remained in print at least until 1734 (Burton 1734).

As might be expected from Crouch's reputation as a hack writer, this book contains no original ideas. His theoretical exposition of earthquakes is back to Aristotle's underground winds, with the extra detail that he distinguishes between natural and supernatural earthquakes, the latter being caused directly by God.

The book has more interest as an earthquake catalogue, the first such to be compiled in Britain since Fleming in 1580. But the work is not intended as anything more than a popular digest, and as a serious work is far behind the near-contemporary earthquake catalogue of Bonito (1691) in Italy.

3.3 The 18th century

In the eighteenth century, once more developments were driven by events, in particular the London earthquakes of 8 February and 8 March 1750, and the Lisbon earthquake of 1 November 1755. The London earthquakes were, like the 1666 Buckingham earthquake, insignificant seismologically, with magnitudes less than 3 ML. It was the fact that they occurred directly under the city, causing much alarm, that prompted a response from the scientific community of the day. In contrast, the great Lisbon earthquake was a disaster that shook the whole of Europe metaphorically if not literally, and raised earthquakes up the scientific agenda across the whole continent (Kendrick 1956).

Ironically, it was one year before the London earthquakes that Thomas Short published his now-notorious earthquake catalogue, and it is to this that we must turn first. The following account is based largely on Musson (2005).

3.3.1 Short

Thomas Short (1690–1772) was a physician from Derbyshire in the English Midlands. He was active in research, and had two main subjects of interest: one was the properties of various mineral waters, and the other was mortality and its possible links to different factors, especially of a meteorological nature. The latter interest gave rise to the work for which he is best known today, "A general chronological history of the air, weather, seasons, meteors, &c. in sundry places and different times : more particularly for the space of 250 years : together with some of their most remarkable effects on animal (especially human) bodies and vegetables", to give its full title (Short 1749).

His intention in writing this two-volume work was commendably scientific: to compile a chronological study of all known disease epidemics, combine it with whatever data could be found on the state of the weather, and look for correlations. As was typical for the period, he took a very broad view of what constituted "weather", and he consequently collected data also on earthquakes, comets, eclipses, etc.

The bulk of the two volumes is taken up by a chronological text in which all types of event are mixed up together. The following short extract for 1087–1088 gives the flavour:

> Great Thunder and Lightning; one half of all the People of England was seized with a violent burning Fever, which began last Year, and proved very fatal to Multitudes. At the same Time a Murrain made sad havock among Cattle; the remainder of tame Fowls, as Hens, Geese, &c. fled to the Woods; devouring Flames consumed most of the great Buildings in England; with all bad Seasons, followed by a general Famine, no less fatal an Earthquake. A fiery Dragon was seen flying in the Air, cast forth Flames out of his Mouth: Soon after followed the epidemic Ignis Sater. Isac. Chron.

Sources are cited, if rather tersely. Possibly not always correctly: "Isac. Chron." [sic] in the extract above proves to be rather hard to trace, but it seems that it can only refer to Isaacson (1633). However, Isaacson (1633) does not contain the information in the passage above.

After nearly 800 pages of this sort of writing, the work concludes with an appendix. This appendix consists of what is described as "A General Chronological TABLE of Meteors, Weather, Seasons, Diseases, &c". Each phenomenon is given its own section, starting with earthquakes, in which events are listed in order of date, from the very earliest times up to the early eighteenth century. The earliest earthquake listed is "A Great one at Babylon" in 2407 AM (1354 BC) and the last is a Sussex (South England) event in 1734.

This table contains a great many pre-millennial earthquakes, mostly in Britain, not known from any other sources. These earthquakes, starting with one that reportedly shook Edinburgh Castle in the year 10, cannot be genuine because there are simply no sources from this period that could have reported them. There was no Edinburgh Castle in 10 AD (though there was an Iron Age hill fort in its place) and there is no mention of Edinburgh as a place in any document until around 600. None of the events in the tables are given any source citation, and none of the doubtful ones appear in the text. However, many later writers, starting with Meldola and White (1885), followed by Roper (1889) and Milne (1912) have used this table uncritically. It was only when Davison (1924) produced his catalogue that anyone began to cast doubt on these records. Even so, they have proved hard to eradicate. The most of enduring of them, the "earthquake" of 811 which supposedly destroyed the town of St Andrews in Eastern Scotland, with a death toll of 1400, appears in the study of world earthquakes by Tiedemann (1992) and can now be found in the NEIC online world earthquake database, complete with epicentre (but no magnitude).

One can only speculate as to the origin of these reports, but Short states that he spent sixteen years compiling his chronology, and no doubt he called on as many friends and acquaintances as he could to supply him with data. The work, according to Short, could not have been done "whilst these scraps of histories lay scattered in a vast multitude of authors of different designs and professions". It is possible that someone attempted to play a practical joke and supplied Short with a fantasy list of occurrences to see if he would detect the imposture. If, perhaps, Short was suspicious of this list but not altogether certain, one can imagine that he might relegate it to the appendix. That way, if it were genuine, the information would not be lost, and if it were fake, the main substance of his work (the text) would not be sullied by these false data (Musson 2005).

Probably few modern readers have bothered to look at Short's actual conclusions, in which he attempts to find temporal correlations between weather events and epidemics, without the benefit of any statistical means of assessment. He finds insufficient evidence of any tendency for earthquakes to be followed by outbreaks of disease, but does find a suggestion of the reverse: that epidemics are followed by earthquakes. Wisely, he refrains from attempting to propose any reason for this.

3.3.2 Writing after the London earthquakes of 1750

It is a commonly-made complaint today that the national media give far more attention to what happens in London than events anywhere else in the country. This is nothing new. While the 1580 earthquake generated numerous pamphlets, a damaging earthquake in the West Midlands 5 years previously generated little attention (Musson 2008). Two years before the Verviers earthquake, a strong earthquake in north-west Wales similarly had little impact on the intelligentsia of the day. But in 1750, two very minor earthquakes (on 8 February and 8 March), with magnitudes probably less than 3 ML (Musson 1994), but epicentres within the city of London, produced enormous interest and another spate of writing and publishing.

Prominent at the time was the printed edition of a sermon by Thomas Sherlock (1678–1761), then Bishop of London. "A letter from the Lord Bishop of London, to the clergy and people of London and Westminster; on occasion of the late earthquakes" (Sherlock 1750) pronounced the earthquakes to be manifestations of God's wrath and the depravity of the citizens of London, particularly plays, operas, cock-fighting and novels. According to Horace Walpole, "ten thousand were sold in 2 days and fifty thousand have been subscribed for since the first two editions" (Yonge 1890). Sherlock's eagerness to promote the work was satirised at the time (Bentley and Whitehead 1750).

Sherlock has been credited as the author of an anonymous work (Anon 1750a) entitled "The theory and history of earthquakes: Containing, I. A rational account of their causes and effects, illustrated by experiments and observations on subterraneous vapours, and the manner of making artificial earthquakes; II. A particular and authentic history of those which have happened in these kingdoms, and the most remarkable of those abroad, viz. in Sicily, Jamaica, and Lima, with the most considerable eruptions of Vesuvius and Aetna; III. Some seasonable reflections on the two late earthquakes, with a pathetic address, on that occasion, to the inhabitants of London and Westminster. Humbly inscribed to the Right Rev. Thomas, Lord Archbishop of Canterbury" (e.g. see http://books.ioba.org/books/256385747.html). This attribution is almost certainly wrong. Sherlock used Whiston of Fleet Street to publish his sermon, whereas the "theory and history" was published by Newbery of St Paul's Churchyard. Perhaps more telling is that the anonymous author of "theory and history" rails against the evils of deism, a word not used by Sherlock (1750).

Anon (1750a) can be passed over with just a mention that its author believed earthquakes to be caused by a mixture or iron, sulphuric acid and water.

I also pass over Zachery Grey, who published under the pseudonym "A Gentleman of the University of Cambridge" (Grey 1750). He produced a short catalogue of notable earthquakes, with British events helpfully listed in a separate appendix. The most interesting thing about it is that the author felt it worthwhile to publish a continuation of it after the Lisbon earthquake five years later (Grey 1756).

Of the authors who attempted to explain earthquakes scientifically in 1750, the most important is William Stukeley (1687–1765).

3.3.3 Stukeley

The Rev. William Stukeley (Fig. 4) is best known today for his antiquarian writings, particularly concerning prehistoric stone circles such as Avebury and Stonehenge. But after the London earthquakes he gave an address to the Royal Society setting out his ideas on the causes of earthquakes, on 15 and 22 March 1750, as well as preaching a sermon on the theological aspects of earthquakes. He then collected both parts and published them in book form as "The Philosophy of Earthquakes, Natural and Religious". Stukeley was thus another dualist: earthquakes were divinely ordained, but operated through natural means.

Stukeley (1750) starts, as did Flamsteed, by setting out a number of propositions to be taken as initial evidence. They can be summarised here:

(1) Earthquakes always happen in calm weather.
(2) They can be felt at sea when the sea is calm.
(3) Earthquakes vary in magnitude; some shake much larger areas than others. (Stukeley actually uses the word "magnitude" here, nearly 200 years before Richter).
(4) Strength of shaking ("vibratory motion") varies between earthquakes.
(5) Earthquakes are accompanied or preceded by a noise like thunder.
(6) Earthquakes are felt more strongly in the upper floors of buildings.
(7) The shock is more violent for stone buildings than for lightly-built ones.
(8) Many people feel sick or have headaches after an earthquake.
(9) Earthquakes mostly affect cities, particularly coastal ones.
(10) Earthquakes do not damage springs, but render the water cloudy.
(11) Earthquakes are more common near volcanoes.

Fig. 4 William Stukeley (Lukis 1882)

This is an interesting mixture of astute and false observation. The common myth of earthquakes occurring in fair weather reappears, and evidently earthquakes are more reported from cities, since that is where the people are.

Stukeley next dismisses all ideas of subterranean winds or fires. Earthquakes occur instantaneously over large areas, which cannot be explained by the movement of wind or fire. He also draws attention to point (11) as having misled previous writers into making an association with subterranean fire.

He dismisses Hallywell's ideas about the largely hollow earth by noting that areas of extensive coal mines in Britain are not particularly prone to earthquakes. He also argues that since there are instances of coal mines being set on fire, and in such cases the effects have been nothing like an earthquake, then conflagration of subterranean combustible matter cannot be the cause of earthquakes. This is a very effective argument. "If the movement of a superficies of 30 miles diameter was owing to fumes, and vapours; we ought reasonably to find some great discharges of them, belching out smoke and fire, for a long time after …" (Stukeley 1750).

A further argument is produced from observations of springs, though this is less good. Springs are the surface manifestation of a system of underwater pipes and conduits laid by God at the time of Creation. If earthquakes arose from any underground cause, the whole system would by now have been thoroughly wrecked, and spring water could not reach the ground surface.

Another clever but specious argument runs as follows: siege engineers know that when an underground explosion is detonated, the volume affected by the shock is like an inverted cone. Therefore, for an earthquake such as those reported from Asia Minor with a felt area of diameter 300 miles, if it originated from an underground explosion, the axis of the cone of effect would be 200 miles, therefore the originating source would have to be 200 miles deep, which is an absurdity. If any explosion were powerful enough to affect such a huge volume, all springs in the area would be completely disrupted.

Stukeley now refers explicitly to Flamsteed (1750), which he encountered after giving his lectures to the Royal Society, but before book publication. He concludes that Flamsteed was on the right lines, but hampered by not knowing about the principles of electricity, which had been developed since Flamsteed wrote in 1693.

Electricity is the key for Stukeley. "… we see, upon a touch, or an approach, between a non-electric and an electrified body, what a wonderful vibration is produced! … how violent a shock! Is it to be wonder'd at, that hither we turn our thoughts, for the solution of the prodigious appearance of an earthquake?" (Stukeley 1750). From here he goes on to discuss the state of the weather before various earthquakes, and concludes, "if a non-electric cloud discharges its contents upon any part of the earth, when in a highly electrified state, an earthquake must necessarily ensue."

He now runs through his eleven propositions and finds they are all compatible with his electrical hypothesis. For instance, for number (7), "… the more substantial the building, the more violent is the shock: exactly the mode of electrical vibration" (Stukeley 1750). Whereas, of course, the observation really stems from the greater resilience of wooden buildings to earthquake vibration. However, Stukeley's thinking is far more well thought out than the likes of contemporaries like pseudo-Sherlock (Anon 1750a).

Stukeley's ideas, though a blind alley, proved remarkably resilient, and certainly as late as the nineteenth century one can find references to supposed electrical observations after earthquakes. The Australian amateur seismologist Albert Biggs found it necessary, in 1892, to inform the Launceston Examiner (28 January 1892 p3) on the occasion of a strong earthquake in the Tasman Sea, that the shock could not have been electrical in nature as electrical telegraph equipment was unaffected.

3.3.4 The "dissertation"

A further anonymous work, the author of which it has not been possible to trace, is a work issued by the publisher James Roberts, entitled "A dissertation upon earthquakes, their causes and consequences; Comprehending an explanation of the nature and composition of subterraneous vapours, their amazing force, and the manner in which they operate; the sentiments, on this head, of the most learned philosophers ancient and modern; the different kinds of earthquakes, distinguished by their Effects; a copious Collection of authentick Relations digested under those titles, the greater part of which have happened in Great Britain. Together with a distinct account of, and some remarks upon, the shock of an earthquake, felt in the cities of London and Westminster, on Thursday, February 8, 1749–1750" (Anon 1750b). This is another work espousing the cause of underground explosions. Its chief interest is in its exposition of the various kinds of earthquakes, which demonstrates how loosely the word was still used to mean any sort of disruption of the land, including landslips and volcanic eruptions.

These are listed as follows:

(1) The earth lying level with the sea is raised up.
(2) The bottom of the sea is raised and becomes dry land.
(3) A plain is heaved up into a hill.
(4) The earth is raised up by fresh earth being laid on it (i.e. lava or volcanic ash).
(5) The earth sinks, leaving a valley or lake.
(6) Land lying level sinks suddenly and violently.
(7) Parts of the sea bed sink, causing whirlpools.
(8) Landslips.
(9) Mild earthquakes without ground displacement.
(10) Convulsions deep in the Earth not perceptible at the surface.

The author gives "examples" of all of these, noting that most British earthquakes are of the ninth type (the 1580 earthquake is classified as the sixth, for some reason). One can see the influence of Hooke (who is mentioned by name). Unlike Stukeley (1750); Anon (1750b) is not much concerned with earthquakes as a shaking phenomenon.

3.3.5 Writing after the Lisbon earthquake of 1755

While the 1750 London earthquakes inspired an effusion of writing about earthquakes, for obvious reasons, this sudden interest in the subject was confined to Britain. The great Lisbon earthquake of 1 November 1755 was a different matter. The effects of the earthquake, at least so far as seiches, extended as far as Scotland and Scandinavia. Equally, the scale of the disaster sent an intellectual shockwave across Europe. The earthquake occurred in the morning of All Saint's Day, and collapsing churches killed many worshippers. On the other hand, those who skipped Divine service and were taking their pleasure in the parks, survived. If earthquakes were instruments of Divine will, this was hard to explain.

Possibly the best known dissertation on earthquakes written after 1755 was an essay complied far away in Konigsberg (now Kaliningrad) on the Baltic coast. It is well known not because it was particularly insightful, but because the author was one Immanuel Kant (1756a, 1756b, 1756c) who later went on to greater things.

I pass over various hack publications in Britain such as Hunter (1756) and Montagu (1756). But one British writer after 1755 presents a departure from the theoretical speculations that have been discussed up to now in this treatise. The interest today in the writings of people like Flamsteed and Stukeley is seeing how they reasoned from the data available to them. Their conclusions may have been wrong, but their arguments were scientific. Now we need to consider for the first time a writer whose importance is that he got something right. His name was John Michell.

3.3.6 Michell

Though one of the most original scientific thinkers of the eighteenth century, John Michell is sadly little remembered much today. His work on magnetism and gravity were both important: he invented a way to make artificial magnets, showed that magnetic force decreases as the square of distance, and laid the foundations for the work of Henry Cavendish in making the first accurate measurement of the mass of the Earth. In astronomy, amongst other advances, he made observations on stellar parallax that allowed him to make an accurate estimation of the distance from the solar system to the nearest star. In 1784 he was considering the effect of gravity on light, and postulated that a heavy enough stellar object could prevent light from escaping—the first description of what we now know as a black hole.

He was born in 1724, most likely in Nottingham, and entered Queen's College, Cambridge, at the age of 17, and was elected a Fellow of the college after graduation, and then appointed Woodwardian Professor of Geology. On the occasion of his marriage in 1764 he gave up both Fellowship and Professorship and left Cambridge for the quiet life of a country rector, first in Hampshire, and then finally at the little Yorkshire village of Thornhill. where he died in 1793.

Michell was also inspired by the disaster at Lisbon to consider the subject of earthquakes. He gave an address to the Royal Society in 1759 in which he outlined his ideas (Michell 1759); it was published the following year (Michell 1760).

Up until this point, a major problem to understanding earthquakes was the issue of "action at a distance"—something considered impossible without physical contact. The transmission of earthquake effects required some medium, and the only possible candidate seemed to be air. Either this was air in underground caverns, as with the Aristotelians, or the atmosphere, as for Flamsteed. In both cases, it was air or gas that provided a fluid medium for the propagation of the shock from one place to another.

It was Michell who realised that the true mode of propagation was by means of elastic waves through a solid medium. Compression at the earthquake source, he wrote, "must be propagated on account of the elasticity of the earth, in the same manner as a pulse is propagated through the air … a dilation will succeed to the compression; and these two following each other alternately for some time a vibratory motion will be produced at the surface of the earth" (Michell 1760).

It had long been known (since Leonardo da Vinci) that sound travelled as waves. It was Michell who made the fundamental realisation that earthquakes did as well. This allowed him also to explain earthquake sound, which hitherto had been related to explosions. "If these alternate dilations and compressions should succeed one another at very short intervals, they would excite a like motion in the air, and thereby occasion a considerable noise" (Michell 1760). Or to put it another way, some of the wave energy bleeds from the earth into the atmosphere, where it is perceived as sound.

The idea that earthquakes are perceptible as elastic waves also implies that wave energy radiates out from a relatively small source, whereas underground winds have no particular source. Furthermore, it should be possible, at least in theory, to locate the source observationally. Thus, Michell was able to devise an outline concept of earthquake location. Given two observers at different locations, if one feels an earthquake shock coming from the south-west, and the other feels the same shock coming from the south-east, then by drawing a line south-westwards from the first observer and south-eastwards from the second, the origin of the earthquake should be where the two lines cross.

Alternatively, if the shock arrived at the first observer before it arrived at the second, the origin must be closer to the first observer. With enough observations, one should be able to pinpoint the origin. However, as Michell realised, human powers of sensing direction or time-keeping were hardly up to the task, and he admitted that neither of these methods were really practical. However, they do anticipate the basic principles of earthquake location as are used today. They also anticipate attempts to locate earthquakes from non-instrumental data in the following century, by Mallet, and later by Davison.

Michell had less success in suggesting a cause for earthquakes. Like many before him, he noted that there seemed to be a connection between earthquakes and volcanoes. He supposed that in various places there existed great subterranean fire-pits of burning material (perhaps coal or shale). The rock strata above one of these fire-pits would be arched upwards by the heat.

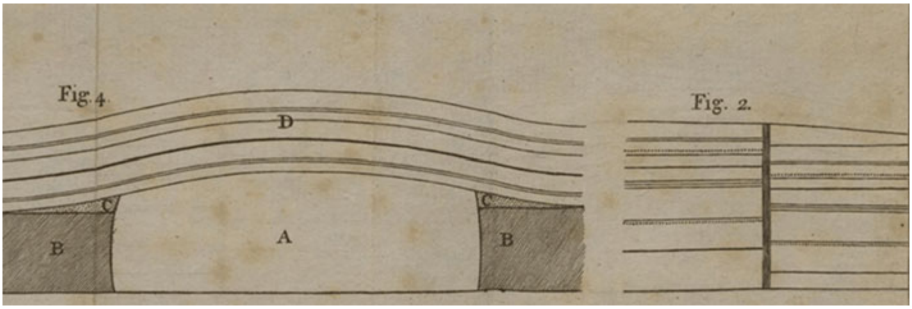


Fig. 5 Michell's hypothesis of earthquake formation (*left*) and diagram of fault displacement (*right*) (Michell 1760)

Periodically bits of it would collapse, water in the rock would turn into vapour instantaneously as it fell into the fire, and this would produce a concussion that would engender an earthquake. This did seem to explain why earthquakes seemed to happen repeatedly in the same place; after a roof-fall there would be a recuperation phase where the strata arched upwards again before the next fall could take place.

However, at one point he came tantalising close to the truth. He wrote:

> 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 1760)

This passage is accompanied with a diagram, reproduced here (as Fig. 5), of fault displacement.

Michell also discusses the 1755 tsunami, and concludes that for the wave to arrive at different places at the reported times required differing velocities along different paths, which he correctly, and far-sightedly, explains as due to different water depths.

Michell was ahead of his time. The theory of the propagation of elastic waves in solids had to wait for the work of Siméon Poisson and Augustin-Louis Cauchy in the 1820s, and even they were well ahead of observational data.

3.3.7 Bevis

John Bevis (1693–1771) deserves a brief mention here on account of his book "The history and philosophy of earthquakes", published under the pseudonym "A member of the Royal Academy of Berlin" (Bevis 1757). This is not so much an original work as a compilation of writings on earthquakes by the ten "most considerable writers on the subject", including both Hooke and Stukeley. Davison (1927a) found the work of sufficient interest to accord Bevis a mention in his "Founders of seismology", possibly due to the fact that it was used for reference by Michell (1760). It is hard to agree with Ben-Menahem (1995) that Bevis's study is "pioneering" or marks "the dawn of modern seismology".

3.4 The end of the 18th century, beginning of the 19th

Between the Lisbon earthquake and the start of investigations at Comrie, there is little to report in the annals of British seismology. The only figure to be mentioned is the polymath Thomas Young (1773–1829), who was appointed Professor of Natural Philosophy at the Royal Institution in 1801, and whose lectures were published in two volumes in 1807. Young is best known for his work in the fields of vision, light, solid mechanics, energy, physiology, language, musical harmony, and Egyptology (he worked on the decipherment of the Rosetta Stone).

3.4.1 Young

In the course of lectures on natural philosophy published as Young (1807), discussion of earthquakes occurs in Lecture 57 "On aqueous and igneous meteors", not a title that would be used today. Young believed that earthquakes and volcanic eruptions shared a common cause, which was essentially chemical in nature. To explain observations of earthquakes in non-volcanic regions, Young says, "where the agitation produced by an earthquake extends further than there is any reason to suspect a subterraneous commotion, it is probably propagated through the earth nearly in the same manner as a noise is conveyed through the air" (Young 1807).

Young is given credit as being an early proponent of earthquakes consisting of wave motion on the strength of this; Milne (1883) called this sentence "the first true conception of earthquake motion and the manner of its propagation", which seems too strong praise.

Young also cites the observation of Sir William Hamilton (1783) with respect to the 1783 Calabrian earthquake and its aftershocks that the earthquake "appeared to consist in a sudden elevation of the ground to a considerable height, which was propagated somewhat like a wave, from west to east: besides this, the ground had also a horizontal motion backwards and forwards, and in some measure in a circular direction" (Young 1807).

Volume Two of Young's (1807) lectures contains a rather curious sort of earthquake catalogue entitled "Earthquakes and Agitations. In order of time." This begins with an earthquake in 17 AD that destroyed twelve cities in Asia, and runs through to a Peruvian event in 1797. Despite its title, it is more of a chronological bibliography, including entries for Stukeley, Michell and many others. Some entries are somewhat cryptic today, such as "Wark's method of measuring earthquakes. Ed. ess. III. 142. Roz. I. 376. By powdering the inside of a vessel partly filled with water" (Young 1807). For anyone interested, the full reference is Wark (1771). The Rev. David Wark experienced an earthquake at Lisbon on 26 December 1764, and dictated a short note on detecting earthquakes a few days before he died.

Davison (1927a) hails this as the first bibliography of seismological publications, and notes that the importance of it is that it was Young who managed to trace the authorship of a number of anonymously-published works, including Bevis (1757).

4 Investigations at Comrie in the 19th century

The Perthshire village of Comrie, located north-west of Perth itself and in the valley of Strathearn, just south of the Highland Boundary Fault, has an important place in the history of seismology. This is a result of its being subject to occasional earthquake swarms: periods of intense earthquake activity lasting several years, and not fitting a typical mainshock-aftershock model.

The first of these swarms to be recorded took place between 1788 and 1801, culminating in the largest event of the series on 7 September 1801 (4.6 ML). This is unusual for a swarm to have something resembling a clear mainshock; equally it is unconventional for a mainshock to be preceded by 13 years of foreshocks. The sequence was documented by two local ministers; the Rev Samuel Gilfillan at Comrie and Ralph Taylor at Ochertyre, near Crieff. The former acquired the soubriquet “Secretary to the Earthquakes” as a result (Musson 1993).

Prior to the 1788–1801 activity, it seems very likely that the earthquake of 8 November 1608 (∼4.5 ML) was another Comrie earthquake, though there are no reports closer to Comrie than Perth. Some lesser shocks in Perthshire in the following decades may be related.

It is always tempting, given a seismic source known to be repeatedly active, to attribute other, poorly-known events to the same source. This can lead to errors, as with Davison’s attempts to locate all earthquakes in Herefordshire to a single focus between Hereford and Ross-on-Wye (Davison 1924, 1927b)—in that case, in the teeth of good evidence to the contrary. However, just because it is sometimes wrong, does not make it always wrong, and it is tempting to suggest that the January–February 1202 sequence of earthquakes in Scotland may have originated at Comrie, partly because the length of the sequence (from 6 January into the following month) is commented on by the original sources (Musson 1994, 2008). Of course, other interpretations are possible, including a source near Stirling.

Intermittent small earthquakes at Comrie continued sporadically over the first three decades of the nineteenth century, but it was in October 1839 that the next swarm began, which was to make the name of Comrie in the history of seismology. The first smart shock occurred on 3 October 1839, after which the frequency and strength of shocks increased rapidly. There was then a pause for a few days with no events, a few days in which it rained almost incessantly. Then on the night of 23 October 1839 the strongest event struck, with an estimated magnitude of 4.8 ML (Musson 1993). The unfortunate villagers rushed out of their houses into the pouring rain, and it was some time before they could be persuaded it was safe to return to their homes.

The 23 October event was followed by a period of intense earthquake activity, with multiple events felt in Comrie each day.

4.1 The British Association Committee 1840–1841

At the Glasgow meeting of the British Association for the Advancement of Science (BAAS) in August 1840, ten months after the Comrie swarm had started, a resolution was passed to “register the shocks of earthquakes in Scotland and Ireland”—a minor earthquake had also been reported in Ireland (in Inishowen) at this time (British Association 1841; 1842). The committee was to consist of Lord Greenock (Charles Cathcart, 1783–1859, a retired general and amateur geologist, then residing in Edinburgh), David Milne (of whom more later), James Forbes (ditto), a Mr Paterson or Patterson, apparently in Ireland, Joseph Portlock (1794–1864, a geologist then resident in Belfast) and James Bryce (1806–1877, also a geologist in Northern Ireland, later moving to Glasgow). A grant of £20 was awarded to support the work of the committee. As Krehl (2009) notes, this was the first occasion on which such a committee for seismology had ever been created.

No record of the discussions around the formation of the committee are recorded. It may be noted in passing that Robert Mallet was present at the Glasgow meeting; the significance of this will be seen later. I have heard folklore to the effect that the committee was set up at the request of locals in Comrie; there is no evidence for this that I am aware of, and it seems extremely unlikely.

Fig. 6 Macfarlane's observatory in Comrie (BGS archive image)

From the outset, the objectives of the committee became exclusively focused on the Scottish activity. The first report of the committee, presented at the 1841 British Association meeting, noted difficulties in communication between the Scottish and Irish contingents on the committee (British Association 1842).

The committee had several meetings "at the beginning of winter" in 1840 to discuss the need for instrumental monitoring at Comrie. It seems that the committee had already been in communication with Peter Macfarlane in Comrie, who since the 23 October shock had been keeping a register of felt events, and had also been experimenting himself with instruments to measure the shocks. It was noted that Macfarlane's instruments were too insensitive to record more than a small proportion of the earthquakes felt.

Peter Macfarlane (d. 1874) was the local postmaster and storekeeper in the village. His local nickname was "Patey-a'-thing", because anything you wanted, you would get at his store (Brough 1950 *unpublished*). He built a cupola on the roof of his store (later taken over by Brough and Macpherson) in the centre of Comrie, opposite the church, to accommodate his instruments; this survived for many years after his death, but was eventually destroyed by fire in 1903. Fig. 6 is a period photograph of the building, showing the cupola.

Tradition has it that the top-hatted figure on the left of the group of three in Fig. 6 is Macfarlane, but this is probably without good foundation. Macfarlane evidently died a wealthy man, since he left a very large bequest towards the building of the new Comrie parish church.

To facilitate keeping a register of shocks, Macfarlane devised one of the earliest ever intensity scales (Davison 1921a). However, the scale was entirely subjective in operation. He took the strength of the 23 October event as intensity 10, and then any subsequent shock he compared the strength, and rated it as half as strong (intensity 5) or whatever other fraction seemed appropriate. Macfarlane was to act as the British Association committee's "man on the spot".

In the committee's first deliberations, they chose to design two types of instrument, one based on the common pendulum, the other on the inverted pendulum principle (see Sect. 4.2 below). One instrument of the former type was built in 1840 and two of the latter. They were sent to Comrie, where they were placed in Macfarlane's charge. The smaller inverted

Fig. 7 St Kessog's Church Comrie in 1992; one of Forbes's seismometers was installed in the steeple (Author's photo)

pendulum instrument had a pendulum 1 m long and was installed in Comrie House; the larger had a pendulum 3.25 m long, and went into the steeple of the parish church (Fig. 7; note that the corner of the building just on the far right of this photo is the same building as in Fig. 6). The common pendulum was set up at Garriechrow, just west of Comrie (British Association 1842). The situation at the beginning of 1841 is shown in Fig. 8.

In the first seven months of 1841, the inverted pendulums were only affected by four shocks, on 10 and 22 March, and 25 and 26 July. The common pendulum at Garriechrow proved to be too insensitive to register at all. As an indication of the degree of friction affecting the inverted pendulum instruments, when the pendulums were moved by the March shocks, they stuck in their displaced positions and had to be recentred by hand.

By the time of the first report of the committee, they had concluded (a) that many or most of the earthquakes produced a vertical shock, which might require a different type of instrument, and (b) that the Comrie earthquakes were originating from a single spot, and that given sufficient instruments, this spot could be located.

In an offhand remark in a letter appended to the first report, David Milne states that, on 25 July, "an instrument of my own there [in the parish church] also indicated an upward movement to the extent of half an inch". What this instrument was is otherwise not indicated, but it seems that Milne was already trying to investigate vertical movements.

In addition, the committee was attempting to keep abreast with extra-British developments, citing an earthquake register recently published in the Transactions of the Royal Academy of Turin. They also speculated that it might be desirable to have meteorological instruments installed at Comrie, in case the state of the atmosphere was relevant, or the cause was electrically related (shades of Stukeley).

Fig. 8 1840s map of Comrie, showing location of instruments at the beginning of 1841 (Author's photo)

The report concludes by recommending that the committee deal only with earthquakes in Scotland, and that its membership, dropping the Irish contingent, should be swelled with five new members (Robison, Jameson, Traill, Christison and Torrie, none of whom feature much in subsequent activities).

4.2 James Forbes

James Forbes (1809–1868), at the time of the Comrie investigations, was Professor of Natural Philosophy at Edinburgh University (Fig. 9). He is perhaps best known for his work on glaciers, and the Forbes Glacier in New Zealand is named after him. He lived at Greenhill in Edinburgh, on the south side of Bruntsfield Links.

Of the British Association committee, it was Forbes who was given the task of designing an instrument to be deployed at Comrie. At the outset, he was shown some existing devices by Milne and Greenock, the nature of which has not been described in any surviving account. Forbes's aim was to come up with something that combined the properties of "simplicity, compactness, comparability and an easy adjustment of sensibility" (Forbes 1844).

A design based on a common pendulum had been already suggested for the investigation. Such instruments had already been employed in Italy since the middle of the previous century (Bina 1751). These generally employed long pendulums for adequate sensitivity, typically of the order of 2–3 m (Wartnaby 1972). Forbes was afraid that a length exceeding 6m would be required, and since the sensitivity would depend on the length of the pendulum and not the weight of the bob, this could not easily be adjusted.

Forbes drew inspiration from a paper describing an adjustable pendulum that could be used as a gravimeter (Kater 1818). Forbes's idea was to use an inverted pendulum consisting of a vertical rod with a lead weight on it, balanced on a wire that could be made more or less stiff by means of an adjusting screw. By means of changing the height of the lead weight

Fig. 9 James Forbes (courtesy of St Andrews University Library)

or the stiffness of the wire, the stability of the instrument could be altered, and hence its sensitivity to a lateral shock. The arrangement is shown in Fig. 10, taken from Forbes (1844). The registration part of the instrument consisted of a pencil on top of the rod, that could write on a paper disc fixed to the upper dome.

Of course, the use of a pencil to write a trace introduced significant friction into the design. Forbes's intention was to overcome this by increasing the mass of the pendulum. The additional instability of a heavier mass could be compensated for, if necessary, by shortening the wire using the pinching screw (D and E in Fig. 10).

Unlike a common pendulum instrument, the great advantage of the inverted design was that the sensitivity was independent of the dimensions of the instrument. Thus it would be possible to make instruments sufficiently small to be easily portable. In fact, as Forbes (1844) pointed out, the larger the instrument, the greater the stiffness of the spring, and hence a larger instrument would actually be less sensitive. Forbes's preferred length was 50 cm.

Forbes presented a paper to the Royal Society of Edinburgh on 19 April 1841 in which he set out the description of his instrument, and described its mathematical properties in some detail. This was subsequently published three years later (Forbes 1844).

It was Milne who proposed the name for the device—"seismometer". This was the first occasion on which any "seismo-" word had been employed, and words such as "seismograph",

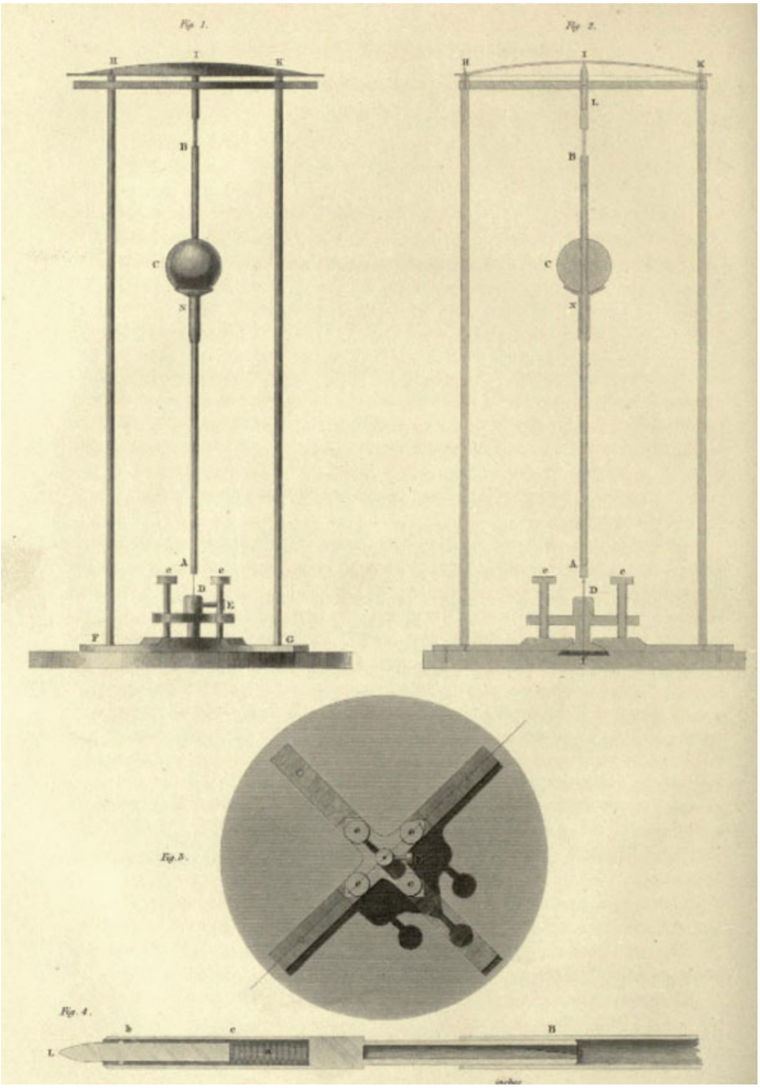


Fig. 10 Forbes's "seismometer" (Forbes 1844)

"seismology" and "seismologist" are all back-formations from Milne's coinage. Of course, lacking any time recording, the instrument would now be considered a seismoscope.

One surviving example of Forbes's instruments is in the hands of the Royal Scottish Museum in Edinburgh, but unfortunately it has lost its framework and dome. I have heard rumours that two other specimens exist in private hands, but have never been able to locate them.

4.3 Further investigations at Comrie to 1843

The second annual report of the Committee (Milne 1843a) included a register of shocks at Comrie from 23 July 1841 to 8 June 1842. In this second report, the title of the Committee

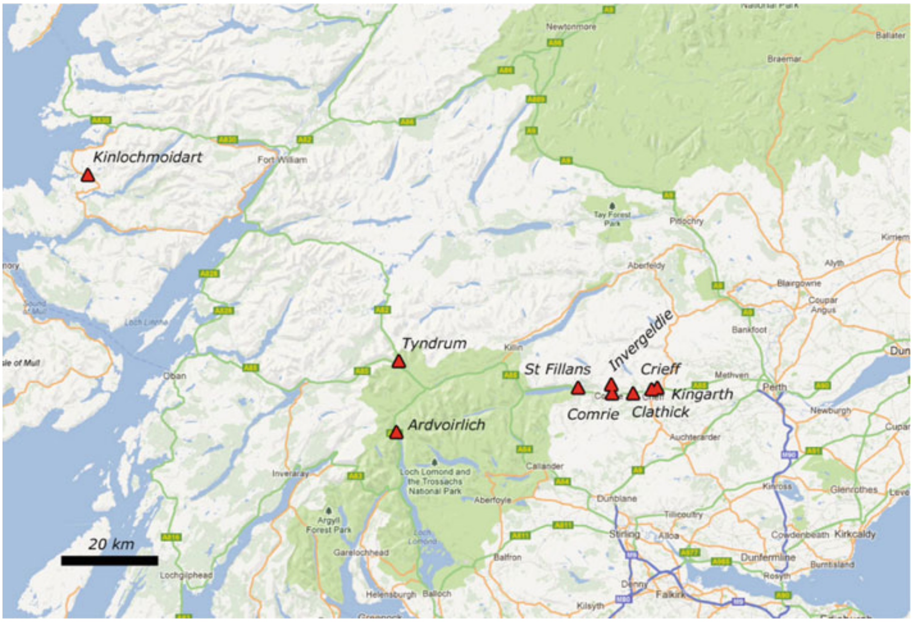


Fig. 11 The British Association seismic network in 1843

has changed to make its scope earthquakes in Great Britain, rather than just Scotland and Ireland. Out of 60 events felt in Comrie, only on three occasions did the instruments trigger. The Committee concluded that whereas the instruments were suited for the larger events, they were not sufficiently sensitive to record the smaller ones that were continuing.

Since the first year report, another seven instruments had been installed, four of which were of the Forbes design. Of these, one was sent to Crieff, east of Comrie, one to St Fillans, west of Comrie, and one to Invergeldie, a location just north of Comrie (and now the location of a station in the current BGS broadband network). The last instrument was sent to Kinlochmoidart, on the west coast of Scotland, since shocks had occasionally been felt there (Fig. 11).

A further seismoscope made of horizontal glass tubes with upturned ends, filled with mercury, was installed in Macfarlane's observatory. This instrument, designed by Charles Wheatstone, was intended to indicate horizontal movements or inclinations of the ground, by the direction of mercury running out of the ends of the tubes.

Since both this and Forbes's instruments were intended to register horizontal shocks, there was now a need for something to register vertical movements, and for this a horizontal pendulum was designed. This instrument is rather interesting, as it was the first ever use of a horizontal pendulum in seismology (see the recent study of horizontal pendulums by Fréchet and Rivera 2012, which does not actually mention the Comrie instrument). As this is a significant milestone in seismography, it is rather regrettable that we have no idea who actually designed it. Possibly it was Milne, who as Secretary of the Committee and thus author of the report, may have hesitated to claim credit.

The report describes it as "a horizontal bar, fixed to a solid bar by means of a strong flat watch-spring, and loaded at the opposite end. If the wall suddenly raises or sinks, the loaded end of this horizontal rod remains, from its *vis inertia*, nearly at rest, and thus can move any light substance (as paper or a straw) brought against it by the vertical movement of the ground, and which light substance is so adjusted to stick wherever the rod leaves it." (Milne 1843a).

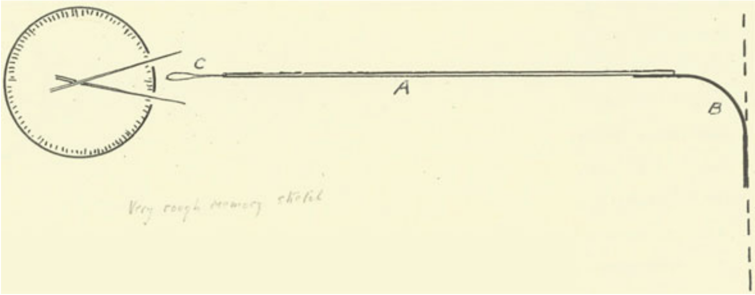


Fig. 12 The horizontal pendulum instrument in Macfarlane's observatory (Brough 1950 unpublished)

Two of these instruments were made; one went in Macfarlane's observatory, and the other went out to the Kinlochmoidart estate, entrusted to the proprietor of the estate, one William Robertson.

Fortunately, a second description of the instrument exists, which has not hithero been published. This was made by JCS Brough, who as a boy had access to the still-surviving observatory of Macfarlane, which remained intact for many years after Macfarlane's death. JCS Brough was the son of J Brough, who took over Macfarlane's store. He made a sketch from memory, which is reproduced here as Fig. 12 (Brough 1950 *unpublished*).

> The rod (A), of a very light-weight wood, was balanced in a horizontal position by a steel spring (B) and would vibrate at the gentlest touch. To the far pointed end was fixed a fine looped wire (C). The instrument operated thus: to the end of the cupboard [the observatory room] was fitted a dial (perhaps 9" or 10" in diameter), this being spaced around the circumference like a clock. It had a pair of very thin wood hands, one lighter than the other, both so adjusted in length that the loop of the wire came between them, the idea being that any movement of the rod would move the delicate hands. (Brough 1950 *unpublished*)

Brough described it as "a home-made device", but if it was one of two, it seems likely it was made in Edinburgh and assembled at Comrie.

Comrie was now surrounded by instruments on three sides, together with those in the village itself, with the object of pinpointing the source of the shocks by tracing back the recorded directions. In effect, this was the first ever use of a local network in seismology.

The Committee continued to be interested in whether the shocks were accompanied by any changes in the state of the atmosphere.

> Some persons, indeed, maintain that the shocks are nothing else than electrical discharges from the earth, and are preceded as well as followed by certain meteorological symptoms, which are capable of accurate registration. This view of the matter is, for the sake of the theory of earthquakes, deserving of being tested … (Milne 1843a).

This is an interesting insight into the state of scientific knowledge at the time. While a barometer and thermometer had now been installed at Comrie, they were only being read twice a day. The Committee reported that it would be better to have the instruments read hourly, 24 h a day, but that the cost of hiring people to do this would be £40 a year.

By mid 1842, the earthquakes at Comrie were dying down. Nevertheless, in the British Association Committee's report for the period July 1842 to June 1843, the committee was able to append to its report a register of shocks with date, time, intensity, duration, amplitude on the recording instruments (if any), and meteorological conditions, in a format that markedly resembles a twentieth century station bulletin (British Association 1844).

For the first time, the Committee's third year report contains information on earthquakes elsewhere in Britain, including a description of the strong earthquake of 17 March 1843, which was felt across north-west England and in the Isle of Man and Northern Ireland (see Burton et al. 1984a). The report was signed by the geologist William Buckland as well as by Milne; Buckland having evidently joined the Committee. It seems that Buckland was perhaps now one of the most active members; of the surviving letters from Milne (which are not many), Buckland seems to have been the most common correspondent.

The committee now took an interest in overseas earthquakes as well.

> But if it be an object worthy the attention of the Association to collect from all quarters information calculated to throw light on the causes of earthquakes, there seems no reason why they should not make it part of the business of this Committee to receive and digest notices of foreign earthquakes. (British Association 1844)

In particular, the committee had been contacted by two people interested in their work, one in India, and one about to move to Peru. The Indian correspondent was Richard Baird Smith of the Bengal Engineers, whose papers on earthquakes in India are an important resource in historical earthquake studies in that country (Bilham 2004). The other was Mathie Hamilton (1793–1869), best known today as translator of a study of yellow fever (Ardévol 1853). Buckland and Milne suggested that "two or more instruments, at the expense of the Association, should be put under Mr Hamilton's charge, if he will undertake to register their indications and report them half-yearly to the Committee" (British Association 1844). In a strange foreshadowing of John Milne's global network, David Milne was proposing to send seismometers to Peru 60 years earlier!

Meanwhile, new instruments were ordered to be installed at Ardvoirlich and Tyndrum, west and north-west of Comrie respectively. Figure 11 shows the network at its fullest extent.

That not everything was being fully reported to the British Association is evident from the discussion in the fourth annual report of the effects of the shock of 25 August 1843, which disturbed the instruments in four different locations: Kingarth, Clathick, Crieff and Invergeldie. However, this is the first mention of instruments at the first two of those locations. An inverted pendulum had been installed at Kingarth at some point, and at Clathick, a "spiral pendulum and sand glass" (British Association 1845). A further "spiral pendulum" was installed at Dunira in late 1843; there are no descriptions of this instrument, nor of the sand glass at Clathick.

The report for 1843–1844 was the last to be published, though evidently the instruments continued to be maintained, and the register kept (British Association 1871). It was suggested by Wartnaby (1972) that no further reports were produced in the 1840s because the activity at Comrie had died away, but this is not really the case—the sequence made a final climax in 1846, which included the second strongest quake after the 23 October 1839 event. Indeed, lacking any further reports from the British Association Committee, the earthquake of 24 November 1846 remained obscure, and is barely mentioned by Davison (1924).

In fact, there was another reason for the silence of the Committee, as will become apparent.

4.4 David Milne

Fate has not been kind to the memory of David Milne (1805–1890). He has been remembered chiefly for two things: being junior counsel for the defence in the trial of William Burke (grave-robbing partner of William Hare), and being the last geologist to adhere to

Fig. 13 David Milne, in a portrait by Lutyens (Author's photo)

diluvialism as a viable paradigm (at least until the present-day generation of creationists). His obituary notice remembered him largely for his studies of "boulders" (meaning erratics), and altogether failed to mention his earthquake investigations of the 1840s. Yet it is these that constitute his lasting achievement. He is the first person in history, it seems to me, who combined all the various activities of a modern seismologist—establishing an instrumental network, publishing a bulletin, conducting macroseismic enquiries, compiling an earthquake history and probing into the theory of earthquakes. I like to think of him, therefore, as the "first seismologist", at least for the period when he was Secretary of the British Association Committee (Fig. 13). It is highly appropriate, therefore, that he was the one who introduced the "seismo-" prefix into the English language.

4.4.1 Life

Milne was a lawyer by training, and practiced in Edinburgh. His father was Admiral Sir David Milne, and his son, another David, entered military service, rising to the rank of Colonel. It is therefore necessary to be careful in avoiding confusion between three generations of Davids.

He also had a considerable capacity for hard work. In the 1830s, needing sufficient funds to be able to marry his childhood sweetheart (and neighbour at Wedderburn), it was said by his contemporaries at the Scottish Bar that no advocate ever amassed so much money (£500 per annum) so rapidly (Milne Home 1891). His father was not particularly wealthy, so Milne had to be self-supporting. However, when his father-in-law died in 1852, he inherited the Paxton estates, and became a wealthy man. At this point he changed his name to David Milne Home. That, however, was long after the period of Milne's life that is of most interest in the history of science.

Milne was fascinated by geology even as a child, and it was a life-long passion. He was already a Fellow of the Royal Society at the age of 21, which seems unimaginable today. This led to friction with his father, who was afraid, with some justification, that Milne was frittering away his time on geological pursuits that would never amount to anything, and neglecting his professional work in the Law. By 1839, his father's admonitions were beginning to sink in. He recorded in his private journal that his income was beginning to decline perceptibly. (Paxton House Archives Book 9).

This did not stop him from taking on charge of the British Association Committee on earthquakes, along with his long-standing friend, James Forbes. However, the considerable workload of the earthquake investigations occupied even more of his time.

Things came to a head on 13 November 1843. He wrote in his journal on that day,

> I find that my civil business has left me, almost entirely … I am afraid however that it is to a great extent my own fault, that I have lost my business … Now how can my Geological or other scientific pursuits advance me in the world? (Paxton House Archives Book 9).

Ruefully conceding that he should have listened earlier to his father's advice, he resolved to make amends and carry on no more scientific investigations. But he would make one exception—he would finish the papers on earthquakes he had already started writing, since it would be a disadvantage not to take the credit for the work already begun.

This appears to be the real reason why the report of the earthquake committee for 1843–1844 was the last one; Milne dropped out to devote more time to rebuilding his legal practice, and the Committee could not survive without him.

It did not last. In 1845 Sir David Milne died, and Milne's resolution crumbled, and he retired almost completely from the legal profession. However, the earthquake committee had now broken up. Milne's only further involvement in seismology occurred late in life when he bundled together his papers on the subject and published them as a stand-alone work in a small edition, chiefly for friends (Milne 1887).

4.4.2 Works

Besides the reports to the British Association already referred to, Milne's publications consist of a series of seven papers published between 1842 and 1844, under the common title "Notices of earthquake-shocks felt in Great Britain, and especially in Scotland, with inferences suggested by these notices as to the causes of such shocks" Milne (1842a,b,c, 1843b,c, 1844)). The subjects covered can be divided into three general headings: Milne's historical earthquake catalogue, his macroseismic investigations, and his theoretical speculations. These will be considered in turn.

4.4.3 Milne's catalogue

Milne's first paper consists of a catalogue of British earthquakes, from 1608 to 1839. This is probably the first attempt to publish such a catalogue since Grey (1750). Milne's sources are the following. For earlier Comrie earthquakes, he drew on the register compiled by Samuel Gilfillan. For Inverness earthquakes (around 1816), papers collected by T Lauder Dick. For the Chichester earthquakes in the 1830s, a report from local investigators (see Neilson et al. 1984b). Otherwise, he drew largely on Philosophical Transactions of the Royal Society, Gentleman's Magazine, and Scots Magazine, plus a number of unacknowledged sources, some of which must have been newspapers. He noted that

> … this register, compiled as it has been chiefly from notices in magazines and other periodicals, must not be too implicitly relied on for the correctness of every particular fact related in it. (Milne 1842a)

The catalogue certainly contains errors; it is occasionally prone to duplicating earthquakes. So for instance, Leicestershire earthquakes appear on both 25 February 1792 and 2 March 1792, both at 20 h 45 m, from different sources—obviously two reports of the same event, one of them misdated. For this reason, there has been a tendency for recent authors to denigrate it; but one should be careful not to view it other than a product of its time. Modern earthquake cataloguers are used to taking existing lists as a starting point; Milne was working largely from scratch, and the compilation of the catalogue must have been a significant labour. Milne's catalogue, in turn, has been a resource for later compilers such as Davison (1924).

4.4.4 Macroseismic investigations

The largest part of Milne's papers published between 1842 and 1844 consists of descriptions of the larger events, especially that of 23 October 1839, collected from correspondents. Milne took pains to give each account in the words of its author, rather than abstracting details, and the result is by far the lengthiest and most complete description of any British earthquake up to that date. Milne sought information about the extent of the 1839 earthquake, and arranged the material he collected by region.

This gave Milne a very good idea of the variation in strength of the shock with distance from Comrie, but he did not take the further step of devising any system analogous to an intensity scale, despite the fact that Macfarlane had already attempted a intensity scale, albeit subjective. In fact, an intensity scale in the modern sense of the term had previously been devised in the 1820s by the German mathematician Egen PNC (1828). It seems that Egen's paper was not widely read, and intensity scales did not come into general use until the 1870s (Davison 1921a; Musson et al. 2010).

Even without an intensity scale, Milne could have attempted a map of the area affected by the earthquake, but did not do so.

4.4.5 Theoretical speculations

In Milne's second paper (Milne 1842b), he begins to attempt to draw conclusions about earthquake phenomena from the material gathered.

4.4.6 Nature of earthquakes

Milne starts by observing that vibration is always felt by observers of an earthquake, concussive shocks only sometimes. He does not refer to Michell, whose works he may not have been aware of. He notices that earthquake vibration is felt most intensely at the centre of the felt area, and infers correctly the existence of a focus, and that transmission of vibration to the edge of the felt area involves an oblique travel path, which will be longer than the vertical path from the focus directly upwards. He concludes from this that it must be possible to determine the depth of focus of an earthquake.

Then follows a fascinating examination of data from both the 1755 and 1761 great Lisbon earthquakes, in which he tabulates the time at which the shock was observed at different places, arranged in order of increasing distance, and similarly with the tsunami observations. From the latter, he observes that the 1755 tsunami crossed the Atlantic at much higher speed than it reached Britain, and correctly concludes (as did Michell) that this is due to differences in ocean depth, and Milne even proposes that the depth of the Atlantic could be estimated by this means.

He repeats the calculations for 1761 and finds the data are compatible. But he finds the 1761 tsunami took longer to travel to Lisbon and Cornwall, and therefore concludes the origin of the 1761 earthquake was closer to the coast, and thus in shallower water. Despite the fact that Milne was anticipated by Michell, whom he seems not to have read, his discussion here reads as uncannily modern.

He questions whether tsunamis are due to the uplift of the seabed or can be produced merely by the effects of vibration, and decides the former is more likely, but points out that only some earthquakes produce such uplift. He had no knowledge of thrust faulting, but his observation is generally correct.

4.4.7 Nature of earthquake motion

Milne's second topic is the nature of earthquake observations, which he divides into three: vertical, horizontal and undulating. Like Flamsteed, he is aware that earthquake motion is strongest in upper parts of buildings, but unlike Flamsteed, he correctly deduces that this is because the top part of a building has greater capacity for movement. He also notes that earthquake shocks are felt more strongly on alluvium, and that this is to be expected from the theory of vibration.

4.4.8 Fissures and subsidence

The third topic considered is the occasional occurrence of fissures in the ground, or of subsidence, which Milne notes are occasionally observed, but he has little to remark on the subject.

4.4.9 Foreign origin of earthquakes

Milne refutes the opinion, often raised in reportage of British earthquakes, that they are the backwash of some distant convulsion. The distribution of effects over a limited area, with the strongest in the centre, is proof that the origin is both local and fairly shallow.

He then tackles the question as to whether there is some connection between earthquakes occurring overseas and those occurring in Britain, and points out a number of coincidences in time. He then infers correctly that since earthquakes are rather frequent in more active parts of the world, it is not unlikely that such coincidences would be observed by chance. Again, this is excellent scientific thinking.

4.4.10 Earthquakes and geology

In attempting to explain why earthquakes are more common in Strathearn and the Great Glen than other parts of Scotland, Milne comes to the conclusion that this is connected to the presence of major faults, and also refers to connections with faults in England and Wales, mentioning the Tyndale and Craven Faults by name. In a prophetic passage, he writes

> If, as most geologists believe, earthquakes are caused by the development of some mechanical power beneath the earth's crust … it is evident, that this power will produce the greatest effect on those parts that are the least able to resist it … it is easy to understand how the upheaving forces should obtain vent only or chiefly in those districts which, in consequence of extensive dislocations, are capable of most easily yielding; and how the indications of these forces should be most distinct along the lines of dislocation (Milne 1842b).

This is not saying that earthquakes are caused by faulting, but it comes close to implying it.

4.4.11 Other phenomena

Milne's remaining sections are less interesting, so can be taken together. Section 6 merely comments on the fact that shocks seldom come singly; under which observation come both the "two shocks" description from P and S arrivals, plus the occurrence of aftershocks. Section 7 describes reported earthquake sounds. Milne had no idea how a sound in the air could be produced by an earthquake, showing he had not read Michell's work.

Section 8 is the inevitable analysis of frequency by time of year, in which he finds that fewer earthquakes occur in the Spring, and that this agrees with results obtained from catalogues by von Hoff and Perrey (showing he was familiar with those authors) and also Peter Merian's observations of earthquakes felt at Basel. However, he had no mathematical insight into the notion of statistical significance. Instead, he concludes that there must be some connection between earthquake frequency and the state of the atmosphere.

This brings him to a consideration of the weather at the time of the earthquakes, which he states in Sect. 9 is generally warm or sultry. He also states in Sect. 10 that fogs frequently occur at the same time as earthquakes. In Sect. 11, he alludes to the appearance of fine dust that appeared over large areas of country in two cases: 1755 and 1837. The first he correctly identifies as the product of an eruption of Katla in Iceland (he uses the name Kötlugia). The latter is more inexplicable, and analysis of the dust in question revealed it to be mostly carbon. Rain is discussed in what should be Sect. 12, except that through misnumbering it appears as another Sect. 11. Milne notes that rain frequently falls before earthquakes. Or after them. Or at the same time. Section 12 finds that the barometer is generally low when shocks are severe or frequent, while Sect. 13 discusses wind, thunder and lightning, meteors, and aurora borealis. In all these, there is no apprehension that there is always some weather or other at the time of an earthquake, and that the observations enumerated have no statistical significance.

Milne is finally unable to come to a conclusion as to the causes of earthquakes, but his suspicion is that it is not unconnected with electricity; but not atmospheric electricity, rather discharges from within the earth.

4.5 James Drummond

Anyone perusing tourist literature or local history guides for Comrie will find another name bracketed with Peter Macfarlane as the "Comrie Pioneers". An example will be found at http://www.strathearn.com/pl/earthquake.htm: "With the 'Great Earthquake of 1839' The Comrie Pioneers, postmaster Peter Macfarlane and shoemaker James Drummond, began to keep records." James Drummond was indeed the village soutar (shoemaker) and he did indeed keep his own register of earthquake shocks, independently of Macfarlane's. But I have never been able to establish who coined the phrase "Comrie Pioneers". It has to be a twentieth century invention, and is highly inappropriate, implying as it does that Macfarlane and Drummond were in league in the investigations of the 1839-1846 earthquake sequence. Nothing could be further from the truth.

Macfarlane left no written accounts of his activities besides the register of shocks he kept, and even that only survives in those parts published by Milne. So all that is known about the activities of Drummond derives from his own accounts, which are evidently telling only one side of the story. It is not hard, however, to read between the lines.

Drummond's publications are two in number. The first, Drummond (1842), was published in the Philosophical Magazine, at the height of the Comrie sequence, and contained his register of shocks up to the end of 1841, together with some observations about the nature of the shocks, which he regarded as "gas explosions … of hydrogen or oxygen gas". He apparently believed that they were associated with the flooding of the River Lednock, and originated from where the river passes the northern corner of Comrie Park. He concludes the paper with a dig at Macfarlane, without naming him:

> I am aware that some persons who have been keeping a list of shocks reject the very small sounds, and mark the louder ones as slight shocks of earthquakes, whether those sounds be occasioned by the explosions of hydrogen or oxygen gas or not; it is contrary to common sense to call a slight sound a shock of an earthquake. (Drummond 1842)

Since Macfarlane was the only other list maker, the target is clear.

Drummond's second publication (Drummond 1875) is a pamphlet of self-justification; Drummond evidently waited until Macfarlane was dead and was no longer able to reply (or sue). Again, he refrains from naming Macfarlane, but his meaning is obvious. Drummond's story was that, having started a register of shocks, he came to the conclusion that he had solved the mystery of the origin of the shocks, and that the prevailing theory that the source was somewhere in the mountains north of Comrie was wrong. In 1840 he confided to the local minister, Robert Walker. Walker sent him to Macfarlane, suggesting that Macfarlane should write to Milne on Drummond's behalf. According to Drummond, Macfarlane then offered him a bribe to obtain the credit for Drummond's work.

This makes no sense, since Macfarlane had his own register, and didn't need Drummond's. It may be that Macfarlane offered Drummond some remuneration for putting his register at the disposal of the British Association Committee. According to Drummond (1875), the offer of a bribe bounced Drummond into prematurely publishing his paper in the Philosophical Magazine. After publication, Drummond changed his mind about the gas explosions, and decided the shocks were entirely electrical in nature.

Shortly after Drummond's encounter with Macfarlane, Milne himself arrived in Comrie, and a meeting occurred between Drummond, Macfarlane, and Milne. It appears that Drummond harangued the other two so rudely that both Macfarlane and Milne stood up and walked out of the room. "Considering the position in society Mr Milne occupies," wrote Drummond, "and his reputation as a scientific man, it was not likely that he would readily acknowledge

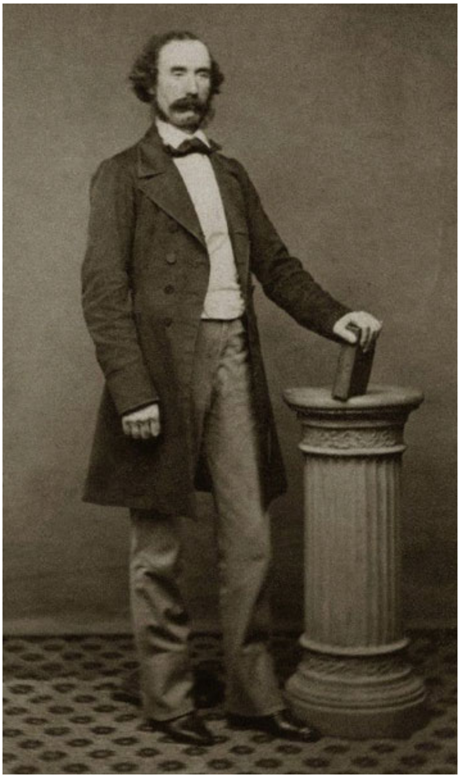


Fig. 14 Robert Mallet (image courtesy of the Dublin Historical Society)

himself outrun in a scientific race by one in the humble station of life in which I am placed" (Drummond 1875).

So much for the "Pioneers".

5 Robert Mallet

Robert Mallet (1810–1881), the first of the two founders of seismology after whom this lecture is named (Fig. 14), was born in Dublin to John Mallet, "plumber, hydraulic engine maker and iron founder". After taking his BA degree at Trinity College Dublin, in due course he joined his father's business as an engineer. A full account of his early life will be found in Cox (1982). His achievements are also reviewed at length by Muir Wood (1988) and Dean (1991).

Mallet's work as an engineer soon developed into scientific interests, including the earth sciences. Work on a dock development at Galway led him to prepare a paper on the igneous rocks revealed in the sections, presented to the Royal Irish Academy in 1837 (Herries Davies 1982). By 1838 he was attending regularly at the meetings of the British Association, and at the time of the formation of the British Association Committee on earthquakes, he was involved in research for the Association on corrosion. At the same meeting that saw the first report of Milne's committee, Mallet was presenting a "Second Report upon the action of Air and Water, whether fresh or salt, clear or foul, and at various temperatures, upon Cast Iron, Wrought Iron, and Steel" (Mallet 1841).

It would be interesting, if one had a time machine, to go back and compare the conduct of a British Association meeting in the 1840s with a modern scientific conference. Was there the same amount of networking in coffee breaks as there is today? There is no evidence that Mallet and David Milne ever talked together, but it is evident that Mallet would have been aware of the investigations of the Earthquake Committee at the time.

5.1 Earthquake mechanics

Mallet's own involvement in earthquake research began later, in the mid 1840s, and seems to have been stimulated initially by the problem of rotational seismology—specifically, whether the rotation of monuments implies a vorticose motion, or whether it can be produced (as Mallet correctly proved) by purely linear motion. His interest in this was sparked by a passage, and engraving, in Lyell's (1837) *Principles of Geology*. The Proceedings of the Royal Irish Academy (1847) gave a report of Mallet's paper in abstracted form. The reporter writes:

> From this single principle, viz., that the true earthquake-shock consists simply in the transit through the solid crust of the earth of a wave of elastic compression, which the author believs to be now, by him, for the first time, enunciated, he proceeds to develope [sic] and account for, in detail, all the more important recorded phenomena of earthquakes, as well as many of the more perplexing secondary phenomena. (Royal Irish Academy 1847)

Of course, Michell had expounded the same thing 80 years earlier, as Mallet was well aware. In the full paper the following year (Mallet 1848a), Mallet is rather snippy about Michell: "Michell wholly mistook the nature of the wave itself; and the existence of such a wave as he assumes is totally inconsistent with the phenomena of earthquake motion …" (Mallet 1848a). As Davison (1927a) later wrote of Mallet's paper, "There is perhaps little that is really novel in the whole memoir …[Mallet's] account of Michell's work is strangely inaccurate and incomplete".

Mallet continued to discuss the production and speed of tsunamis, with essentially the same ideas as Milne a few years previously. Mallet makes almost no mention of the investigations at Comrie, or the British Association reports, except to state that "instruments, though by no means well devised nor self-registering, have been already in use in Scotland" (Mallet 1848a).

To continue with Davison (1927a), the chief merit of Mallet (1848a) "is that it does form an attempt to explain the more important phenomena of earthquakes by the light of one guiding principle, and thus, as he says, to bring them within the range of exact science". It is still a seminal paper in the history of seismology, as Herries Davies (1982) claims. Mallet (1848a) correctly explained rotational seismology, and of particular interest is his summarising definition of what an earthquake is: "a wave of elastic compression, produced either by the sudden flexure and constraint of the elastic materials forming a portion of the earth's crust, or by the sudden relief of this constraint by withdrawal of the force, or by their giving way, and becoming fractured".

Here Mallet gives three possible causes for an earthquake to occur, of which the first two are wrong, but the third is both original and correct. It is a rather frustrating definition to the modern reader. The question is what Mallet meant by "becoming fractured". This sentence is possibly the first proposal that earthquakes are caused by faulting, as faulting is the means by which rocks become fractured. But Mallet does not exactly say this. It appears that Mallet was thinking chiefly in terms of igneous intrusions pushing strata upwards, with fracturing occurring as strata are arched by the igneous mass (Fig. 15).

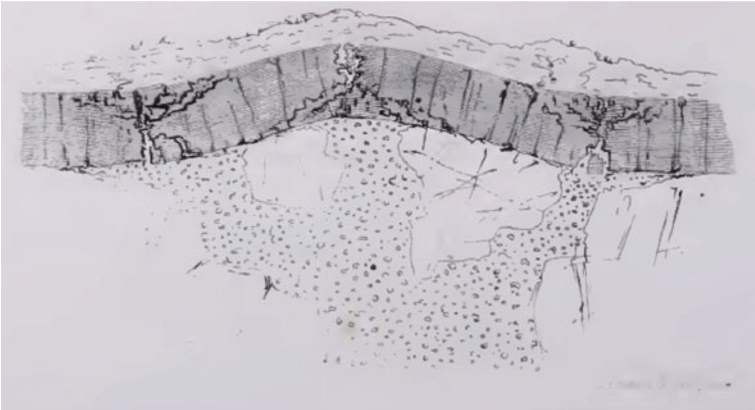


Fig. 15 Mallet's idea of earthquake fractures (Mallet 1848a)

Also of considerable interest is Mallet's proposal that "by earthquake observatories, established with suitable instruments, at distant localities, in South America or Central Asia for instance, where earthquakes, greater or less, are of almost daily occurrence, a very complete knowledge of the time of wave transit … would soon be obtained" (Mallet 1848a). It is clear that what Mallet is proposing is a global seismological monitoring network, that would only be realised 50 years later with the work of John Milne.

5.2 Mallet's seismograph

Mallet followed up this paper on the mechanics of earthquakes with a second one on the design of a suitable instrument to record them (Mallet 1848b). He starts off by calling Forbes's inverted pendulum design "objectionable"—one gets the feeling that Mallet was not a man fond of giving credit to others. As Herries Davies (1982) writes, "he emerges as a man who depreciated the achievements of his predecessors and who seems to have viewed himself as an isolated island of talent set within a sea of mediocrity". Neither was this lost on his contemporaries (Scrope 1874).

Mallet proposes that an instrument for monitoring earthquakes should ideally be able to record onset time to a tenth of a second, vertical and horizontal amplitudes, and the direction of motion.

Mallet's (1848b) design consisted of an instrument built around two vertical and four horizontal tubes of mercury, each arranged to be just in electrical contact with a battery (Fig. 16). The passage of an earthquake wave would disturb the mercury and break the electric contact. This would then cause pencils to make contact with a rotating drum to make a time-registered record. The length of time that contact was broken would be proportional to the amplitude of the wave.

This instrument was never built, but according to Wartnaby (1972), Mallet's (1848b) paper was influential on Palmieri in his design of the first working time-registering seismograph. However, there was an attempt to build it, and for this purpose a new British Association Committee was formed at the 1849 meeting under Mallet (with no reference to the previous one), consisting of "R. Mallet, Esq., Rev. Dr. Robinson, Rev. Prof. Lloyd, and Prof. Oldham" and the objective "to determine by instruments the elements of the transit of natural and artificial earthquake waves", with a grant of £50 (British Association 1850).

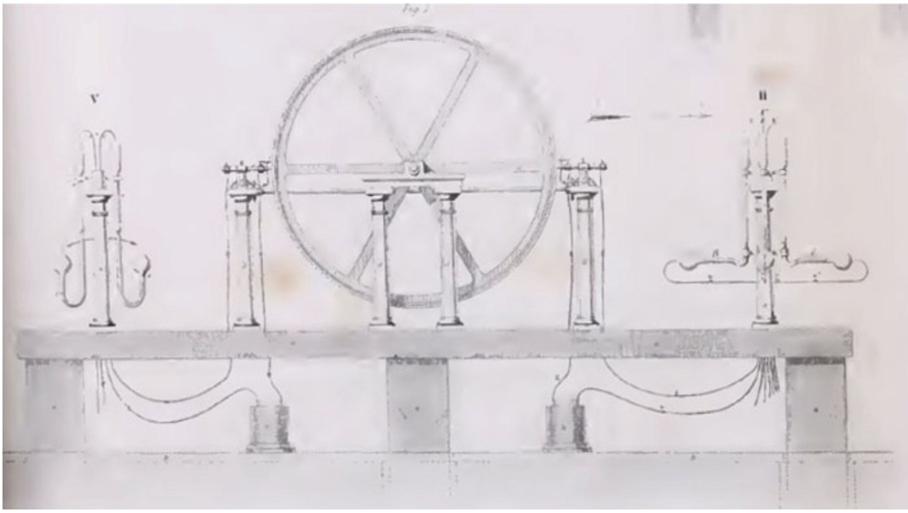


Fig. 16 Mallet's "earthquake registration instrument" (Mallet 1848b)

Ironically, the design of Mallet that did get built was something much less sophisticated, and which suffered from all the faults that Mallet accused Forbes's instrument of having—insensitive and not self-registering (though Mallet did suggest it could be connected to a clock; Mallet 1859). The design was submitted to the Admiralty Handbook, and consisted of two pieces of wood in the shape of a cross, placed flat on a bed of sand (Mallet 1849). On each arm of the cross were to be balanced a number of wooden pegs of varying diameters. The idea was that the stronger the earthquake shock, the more pegs would be thrown down, and falling into the sand, they would indicate the direction from which the shock came.

One such instrument was famously deployed at Comrie in the 1870s (see 6.4 below), and a modern replica can be seen there today. According to Davison (1927a) it was also tried in Japan without success.

5.3 Mallet's catalogue

One of Mallet's most influential contributions to seismology was the compilation of his massive earthquake catalogue, on which he worked together with his son, John William Mallet (1832–1912), who was still a student at the time. Mallet junior shortly afterwards emigrated to the USA, where he was a chemist of some distinction, holding professorships first at Amherst and then Alabama. It has never been resolved just how much of the work on Mallet's catalogue was due to the father and how much to the son. Given that William Mallet (Mallet always calls him by his second name) was studying at Göttingen between 1851–1852, he had the easier access to continental sources.

After the two papers published in the Transactions of the Royal Irish Academy in 1848, Mallet turned his attention to the British Association for the Advancement of Science, and compiled a series of four large reports, of which the first (Mallet 1851) is a comprehensive discourse on the observed nature of earthquakes, reprising to some extent Mallet (1848a). To the modern reader it is remarkable for its inclusion of several pages in Greek in the course of the discussion of ancient ideas on earthquakes—a reminder that at this period it was still considered suitable for an engineer to be versed in the classics.

The second report to the British Association for the Advancement of Science consists of an account of Mallet's controlled source experiments (see 5.5 below) and the introduction to the catalogue (Mallet 1852); the third report is the catalogue itself, and the fourth is Mallet's discussion of it.

The catalogue is in the form of an immense table, printed sideways on the page, beginning in 1606 BC with an earthquake in Sinai related in Exodus xix 18, and concluding at the end of 1842. A note explains that Mallet's original intention was to conclude the catalogue in 1850, but the published work of the French seismologist Alexis Perrey made continuation beyond 1842 redundant. It was initially published in three separate parts, (Mallet 1853, 1854a, 1855), and then reissued in one volume in 1858 (Mallet and Mallet 1858). The whole catalogue is 596 pages in length, and the total number of events is estimated by the authors as "between 6,000 and 7,000".

As a table, it is arranged in 6 columns:

1. Date
2. Locality
3. Direction, duration and number of shocks
4. Phenomena connected with the sea
5. Meteorological and other phenomena
6. Authority

As an example taken at random to show the character of the work, one reads:

1. 1783. Feb. 13.
2. Neustadt in Hungary
3. Some slight vibratory shocks.
4. …
5. …
6. Gazette de France, 14 Mars; Cotte; v. Hoff

Gazette de France is a newspaper source often cited in the catalogue. "Cotte" refers to Cotte (1776), and "v. Hoff" is the earthquake catalogue of von Hoff (1840). "Neustadt in Hungary" refers to Wiener Neustadt, now in Austria.

The complete list of sources used by the Mallets is long, but not exhaustive. One major omission is the earthquake catalogue of Bonito (1691), which was apparently overlooked when one of the pair visited Naples, owing to the deficiency of the library catalogue at the Mineralogical Museum. Mallet and Mallet (1858) contains a "Bibliography of Earthquakes" arranged by collection, which one may take to be the source list for the catalogue. The collections are:

1. British Museum
2. Royal Society of London
3. Trinity College Dublin
4. Royal Library, Berlin
5. Naturforschenden Freunde of Berlin
6. Royal School of Mines, Berlin
7. Library of the University of Göttingen
8. Royal Library of Munich
9. Royal Library of Dresden
10. Library of Gand, Belgium
11. Library of the Mineralogical Museum, Naples
12. Various other sources

One notes the preponderance of German libraries used (convenient for William Mallet) and the notable absence of the Bibliothèque Nationale, Paris.

The Mallet catalogue, and those of Perrey, have cast a long shadow on historical seismology. Even today, when investigating historical earthquake studies in different parts of the globe, it is not uncommon to find that an entry in a modern parametric catalogue of historical earthquakes ultimately traces back to an entry in Mallet or Perrey's work (Albini et al. 2012). Yet the reliability of entries varies with the sources used. In the case of the 1738 earthquake cited above, one may wonder how well-informed the *Gazette de France* was about the affairs of Wiener Neustadt.

As a specific instance, in 1638 "end of the year", Mallet and Mallet (1858) record an earthquake in Chichester that "did great damage", which subsequently appears in later catalogues of British earthquakes (e.g. Davison 1924). I recall looking in vain for any trace of this damaging earthquake in the Sussex County Archives. Mallet's source is German and eighteenth century—the newspaper *Dresdener Gelehrte Anzeigen*, which one might reasonably suppose not to be very accurate about English affairs in the lead-up to the Civil War. Which indeed it was not—the 1638 Chichester earthquake turns out on further research to have been a ball lightning report, not in Chichester, but in Devon (Musson 2009).

Mallet and Mallet (1858) was followed by Mallet's "Fourth Report", much of which is a lengthy discussion of the catalogue, and what can be inferred from it (Mallet 1859). The first paragraph is notable for being the first appearance in print of the word "seismology". The sentence reads "The pressure of other occupations [have delayed the catalogue, but the delay] has enabled me … to derive advantage from the contemporaneous labours of the few physicists who are engaged in Seismology; foremost amongst whom stands M. Perrey of Dijon." One notes that the word is not introduced as a novelty, nor is it italicised, whereas two paragraphs down, when Mallet introduces the word "seismologue" for an earthquake catalogue (a coinage that has not caught on), the word is italicised. It suggests that the word "seismology" may have already been in use informally before Mallet committed it to print.

Mallet (1859) begins, in fact, mostly by discussing Perrey's regional catalogues, and in particular, a long analysis of such evidence as could be found for periodicity, and links between earthquakes and either the seasons of the movements of the sun and moon. As usual in discussions of this sort, no consideration is given to what variations in numbers between seasons could arise by chance, so these speculations lack any interest for the modern reader.

In the part of this discussion concerning Great Britain, Mallet inserts an apology for neglecting David Milne's work in his previous publications. However, he then manages to criticise Milne for something in which Milne was actually correct—that local earthquakes have local causes, or specifically, "that all British earthquakes have had an origin of disturbance immediately beneath Great Britain, and not at some distant point beyond" (Mallet 1859). Mallet argues that "local variable surface displacement … arise, amongst other reasons, from the heterogeneous and dislocated materials of the earth's crust perturbing the elastic wave". This is not very logical—a shock felt only in one region and not in adjacent regions should originate in that region, as Milne understood. Mallet tries to get round this to some extent by suggesting that small local shocks felt in only one or two localities (such as Comrie), are not really earthquakes, "perhaps no more than tremors, more or less forcible at the surface, due to the fracturing of rocky masses below" (Mallet 1859)—but it is difficult to see how, given Mallet's ideas on earthquake origins, this makes these "tremors" something other than earthquakes.

If Mallet's (1859) discussion of earthquake periodicity has little to offer the modern reader, his section "On Seismic Energy in relation to Time" is much more interesting. Here Mallet more or less invents the modern idea of catalogue completeness. "… up to, and even beyond the Christian era, no record of earthquakes exists for any portions of the earth's surface, except for limited areas of Europe and Asia, and a still more restricted patch of Northern Africa, and … Japan …" (Mallet 1859). In the latest survey of world historical earthquakes (Albini et al. 2012) one can find almost exactly the same conclusion. Mallet continues, "Yet, of the enormously larger areas of the outer and unknown world since discovered, it is not to be supposed but that there was a proportionate … amount of earthquake energy, though not recorded …". Mallet (1859) is quite explicit that temporal variations in the number of earthquakes are due to improved historical records only. In one of the most startlingly modern pronouncements in Mallet's writings, he says, "If, however, the curve of total energy … be examined and compared by a broad glance with the great outlines of human progress, the conclusions appears sufficiently warranted, that during all historic time the amount of seismic energy over the observed portions of our world must have been nearly constant." It is particularly notable that Mallet here talks about seismic energy, and he explicitly states that he does not attempt, in the catalogue, to give every minor event, as this would distort the numerical count and give some districts a higher apparent importance than they really merited.

Having discussed the distribution of earthquakes in time, Mallet (1859) turns to distribution in space, and initiates a discussion of his world earthquake map, which is surely one of the great achievements of Victorian seismology (Mallet and Mallet 1858). The bands of seismicity are for the most part, so accurately plotted (Mallet of course had no way of tracing the mid-oceanic ridges) that one could almost work out plate tectonics from examination of the pattern. An early draft of the map was shown to the Royal Irish Academy in 1852; again it was a joint production of father and son (Herries Davies 1982).

It is sometimes assumed that this was the first map of world seismicity (e.g. Ben-Menahem 1995), which interestingly is not the case. Two predecessors are Berghaus (1845) and Johnston (1849). Mallet knew of both of these, since he refers to them (and criticises what he thought was their weak points), and the style of shading for more seismically active areas is similar in all three. Both the maps of Berghaus (1845) and Johnston (1849) are plates in thematic atlases rather than independent works, and both include both volcanoes and earthquakes on the same maps (as does Mallet's). Berghaus's (1845) map only covers the Atlantic hemisphere; his equivalent map for the Pacific has volcanoes but no earthquakes. So the first actual map of seismicity for the whole world (Fig. 17) seems to be that of Alexander Keith Johnston (1804–1871), a Scottish cartographer, who would attract more attention in this monograph if his map had been accompanied by any writings of a seismological nature.

But if Mallet's map is not the first, it is certainly the best, and could hardly be otherwise, given how much more data he had available than either Berghaus or Johnston (Fig. 18). The original is in the archives of the Royal Society.

Mallet has some interesting remarks about the mapping of earthquakes. Whereas the modern seismologist thinks first and foremost of plotting epicentres, Mallet was more interested in defining an earthquake in terms of the area affected by the earthquake wave, which he allows to be strictly indeterminate, given that an earthquake wave from a large enough earthquake can be recorded instrumentally at any distance—this was long before the first actual teleseismic earthquake recording, but Mallet was aware that in theory at least, such a thing was possible. Therefore for practical purposes, he considers that an earthquake is defined by the area where the shock can be felt without instruments.

He proposes a threefold classification of earthquakes that anticipates that of Milne (1912): "great earthquakes", which cause destruction over a wide area and produce major effects on the landscape; "mean earthquakes", which have a large felt area, but cause much less

Fig. 17 Johnston's map of world seismicity (Johnston 1849, courtesy of the David Rumsey collection)

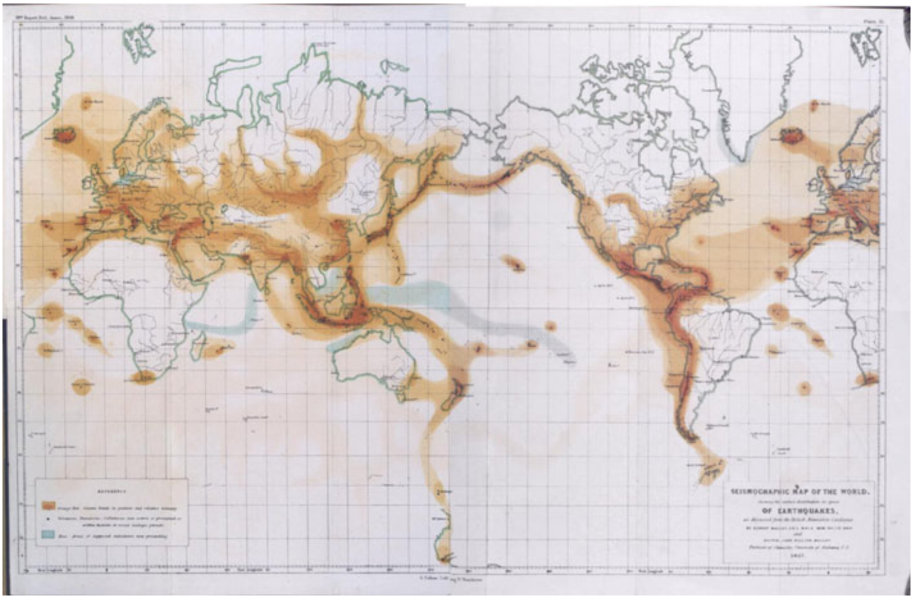


Fig. 18 Mallet's map of world seismicity (Mallet and Mallet 1858)

destruction and have few casualties, and "minor earthquakes", which leave few traces of their passing.

The remainder of Mallet's fourth report is a summing up of the state of the art of seismometry, in which Mallet concedes that his previous conviction that producing an effective instrument was overly optimistic. He discusses various designs in use, including that of Palmieri, and recounts meeting Palmieri and seeing the seismograph he designed. He also proposes some principles for siting instruments

> If several seismometers be set up in the area, they should all be placed on corresponding formations, either all on rock, or all on deep alluvium. The rock, when attainable, is always to be preferred. Three seismometers, at as many distant stations, will be generally found sufficient, if the object be chiefly to seek the focal situation and depth (Mallet 1859).

This is followed by some principles for calculating the seismic focus from observations; the first introduction of the term "focus" into seismological usage. The report concludes with several appendices. The first is a chronological table of human achievements, intended to give some context to the catalogue. This is followed by a transcript of a letter describing the New Zealand earthquake of 1855. The third appendix is Mallet's bibliography of earthquakes, which serves not only as an indication of what the Mallets consulted, but as an interesting guide to historical source materials that still can usefully be consulted today.

Appendix 4 is a catalogue of Perrey's memoirs on earthquakes that were in print up to that date—supplied by Perrey himself.

Finally, Mallet (1859) has a section on "ill-understood phenomena", under which heading he discusses: "great sea-waves" (tsunamis), "doubtful great sea-waves" (ghost storms), "stoppage of rivers", "nausea at the moment of shock", and lastly "indirect estimation of the force due to the shock" (a collection of descriptions of great explosions).

5.4 Investigations of earthquakes

Mallet had two occasions to make practical investigations of earthquakes himself: one British event and one foreign. The latter was the 16 December 1857 Neapolitan earthquake, and Mallet's investigation of it is a landmark in the development of seismology. The earlier event was the Carnarvon earthquake of 9 November 1852, Mallet's study of which is less well known.

5.4.1 The earthquake of 9 November 1852

The earthquake of 1852 was one of the largest in the UK, with an estimated magnitude of 5.3 ML (Musson 1994). It greatly resembles in effect the 19 July 1984 earthquake in the Lleyn Peninsula, magnitude 5.4 ML (Turbitt et al. 1985). Like the 1984 event, the 1852 earthquake was significantly felt in Dublin, and Mallet himself was woken by the shock at his house in Glasnevin, just outside the city (Mallet 1854a,b). Together with Edward Clibborn (a member of the Royal Dublin Society, whose other, rather tenuous link to seismology was being a friend of the geologist Thomas Oldham, later to compile a famous catalogue of earthquakes in India), Mallet collected observations of the earthquake. Mallet's approach to gathering data consisted of publishing a request for information in several newspapers (his letter was declined by "the leading London journal"), and requesting that the Dublin police on duty be questioned as to their experiences. In addition, Clibborn gathered such newspaper accounts as he could (Mallet 1854b).

The results were tabulated by Mallet using a format similar to that of his catalogue, the column headings being locality, apparent direction, number of shocks, duration and time, observed phenomena and authority. All in all, he obtained observations from 24 named places in Great Britain (some of which are run together in one entry), and 25 in Ireland (again some, like various suburbs of Dublin, are treated as one entry). The entry for Dublin is as long as all the other Irish entries run together.

Mallet also produced a map, shown here as Fig. 19, in which he coloured places where he knew the earthquake was felt in red, added the time of observation (if known), and with an arrow indicating the directions of shock (if known).

Mallet considered that the most important things to observe were:

1. The direction of emergence of the wave
2. The time of its emergence
3. Its velocity
4. "The dimensions of the wave, or rather its altitude …" (we would say amplitude).

He notes that the observations collected shed little light on any of these—"such must ever be the case until self-registering seismometers are to be found in all our observatories" (Mallet 1854b).

From the observation of the fall of a picture in his office, he concludes that the angle of emergence in Dublin was from south to north (with a slight westerly inclination) and upwards at an angle of 25–30° to the horizontal. This is despite acknowledging later on in the paper that local directions could be altered by various factors (he cites the complexity of local geology). With similar inconsistency, he criticises the Dublin policemen for giving various descriptions of the direction of the shock, as a result of their being untrained observers.

Mallet (1854b) is not much interested in intensity, though he notes that the strongest effects were in the vicinity of Shrewsbury. (In fact, the intensity distribution was capricious, as is common with relatively deep-focus events. There were strong effects in north-west Wales, but they were not widely reported, unlike the damage that occurred in Shrewsbury.)

From the direction of the shock, and from the observation that the effects were strongest at Shrewsbury, Mallet comes to the remarkable conclusion that the origin of the earthquake was deep below the Canary Islands. Mallet seems to have been under the impression that the earthquake wave would be confined to a single narrow ray, rather than spreading equally in all directions. He also conjectures that "an intermediate point of convulsive energy exists in or about the latitude of Lisbon", in support of which he notes that the noise preceding the shock resembles that observed in Portuguese earthquakes, and that a minor earthquake was felt in Malaga one or two days prior to the 9 November quake (by Mallet's reckoning—in fact it occurred on 3 November). Mallet's reasoning is hard to follow.

He concludes with an assertion, based on data from Perrey (1849), that British earthquakes originate from various centres more distant than the Canaries, contrary to Milne's (1842a,b,c) correct conclusion that British earthquakes were due to local origins.

5.4.2 The earthquake of 16 December 1857

Mallet's second opportunity to study an earthquake first-hand came 5 years later, when a highly destructive earthquake struck the area north-east of Naples. On hearing about the disaster, Mallet wrote to the Royal Society requesting a grant to travel to Italy, stressing the advantages to science of having a trained observer examine the effects of the earthquake, and the need for prompt action. His application was supported by Charles Lyell and others, and the required funds were forthcoming (Cox 1982; Ferrarri and McConnell 2005).

Fig. 19 Mallet's map of the 9 November 1852 earthquake (Mallet 1854b)

Accordingly, Mallet left London on 27 January 1858 and travelled south through France, stopping off at Dijon to see Perrey, before heading by sea from Marseilles to Naples, where he arrived on 5 February. He spent the next few days arranging for permission to visit the interior, obtaining safe passes, recruiting an interpreter and assistants, procuring equipment,

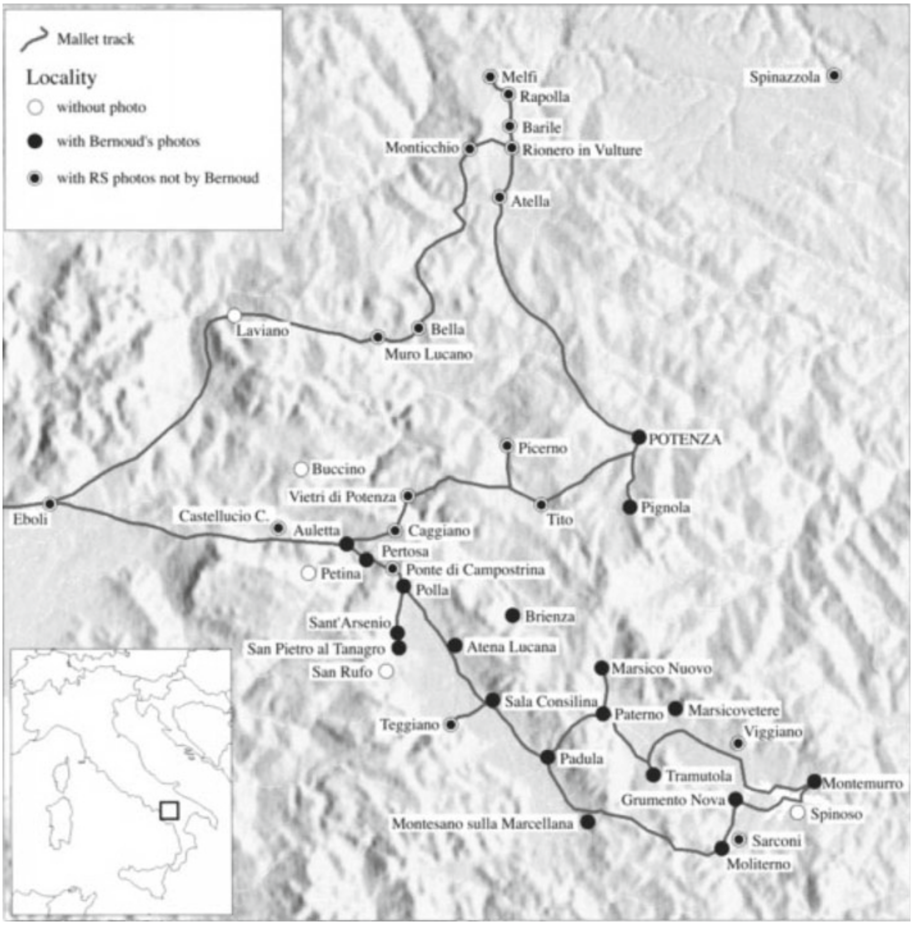


Fig. 20 Mallet's itinerary, from Ferrarri and McConnell (2005)

provisions and transport to enable him to travel through rugged terrain at the height of winter, in an area devastated by the recent earthquake. He also stopped by to talk to Palmieri.

Mallet spent the next few weeks visiting the affected areas, camping out in winter weather and collecting data on the effects of the earthquake. His itinerary has been reconstructed by Ferrarri and McConnell (2005), whose map is shown here as Fig. 20. He arrived back in Naples on 28 February and wrote a letter to Lyell, describing his successful results despite the considerable hardship ("very rough work, all much riding with much rain and the cold at night intense"). He regrets not being able to take a photographer with him. The photos that had been taken by a French photographer working in the area, he complained, were not of use to science, and he resolved to try and arrange for someone to return and take photos according to a list of desirable views that he compiled. These photos were ultimately taken (many as stereographs) by Alphonse Bernoud and perhaps a second photographer identified only as "Grellier". The history of the photos is confused, and a full account, so far as can be traced, is given by Ferrarri and McConnell (2005). The confusion is all the more regrettable

Fig. 21 Damage at Pertosa—photograph by Bernoud (or Grellier) (BGS archive image)

considering that these are the first photographs of earthquake damage ever taken. An example is shown as Fig. 21, showing the village of Pertosa, close to the epicentre.

Mallet then headed back towards Potenza by carriage to visit the northern parts of the affected area, arriving at Rome on 11 March, returning to Britain at the end of the month.

Getting his report on his investigation published turned out to be almost as hard an ordeal for Mallet as the investigation itself. The MS submitted to the Royal Society consisted of 700 pages of text, another 100 of appendices, plus maps, plans and illustrations. The tortuous route to final publication is chronicled by Ferrarri and McConnell (2005); it eventually emerged as a two-volume monograph (Mallet 1862). Davison (1927a) remarks that no earthquake report was to exceed it in richness of material until the official investigation of the 1906 San Francisco earthquake. Mallet's (1862) title is significant; he called the work "Great Neapolitan earthquake of 1857: The first principles of observational seismology", with the subtitle in much larger type. Mallet saw his work as founding the scientific methodology for the study of earthquakes, more than being merely a record of a single earthquake.

If, in his study of the 1852 earthquake, Mallet had to rely, for observations of the direction of shock, the single fall of a picture (aside from subjective estimations), in Basilicata he was faced with the opposite problem, as all around was a confused mass of destroyed masonry. Mallet attempted to find the average direction of fall at a total of 78 places. This proved to be fruitful; it was later shown by Omori (1913) that the average of a large number of observed directions tends to give a very good approximation to the true direction.

This was borne out when Mallet plotted the estimated directions, and found they converged strongly at a point close to the village of Caggiano. Figure 22 is a simplified version of Mallet's map, from Davison (1905). In addition to the directions of shock, Mallet plotted four isoseismals, of which the inner three are shown in Fig. 22. His intensity scale runs as follows (my synopsis):

1 Towns largely destroyed
2 Heavy damage in part, with fatalities
3 Slight damage, no loss of life
4 Felt

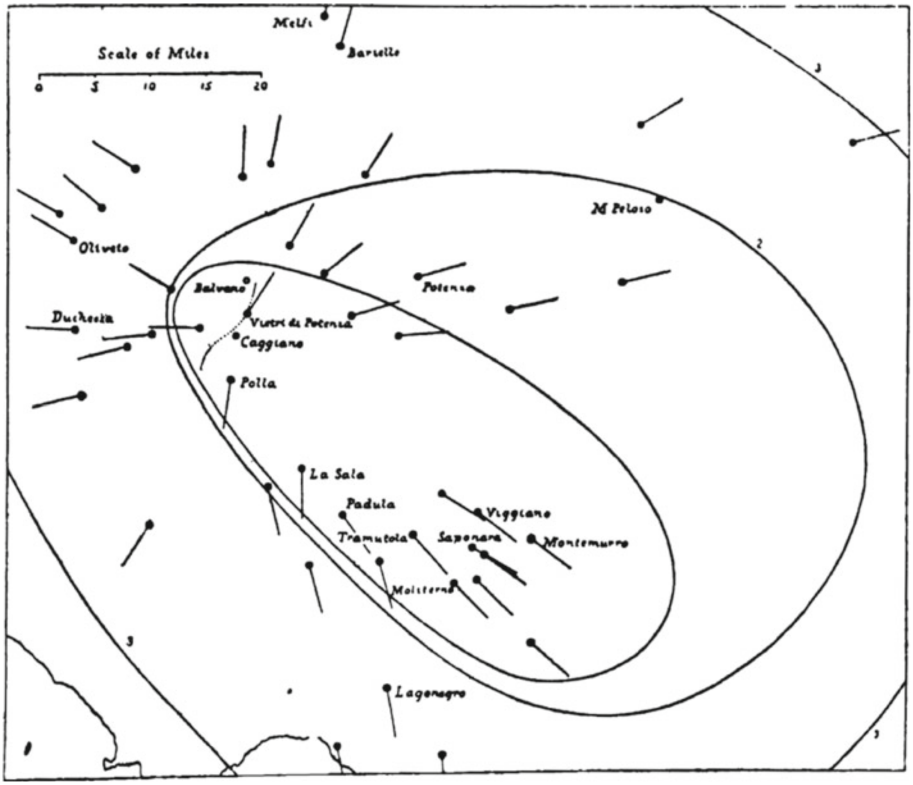


Fig. 22 Detail of Mallet's map of the 1857 earthquake, redrawn by Davison (1905)

This is not the first construction of an isoseismal map—the early history of isoseismal maps is traced by Varga (2008)—but it is the first use of the word "isoseismal", another Mallet coinage.

Mallet was convinced that the convergence of the lines of direction gave him the true epicentre (a word he did not coin; it was introduced by JFJ Schmidt in 1874), but was puzzled by its eccentricity to the innermost isoseismal. He attributed this largely to the structure of the countryside (Mallet 1862). He then attempted to estimate the focal depth by estimating the angle of emergence at difference places from the pattern of damage, arriving at a mean value of 10.6 km (not an unreasonable value). As Davison (1905) points out, the problem with this method is it assumes that all ray paths from the focus to the surface are straight, which may not be the case.

5.5 Controlled source seismology

Though writers before him, starting with Michell, had attempted to estimate the speed of earthquake waves from reported data, Mallet was the first person to attempt to measure the speed of shock waves through the ground in a controlled experiment. He undertook a remarkable series of experiments, starting in 1849, soon after he first became interested in earthquake phenomena, in which he measured the speed of waves produced by the explosion of gunpowder charges. His stated objective was to measure the extreme limits of wave speed, by repeating the experiment in the softest and hardest media available (Mallet 1852).

His first attempt was carried out at Killiney Bay, just south of Dublin, using a mercury seismoscope to detect the shock wave, a Wheatstone chronograph to register the time of the explosion, and a Sharp chronograph to determine the arrival time. In this, Mallet had advice from Charles Wheatstone, who lent him an instrument. This is the first occurrence of the word "seismoscope", another of Mallet's coinages. The setting up of the experiment is described in great detail in Mallet (1852), and he took great pains to make the time observations as accurate as possible, though his set-up did require an observer to watch the seismoscope, and stop the second chronograph manually at the moment the passing of the shock wave was noticed. He was assisted by his son William, who was 18 at the time, in carrying out the experiments. Setting up the experiment involved some preliminary testing; Mallet originally wanted to measure velocity over the course of a mile, but he found that this would require an excessively large explosive charge, so he settled for half a mile (0.8 km) and 25 lb of gunpowder (11 kg), buried 2 m down in the sand.

Mallet made a further check on the accuracy by repeating the shot eight times and taking the average result, achieving a result of 276 m/s, with slight corrections needed for delays occasioned by the operation of the firing and observing equipment.

The Killiney Bay experiments provided Mallet with an estimate of wave velocity in sand, which he took as his minimum value. For a maximum, he settled on granite, and chose a site on Dalkey Island, not far away. Setting up the experiment took some effort, as suitable holes for the gunpowder charges had to be bored in the granite to a depth of 4 m. The labour involved can be judged from Mallet's (1852) detailed descriptions. Mallet made ten shots in all, and divided the results into two groups, according to whether the granite was fissured or intact, obtaining velocities of 396 and 506 m/s respectively, once corrections had been made for timing delays.

Mallet later returned to these experiments; this time, having tested sand and granite, he now wished to examine velocity in stratified rocks, and realised he could take advantage of blasting works already in progress for the purpose of harbour construction at Holyhead in Anglesey. This blasting involved much larger charges of explosives, some of the blasts involving 9,000 kg of gunpowder backed into the cliff face. Mallet's attention was first attracted to the Holyhead operations in 1853, but it was to be 1856 before Mallet was able to commence work on setting up his instruments. Foreseeing the need for frequent trips between Dublin and Holyhead, Mallet requested free passage for himself, his assistants and equipment from the company running the Kingstown (Dun Laoghaire) to Holyhead ferry—perhaps not very surprisingly this request was turned down "which greatly increased the expenditure for these experiments" (Mallet 1861). It says something of Mallet's character not only that he should make the request, but record its refusal, perhaps wishing the obloquy of posterity upon the company's name.

Mallet's problem at Holyhead was that, whereas at Killiney and Dalkey he had full control of the explosions, at Holyhead he was dependent on the timetable of the harbour excavations, which meant that he often had very short notice of the time of the next explosion, and the long intervals between blasts made it difficult to maintain his monitoring equipment in order. In all, he was only able to make six observations over the period 1856 to 1861. His final result was a wave velocity of 332 m/s, as expected, between the results for sand and broken granite. He also concluded that a larger explosion resulted in a faster wave velocity.

Mallet himself was surprised at the low velocities he obtained, given that he could compare these with known elastic constants. He attributed the slow speeds to the inhomogeneity of the rocks; more likely he over-estimated the ability of his instruments to detect first arrivals from the explosions (Prodehl and Mooney 2011). More realistic values were not obtained until the work of Henry Abbot in the USA in 1876 using rather more sensitive instruments

(Abbot 1878; Prodehl and Mooney 2011). Abbot sent Mallet a copy of his results via William Mallet; Mallet was not impressed. “I cannot but regret the experiment was ever attempted, as the circumstances of the case are so ill suited to the determination of so delicate a physical question as, in my judgement, to be rather calculated to retard than advance our knowledge of observational seismology” (Mallet 1877).

6 British seismology between Mallet and Milne

While it has been a theme of this monograph that prior to John Milne, developments in British seismology were largely driven by events, the inverse was not always true. The strong earthquake on 17 March 1843 that struck north-western England, probably with epicentre in the eastern Irish Sea (5.0 ML) received a mention from David Milne (British Association 1844) but no study. The two largest British earthquakes of the mid-nineteenth century were those of 9 November 1852 (Caernarvon, 5.3 ML) and 6 October 1863 (Hereford, 5.2 ML). The study of the former by Mallet (1854b) was not Mallet’s finest piece of work (Sect. 5.4.1 above); the study of the latter by Lowe (1864a) is a more accomplished piece of work (Sect. 6.3.2 below). But neither the Neath earthquake of 30 October 1868 nor the Appleby earthquake of 17 March 1871 (both 4.9 ML) attracted much scientific interest. The largest Scottish earthquake, the Argyll event of 28 November 1880 (5.2 ML), did result in a study by Stevenson (1881), dealt with here in Sect. 6.5. But the most important earthquake of the period, due to its high intensity, was the Colchester earthquake of 22 April 1884. By good fortune, this attracted the attention of two highly capable investigators, and while the field study of Meldola and White (1885) resulted in no theoretical advances, it was an excellent piece of work, illuminating the effects of this most damaging of British earthquakes (Sect. 6.7 below).

6.1 Hopkins

The mathematician William Hopkins (1793–1866) is distinguished by having brought mathematical analysis into geology, defining the discipline of physical geology. It is said that he used to complain that geologists couldn’t understand his mathematics, and mathematicians refused to be interested in his geology (Davison 1927a).

His memoir on earthquakes at the 1847 meeting of the British Association for the Advancement of Science (Hopkins 1848) just follows Mallet’s first paper (Mallet 1848a), which was to overshadow it. Like Mallet, Hopkins was well aware that earthquakes took the form of waves in an elastic medium, and could be treated mathematically as such. Hopkins goes to great lengths to try and determine the structure of the earth, concluding that the solid crust must be at least a quarter or a fifth of the total radius of the planet, and that volcanoes rely on individual magma chambers within the solid crust, rather than tapping a common source of molten material below a very thin crust.

Hopkins, like Mallet, comes close to establishing a relationship between faulting and earthquakes without ever saying it. He discusses the relative displacement of strata at a fault, noting that the most common configuration is what we would now call normal faulting. He argues that elevations in the earth’s crust are as a result of internal pressures produced by fluids, and recognised that these could be resolved into three mutually perpendicular directions (that we would now call S_1, S_2 and S_3). He then discusses the “formation of fissures”, which Hopkins considers as the “primary phenomena of elevation”. It is not totally clear what Hopkins means by “fissures”. He states that “The same parallelism … [that] must frequently characterize systems of fissures … will be equally applicable to faults, dykes, mineral veins &c" (Hopkins 1848). So "fissures" are not faults. Perhaps he had in mind the sort of chasms that open up in what one can perhaps call "B-movie earthquakes".

However, Hopkins states that "the instant a fissure should commence, a modification of the state of tension of the mass in the immediate vicinity would take place almost *instantaneously* … The formation of the fissure would, in fact, cause a *vibration* … and the corresponding modifications of the tensions would be propagated with the same velocity as the vibration itself" (Hopkins 1848). This is the clearest statement yet on the causation of earthquakes, and if Hopkins had only used the word "fault" in place of "fissure", he would be more celebrated in the history of seismology.

When Hopkins turns to earthquakes themselves, his opening remarks are worth quoting at length.

> … these vibrations would be of great intensity near the regions where they originated, and it is possible that they might extend, in such cases, to very extensive portions of the globe, before their intensity should become sufficiently weakened to be no longer sensible. These secondary effects of the great elevating forces … not being calculated to produce any permanent modification of character in the rocks through which the vibrations were transmitted, are matters of little interest to geologists as regard their existence at remote epochs; but they become, on the contrary, of especial interest when considered with reference to modern earthquakes (Hopkins 1848).

A good antidote to Hooke.

Hopkins then continues with the theory of vibration, and, with reference to the work of Poisson, Cauchy and Stokes, identifies that vibrations propagating through a mass would take the form of waves in which a particle moves in the line of propagation and transverse to it, and that these waves travel at different velocities, and will thus become separated with distance. Or what we would now distinguish as P and S waves.

He now proposes a method for determining the epicentre, and makes the very astute observation that the apparent direction of shaking from a shear wave is not the direction of the wave, but at right angles to it. This point seems not to have been taken up by many subsequent writers. He recommends, therefore, taking the direction of the wave at the first instant of motion—an uncanny anticipation of modern first-motion readings.

He initially considers cases where an earthquake originates from a small source, "as would be the case, for instance, if the shock were produced by a deep-seated volcanic explosion, the falling in of the roof of a subterranean cavity, or the sudden rending of the solid rock around it" (Hopkins 1848), and then goes on to more complex cases where the source is large, and the shock therefore felt almost simultaneously over a large area.

Hopkins was perhaps unfortunate, firstly in being upstaged by Mallet, and secondly, in being remembered more for what he got wrong than what he got right. But his paper on earthquakes is remarkably prescient, and one is inclined to agree with Davison's (1927a) judgement that it is to be regretted that he never returned to the subject.

6.2 Lyell

One of the problems in any sort of historical narrative is that it is easy to slip into the habit of thinking about prominent individuals in isolation. Thus one can think and write about Mallet and Mallet's work, and Perrey and Perrey's work, without realising that the two of them were good friends who maintained a steady correspondence. Charles Lyell (1797–1875) is hard to place in a chronological narrative, as he greatly overlaps with the careers of other names in this monograph.

His background partly resembles that of David Milne, being Scottish-born and training originally as a lawyer, though whereas Milne was called to the Edinburgh bar, Lyell entered Lincoln's Inn in London. After developing eye problems, he gave up law and took up geology full time.

He is best remembered for his four volume "Principles of geology", the success of which can be gauged from the fact that it went through twelve editions between 1830 and 1875, the third edition of 1834 being the first full four-volume set. The book was important in establishing Hutton's ideas on uniformity as geological orthodoxy; the influence of Lyell's work can not be overestimated.

Lyell devotes several chapters to earthquakes, mostly a series of descriptions of particular events. Like Hooke, he is interested in earthquakes as a geological force changing the landscape; thus he concerns himself with major earthquakes that were accompanied by prominent upheavals or subsidence, such as the 1783 Calabrian earthquake or the 1819 Rann of Kutch event.

In dealing with the former event, he reproduces an engraving showing two obelisks from the façade of the convent of St Bruno in Stefano del Bosco, where the individual stones were rotated and displaced while the pedestals were unmoved. He wrote, "It appears that the wave-like motions, and those which are called vorticose … often produced effects of the most capricious kind" (Lyell 1837), and cited the rotation as an example: "The shock which agitated the building … having been horizontal and vorticose". It was precisely this passage that started Mallet on his seismological career, as he saw that such a rotational effect could be produced without vorticose motion.

Like Hooke, Lyell was not much interested in the mechanics of earthquakes, and he overstates the theme of elevation and subsidence. Thus he writes, "Towns engulphed during one earthquake may, by repeated shocks, have sunk to enormous depths beneath the surface …" (Lyell 1837).

In subsequent editions, Lyell added new accounts of earthquakes, of which the most important was the Wairarapa earthquake in New Zealand on 23 January 1855. Lyell published a separate paper on this event (Lyell 1856), having obtained reports of the earthquake from several eye-witnesses. The history of Lyell's involvement with this earthquake is recounted by Grapes and Downes (2010) who point out that this was the first unambiguous demonstration of the link between earthquakes and faults, thanks to direct observation of fault rupture at the surface. In the last edition of "Principles of geology", Lyell wrote, "The geologist has rarely enjoyed so good an opportunity as that afforded him by this convulsion in New Zealand of observing one of the steps by which those great displacements of the rocks known as 'faults' may in the course of ages be brought about" (Lyell 1875).

This is a more explicit statement that earthquakes and faults are causally related than was made by David Milne or Mallet, but the direction of the causality is not stated directly, and the most obvious reading of the text is that earthquakes cause faulting, rather than the other way about.

6.3 Lowe

Edward Joseph Lowe (1825–1900) was a man of wide scientific interests, notably meteorology, astronomy and botany (Fig. 23). He began his scientific career at the age of fifteen, making meteorological observations at Nottingham, which he continued for the next 42 years until he retired to Chepstow. His obituary notice (Royal Astronomical Society 1901) credits him with at least 46 published papers on a wide variety of subjects, and as a botanist, he

Fig. 23 EJ Lowe (courtesy Royal Astronomical Society)

is remembered for his work on ferns (Lowe 1864b). He was also a founder member of the Meteorological Society.

His involvement in seismology comes under three headings: his observatory, his study of the 1863 Hereford earthquake, and his earthquake catalogue.

6.3.1 Beeston observatory

Lowe's father, Joseph Lowe, was a wealthy businessman, and who appears also to have had an interest in science that he passed down to his son. It was evidently Joseph who built the strange octagonal tower at Beeston in the 1840s, on flat land near the River Trent. The tower was eventually demolished in the 1960s, but a surviving photo (Fig. 24) shows it to have developed a pronounced list, no doubt due to soft ground and inadequate foundations.

The tower served as a meteorological observatory, though Lovell and Henni (1999) suggest that its construction as a high tower was deliberately conceived to accommodate a pendulum for detecting earthquakes—as Forbes (1844) pointed out, the use of a common pendulum requires a very large instrument. The one at Beeston, described by Lowe (1864a), was installed

Fig. 24 Beeston Observatory in the mid-twentieth century (BGS archive image)

in 1850 and consisted of a 30′ (9.1 m) wooden rod suspended from the upper part of the tower. The pendulum had a brass weight as bob ("the size of an orange"), to which was fixed a steel pointer. Registration was done by means of the pointer tracing a furrow in a bed of chalk dust as the pendulum moved.

6.3.2 The Hereford earthquake of 1863

With an estimated magnitude of 5.2 ML, the earthquake of 6 October 1863 was one of the largest onshore British earthquakes on record (Musson 1994). The epicentre was in the Golden Valley, south-west of Hereford, and though the earthquake was felt over most of England and Wales, there was not much damage, indicating that the focus was not shallow.

Lowe set about making a survey of the effects of the earthquake. To do this, he relied on three sources of information. First, he gathered as many newspaper reports as he could. Second, he wrote an appeal for information which appeared in the letters column of the Times. Lastly, and rather interestingly, he applied to the Midland Railway Company for information from stationmasters. This gave him a set of observers who were not self-selecting, and therefore not biased towards those who felt the quake the most strongly. As a result, he was able to collect information on where the earthquake had positively not been felt, which was of great benefit in defining the boundary of the felt area (Lowe 1864a).

In the appendix to his paper, Lowe (1864a) listed a synopsis of all the information he had collected, arranged by county. He then produced two maps, one based on a street plan of

Fig. 25 Lowe's map of the 1863 Hereford earthquake

the city of Hereford, showing the apparent direction of motion (correctly perceived as being from the west), and a map of the whole felt area (Fig. 25), in which he uses a polygon to trace the limits of observation, and a shaded region for the area "where it was most severely felt", thus giving a very crude and subjective division of intensity.

The shock was recorded by Lowe's pendulum at Beeston, the pointer tracing an oval $1/2''$ (1.3 cm) long. It also produced a thickening of the line on six meteorological drum recorders at the same instant. This was therefore the first English earthquake to be instrumentally recorded, excepting some uncertain records obtained by Lowe between 1852 and 1854, which cannot be associated with any felt observations (Perrey 1865).

6.3.3 Lowe's catalogue

In 1870, Lowe published what was intended to be the first part of an ambitious chronology of natural phenomena, entitled "Natural phenomena and chronology of the seasons". Lowe's intention was to produce two volumes, one on "Chronology of the seasons in the British Isles", and a second on "Chronology of the seasons of foreign countries". The first volume contains a note to the effect that it would be published in three parts in quick succession, but only the first ever appeared in print.

The plan of the work was similar to that of Short (1749), of which Lowe seems not to have been aware. It takes an inclusive approach to natural phenomena, and opens in the year 220, the first entries being for frost, frost, inundation, famine, frost, famine, famine, frost, flood, frost, earthquake, earthquake, earthquake, plague, frost, and in 526, dry fog "accompanied by earthquake and volcanic eruptions". These very early earthquakes are not actually British earthquakes at all, but large European events that were originally described as affecting "nearly the whole world" (i.e. most of the Eastern Mediterranean). Lowe takes the expression literally and concludes they must have extended to Britain.

Lowe prepared the work from a perusal of various local histories and periodicals, mentioning Gentlemen's Magazine and Annual Register as two of the most important. He writes, "the British Museum contains much requiring examination, but the author has found a complete research in that gigantic library a Herculean task" (Lowe 1870).

The level of scholarship is not high, and sources are seldom given. For earthquakes he seems to have relied heavily on Mallet's catalogue. The record ends in the middle of 1753.

6.4 Further investigations at Comrie

On the evening of the 8 May 1867, an earthquake shock rattled the area around Comrie once more, after two decades of more or less quiescence. It was not a large event, estimated at 3.0 ML in Musson (1994), but it was sufficient to re-awaken interest in earthquakes in Scotland, and the British Association Committee reformed, and made the first of a new series of reports at the 1870 British Association meeting. The Committee now consisted of Sir William Thomson (later Lord Kelvin), David Milne, now David Milne-Home, Macfarlane, and James Bryce, who now took over the running of the Committee.

Bryce was dissatisfied with the huge Forbes instrument, still installed in the tower of the parish church at Comrie. The height of the instrument made it very difficult to see the recording disc, which was of course right at the top. Also, the instrument was too insensitive to record the smaller shocks that were being felt in the village (British Association 1871).

The second report of the reformed Committee, which now included J Brough, a Comrie shopkeeper (and Macfarlane's nephew by marriage, who had taken over Macfarlane's store), reports little progress, beyond a mention of the Spean Bridge earthquake of 9 March 1869, and a suggestion by Bryce that it would be useful to negotiate with the Meteorological Society with a view to establishing seismometers at each of their observing stations, an interesting proposal for a wider seismological monitoring network, though nothing came of it (British Association 1872).

The third report again has little to say. Bryce expresses a desire that a suitable instrument should be compact, easy to install and easy to read. He suggests that James Forbes's son should be invited on to the Committee (Forbes having died in 1868). George Forbes is described as having had practical experience of the subject in Naples, which suggests that he knew Palmieri (British Association 1873). Macfarlane's name is now dropped from the Committee—Macfarlane was nearing the end of his life and may have been ill.

The fourth report (which is actually titled "Fourth Report", the previous ones having been without numbers) is rather more substantial, carrying a long description of the 8 August 1872 Dunblane earthquake (2.9 ML). The Committee now includes George Forbes, but David Milne-Home has silently stepped down (British Association 1874).

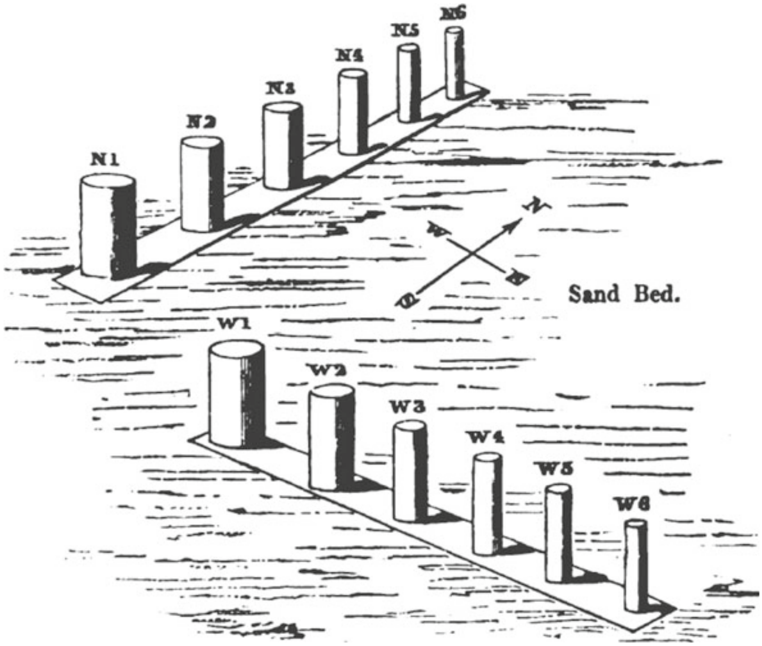


Fig. 26 Mallet's design for a seismoscope (Mallet and Mallet 1858)

The membership of the Committee has changed again in the fifth report: David Milne-Home is back, and added to the list of names is "J Thomson", presumably James Thomson (1822–1892), elder brother of Sir William, and then at the University of Glasgow. The report acknowledges the death of Peter Macfarlane the previous June (British Association 1875).

The report outlines the Committee's plan for proceeding—the new instrument will be of the type recommended by Mallet for the Admiralty Handbook (Fig. 26), consisting of upright cylinders on two planks resting on a bed of sand (mentioned also in Sect. 5.2 above). The idea behind this was that the number of cylinders knocked over would indicate the strength of the shock, and since the sand would prevent them from rolling, one could also observe the direction of the shock. This instrument was to be placed in a separate building constructed for the purpose on a site to the west of the village, granted by Peter Drummond (no relation to James Drummond, but an uncle of Brough). The building was to be constructed on rock, the better to be receptive.

The building is described in the next report (British Association 1876). It was designed to be resistant to any wind disturbance, both by the sheltered nature of the site, and the strength of the building itself.

> The building is of stone and lime, very substantial, about 10 feet square, and 11 feet high to the top of the roof, ceiled and floored. On the perfectly level floor two narrow smooth planks are placed, one directed N & S and the other E & W. On each of these are placed six cylinders of boxwood, carefully turned on the lathe, at such distances apart on the planks that one cannot strike against the other in falling. The floor is levelled up to the planks with fine sand, on which the cylinders must rest, without rolling, if they fall. The cylinders are all of the same height, but of different diameters, so they are of various degrees of stability. In this way the exact direction of a shock is indicated, and

a rough scale of intensity is had. The narrowest cylinder is of so small diameter that it is hoped a very feeble shock will be marked by its fall. (British Association 1876)

The cost of the building was reduced by the assistance of Peter Drummond helping with the preparations, and the final cost to the Committee was £15—in 2013 money, about £3,000. This was the building later to be called "Earthquake House", and which one could probably fairly describe as the first ever purpose-built seismological observatory.

For the 1876 report, the membership of the British Association Committee now includes Peter Drummond, but James Thomson has departed. The report notes that two weak shocks felt at Comrie on 14 and 16 January failed to dislodge even the thinnest of the cylinders, and the Committee resolves to add a further pair of even thinner cylinders. It is also proposed that further sites should be found for similar instruments (British Association 1877).

And there the reports of the Committee cease. No report from the Committee is made in the 1877 proceedings. The reason was nothing to do with the lack of earthquakes, since it is clear the Committee was intending to extend their observations. Rather, the cause of the curtailment of the Committee was the accidental death of James Bryce early in 1877, who fell down a cliff at Inverfarigaig, in the Great Glen, while on a geological excursion. It appears that no-one else was able or willing to take over the running of the Committee, and it effectively ceased to function.

It is unclear whether any more of the Mallet seismoscopes were ever installed. It appears that a minister by the name of J Campbell, in a conversation with ATJ Dollar in 1937, recalled seeing a second seismoscope in the tower of Comrie church (St Kessog's) in 1888. It was described as "a table sprinkled with dry sand in which was set more than a score of boxwood cones of various heights and base areas" (Dollar 1938, unpublished). Dollar was doubtful whether Campbell really meant cones as opposed to cylinders. No more about this is known, and one can only speculate as to who might have installed it.

As for Earthquake House, according to Brough (1950, unpublished), it was "presented to the relevant Authority in Edinburgh, but was left neglected". By the beginning of the twentieth century, the building was dilapidated. It was visited in 1911 by Cecil Carus-Wilson, who noted its ruined state. The wooden pins were long gone, but some of the original sand could still be seen on the floor (Carus-Wilson 1911). Carus-Wilson proposed that a fund should be subscribed for the restoration of the building, the cost of which he estimated at about £30. (This is possibly the first occasion on which the name "Earthquake House" appears in print).

Nothing came of this, but in 1988 the Perth and Kinross District Council resolved to restore the building as an important historical monument, and also a tourist attraction. Earthquake House now houses a replica of the original Mallet seismoscope, plus a modern seismometer belonging to BGS, connected to a Lennartz chart recorder, which is maintained by a local resident.

Since neither the original instrument in Earthquake House nor its replica ever had a single pin fall, in 2011 it was resolved by a TV company producing a documentary for the BBC to try and make good the deficiency by exploding gunpowder charges in the field below the building. Three successive charges, each twice the size of the previous, failed to dislodge any pins—but the trace recorded on the modern instrument was so weak it was clear that most of the explosive force was dispersed into the air.

6.5 Stevenson

While it is common enough to think of certain professions, say lawyer or doctor, as being family vocations where the son follows the father, one might not think the same about lighthouse building. Nevertheless, such was the case with the Stevenson family, starting with Robert Stevenson (1772–1850), builder of the Bell Rock lighthouse, his son David Stevenson (1815–1886), and his grandson Charles Alexander Stevenson (1855–1950), and even his great-grandson Alan Stevenson (1891–1971). It was David Stevenson who was the first member of the family to take an interest in earthquakes, initially by being involved in lighthouse construction projects in Japan, for which he needed to devise means of incorporating earthquake resistance.

When two minor earthquakes (3.3 and 3.2 ML) affected Mull on the west coast of Scotland in the spring of 1877, David Stevenson collected reports of the shaking from lighthouse keepers, and submitted the result to the Royal Society of Edinburgh (Stevenson 1878). It was the intention of James Bryce to follow this up with a visit to the west of Scotland, a visit that never took place because of Bryce's fatal accident (6.4 above), an indication that it was only the death of Bryce that was responsible for the inactivity of the British Association Committee after 1877.

However, on 28 November 1880, a much stronger earthquake affected the west of Scotland, and was actually felt over most of mainland Scotland, the Hebrides, and Ulster. This is the largest earthquake known to have occurred in Scotland, with an estimated magnitude of 5.2 ML (Musson 1994). The distribution of effects was highly irregular, making it difficult to determine a macroseismic epicentre. It seems probable that this was at least partly due to differential attenuation across the geological divide of the Highland Boundary Fault, shaking being more efficiently transmitted through the harder rocks of the Scottish Highlands—such an effect was seen in the macroseismic effects of earthquakes in this region in 1985 and 1986 (Musson 1994, 2007). Aftershock evidence suggests that the epicentre was between Oban and Inveraray.

David Stevenson's son, Charles Alexander Stevenson, set out to apply his father's method of investigation, and called in as many reports from lighthouse keepers as could be obtained. Since this was clearly insufficient to cover the whole affected area, he obtained further information from newspapers.

The results were published as Stevenson (1881). In this he takes pains to list barometer and thermometer readings, supposing these still to be potentially useful. Unlike Mallet's approach of locating an earthquake's focus from the estimated direction of waves, Stevenson is concerned with times, supposing these to be sufficiently accurate to triangulate the centre, though he does include the use of two direction reports in refining his final position to a point between Phladda and Colonsay (Fig. 27). He concludes that the wave velocity varied between 360 and 725 kph, being faster over the sea, because "the crust is … thinner and lighter".

He also notes the capriciousness of the felt effects, with one lighthouse feeling the shock strongly, while a neighbouring one did not feel it at all. Stevenson also notes the proximity of his estimated centre to the Great Glen Fault—"the shock was probably due to a rupture of the crust of the earth at this very distinct line of fracture" (Stevenson 1881), which, while not absolutely specific, is the clearest instance of a British earthquake being attributed to faulting on a named structure until the work of Charles Davison.

Stevenson also came up with his own idea for a seismoscope, which he thought would be easier to make and maintain than any instrument previously in use, including those that had been deployed at Comrie (Stevenson 1882). Since this instrument is largely forgotten today, it is reproduced as Fig. 28.

It consists of two glass plates, about 12 cm square, separated by three ivory balls. The upper plate is connected to a short rod at the end of which is a needle that rests on a piece of smoked paper. The idea is based on David Stevenson's base isolation design for Japanese lighthouses; the balls are intended to decouple the upper plate (B in Fig. 28) from the lower

Fig. 27 Stevenson's (1881) map of the 1880 Argyllshire earthquake

plate (A) and hence the recording surface (D). There is no indication as to whether any of these were ever made; one presumes not.

6.6 O'Reilly

Joseph O'Reilly (1829–1905) was Professor of Mineralogy and Mining at the Royal College of Science for Ireland, a position he held from 1868 to 1898, when ill-health forced his

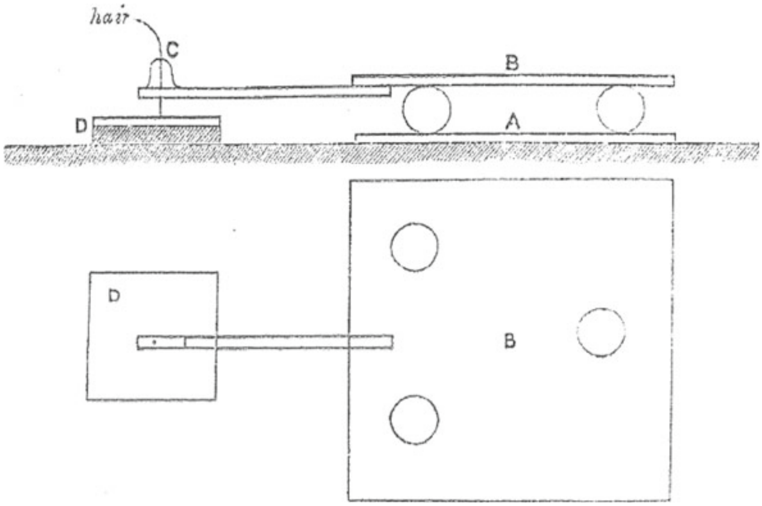


Fig. 28 Stevenson's (1882) seismoscope

retirement (Praeger 1949). As a geologist, he was interested in the possible connections between earthquakes and "lines of physical structure", such as coastlines, mountain chains and geological boundaries—all of which can, of course, be fault controlled. He remarked that "a geological map, to be really complete, should, so far as is possible, show the distribution of the centres of earthquake action … An earthquake map becomes thus the necessary *pendant* of the geological map" (O'Reilly 1884).

How this was to be attempted gave him some thought. He decided that as representing the intensity of a shock would be difficult, relative frequency of occurrence could be represented by gradation of tint—much the same approach as followed by Mallet and Mallet (1858), though he does not mention this.

In order to try and accomplish this, he constructed his own catalogue of earthquakes in the British Isles to act as the basis of an earthquake map of the British Isles. The catalogue itself is rather terse, giving for each entry only the date and a brief descriptive text, and O'Reilly states that even the date is unnecessary, except to distinguish when an earthquake felt at different places is in fact one event. His data are presented in several different ways—not just a chronological listing, but also arranged by place.

The map itself is a fine piece of cartography in full colour (Fig. 29), and certainly the first attempt to produce an earthquake map of the British Isles, excepting the tiny space that the islands occupy in the maps of Berghaus, Johnston and Mallet. In addition to the graded red tint of frequency of shaking, he also uses symbols for towns according to the number of times earthquakes have been felt there, draws circles delimiting the approximate felt areas of some significant events (such as the 1871 Appleby earthquake), shades in the major coalfields, plus various lines he considered significant, such as prominent directions of coastlines, or the projection of coastline orientations from elsewhere (including Portugal and Morocco). He has a particular interest in lines that may be at an angle of 70° to one another, for reasons not entirely clear.

O'Reilly devotes some attention to the possible link between earthquakes and colliery explosions, but imagined that elucidating this would lead to an improved understanding of the formation of coal.

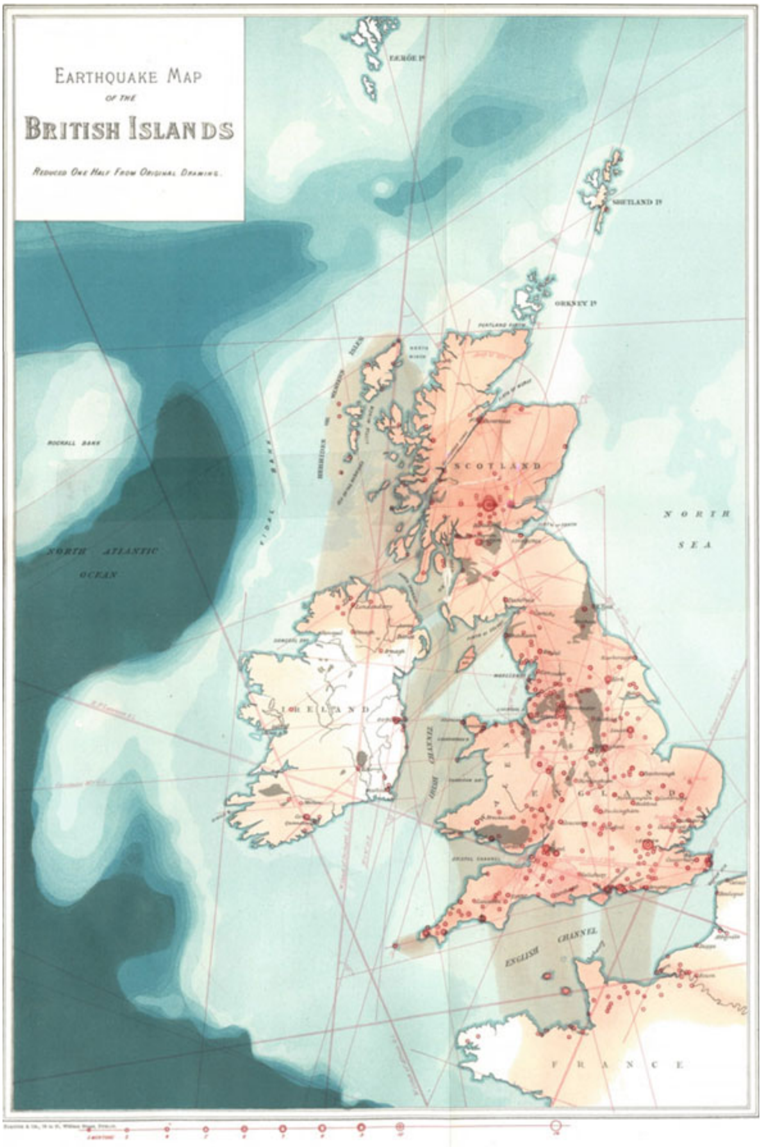


Fig. 29 O'Reilly's (1884) earthquake map of the British Isles

Interestingly, O'Reilly picks out Stevenson's (1881) remark about the possible origin of the 1880 earthquake being on the Great Glen fault. This he gives as an indication of the fact "that main lines of jointing [have] intimate relations with earthquake shocks", though it is unfortunate that he should use the word "jointing" instead of "faulting".

The catalogue itself begins with a poorly-known event in 1426, and runs to 1880, and contains 53 events in all—not a very large number. Among the larger events missing are the South Wales earthquakes of 19 July 1727, 8 September 1775 and 30 October 1868, the latter particularly surprising since he does list two much smaller shocks in 1868.

The 1727 earthquake is also missing from Milne's (1842a) catalogue, but the 1775 event is not.

After the chronological listing, O'Reilly gives the total number of shocks by county (Anglesey 5, Derbyshire 1, etc). Then follows a table of all places that have felt more than one earthquake: at the top of the league are Inverness and Portsmouth with eight, Manchester with nine, Chichester with ten, London with thirteen, and Comrie with 259. This material is then rearranged alphabetically by place. For each locality, from Aberbarden to York, the years when earthquakes were felt are given, so Gloucester has two years given, 1574 and 1768, while Glasgow has six: 1755, 1756, 1786, 1801, 1821 and 1839.

The obvious flaw that seems to have escaped O'Reilly is that any earthquake reported to have been felt at two places was probably felt at all the places in between, whether this was reported or not. Thus his scheme completely exaggerates the seismicity of important cities like Glasgow, simply because what happens there is more likely to get into the written records. Otherwise, O'Reilly's table is an interesting precursor of the idea of "seismic histories" (Stucchi et al. 1999).

O'Reilly's map does not bear a great resemblance to a modern map of British seismicity. He does pick up the relative lack of seismicity in Ireland—the shading in the north of Ireland comes from the 1880 Argyll earthquake. But the east of Scotland attracts a dark tint where it is one of the least active parts of the country; Wales is under-represented, and so on.

Nevertheless, O'Reilly succeeded in producing the first map of seismicity in the UK—almost the first seismic hazard map, and this should count as a landmark.

6.7 Meldola

It is possible that if Raphael Meldola (1849–1915) had not lived in Essex, he would not have become involved with seismology (Fig. 30). His main area of expertise was in chemistry, and was also involved with entomology. As a natural historian, he was an active member of the Essex Field Club, in the days when it was common for amateur societies, which were often based at a county level, to pursue studies into local history or science, often to quite a high standard. The Essex Field Club was the only such scientific society in Essex. On the morning of 22 April 1884, a damaging earthquake struck the area around Colchester, and as soon as Meldola was certain that the earthquake was indeed a local phenomenon, he decided that the Essex Field Club was the logical organisation to make an investigation.

At a meeting on 26 April, four days after the earthquake, Meldola raised the issue, and offered to take responsibility for the report; in this he had the assistance of William White, a local geologist.

Meldola was not able to travel to Colchester to see the damage for himself until 3 May. He was accompanied on this expedition by the geologist TV Holmes, and William Cole, the Secretary of the Essex Field Club. They were further assisted by two local members of the Field Club, spending 2 days in Colchester and the surrounding villages, making notes and sketches. This was the first time a field survey of earthquake damage for scientific purposes had ever been made in Britain; and probably the only one until very recent times.

The importance of examining earthquake damage as soon as possible is self-evident, and two other investigators were more prompt than Meldola in getting to the scene. GJ Symons, a prominent meteorologist, was able to tour the affected villages the very day after the earthquake, and so also did a WH Bird, another Essex Field Club Member. Both these placed their notes at the disposal of Meldola and White.

Appeals for information were launched by other means also, including local newspapers, for which purpose White drew up a list of ten questions, intended to counter what Meldola

Fig. 30 Raphael Meldola (Portrait by Solomon Joseph Solomon, ©National Portrait Gallery, London)

described as "the want of knowledge as to what to observe", though the priorities were rather different from those of a modern earthquake questionnaire. For instance, question 9 asks for the direction of fall of chimneys or walls, but nothing about the amount of damage. Question 7 asks if a shower or wind followed the earthquake (Meldola and White 1885).

Meldola was also in touch with other seismologists for expert advice—he acknowledges communications received from O' Reilly (6.6 above), Ewing and John Milne.

Such were the materials that Meldola and White were able to draw upon in compiling their report, which was published in book form, occupying 223 pages, plus four plates. The bulk of it is given over to a "descriptive report" (pages 44–155), starting with descriptions of the effects in the worst affected areas, and working out to the edge of the affected area. The text consistently gives the impression of careful and informed scientific observation, and it is notable that they pay attention also to what they did not see—Meldola and White (1885) took pains to determine if any disturbance of sea or river estuary was observed, and dutifully reported this negative observation.

This is in contrast to some later accounts of the earthquake, particularly that of Haining (1976), who, in the course of trying to dramatise the event (partly by invention), has to attempt to dismiss Meldola and White's investigation as a sort of cover-up of the extent of the disaster. In this light, particular significance attaches to a remark of Meldola's that he was impressed by the overall accuracy of the newspaper reports, which implies that newspaper coverage of other earthquakes of the period is also probably reliable (see the discussions in Burton et al. 1984b and Musson 1986).

Meldola decided it would be useful to start by putting the Colchester earthquake into historical perspective, by providing a "complete chronological catalogue" of British earthquakes, "as no such catalogue at present exists" (Meldola and White 1885). Presumably Meldola either discounted David Milne's catalogue because it did not include medieval earthquakes, or because it did not have events after the 1840s. White started work on producing a catalogue, but discontinued work when he learned that O'Reilly was working on the same endeavour. Considering that when White stopped, he had compiled 350 entries down to the year 1843, whereas O'Reilly's catalogue has only 53 discrete entries, it is rather a shame we do not have White's catalogue.The fact that he stopped at 1843, which is when David Milne retired from seismology, seems significant.

Instead, Meldola and White (1885) elected to compile a list only of British earthquakes that caused structural damage. This catalogue has caused some problems ever afterwards; Charles Leeson Prince of Crowborough Observatory supplied Meldola with the chronological table of earthquakes from Short (1749), and it is from Meldola and White (1885) that these spurious events entered the seismological record, from which they have been so difficult to expunge (Musson 2005). It seems that Meldola did have doubts about what records Short could possibly have consulted, but was reassured by Short's assertion that he only proceeded "as far as [he] had vouchers" (Short 1749). I have suggested (Musson 2005) that this applies to Short's main text (which gives authorities) and not to the table (see Sect. 3.3.1 above).

Another long-lived error introduced by Meldola and White (1885) is the reference to an earthquake in Ireland in August 1734 that destroyed over 100 houses and five churches. This is a simple misreading—the earthquake occurred in Iceland.

Whatever one might think of White's questions about atmospheric conditions, the authors are careful to report that the evidence collected is conclusive that no special meteorological conditions preceded, accompanied or succeeded the earthquake. Meldola concedes that a sudden drop in barometric pressure might trigger an earthquake that was imminent anyway, but otherwise, winds, waterspouts and so on, if they were observed at the time of the earthquake, can be counted coincidences.

Meldola and White's (1885) report is also the first study of a British earthquake to use the word "epicentre", or to be more precise, its older form "epicentrum", this term having been introduced by Schmidt (1874) not long previously.

Although the 1884 earthquake was only instrumentally recorded on devices intended to record things other than earthquakes—either barographs or magnetometers—Meldola and White's report is the first occasion on which discussion of a British earthquake is influenced by knowledge gained from instrumental seismology, in particular, from Ewing and Milne's work in Japan. Meldola and White note that, since an earthquake originates from a source of finite size "such as a fissure or fault", the reflected and refracted waves will be of great complexity at the surface, as evidenced by recordings made of earthquakes in Japan.

Thus reports of a series of shocks may simply be the observer's sensation of a series of oscillations of the building. However, the common description of there being two shocks attracts a thoughtful discussion. Meldola and White (1885) advance three possibilities: the time lag between the arrival of primary and shear waves (not that they use these terms), direct and reflected arrivals of the shock (which does not mean that Meldola anticipated Mohorovičić; he thought the shock might be reflected by the valley of the river Colne), and the arrival of faster waves propagating through hard rock, followed by slower waves propagating through overlying soft sediment. Meldola leaves it undecided, but favours the third explanation.

The report is also the first study of a British earthquake to mention that damage to buildings is related to ground acceleration (known from work in Japan), but obviously, Meldola and

White had no data by which ground acceleration could be measured. Previous investigators of earthquakes in Britain (Lowe in 1863, Stevenson in 1880, etc) were dealing with events that were for the most part non-damaging. Meldola and White had the opportunity of studying one of the very few British earthquakes that produced high-intensity effects, and they rose to the occasion, considering such issues, for instance, as whether there were variations in degree of damage according to local geology, recognising that damage was likely to be less on hard rock—not that there was any hard rock in Essex to exhibit this difference. The difference between the amount of damage on clay and on drift they attributed to the location of the epicentre with respect to the distribution of these two deposits. They also made a point of recording all cases where the earthquake affected ground water.

One thing lacking was a detailed study of the earthquake intensity. Meldola and White were aware of the concept, and an English version of the Forel Scale is included in the course of a footnote. They estimate that the maximum intensity was either 8 or 8–9 on the Forel Scale, but did not consider assigning intensities to each place from which they had data.

For determining the epicentre, Meldola and White attempted the use both of time records and estimates of direction, making considerable effort to attempt to distinguish the more reliable records. The time data proved to be no use at all; the determination from direction à la Mallet gave a rather uncertain position, but it did at least coincide with the area of greatest damage.

In discussing the cause of the earthquake, they conclude that there was no apparent relation to known lines of faulting (not surprising given the extensive drift cover), but that the quake "could have resulted from the rupture of deep-seated rocks under strain or pressure, such as the sudden production or extension of a line of faulting" (Meldola and White 1885).

Their estimate of the epicentre is between Abberton and Peldon, or at least, they state that the shock originated beneath these two villages. This is in good agreement with modern studies (Musson et al. 1990).

All in all, given the constraints of the period, Meldola and White's (1885) report on the 1884 Colchester earthquake is probably the most thorough contemporary investigation of a British earthquake in the published literature.

6.8 Teleseisms before Rebeur-Paschwitz: Investigations at Marsden Colliery

History records that the first teleseismic record of an earthquake was made by Ernst von Rebeur-Paschwitz on 17 April 1889, at Potsdam, using a pendulum designed to measure the changes of the gravitational attraction on the Earth caused by the movement of other planets. But it is interesting to note that this was not actually the first *claim* to have recorded an earthquake at teleseismic distances. The first claim of such a recording was made a few years before, the earthquake in question being a not particularly famous shock in the USA, near Vincennes IN, on 6 February 1887. The claim was made by Martin Walton Brown (1855–1907), a mining engineer in the north-east of England.

In the 1880s, the North of England Institution of Mining and Mechanical Engineers were concerned about earth tremors in mining districts and their possible link to gas in mines. Rather on the model of the British Association, a Committee was appointed to look into the subject. The exact membership is uncertain, but it certainly included William Garnett (1850–1932) and George Lebour (1847–1918), both from Durham College of Science.

Garnett's role was to advise on the most suitable instruments to employ, and he decided on a Ewing duplex pendulum instrument, to be supplied by the Cambridge Scientific Instrument Company. However, before this instrument was received, work was already in progress at Marsden Colliery, on the north-east coast of England, between South Shields and Sunderland.

At surface level, an instrument was deployed "in accordance with the drawings of Mr Walton Brown and similar in all details to those employed by Professor Ewing in Japan" (Transactions 1888).

This instrument consisted of a pendulum arranged in such a manner that any movement away from the vertical would complete an electrical circuit, which would operate a needle that would pierce a paper disc, thus making a record of the time of the shock. At the same time, a bell would ring. This instrument was in operation continually from 19 October 1886 to 12 March 1887.

The report of the Committee, and one assumes it is Walton Brown who is writing, states that

> An interesting feature of these records is the irregular and perturbed movements which lasted from February 7th to March 12th 1887. They appear to be connected with disturbances originating at places very distant from the observatory at Marsden.
> It seems highly probable that the shocks experienced at St. Louis in the United States on February 7th were a more violent result of the motions recorded at Marsden on the same day. These motions continued until February 23rd, the date of the disturbance at Nice and adjacent district, and ceased on March 12th when that series of Italian disturbances ceased. The shocks recorded on March 14th seem to have been a reverberation of those experienced in Bohemia and Burmah (Transactions 1888).

The reproduction of the record does not throw much light on this. Firstly, while it appears that this instrument was continually recording, unlike many at this period, it ran extremely slowly, and did not record amplitudes at all, merely a punched hole when the circuit completed. The reproduction shows a wavy line which is unexplained and may be a barograph. Below is the track of the seismic recording, which consists of vertical lines indicating "seismograph recorded shock" and shaded bars running over several days at a time indicating "recorded motion during period thus marked", which presumably means many punched holes.

It would be momentous if Walton Brown's claim were correct, and the history of seismology would need to be re-written, but the mere indication of a few punched holes does not vindicate the claims made, which are highly implausible to start with.

The refutation was not long in coming. A few months later, a paper was submitted by Alexander Herschel (1836–1907), entitled "On an improved form of seismoscope" (Herschel 1888). Herschel starts his paper

> … Mr M. Walton Brown … obtained with an ingenious form of Seismoscope at Marsden, during the months from October, 1886, to April, 1887, a defective action of the instrument in indicating, apparently, incessant vibratory motion at the station for days, weeks, and even for a whole month together … due to permanent departures of the tremor indicator from its neutral point … (Herschel 1888).

Herschel then proposed an improved form of seismoscope, which, he suggests, would be impervious to this sort of drift.

Herschel was not actually present at the meeting at which his paper was read. Walton Brown was.

> The CHAIRMAN - Do you know if Prof. Herschel has made an instrument of the kind described?
> Mr WALTON BROWN - No, he has not made one. (Transactions 1888)

The Committee would resurface in a few years in a new guise, as will be seen.

Fig. 31 John Milne (BGS archive image)

7 Milne

Possibly more has been written about John Milne (1850–1913) than any other seismologist (Fig. 31). There are two full biographies of him in book form (Herbert-Gustar and Nott 1980; Kabrna 2007) compared to only one for Charles Richter (Hough 2007). The first biographical sketch of Milne was by Hoover (1912), who later went on to become First Lady of the United States. ("Professor Milne frankly confesses to being thoroughly tired of earthquakes himself, and as soon as news of a fair-sized one arrives by cable he takes to the woods—in this case an excellent golf course, the first tee of which lies immediately above his own house.") Milne also has the unusual distinction of being the only seismologist ever to have a pub named after him (near his home town of Rochdale).

His life falls into three divisions. Originally he trained as a mining engineer, and travelled widely, including trips to Newfoundland, Iceland, and the Sinai peninsula. About this time, the Japanese government was attempting to modernise their country, and to this end engaged a number of Europeans to university positions. Milne was offered the position of Professor of Mining and Geology at the Imperial College of Engineering in Tokyo, which he took up on 8 March 1876 after travelling to Japan via Siberia.

The second period of his life starts in 1880, when he became drawn to seismology after feeling an earthquake that struck Tokyo that year. He became involved in instrument design, along with two other expatriates, Alfred Ewing and Thomas Gray. The Milne horizontal pendulum design of 1895 was the first capable of recording teleseisms (Wartnaby 1957), and thus opened the way to global seismology.

A catastrophic fire on 17 February 1895 destroyed Milne's home and laboratory and most of his papers and possessions. After this, he gave up his positions in Japan and returned to England with his Japanese wife. He made his home at Shide, in the Isle of Wight, and until Milne's death in 1913, this unlikely village became the centre of world seismology.

Someone once remarked that "there is always a Father of Modern Seismology. To English-speakers his name is Milne, to German-speakers his name is Wiechert, to Russian-speakers his name is Galitzin". All three were important in early seismometer design, but the historical importance of Milne in the development of global seismology gives Milne a superior claim to the title. Or as Galitzin himself wrote, "Nearly all the problems of modern Seismology have been considered and treated by Milne, and he can be considered the as the real founder and promoter of this new and important branch of geophysics" (quoted in Bellamy and Bellamy 1931).

A short chronology of Milne's life is given in Table 1, loosely based on Nott (2011, unpublished) and Anon (2012).

7.1 Milne in Japan

It is not the intention in this monograph to go into great detail of Milne's life, nor into his work in Japan. The former is well covered by the two biographies cited above, and the latter is examined in detail by Wartnaby (1972) and Muir Wood (1988). Instead, the focus will be on his work after he returned to Britain in 1895, and its importance for further developments in seismology.

After the earthquake of 22 April 1880, Milne helped to found the Seismological Society of Japan, and the Transactions of that society provided him with an outlet for publication. But it was not the only outlet, for Milne had not lost contact with science in Britain.

Although the British Association's Earthquake Committee had been silent after Bryce's death in 1877, a new committee was set up a few years later consisting of just Milne and Andrew Ramsay, who had taught Milne at the Royal School of Mines. This was the "Committee for investigating the Earthquake Phenomena of Japan", and through it, Milne and Ramsay received a grant of £25 per year. In the first report of the committee, made to the British Association meeting of 1881 (British Association 1882), Milne enumerated his objectives.

These were: (1) to determine the origin of earthquakes that shook Tokyo and Yokohama, like that of February 1880; (2) to determine the nature of earthquake motion (wave type, amplitude and period); (3) to record small tremors to see if they bore any connection with larger shocks; (4) to investigate the variation of earthquake motion over a region of geological variation; (5) investigations of "artificial earthquakes" (from dropping a large iron ball) conducted with Thomas Gray (Sect. 8.1 below).

Figure 32 (from British Association 1884) shows the sort of seismogram that Milne and Gray were obtaining in the early 1880s with a two-component horizontal pendulum instrument. It looks very unlike a modern seismogram. Continuous recording had not yet been developed (it would come a couple of years later), so the seismogram begins at the first detected motion, like a modern accelerometer, and the recording runs in a circle. Note the division into "the shock" which is considered separate from "preliminary tremors" and "subsequent vibrations".

In 1882, Gray joined the committee, and the following year Ramsay retired and was replaced by the geologist Robert Etheridge (British Association 1883; 1884). The only one of the three in Japan was now Milne, who continued to send reports back to the British

Table 1 John Milne: chronology

Year	Event
1850	Born in Liverpool on 30 December
1851	Family moves to Rochdale
1871	Expedition to Iceland
1873	Works as geologist in Newfoundland
1874	Expedition to Mt Sinai
	Elected Fellow of the Geological Society
	Imperial College of Engineering founded in Tokyo
1875	Receives appointment to Imperial College of Engineering
1876	Arrives Japan
1880	Co-founds Seismological Society of Japan
1881	Marries
	Manufacture of Gray-Milne seismometer
	Starts controlled-source seismology experiments with T Gray
	Compiles catalogue of great Japanese earthquakes 295 BC to 1872
1886	1st edition of “Earthquakes and other earth movements”
1887	Elected FRS
1888	Receives Order of Merit from Japanese Emperor
1892	Seismological Society of Japan wound up
1893	Milne Horizontal Pendulum design completed
1894	Awarded Lyell Medal by Geological Society
1895	Home and laboratory destroyed by fire on 17 February
	Publishes catalogue of Japanese earthquakes, 1885-1892
	Awarded Third Order of Merit with the Order of the Rising Sun
	Leaves Japan, return to Britain and moves to Shide, Isle of Wight
	Becomes Secretary of British Association Earthquakes Committee
1898	1st edition of “Seismology”
1902	Awarded Emeritus Professor of Seismology at Tokyo
1908	Receives Royal Medal from Royal Society
	Receives honorary doctorate from Oxford
1912	Catalogue of world earthquakes published
1913	Milne-Shaw seismometer developed with JJ Shaw
	Dies at Shide on 31 July

Association each year throughout the 1880s, towards the end of which period its scope widened to include volcanoes as well as earthquakes. The committee membership remained unchanged until 1890, when it was joined by Lord Kelvin, John Perry (who had also worked in Japan), and the geologist Henry Woodward.

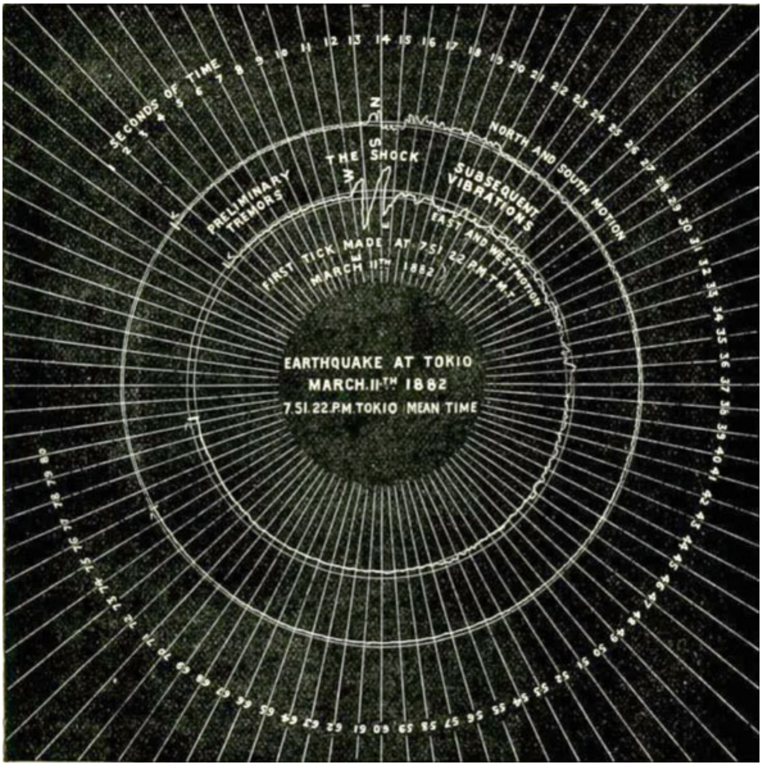


Fig. 32 Gray-Milne seismogram of an earthquake in 1882 (British Association 1884)

Milne's important innovation came in 1893, when he adapted his instrument so that it recorded on photographic paper using a beam of light, rather than a needle on smoked paper (Milne 1894). This removed the problem of friction between needle and paper. The Milne seismograph was now a practical instrument for widespread installation and global use.

7.2 Milne's global network

Milne's first thoughts on a global seismic network were noted early on: that with suitable instrumentation, all large earthquakes anywhere in the world could be recorded (Milne 1883). Of course, this aspiration had already been mooted by Mallet, and even, to a degree, by David Milne back in the 1840s.

In 1895 Milne issued a circular calling attention to the desirability of recording distant earthquakes (Milne 1895). Ernst von Rebeur-Paschwitz had already drawn up suggestions for the establishment of an international network of earthquake observatories in 1894, realising also that through the study of earthquake waves it would be possible to discover the structure of the earth; but these were only published in 1895 (von Rebeur-Paschwitz 1895a,b; Davison 1927a; Schweitzer 2002).

At the 6th International Geographical Congress, London, in July-August 1895, von Rebeur-Paschwitz's ideas were presented by his friend and mentor Georg Gerland. The proposal was adopted, and versions in both French and German were issued by Gerland with the signatures of 25 supporters, including Milne, Gerland, von Rebeur-Paschwitz, and also

GH Darwin and Charles Davison (Schweitzer 2002).Von Rebeur-Paschwitz's contributions to seismology were sadly cut short by his early death in October 1895, shortly before the London resolution was printed and distributed.

In 1897, Milne later gave a completely different account of this, stating that "after the death of Von Rebeur [sic] these suggestions were translated into French and issued by Dr G. Gerland of Strassburg, on his own responsibility" (British Association 1898). After visiting Strasbourg the following year, Milne was evidently reconciled with Gerland (whom he describes as "genial") and published a correction, giving Rebeur-Paschwitz and Gerland proper credit (British Association 1899).

With Milne back in England in 1895, he could obviously no longer report to the British Association on earthquakes in Japan. Also, since 1893, there had been another earthquakes committee reporting to the British Association.

This committee was none other than a British Association incarnation of that created by the North of England Institution of Mining and Mechanical Engineers (Sect. 6.8 above), set up "to investigate the advisability and desirability of establishing in other parts of the country observations upon the prevalence of earth-tremors similar to those now being made in Durham" (British Association 1894). The Secretary of this Committee was Charles Davison (see chapter 9), and the membership included Lebour and Walton Brown from Durham, and returnees from Japan: Cargill Knott, Thomas Gray and Alfred Ewing (chapter 8). Raphael Meldola was also a member—his interest in seismology, it appears, did not end in 1885. George and Horace Darwin, who had been developing pendulum instruments for studying gravity were also members, and the Committee was in close contact with Rebeur-Paschwitz in Potsdam.

The solution was to merge the two, creating the British Association Seismological Committee, the sixth British Association committee on seismology after those of David Milne, Mallet, Bryce, John Milne and Davison.

The first meeting of the combined committee was at Liverpool in 1896. Milne and Davison shared the post of Secretary, though most of the work would fall to Milne. The membership was quite large, reflecting the fact that it consisted of two committees merged.

Lord Kelvin was the one surviving member from the 1870s incarnation. In addition to those already named above, the membership included the physicist William Adams, James Bottomley, also a physicist and Lord Kelvin's nephew, Sir Frederick Bramwell, another colleague of Lord Kelvin's, the engineer GF Deacon, the geologist Alexander Green (who died the same year), and Isaac Roberts, an engineer mostly remembered now for his work in astronomy.

In the first report of the committee (British Association 1896), Milne starts to lay out the plan for a global recording network. The aim, he records, is to have stations within 1,000 miles of each other around important centres of seismicity, such as the west coast of South America, Japan, the Philippines or the Himalayas. He estimated the number of stations needed to be in the order of fifteen to twenty.

He then enumerates the types of instrument that might be suitable—the Vicentini design, that of Rebeur-Paschwitz, the Milne instrument, or Darwin's bifilar pendulum. He runs through the characteristics of each, including the cost, and suggests one of each be set up together for purposes of comparison. (The cost of a Milne instrument is given as £45 including the clock, probably about £9,000 at today's prices).

There follows an account of Milne's observatory at Shide; not only did he have an instrument in Shide Hill House (Fig. 33), but a second one was installed in the grounds of Carisbrooke Castle, nearby. Milne presents in this report what is effectively the first British seismological bulletin (apart from the Comrie register from the 1840s)—a record of

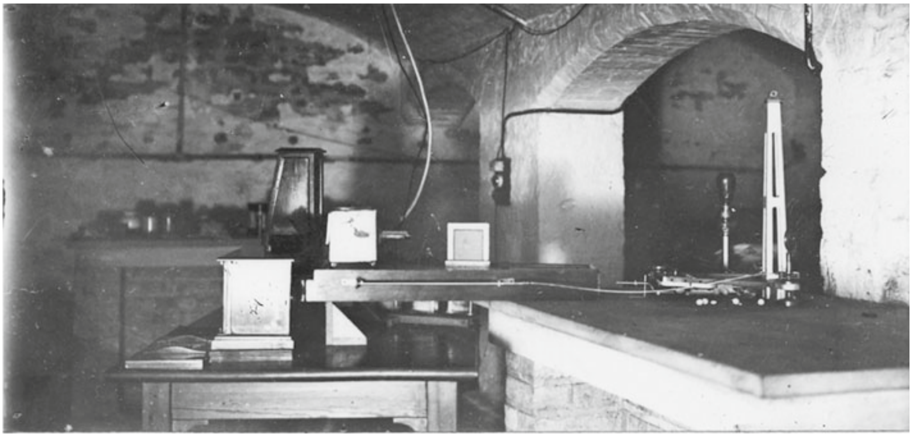


Fig. 33 Milne's instrument vault at Shide (BGS archive image)

disturbances recorded at Shide from 19 August 1895 to 22 March 1896, in four columns: date, time correction, time, and remarks, which includes duration of shaking and/or comments such as "slight" or "sudden". However, comparing his records with those obtained at Carisbrooke, he found that the various "sudden disturbances" and "decided shocks" did not correspond.

He also sent a copy of the bulletin to several European observatories, and had replies back from Eschenhagen in Potsdam, Agamennone in Istanbul, and Cancani at Roca di Papa. Altogether, it appeared that twelve of the various disturbances recorded at Shide could be matched up with earthquakes recorded elsewhere in Europe.

At the 1897 meeting, Milne recorded that, Gerland's dissemination of the resolution from the London conference of 1895 having produced no results, the British Association Committee had taken it upon themselves to contact various observatories around the world. Favourable replies had already been received from the Government of Madras and the Geological Survey of India, and from Kew Observatory and Oxford University. In February 1897, Milne wrote to the Foreign Office requesting that British diplomatic representatives in Chile, Peru, Ecuador, Venezuela, Columbia, Mexico, Brazil, the Netherlands (for Java), Greece, Spain, Portugal (for the Azores), Russia and Japan, should intercede with the governments of those countries on behalf of the British Association Committee, to encourage them to set up seismic observatories. Playing the Empire card, Milne wrote in similar terms to the Colonial Office, with the same request with respect to Newfoundland, Bermuda, Barbados, Trinidad, Jamaica, Honduras, Guiana, St Helena, the Falklands, Cyprus and Malta. This list was already ambitious in scope, but it was further broadened by letters to 31 specific observatories. These included five in the USA (one of which was Terre Haute IN, where Thomas Gray had ended up, see Sect. 8.1 below), four in Australia, two in China, two in South Africa, and eight in Europe (British Association 1898).

The circular sent to these observatories stressed the need to use common instruments, the recommended one being the Milne. It gave as the prime objective the determination of the velocity of earthquake waves round or through the earth, but that numerous other useful results could be expected, including the location for the first time of oceanic earthquakes. The circular was signed by Symons, Davison and Milne. The text is of some interest for the history of seismology, and is reproduced in full here:

Sir, — It has been established that the movements resulting from a large earthquake originating in any one portion of our globe can, with the aid of suitable instruments, be recorded at any other portion of the same; therefore the Seismological Investigation Committee of the British Association are desirous of your co-operation in an endeavour to extend and systematise the observation of such disturbances.

Similar instruments should be used at all stations; and the one recommended by this Committee as being simple to work, and one that furnishes results sufficiently accurate for the main objects in view, is indicated in the accompanying report (see pp. 2–4) by the letter M; a sketch of the same is shown on p. 7, whilst there is an example of one of its records on p. 49.

We desire to know whether you are disposed to purchase, and make observations with, one of these instruments, the cost of which, including photographic material to last one year, packed for shipment, is about 50l.

Should you reply in the affirmative, we shall be pleased to arrange with a competent maker for the construction of an instrument for you, and to furnish instructions respecting installation and working. In case an instrument be established at your observatory, we should ask that notes of disturbances having an earthquake character be sent to us for analysis and comparison with the records from other stations. From time to time the results of these examinations would be forwarded to your observatory.

The first object we have in view is to determine the velocity with which motion is propagated round or possibly through our earth. To attain this, all that we require from a given station are the times at which various phases of motion are recorded; for which purpose, for the present at least, we consider an instrument recording a single component of horizontal motion to be sufficient. Other results which may be obtained from the proposed observations are numerous.

The foci of submarine disturbances, such, for example, as those which from time to time have interfered with telegraph-cables, may possibly be determined, and new light thrown upon changes taking place in ocean beds.

The records throw light upon certain classes of disturbances now and then noted in magnetometers and other instruments susceptible to slight movements; whilst local changes of level, some of which may have a diurnal character, may, under certain conditions, become apparent.

Trusting that you will find it possible to co-operate in this endeavour to extend our knowledge of the earth on which we live,

We remain, Sir (on behalf of the Committee),

Your obedient servants,

G. J. SYMONS, Chairman, C. DAVISON, J. MILNE, Joint Honorary Secretaries.

Immediate positive replies were received from Cambridge MA, Cape Town and Toronto; two observatories (Bucharest and Vienna) replied that they would join but using other types of instrument, at least at first, and Yerkes Observatory WI and Perth Observatory in Australia replied that they might join at some future date. St Louis MI, Sydney, Gozo (Malta), Zika-wei (China) and Uccle (Belgium) all declined to participate.

The Committee's report for 1897 continues the Shide bulletin from the previous year's report, but records are now flagged if they correspond to recordings at Ischia, Potsdam, Nicolaiew (now Mikolaiv, Ukraine) or Edinburgh. This report really marks the beginning of global seismology. Whereas the previous bulletin was really a record of disturbances, some of which were evidently non-seismic, the bulletin for 1896–1897 is now a list of earthquakes. The events are numbered, with "earthquake no. 1", being an event on 14 June 1896 off the northeast coast of Honshu (15 June by Japanese time), which was also recorded at Padua, Ischia, Rocca di Papa, Potsdam and Nicolaiew, in addition to observatories in Japan.

Milne reviews the data for this earthquake and several others in sequence. Earthquake no. 73 turns out to be British—the Hereford earthquake of 17 December 1896. "The booms of the seismographs at Shide were not slowly tilted from side to side, as is the case when they record earthquakes originating at a great distance, but merely set in a state of elastic vibration, behaving, in fact, like the pointers of seismographs intended to record movements which we feel" (British Association 1898). These tremors commenced at about 23 h on 16 December and continued intermittently for about 12 h. The duration of the tremors observed by Milne make it certain that they cannot have been connected with the Hereford earthquake.

In anticipation of the "Shide Circulars", the 1897 report presents several other bulletins complete, from the observatories at Edinburgh, Nicolaiew, Ischia, Potsdam, and Roca di Papa, and the data collected now permit the first attempts to calculate wave velocities.

Events now moved rapidly. Milne's letters to the Foreign and Colonial Offices bore fruit, and the requested assistance from the British diplomatic corps was forthcoming. By the time of the third meeting of the Seismology Committee (which now included HH Turner at Oxford amongst its members), Milne was able to report that 22 stations were either established or in the course of being established (British Association 1899). These were, in the order in which Milne described them: Toronto, Cambridge MA, Madras, Cadiz, Philadelphia PA, Tokyo (run by Omori), Kew, Victoria BC, Batavia (Djakarta), Cape Town, Cordova (Argentina), Bombay, Calcutta, Mauritius, Wellington, Cairo, Paisley, Mexico, Beirut. These are nineteen locations, but some received two instruments, making the total up to 22 Milne instruments, with every continent bar Antarctica being represented. Considering that Milne had originally estimated that fifteen to twenty stations were needed, this target was already reached.

Other replies were varying. The US Coast and Geodetic Survey and US Naval Observatory, both in Washington DC, expressed an interest but were at present unable to co-operate. Similarly, replies from Bogota, Honolulu, St Petersburg, Hammerfest and Dublin offered the possibility of joining in the future. Some observatories, such as Sydney, Zika-wei, and Gozo (Malta) replied that they had a lack of funds or manpower. In Melbourne, Milne's circular arrived at a propitious time, South Australia having been just shaken by one of the strongest Australian earthquakes, the 10 May 1897 Beachport event (Musson 2012b), which very much raised the profile of seismology in the state.

It is impossible fully to summarise here the mass of material in this third report. It included bulletins from different stations, descriptions of important earthquakes, reproductions of seismograms, and discussions of technical problems such as the issue of time-keeping on a global scale.

Milne was already working on exploiting this flood of new data for scientific purposes. The plot reproduced here (Fig. 34) is a first attempt to plot P to S delays as a function of distance, in anticipation of the travel time curves that would later be developed. Here the delay is described as "intervals by which Preliminary Tremors have outraced Long-period Waves".

In May 1898 he made a visit to several international observatories, travelling to Catania, Cassamicciola (Ischia), Rome, Roca di Papa, Padua, and Strasbourg (then Strassburg). He left detailed descriptions of the observatories and instruments he saw (British Association 1899).

The annual reports of the Seismological Committee continued each year, but from 1899, such was the volume of material, the station bulletins were published separately as the "Shide Circulars", while the annual reports published other developments: the gradual expansion of the network as new stations were opened, and technical papers as the mass of data that

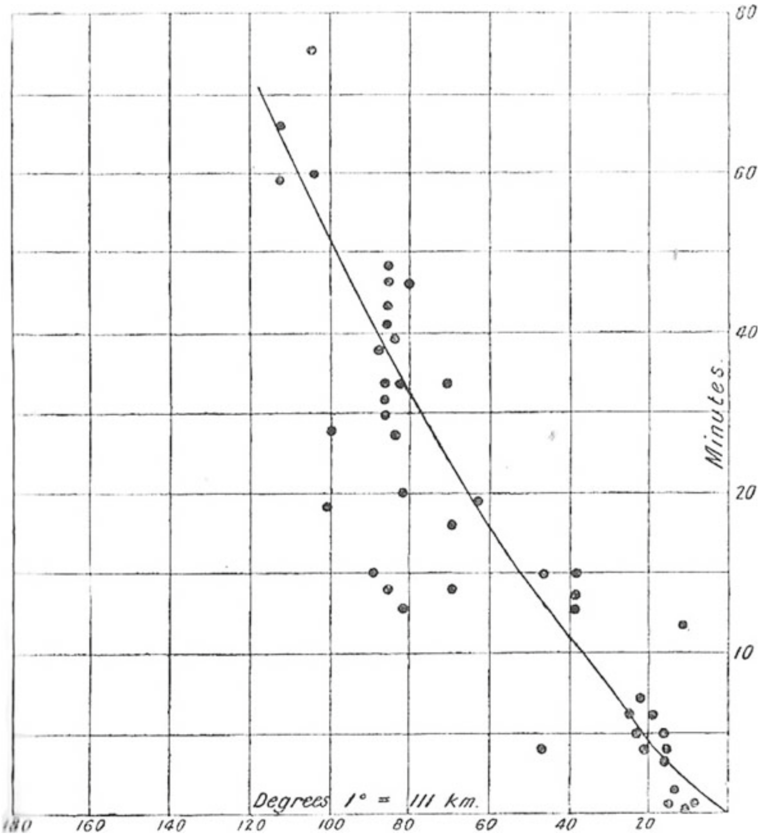


Fig. 34 Milne's first plot of P to S delays (British Association 1899)

was being gathered was interpreted. Fig. 35, from British Association (1900), shows Milne's global network as of 1900. The numbers refer to earthquakes in 1899, the numbers being plotted roughly according to epicentral region.

The Shide Circulars are something of a foundation document in modern seismology. It seems that they were not printed in very large numbers. At the time of the preparation of the IASPEI Centenary Handbook (Lee et al. 2002), WHK Lee could only trace two copies in the whole of the United States, and neither were easy of access. A copy has now been scanned and made widely available.

The era of global seismology had arrived.

All this was not happening in isolation, however, as things were moving forward in Europe—not least the invention by Emil Wiechert in 1899 of a rather more accurate seismometer than Milne's design. Four years after the 1895 London Congress, the Seventh International Congress of Geography was held in Berlin, and Gerland continued his own proposals. He suggested the establishment of an "International Society of Seismology" which would have the following aims.

1. Promote and extend as much as possible the macroseismic study of all countries;
2. Create a systematic organisation of instrumental observations;
3. Centralise the publications in appendices to the "Beiträge zur Geophysik".

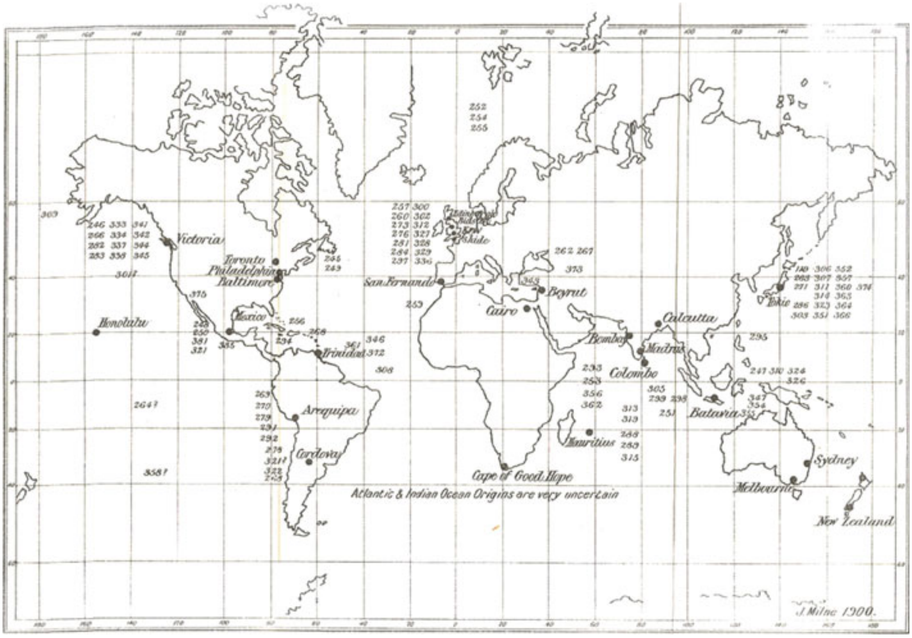


Fig. 35 Milne's global network in 1900, and earthquakes in 1899 (British Association 1900)

"Stations from all over the world will be members of the Society. The members of the Society will undertake the organisation of their country's seismological service, the publication of the collected data, and their mailing to the central station in Strasbourg" (Rothé 1981).

The Congress, after some discussion, passed a resolution stating, " The Congress approves the establishment of an International Society of Seismology and thinks it advisable to form a Permanent Commission for the international study of earthquakes. The Congress instructs the Bureau of the Congress to undertake the formation of such a Commission". The Permanent Commission, when assembled, consisted of Lagrange (Belgium), Futterer, Gerland, Helmert, Wagner and Wiechert (Germany), Omori (Japan), Belar and Laska (Austria), Levitsky (Russia), and Forel (Switzerland).

This was probably not welcome news to Milne, as worldwide stations were already mailing data to the "central station" at Shide. Gerland proceeded to organise the First International Conference on Seismology on 11–13 April 1901. Neither Milne nor anyone else from Britain was present, nor was the conference mentioned in the subsequent British Association report.

The most important resolution to be passed at the First International Conference on Seismology proposed that " The Conference of Seismology, held in Strasbourg, considering the great interest of a world-wide collaboration recommends the establishment of an Association of States for the carrying out of certain tasks which cannot be accomplished by individuals or a regional initiative. The conference requests the government of the German Empire to take the necessary steps preparatory to a Treaty of Association". Various countries were informed through diplomatic channels of the resolutions adopted by the Strasbourg conference in 1901, and were invited to become members of the International Association (Rothé 1981).

This will be recognised today as the precursor of the system of international membership that is the backbone of IASPEI and its sister associations. But aside from whatever Milne may have felt personally about Gerland encroaching on his domain, the events must be seen through the perspective of the period, in which Britain as the world's superpower was highly suspicious of German attempts to find a "place in the sun"—it was in 1901 that the phrase was uttered by Kaiser Wilhelm II in a speech to the North German Regatta Association.

Since the invitation was issued through diplomatic channels, it went first to the British Government, who forwarded it to the Royal Society to deal with. The date for the Second International Conference had been set for 23–28 July 1903, and GH Darwin and Milne were appointed the British delegates. Milne pointedly reported back that out of 100 delegates, 62 were Germans! However, votes were taken on the basis of one country, one vote (British Association 1905).

The proposals that came out of the conference were for a Central Association for Seismology with its headquarters in Strasbourg (as Strassburg, it was German between 1871 and 1918). There was to be a Governing Committee, made up of national delegates, a Chief in Strasbourg and a President and General Secretary from outside Germany. The costs were to be shared amongst participating countries according to a pro rata based on population. For Great Britain this worked out at £160 per year, but Milne estimated that with the expense of contributing to the Governing Committee, it would come to about £200 per year.

Darwin and Milne explained that they did not have the authority to make a commitment. According to Rothé (1981), the two British delegates expressed strong differences of opinion, Milne being concerned that the Association would insist on the use of a standard instrument design (which would not be the Milne instrument that he had already been sending round the world). Also, he was concerned that the Central Bureau might have powers that would interfere with national undertakings. Rothé (1981) remarked, years later, "it may be said that Milne's attitude in 1903 was to be a burden on the whole future development of the Association".

On the return of Darwin and Milne, the issue was handed back to the Seismological Committee of the Royal Society, chaired by the geologist (and friend of Milne) John Wesley Judd (1840–1916), and which shared a good proportion of its membership with the British Association committee. The Royal Society pronounced that the money that would be needed would be better spent supporting the work of the British Association committee. If the needs of the British Association work were satisfied, *then* any additional funds could be used to join the Central Association.

The matter was then referred to a committee of the International Association of Academies, who recommended that the Associated Academies, with the Governments, should join the Seismological Association on two conditions: that membership could either be through a Government itself or through one of its scientific bodies; and that the status of Strasbourg as the headquarters should not be enshrined in the terms of the Convention. The Seismological Committee of the Royal Society then added a further three conditions for British membership: that the changes proposed by the International Association of Academies should be accepted, that the USA and France should join (France was not represented at the Second Conference, and political relations between France and Germany at the time were worse than those between Britain and Germany), and that seismology should receive state support in Britain.

The International Seismological Association was duly formed on 1 April 1904, and included the USA, but not France or Great Britain. Britain was not entirely isolated from the Association; in fact it was decided to invite someone from Britain to be the President, but the invitation went to Arthur Schuster from Manchester University, who was not a member of the British Association committee (he joined later, in 1910). This put Schuster in a difficult position, since Britain had not formally joined, and he declined.

Schuster (1851–1934) is remembered today more for his contributions to other branches of physics than for contributions to seismology, perhaps with the exception of Schuster (1897) in which for the first time claims of periodicity in earthquakes were refuted mathematically. That he was born in Frankfurt (he became a British citizen in 1875) may have helped his position with respect to the negotiations in Strasbourg.

However, by the time of the second meeting of the Permanent Commission, Britain was a full member, and Schuster was elected President, with Forel from Switzerland as Vice-President.

It was not long before there was more friction between the International Seismological Association and Milne's committee. At the Dublin meeting of the British Association, Milne reported that the International Seismological Association had been communicating directly with stations in the British Association network and borrowing original seismograms that then got delayed or lost in the post. "The result has been that serious inconvenience has been experienced" (British Association 1909).

The Fourth International Conference, the last before the outbreak of WW I, was held at Manchester with Schuster as host, on 18–21 July 1911. (The Fifth Conference was scheduled for September 1914 in St Petersburg, but never took place.) Milne could not easily refuse to attend, but he took little part in the proceedings. Other British attendees included GH Darwin, CG Knott, RD Oldham (who had retired from being Director of the Geological Survey of India and was back in Britain), J Perry and JJ Shaw.

On the second day of the conference, Schuster read out a telegram from the President of the Board of Education:

"His Majesty's Government heartily welcome your Association, and wish success to their deliberations. They rejoice to see the attempt to organise seismological observations internationally, which was initiated by the British Association, under the influence of Dr MILNE, bearing fruit through your Association on British soil."

Further tribute to Milne was paid in the report of the Secretary General, Radó Kövesligethy, but how much this was appreciated by Milne is open to imagination. For the last 15 years he had been used to being in control; now it was clear that things were slipping away from him (Herbert-Gustar and Nott 1980).

It was inevitable, however, that global seismology needed to organised on a global basis; it could not continue to be run forever as a British monopoly. In 1895 there were few seismic observatories outside Europe; by the time of the Fourth International Conference, they were spread across the world; a short-lived station had even been set up in Antarctica as part of the Discovery expedition (Lovell and Henni 1999). This was Milne's lasting achievement and legacy.

7.3 Milne's writings

Milne's publications are many and various, cover a range of subjects beyond seismology, and some were published under pseudonyms ("Mark Kershaw" was one). A survey was made shortly after his death by Ballore (1914).

Besides his many papers, Milne published two books on earthquakes, one fairly early in his career, the other 15 years later. They give a good idea of Milne's views on the subject. The first of these, "Earthquakes and other earth movements" (Milne 1883), was written while Milne was still in Japan, and he lamented that there might be inaccuracies in the book that could have been avoided had he been in a position to consult libraries. The title may give one pause as to what the other "earth movements" might be. Milne distinguishes between earthquakes, "earth tremors", defined as "minute movements which escape our attention by the smallness of their amplitude", then "earth pulsations … overlooked on account of the length of their period", and "earth oscillations, or movements of long period and large amplitude".

Milne's (1883) use of terminology is not quite what we expect today. The epicentre is still the "epicentrum", and the focus is the "centrum", or origin, or "focal cavity", the last term redolent of old ideas of earthquakes being collapse events. What would be considered today the depth is the "seismic vertical".

In organising his material, Milne (1883) starts with seismometry, describing the various evolutions of earthquake recording from the simplest seismoscopes up to the Gray-Milne instrument. This is followed by three chapters on the nature of earthquake motion, two on effects on buildings, and one on effects on landscapes. This is followed by one on "disturbances in the ocean". This deals both with what we now call tsunamis, and also earthquake shocks felt at sea.

Next are two chapters on earthquake location, one on epicentre and one on depth. Surprisingly, perhaps, these are both largely based on non-instrumental observations, such as Mallet used. Then three chapters on the distribution of earthquakes in space and time, particularly time, in which the usual questions of periodicity, relationship to movements of the sun and moon, seasons, etc, are considered. This is followed logically by a chapter on earthquakes and meteorological conditions. Next comes a chapter on earthquakes and volcanoes.

The last chapter on earthquakes is on earthquake prediction, and then the other earth movements get one chapter each.

As befitted someone employed in a college of engineering, Milne (1883) demonstrates his practical concerns, and branches out from seismology into early earthquake engineering. He recounts a series of experiments undertaken in Japan in 1880 to investigate the effects of earthquakes on heavy brick arches, by taking measurements of arches in the building of the Imperial College of Engineering itself. He concludes that by designing a building with cracks or joints between parts that have different periods of vibration, one could prevent a degree of earthquake damage—thus anticipating some modern damage avoidance design practices. It is evident that such experiments could not have been carried out in Britain! Milne discusses the importance of the fundamental period of buildings, and outlines the practical demonstration of causing vertical springs of different period to vibrate at different speeds, a staple of open days and school talks ever since.

Milne's idea on the safest house one could design against earthquakes was for a one-storey construction with strong timber frame, "resting on a quantity of small cast-iron balls carried on flat plates bedded in the foundations" (Milne 1883, British Association 1885). This early proposal for seismic base isolation triggered an accusation of plagiarism from Stevenson (Sect. 6.5 above) on account of the same idea having been developed by his father David for use in Japanese lighthouses (Stevenson and Smyth 1885). For more details of the exchange, see Muir Wood (1988),

On earthquake causation, in 1883 Milne believed that the majority of earthquakes were due to explosions linked to volcanism, specifically to water reaching down into the crust via subsea fissures, meeting heated rocks beneath, and generating explosive bursts of steam. However, some earthquakes "are produced by the sudden fracture of rocky strata or the production of faults" which might be due to stresses brought about by crustal elevation.

By 1898 things had changed somewhat. In the introduction to his second book, Milne writes that whereas some chapters in the new book have the same titles as those in Milne (1883), "the subject matter of these largely consists of observations which are not only new, but more extensive and trustworthy than were formerly obtainable. The result of this is that the conclusions which are formulated are not only more definite than those hitherto arrived at, but are in many instances novel in character" (Milne 1898).

A major difference was that in 1883, the first teleseismic record of an earthquake had yet to be obtained. In 1898 Milne was back in England, where he found that the study of earthquakes was just as practical as in Japan, thanks to the developments in seismometry.

A marked change is the discussion on causes of earthquakes, now moved to chapter three.

> To produce earthquakes which are felt over areas of five or ten thousand square miles, and which give rise to waves which may be recorded at any point on our globe, it is difficult to imagine how the primary impulse could have originated at a volcanic focus … to shake the whole surface of our globe it would appear necessary that the initial effort should be exerted on a surface very much larger than we can reasonably expect to exist beneath a volcano (Milne 1898).

Milne observes that great earthquakes are associated with oceanic trenches; and also with mountain ranges such as the Apennines. For the modern reader it seems strange that Milne's book should start with a chapter on bradyseisms, but Milne uses the term not quite in the modern sense, but to indicate what we would call tectonic uplift and subsidence. "Wherever bending is taking place in the Earth's crust we find earthquakes, while if this process is going on in the vicinity of an ocean we find both earthquakes and volcanoes" (Milne 1898). Milne has observed the elements of subduction zones, without, of course, understanding the process. In contrast to Milne (1883), while some earthquakes "are due to explosions at volcanic foci", these are few and small. "… the majority of earthquakes, including all of any magnitude, are spasmodic accelerations in the secular folding or 'creep' of rock masses" (Milne 1898).

7.4 Milne's world catalogue

Produced towards the end of his life, Milne's world earthquake catalogue is a curious document. It was the first published attempt at a global historical earthquake catalogue since Mallet, a fact of which he was well aware. In the introduction to the catalogue, which is interesting in itself, Milne (1912) actually quotes Mallet remarking that after his (Mallet's) endeavours, any "further expenditure of labour in earthquake catalogues … is now a waste of scientific time".

Milne disagreed, for various reasons: there were now new sources of data that had not been available to Mallet; Mallet had made errors that could now be corrected; and most importantly, there was a need for a catalogue that was consistent with regard to its threshold. This last point would now be expressed in terms of a threshold magnitude, but this concept had yet to be invented. Milne was concerned that his network was detecting "world-shaking earthquakes" at the rate of about 60 a year. What he wanted was a catalogue of earthquakes in history that were equivalent to the 60 or so per year that he was detecting. Whereas Mallet, and other cataloguers, were putting in both minor and major earthquakes.

So the title of Milne's (1912) catalogue is significant: "A catalogue of destructive earthquakes". Milne proposed a simple three-part classification, which he referred to as intensity, but in fact was a classification based on maximum intensity. The scale can be summarised roughly (and very much reworded) as follows:

- I Walls cracked, chimneys broken, damage within a radius of 8 km only, PGA of about 0.1g.
- II Buildings unroofed or shattered, damage within a radius of 30 km, PGA of about 0.15g.
- III Towns destroyed, districts devastated, PGA of around 0.3 g.

Obviously there is something of a gap between II and III, but it is interesting to see that Milne is already linking intensity to ground acceleration.

Milne therefore took his various sources and reduced them by removing weaker events. He gives as an example that between 1800-1808 Mallet has 407 events, which Milne reduced to 37 by removing those that caused no damage.

Milne's sources are interesting. The backbone of Milne's catalogue is made up of the various catalogues of Milne, Perrey and Carl Fuchs (1837–1886), who was a continuator of Perrey. To this he added a number of regional or national catalogues; for Britain he used Meldola and White (1885) and Roper (1889). But particularly intriguingly, he writes, "I have had the advantage of a large number of lists and documents relating to earthquakes collected from various parts of the world and put at my disposal by the Foreign, Colonial and India Offices" (Milne 1912). One can only wonder at what these documents were, and what happened to them? They are not amongst Milne's surviving papers. Was Milne passed bundles of original documents that have subsequently been lost, or did some clerk copy out parts of documents the originals of which are still buried in the appropriate archive? What one can tell is which earthquakes are so referenced; they are flagged as "O.D." for "official documents" in the catalogue, and are generally from the 1860s on.

Milne made some attempt at historical accuracy, rejecting some of Mallet's events as legendary, and attempting to cope with different dating systems. But he swallows Short's (1749) early events as genuine, via the medium of both Meldola and White (1885) and Roper (1889).

The actual format of the catalogue, which is not parametric, is: date, time if known, country, places affected, intensity and authority. Thus a typical entry reads:

"1511 Mar. 26 Italy, Cividale in (Friuli), Udine and many towns in (Venetia) and Triest. III B."

This is the well-known 1511 Idrija earthquake, and "B.", the source, is Baratta (1901).

More interesting is the following:

"1888 Oct. 10 India (Burmah), Rangoon I O.D. & Ti."

Here "Ti." is the Times newspaper. Reports of this earthquake in the European newspaper press suggest that Rangoon was on the edge of the felt area, and the earthquake is deeply obscure, so it would be very interesting to know exactly what was in the "official documents".

Today, Milne's catalogue is an interesting endeavour of its period, particularly in its aims to examine what might be comparable historical precedents for the events that Milne was locating. He was well aware that the historical record was seriously incomplete, not least because of the absence of oceanic events. But it is not a particularly useful document of itself.

8 Milne's British contemporaries in Japan

Milne was not the only person from the UK who was invited out to Japan to teach. Aside from his Japanese colleagues, he worked closely with several other expatriates, whose contribution needs to be considered here. The most important were Thomas Gray and Alfred Ewing. One can also mention, among others, Cargill Knott, who replaced Ewing in 1883, and is best remembered today for his book "The physics of earthquake phenomena" (Knott 1908), and also for his work on reflection and refraction of seismic waves (Knott 1899).

8.1 Thomas Gray

Thomas Gray (1850–1908) was born in Fife and educated at Glasgow, where he studied under Lord Kelvin, whose name has already appeared in connection with seismology. In 1879, he

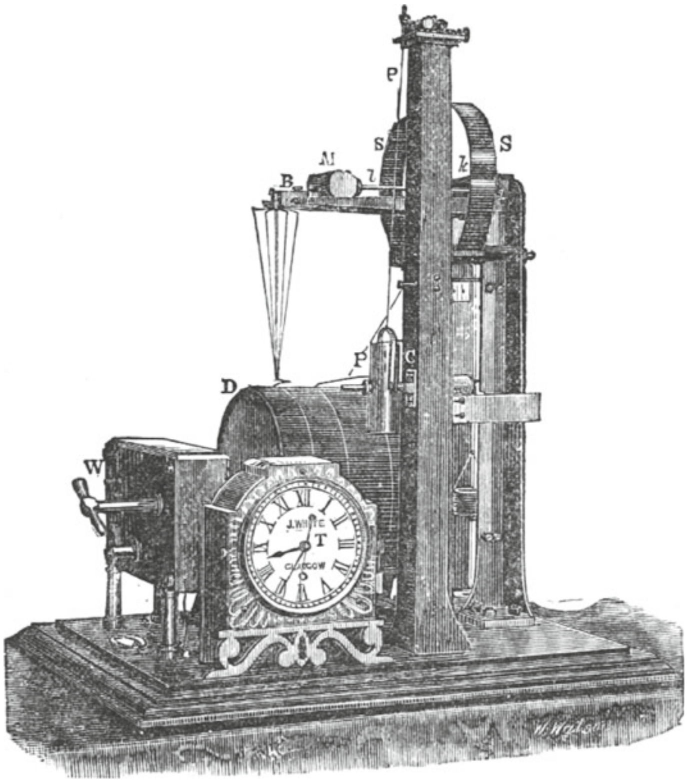


Fig. 36 Gray-Milne seismograph (Milne 1886)

travelled out to Japan to take up the position of Professor of Telegraphic Engineering, a post he held until 1881, when he returned to Glasgow (Rose Institute 1909, Davison 1927a; Muir Wood 1988). In 1888 he took a position as professor of dynamic engineering at the Rose Institute, Terre Haute, Indiana, where he remained for the rest of his life.

While in Japan, he acted as an assistant to Milne, who had given the recommendation that led to his appointment in Tokyo, and the two of them collaborated on a number of seismographs (Fig. 36). One of these, produced in 1885, is kept in the Science Museum and described by Wartnaby (1957); it is a three-component instrument with three recording drums. It was Gray who first came up with a design for recording vertical wave motion (Dewey and Byerly 1969).

Milne visited Gray in 1883, when he took a break from his work in Japan to travel back to Europe via the USA. Together, they arranged for the Gray Milne seismograph to be manufactured by James White, a Glasgow instrument maker (British Association 1884;Wartnaby 1957).

Gray made little contribution to seismology after returning from Japan, though he was appointed “heir to Mallet” in composing the section on earthquakes in the 5th edition of Admiralty Handbook (Muir Wood 1988).

It is something of a coincidence that in addition to the Gray-Milne seismograph, there is also a Gray-Milne Fund; same Milne, but different Gray. The Gray-Milne Trust was set up by Matthew Gray, a Kentish businessman who was inspired to an interest in seismology

Fig. 37 Alfred Ewing (BGS archive image)

by Milne, and who lent financial support to Milne's operations in Shide. The Trust was set up as a fund for supporting research into the physics of the earth, and Milne contributed a further legacy of £1,000 on his death, the whole being invested at the time in Canadian Pacific Railway shares. The Gray-Milne Fund was administered by the British Association's Seismological Committee until its dissolution in the 1980s; after that it was managed for a while by John Hudson of Cambridge University, and then transferred to the British Geophysical Assocation, a joint association of the Royal Astronomical Society and the Geological Society. It is currently used to support the annual "New Advances in Geophysics" symposium.

8.2 Alfred Ewing

James Alfred Ewing (1855–1935) was 5 years younger than Milne and Gray. Born in Dundee, he studied at Edinburgh before moving out to Japan to take a post as Professor of Engineering and Physics at Tokyo Imperial University in 1878 (Fig. 37). As Glazebrook (1935) put it, for an engineer in Japan, researching earthquakes was an obvious choice. His first instrument was built in 1879, and used a 7m-long common pendulum. This seismograph was not operated continuously and did not record its first earthquake until over a year later. By then, a more sophisticated instrument, Ewing's horizontal pendulum seismograph, was also in operation (Dewey and Byerly 1969).

It was this horizontal pendulum instrument, developed in 1880, that was capable of recording two horizontal components of motion on a continual basis, and produced the first clear recordings of earthquake motion (Ewing 1883; Davison 1935; Muir Wood 1988). According to Dewey and Byerly (1969), this was the first occasion on which a horizontal pendulum instrument was used to detect an earthquake—neglecting the primitive horizontal pendulum used at Comrie in the 1840s.

Previous instruments such as Palmieri's had used a clock that only started when the earthquake was detected. Ewing was the first to have a continual time measurement, and was therefore able to demonstrate that the first arrivals of earthquake motion were not the strongest, and therefore that instruments without continual recording would miss the first waves.

Most accounts of the developments in seismometry in Japan in the 1880s portray Milne, Gray and Ewing as colleagues, though the narrative given by Muir Wood (1988) has them as fierce rivals, Milne and Gray at the Imperial College on the one hand, and Ewing at Tokyo University on the other. Certainly, in Milne's various reports he seldom mentions Ewing.

After Ewing returned home from Japan in 1883, Milne was left alone and in control of further seismological developments in Japan. Ewing took up a post in his home town of Dundee as Professor of Engineering, and his research interests turned largely to magnetism. A publication on his duplex pendulum instruments (Ewing 1886) seems to have given rise to a spat in print between Gray and Ewing, the course of which will be found in Muir Wood (1988).

Although Ewing's main research interest after returning to Britain was in magnetic hysteresis, which he discovered and named, he still had some involvement in seismology—he was a member of the British Association's Committee on Earth Tremors in 1893, and stayed on when this was merged with Milne's committee in 1895. He was involved in the setting up of the observatory on the summit of Ben Nevis (Ewing 1886), though he had reservations about the usefulness of the site as regards seismology: "a visit to the Observatory there has convinced the writer that to use [a seismometer] on that site … would be a matter of extreme difficulty" (Ewing 1885).

Ironically, one of the members of the British Association committee that initiated the Ben Nevis Observatory was none other than David Milne Home before his death in 1890. Ewing also mounted an experiment to measure the vibrations caused by trains crossing the Tay Bridge, using a duplex pendulum (Ewing 1888), realising that methods developed to record earthquakes could also be used to measure the response of engineered structures to vibration due to wind, moving loads, or other causes.

Ewing became Principal of Edinburgh University in 1916, and saw through the acquisition of land on the southern edge of the city on which to build the university's new science campus, King's Buildings—which very appropriately became the home of British seismology in 1975 when the Institute of Geological Sciences (now BGS) occupied Murchison House on the northwest corner of the site.

He was knighted in 1906.

9 Davison and macroseismic monitoring in Britain

Charles Davison (1858–1940) is seen today as a marginal figure in the development of seismology, but in his lifetime he was one of the best-known names in Britain in connection with the subject of earthquakes, partly due to his popular books on the subject, and particularly due to his letters appealing for information that followed most British earthquakes around

Fig. 38 Charles Davison (BGS archive image)

the end of the nineteenth century and beginning of the twentieth. He was perhaps an unlikely person to fill this role, being by profession a schoolmaster (he taught mathematics at King Edward's school, Birmingham, until he retired to Cambridge in 1920). Seismology was therefore a hobby, and it is quite remarkable how much he achieved, given that his single-handed labours had to be accommodated in his spare time (Fig. 38).

Davison has already appeared in this monograph as Secretary of the British Association's Committee on earth tremors in mining districts from 1893, Joint Secretary of the British Association's Seismological Committee from 1896, and as a signatory to the resolution passed at the 6th International Geographical Congress calling for a global seismic monitoring network. It is clear that Davison was highly regarded as a seismologist in his day, and not solely in Britain. Davison records in the dedication of one of his books his friendship with John Milne, Omori, and Rebeur-Paschwitz. It is strange in the twenty-first century to imagine an amateur and, essentially, a hobbyist, to be so close to the top of a scientific community. For someone whose profession was that of schoolmaster, Davison was very well connected with the seismological community of his day.

What induced him to adopt this hobby is not known for certain, but one can make a fair guess: his first earthquake investigations started in 1889, the year after a minor earthquake rattled the Birmingham area, most strongly felt in Edgbaston, which is where Davison was living at the time (Musson 1994).

Up until that date, macroseismic surveys of British earthquakes had been one-off affairs, with the exception of the investigations at Comrie: Lowe in 1863, Stevenson in 1880, Meldola and White in 1884, as already discussed. Davison decided there was a need to study British earthquakes on a regular and consistent basis, and that he would take charge of this.

He also had some pronounced views on the subject. Firstly, he would quantify the data he collected by applying an intensity scale. No investigator at this time had attempted a numerate intensity map of a British earthquake. Macfarlane had applied his subjective scale only to what he personally felt in Comrie; Lowe only distinguished an area of stronger effects in 1863, and Sorby only described what a map of variations in intensity in 1884 would look like (Musson et al. 1990 reconstructed a map based on his description). Davison was aware of the use of the Rossi-Forel intensity scale in other countries, and resolved to apply it (with some modification) to Britain.

Secondly, Davison was well aware that earthquakes were produced by fault movements. He set himself the task of demonstrating this in Britain, and identifying, if possible, the actual faults responsible. He even believed that he could, with macroseismic data alone, determine something comparable to a modern fault-plane solution, identifying the sense of slip on a fault from the distribution of apparent directions of motion perceived by observers.

His failure in these objectives was due to lack of knowledge of two important things. He was not a geologist, and failed to grasp the three-dimensional nature of fault geology. Thus in the case of the 1903 Caernarvon earthquake in North Wales, he noted the proximity of the epicentre (which he probably determined fairly accurately) to the surface trace of a structure related to the Lleyn Shear Zone, which he believed to be the causative fault. He was unaware that the fault dips to the north-west, and the focus of the earthquake thus lay well within the footwall block.

Secondly, he greatly overestimated the ability of observers indoors to estimate the direction of an earthquake shock. It is well known now, but not in Davison's day, that seismic waves are distorted by interaction with the built environment, and therefore the apparent direction of motion indoors may not be the true one.

Davison also developed a tendency to jump to conclusions and attempt to fit data to his hypotheses. His first investigation of the 1890 Inverness earthquake (Davison 1891) concluded the causative feature was not the Great Glen Fault. Later, after having decided that the Great Glen Fault was the controlling structure for earthquakes in Northern Scotland, he revisited the 1890 data and concluded it pointed to the Great Glen Fault after all (Davison 1902).

Likewise, having decided that a fault between Hereford and Ross on Wye was responsible for all Hereford earthquakes, he resolutely concluded that the earthquake of 15 August 1926 must originate on the same structure, when in fact the data point to an epicentre on the Hereford-Shropshire border, well to the north.

In addition to his macroseismic work, Davison ran his own seismometer, an Omori, in Birmingham, and it is from this that we have the first surviving seismographic record of a British earthquake, an event near Derby in 1903 (Davison 1924; Neilson and Burton 1984), not counting the recording of the Colchester 1884 earthquake on a magnetometer at Richmond.

9.1 Davison's intensity scale

Davison made the study of intensity something of a speciality. His two papers in the Bulletin of the Seismological Society of America on intensity scales (Davison 1921a, 1933) are still useful today. The main scale in use internationally when Davison began his seismological career was the Rossi-Forel scale, which Davison nominally adopted, but in fact he modified it totally, creating what he later referred to more accurately as the Davison Scale.

The theory of intensity scales has not been greatly gone into in seismology. In composing an intensity scale, there are two conflicting priorities: consistency and inclusiveness (Musson 1996). Consistency is the principle that all diagnostics within one degree of the scale should

Table 2 Equivalence of Davison Scale and EMS

DS	EMS
1	1
2	2
3	3
4	4
5	4
6	5
7	5
8	6
9	7

refer to the same strength of shaking. Inclusiveness is the principle that the scale should include as many diagnostics as possible so as to be applicable in as many circumstances as possible. The problem, of course, is that the more diagnostics there are in any one degree of the scale, the less likely it is that all are equivalent.

Davison had a horror of inconsistency in a scale, and to achieve complete consistency, he was prepared to sacrifice inclusiveness. He reckoned that the one way he could be certain of consistency was to have only one diagnostic per degree of the scale. Thus guarantees internal consistency, but requires the diagnostic to be observable for the scale to be usable.

An advantage of this approach for Davison was that it fitted his means of gathering data. Given his circumstances, the only way he could collect data from across the country was the time-honoured method of the appeal via newspaper. This he could achieve by means of sending a letter to local newspapers in the affected area after an earthquake occurred, with a few short questions, each relating to a single diagnostic, and hence a degree of the scale. The highest numbered question to which the answer was "yes" would determine the intensity, and no subjective judgement would be required. So if the answer to "Did chandeliers, pictures, etc swing?" was "yes", the intensity would be marked as 6 DS (Davison Scale). This did rely on having observers in the vicinity of chandeliers and swingable pictures.

Given the capriciousness of earthquake effects, in reality it is possible to get the occasional toppling ornament (7 DS) when windows did not rattle (<4DS), but given enough observations such occasions should be infrequent. Where Davison's scale seriously broke down was in the wording of intensity 5 DS, which was defined as "the observer's seat is perceptibly moved or raised". What does this mean? The word "moved" is ambiguous—it could mean "moved" as in "displaced", or "moved" as in "shook". And does "seat" only mean a chair, or can it mean a bed as well? It does not matter what Davison intended by this badly-worded degree—what matters is what his respondents thought it meant, and there is no telling if different observers interpreted it in the same way or not. Thus for practical purposes, 5 DS is a meaningless intensity, or at least, it is not distinguishable from 4 DS.

One can therefore make a rough approximation between DS and EMS as follows (Table 2):

9.2 Macroseismic monitoring in Britain, 1889–1926

For someone to set himself up as a one-man macroseismic surveying operation for the whole of Great Britain, as a hobby, was a remarkable task. Yet this is what Charles Davison did, starting in 1889 with a study of the Bolton earthquake of that year, and continuing over the next few decades. The bulk of Davison's activity in this regard was to be over the next 20 years, in which period he undertook studies of all the major earthquakes in Britain and most of the minor ones that were felt to any degree. After about 1910, his activity became intermittent. He continued to collect data on the Ochil Hills earthquake swarm over the course of the first two decades of the twentieth century, but although he made a study of the 1916 Stafford earthquake, he did not publish anything on the 1915 Carlisle earthquake (though he did collect data on it).

Although he had largely stopped work by 1915, Davison's final study was of the 15 August 1926 event, which he located near Hereford, though the true epicentre was on the Herefordshire-Shropshire border. Davison's last appearance in print on the subject of a British earthquake was, so far as I am aware, a short note on the 1931 North Sea earthquake, in which he comments that the 1185 Lincoln earthquake might have had a similar offshore epicentre (Davison 1931).

Davison's procedure was, after a significant earthquake, to write to local newspapers and ask them to insert his standard questionnaire in their correspondence column. This questionnaire, because of the structure of Davison's intensity scale, was quite short, so this was not onerous for the newspaper. His expenses were met by a Government grant, official recognition of the usefulness of his activity. (One might also ask how many people have ever received financial assistance of this sort to fund a hobby).

In the case of a relatively major event, Davison would publish a paper devoted to the results. Minor earthquakes he would collect together, and issue a paper summarising the smaller earthquakes of a given year or span of years. Much of the discussion in these papers is devoted to reported times of observation, estimates of direction, number and character of shocks, and sounds heard, most of which is uninteresting to the modern reader. He usually has little to say about the physical effects of an earthquake, leaving this to the isoseismal map. Since he drew very smooth isoseismals, and usually did not plot individual intensity points (Fig. 39), and given the eccentricity of the Davison Scale, these maps are not of great use today in recovering the macroseismic field. Furthermore, all the reports that Davison collected are lost—reputedly his papers were burnt by his widow.

The one major exception is the 17 December 1896 Hereford earthquake. On this occasion, Davison published all his questionnaire data in the form of a book (Davison 1899), and this book affords a much better insight into the details of Davison's working methods (for instance, how he constructed his isoseismals) than do his various papers. Though the same problem arises for the modern seismologist: intensity data gathered using Davison's ambiguous scale is not easily usable today.

Davison's scrapbook of newspaper cuttings for the 1896 earthquake also survives, in the Milne Library collection, formerly kept in the Science Museum and now at Imperial College Library. This is particularly good fortune; during WW II, a German bomb struck the building holding the British Museum Library's newspaper collections. Since these were stored by year, the explosion took out almost all the local newspaper holdings for, as it happened, 1896. Thanks to the survival of Davison's scrapbook, the descriptions of this earthquake from the local newspaper press still survive.

Whereas Stevenson and Meldola were aware of a possible link between earthquakes and local faults, Davison was the first person in Britain to set out purposefully to identify individual faults for British earthquakes using seismological data. His methods were over-ambitious, and his geological knowledge inadequate, but he deserves full credit for understanding that an appropriate task for the observational seismologist is to try and identify the causative structure for any individual earthquake, and for putting together a programme to try and achieve that goal to the best of his ability.

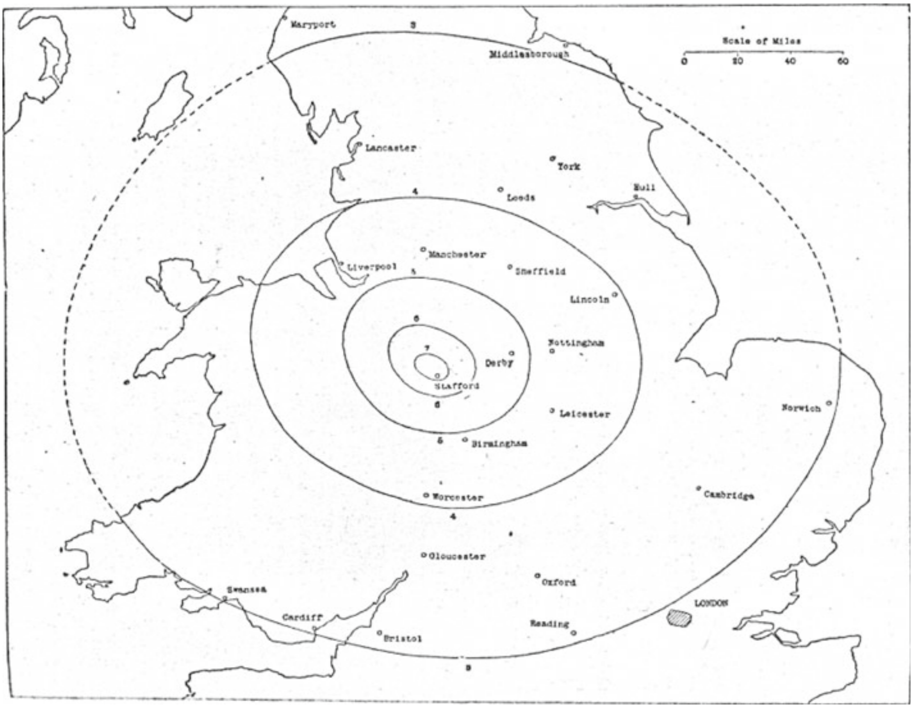


Fig. 39 A typical Davison isoseismal map; the 1916 Stafford earthquake Davison (1919)

9.3 Davison's catalogue

At 416 pages in length, "A history of British earthquakes" (Davison 1924) was the most ambitious catalogue of purely British earthquakes to date. To some extent it is a summary of Davison's career in monitoring British earthquakes; a large amount of the text is occupied by accounts of earthquakes between 1890 and 1920, with a synopsis of Davison's observations, and with isoseismal maps for the larger events. However, it is intended truly to be a complete catalogue, and the earliest entry is the earthquake of 974, known from the Anglo-Saxon Chronicle.

Compiling the catalogue was a long-term project. It seems that Davison conceived the idea some time in the late 1890s, as around this time he issued an appeal for information in the form of letters to newspapers. So the overall gestation must have been of the order of 30 years, the final preparation of the volume only done after he retired from school teaching. Davison (1924) opens the preface to his catalogue by speaking of "a study that has occupied the greater part of my leisure time during the last 35 years", but this is clearly a reference to his involvement in seismology overall.

Davison (1924) opens with a list of sources used, starting with medieval chronicles, then scientific journals and other periodicals, including inevitably Gentleman's Magazine and the Annual Register, and then previous earthquake catalogues. Of these he notes fifteen, of which some are minor, like Drummond's (1842) list of events at Comrie. The major sources are Mallet (1852–1854a), Milne (1842a); Perrey (1849) and Roper (1889). Like Meldola and White (1885); Roper (1889) and for that matter, Milne (1912); Davison (1924) is aware of the earthquake list by Short (1749), but unlike the previous authors, he smells a rat, and is the first writer to realise that there is no valid source material that Short could have drawn on, and therefore the records are at best highly dubious. Davison believed Short (1749) to be the earliest catalogue to include British earthquakes, which is obviously not true.

The catalogue itself is arranged as a table with six columns: an index number, the date, the time (if known), a descriptor for the location, which may be as vague as a county or even “England”, the maximum intensity (DS), and in some cases the “disturbed area” in square miles. The table contains 1,191 events, the last being a Hereford earthquake on 26 January 1924.

The bulk of the work which follows is a descriptive catalogue arranged in regions, starting with a chapter on the north of Scotland, and working generally southwards and eastwards, and finishing with a section on events of uncertain location. Within each chapter, events are further divided into “centres”, each centre corresponding, as Davison thought, to a particular causative fault, known or unknown. Thus there are Perth earthquakes, Dunning earthquakes, Glasgow earthquakes, and so on, each centre named after a nearby town or city for ease of reference. Earthquakes that Davison regarded as “twin” events are separated out from the others. For each centre, quakes are presented in chronological order. Earthquakes that Davison (1924) believed genuine have a paragraph heading with their catalogue number; events considered doubtful are simply mentioned in passing.

Although Davison was not a historian, he was at least aware of some of the problems of historical research, for instance, the problems of the dating of medieval earthquakes, and the tendency of early writers to use “terra motus” for events that are not actually earthquakes. Although he made an attempt to sort out these problems, he was not always successful.

Mining events, which Davison referred to as “earth-shakes” are relegated to a chapter at the end of the book, along with earthquakes in Ireland, the Channel Islands, the Shetlands, and the 1755 Lisbon earthquake.

Davison concludes with a chapter on sound effects of earthquakes, an analysis of spatial and temporal distribution (the usual question of monthly frequency), and finally some speculations on the origins of earthquakes in Britain.

Davison (1924) is not quite a parametric catalogue. There are no co-ordinates given for any event, but from Davison’s map of the discrete centres of activity, it would be possible to assess the latitude and longitude Davison would have assigned had he been minded to do so. This was actually attempted much later by Roy Lilwall of the Institute of Geological Sciences, in the mid 1970s (Burton et al. 1984b). And of course, there are no magnitudes, since the concept of magnitude had not been invented in Davison’s day.

9.4 Other writings

Charles Davison probably published more papers and books than any other British seismologist up to that point, no mean achievement for an amateur. His papers, mostly appearing in Geological Magazine and Quarterly Journal of the Geological Society, can be divided into several categories:

- Papers on individual British earthquakes (the larger ones);
- Papers grouping together several minor British earthquakes over one or two years;
- Papers on general topics of seismology.

The papers falling into the first two categories, between them, cover most British earthquakes for the first two decades of Davison’s activities, although for the modern researcher, the information he includes is very limited in its usefulness. Details of the effects of each earthquake are largely restricted to smooth isoseismal maps (like Fig. 39), and given Davison's methods, it is not possible to estimate over how much of the broad ellipses the earthquakes were actually felt, or whether the individual towns plotted on the maps are actually places from which Davison had data, or just plotted for geographical reference.

Davison's main interest was attempting to assess the equivalent of a modern focal mechanism using only macroseismic data, thus his interest is in the timing of observations and estimates of direction, rather than strength of shaking.

His thematic papers cover a wide variety of topics, some of which are not of great interest today, such as studies on earthquake periodicity. As mentioned, two that remain of considerable usefulness are his two surveys of intensity scales (Davison 1921a, 1933) published in the Bulletin of the Seismological Society of America.

Davison also published a number of books aimed more or less at a general readership (e.g. Davison 1905). Two of his books (apart from his catalogue) deserve particular mention here. The first is his "The Founders of Seismology" (Davison 1927a), a history of seismology from Bevis to Omori. This partly draws on three papers from the Geological Magazine (Davison 1921b,c,d), and the rest of the book was written, interestingly, at the suggestion of Sir Archibald Geikie.

Davison (1927a) is of lasting value as a reference, and proved a useful resource in the writing of the present monograph. One cannot say the same of "A Manual of Seismology" (Davison 1921e), which is more of interest as a reflection of the state of knowledge of its time, and even then it has lapses—one looks in vain for any mention of elastic rebound. It is more of a textbook than a manual; it covers a wide range of subjects, some of which reflect particular interests of Davison, that would not merit the same treatment today. For instance, a whole chapter is devoted to earthquake sounds. One might even argue this is a particularly British interest—in a low-seismicity, low-intensity intraplate environment, the sound an earthquake makes tends to be a large part of what observers report.

Davison (1921e) reprises a topic found in many of his papers—the idea that some earthquakes are "twin" events, in which two similar and related shocks occur a few seconds apart. Davison's evidence for this seems largely to be a misinterpretation of reports from observers of feeling two shocks, that actually reflect P and S-wave arrivals, and Davison's twin earthquakes were ridiculed accordingly by Richter (1958). Although Davison's interpretation might be wrong, he was not wrong about the existence of such events—the 1988 Ambleside earthquake was a true twin earthquake, as shown from instrumental data (Ford et al. 1997).

In fact, Davison (1921e) goes further, and divides earthquakes into simple, twin and complex, the latter class being those great earthquakes with rupture durations of "3 or 4 min, during which there are many variations of strength and rapid changes of direction".

As to the ultimate cause of earthquakes, Davison (1921e) advances a connection between earthquake faulting and mountain building, but "as to the precise cause of the great and widespread movements we are still ignorant".

10 Davison's successors

When Davison retired from undertaking macroseismic surveys in the mid 1920s, he left a gap which could not easily be filled. While the earthquakes Committee of the British Association continued to meet, there was no obvious institution to take over the task, so future macroseismic surveys, up until 1974, rested on some enterprising person deciding to take on the task. Davison's mantle eventually fell on the shoulders of the geologist ATJ Dollar.

10.1 Mourant

Arthur Mourant (1904–1994) was a native of Jersey, and by profession was a haematologist with a special interest in blood transfusion. But his interests were wide, and included earthquakes. He made a special study of earthquakes in the Channel Islands, both modern and historical (Mourant 1930, 1931, 1937 amongst others), and conducted a macroseismic survey of the 17 February 1927 Channel Islands earthquake, one of the largest earthquakes to affect Britain in the twentieth century, with a magnitude of 5.4 ML.

In the 1920s he was a student at Oxford, and worked with Harold Jeffreys and HH Turner. He maintained communication with HH Turner up until his death, and throughout the 1930s with JP Rowland at Stonyhurst, as he told me in a series of letters and phone calls in the early 1990s. It was rather a strange experience in the 1990s to be talking to someone who was active in seismology in the 1920s.

10.2 Tyrell

George Tyrell (1883-1961) was an English geologist hired as a lecturer at Glasgow by JW Gregory in 1913. Gregory had already made a venture into earthquake investigation, with his study of the 1910 Glasgow earthquake (Gregory 1911—the data collected by Gregory are still in the archives of Glasgow University). It may have been Gregory who suggested to Tyrell that he should mount a study of the 23 December 1925 Oban earthquake. Tyrell collected macroseismic data on this and two other earthquakes in the next two years, the important 24 January 1927 North Sea (Viking Graben) earthquake, the second largest earthquake to affect the UK in the twentieth century, and the rather more minor Colintraive earthquake three days later on the 27th.

The results of these were collected in a paper on "Recent Scottish earthquakes" (Tyrell 1932). Comparing Tyrell's results with contemporary accounts of these three earthquakes makes clear the deficiencies of Tyrell's work. His smooth isoseismals grossly overestimate the effects of the earthquake. The Oban earthquake, for instance, produced a single report of a window rattling in a railway signal box in the Great Glen, to which Tyrell assigns an intensity 4 RF (Rossi-Forel), and uses this single report to extend his isoseismal 4 way up to the northeast of where it should run.

10.3 Versey

On the occasion of the largest British earthquake on record, on 7 June 1931 (epicentre in the North Sea) it was the turn of the geologist Henry Versey (1894–1990). Versey was a Yorkshire geologist, primarily associated with the University of Leeds, where he studied as an undergraduate, and eventually rose to the position of Emeritus Professor of Geology. His study of the 1931 earthquake was predominantly macroseismic, though he cites instrumental data collected by Father Rowland of Stonyhurst (Versey 1939). He reported that he had collected 400 reports of the earthquake, which he assessed using the MCS (Mercalli-Cancani-Sieberg) scale, the better to be able to compare his data with that collected on the continent (the earthquake was felt on all coasts of the North Sea), though no such collation was ever published, and the first attempts to produce an isoseismal map for the whole felt area of the event were those of Principia (1982) and Ambraseys (1985).

Fig. 40 ATJ Dollar in 1977 (BGS archive image)

Following Davison's practice of attaching a location name to earthquakes after the nearest settlement, Versey (1939) gave this offshore event the name "the Withernsea earthquake", after a coastal village north of the Humber. The name did not stick.

Versey made one other foray into seismology, on the occasion of the Skipton earthquake of 1944, very much closer to home, again collecting macroseismic data via newspaper appeals for the earthquake and producing an isoseismal map, this time explicitly using the Davison Scale (Versey 1948). He attributed the earthquake to the Craven Fault.

10.4 Dollar

The geologist ATJ Dollar is most frequently referred to by his initials, but his full name was Archibald Thomas John Dollar, or John to friends. He graduated in geology from King's College, London in 1931, and moved to Emmanuel College, Cambridge, where, supervised by CE Tilley, he gained his PhD on the petrology and structure of Lundy. In 1935 he became a Research Fellow at Emmanuel, and after a brief period at St Andrews University moved to Glasgow University in 1939. In 1950 Dollar became head of the Geology Department at Birkbeck College, and remained there until his retirement (Seager 1985a; Lovell 1999). A photo of Dollar in old age is reproduced here as Fig. 40.

It was while Dollar was still at Cambridge that he became interested in British earthquakes. We cannot be sure of the exact catalyst, but Cambridge was where Davison was living (in retirement) at the time, and the two certainly met and discussed macroseismic methods. Thanks to the careful preservation of Dollar's papers (unlike those of Davison), a lot is known about Dollar's working methods, and also the state of seismology in Britain at the time. It is through Dollar's correspondence that many details of minor seismological observatories are known (chapter 11). Dollar's papers are described in Lovell (1999) in more detail than is possible here.

10.4.1 Investigations of British earthquakes

Dollar's first aim was to continue Davison's (1924) earthquake catalogue beyond 1915 (the date at which it becomes rather incomplete). In a major initiative lasting a year or more he circulated libraries, museums, newspapers, universities, scientific societies, etc., asking for information on earthquakes since January 1916. However, his catalogue, when it was eventually published in 1950, only covered earthquakes in Scotland (Dollar 1950). All that survives of his complete post-Davison UK catalogue is a small red tin box holding index cards, one for each earthquake. Strangely, these are arranged, not chronologically, but in alphabetical order. So the Glasgow earthquake of 7 March 1964 is found under "C" for Clydesdale. The card collection begins with the 1901 Inverness earthquake, and runs up to the 13 January 1970 Chichester earthquake. In most cases the cards give only a place, date and time.

Dollar also elected to continue Davison's work of macroseismic investigation of current British earthquakes, but using a somewhat different system. Instead of placing questionnaires in newspapers after an event, he recruited a network of volunteer observers up and down the country. Each observer was supplied with printed questionnaires on small cards, which after an earthquake could be filled in, folded over and posted to Dollar. He later changed the system to questionnaires that could be inserted in special envelopes that had printed in red, "British Earthquake Enquiry Birkbeck College London URGENT".

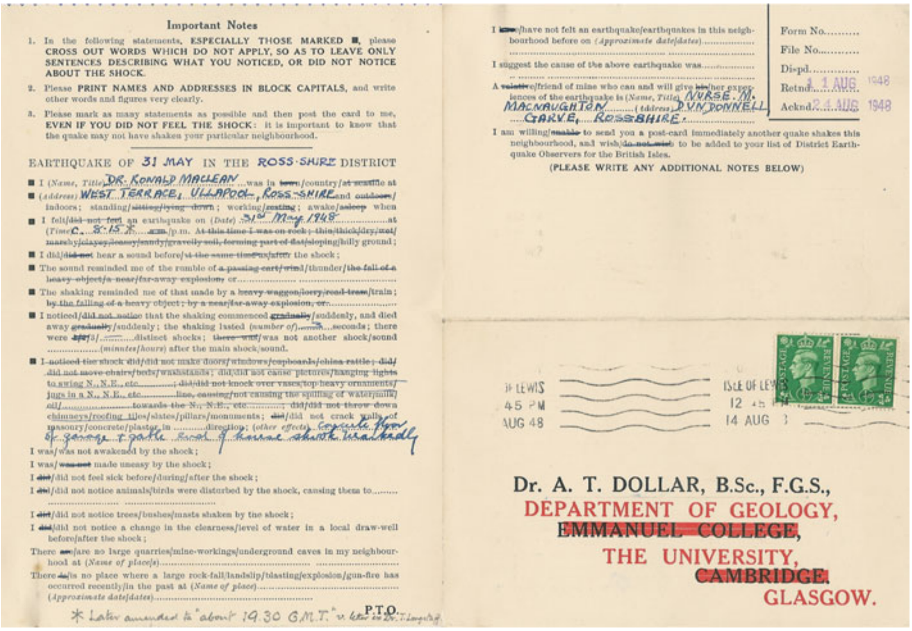
While this system may net fewer replies than an appeal through a newspaper, it is has the advantage that no special action is required after an earthquake—the observer network will kick in automatically. The seismologist can also have some control over the quality of the respondents and hence the data collected, and the same system was run very successfully in Slovenia in the late twentieth century (Cecić 1992).

Since Dollar's aim was to continue and extend Davison's work, to that end he adopted much of Davison's methodology. His questionnaire (an example is shown as Fig. 41) is based on Davison's, and he continued to use the Davison intensity scale.

This means that, unfortunately, while his collected data still survive, their usefulness to the modern seismologist is limited, in that isolated reports with little description of effects do not allow one to make reliable inferences about the intensity distribution.

Also, Dollar was not very conscientious in publishing his results. Descriptions (but not maps) of Scottish earthquakes in the period 1916-1949, partly based on data from his observer network, are found in Dollar (1950), and the Inverness-shire earthquake of 25 December 1946 was the subject of a separate paper (Dollar 1947). However, most of the material he collected for English and Welsh earthquakes of the period never saw the light of day, and some of it contains details of importance, for example, on the 12 December 1940 North Wales earthquake, which was under-reported by the newspapers of the day because of the war. It is through this material, for instance, that one discovers that two deaths, rather than one, resulted from this earthquake, the last earthquake fatality in Britain (Musson 2003). The earthquakes for which Dollar collected data can be found in the list of papers compiled by Lovell (1999).

The exception is the Castle Donnington earthquake of 11 February 1957, the largest earthquake that Dollar ever worked on. His findings were published as Dollar (1957). He made preparations to study the 23 July 1966 Helston earthquake, since a pile of pre-printed questionnaires with the date of the earthquake have been found, but he does not seem to have actually distributed them. Seager (1985b) dates the end of Dollar's observer network as 1973.The last earthquake mentioned in any of his papers is the 3 November 1976 Widnes event, but all that is to be found is calculations based on instrumental data (Lovell 1999).


Important Notes

1. In the following statements, ESPECIALLY THOSE MARKED ■, please CROSS OUT WORDS WHICH DO NOT APPLY, SO AS TO LEAVE ONLY SENTENCES DESCRIBING WHAT YOU NOTICED, OR DID NOT NOTICE ABOUT THE SHOCK.
2. Please PRINT NAMES AND ADDRESSES IN BLOCK CAPITALS, and write other words and figures very clearly.
3. Please mark as many statements as possible and then post the card to me, EVEN IF YOU DID NOT FEEL THE SHOCK: it is important to know that the quake may not have shaken your particular neighbourhood.

EARTHQUAKE OF 31 MAY IN THE ROSS-SHIRE DISTRICT

■ I (Name, Title) DR. KONALD MACLEAN ...was in town/country/at seaside at
■ (Address) WEST TERRACE, ULLAPOOL, ROSS-SHIRE and outdoors/indoors; standing/sitting/lying down; working/resting; awake/asleep when
■ I felt/did not feel an earthquake on (Date) 31st May 1948 at (Time) C. 8.15 * a.m./p.m. At this time I was on rock; thin/thick/dry/wet/marshy/clayey/loamy/sandy/gravelly soil, forming part of flat/sloping/hilly ground;
■ I did/did not hear a sound before/at the same time as/after the shock;
■ The sound reminded me of the rumble of a passing cart/wind/thunder/the fall of a heavy object/a near/far-away explosion, or....................
■ The shaking reminded me of that made by a heavy waggon/lorry/road-tram/train; by the falling of a heavy object; by a near/far-away explosion, or....................
■ I noticed/did not notice that the shaking commenced gradually/suddenly, and died away gradually/suddenly; the shaking lasted (number of) ...3... seconds; there were 2/3/........distinct shocks; there was/was not another shock/sound ....(minutes/hours) after the main shock/sound.
■ I noticed the shock did/did not make doors/windows/cupboards/china rattle; did/did not move chairs/beds/washstands; did/did not cause pictures/hanging lights to swing N., N.E., etc............; did/did not knock over vases/top heavy ornaments/jugs in a N., N.E., etc............line, causing/not causing the spilling of water/milk/oil/...................towards the N., N.E., etc...........; did/did not throw down chimneys/roofing tiles/slates/pillars/monuments; did/did not crack walls of masonry/concrete/plaster in ....direction; (other effects) Concrete floor of garage & gable end of house shook markedly
I was/was not awakened by the shock;
I was/was not made uneasy by the shock;
I did/did not feel sick before/during/after the shock;
I did/did not notice animals/birds were disturbed by the shock, causing them to..........
I did/did not notice trees/bushes/masts shaken by the shock;
I did/did not notice a change in the clearness/level of water in a local draw-well before/after the shock;
There are/are no large quarries/mine-workings/underground caves in my neighbourhood at (Name of place/s)....................
There is/is no place where a large rock-fall/landslip/blasting/explosion/gun-fire has occurred recently/in the past at (Name of place)....................
(Approximate date/dates)....................

P.T.O.

* Later amended to "about 19.30 G.M.T." v. letter to Dr. T. Longstaff

I have/have not felt an earthquake/earthquakes in this neighbourhood before on (Approximate date/dates)....................

I suggest the cause of the above earthquake was....................

A relative/friend of mine who can and will give his/her experiences of the earthquake is (Name, Title) NURSE M. MACNAUGHTON ....(Address) DUNDONNELL, GARVE, ROSS-SHIRE.

I am willing/unable to send you a post-card immediately another quake shakes this neighbourhood, and wish/do not wish to be added to your list of District Earthquake Observers for the British Isles.

(PLEASE WRITE ANY ADDITIONAL NOTES BELOW)

Form No..........
File No..........
Dispd..........
Retnd. 11 AUG 1948
Acknd. 24 AUG 1948

ISLE OF LEWIS 12 45 PM 14 AUG

POSTAGE REVENUE

Dr. A. T. DOLLAR, B.Sc., F.G.S.,
DEPARTMENT OF GEOLOGY,
~~EMMANUEL COLLEGE,~~
THE UNIVERSITY,
~~CAMBRIDGE.~~
GLASGOW.

Fig. 41 A Dollar questionnaire card (*both sides shown*). Note the change of address

10.4.2 Dollar's shock recorder

The Jaggar shock recorder was a simple seismograph designed by Thomas Jaggar (1871–1953) of the Hawaii Volcano Observatory for use in volcano seismology. The principle behind it (Jaggar 1929) was a simple design that was cheap ($25 in 1929), robust, and easy to build, and could be deployed round a volcano to record volcanic shocks. Several of an improved design were built by CF Powell of Bristol University in connection with an expedition to monitor the eruption of Soufriere Hills, Montserrat (Powell 1938). One of these subsequently came into the hands of the Geology Department at Glasgow, and was set up by ATJ Dollar at Dunira House, just west of Comrie, in September 1938.

The reason for choosing Comrie was presumably that on past record it was the place in Scotland most likely to be visited by a strong earthquake. The instrument was deployed in the croquet pavilion of Dunira House, and tended by one of the gardening staff, who was already in the habit of making daily meteorological observations. The instrument is shown in situ at Dunira in Fig. 42; the recordings are made on a cardboard disc, which had to be changed daily.

Although a number of local disturbances were recorded, no strong earthquakes were to occur at Comrie throughout the twentieth century. Then in December 1947, Dunira House burned down. The main house was completely destroyed, and the ruins can be seen today. An annexe to one side, that served as the servants' quarters, survived, and this was now sold off as a private home. The croquet pavilion had been untouched by the fire, but with the change in ownership, continued operation of the instrument was no longer possible.

Dollar therefore cast about for another location, and here he seems to have been influenced by the occurrence of an earthquake at Glen Roy on 25 December 1946. This he attributed to the Great Glen Fault (Dollar 1947), almost certainly wrongly, but it suggested somewhere near the Great Glen Fault as a suitable location for the Dunira shock recorder.

Fig. 42 Dollar's Jaggar shock recorder in the croquet pavilion at Dunira (BGS archive image)

He approached Fort Augusts Abbey, a small Benedictine foundation dating back to 1880 (the year the buildings were completed). The Jaggar instrument was installed in the Abbey by Dollar, and charge of it given to Father Andrew McKillop.

Having set the instrument up, it appears that Dollar had little further to do with it. Unfortunately it had been placed at the end of a corridor that led to a large wooden outside door, and every time the door banged closed, it registered on the Jaggar. Before long, the monks decided not to continue maintaining it, and it gathered dust until enquiries were made by BGS in 1998 as to its fate. This proved to be just in time to prevent the instrument, which seems to be the last surviving Jaggar shock recorder anywhere in the world, from being thrown out. It is now preserved in the National Seismological Archive (at the British Geological Survey in Edinburgh). The Abbey, which was suffering financial difficulties, closed shortly afterwards.

10.5 Tillotson

This chapter would not be complete without a mention of Ernest Tillotson (1904–1981). Tillotson was another prominent amateur seismologist, who, like Davison, was a schoolmaster by profession, holding the position of physics teacher at Aireborough Grammar School from 1927 to his retirement (Fig. 43). Tillotson had been a pupil of Robert Stoneley, and remained

Fig. 43 Ernest Tillotson in 1959 (BGS archive image)

a friend both of Stoneley and Harold Jeffreys. He published papers and popular articles on various seismological subjects, including overseas earthquakes such as the 1939 Accra event (Tillotson 1941), between the years of 1927 and 1974 (Lapwood 1981). In 1946 he took over the position of Secretary of the British Association Committee from JJ Shaw, and held the post until 1979.

While he did not conduct macroseismic surveys in the manner of Davison and Dollar, he made notes of such British earthquakes as were reported, and inserted these into the annual reports of the Committee. Towards the end of his career he collected these together and published them as a small catalogue (Tillotson 1974), which forms a bridge between the work of Davison and Dollar, and the subsequent monitoring of British earthquakes by the Institute of Geological Sciences.

11 Seismological observatories in Britain, 1900–1969

For the greater part of the twentieth century in Britain, there was no particular policy concerning the setting up or maintaining of seismological observatories. Observatories came and went so long as someone was interested in running them. This tended to mean that when an observatory closed, often due to the retirement or death of the chief person running it, the records would be lost or destroyed.

One can make a broad distinction between what might be considered "professional" observatories, where a seismometer was run as part of a station that was set up principally for meteorological or astronomical purposes, or by university departments, and "amateur" observatories, run by an individual as a hobby, because of an interest in seismology. One such instance has already been mentioned, in the case of Davison's Omori in Birmingham. In some cases the division is borderline.

Table 3 "Professional" seismological observatories 1900–1969

	Lat	Lon	Start	End
Aberdeen University	57.17	−2.10	1936	1967
Bidston Observatory	53.40	−3.07	1898	1957
Blacknest	51.36	−1.19	1961	Present
Cambridge Observatory	52.22	0.10	1956	*1960*
Cardiff Observatory	52.22	−3.17	1910	Unknown
Cork	51.88	−8.47	1911	1919
Down House	51.33	0.05	1947	*1949*
Edinburgh	55.93	−3.18	1894	Present
Eskdalemuir Array	55.33	−3.16	1962	Present
Eskdalemuir Observatory	55.31	−3.21	1908	Present
Fort William	56.80	−5.08	1883	1904
Galway	53.27	−9.05	*1960*	Unknown
Gorthleck			1962	1964
Herstmonceux	50.87	0.35	1960	1960
Jersey	49.19	−2.10	1935	Present
Kew	52.35	−1.58	1898	1969
Limerick	52.63	−8.68	1907	1915
Liverpool	53.42	−2.93	1932	*1950*
Maynooth	53.38	−6.58	Unknown	Unknown
Oxford	51.77	−1.25	1918	1947
Paisley	55.85	−4.43	1898	1918
Rathfarnham	53.30	−6.28	1916	1964
Shide	80.69	−1.29	1895	1918
Valentia	51.93	−10.25	1962	Present
Wolverton	51.31	−1.22	Unknown	Present
Wormley	51.13	−0.64	*1957*	Unknown

Because the overall number of operational seismometers in Britain was small, these amateur stations have more interest than might otherwise have been the case. A short study covering the whole period was published by Neilson and Burton (1988). Lovell and Henni (1999), made a systematic survey of historical observatories in Britain as part of a BGS project, and retrieved many interesting details, and this report remains the definitive text on the subject.

11.1 Professional observatories

Table 3 lists the various stations that I classify here as "professional" in that they were run by a university or observatory, and not as a hobby.

The information is largely from Lovell and Henni (1999) in condensed form. Where dates are conjectural, they are written in italics, and the range given does not consider gaps in operation. For example, Eskdalemuir Observatory started seismic recording in 1908, but stopped in 1925 when the instruments were transferred to Kew. It then resumed operation under the WWSSN (World Wide Standard Seismograph Network) programme in 1963. Given the significance of Eskdalemuir to British seismology, as the location of Britain's WWSSN station and also of the separate Eskdalemuir Array, run by the UK Atomic Weapons Establishment for test ban treaty monitoring since 1962, it is interesting to note that according to a letter preserved at the observatory, Milne visited the site along with Prince Galitzin, and expressed the opinion that it would not be a good site for seismology.

Also in Table 3, no account is taken of minor moves of site. Seismological observation in Edinburgh started on Calton Hill, just east of the city centre, but soon moved to the present site of the Royal Observatory on Blackford Hill on the southern edge of the city. The entry for Fort William stands for both the Ben Nevis summit observatory, and the subsequent low-level site at the foot of the mountain.

The list should be complete for stations that operated any time in the 1900–1969 period, with the exception of stations set up in the late 1960s that formed part of LOWNET when it officially started operation in 1970. I also miss out Dollar's Jaggar shock recorder at Comrie and Fort Augustus, since this has already been described.

In most cases they were manned stations, running classical instruments of the Milne-Shaw or similar types, except for some of the later ones. An exception is Gorthleck, near Inverness, which was a short-term deployment of a Willmore short-period instrument; the exact co-ordinates are unknown.

Some of the stations listed are obscure and were short-lived. For instance, the station at Cardiff is little known. The history of the seismological observatory in Cardiff is unusual, in that the body responsible was not a scientific or academic institute, but the Municipal Corporation. The history is given in full in Griffiths (1908) and Walford (1911), with a summary in Lovell and Henni (1999). The original impulse was provided by the Cardiff Naturalists' Society, who donated a Milne instrument in 1909 to Cardiff Corporation, after some years of discussion. A telescope had previously been gifted to the city in 1896, which was eventually (in 1906) accommodated in a small, purpose-built observatory at Pen-y-Lan. This now provided the home for a seismometer, set up under Milne's supervision, and funded partly by the Corporation and partly by public subscription. Recording started in 1910; it is unclear when it ceased. The last definite reference to it as being in operation is 1912, so its life may have been short (British Association 1912). The observatory building was ultimately demolished in the 1980s (Lovell and Henni 1999).

A perennial problem has been the fate of records. For example, Stonyhurst College, a Jesuit school, founded in 1794, had an observatory which carried out meteorological measurements from about 1838, and geomagnetic observations from 1863 (Robinson 1982). Its original seismograph was a Milne pendulum installed in 1908 (this instrument had previously been used in the Antarctic by Scott's 'Discovery' expedition in 1901-1904). The Milne instrument was replaced by a Milne-Shaw instrument in 1923 with the aid of a grant from the Royal Society (Anon 1949). The seismologist in charge was Father James Peter Rowland (1875–1947). Rowland became something of an authority in British seismology, and was often consulted by the press to comment on recent earthquakes (Fig. 44). However, when he died at the end of 1947, there was no-one else to take his place. Not only did seismological observation cease, but at some point someone threw out the instruments and all the seismograms.

Some years ago I visited Stonyhurst in search of any surviving material, but apart from one torn off page from a pamphlet, there was nothing to be found of 40 years of seismological observation.

The fate of the station at Oxford University was similar, except in this case three seismograms survive—one only because it was borrowed by ATJ Dollar who forgot to return it.

Fig. 44 Father JP Rowland at work. Inset: the Observatory at Stonyhurst College in Father Rowland's day (BGS archive image)

In the 1990s a great effort was made by BGS to make sure no further destruction occurred. All surviving seismogram collections were traced, and as a result, the seismograms from Aberdeen, Paisley, Eskdalemuir, Durham, Bidston, Kew and Jersey have been collected together and are stored under proper archival conditions, with microfilm backup. The original Milne-Shaw seismograms from Edinburgh were regrettably destroyed by Patrick Willmore in the 1970s, in the mistaken belief that having microfilm copies was sufficient. The microfilm has been recopied for security. The early Milne seismograms from Edinburgh fortunately still survive.

11.2 Amateur observatories

Observatories run by amateurs as a hobby are predictably obscure and even less likely to have any surviving material. In many cases the very existence of such instruments is only known from passing references in newspaper articles, or in some cases, from station co-ordinates having been passed to the British Association, and thence published without any other details. Those known are listed in Table 4, which follows the same format as Table 3.

Table 4 Amateur seismological observatories 1900–1969

	Lat	Lon	Start	End
Binstead	50.73	−1.18	*1900*	1947
Birmingham	52.47	−1.88	*1900*	*1920*
Bristol	51.48	−2.64	1931	*1939*
Dorking	51.23	−0.34	1948	*1960*
Guildford	51.25	−0.59	1910	*1915*
Haslemere	51.08	−0.72	*1909*	*1916*
Henley	51.61	−0.89	*1933*	*1946*
Kenilworth	52.35	−1.58	*1936*	Unknown
Leamington Spa	52.28	−1.53	*1936*	Unknown
Newport IoW	50.68	−1.28	*1915*	Unknown
Newport Mon.	51.59	−3.00	*1906*	Unknown
Parkhill House	57.22	−2.17	1914	1932
Plymouth	50.37	−4.15	*1923*	Unknown
Selfridges	51.51	−0.15	1932	1947
West Bromwich	52.52	−1.98	1908	1948

The most important was undoubtedly that at West Bromwich, run by JJ Shaw, who developed (with Milne, towards the end of his life) the Milne horizontal pendulum into the much superior Milne-Shaw instrument. Shaw was actually a pawnbroker by trade, so seismology was a hobby, though as with Davison, a very serious one. The observatory at West Bromwich was carefully run, first by Shaw and subsequently by his son. Like Rowland, Shaw was frequently sought after by the press on the occasion of major earthquakes.

The remnants of Shaw's material are held in the Lapworth Museum, School of Earth Sciences, University of Birmingham and detailed in Musson (1995). These include notebooks and bulletins, but only a few seismograms. The problem is that a collection of Milne-Shaw seismograms is bulky and heavy and difficult to store. The surviving records suggest that Shaw, rather than keeping all his records intact, was in the habit of cutting out the interesting bits for selected earthquakes.

This is also consistent with the one surviving West Bromwich seismogram outside the Lapworth Museum, for an earthquake on 24 June 1935. In this case, Shaw cut out the critical bit of the seismogram with the record of the earthquake, and used it as a bookmark in his copy of Davison (1924). The book subsequently came into my possession via a bookseller in Leamington Spa, and the seismogram with it.

Some of these amateur observatories were set up by people who came into contact with Milne and were inspired by him. One such was Evelyn Pollard, a chemist, who ran a pharmacy at Ryde, in the Isle of Wight, where he came into contact with Milne, who inspired in him an interest in seismology. Pollard dabbled with some experimental instrument while still at Ryde, but his activities became more serious after 1938. He moved from Ryde to Binstead, a short way out of Ryde on the road to Newport, in 1923. Here he constructed a concrete bunker in his garden, initially as an air raid shelter, but which proved to be eminently suitable as a seismometer vault. The vault, about 2.4 m square, still exists today (Fig. 45; Lovell and Henni 1999).

Fig. 45 Pollard's seismometer bunker at Binstead (BGS archive image)

His instrument seems to have been of his own design, though based on the Milne model. His first successful recording was of an earthquake in Mexico on 19 May 1940.

Pollard retired in 1947 and moved to Dorking, where he set up an instrument in what was described in a newspaper article as "a small wool-lined toolshed". This proved to be too close to a main road, so he moved to a quieter location nearby in 1953. He continued running his seismometer at least until 1960, and died in 1961. His instrument was subsequently moved to Whetstone, North London, by Pollard's son-in-law, but its ultimate fate is unknown (Lovell and Henni 1999).

In the absence of any statutory body in charge of seismology in Britain, Pollard became an amateur expert for the media, and his name turns up in British newspapers giving opinions about various earthquakes in the 1950s. He kept up his connections with the Isle of Wight to the end, and his surviving seismological papers are in Carisbrooke Castle Museum, and he is buried at Ryde.

Running a well-equipped seismological observatory could be expensive, especially if the person running it went to the trouble of building a proper vault as Pollard did. Another example was Frederick Vanderplank in Bristol. Most of what is known about the seismological activities of Vanderplank comes from correspondence with ATJ Dollar, now in the National Seismological Archive. It is summarised in Lovell and Henni (1999). For an amateur (by profession he was a naturalist, working at Bristol Zoo) he had a very well-equipped observatory, running several instruments (Milne-Shaw, Galitizin, Wiechert and an unspecified vertical instrument) in an underground bunker 20 m from his house, set up on a reinforced concrete pier resting directly on limestone.

His work came to the attention of Dollar in the 1930s, when Dollar was appealing for volunteers for his network of observers, and it is from letters to Dollar that much of the detail about his observatory is known. He commenced recording in about 1931, and stopped in 1939, when his research on the tse-tse fly took him to Tanganyika (now Tanzania). He offered his instruments to Bristol University, who declined, due to a lack of staff or resources to maintain them. No seismograms or bulletins survive.

Vanderplank had a moment of fame in 1934, when a small earthquake occurred near Bristol on 17 March (2.8 ML). His recording of the event was featured in the local press, and he stated that his instruments were even damaged. He collected felt information on the event, and this small macroseismic study is the one surviving scientific output (in MS) from Vanderplank's work.

Expense is probably one reason why most of the observatories are in the south of England, in the wealthier parts of the country. The prominent exception is James Crombie, who was a partner in the Crombie family woollen business, which was founded in Aberdeen in 1805, and continues to this day, though no longer under family control. He graduated from Aberdeen University, and was a philanthropist who donated to many worthy causes, including seismology. It was he who financed the establishment of the station at Oxford, and also left a considerable bequest to the British Association for supporting seismological research.

He lived at Parkhill House, Dyce, and here he set up his own seismological observatory in 1914, with two Mainka instruments aligned N–S and E–W. A Milne-Shaw (also horizontal) was added in 1918, and he also possessed an Agamennone instrument. These continued to run until Crombie's death in 1932, whereupon the instruments were dispersed: two to the Science Museum (the Agamennone and one of the Mainkas), and one to Aberdeen University (the Milne-Shaw). The remaining Mainka was given to the British Association, and its ultimate fate is obscure.

Given the care taken in finding homes for the instruments, one would like to think that a similar effort was made to preserve the seismograms, assuming that Crombie did actually archive these. However, there is no trace of them, bar a single surviving example from 1924 (Neilson 1981; Neilson and Burton 1988; Lovell and Henni 1999).

The strangest case is that of the instrument installed as a publicity stunt at Selfridge's store on Oxford Street in central London. Harry Gordon Selfridge (1856–1947), the founder and owner of the store, was genuinely interested in education and science, and believed that by attracting people into his store with educational and scientific exhibits he would induce then to spend time and money there, and perhaps return. In line with this philosophy he had on display at different times various exhibits of scientific interest, including planes, cars and recent inventions Honeycombe (1984).

As part of this scheme, in 1932 Selfridge had installed an instrument similar to a Milne-Shaw, but which appears to have been a 'one-off' specially designed by Shaw for use in buildings (Fig. 46). Next to the instrument in its glass case was a notice board detailing recent earthquakes. This board was kept up-to-date, and generated considerable public interest. The seismograph boom was attached to one of the building's main stanchions, and was unaffected by traffic or people. The instrument recorded on smoked paper.

At the outbreak of war the seismograph was moved from its original site near the Post Office to another part of the store, near the Information Bureau, but owing to subsequent building alterations neither site can now be traced. It did not record seriously there though, and was presented to the Science Museum, probably in 1947 from its Museum Inventory number (1947-121). After a period in the Science Museum, it was sent to the National Museum of Scotland in Edinburgh, where, although not calibrated, it was displayed working from 1961 until the late 1980s in an area now converted to kitchens. It was eventually decommissioned because the use of the carcinogen benzene in the smoking of the recording papers was forbidden by Health and Safety legislation, and it is currently in store (Lovell and Henni 1999).

In recent years, the number of non-professional observatories has swollen with the School Seismology project, supported by BGS since 1998. As of January 2013, 185 schools

Fig. 46 Seismology in Selfridge's, Oxford Street, London (BGS archive image)

in the British Isles are listed as participating (http://www.bgs.ac.uk/schoolSeismology/schoolSeismology.cfc?method=viewStations), using a simple and cheap standard seismometer design (Denton 2008).

12 The ISS and ISC

The death of John Milne in 1913 precipitated something of a crisis for British seismology, and HH Turner's report of the situation to the British Association committee deserves to be quoted in full:

> The death of John Milne, in July 1913, creates a situation of some difficulty and anxiety. He organised a world-wide seismological service with very little financial help from others. In many of the outlying stations the instrumental equipment was provided either by himself or by one of his friends, and the care of it has been generously undertaken by a volunteer who is often busily engaged in other work. The collation of results was

> in the early years undertaken by Milne himself, with the able help of Shinobu Hirota. Of late years a subsidy of 200L a year from the Government Grant Fund allowed of paid assistants; and Shinobu Hirota thus obtained a well-deserved official position; but for many years the only salary he received was paid from Milne's own pocket. It is by no means certain that the volunteer services at the stations, and the subsidy from the Government Grant Fund which makes it possible to keep running the central station at Shide, can be long continued; and it seems in any case very improbable that they can be rendered permanent. But a much more serious difficulty is the want of a salary for a Director or Superintendent of the whole British network of stations, who can give undivided attention to the valuable results which they have accumulated and to which they are daily adding. The salary of a competent Director, with the requisite mathematical knowledge, cannot be put lower than £500 or £600 a year, and there is at present no prospect of obtaining even this endowment. The superintendence has, of course, been hitherto provided voluntarily by Milne himself; and a certain amount of volunteer attention is available for the present. But seismology is developing so rapidly that the wholehearted attention of at least one English mathematician should be devoted to it; and if an endowment for a British Director could be obtained this would surely be the most direct method of doing justice to a new and fascinating science which was nurtured by an Englishman. The negative result of previous appeals to the Government does not encourage the hope of their taking any action, and the chief hope thus lies in the direction of private benefaction. Is it too much to hope that some generous benefactor will provide a firm footing for seismology?
> The present state of affairs is as follows: — The Shide Observatory is rented from Mrs. Milne at £20 a year. The work of the Shide station and the collation of results from other stations is being done by Mr. J. H. Burgess, who assisted Professor Milne in' the later years of his life, especially after the return of Shinobu Hirota to Japan. At the time of Professor Milne's death the work of collation was in arrear; and in order to bring it up to date assistance is being temporarily rendered by Mr. S. W. Pring (who had already considerable knowledge of the work) and his daughter. The general superintendence is undertaken by the Chairman of this Committee, partly by correspondence and partly by personal visits to Shide (on September 20-21, January 17-20, March 29-April 2, and May 9-11). (British Association 1915)

Initially, the Shide circulars were continued by Burgess, but with an important change: instead of the different bulletins from reporting stations being listed separately, Burgess printed the data collated by earthquake, a rather more useful method of arrangement. In fact, as Turner noted (British Association 1915), this entailed little extra work as the data were already being collated at Shide in order to check against errors. The name was also changed to the "Monthly Bulletin".

The outbreak of war in August 1914 meant that seismology was low down anyone's list of priorities for the next four years. Furthermore, the convention that established the International Association of Seismology expired in 1916, so at the end of the war the Association essentially no longer existed. A General Assembly was held in April 1922 in Strasbourg to declare formally that the Association had ceased to exist as from 31 March 1916, and to arrange for refunding the balance of payments and disposing the Association's assets.

The delegates attending the Strasbourg meeting travelled directly on to Rome for the first General Assembly of the International Union of Geodesy and Geophysics (IUGG), to establish a Seismology Section, which was renamed the International Association of Seismology in 1930, and eventually went on to become the International Association of Seismology and

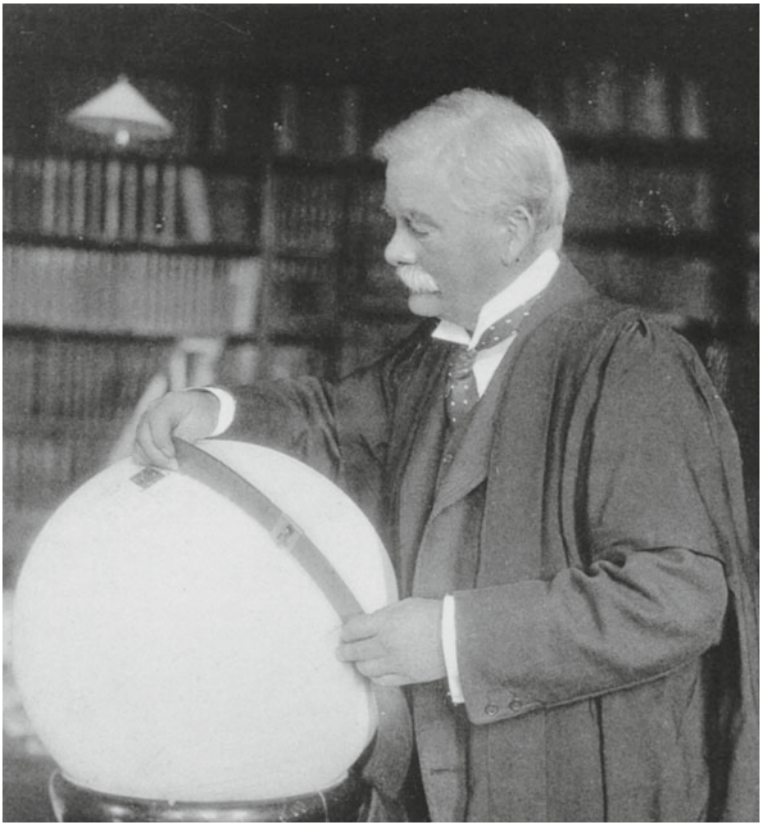


Fig. 47 HH Turner (BGS archive image)

Physics of the Earth's Interior (IASPEI), taking that name in 1951. The British delegate was HH Turner from Oxford (Fig. 47).

Turner had already taken over chairmanship of the British Association committee from Judd in 1908, and on Milne's death, JJ Shaw took over the post of Secretary. Turner was looking for two things from the Rome meeting. Firstly, to carry on production of Monthly Bulletins, and secondly, to establish Oxford University as the international centre for seismology. He was successful in the first but not the second.

It was clear that so much had already been invested in buildings and equipment in Strasbourg that it made no sense to start again in a new location. Also France, which had now recovered the city, was desirous of maintaining the city's role.

As for the Monthly Bulletins, three things were agreed. Firstly, the work in preparing and publishing them would remain in Oxford under the control of Turner. However, oversight of the operation would no longer be vested in the BA Committee; instead, the IUGG Seismology Section would have ultimate authority. Lastly, IUGG would support the work financially with a grant of 10,000 F per year. Turner also became President of the Section.

The new publication was christened the International Seismological Summary (ISS), and the first volume was published in 1923, carrying data for the year 1918, continuing from where Burgess had left off (Turner 1923). A relict from the Shide days was that the Bulletins continued to be printed in Newport by the Isle of Wight County Press. In addition to the grant from IUGG, the work continued to be supported by the British Association, and further financial support was provided by Oxford University itself, the Royal Society, the Department of Scientific and Industrial Research and the generosity of amateur seismologist James Crombie. (The annual grant of 10,000 F proved completely inadequate, partly due to the relative value of the franc and pound.) The preparation of the ISS bulletin was largely the work of Ethel Bellamy (1881–1960), who had trained in seismology at Shide, and Joseph Hughes (1898–1966), who joined in 1923.

Sadly, Turner died suddenly of a brain haemorrhage at the IUGG General Assembly at Stockholm in 1930. He was succeeded by Harry Plaskett (1898–1980), who took over administrative and financial supervision, while Bellamy and Hughes did the actual preparation of the bulletins. During World War II, both Plaskett and Hughes were absent on war service, leaving Bellamy to continue single-handed, which unsurprisingly led to a considerable backlog, amounting to the ISS being eleven years in arrears (Stoneley 1970).

Catching up with this was made more difficult by the increase in the number of stations worldwide after the end of the war. There were also financial problems. Oxford University withdrew support in 1946, and the ISS now moved to Kew Observatory, with Harold Jeffreys (1891–1989) as Honorary Director and Hughes as Assistant Director. Additional financial support was forthcoming in 1947 from UNESCO, and later from other international bodies (Stoneley 1970).

In 1957, Jeffreys was succeeded by Robert Stoneley (1894-1976), who remained in charge until 1963, when he took a position as Professor of Geophysics at Princeton (Jeffreys 1976). Stoneley was also Chairman of the British Association Seismological Committee, which was still meeting annually, from 1945 to the year before his death, 1975.

Stoneley was succeeded by Patrick Willmore (1921–1994). Willmore, who had returned to Britain from a post at the Dominion Observatory, Ottawa, had been acting director of ISS since 1961, covering for Stoneley while the latter was visiting the USA. In 1962, Stoneley reported to British Association (1964), "During the past year Dr. P. L. Willmore has carried on the direction of the I.S.S. during Dr. Stoneley's visit to the United States", and Willmore is referred to as Deputy Director in Stoneley's absence in IUGG (1964). This became permanent when Stoneley took a position at Princeton.

Willmore's main interest, ever since he was a research student at Cambridge, was seismometer development, particularly the use of radio-linked high-frequency portable instruments. Around 1960, Willmore took a position at Cambridge, but soon found that the combination of local geology and heavy traffic made Cambridge an unsuitable location for seismometer development. "It was therefore decided to transfer this work to Edinburgh, so as to make use of the favourable combination of hard rock and low population density which is available in Scotland" (British Association 1964). He now took up a position at the Royal Observatory, Edinburgh leading a small Seismological Research Group as a joint activity between the Observatory and Edinburgh University.

The transition from the International Seismological Summary to the International Seismological Centre is described by Adams (2010) and Argent (2012), but the exact sequence of events is quite hard to reconstruct. In 1961, the staff of ISS were still based in Kew, but the ISS was experiencing financial difficulties, and was falling further and further in arrears, as the number of worldwide stations increased (IUGG 1958).

This was a time of greater world-wide interest in seismology, with the establishment on the World Wide Standard Seismograph Network (WWSSN). An international meeting was convened by UNESCO to consider the problems facing international seismology, and as a result of this, a resolution was passed to set up a new body to collect data, and produce definitive bulletins of relocated earthquakes, and their associated station readings, taking over this responsibility from the ISS. Funding was to be provided chiefly by the USA and UK. This was to become the International Seismological Centre (ISC). While it would be based in Britain, it would be an internationally-controlled agency. The early funding arrangements are given in detail by Adams (2010) and Argent (2012), and the discussions involved are documented in IUGG (1964; 1968).

A small committee was set up under the chairmanship of Perry Byerly to determine where this new "World Centre" (as it was referred to in the initial discussions) would be located (International Union of Geodesy and Geophysics 1964). Edinburgh was the favoured location. There was already an active seismological research group there (Willmore's), and the computing facilities were good. The chief rival was (as one might expect) Strasbourg, but the conclusion of the governing ISS Committee was that only the choice of Edinburgh would allow the envisaged expansion of ISS to cope with the increased flow of data.

Willmore was thus appointed Director of the new ISC in late 1963. He and his group moved from the Observatory on Blackford Hill to premises nearby made available by the University, at South Oswald Road, but for the time being the ISS staff, who were now the ISC staff, stayed in Kew, eventually moving up to Edinburgh in 1967. The first year of seismicity data to be published as the work of ISC was 1964; the data for 1962 and 1963, the last years of ISS were published from ISC in Edinburgh after the move. Willmore wanted to call it the International Seismological Research Centre, and referred to it as such in some early publications (e.g. British Association 1965). But its role was monitoring, not research, and so Willmore was obliged to stop using the ISRC name. It was indicative, though, of where Willmore's interests lay.

The administrative basis of ISC was revamped after the IASPEI General Assembly of 1969 in Madrid. There was now a Governing Council, made up of representatives from national seismological agencies that subscribed at least a minimum contribution. The first meeting was in London in 1970, the full members being representatives from Canada, New Zealand, Norway, Sweden, UK, USA, and USSR, plus a UNESCO representative, and observers from other countries who would later participate. At this meeting it was decided that a full-time Director was needed for ISC. Willmore was also managing his research group, which was by this time the Global Seismology Unit of the Institute of Geological Sciences (IGS), following the reorganisation of British environmental sciences in 1965 (see next chapter), so he could only devote part of his time to running the ISC; also he was perhaps blurring the boundary between his two responsibilities, to the disadvantage of ISC.

Edouard Arnold (1930–2006) was his replacement. Arnold had worked briefly at the ISC in 1965, before returning home to the USA to work for the National Oceanic and Atmospheric Administration (NOAA). He put the location software that ISC was using on a firm footing; the software developed under Willmore was somewhat erratic, and capable of returning negative depths and latitudes greater than 90° (Adams 2010). The new software developed by Arnold (who had studied with Sir Harold Jeffreys), by contrast, was to be used for the next 30 years (Hughes 2007). Arnold remained as Director until 1977, when he returned once more to the USA.

It was during Arnold's tenure that the ISC moved away from Edinburgh. In the early 1970s it was decided to close several IGS offices, including South Oswald Road, and concentrate the bulk of Scottish staff in a new purpose-built building on a corner of the King's Building site, the science campus of Edinburgh University (established by Ewing, chapter 8.2 above). This was Murchison House, named after the Scottish geologist Sir Roderick Impey Murchison (1792–1871). The building was formally opened in 1977, but staff had already been occupying it since 1975, with the seismologists among the first to arrive. The Global Seismology Unit was allocated the front part of the top floor (which it still occupies), and the large room at

Fig. 48 Aerial view of Blackford Hill, Edinburgh. South Oswald Road is just off the image to the *left* (BGS archive image)

the centre of this space was designed to be the home of ISC. This was not to be; for some time the ISC's computing had been done using an IBM computer at the Rutherford-Appleton Laboratory at Chilton in Oxfordshire. This meant continually shipping punch cards from Edinburgh to Oxfordshire, which was inefficient. The logical thing was to find a new home for ISC closer to Chilton. In 1974 ISC moved to an office in Newbury, and subsequently premises were purchased in 1986 on a small industrial estate at Thatcham, which is still ISC's home (Argent 2012).

Arnold was succeeded in 1977 by Anthony Hughes, who was Director for the next 20 years. He was the last British seismologist up to the time of writing to occupy the post, the subsequent Directors being Ray Willemann from the USA for the period 1998–2003, followed by Avi Shapira from Israel for 2004–2007, and since then to the present, Dmitry Storchak from Russia.

The ISC may be based in Britain as a legacy of John Milne's observatory in Shide, but it is a truly (and fittingly) international agency.

13 Seismic monitoring in Britain since 1970

As mentioned in the previous chapter, in 1962, Patrick Willmore moved from Cambridge to Edinburgh, managing a small Seismological Research Group, which was a joint venture of the Royal Observatory Edinburgh, and Edinburgh University (Fig. 48). In the unpublished draft report of the British Association Committee for 1965, Willmore referred to two groups, one belonging to the Observatory and one to the University, but the evidence indicates it was one group administered by the University's Department of Astronomy. Willmore's interest was in developing a compact, high-frequency instrument suitable for field deployment, using radio-linking and tape storage. The most successful of Willmore's designs was the Mk III instrument. No longer was it necessary to connect a seismometer to a photographic recording system that required the paper to be changed each day. The Willmore Mk III could be installed on a remote hilltop, and radio-linked to a base station where the signal could be stored as an audio file on magnetic tape. For this, slow running, FM magnetic tape systems were used, which recorded up to twelve seismometers continuously. The most advanced of these (the Geostore) recorded for one week, at 16 Hz bandwidth, between tape changes.

In 1965, a major reorganisation of British science was instigated following the Science and Technology Act (Bowie 2010), and a number of environmental science agencies were grouped together under the oversight of the newly-created Natural Environmental Research Council (NERC). These included the Geological Survey of Great Britain, which was merged with the Overseas Geological Surveys and renamed the Institute of Geological Sciences (IGS). The Geological Survey had at that time a number of regional offices, including one in Edinburgh for what had originally been the Geological Survey of Scotland. NERC, in its Royal Charter, was specifically given responsibility for seismology in the UK.

13.1 Instrumental monitoring of British earthquakes after 1970

Willmore's group now was moved from the Observatory/University to IGS, to become the Global Seismology Unit (GSU). In the late 1960s, an eight-station short period network of Willmore Mk III seismometers was installed in Central Scotland, with stations linked to Edinburgh by radio. This was LOWNET (Crampin et al. 1970); it was initially conceived and developed by Stuart Crampin, for the study of local seismicity (Crampin 1967 *unpublished*). It also served as test-bed for the development of networks that could be deployed in more earthquake prone regions of the World (particularly Turkey, Iran and Papua New Guinea). In fact, an early experimental array had been run at Early Burn, south of Edinburgh, as early as 1966, in preparation for installing a seismometer array in Brazil. This was part of a co-operative programme launched by Edinburgh University and the Istituto Geofisico del Peru (Berrocal 1968).

The availability of reliable and timely data, certainly raised the profile of British earthquakes with local authorities, the media and the public (Walker et al. 2003). The network was considered officially to be running as of the start of 1970, and the first LOWNET bulletin, containing the first ten years of data, was published by Burton and Neilson (1980). However, it only recorded local events, plus larger earthquakes elsewhere in the UK, but for those, its epicentre locations were inaccurate. For example, the Newport, South Wales and the Bala, North Wales earthquakes of early 1974 (3.5 and 4.1 ML) were better located by macroseismic methods. The Lewes earthquake of 16 January 1975 (3.2 ML) was not recorded at all, and could only be assessed from macroseismic data.

The first opportunity to expand the monitoring network came in response to mining-induced earthquakes in the Potteries, in 1975–1976. Stations were installed in the Midlands and South Wales with the support of the Department of the Environment (DoE). The occurrence of a series of earthquakes in the Kintail area led to new stations being installed in northwest Scotland. A separate network was started in the Republic of Ireland in 1978, under the direction of Brian Jacob (1938–2001), who had been a member of Willmore's group, but moved to Dublin in 1976 to become Professor of Geophysics at the Dublin Institute of Advanced Studies (Khan 2002).

At the end of the 1970s and in the early 1980s, the Department of Energy sponsored studies of North Sea seismicity, which led to further stations being added in eastern Britain. For a while, sea-bottom stations were even operated near the Beryl and Statfjord oil platforms; predictably, these were very noisy, and there was a continual problem of the buoy being struck by fishing boats.

Around the same time, a further network expansion was funded in Cornwall, in connection with the Hot Dry Rock geothermal project, and, with Jersey New Waterworks Company and Jersey Meteorological Office, a small monitoring network was installed on Jersey to cover the Channel Islands.

By this time Willmore had retired, and in 1984 IGS had been rechristened the British Geological Survey (BGS). The GSU was now led by Chris Browitt. On 19 July 1984, a 5.4 ML earthquake occurred with an epicentre in the Lleyn Peninsula, with an exceptionally intense aftershock sequence (Turbitt et al. 1985). This occurred at a time of austerity in UK public sector organisations. Despite the official policy not to spend money, Browitt recognised that this was a once-in-a-generation event, and threw all possible resources into handling the aftermath of the event and capturing the aftershock sequence with a temporary network, to the great benefit of British seismology. As an aside, it can be mentioned here that immediately after the earthquake, the switchboard apparatus at the BGS Edinburgh office literally blew up. The number of simultaneous incoming calls exceeded the maximum imagined by the designers by over an order of magnitude, and sparks and smoke came out the back of the console.

After the 1984 aftershock sequence eventually died down, it was obviously necessary to keep some stations operating in northwest Wales, and it was becoming increasingly obvious that expanding the UK network needed a coherent national policy to achieve a reasonably even distribution of stations across the country.

In 1988, a means to this end was established. A "Customer Group" of interested organisations, led by the DoE, and supported by NERC, was established to co-fund a seismic monitoring service in the long term. The UK nuclear industry joined as sponsors, initially through the CEGB (Central Electricity Generating Board) and, later, through several companies when the CEGB was privatised. The Customer Group, which continues to this day, has included representatives from national and local government, hydrocarbon, coal and water companies, and the nuclear industry—including the nuclear regulator, the Nuclear Installations Inspectorate (NII), now Office for Nuclear Regulation (ONR), of the Health and Safety Executive (HSE).

At the same time, international co-operation with seismic monitoring agencies in neighbouring countries also became much improved as a result of the EC-funded, BGS-led, Transfrontier Project. This greatly improved monitoring of the Irish Sea, North Sea and English Channel. At the end of the 1990s stations were added in the Faroes, with support from the Offshore Division of HSE and the Faroese Geological Survey, which also led to the first ever study of the historical seismicity of the Faroes (Musson et al. 2001a).

At its peak, the UK monitoring network consisted of 146 stations (Fig. 49). Most of them consisted of single, vertical component, short-period Willmore MK-III seismometers, with a number of them configured to monitor horizontal motions, together with three-component, strong motion accelerometers. The primary aim of the network was stated to be to develop a national database of seismic activity in the UK for seismic hazard assessment and to provide a response to felt earthquakes (Walker et al. 2003). However, to maximise recording sensitivity,

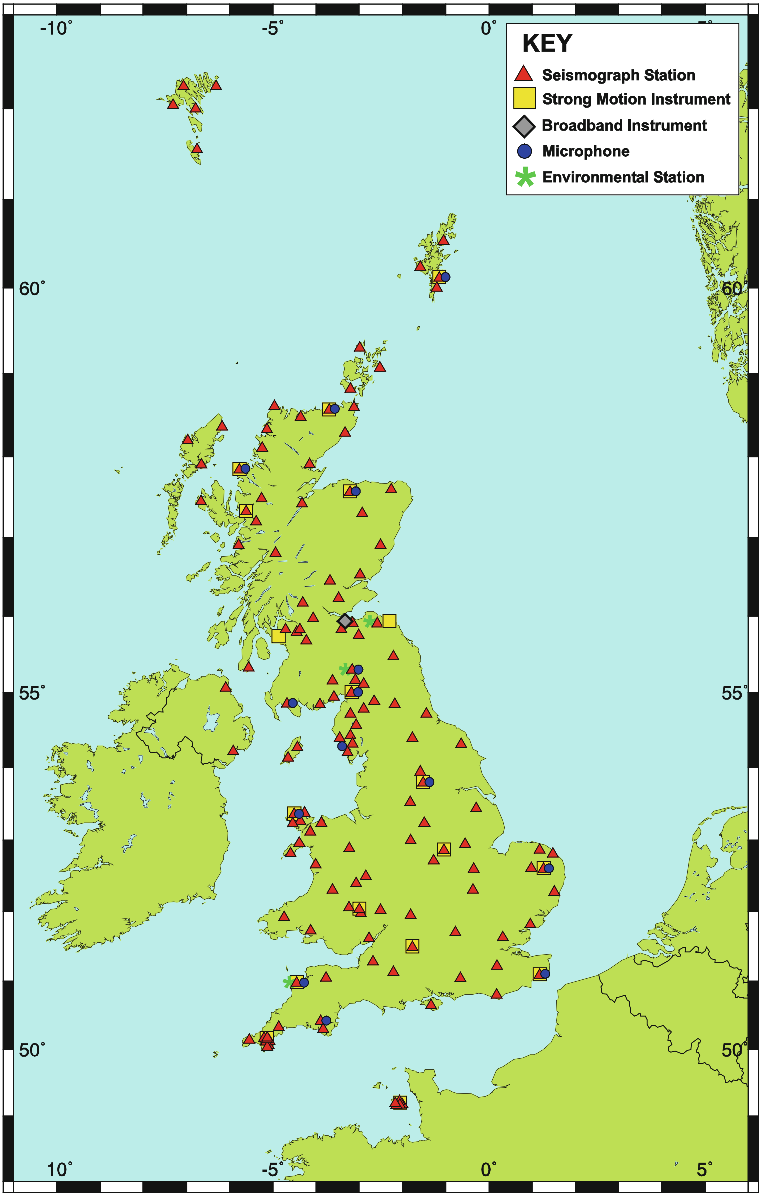


Fig. 49 The UK seismic monitoring network in 2003 (Walker et al. 2003)

the instruments were run at high gain. This was good for recording the maximum number of small events, but it also meant that, with the limited bandwidth and dynamic range of the seismic data acquisition systems, in the case of any earthquake much above 3 ML, most of the network would saturate.

Accordingly, a programme was put in place at the start of the twenty-first century to upgrade the monitoring network bit by bit with broadband technology, and 24-bit digital data acquisition. This allows small events to be detected and larger earthquakes to remain

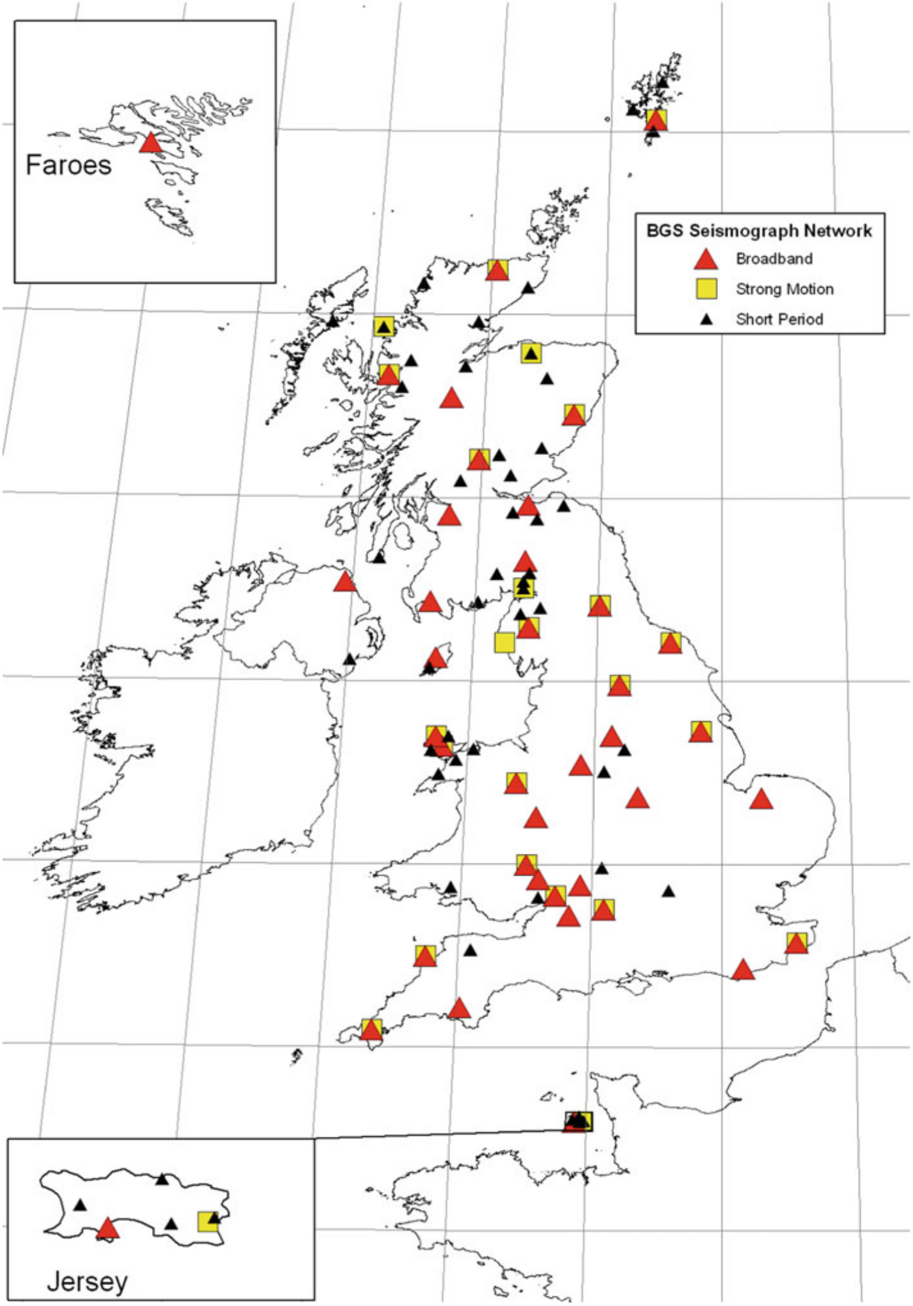


Fig. 50 The UK seismic monitoring network in 2012 (Baptie 2012)

on-scale at every station. Gradually the Willmore Mk III stations have been phased out and replaced with a lesser number of stations running broadband seismometers. Currently about 35 broadband stations are in operation (Fig. 50, Baptie 2012).

13.2 The fate of the British Association Committee

The British Association's Committee on Seismological Investigations continued to function throughout most of the twentieth century, unlike the situation in the nineteenth century, when it might cease to operate if the chairman resigned (David Milne) or died (James Bryce). The annual reports continued to report on the state of seismology in Britain. Most were published in the British Association's journal "The Advancement of Science"; some of the later ones in the Quarterly Journal of the Royal Astronomical Society, and others survive only in draft.

These reports all take much the same format: accounts for the Gray-Milne Fund, reports from individual institutions, notes on recent British earthquakes, and "any other business". The report for 1959, the 64th, will stand as an example of the situation before the reorganisations of the 1960s (British Association 1959).

The number of members of the committee listed is 21, and some names are unfamiliar to seismology today (such as KF Chackett, of Birmingham University, later of Rutherford High Energy Laboratory, whose main field of expertise was radionuclides). Stoneley was Chairman, Tillotson Secretary, and the more familiar names included Ethel Bellamy, Edward Bullard, ATJ Dollar, Joseph Hughes, Harold Jeffreys, and HV Shaw, who was JJ Shaw's son.

After the statement of accounts for the Gray-Milne Fund, there is a list of instruments still belonging to the Committee, including one in South Africa, one in Australia and one in Fiji—and the Jaggar shock recorder at Fort Augustus is also listed. The submitted reports are from the ISS, Kew Observatory, the National Institute of Oceanography (working on microseisms), and Cambridge University (running Benioff short-period instruments). Three earthquakes are reported: an event felt in Jersey on 2 January 1959, and two Comrie quakes on 5 October 1958 and 17 July 1959. The report concludes with mention of two papers by G Lees on the 1958 East Midlands earthquake, a note that Sir Harold Jeffreys is on a world tour, and notice of the next meeting of the European Seismological Commission.

Taken year on year, these reports are invaluable in terms of reconstructing exactly what different observatories were operating. In this respect they were exploited extensively by Lovell and Henni (1999).

Paul Burton, then at IGS, took over the Committee from Tillotson. The report for 1981 (Burton 1982) is much more detailed than those of the 1950s and 1960s, and includes substantial details of various research projects underway. This was the last to be published; the report for 1984 is the last that exists in draft form. Thereafter the Committee ceased to function from lack of interest. After 88 years of uninterrupted existence, it seemed that it was no longer relevant and it faded away.

The archives of the Committee were curated for a while by John Hudson at Cambridge, and then transferred to the National Seismological Archive of BGS in Edinburgh. They mostly consist of printed copies of the published annual reports, and copies of the drafts of those that were not published. The only manuscript material concerns the running of the Gray-Milne Fund.

13.3 Macroseismic monitoring of British earthquakes after 1970

After the retirement of ATJ Dollar, there was little interest in macroseismic studies in Britain in the 1960s. In the early 1970s, more attention began to paid to this subject, partly in IGS, and also Durham University.

The first macroseismic study of a British earthquake since the abortive Dollar investigation of the 1966 Helston earthquake, was that by Geoff Browning of Durham University, of the 9 August 1970 Kirkby Stephen earthquake. The paper published by Browning and Jacob

(1970) on the earthquake contains no macroseismic data; all that survives of Browning's survey is a draft isoseismal map. Willmore himself had little interest in macroseismology, which he believed was irrelevant when earthquake parameters could be accurately derived from instrumental data. Reputedly, he was dismissive of Dollar's work.

A questionnaire study was made of the 7 March 1972 Todmorden earthquake as part of a joint activity of IGS and Durham to study the earthquake, which like the 1970 quake, was widely felt in Northern England. The macroseismic side of the investigation was handled by G Riddle of Durham, using questionnaires, press reports and field interviews, with intensities assessed using the Modified Mercalli Scale (presumably the 1956 version), marking a shift away from the Davison Scale. The data collected do not appear to have survived, and the final publication (Lilwall and Riddle 1973) has only a brief textual synopsis of the macroseismic field, and no map.

The following year, some felt information was collected for the Fair Isle earthquake of 9 February 1973 (3.4 ML) by Graham Neilson of IGS. Not surprisingly, little could be collected for an earthquake occurring between the Orkneys and Shetlands, and no publication resulted.

The resumption of systematic macroseismology in Britain was the initiative of Browitt, starting with the 23 January 1974 Bala earthquake, the study of which included not only a survey via questionnaires in newspapers in the Davison manner, but also a field survey conducted by Chris Browitt, Roy Lilwall, Bob McGonigle and Joe Lawrence. This response, which might be thought excessive for a 3.5 ML event, was prompted in part by the coincidental meteor shower, which caused intense speculation as to whether what had happened was actually an earthquake, a meteorite impact, or a plane crash (Musson 2006a).

Thereafter, questionnaire surveys would be launched for all British earthquakes that were significantly felt—generally speaking, onshore events $\geq$3.5 ML.

Browitt fixed upon a methodology that would avoid the limitations of that used by Davison and Dollar. In Davison's day, and for that matter, Dollar's, instrumental data for British earthquakes was generally either non-existent or inadequate. Thus collecting anything that might be useful, like direction estimates, was a possible substitute.

In the 1970s, magnitude, but not location, could be determined accurately from LOWNET for the larger earthquakes, so macroseismic data was still useful in improving locations; but not from estimates of wave direction. It was by now well understood in seismology that what mattered was the intensity field, and particularly the location of highest intensities.

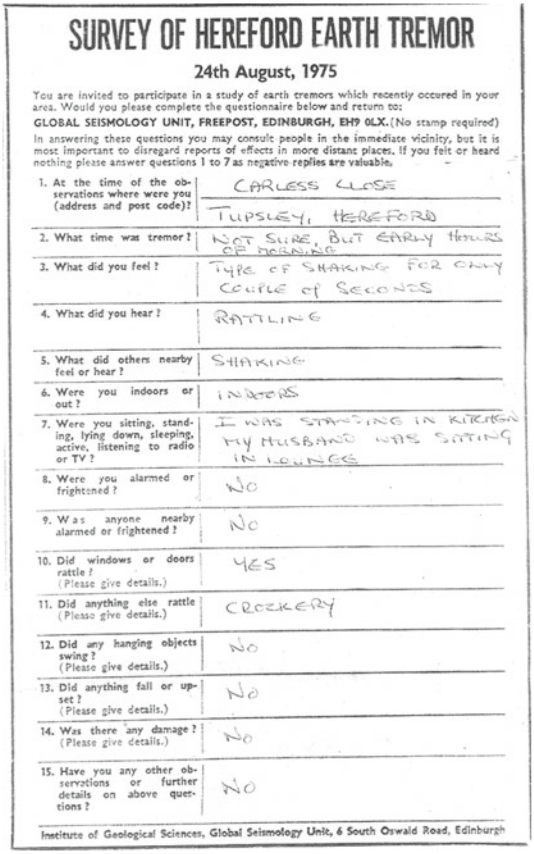
Browitt therefore devised a questionnaire consisting of very open-ended questions that invited the respondent to reply in their own words, but with a steer towards what was important: for instance, rattling windows, falling objects, and so on.

It was also clear from the late 1970s on, that collecting macroseismic data for modern earthquakes would play a crucial role in calibrating parameters for historical British earthquakes, which could only be assessed from macroseismics.

The questionnaire, an example of which is shown in Fig. 51, included space to write the answer, so that the form could be clipped out of the newspaper and posted to IGS/BGS free of charge. (Davison's respondents had to write a letter and send it).

However, the questionnaire form could hardly be put in a newspaper's correspondence column, so the whole thing had to be inserted as a paid advertisement. The cost of mounting a survey using national and local papers for earthquakes felt over a large area, such as the 19 July 1984 event, could be significant.

The advantage of the free-form questionnaire was that the maximum information could be retrieved, since the respondents were not constrained to ticking boxes or answering yes/no. The disadvantage was that the data were extremely time-consuming to process. After the 2 April 1990 Bishop's Castle earthquake about 30,000 replies were received, and the final



SURVEY OF HEREFORD EARTH TREMOR

24th August, 1975

You are invited to participate in a study of earth tremors which recently occured in your area. Would you please complete the questionnaire below and return to:

GLOBAL SEISMOLOGY UNIT, FREEPOST, EDINBURGH, EH9 0LX. (No stamp required)

In answering these questions you may consult people in the immediate vicinity, but it is most important to disregard reports of effects in more distant places. If you felt or heard nothing please answer questions 1 to 7 as negative replies are valuable.

1. At the time of the observations where were you (address and post code)?	CARLESS CLOSE TUPSLEY, HEREFORD
2. What time was tremor?	NOT SURE, BUT EARLY HOURS OF MORNING
3. What did you feel?	TYPE OF SHAKING FOR ONLY COUPLE OF SECONDS
4. What did you hear?	RATTLING
5. What did others nearby feel or hear?	SHAKING
6. Were you indoors or out?	INDOORS
7. Were you sitting, standing, lying down, sleeping, active, listening to radio or TV?	I WAS STANDING IN KITCHEN MY HUSBAND WAS SITTING IN LOUNGE
8. Were you alarmed or frightened?	NO
9. Was anyone nearby alarmed or frightened?	NO
10. Did windows or doors rattle? (Please give details.)	YES
11. Did anything else rattle (Please give details.)	CROCKERY
12. Did any hanging objects swing? (Please give details.)	NO
13. Did anything fall or upset? (Please give details.)	NO
14. Was there any damage? (Please give details.)	NO
15. Have you any other observations or further details on above questions?	NO

Institute of Geological Sciences, Global Seismology Unit, 6 South Oswald Road, Edinburgh

Fig. 51 Typical IGS newspaper questionnaire from the 1970s (BGS archive image)

intensity map was not completed until thirteen years later. In fact, throughout the 1970s none of the macroseismic surveys carried out by IGS/BGS were written up, though the questionnaires and other reports received were carefully archived. The first contemporary intensity map (using Medvedev-Sponheur-Karník, MSK intensities) was by Redmayne and Musson (1987) for the Ardentinny earthquake of 16 September 1985. Thereafter, the results of macroseismic surveys were published more or less promptly, with the exception of the delay in processing the data for the 1990 Bishop's Castle earthquake. In addition, all the questionnaires collected for previous earthquakes back to 1974 have since been processed and intensity data points assessed, with the exception of 19 July 1984, processing of which is only partially complete.

Having been involved in the development of the European Macroseismic Scale (EMS) in the 1990s, in the following decade I became increasingly interested in the idea of automating the process of intensity assignment. At the same time, it became clear that use of the internet was becoming so prevalent in the UK that it could be used as a means of collecting macroseismic data, as was already happening in the USA (Wald et al. 1999). Given that EMS is a scale that relies on the proportion in which an effect is observed amongst observers in a particular place, it appeared that the best system would be one that mimicked the human thought processes of a seismologist making a traditional intensity assessment. This required revising the BGS questionnaire that Browitt had composed in 1974, which had been in continual use

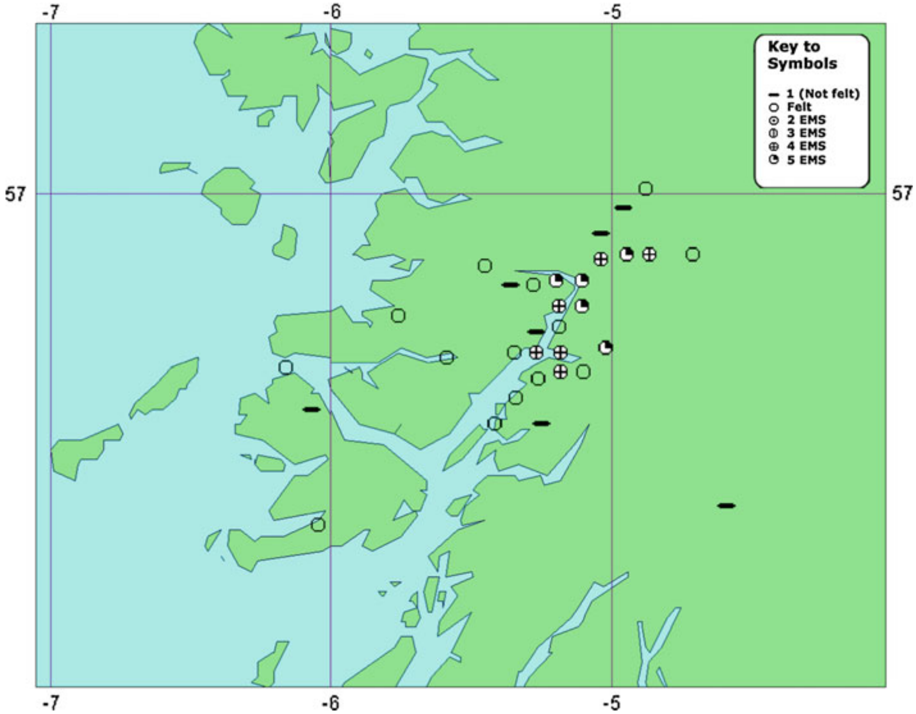


Fig. 52 Intensity map for the 10 December 2005 Fort William earthquake—first UK intensity map to be derived from internet data

since with only very minor modifications. A questionnaire that will operate on a web page and feed data into an automatic algorithm for intensity assessment needs perforce to rely on questions that have predefined alternative answers, either yes/no or using some graded scale of no/a little/a lot/all or some such.

This new system received its first test with the 10 December 2005 Fort William earthquake, which produced results clearly compatible with those for previous events (Fig. 52). The algorithm is described in detail in Musson (2006b), and is also now in use in Austria and by the European-Mediterranean Seismological Centre (EMSC). It allows the construction of a dynamic online intensity map, which is a good tool for communicating science after an earthquake. On the occasion of the 27 February 2008 Market Rasen earthquake, which was felt over most of England, over 31,000 questionnaires were received, all of which could be turned into a finished intensity map in a few minutes, rather than the decade or so it took before with the limited manpower resources available.

13.4 Historical earthquake research

Dollar may have desired to extend Davison's (1924) catalogue forwards in time, but he had no desire to attempt to improve the historical part of it. Consequently, right up to the 1970s, Davison (1924) was the best available account of past British seismicity, despite the fact that it was non-parametric, and was written before magnitude was even invented.

In the mid 1970s there was a reawakening of interest in historical earthquake studies both at IGS and at Imperial College London, where NN Ambraseys was active.

At the beginning of the 1980s a large expansion in historical earthquake studies occurred in the UK, in order to inform seismic hazard studies for the UK nuclear power programme. Three major studies were commissioned at the same time, from two consultancies, Principia Mechanica Ltd and Soil Mechanics Ltd, and from BGS. A fourth study was completed in the same time frame (Ambraseys and Melville 1983).

The following summary is adapted from Musson (2004).

13.4.1 Lilwall's catalogue

It was Roy Lilwall at IGS who attempted to create a properly parametric catalogue of UK earthquakes for the first time. This was largely done by taking Davison's catalogue, assigning latitudes and longitudes to the various seismic "centres", and making some preliminary assumptions about magnitudes, including setting a default "minimum probable" magnitude for use where no other information existed. This was extended forwards in time by adding data from Dollar (1950) and Tillotson (1974). The report by Lilwall (1976) presents some analysis based on the preliminary version of the catalogue that never went beyond being a working file; it was not published in full by Lilwall because it was not regarded as a finished work. The file was later published by Burton et al. (1984b) specifically so that it could be cited with reference to a BGS publication, after it had been included in a report by Soil Mechanics (1982). This file also seems to be the basis for the listing of UK seismicity in Van Gils (1988).

13.4.2 The Inverness report

A study of earthquakes in the vicinity of Inverness (Northern Scotland) by Browitt et al. (1976) seems to be the first published study in the UK in which historical earthquakes are critically revaluated from original source data. This study reproduces verbatim historical accounts from original newspaper descriptions, and assesses epicentral intensity values (MM and MSK), and felt areas, and gives approximate magnitudes, but how these are derived is not discussed. The study makes the point that earthquakes in the mountainous north of Scotland have tended to "migrate" to settled regions; in other words, the population distribution affects the distribution of felt reports and this has caused misperceptions as to where the epicentres lay.

This study provides the first example in Britain of detailed historical work being undertaken to show that an event was a fake. This is the case of the 14 November 1769 Inverness earthquake, which supposedly threw down houses and killed several persons. This appears in various catalogues, including Davison (1924) and can be traced back to a single contemporary record in a periodical published in London (the London Magazine) in which the information is given on the authority of a letter from York. Browitt et al. (1976) examined specifically the Scottish sources that should have reported such an occurrence and found no local evidence that this earthquake ever occurred. Although no explanation has ever been found (one suggestion is that it relates to some other Inverness, perhaps in the Americas), all subsequent investigations of this earthquake have agreed with Browitt et al.'s (1976) conclusion that the event is fake.

13.4.3 Principia Mechanica

The scope of the Principia (1982) study was ambitious—to produce a complete catalogue of historical British earthquakes from original sources, in one year. The final report consists of three volumes. The first contains a wide-ranging discussion of British seismicity including subjects such as magnitude calibration and attenuation of strong ground motion, concluding with a descriptive (rather than parametric) list of British earthquakes from the earliest times up to the 1970s (the list is deliberately incomplete for modern events). The remaining two volumes give detailed accounts of each earthquake. One volume deals with the nineteenth century (and selected earlier events), the other with the twentieth century. For each earthquake a list of the sources used is given, some brief notes of maximum effects, maximum intensity (using a home-made scale of eight degrees), radius of felt area, and a map of data points. Larger earthquakes are also given isoseismal maps. Tables of intensity data points (IDPs) are not given, and were not even made, the intensity values being entered directly onto the maps.

The amount of information processed in this study is impressive, although inevitably, in the attempt to do so much so quickly, mistakes were made in terms of under-rating some earthquakes, over-rating others, and failing to identify fake events. Many earthquakes receive cursory attention. Numerical parameters are estimated only for some larger earthquakes, and not in the catalogue, but in a table in the text of the first volume.

13.4.4 Soil Mechanics

The study by Soil Mechanics (1982) was conducted in parallel to the Principia (1982) one. Rather than producing a catalogue, this study took a representative sample of 72 earthquakes from all parts of the UK and all time periods, and subjected each to a more thorough historical study, using more sources than was possible within the scope of the Principia (1982) report. Intensities were assessed using the MSK scale, and magnitudes were estimated using seven different formulae taken from the literature. The situation with regard to IDPs is no different to the Principia (1982) study.

13.4.5 Burton, Musson and Neilson

The BGS study that was conducted at the same time as Principia (1982) and Soil Mechanics (1982) was spread over three years instead of one, and published as Burton et al. (1984b). In outline it was similar to the study of Soil Mechanics in taking a sample of events; however, the sample was larger (102 against 72; one turned out to be a fake event) and contained most of the larger earthquakes after 1700.

This study was different in two important respects. First, the original source data on which intensity assessments were based were transcribed and reproduced verbatim, arranged by place. This means that the reports issued by this project contain the actual evidence of what occurred during each earthquake alongside the interpretations that are given in the form of intensity values. Secondly, the IDPs (MSK values) for each earthquake are listed in full. This means that further analysis of the data collected by Burton et al. (1984b) is relatively easy, whereas for Principia (1982) and Soil Mechanics (1982) it is practically impossible.

Owing to the sheer volume of the verbatim source material, the initial product of this study numbered fifteen volumes (listed in Burton et al. 1984b). Further volumes, in more or less the same style, were issued in subsequent years, covering additional earthquakes (see Musson 2004).

13.4.6 Ambraseys and Melville

The study by Ambraseys and Melville (1983) was concerned principally with earthquakes in the eastern part of the UK, and only with earthquakes before 1800. IDPs are not listed. The important feature of this study is that it disentangled the complexities of dating of medieval earthquakes, and thus cleared up many of the fake earthquakes (from dating errors) from the earlier part of the UK catalogue once and for all.

13.4.7 Synthesis

At the end of the 1980s, the situation with regard to the understanding of historical British earthquakes was completely transformed with respect to what it had been 10 years before. The amount of data researched and published was now very large, and based almost entirely on primary historical source material. Earlier catalogues were used only as starting points for research.

What was now needed was synthesis of the data already uncovered in the form of complete numerate catalogues. One such was published by Ambraseys (1988), for the period after 1700 only.

The first attempt at a synthetic catalogue was that of Arup (1993), which draws on all the studies already listed, and also some unpublished investigations from site-specific safety cases by other authors. This study derives parameters (excluding depth) for events larger than 4 Ms since 1000. Maximum intensities (Imax) are given as MSK values, and areas for isoseismal 4 MSK are also listed. Some earthquakes below 4 Ms are included (down to 3.7 Ms) and the total number of events is 106.

A problem with this catalogue is that, in the drive to make it fully numerate, the authors proceeded to estimate parameters for some medieval earthquakes on data so slender that no weight can be given to the results. The text states that these values are "VERY APPROXIMATE"; the problem is that it often happens that a parametric catalogue becomes separated from its accompanying text, and that someone will attempt to draw conclusions from these parameters regardless of the fact that the uncertainty is so high that the epicentre could be almost anywhere in England.

The second such catalogue is that of Musson (1994), which supplements the published studies listed above with archive data. For the period before 1700 only events considered to be larger than 4 ML are listed, and parameters are not estimated where these are very uncertain. After 1700 all events above 3 ML are listed, and some events less than 3 ML where these are particularly interesting. It also integrates with modern BGS data.

Since then, a UK earthquake database has been maintained by BGS that combines historical and modern instrumental data, which provides a primary resource for seismic hazard studies. A database of intensity data points for pre-1900 British earthquakes was prepared as part of a European distributed online archive of historical intensity data in the AHEAD (Archive of Historical Earthquake Data) project (Stucchi et al. 2012), using dedicated software developed by Mario Locati of Istituto Nazionale Geofisica e Vulcanologica (INGV). This is in the process of being expanded to include all British earthquakes for which intensity data exist.

13.5 Hazard studies in the UK

Given that Britain is in a region of low to moderate seismicity, seismic hazard does not feature largely in the design of most structures. The exception is those for which the consequences of failure would be high. The first structure in the UK, so far as I am aware, that was built with potential earthquake hazard in mind, was the Kessock Bridge near Inverness, and here the impetus was the belief that the Great Glen Fault, which the bridge spans, is still an active strike-slip feature (which is open to doubt). The assessment of seismicity in the region made by Browitt et al. (1976) has already been mentioned. The companion hazard report by Burton and Browitt (1976) made an informal estimate of the 100-year event as 5.0 mb, and selected some strong ground motion records from California that were considered comparable to what might be expected at Inverness.

The first generation of nuclear power plants (NPPs) in the UK were built without any consideration of seismic safety, and it was in the late 1970s that the Nuclear Industry, both the chief operator, the Central Electricity Generating Board (CEGB), and also the Nuclear Installations Inspectorate as regulator, realised that this was a lack that needed to be addressed, hence the commissioning in the early 1980s of the reassessments of British seismicity already discussed.

One interesting fact about the development of seismic hazard analysis in the UK at this early period is that there was never any interest in the quasi-deterministic method popular in Europe, based on maximum observed intensity. Right from the outset, hazard was conceived in probabilistic terms, either from a Cornell-like approach or from extreme value methods (which is what Lilwall 1976 employed). The only other method used for hazard calculation has been a stochastic approach, introduced by BGS in the mid 1990s, and even this gives identical results to the Cornell method given the same input (Musson 2012b).

The earliest hazard calculation specifically intended to be relevant to the nuclear industry was made by Irving (1982), who used a single uniform source covering the whole country to come up with a "typical" UK hazard value of 0.25 g peak ground acceleration (PGA) for a 10,000 year return period, a result that was later to be much used for mental "anchoring".

Early hazard software used for site-specific analysis was EQRISK (McGuire 1976), used by the consultancy Principia Mechanica, and in BGS an extreme value hazard program developed by Makropoulos (1978) was employed. When the use of extreme value methods was discontinued, SEISRISK III (Bender and Perkins 1987) was used for a while in BGS, later replaced by in-house software for stochastic simulation hazard estimation. Early examples of studies include Woo (1983)—hazard assessment for a nuclear site using EQRISK, and Burton et al. (1981)—hazard assessment for a hydrocarbon facility using extreme value statistics.

During the 1980s and 1990s, the majority of site-specific hazard studies for NPPs were undertaken by a group of consultants led by David Mallard of the CEGB, and including staff from Principia Mechanica and Soil Mechanics. The Seismic Hazard Working Party (SHWP), as they were called, developed a consistent set of working practices for probabilistic seismic hazard analysis (PSHA) based around a rewritten version of EQRISK called PRISK, developed by Gordon Woo, which included the ability to implement a logic tree structure for handling epistemic uncertainty.

After 1995, there was an end to new NPP construction in the UK, and after a 2001 BGS assessment (Musson et al. 2001b) of seismic hazard for the Wylfa site in North Wales (opened 1971) no further major NPP seismic hazard assessments were completed. However, a government policy review in 2006 announced the resumption of NPP construction, and since then there has been extensive work on site selection (a mixture of previous NPP locations and new sites). ONR recently commissioned a report on capable faulting in the UK as a general set of guidelines, and founded a permanent panel of reviewers to provide guidance on future hazard assessments. PSHA studies are currently in progress for one of the first new build sites to be considered, and a second is in the planning stage. These will doubtless reflect advances in PSHA methodology since the last century.

Aside from seismic hazard assessment for critical facilities (and not just NPPs and bridges; various other facilities have been the beneficiaries of seismic hazard studies), there is a long-standing interest in seismic hazard maps as a general indication of varying levels of hazard across the country.

In fact, the first recognisable hazard study of the UK, in the sense of plotting some form of ground motion values with an associated probability, seems to be that of Lilwall (1976). The output from this study was a hazard map of Great Britain plotting macroseismic intensities with a 200 year return period. The intensity scale used was the Davison Scale, possibly the last time this scale ever saw use. The method used was an extreme value approach following Milne and Davenport (1969).

The first study to attempt to improve on this was commissioned by the Department of the Environment to produce a comprehensive overview of both seismic hazard and risk in the UK. It was undertaken by Arup (1993). This study chose not to calculate actual hazard maps; possibly due to restrictions of computer software and hardware at that time. Instead, Arup (1993) opted to calculate seismic hazard values for eleven cities spaced more or less evenly over the country, including Belfast, in Northern Ireland.

Very shortly afterwards, a set of hazard maps for the UK offshore area was commissioned by the Offshore Department of the Health and Safety Executive, as a collaboration between Arup and BGS. Although this work was completed in 1992, due to administrative delays it was not published until five years later (Musson et al. 1997), and thus appears out of place in the chronological sequence.

This was the first time that fully probabilistic seismic hazard maps had been produced in the UK; the problems of sufficient computing resources were solved by the use of facilities provided by Edinburgh University, who made time available on an experimental advanced machine. The hazard contours in the maps covered only the UK's territorial waters, and land areas were blanked out. However, the source model covered the entire area from Denmark to the mid-Atlantic ridge.

Soon after the offshore study had been completed (i.e. around 1994) a further project, again as a result of a government commission, was started to produce the first onshore hazard maps for the UK. This work was a collaboration between BGS and what was then AEA Technology. Whereas the offshore study had used the well-known PSHA program SEISRISK III (Bender and Perkins 1987), this new study developed a program SUNMIC, an adaptation of SEISRISK III that allowed the use of logic trees (Musson and Winter 1996, 1997).

Development of the Musson and Winter (1996) source model continued in the context of GSHAP (Global Seismic Hazard Assessment Programme, Grünthal et al. 1996, 1999). This model has proved robust. It reappeared in the SESAME project (Jiménez et al. 2001), and was used in a testing exercise in Musson (2004), Musson and Winter (2011) and shown to give good consistency with the historical record.

Around the same period, another offshore map was published by EQE (2002) as a result of a collaboration with NORSAR. As in Musson et al. (1997), the hazard contour maps show land areas blanked out.

In the mid 2000s, a new national seismic hazard study was commissioned, expressly for the purpose of being used with the UK National Annex to the Eurocode 8 building code (Booth 2008). The source model for this was developed following an approach to zonation suggested by Musson et al. (2009) based on work done in the PEGASOS project (Abrahamson et al. 2002) at the beginning of the decade. This envisages a three-stage process:

> The first stage involves the determination of the kinematic model. This is the basic element of the conceptual model of the seismic process at a sub-continental scale. The kinematic model describes, at the broadest scale, what is the relationship between large blocks in the Earth's crust in terms of relative movement … This describes the basic mechanisms for earthquakes that are to be expected in different parts of the area under examination. The second stage refines the kinematic model into the seismotectonic

> framework. In this part of the process, the very broad divisions used in the kinematic model are looked at in more detail, with the aim of dividing them up into volumes of crust that are sufficiently structurally distinct that it is improbable that seismicity could be considered to be uniform across the boundaries of such divisions … one is seeking to characterise areas that have a similar style of faulting, are experiencing a similar pattern of crustal stresses, and so on … The third stage is the final construction of the seismic source definition. Here the final partition of the seismic source model is made from analysis of the seismotectonic framework together with the detailed pattern of observed seismicity and local geological structure. (Musson et al. 2009)

The basis of the new model was an extensive study of the tectonics of the UK carried out by Chadwick et al. (1996). This included the preparation of a seismotectonic model for the UK, with divisions made on the basis of zones representing "the surface projections of subsurface volumes of characteristic upper crustal geological structure" (Chadwick et al. 1996). The same report also includes a kinematic model that attempts to show how the major structural blocks respond to the overall crustal stresses applied as a result of Atlantic opening, and how this is likely to induce areas of stress concentration and stress shadow (see also Musson 2007).

Chadwick et al.'s (1996) zones are classified according to a system that starts with the geological terrane (e.g. Scottish Caledonides) and then numbers the individual zones. Thus the zones that make up the area of the Scottish Caledonides have identifying codes SC1, SC2, etc. These formed the basis of the model developed by Musson and Sargeant (2007), but modified in the following manner. Firstly, aseismic zones were ignored (there was no background zone, and the model did not tesselate over the whole British Isles). Secondly, zones that appeared to be closely similar, especially with respect to seismicity, were merged. Thus seismotectonic zones SC7 and SC8 became seismic source zone SC78. Thirdly, zones that appeared strongly inhomogeneous with respect to seismicity were divided into high and medium parts, or occasionally high, medium and low. Thus seismotectonic zone SC4 became seismic source zones SC4H and SC4M; the remainder of SC4 was simply dropped from the model. Hazard maps were prepared for 475 and 2,475 year return periods, PGA only.

Since 2007, this source model has been developed further, primarily in the context of the SHARE project (Seismic Hazard Harmonisation in Europe), where it has been integrated with models for Norway, the Netherlands, Belgium, France and the Atlantic, producing a harmonised model for the whole North European area (Giardini et al. 2009).

14 Conclusions: history past, present and future

At the end of this short journey through one and a half thousand years of British seismology, what can one conclude? Is there anything particularly British about it, apart from it having been undertaken in Britain? Or to put it another way, if Robert Mallet had been born an Italian, would his work have been any different?

There are several factors that have shaped seismology in Britain that have given it something of a distinctive character. The first of these is a question of resources. It will have been very apparent from this monograph what a huge role the British Association for the Advancement of Science played in the development of seismology from 1840 onwards, in bringing together the best minds with varied interests at the annual meetings, and encouraging activity through the Association's committee system. Other countries have had their own learned societies, but none of these promoted the investigation of earthquakes so assiduously as the British Association.

Secondly, there are two linked factors that contributed to the way in which global seismology started in Britain: the status of Britain as chief global superpower at the end of the nineteenth century, and the good relations between Britain and Japan that led to so many able British scientists taking positions in Japanese universities. If Milne, Ewing, Gray and Knott had never gone to Japan, would they have ever become so involved in the study of earthquakes? And would Milne have been so successful in establishing his global network if he had not had the backing of the British Empire?

But there is another strand to British seismology that relates to the seeming paradox that a country with so little seismicity has played such a major role in the development of earthquake science. Much of the character of seismological investigation in Britain has been conditioned by the fact that people like David Milne, Lowe, Davison and the others had mostly small, non-damaging earthquakes to deal with. Flamsteed and Michell could speculate on the causes of earthquakes on the basis of reports from Italy or Portugal, but before the start of British involvement in Japanese academia, the only person in this monograph who actually saw the effects of a major earthquake first-hand was Robert Mallet, and the only person who ever made a field investigation of earthquake damage in Britain was Raphael Meldola, and it would be difficult to say with certainty what was the next occasion on which damage from a British earthquake was examined first-hand by a seismologist.

As a result, there has been far more interest in British earthquake investigations of non-damaging intensities than would be the case, say, in Italian seismology. For the simple reason that often enough, that is all there was to record. As a result, the British historical earthquake record is reasonably complete down to much smaller magnitudes than would be the case in most other countries—because even small earthquakes were interesting, rare phenomena.

Lastly, unless one is a historian of the Marxist school, there is the role of happenstance. If Mallet had been born an Italian, it is certain that much would have turned out differently. Suppose David Milne had stuck to his law business? Suppose John Milne had settled down in Newfoundland? Which brings me to a final, and general point that applies to more than just the history of science in one country.

History is what you can remember; but memory is a form of knowledge, and knowledge, as Dr Johnson famously said, is of two sorts. You can know a thing yourself, and you can know where to find it. That is, if it can be found. Writing the history of British seismology, or indeed, the history of anything, is an exercise in discovering what can be found and what is forever lost.

And things may be lost partly because they were never committed to paper. When someone a hundred years hence comes to write the history of seismology in the twenty-first century, what will they have to go on? Published papers, certainly, as a dry scientific record of what work was done. But how it came to be done is another matter. Scientists are human, and what they do is often shaped by human concerns, conversations, arguments and rivalries. These are seldom written down, yet they are instrumental in shaping the course of science.

Most of us can probably think of some salacious stories about colleagues that we might share in confidence over a glass of beer, but would not dream of committing to print. But in some cases, these stories did have an impact in the ways in which careers developed. We only know about the poisonous atmosphere in Comrie during the 1840s because Drummond, at the end of his life, broke the code and committed his imagined grievances to print. For this he was condemned by Davison (1924) for acting to “revive and preserve memory of a painful controversy”. (Davison’s choice of words suggests that he knew about this from other sources, presumably word of mouth, now lost to us). However painful a controversy it may have been, it is better that we know the truth than nurture a false cosy image of the “Comrie pioneers”.

Then there is the premature end of the first British Association Committee; not due to any scientific reason but to David Milne's realisation of the decline in his personal fortunes, committed to paper in a notebook that luckily survives. Milne's letters would be interesting to read, but only a few survive, and of those, even fewer deal with seismology. Those that do are entirely concerned with the finances of the British Association Committee, and most are to William Buckland (whose replies are instantly recognisable from his wild handwriting).

How much else was never written down? In a modern seismology conference, much of the most important business is conducted in the coffee breaks rather than the sessions, and most likely the same was true of the British Association meetings in the nineteenth century. Did Robert Mallet fail to get along with James Forbes? How bad was the friction between John Milne and Alfred Ewing, and how different would the development of seismology have been if they had got on better?

These questions can never be answered, because those who were eye-witnesses never wrote their stories, and word of mouth finally faded away with the passing of generations. Politicians often have an eye on posterity, and write their memoirs of self-justification for future historians to pick over. A British cabinet minister knows he is making history; a British seismologist is rather less likely to think in such terms, and many of the people mentioned in this monograph would probably have been astounded to imagine that anyone would be writing about them in such a way in the second decade of the twenty-first century.

But how much sadder are the questions that can never be answered because the information was written down but then lost. The great contrast here is between Davison and Dollar. A great debt of gratitude is due to Jenny Dollar for ensuring that her husband's papers were preserved after his death. As a result we know far more about British seismology in the twentieth century, and British earthquakes too, than we otherwise would. The present monograph gives only a limited synopsis; much more detail is to be found in Lovell and Henni (1999).

How much more would we know, if Davison's widow had done the same. Davison's letters to Rebeur-Paschwitz, Omori, Milne and others would make fascinating reading today and doubtless throw much light on the beginnings of modern seismology. And for all the limitations of Davison's macroseismic investigations, having his files on the various earthquakes he investigated would certainly improve our reconstructions of them.

Yet before we condemn Mrs Davison too much, we should perhaps imagine her situation. Who could she have entrusted her husband's papers to? There was no obvious home for British seismology at the time. Perhaps Kew Observatory might have taken them, but the practicalities of finding a home for such an archive in 1940, with all the difficulties imposed by wartime conditions, would have been daunting. Many were on military service, including Dollar. Why didn't Davison leave provisions for the preservation of his legacy? His only will was made on his wedding day, and is not much more than one sentence long. Was his mental health in decline towards the end? Did he even destroy most of his papers himself? We cannot tell.

It is essential, therefore, for the health of seismology in any country, to have a recognised archive where such material can be preserved for posterity. In Britain, credit is due to Paul Burton and Graham Neilson for establishing such an archive at BGS, and ensuring the preservation of important collections, including the Dollar archive (and also the Willmore archive, thanks to Mrs Willmore), and also, of major importance, the archives of the defunct Kew Observatory, which include the largest collections of historical seismograms in Britain, and the largest collection of seismological bulletins.

The great danger is that in a time of economic recession archives will come under pressure from accountants who want only to save money, and who have no understanding of seismology. What is lost once can never be recovered.

There are various arguments that will be advanced in such cases. “It’s all on the internet”. It’s amazing how much *is* available on the internet, including such highly obscure things as “Wark’s method of measuring earthquakes” already referred to. But equally, there is much that isn’t.

It is critical here to take a long view. The internet is a wonderful resource, and it has been a boon in writing this monograph to be able to download pdf files of most of the British Association reports without having to go to the library to consult them. However, the internet is barely 20 years old, and its long-term vulnerabilities are as yet unknown. As an example, many of the books digitised by Google and made available through Google Books have been withdrawn. Some are still available elsewhere, but many are not. Do you need those shelves full of old journals when you can access any issue over the net? Yes you do; if you have that paper copy, it’s yours as long as you hold on to it. A digital copy on a remote website can be withdrawn at any time, as with Google Books. It would be folly to rely for one’s scientific resources on a website you don’t control which might vanish for unexpected reasons.

Then, do you really need to keep photocopies of things? Perhaps you do—one result of the investigation of historical British earthquakes that led to the publication of Burton et al. (1984b) and associated reports was a large collection of full-page photocopies of local newspaper reports of historical earthquakes. In many cases, back in the early 1980s, the original newspapers, printed on cheap acidic paper, were already crumbling. Now they are so deteriorated as to be inaccessible. For anyone wanting to read the original accounts of British earthquakes, that collection of photocopies is at present the only way to do so.

Then “everything should be digitised”. This is a good idea; a digital version provides a backup in case of catastrophe and is more easily distributed. But it does *not* mean you no longer need to store the originals. There are three reasons for this. One is that the copy may lack something the original has, which you cannot anticipate yet. Willmore had no idea that anyone would want to digitise seismograms to calculate seismic moment when he decided that it was sufficient to store microfilm copies of the Edinburgh Observatory Milne-Shaw seismograms, and decided that the original records need not be kept. The result: irreplaceable data lost. The second is that copies to new forms of media have vulnerabilities that cannot be anticipated, and, though one may resolve to migrate records to new forms of storage as technology changes, this may not work out in future. There are some sad stories of major historical data sets (not in seismology) that are inaccessible simply because it is no longer possible to read the media they were stored on. It happens. If you have the originals, you can always return to them. Thirdly, as a simple question, if you had the manuscript of Newton’s *Principia*, would you burn it because who needs the manuscript when the book is in print?

Accountants like to demonstrate “innovative cutting-edge science”. Keeping archives is not that. The history of seismology, British or otherwise, is not that. But it is what has shaped the scientific landscape in which we work. It is part of our culture as seismologists. It is our patrimony. We must not lose it.

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