# A history of Ohm's Law

## Investigating the flow of electrical ideas through the instruments of their production

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This thesis is submitted for the degree of Doctor of Philosophy.

# Preface

This thesis is the result of my own work and includes nothing which is the outcome of work done in collaboration except as declared in the Preface and specified in the text. I further state that no substantial part of my thesis has already been submitted, or, is being concurrently submitted for any such degree, diploma or other qualification at the University of Cambridge or any other University or similar institution except as declared in the Preface and specified in the text.

It does not exceed the prescribed 80,000 word limit (including footnotes, but excluding bibliography) for the History and Philosophy of Science Degree Committee. The Appendix is excluded from the word limit by prior agreement with the History and Philosophy of Science Degree Committee.

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# Summary: A history of Ohm's Law: Investigating the flow of electrical ideas through the instruments of their production

Charlotte Elizabeth Connelly

This thesis takes a deep dive into the electrical work of Georg Simon Ohm. It is tightly focused on the period 1825-1827 when he developed and published the famous law we now think of as "Ohm's law". This work differs from previous studies of Ohm's investigations by putting material culture at its heart. Using Ohm's research as a case study, this project asks: how can the material culture of the physical sciences contribute to contemporary historiography in the history of science? What can we uncover from a material-led investigation that would remain inaccessible in a text-led investigation? And, in this particular case, how does looking closely at Ohm's experimental apparatus and practice help us to understand the development of his law? As part of the material-led study, this project incorporates a reconstruction and reworking of Ohm's 1826 experimental apparatus.

As something of an outsider, Ohm has defied ready categorisation by historians. This project looks at why that might be, and what his theoretical and methodological influences were. Looking at the way Ohm's instrument was designed and used, we can see the influence of natural philosophers including Coulomb, Ørsted, Seebeck, Ampère and others, while looking at Ohm's mathematical treatise reveals the strong influence of Fourier. Perhaps most notable from Ohm's work in this period is how readily he changed his conceptual approach. Over the course of three years, three distinct phases of work can be identified: in the first phase Ohm used a voltaic, or hydroelectric, pile and described his observations in terms of "loss of force"; in the second he replaced the unstable voltaic pile with a thermocouple and described his observations in terms of "exciting force"; and in the third he set aside his experimental work and focused instead on making mathematical arguments, shifting his language again to describe what was happening in an electrical circuit in terms of "electroscopic force".

The shift between Ohm's first two phases of work can, this project argues, be made sense of by understanding changes in Ohm's experimental setup and the way they affected his interaction with the phenomena he was studying. This thesis presents objects and instruments as integrated parts of the thought process of an experimenter, in which the apparatus shapes the thinking of the experimenter as much as the experimenter's thoughts shape the design of the apparatus. This framework is then applied to Ohm's work as a case study, leading to the suggestion that Ohm's material interactions with his experiment shaped his conceptualisation of what was happening in an electrical circuit. It is possible to draw clear links between the conceptual tools and the material tools that Ohm used in his different phases of work; and, in understanding the practical experience Ohm had of using his apparatus, to make sense of his shifting use of language as he moved through different phases of work.

Through its material-led approach, this project brings together history, philosophy and material culture of science in a single case study. It presents novel findings about Ohm's work, uncovered through material engagement with his experimental setup, and offers a set of tools for material-led studies in the history of science.

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As a material-led study, this work is dependent on collections professionals who care for objects, archives and heritage sites and make them available for access. Particular thanks are due to Dr Johannes-Geert Hagmann, Professor Jürgen Teichmann and the archive and conservation staff of the Deutsches Museum. I was particularly grateful to Dr Hagmann for enabling me to access the Ohm collection in the brief window when it was available between display and storage, during a very busy period as the museum embarked on a major refurbishment. Thanks, too, are due to Giulia Simonini, without whose palaeographic services I would not have been able to make sense of Ohm's archive documents.

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Finally, I thank my parents who showed me that combining work and study is not only possible, but enjoyable, and who shared their interest in the history of science with me from a young age. My Dad receives full acknowledgement in the main body of this thesis for his role as the instrument maker for this project. More than that, growing up he showed me that it is possible to design and make material things to a high standard, and that there is pleasure to be had in the process. The sense, from both my parents, that seeing a significant project through to completion can be deeply satisfying permeates this entire piece of work. Undertaking this project has been a pleasure and a privilege, from start to finish.

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# Introduction

This project takes a deep dive into Georg Simon Ohm's 1826 experiment, and the role it played in developing his now-famous electrical law. It presents a material-led approach to studying and understanding Ohm's work, taking evidence from material things such as museum collections and heritage sites alongside more traditional text-based materials. Using Ohm's experimental apparatus as a framework, this work takes the three component parts of the instrument in turn. It investigates the intellectual ideas they embody, and what they tell us about Ohm's influences. By looking at Ohm's instrument, we can see residues of the thoughts and ideas of a number of other researchers embedded in the apparatus. For example, we can see from looking at Ohm's introduction of the thermocouple that he has adopted Ørsted's theory of thermoelectricity (Chapter 4), and from looking closely at the setup of Ohm's torsion balance we can see traces of Ampère's "current balance" (Chapter 5). Ohm's instrument illustrates the influences and networks he was exposed to, as well as his technical knowledge and skills, in a way that texts alone do not.

As part of its material-led approach, this project includes a reconstruction of Ohm's apparatus and a reworking of his experiment. The original instrument does not survive, so this method enables close engagement with the materials of Ohm's experiment. By making the materiality of Ohm's experimental work central to this study, I have been able to draw novel conclusions about Ohm's knowledge, skills and networks and how they all contributed to the development of his law. At the heart of this thesis is a study of the two-way relationship between Ohm as an experimenter and the materials he used, and an argument that interactions with material things shape the ways that experimenters conceptualise or theorise about the natural world. Along the way, this thesis draws novel conclusions about Ohm's influences and practice, and offers insight into a variety of ways that material-centred research in the history of science can be productive.

### Historiographical context

As we approach the two-hundredth anniversary of Ohm's Law, a close study of his apparatus to better understand his work feels particularly timely. My study of, initially, how electrical current was first theorised dates back to 2007-08 when I carried out a research project as part of an undergraduate module focused on the history of electricity. The questions raised by that project stayed with me, and in June 2012 I began seriously putting together a grant

application for funding to continue with the research as a doctoral student. In the intervening years I became a museum curator, and my professional life became preoccupied with objects and the stories that they can tell. It seems curious now, reflecting on my work in the context of other related scholarship, that there is such a strong focus on reworking physical science instruments and experiments from, approximately, 1780-1850 (Chapter 2). Is it because they are often used as canonical experiments in the teaching of physics, and therefore follow a tradition of pedagogically-motivated reworking? Perhaps, despite the complexities revealed in my project, it is because they are relatively comprehensible and possible to manufacture? Could it be that the historiographical tradition of elevating historical figures to hero status extends to their experiments, particularly in the history of physics where heroic names are enshrined as everyday units of measurement? My notes from 2012, when I was scoping possibilities for a funding application, suggest that my principal interests were (a) philosophical – how do you define voltage, current and resistance when they all rely on each other for their definitions? – and (b) methodological – what could be learnt from close reading of museum objects and replication of historical activities? At that stage Ohm's experiment was just one of several possible case studies, suggesting that I was less influenced by the heroic appeal of a well-known name, and more by curatorial and philosophical concerns combined with a broad interest in electrical science.

Aside from the trajectory my personal engagement with this project took, there are a few different ways to understand how this research fits within the context of current scholarship. The project began life as a Collaborative Doctoral Award (CDA), funded by the UK's Arts and Humanities Research Council, and its conception was shaped by the funding stream and its priorities. Under the CDA scheme, the subject matter for the doctoral research is agreed between a partner organisation and a higher educational institution, in my case the Science Museum and the University of Cambridge. It enables museums and galleries to attract students to work with them in ways that "will support their objectives and to better understand their collections, organisations, audiences etc." (Hill & Meek, 2019, p. 12). The shape of this project is informed both by my personal interest and the current research landscape, which in turn is shaped by the strategy of the UK's national funding organisation.

My professional context has strongly informed the production of this research. As a professional museum curator and manager, I am required to report regularly to funders and stakeholders about the number of research enquiries I and my colleagues facilitate every year. The continual need to demonstrate good value for money as we undertake the resource-

intensive task of caring for growing collections of objects, with research access providing one possible metric for assessing our progress, is a subtle but ever-present motivation for me as a manager in a museum, and in part drives my research methodology and questions. How can the material culture of the sciences contribute to contemporary historiography in the history of science? In this particular case, how does looking closely at Ohm's experiment help us to understand the development of his law? Can we glean insights by working this way that would otherwise remain inaccessible? How are museums supporting wider research agendas, and are their collections being effectively used as a research resource for historians of science? This project was also designed to respond to a perceived need among some historians of science to more enthusiastically embrace the "material turn" and incorporate the material culture of science into our historiography. This is discussed in more detail in Chapter 2, which presents an argument that our museum collections are underutilised by historians of science, and suggests that there is a textual bias within the discipline.

Finally, it may be helpful to consider how this piece of work is positioned in relation to the integrated history and philosophy of science (integrated HPS) movement. In recent years, several scholars have made a case for the integration of the two disciplines (for example Arabatzis & Schickore, 2012; Scholl & Räz, 2016). The movement responds to concerns most overtly raised in 2002 in a special issue of *Perspectives on Science* which addressed "the precarious relationship between history and philosophy of science" (Burian, 2002). Although my research is somewhat influenced by integrated HPS scholarship, it is not a self-conscious exercise in combining the two disciplines. Rather, the understanding of some basic arguments in the philosophy of measurement is essential for interpreting the history of a measurement instrument in the development of a physical law. In that sense, Burian's statement about the research papers in the special issue cited above could just as easily apply to my research: "Each of the papers in this issue needs to intertwine history of science and philosophy of science to push its argument home. For all the differences in the methods and positions of the authors, the issues they open up cannot be resolved without recourse to a combination of historical and philosophical work" (Burian, 2002, p. 406). However, I would argue that my project goes further, and not only investigates the history and philosophy of the development of Ohm's law, but also a historical and philosophical approach to analysing his experimental apparatus and the role of scientific instruments in shaping his theory development more widely. An integrated history, philosophy and materiality of science hardly rolls off the tongue, but nevertheless, that is what this project presents. In this work the

three component parts are all interrelated, and all integral, their different perspectives each helping to reconstruct Ohm's work and motivations. Ohm's apparatus acts as a boundary object, enabling us to explore philosophical, historical and material concerns, and therefore to better understand Ohm himself.

**The narrative**

My exposition of the development of Ohm's law begins with an introduction to Ohm, with a focus on his educational upbringing and the professional and scholarly networks he was part of. Chapter 1 presents the context that Ohm was working in through the usual literature review, together with a material culture review which considers the material residues and influences of Ohm's work. This is followed by Chapter 2 which explores methodological approaches to working with the material culture of experiment, and the particular approach I have taken in this project. Chapters 3, 4 and 5 each take one of three component parts of the experimental setup in turn, the conducting wires, thermocouple and torsion balance respectively. Early on in my research I thought of these chapters as "current", "voltage" and "resistance", however, as my work progressed it became increasingly clear that those terms were ahistorical, making them inappropriate chapter headings. These chapters explore the theories that underpin the components, together with the ways Ohm used and interpreted each part of the apparatus. Chapter 6 pulls the dissected pieces of the experiment back together and considers Ohm's turn to a mathematical description of his findings, before the Conclusion presents some of the overarching findings of this work, including a summary of the tools used for "reading" material evidence.[1] Interwoven throughout the final four chapters is my interpretation of Ohm's work taking place in three distinct phases (this interpretation builds on the description of two experimental phases first presented by Luchetti, 2020).

*Phase 1*

The key document for Ohm's first phase of work is his 1825 paper, "Preliminary announcement of the law according to which metals conduct contact electricity." This phase is characterised by Ohm's attention to "loss of force" (*kraftverlust*), which in his experiment

[1] Throughout this work I use the verb "to read" in its most generous sense, to describe the act of discerning the meaning or intention of a thing or situation, whether written, material or situational.

could be observed as a reduction in the deflection of a magnetised needle in a torsion balance. Chapter 3 discusses electrostatic conceptions of electrical conduction as a leakage of charged conductors. Ohm's use of "loss of force" can be interpreted as following a similar mode of thinking. There is strong evidence that Ohm took inspiration from Coulomb, including his use of a torsion balance, the application of a logarithmic law to his findings and, perhaps, the echoes of electrostatic theory in his "loss of force" concept. Experimentally, this phase of work centred on a setup comprising an unstable voltaic pile, a torsion balance for measuring "force" (*Kraft*) from within the circuit, and a set of test wires of varying lengths and, therefore, conductivities.

*Phase 2*

For Ohm's second phase of work the key document is his 1826 paper, "Announcement of the law according to which metals conduct contact electricity, together with a draft for a theory of the Voltaic apparatus and the Schweigger multiplier", in which he corrects his earlier logarithmic equation. Instead, he presents something we can now recognise as Ohm's law. Experimentally, his setup appears very similar to the experiment described in his 1825 paper, with the exception that the unstable voltaic pile was replaced with a much more stable thermocouple. However, Ohm's language changed for the second phase. Instead of discussing "loss of force", he discussed "exciting force" (*erregenden Kraft*), suggesting a shift from a concept of current leaking away from its source towards one where electricity flows freely through a conductor. In this period, Ohm was influenced by other studies of conductivity, as well as Ørsted and Fourier's work to investigate the thermocouple. He also made a distinction between the operation of "hydroelectric" circuits, by which he meant those using the voltaic pile, and his circuit without any "fluid" conductor. The shift between Ohm's first and second phases of work is explored in Chapters 3 and 4.

*Phase 3*

Chapter 6 deals largely with Ohm's transition from his second to third phases of work. In his third phase, Ohm wrote a book-length mathematical treatise, *The Galvanic Circuit, Investigated Mathematically*, which was published in 1827. In this phase, the language Ohm used to describe what happens within a circuit changed again, shifting to "electroscopic force" (*elektroskopische Kraft*). The book itself overtly uses Fourier's work as a starting

point, following his framing techniques and use of calculus. The book goes further than the two preceding papers in making claims about the nature of the electrical circuit, making some assertions about how electricity might move through a circuit, though Ohm refrained from speculating about the nature of electricity itself.

Following these three phases, we can unpick the novelty of Ohm's arguments in contrast to those of his predecessors. The ability to measure electric current, as opposed to static electricity, had only arisen with Ørsted's discovery of electromagnetism in 1820. Ampère had led the way in developing a holistic theory of a circuit, moving beyond studies of the pile or a conductor as separate components and highlighting some important differences between electrostatics and electrodynamics. It was only with Ohm's work that electrical resistance came to be understood as a key linkage between electric current and electric tension, at a time when all of these terms were still being negotiated. By end of his 1827 monograph Ohm had presented not only a simple mathematical relationship to relate all three concepts, but had gone further than any of his predecessors in making sense of the impact that changing the qualities of any part of a circuit had on the circuit as a whole.

**Interpreting and reworking Ohm's experiment**

When this project was initially conceived, the project proposal included the intention to replicate Ohm's experimental apparatus in order to repeat his experiment. The motivation behind this was an early recognition that the language around electrical ideas was still not settled, and that copying Ohm's interactions with the phenomena he sought to describe might help to clarify the meanings behind poorly defined terms. What very rapidly became clear was that "replicating" and "copying", by which I meant producing precise facsimiles of things and actions, were impossibilities. Not only does Ohm not provide enough detail to enable a reliable replica of his instrument, but it is also apparent that it is impossible for me to effectively copy his thoughts and actions because I can never think as Ohm did. Even if all physical aspects of the experiment could be replicated, the way I think and work would still be shaped by the particularities of my upbringing, education and experiences. Relatively quickly, then, the approach shifted away from replicating Ohm's apparatus towards reinterpreting and reproducing it, and from copying his experiment to reworking and restaging it. I am not the first person to wrangle with these nuanced terminologies. As Fors et al. have written,

> Many terms have been used to designate the act of doing experiments as a historian of science and technology, including "replication," "reconstruction," "re-enacting," "restaging," and even, when visits to a particular geographical site is involved, "retreading."
>
> (Fors, Principe, & Sibum, 2016)

Doron Swade also offers a set of guidelines, suggesting that "replication" implies creating a copy, or taking an impression, from an existing object; "reproduction" suggests an imitative copy that is "sometimes exact"; and finally, "reconstruction" may be reserved for the production of an object for which there are no surviving intact examples (Swade, 2011, pp. 107-108). My own principal concern in selecting appropriate terminology is to make clear that my efforts by necessity include considerable intellectual input from me and the maker I worked with, and that the apparatus is constrained and shaped by the tools, commodities and skills available in the twenty-first century. These factors combine with Ohm's work to produce something new, and categorically not a straightforward replica or copy of his apparatus and experimental practice. Because of the impossibility of following Ohm's instructions without introducing my own interpretations, this reworking of his experiment instead focuses on revealing the many gaps and omissions in his texts and uses the reinterpretation of his apparatus as a mechanism for investigating the unrecorded influences upon its design, manufacture and use. This in turn helps to understand some of the forces that helped to shape Ohm's thinking about electrical circuits.

**How to read this text**

This thesis interweaves two different complementary approaches to investigating Ohm's apparatus. The first is a relatively straightforward historical presentation of Ohm's work, taking the three component parts of his instrument in turn and discussing them in the context of the wider history of electrical research. This can be found in Part A of each chapter, and if the reader wishes they can read all of the Part As in sequence for a straightforward historical account of Ohm's work. The second strand of work is an analysis of my reworking of Ohm's experiment, including building an interpretation of his apparatus. This can be found in Part B of each chapter, and for a thorough discussion of what can be learnt from reworking Ohm's experiment the reader could read all the Part Bs in sequence. However, it is hoped that understandings of both strands of work are enriched by reading the two interlaced parts in

turn as presented. For a full, detailed report of the production and operation of the reworked experimental apparatus, the reader can turn to the Appendix. It may be helpful to have the Appendix available to read side by side with the main text, particularly when reading Chapters 3-6. The Appendix does not contain argumentation in support of the thesis, that is all in the main body of the work, however, it is regularly referred to throughout the text. To paraphrase the work of Lambros Malafouris, which will be introduced in Chapter 2 (p. 79), the material presented in the Appendix can be understood not as evidence of, or reflections of, my underlying thought processes in producing an interpretation of Ohm's apparatus. It is instead a manifestation of the thinking process itself, as indeed is the reworked instrument.

**Closing remarks**

In summary, this thesis presents a thorough discussion of Ohm's development of his famous law. By taking an integrated approach, bringing together philosophical, historical and material concerns, it offers a better understanding of the networks that Ohm was part of. The focus on Ohm's experimental apparatus reveals evidence of Ohm's influences that are not available in his writings, and also provides insight into the linkages between the context Ohm was working in – the commodity networks and skilled craftspeople he had access to, the communities and texts he was able to access – and the ways in which those contexts became entangled in his apparatus and his thinking. Beyond this particular case study, this piece of work presents a range of ways in which material things can be read and questioned as pieces of historical evidence, suggesting some methods and directions for future research projects to follow and build on.

# 1. Contextualising Ohm

## PART A

Georg Simon Ohm (1789-1854) was a German mathematician and physicist. In 1826 he published a research paper describing a series of electrical experiments he had carried out, concluding with the first appearance of the electrical law now known as Ohm's law. Fast forward to the 1860s, and a committee of the British Association for the Advancement of Science agreed that a standard unit of electrical resistance should be called the "ohmad", later shortened to the "ohm" (Hunt, 1994). Enshrining Ohm's name as an important unit of measurement suggests a deep appreciation for Ohm's work among the leading physicists who followed him. However, his work did not receive instant recognition.

This chapter offers context to Ohm's early career up to the publication of his 1827 treatise, *Die galvanische Kette mathematisch bearbeitet* (*The galvanic circuit, investigated mathematically*). The publication marked the end of an intense period of work for Ohm between 1825 and 1827, in which he established his law experimentally and then subsequently derived it mathematically. This first chapter sets the scene for those works, discussing the environment Ohm grew up in and the networks he had access to.

The early nineteenth century was a period of huge social change in the German states, and education systems in Germanic countries were heavily manipulated by politicians as they sought to manage international relationships. The opening section will also explore the competition for access to research facilities for teachers, like Ohm, who wished to conduct their own research. The second section looks more particularly at cultures of electrical research in this period. It introduces discussions around what historians have described as competing contact and chemical theories of electricity, which are revisited in Chapter 4, and explores the material that Ohm was reading. It touches on his scientific correspondents and their views, and the role that Ohm's mathematical training might have had on his outlook. The final section provides an overview of Ohm's electrical research from 1825-1827, centred on Ohm's 1826 paper in which he first articulated his now-famous law.

### Ohm's upbringing and education

Ohm has been surprisingly little written about in the English language, despite his famous name. There are some biographical descriptions of Ohm written in English, though few are

recent and most are extremely brief (see Abbott, 1984; Baigrie, 2007, p. 72; Heering, Keck, & Rohlfs, 2020; Meyer, 1971, p. 51). Furthermore, places where Ohm might be expected to appear have little or nothing about him (see Dunsheath, 1962; Hunt, 2002). With a few notable exceptions the biographies that do appear follow a similar narrative which can be approximated as follows:

> Georg Simon Ohm (1789-1854), the son of a humble locksmith, was born in 1789 and brought up in Erlangen, Germany. He became interested in mathematics and physics, and pursued electrical research in part because his father's trade as a locksmith had bestowed Ohm with skills in drawing wires out. Ohm became a teacher, and harboured a deep desire to become a university professor. In 1826 Ohm published the law that would later make him famous in a little-read paper, followed up the next year by his book *The galvanic circuit, investigated mathematically.* The book was poorly received. It was filled with complicated mathematical workings, modelled on Fourier's analysis of heat flow, which many of its readers found difficult to comprehend. Professor Pohl in Berlin, a staunch Hegelian, was disdainful, and the local minister for education was also very unimpressed by the contents of the book. Ohm felt that his only option was to resign his teaching position. Several difficult years followed for Ohm. He struggled to find employment and his hoped-for University position began to feel like a distant dream. Gradually people from outside of Germany, away from the prevalent Hegelian ideology, began to read and understand Ohm's work for what it was: a masterful piece of patient and detailed theorising, shedding new light on a popular area of research. Although some investigators before Ohm had come close to Ohm's law, none had produced anything as rigorous or high in quality. At long last, Ohm received the recognition he had hoped for. In 1841 he was awarded the Copley Medal by London's Royal Society, in 1849 he was appointed extraordinary professor at the University of Munich, and in 1852 he was made ordinary professor. He died just two years later.
>
> (Drawn from a range of sources including Atherton, 1986; Rogers, 1960; Winter, 1944)

Except for factual names, places and dates, almost every aspect of this account has been challenged by a handful of researchers who have sought to place Ohm's work in a wider context. Perhaps unsurprisingly, there are more comprehensive biographies of Ohm available in German. The most thorough text is by Heinrich von Füchtbauer, Georg Simon Ohm's great-nephew, whose biography was popular enough for a second edition to be printed. It offers a family history as well as an insight into Ohm's upbringing and career (Füchtbauer, 1939, 1947). Teichmann's 1976 "Festschrift", marking 150 years of Ohm's Law, provides an excellent overview, as well as helpful listings of the holdings of the Deutsches Museum's

artefact collection and paper archive (Teichmann, 1976). McKnight notes that one of the most important biographical documents about Ohm's life, his brother Martin's autobiography, was burnt and destroyed in the Second World War. Consequently, all that survives from that source are snatches of quotes and references in secondary sources (McKnight, 1967, p. 110). However, there are several volumes of Ohm's collected letters and writings available in German, which, together with the archive collections at the Deutsches Museum, enable a reasonably complete account of Ohm's life and work to be pieced together (Hartmann, 1927; Nürmberger, 2002). As we will see, Ohm struggled himself to find an intellectual home, and was often something of an outsider as a result. However, those challenges are more subtle than the clichéd narrative that is often presented about Ohm's rise from apparently humble origins to long-awaited recognition.

The specific details of Georg Simon Ohm's early life are the subject of some confusion. Rollo Appleyard's 1930 history described the muddled reporting of facts by several sources (a problem which persists, e.g. Boëtius, 2006, p. 109). Appleyard noted that the memorial to Ohm was incorrectly placed on number 6 Fahrstrasse in Erlangen, and that Ohm's date of birth had also been misrecorded in a number of places, including his gravestone in Munich (Appleyard, 1930, pp. 182-185). Both of the errors reported by Appleyard have since been rectified. A memorial to Ohm is now placed on a building at number 11 Fahrstrasse, Erlangen, and today there are no dates on his gravestone in Munich, incorrect or otherwise. It is now marked only with the name "OHM", and an unidentified symbol that may be an interpretation of the omega symbol (Figure 1, visited by the author, 10 August 2015). These historic errors are a reminder that Ohm's rise to fame, together with the mythologisation of his supposedly humble upbringing, happened after his lifetime.



**Figure 1.** Ohm's gravestone in Munich's Alter Südlicher Friedhof.

Ohm's father, Johann Wolfgang Ohm, was indeed a locksmith in the town of Erlangen. He came from a family of locksmiths, and was the master locksmith at Erlangen University. The family had links with the University dating back to the 1760s when Johann Vincentius Ohm was accepted as a "University Citizen", seemingly as an arrangement to enable him to purchase a house from a Theology Professor. This citizenship provided him with access to the local locksmith's guild and enabled him to claim the title "Master" locksmith. As an academic citizen, he made his oath of loyalty to the prorector instead of the mayor, paid his dues to the university treasury instead of to the community, and was subject to the jurisdiction of the university senate (Füchtbauer, 1947, pp. 19-22). The Ohms have been described as an old *Bürgerfamilie* (bourgeois family) that had been locksmiths going back through the generations for as long as they could remember, suggesting the family held a high status in the community (Bauernfeind, 1882, pp. 3-4). Georg Simon Ohm's mother, Maria Elisabeth Beck, the daughter of a master tailor, was also from a successful merchant family (Teichmann, 1999, p. 489). The trades and the skills of craftspeople continue to be a celebrated part of the region's identity, as illustrated by a display of local trades in the City Museum Fembohaus in nearby Nuremberg which highlighted both locksmithing and the related trade of wire drawing as skills that the region had been known for since the fifteenth century (visited 14 August 2015).

Ohm was one of seven children, although only three survived to adulthood: Georg Simon, Martin and Elisabeth Barbara. His formal education began when he attended the Erlangen Gymnasium between 1800 and 1805. There he received a classical style of instruction stressing recitation, translation and interpretation of texts. This, Caneva argues, was "of considerably less importance than his father's tutoring", describing Ohm's father as a "self-sacrificing autodidact" who gave his sons a thorough education in mathematics, physics, chemistry and the philosophies of Kant and Fichte (Caneva, 2008a). Füchtbauer used the rather more poetic description of "*der Philosoph am Schraubstock*", or "the philosopher at the vice" (Füchtbauer, 1947, p. 29). The family lived at 11 Fahrstrasse, within sight of Erlangen University (Figure 2). Johann Wolfgang befriended several teachers at the University, and in 1804 the mathematician Professor Karl Christian von Langsdorf noted the mathematical talents of Georg and Martin, likening them to the famous Bernoullis (Hartmann, 1927, p. 12). Ohm's younger brother, Martin Ohm (1792-1872) went on to earn his doctorate under Langsdorf. He became a well-recognised mathematician in his own right, and was made a full professor at the University of Berlin in 1839. Georg also studied at the University of Erlangen, enrolling in 1805 but staying for only three semesters. He then moved to rural Switzerland, seeking an income and reportedly also escaping his father's displeasure at him spending too much time dancing, playing billiards and ice skating. In 1806 he began his teaching career, teaching mathematics in a private school, (Füchtbauer, 1947, p. 83).



**Figure 2.** 11 Fahrstrasse, Erlangen, the building where Georg Simon Ohm grew up, and where his father's locksmith business was based.

Based on this account it seems probable that Georg Simon Ohm was at the very least aware of a network of people engaged in mathematical research, and quite possibly actively participating in conversation and correspondence with them. This is a clear argument against lone-genius portrayals, such as Cajori's description of Ohm as "an ingenious investigator who, although removed from the influence of personal contact with the great physicists of his time, yet working independently and alone, discovered the great law bearing his name" (Cajori, 1962, p. 234). Ohm's mathematical upbringing, and his family's interaction with the local university and its mathematicians, also explains why he was able to undertake skilled work with advanced mathematics. That the humble son of a locksmith is revealed as perhaps less humble than originally thought is not to suggest that Ohm had an easy path to a career as a mathematician and physicist. As Jungnickel and McCormmach discuss, when Ohm was growing up there was no single German country, but instead many separate states. The rapidly changing geopolitics of his local area was just one aspect of the changing world that Ohm was living in. They give a sense of the uncertainties and disruptions that surrounded Ohm and his contemporaries:

> [From the time of Ohm's birth] Erlangen belonged to a Franconian principality. While Ohm was a student at the local gymnasium, Erlangen was Prussian; when he graduated from the local university, it was Bavarian. Ohm's early years coincided with the Napoleonic wars, when German frontiers changed frequently. The map of Germany

> was redrawn as the French extended their influence and rule, and it was redrawn again after the French were defeated.
>
> (Jungnickel and McCormmach 1986a, p. 3)

Rather than being merely paper exercises in changing allegiances, the effects of the military struggles taking place in the region must have impacted Ohm in more immediate ways. For example, in 1813, the final year that Ohm spent working in Erlangen as a *Privatdozent*, some 33,685 troops were billeted in the town which had a standing population of only 8,000 inhabitants (Appleyard, 1930, p. 190).

The details above help Jungnickel and McCormmach to place Ohm squarely in the context of a politicised education system in a period when German universities were developing rapidly, partly as a consequence of the major political upheaval of the time. By their account, the gymnasium secondary schools, such as the one Ohm attended in Erlangen, taught a humanistic curriculum which aimed to give students *Bildung.* Kathryn Olesko, in an article particularly about Prussia, though this particular claim is more widely applicable, suggests that this pedagogical emphasis grew out of school reforms, causing classical schools to merge with new pedagogical theories. *Bildung* could be achieved by stressing students' individual character development and by following a neo-humanist curriculum based on the classics (Olesko, 1989, p. 96). The emphasis on *Bildung* seems to have penetrated Ohm's thinking as his first book was titled *Basis of an advantageous treatment of geometry as a higher means of Bildung in preparatory schools* (Ohm, 1817). The book promoted students striving, thinking and doing for themselves, which Ohm believed was a form of *Bildung.* Jungnickel and McCormmach argue that Ohm's suggestion that students should strive for themselves and not merely accept the information presented to them was not desirable from the point of view of Ohm's employers, the authoritarian governments he was a subject of, who were busy trying to manage educational reform and difficult wartime politics. They imply that Ohm had selected the wrong audience to write for, saying "more successful scientists, particularly those outside Prussia, appealed to the interests of their governments rather than to those of their fellow teachers. They claimed usefulness for their subjects instead of the advancement of Bildung". Perhaps this explains not only why Ohm's first book was "scorned and then ignored", but also why he was not always popular with the education ministers he dealt with (Jungnickel & McCormmach, 1986b, p. 5).

Educational reforms in this period were also taking place in universities. The inclusion of a philosophical faculty in German universities enabled them to claim superiority over professional, military or engineering schools. However, this superiority did not confer great status on the philosophical faculty itself, which was not thought to be as important as the three professional faculties for training lawyers, clergy and physicians. This lower status for the philosophical faculty had a long history. Frederick Gregory argues that as institutions that were primarily useful for training civil servants, the philosophy faculty lost out to the other three faculties: law, theology and medicine. They were professional schools whereas the philosophical faculty merely "served these 'higher' faculties by acquainting the student with materials frequently so general and preliminary that the border between university and gymnasium was blurred" (F. Gregory, 1989, p. 18). Gregory continues by suggesting that it was only with the growth of the middle classes, who had the personal freedom and financial security to pursue non-utilitarian interests, that natural science became valued. It had, he claims, a "bourgeois character". This in turn led to the creation of institutions, academies and societies for natural science during the middle part of the seventeenth century. After this flurry of establishing such organisations, their rapid growth began to wane towards the end of the century. Universities worked more closely with these various organisations, for example Göttingen University which in 1751 worked closely with the Societät der Wissenschaften, and the societies were sometimes even absorbed by the universities.

One of the consequences of the raised status of science and research was that by 1790 there was an expectation that university professors should be carrying out independent research in addition to their teaching duties (F. Gregory, 1989, p. 19). However, the natural sciences still trailed behind the other three faculties. Jungnickel and McCormmach place Ohm, a schoolteacher at this point in our story, on a level with university staff in this intellectual hierarchy, writing that

> the philosophical faculty was considered not to be much above a secondary school, and physics and mathematics teachers, like their colleagues in the other natural sciences and the humanities, moved easily between the two kinds of institutions - and

> not just in one direction. Furthermore, schools and universities were seen to overlap so strongly that they coveted and competed for each other's physical apparatus.
>
> (Jungnickel & McCormmach, 1986b, p. 7)

Given the overlap in philosophy and natural science teaching between schools and universities, it is not so remarkable that Ohm was undertaking experimental research alongside his role as a mathematics teacher in a school. Why, then, was Ohm so desirous of a university job if schools and universities were on an equal footing both in terms of status and resources? His correspondence with his family shows that he applied for several positions in universities in the period after receiving his doctorate. This might be an indication that university jobs offered more than simply access to physical resources.

Returning to the period from 1805-1809, Ohm did not appear fulfilled in his work in Switzerland, though he benefitted from exposure to French. In early 1809 he wrote to Langsdorf, wishing to follow him to Heidelberg, but instead he was persuaded to continue where he was and spend time studying Euler, Laplace and Lacroix on his own. In Easter 1811, Ohm returned to the University of Erlangen, and on 25 October of that year he received his doctorate (Hartmann, 1927, p. 15; Nürmberger, 2002, pp. 148, 157). Whether or not he produced a written thesis is not clear. Teichmann states simply that he did not, and that he was awarded his doctorate after the unusually short period of three semesters. Writing a century earlier, Bauernfeind is not so sure. He agrees that no thesis was printed, but suggests that a favourable report of Ohm by K. Bayer, the interior minister, might refer to an early version of a treatise by Ohm on light and colour (Bauernfeind, 1882, p. 33; Teichmann, 1999, p. 489). Whatever the circumstances of Ohm's doctoral award, he was employed by the University of Erlangen for three years as a *Privatdozent*, a lowly position in the university teaching system. The *Privatdozenten* were the lowest of three ranks of university teachers beneath "ordinary" professors, who received a salary from the state to teach and additionally fees from their students, and extraordinary professors, who were not members of the faculties and usually received no salaries but did receive student fees. Beneath them both were the *Privatdozenten* (Jungnickel & McCormmach, 1986b, p. 18). The poor prospects for advancement at Erlangen led Ohm to seek alternative employment from the Bavarian government. In the years that followed, Ohm taught at the *Realschule* in Bamberg (January 1813-February 1816) and the Bamberg *Oberprimärschule* (March 1816-November 1817).

After several difficult and unsatisfying years in Bamburg he left the employment of the Bavarian government to work as the *Oberlehrer* of mathematics and physics at the newly reformed Jesuit Gymnasium in Cologne.[2]

One good outcome of working in Bamburg was that Ohm had become friends with a fellow teacher, Franz Göllern, who moved to Cologne shortly before Ohm to work at the new Jesuit school. [3] Göllern wrote enthusiastically to tell his friend about his new surroundings and urged Ohm to join him there. "You have three rooms", he wrote, "near me… with a view of the botanic garden and the northern part of the town opposite Mühlheim. A few steps from your door is the entrance to the observatory, which is, together with the apparatus, completely at your disposal." It seems not all is perfect in the new situation however, the building Göllern lives in is also home to a cashier so the doors are locked at 10 o'clock every night and under orders of the police the residents may not have their own keys. But, he reports positively, "everything is good on the Rhine, good beer, good wine" (Hartmann, 1927, pp. 42-47). Ohm's reply was effusive: "Thanks, huge thanks, dear friend, for your comforting messages; they have raised my expectations…" and "The apparatus, beautiful, excellent!" (Hartmann, 1927, p. 47). This last statement was in response to the list of apparatus that Göllern had sent Ohm, and which was later added to as he made further discoveries:

1. Three air pumps with very complete apparatus
2. An admirable gas apparatus
3. A magnetic apparatus of rare perfection
4. For optical investigations a large, darkened room - an admirable facility remarkably close to your rooms
5. Four cupboards full of optical equipment
6. A very handsome, more than complete electrical apparatus
7. A not insignificant collection of models for mechanics
8. Two globes of 4 feet in diameter
9. A very lovely observatory with several exquisite instruments and an indescribably beautiful view
10. A chemistry laboratory in the beautiful, large botanic garden, with everything necessary provided.

(Hartmann, 1927, p. 43)

[2] As a result of the Congress of Vienna in 1815, Cologne had become part of Prussia and consequently Ohm's teaching in Cologne fell under the Prussian curriculum.
[3] A different spelling, Franz Göller, is used in the *Deutsche Biographie*.

Ohm's duties are briefly described at the end of a later note from Göllern: "You have 18 hours [to teach] and only mornings and only for the three top classes. Your title is *Oberlehrer*, not Professor; so is mine, also" (Hartmann, 1927, p. 52).[4] From these descriptions it can readily be seen that the new surroundings in Cologne were conducive to research. Ohm may not have a university position, but he did have that rare combination of time and resources at his disposal. He was also able to keep up with reading, and in 1820 he wrote to his father that he had read textbooks by Lagrange, Legendre, Laplace and Biot. The following year in another letter he mentioned that his time was taken up with going through the recently discovered electromagnetic phenomena, and intended to pursue some further (Nürmberger, 2002, p. 210; 219).

Olesko describes the availability of physics equipment in Prussian schools in the 1820s as very variable, but makes a strong argument that most schools had some limited provisions. Few schools had budgets set aside for maintaining and purchasing apparatus, particularly in provincial schools, but others were extremely well equipped. For instance, in the late 1820s the physical cabinet at Erfurt Gymnasium was so extensive that a mechanic was hired to keep the equipment in order (Olesko, 1989, p. 106). It appears that Ohm had found himself with a set of equipment in the Jesuit Gymnasium that was not only better than average, but also better than the equipment at any German university of the time (Teichmann, Eckert, & Wolff, 2002, p. 340). It is unclear whether Ohm had support with caring for the apparatus. He writes to his father about maintaining or improving parts of the apparatus himself, but in a footnote to his 1826 paper he mentions that the torsion balance in his apparatus was made by a mechanic, Herr Mauch (Nürmberger, 2002, p. 199; Ohm, 1826b, p. 145). While it is not clear that Mauch had a formal relationship with the school, he is mentioned as somebody who excels in mechanics in an 1828 guidebook to Cologne (Jacob, 1828, p. 199). It also appears that by 1829 Mauch was providing equipment for a Carmelite school in Cologne:

> Around 1829 a proper mechanic is first mentioned again. "The most necessary physical apparatus for the Carmelite College is now being manufactured by the local

[4] At this time Prussian physics teachers were often also maths teachers, and because physics was only taught in the upper gymnasium classes the teachers, like Ohm, were called *Oberlehrer.* Olesko points out that by definition they were considered socially and intellectually superior to their colleagues who taught only the lower science classes (Olesko, 1989, pp. 98-99). It seems that Ohm was progressing professionally, even if he had not yet succeeded in attaining a university position.

> mechanic Mauch under the direction of the physicist," reports the school program of this institution. Dominikus Mauch's workshop in Hämergasse existed until 1860.
>
> (Schmidt, 1917, p. 68)

This might suggest that he produced equipment for the school where Ohm taught, but it is equally plausible that he was hired privately by Ohm. With the facilities of the school laboratory and a seemingly congenial atmosphere, Ohm carried out his independent studies, and in the mid-1820s he took up his experimental electrical research in earnest.

## Cultures of electrical research

Some histories of Ohm place him between two generations of German physicists, or in some cases being ahead of his time. A relatively superficial example of this is a short book section about Ohm titled "The misfortune of having a good idea too early" (Meya & Sibum, 1987, p. 191). A more thorough case is made in Caneva's very detailed study of conceptual and generational change in German physics in the first half of the nineteenth century, which pays special attention to electrical theory. Caneva argues that Ohm fell between two identifiable generations, and further that he does not fit with the identifiable characteristics of either group (Caneva, 1975, 1978). To assess this interpretation, an understanding is first needed of the state of electrical science at the point when Ohm was first introduced to the subject and beginning his own research.

### *Contact and Chemical Theories of the Pile*

Historian Helge Kragh argues that one of the major debates in electricity in the early nineteenth century was between proponents of, on the one hand, a contact theory of electricity and, on the other, a chemical theory of electricity (Kragh, 2000). He traces the debate back to the work of Alessandro Volta at the end of the previous century. Volta's theory was that the contact between two different metals was the source of electricity. He arrived at his conclusions in response to Luigi Galvani's discovery that under certain circumstances electricity caused dissected frog legs to convulse. Volta's claims were contrary to Luigi Galvani's theory, which said that there was electricity somehow stored in the animal and that by touching animals' nerves with pieces of metal it was revealed. Volta's invention of the electric pile in 1799 robustly supported his counter-claim; his pile produced electrical effects without any animal present in the system.

Hot on the heels of the announcement of Volta's pile in London in 1800, William Nicholson and Anthony Carlisle discovered that the pile would decompose water into hydrogen and oxygen. A wave of chemical theories of the pile followed. Perhaps the highest profile early challenge to Volta came from Humphry Davy who argued that "the oxydation of the zinc in the pile, and the chemical changes connected with it are *somehow* the cause of the electrical effects it produces", although he soon changed his mind again (emphasis in original, Kragh, 2000, p. 136). Nevertheless, there were a number of people who strongly argued that the pile was a chemical machine that produced electricity as a product of chemical reactions. Kragh argues that the controversy over the action of the pile was surprisingly long-lived, placing it between 1790 and 1840, and suggesting it died away with the arrival of the theory of conservation of energy in the mid-nineteenth century even though it was never really conclusively resolved. Despite the seemingly binary nature of the debate, for the researchers involved it was far more complex than picking a side. Davy, for example, wavered between the chemical and contact theories for several years, producing a hybrid theory in 1807 and eventually moving towards what Kragh calls "contact orthodoxy" (Kragh, 2000, p. 137). Kragh uses a visual representation to give a sense of where selected researchers fitted into his contact and chemical view categorisations (Table 1). We will also see later that the works of Ohm and Hans Christian Ørsted, another notable researcher for our story, are difficult to characterise as slotting neatly into either category.

**Table 1.** Natural philosophers involved in the voltaic controversy (1792-1845) . From Kragh (2000, 139).

| **Mainly contact view** | **Mainly chemical view** |
|---|---|
| A. Volta (1745-1827) | G. Fabbroni (1782-1822) |
| J. B. Biot (1774-1862) | G. F. Parrot (1767-1852) |
| R. J. Haüy (1743-1822) | W. H. Wollaston (1767-1828) |
| M. van Marum (1750-1837) | W. Nicholson (1753-1815) |
| H. Davy (1778-1829) | W. Cruickshank (1745-1800) |
| L. W. Gilbert (1769-1848) | A. C. Becquerel (1788-1878) |
| J. J. Berzelius (1801-1874) | H. C. Ørsted (1777-1851) |
| M. H. Jacobi (1801-1874) | J. W. Ritter (1776-1810) |
| C. Matteucci (1811-1874) | M. Faraday (1791-1867) |
| G. Zamboni (1776-1847) | C. F. Schönbein (1799-1868) |
| C.H. Pfaff (1773-1852) | A. de la Rive (1801-1873) |
| G. F. Pohl (1788-1849) | W. Ritchie (?-1837) |
| S. G. Marianini (1806-1866) | C. Pouillet (1791-1868) |
| A. Bouchardat (1806-1886) | P. M. Roget (1779-1869) |
| G. T. Fechner (1801-1887) | W. R. Grove (1811-1896) |
| G. S. Ohm (1789-1854) | C. J. Karsten (1782-1853) |
| J. C. Poggendorff (1796-1877) | C. F. Mohr (1806-1879) |
| G. G. Schmidt (1768-1837) | |

By the 1820s, when Ohm became serious about his electrical research, Kragh argues that Volta's contact theory seemed to be almost universally accepted with only a few weak voices, such as that of Parrot, speaking out in favour of the chemical view. Parrot later claimed that there was "something of a propaganda campaign to spread this [Volta's] theory, of which C. H. Pfaff was the self proclaimed champion", especially in Germany (Kragh, 2000, p. 130). This claim seems to be backed up by Kragh's table, where most of the names of German researchers appear in the left-hand column. It seems sensible to assume that Ohm was familiar with German scholarship on electricity when he began his research. Both Schweigger's Journal and Poggendorff's *Annalen* published German language scholarship that Ohm would almost certainly have been exposed to as he prepared his first publications. Perhaps most notably, Pfaff and Fechner are both firmly on the left of Kragh's table, and both were supporters of Ohm's work after the publication of his book in 1827 (Winter, 1944, p. 374). Poggendorff, the publisher of Ohm's work, also settled on the contact electricity side, though like many of the names in the table he wavered between the theories at various times (Ostwald, 1980, p. 362).

One name that is missing from Kragh's table is J. S. C. Schweigger's. Ohm and Schweigger shared Erlangen as their home town and maintained a friendly correspondence, though Schweigger was ten years older than Ohm (Hartmann, 1927, pp. 63-64). Schweigger was best known for editing the *Journal für Chemie und Physik* and for independently discovering the multiplying galvanometer shortly before Poggendorff in 1820 (Horst, 2010). At the start of his scientific career, Schweigger published a handful of papers questioning the validity of Volta's contact theory, suggesting that the liquid conductor between the metal discs must do more than simply prevent metallic contact. Schweigger did not build on the research and, in his biography of Schweigger, Caneva comments that despite these papers he was not a major protagonist of the chemical theory (Caneva, 2008b, p. 254). While it could be that Schweigger simply directed his energies elsewhere later in his life, an alternative interpretation is that outside of researchers interested in the theory of the battery, the notion of a very polarised chemical versus contact debate did not dominate the discourse. We have seen several individuals who were more ambivalent about the underlying theories of the chemical pile, either switching between views or disengaging from the discussion as they pursued other things.

If, as Kragh argues, the Germans were mostly convinced by the contact theory, it seems that by the 1820s the French were more typically swayed by the chemical theory. It enjoyed something of a revival, led by its champions Antoine-César Becquerel and August de la Rive. As described in the first section of this paper, several years earlier Ohm had been reading the work of French mathematicians following the advice of Langsdorf, so it might seem fair to assume that he had equal exposure to chemical and voltaic theories. However, Becquerel had a relatively late conversion to the chemical theory, and had initially supported Volta's view. It was only in 1824 that he was persuaded that "whenever there is a chemical, thermal or mechanical action there is a development of electricity" (Kragh, 2000, p. 139). De la Rive, similarly, did not begin publishing his long series of papers on the chemical theory until 1825. Given that Ohm began serious study of French mathematical physics in around 1811, any theories of the pile he encountered were almost certainly still following the then-orthodox contact theory.

With this background, it seems clear that Ohm's scientific upbringing was largely influenced by the voltaic contact theory of electricity. This suggestion is borne out in his published

works, as two of his publications in the 1820s explicitly mention *Kontakt-Elektrizität* in the title (Ohm, 1825b, 1826a). Later in the 1830s, Ohm actively defended Volta's theory and accused some opponents of the contact theory of distorting Volta's ideas to make their own arguments sound more persuasive (Kipnis, 2001, p. 134). Unlike some of his peers, Ohm seems to have been thoroughly committed to the contact theory:

> He also did not quit his defence of the metal contact theory later, as an elaborate addendum of 1847 to the Italian translation of his 1827 theory (*Die galvanische Kette mattematisch bearbeitet*) testifies. It is not clear whether Volta's arguments and experiments led him to this from the start, or Ohm's position between mathematics and physics, but in any case he stayed away from the chemical theory of the "galvanic" electricity generation.
>
> (Teichmann, 1976, 5)

If the controversy between contact and chemical theory had been dominating electrical science, then Ohm's openly contact theory-based stance could have prejudiced his readers against his work. For Ohm's work to be accepted it had to fit with whichever theory its reader was most convinced by. However, the divisions between contact theorists and chemical theorists may not have been so stark or insurmountable for natural philosophers in the 1820s, and followers of the chemical theory found Ohm's law acceptable.

*Concretizing science and Abstracting science*

Kenneth Caneva takes a different approach to understanding the range of intellectual traditions in studies of electricity. Presenting his work through a Kuhnian lens, he argues that an older generation of German physicists was working within a paradigm of "concretizing science" which favoured decisive yet non-quantitative experiments as a means to resolve disputes. In contrast, a younger generation of German physicists worked within a new, competing paradigm of "abstracting science" which favoured the use of mathematical tools and careful measurement and quantification. This, he claims, was one of the most significant features of the long controversy between the chemical and contact theories of the pile (Caneva, 1978, p. 69). Caneva identified eighteen German physicists and placed them in the two groups (Table 2).

**Table 2.** Timeline of German physicists considered by Caneva (Caneva 1978, 65)

| | |
|---|---|
| | Erman, Paul (1764 – 1851) |
| | Schmidt, Georg Gottlieb (1768-1837) |
| 1770 | Seebeck, Thomas Johann (1770-1831) |
| | Muncke, Georg Wilhelm (1772 – 1847)<br>Pfaff, Christoph Heinrich (1773-1852)<br>Jäger, Carl Christoph Friedrich (1773-1828) |
| | Ritter, Johann Wilhelm (1776-1810)<br>Gauss, Carl Friedrich (1777-1855) |
| 1780 | Schweigger, Johann Salomo Christoph (1799-1857) |
| | Pohl, Georg Friedrich (1788-1849)<br>Ohm, Georg Simon (1789-1854) |
| 1790 | |
| | Poggendorff, Johann Christian (1796-1877) |
| | Neumann, Franz Ernst (1798-1895) |
| 1800 | |
| | Fechner, Gustav Theodor (1801-1887)<br>Jacobi, Moritz Hermann (1801-1874) |
| | Lenz, Heinrich Friedrich Emil (1804-1865)<br>Weber, Wilhelm Eduard (1804-1891) |
| 1800 | Grassman, Herman Günther (1809-1877) |

For Caneva, the older "concretizing" physicists "exhibited a bias toward experiment and a reserve toward theory. Indeed some even harboured the ideal of pure, atheoretical experiment, of facts without any theoretical interpretation… according to which model experiments were done, often without much direction or clear expectation, and the results obtained then commented upon" (Caneva, 1975, p. 23). In contrast the "abstract" physicists of the younger generation were different in four fundamental areas: "(1) the use of mathematics and the concomitant increase in the abstractness of theory; (2) the new precision in experiment and measurement; (3) the general adoption of the hypothetico-deductive method for the verification of theories; and (4) the desire to bring together previously separate realms of phenomena under one unifying explanation" (Caneva, 1975, p. 239). Caneva still recognised the debate between contact theorists and chemical theorists, and implied that until quantifying science arrived in the form of the "abstracting" physicists, the debate between contact and chemical electricity could not be settled. This is in contrast to Kragh's argument that the debate was never truly settled, though it did die down in the 1850s, sometime after Caneva's second generation began to have influence.

Caneva specifically names two individuals who were working in German electrical research in this period, but that sit outside of this pair of paradigms: Georg Simon Ohm and Georg Friedrich Pohl. Ohm, Caneva argues, does not fit because he was doing "abstracting science" before a paradigm had developed to accommodate it. His use of quantification was undoubtedly unusual outside of Paris, and his work was also somewhat unusual in seeking an overarching theory of the electric circuit. Following a similar line of reasoning, Steinle describes the geographical spread of electrical research at the time, suggesting that while work on the voltaic pile was conducted throughout Europe, quantitative efforts were virtually all taking place in Paris with very few people involved in both (Steinle, 2016, pp. 18-19). Once again, Ohm seems to defy historians' categories. The other researcher sitting outside of Caneva's two paradigms was Pohl who, for our story, is notable as the harshest and most famous critic of Ohm's work. Some historians have suggested that Ohm's 1827 book was ignored largely because of the predominantly Hegelian view of physicists working at the time (for example Winter, 1944). This interpretation is a result of Pohl's highly critical review of *The Galvanic Circuit,* given context here by Jungnickel and McCormmach:

> Pohl belonged to an association formed "especially under Hegel's auspices," which published a journal for scientific criticism. As a reviewer of works on physics and

> chemistry for this journal, Pohl followed the policy - which was apparently a general policy for the association - to make the work reviewed the starting point for an essay on the reviewer's own theories and general views. The reviewer was to judge scientific works from a "higher point of view," such as a "simple and dignified view of nature." This policy Pohl applied to Ohm's *Galvanic Circuit.*
>
> (Jungnickel and McCormmach 1986a, p. 56)

Pohl was unusual in his stance at the time, and not taken very seriously by influential figures such as Poggendorff and Schweigger. Indeed, Schweigger encouraged Ohm to simply ignore Pohl's comments, telling him that the book where they were published had a small audience and Ohm would simply be drawing attention to them if he responded (Hartmann, 1927, p. 90). Given Schweigger's well-informed view, it seems unlikely that Pohl's commentary could have impacted so negatively on the reception of Ohm's book as some historians have claimed. Caneva, discussing the tradition of *Naturphilosophie* followed by Pohl, argues that "in no way did it dominate the physics community, and that, to the extent that there was a 'reaction' against it, the men who reacted were the members of the older, contemporary generation, not of the younger generation, who rather drew inspiration from its unificatory approach to understanding" (Caneva 1975, pp. 28-29). Instead, his interpretation of Ohm's work is that

> Ohm was a "premature" representative of a wholly new style of physics which would become dominant in Germany in the 1830's and 1840's. He had the misfortune of falling between two generations and of therefore being judged at first by men who represented a differently directed physics.
>
> (Caneva, 1975, p. 112)

Continuing the theme of Ohm being out of step with his generation, Olesko has also expressed the idea that Ohm's work could not be adopted by many physicists until analytical mathematics was better trusted, saying the acceptance "relied on much more than the dissemination and acceptance of the mathematical methods used in them. It also relied on developing the means for judging the accuracy of those theories through a rigorous quantitative assessment of their empirical foundations" (Olesko, 1991, p. 12). In contrast to Caneva's description of Ohm getting caught up in the middle of a Kuhnian paradigm shift, Olesko's focus is more on the gradual acceptance of mathematics as accurate, and the work that was needed to convince the community of German physicists. Jungnickel and

McCormmach also see the shift as more incremental, though all parties seem to agree that Ohm's use of abstract mathematics came before his audience in Germany was receptive to it. It is also clear that the supposed Hegelian influence was limited almost exclusively to Pohl, who was in himself something of an outsider, albeit a vocal one.

Pulling these arguments together, we can see that a new discipline of physics, or at least a different approach to physics, was emerging in Germany in the 1830s and 40s. The phrase "mathematical physics" begins to describe work like that carried out by Ohm. For instance, Jungnickel and McCormmach suggest that "when the *Galvanic Circuit* appeared, few physicists in Germany knew mathematical physics sufficiently to understand it" (Jungnickel & McCormmach, 1986b, p. 55). Olesko, too, refers in the same sentence to "Ohm's pathbreaking mathematical theory of the galvanic circuit" and "French theories in mathematical physics", drawing very firm links between them (Olesko, 1991, p. 12). Whether, as Caneva suggests, there was a clear distinction between different paradigms and generations of researchers in this period is less clear. Much electrical theory was being negotiated and rapidly changing, and electrical researchers held a wide range of opinions about both theory choice and research methods. All this scientific confusion was set against the backdrop of huge social change brought about by the Napoleonic wars causing enormous upheaval in German schools and universities.

Perhaps one final contribution to an understanding of the culture of research that Ohm was working in is Jungnickel and McCormmach's observation that in Berlin in the late 1820s the number of positions in the university to teach physical and experimental sciences grew quickly (Jungnickel & McCormmach, 1986b, p. 7). The growth in staff at universities, including Berlin, enabled researchers and teachers to begin to specialise more than previously. Before this period there was no specific training for a physicist. Schweigger's doctoral thesis was about Homer's *Iliad* (Horst, 2010), Fechner trained as a physician although he never practised (Jaynes, 2008), and Pfaff, while an exception in that he produced his doctoral work on Galvani and Volta's discoveries, had first studied under renowned naturalist Georges Cuvier (Karsten, 1887). Teaching, too, reflected the diversity of subjects that students studied. Pfaff commented on his own teaching experience that "in physics, especially in the first years, I had listeners from all faculties… It was necessary to avoid a truly penetrating or, as it were, scientific presentation" (Caneva, 1978, p. 134). Claims that Ohm's work was too mathematical to understand have some traction in this context. Caneva also notes that the popular journals were private ventures, supported by subscriptions. If they

became too technical for a wide readership the appeal would be diminished, and in turn the number of readers would drop. So, for as long as the pool of readers with the interest and ability to read heavily mathematical papers was small, there was little incentive for editors to include anything overly technical (Caneva, 1978, p. 141). With the reform of schools and universities, mathematical training improved, starting with school teachers like Ohm. In many ways, Ohm was helping to pave the way for the next generation of researchers who had an interest and the skills needed to participate in mathematical physics.

**Ohm's research, 1825-1827**

This section provides a broad overview of the work Ohm undertook in the development of his now-famous electrical law, particularly in the period 1825-1827. As the Introduction describes, this work can be understood to take place over three distinct phases, with Ohm's conceptual language and framework changing in each phase from an electrostatic influenced model of "loss of force" in Phase 1 (Ohm, 1825a), through an electrodynamic understanding of an "exciting force" in Phase 2 (Ohm, 1826a), and finally onto the language of "electroscopic force" which he used in his mathematical work in Phase 3 (Ohm, 1827).

Having considered the wider research context Ohm was working in, another approach is to think about the particular skills and motivations he had when he embarked on his electrical research. Teichmann suggests four particular traits that Ohm held:

1. his strong mathematical inclination, that initially seemed to lead to a career as a mathematician;
2. the, for its time, excellent fixture of the physical cabinet of the Jesuit Gymnasium in Cologne with equipment and experimental possibilities;
3. his experimental skills;
4. his advocacy for Volta's metal contact theory for the explanation of "galvanic" electricity generation.

(Teichmann, 1976, p. 5)

Of these, Teichmann argues that point three, Ohm's experimental skill, was the most important factor in his discoveries, followed by his mathematical leanings. Indeed, except for point four, all of these traits were necessary for Ohm's work, together with an interest in the quantitative side of physics, which was in no way to be expected at the time. Ohm's correspondence reveals his technical skills. For instance, Ohm realised that the Cologne

cabinet, good though it was, was unable to meet his needs, so he set about equipping a workshop. He often wrote to his father and brother about his latest technical works. For example, in May 1818 he discussed acquiring a lathe and accessories to work with; in April 1822 he described making two balances, including sketches of the forged pieces; and in February 1823 he wrote that he was working on a "circular dividing machine with the mechanic Mauch", by which he presumably means the micrometer for his torsion balance (Nürmberger, 2002, pp. 198, 222-223, 228). His letters suggest attention to detail and a good understanding of the precision of his instruments in comparison with the finest instruments being made elsewhere. Teichmann also agrees with this assessment, and goes further still in his conclusion to his *Festschrift* where he asserts that Ohm's mathematical leanings were, in likelihood, not responsible for the discovery of his law, but rather that it was founded in experiment. As later chapters will show, it seems likely that when Ohm initially began his first phase of research he had an expected mathematical outcome in mind, in the form of a logarithmic equation. However, as he shifted into his second phase of research, and his experimental setup became more stable, it became clear that his logarithmic law was not corroborated. From then on it seems that his experimental findings then led the way to the development of what we now think of as Ohm's law.

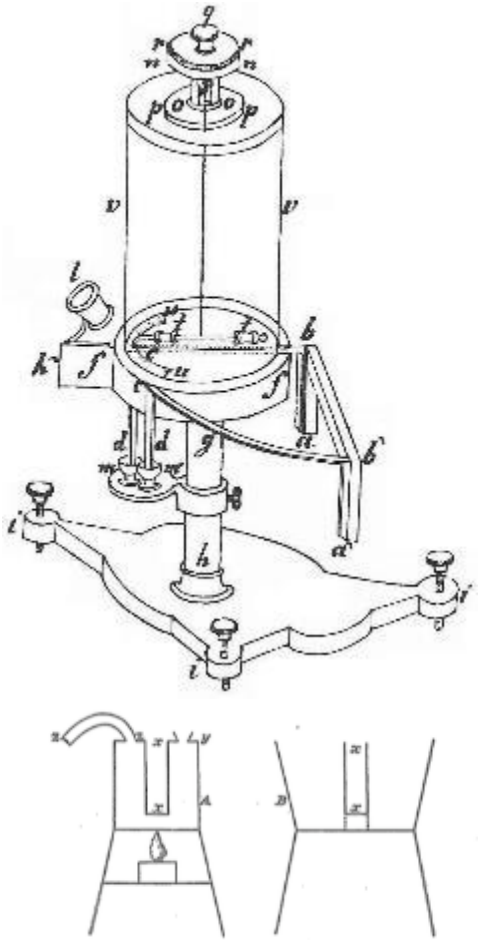


**Figure 3.** Diagrams showing Ohm's experimental apparatus (from plate accompanying Ohm, 1826a).

Ohm's ability to draw conclusions from his experiment was largely reliant on changes to his experimental setup. In his first phase of research, where he appears to have been predisposed to adopting a logarithmic law, his apparatus included a voltaic pile which made the experiment extremely unstable and hard to use. In contrast, his second phase of work is characterised by an instrument that is working well and producing reliable and repeatable results. The instrument is shown in Figure 3, and the following Chapters 3, 4 and 5 offer a dissected view of the instrument, breaking it down into three components: the conducting wires in Chapter 3 (see *mm'* in Figure 3), the thermocouple in Chapter 4 (see *abb'a* in Figure 3), and the torsion balance in Chapter 5 (see *n* through *u* alphabetically in Figure 3).

In modern terms, we might interpret the first area described above as relating to current, the second area as relating to voltage, and the third and final area as relating to resistance. However, in doing so we use modern concepts in a way that is anachronistic and would have been alien to Ohm and his peers. It would be several decades before all three of those terms

began to resemble our modern understandings, and, although Ohm did refer to *Strömung* (current), *Widerstand* (resistance) and *Spannung* (tension, also now translated as voltage) at points in his writings, the meanings associated with the words were still very much under negotiation. Most curiously, although Ohm used the word "resistance" in his 1826 "Announcement of laws…" paper, he did not use it at all in the book that he published the following year, choosing instead to refer to "reduced length". Ohm's use of this term is discussed in Chapters 3 and 6.

The response to Ohm's 1826 paper, where what we recognise as Ohm's law was first announced, was muted. From September of that year, Ohm spent a year away from teaching to work his findings into a mathematical theory. After his sabbatical period, he published his 1827 book, *The Galvanic Circuit Investigated Mathematically*. The book barely referenced his experimental work, and differed significantly from his earlier publications by presenting lengthy mathematical derivations at the heart of his argument. Like his earlier publications, it did not receive a great deal of attention, despite Ohm's efforts to drum up support and interest. Jungnickel and McCormmach argue that part of the reason Ohm struggled to find a readership was that it was unclear whether the work was intended for mathematicians or physicists, and did not quite work for either group. As described above, their discussion leads to the conclusion that Ohm was becoming something new, a "mathematical physicist". They also suggest that, contrary to claims that Ohm's work was too mathematical to be understood by most, there were enough people in Germany able to understand the mathematical derivations Ohm was presenting (Jungnickel & McCormmach, 1986b, pp. 54-56). However, the published responses to Ohm's work at the time do not present a picture of a German peer group that could easily follow his arguments, but rather a few key supporters helped him in his attempts to raise his profile. This, together with a wider discussion of the reception of Ohm's work is more thoroughly covered in Chapter 6.

One question that remains unanswered by this thesis is who Ohm thought his audience was as he wrote his 1827 book. Throughout, he very clearly made use of advanced mathematical tools that were really only being used in Paris at the time, and even there only by a relatively small group of mathematically adept physicists who were pioneering new techniques, yet he wrote in German and put a lot of effort into promoting his work to German peers. The initial passage of the book also refrained from heavy maths, making a version of his argument using geometrical analysis. The argument was then recapitulated using more advanced mathematics. The book was circulated for review within the German states, and Ohm sent

free copies to many people he considered influential, including, it should be noted, the Academy of Science in Paris (Atherton, 1986, p. 471).

Other historians have put forward alternative arguments for the apparent lack of interest in Ohm's work. Schagrin suggests that Ohm's work was partly dismissed because he was still conflating old ideas about static electricity with new models of understanding current electricity (Schagrin, 1962, p. 545). Jungnickel and McCormmach argue that on the contrary Ohm was supported by Schweigger and Poggendorff, but highlight that the most famous review came from Georg Friedrich Pohl and was negative (Jungnickel & McCormmach, 1986b, p. 56). Though as we saw earlier, Pohl was not taken all that seriously by some of Ohm's contemporaries. Meanwhile, as described above, Caneva suggests that although Ohm's work was published, it was not enthusiastically embraced by an older generation of physicists, while the later more sympathetic generation was yet to come:

> Among the representatives of concretizing science born between 1764 and 1779 there was none who embraced Ohm's work wholeheartedly, although to be sure there was also none who rejected it quite so vehemently as Pohl. The recognition he received from Schweigger and Poggendorff was restrained, to say the least, and Pfaff ignored him in his several expositions of electricity and galvanism until 1837.
>
> (Caneva 1975, p. 114)

Ohm's book was in print for eight years, and then out of print for a further 60 years. During that time Ohm's work did slowly gain recognition. Atherton reports that in 1833 Joseph Henry had heard of the law, but not what it stated, and that he did not find an answer until he visited London in 1837 (Atherton, 1986, p. 471). Despite Ohm's efforts, his coveted university position was not immediately forthcoming. On completion of his sabbatical year on half-pay, Ohm returned to teaching and continued to seek employment in an environment that would support his research with equipment and time. He also carried out research into acoustics and crystal optics. Jungnickel and McCormmach suggest that for many of Ohm's contemporaries, experimental confirmation was more meaningful than mathematical derivation. Meanwhile, in France, Pouillet was independently arriving at the same law without any knowledge of Ohm's experimental work. Curiously this might have been the spur for Ohm's reversal of fortunes. In 1841, the same year that Pouillet's work appeared, an English translation of Ohm's 1827 book was published. Teichmann, on the other hand, emphasises the urgent necessity of Ohm's law for the telegraphy industry, which by the early

1840s was rapidly developing (Teichmann, 2001, p. 75). Whatever motivated the interest in Ohm's work, 1841 was the year it was recognised by the scientific establishment with the award of the Royal Society's highly prized Copley Medal. This was a pivotal moment in Ohm's career, which eventually saw him attain the Professorship he had sought.

**Commemorating Ohm**

Anybody who works with electricity knows Ohm's name, principally because it is used as the unit of measurement for electrical resistance. However, relatively few people know much about Ohm the person. This section describes places where Ohm's life and work are commemorated, predominantly in Germany as one might expect. In Ohm's home town of Erlangen, Georg Simon Ohm and his brother Martin are remembered through the naming of a public square, "Ohm Platz", which sits adjacent to the Ohm Gymnasium. Created in 1937, according to a plaque at the site, the square is located at the head of an oak-lined avenue into the city. There is also a plaque marking the house on Fahrstrasse where the Ohm family lived. Perhaps surprisingly, then, the town museum (Stadtmuseum) does not so much as mention Ohm in its display texts, despite having comprehensive displays of the arrival of electrical power in the town, and displays of a range of twentieth century electrical measurement devices that were made locally (Erlangen Ohm Platz and Stadtmuseum, visited 15 August 2015). Ohm has not been forgotten entirely however, for instance one picture book aimed at tourists describes Ohm as "one of Erlangen's most important sons", while another dedicates four pages to Ohm, (Fuchs, 2005, p. 88; Wilkes & Wilkes, 2014, pp. 72-75). There is little public-facing remembrance to be found in nearby Nuremberg, which is home to the "Technische Hochschüle Nürnberg Georg Simon Ohm". Ohm taught at the school later in his career before moving to Munich. On the school's website Ohm is fondly remembered as something of an eccentric, as well as an important figure in the history of physics (Technische Hochschüle Nürnberg Georg Simon Ohm, accessed 17 July 2021). The Nuremberg city museum, Stadtmuseum Fembohaus, has nothing specific to Ohm on display, but, as mentioned earlier, does provide a small display about traditional trades in the region which included locksmithing and wire drawing, both of which are relevant to Ohm's story (visited 14 August 2015).

In the 1825-1827 period this project is concerned with, Ohm was living in Cologne, and traces of him and his work can be found in the city. Despite the widespread destruction

Cologne suffered during the Second World War, the school where Ohm taught still stands, located on Marzellenstraße in the city's historic centre. There is an engraving of Ohm on a plaque on the school's outer wall, along with several other figures from the school's history. Ohm is also celebrated in Cologne's local history books (for example, see Imgrund, 2014, p. 162). Most prominently, Ohm's statue appears on the north face of the tower of the town hall, immortalising him as an icon of the city alongside some 130 others. As with the majority of the other sculptures, Ohm's statue was installed after the tower was reconstructed in 1975 following heavy damage in the Second World War. In his statue, Ohm can be seen holding an instrument that appears to be a wire coil, perhaps modelled on the galvanometer held in the Deutsches Museum (Deutsches Museum, inventory number 2159; Cologne town hall and Ohm's former school visited on 7 September 2015).



**Figure 4.** Statue depicting Ohm on Cologne Town Hall (This image is released under a Creative Commons Attribution-ShareAlike 4.0 Licence, © R. Spekking, retrieved from https://commons.wikimedia.org/wiki/File:Rathausturm_K%C3%B6ln_-_Georg_Simon_Ohm-4865.jpg on 3 January 2021.)

The final place where public commemorations of Ohm were sought was Munich, Ohm's final resting place and where he eventually ended up working as a full Professor. His gravestone lies in the Alter Südliche Friedhof, where many of Munich's most famous names now rest.

Other indications of Ohm's presence in the city include a road named Ohmstraße, which lies a short walk to the north of the main historic buildings of Ludwig-Maximilian Universität (LMU), and a large statue of Ohm which can be found outside the nearby Technische Universität München (Figure 5). The statue is marked with the Omega symbol, Ohm's name and dates. The statue of Ohm himself sits on a chair, and, perhaps oddly for an experimenter, under the chair is a heap of open books. Teichmann et al. take this as an indication that Ohm continued to be better known for his theoretical work, at least by those commissioning and designing the statue (Teichmann et al., 2002).



**Figure 5.** Statue of Ohm outside the Technische Universität München (visited August 2015).

Until it closed for renovation in Autumn 2015, the other main source of Ohm material was the physics gallery of the Deutsches Museum. Before its closure there were two prominent displays dedicated to Ohm's electrical apparatus. The first showed a very large electrical friction machine with a large battery of Leyden jars. According to the gallery label, this apparatus was built and used by Ohm in 1840, by which time he was working as a teacher in Nuremberg (inventory numbers 3919; 3920; see Figure 6). There is evidence that this object

was on display in the museum as far back as 1942, where it is referred to in the museum's guidebook (Conzelmann, 1942, p. 133). The second display presented several objects alongside a small amount of biographical information. The objects on display were described in the labels as follows:

- Dry battery (*Trockenbatterie*), inventory number 2164;
- Copper-Zinc element with handle for immersion in an electrolyte (*Tauchelement*), inventory number 2156;
- Replica of Ohm's experimental apparatus as described in his 1826 paper, inventory number 1976/9/9;
- Needle galvanometer (*Nadelgalvanometer*); inventory number 2159;
- Two magnetic coils (*zwei Magnetspulen*) inventory numbers 2163 and 2155.



**Figure 6.** Friction electricity machine built and used by Ohm. Deutsches Museum inventory numbers 3919 and 3920 (visited August 2015).

The optics displays in the museum also contained a piece of polarising apparatus, and there were further object and archive collections held in the care of the museum and its library and archive. Another notable mention of Ohm within the museum was in a display about the Bavarian Academy of Science. Ohm was the Academy's second curator of instruments, and those instruments went on to form the basis of the Deutsches Museum's collection when it was first established. This relationship also explains why Ohm's own material was acquired

very early in the museum's history. The final place where Ohm could be found in the museum was in the "hall of fame", which featured busts of famous scientists and technologists (Deutsches Museum, visited in August 2015). The "Replica of Ohm's experimental apparatus" is discussed in more detail in Part B of this chapter.

## Conclusions

The story typically told about Ohm is one of an underdog who tirelessly works for something he believes in, before eventually winning his long sought-after prize which, in this case, means recognition and a university chair. This introduction seeks to dispel the rags-to-riches motif that often accompanies accounts of Ohm's life, and instead show that he had access to a surprising breadth of intellectual and material resources. As we will see as we progress through more detailed studies of the component parts of Ohm's experiment, almost every aspect of his work required judgement about theories that were being actively negotiated by the leading thinkers of his time. Ohm drew on a wide variety of influences, which enabled him to make novel claims. This also left him without a clear intellectual home, especially compared with natural philosophers based in major scholarly centres like Paris or Berlin, giving the sense that he was something of an outsider among his contemporaries. The various depictions of Ohm as either a contact theorist or a researcher who fell between paradigms will be revisited and re-evaluated in later chapters. One thing is clear, however, which is that Ohm's work defies easy categorisation. This goes some way to account for the slow recognition of his work, and the occasionally unenthusiastic remembrance of Ohm in his home country.

## PART B

This and the following "Part B" chapter sections present and analyse the work undertaken in the process of reinterpreting Ohm's apparatus and restaging his experiment. In this chapter, I present a comparative survey of four other efforts to reproduce Ohm's 1826 apparatus. The different instruments are referred to throughout by the name of the organisation that now cares for them, which in most cases was also the commissioning organisation:

- Science Museum apparatus (1931)
- Deutsches Museum apparatus (1976)
- Oldenburg University apparatus (c. 1990s)
- University of Flensburg apparatus (2021)

This is not a comprehensive list of every attempt to rework Ohm's apparatus. For instance, another example is discussed by Manfred Achilles, whose book describes a number of historical experiments "functionally reconstructed" for exhibition in the buildings of the Technical University of Berlin in 1987 (Achilles, 1996). Informal conversations also suggested that there was a further instrument at Münster University, though I was unable to corroborate this independently. Each of the reproductions was made at different times, by different people, with a variety of goals and motivations. Some of those motivations can be "read" in the objects themselves as a "material historiography", to borrow Sam Alberti's phrase, while others can be found in texts associated with the objects (Alberti, 2018). Each object embodies a particular historiographical approach, as, inevitably, does my own "Connelly apparatus".

As noted in the Introduction (p. 16), I have found myself using the language of *reinterpreting* Ohm's apparatus, rather than replicating, and *reworking* and *restaging*, rather than copying. The distinctions in this use of language are subtle, and have not been used consistently by museums or historians. For example, the Science Museum apparatus is explicitly described as a "replica" in the museum catalogue, though of all the examples discussed here it is the interpretation that is least replica-like, according to my use of the word. It does not closely follow Ohm's descriptions, and is unlikely to have ever been intended to be usable.

**Science Museum apparatus**



**Figure 7.** Science Museum "replica of Ohm's experimental apparatus", inventory number 1931-1122. (This image is released under a Creative Commons Attribution-NonCommercial-ShareAlike 4.0 Licence, © The Board of Trustees of the Science Museum.)

The earliest reproduction of Ohm's apparatus that I was able to identify is cared for by the Science Museum in London. It was initially manufactured for inclusion in the Faraday Centenary Exhibition of 1931, held at the Royal Albert Hall, and was just one among many reproductions of notable experiments. It entered the museum collection in 1932, where it is now known as inventory number "1931-1122, 'Replica of apparatus used by Ohm to prove Ohm's Law, 1826'" (Figure 7).

On 2 April 2015, I inspected the object at the Science Museum's collections storage facility (Figure 8). This was the first of the instruments I was able to view in person, and at the time I kept a diary to record my impressions. The purpose of my visit was noted as being an opportunity to assess whether it might be possible to use the object to rework Ohm's experiments, provided all necessary conservation conditions could be met. This project was

always conceived as one that required some form of experimental reconstruction, however, at the time of my visit it was still unclear how best that should be undertaken. Using existing apparatus was under consideration as a potential way forward.



**Figure 8.** Science Museum apparatus as stored at Blythe House , April 2015

The object was missing its glass cylinder, though it was located shortly afterwards by curator Alison Boyle. I also noted that "the thin metal strip [torsion wire] that the needle of the torsion balance hangs from has become crumpled under the heavy brass dial [micrometer] in storage." As indicated in the square brackets, the terminology I now use to describe the different parts of the instrument only gradually became settled, and was much more descriptive in early observations.

Reading my diary back, my observations of the instrument read like a condition assessment, by which I mean a standard type of assessment made by curators, conservators or other collections professionals to record the physical condition of an object. For example, I noted the thermocouple twisting under its own weight (Figure 9), which I then remedied by placing the hot and cold vessels in place for support. I also reflected on the condition and

functionality of the pieces of the micrometer, noting that “there is very little friction between the pieces which means they can be very easily turned” (Figure 10; Connelly, reflective diary, 2 April 2015).



**Figure 9.** Science Museum apparatus, showing the thermocouple twisting under its own weight

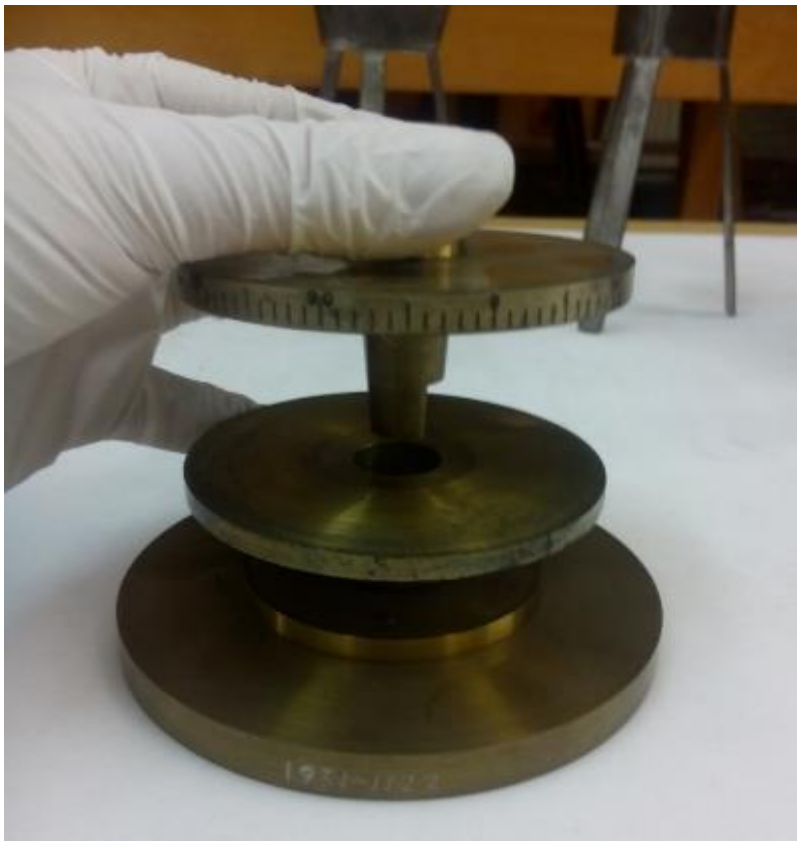


**Figure 10.** Micrometer from Science Museum apparatus.

Alongside my commentary on the condition of the instrument were more basic descriptions that enabled me to draw conclusions about the instrument's use. For instance, "Unlike the gold ribbon described by Ohm in his report, this metal ribbon [torsion wire] was silver in colour." I speculated about the metals used, questioning whether bismuth was used in the reconstructed thermocouple, given that the metal did not appear to have tarnished with "the characteristic pink tinge that bismuth acquires when it oxidises"; and I noted that "the surface of the tips of the copper pieces where they rest in the egg cups appear to have been painted with a dark colour" and speculated that they might have been painted to give an impression of where they rested in mercury (Figure 11). With hindsight, and a better understanding of Ohm's apparatus, it may also be the case that this was a misinterpretation of Ohm's description of dipping the tips of his copper conductors in resin. My understanding of the apparatus grew and developed as my own interpretations of Ohm's writings emerged in the process of reconstructing the instrument, however at this early viewing my understanding was still relatively superficial.



**Figure 11.** Mercury cups in Science Museum apparatus. The bottoms of the two copper pieces placed within the cups are roughly painted in a dark colour. Penny included for scale.

Another point to note is that I repeatedly referred to the mercury cups as the "egg cups" after this visit, which shaped my thinking and influenced some of my research into their detailed design later on in the process. I was surprisingly far into the process of searching for similar vessels when it occurred to me that Ohm never described them as egg cups, and that it was an interpretation I had introduced myself. After that realisation, I avoided using the phrase as it was leading me and the instrument maker to imagine a particular size and design for the cups that is not supported by Ohm's writings or diagrams (Appendix, p. 269).

There were plenty of clues within the object that it was never intended for use. The most obvious example is that the hot and cold vessels both have a large label stuck to them that reads "reduced scale", demonstrating that the apparatus was knowingly not identical to Ohm's description (Figure 12). It also seems unlikely that the burner underneath the hot vessel would have been open to the air as in this example, suggesting that the Science Museum interpretation was principally concerned with looking like the drawing, rather than giving full consideration to the functioning of the instrument.

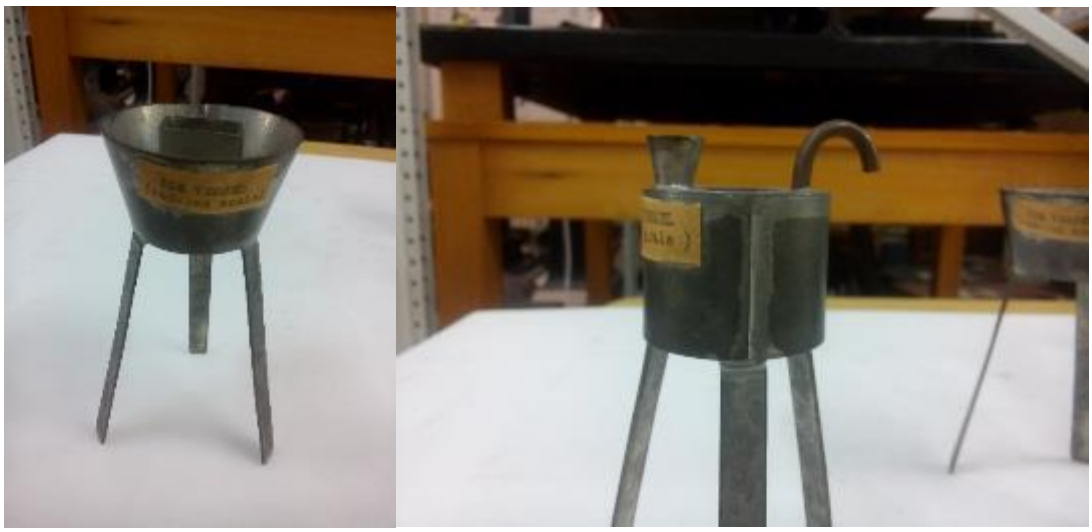


**Figure 12.** Ice vessel (left) and steam vessel (right) from the Science Museum apparatus. Label reads "reduced scale".

It is clear from reading my diary that looking at the instrument, as opposed to reading Ohm's descriptions, prompted new questions. I noted that the dial was marked up with 100 divisions, as Ohm described, but queried whether that was a fine enough gradation to achieve the results Ohm reported, or whether some sort of Vernier scale might have been used (discussed further on p. 247). It also led me to consider the role of the lower scale, given that the upper scale

was the one used for recording observations (role of the lower scale discussed further in Chapters 5, 6 and Appendix, p. 185; Figure 13).



**Figure 13.** Lower scale of Science Museum apparatus

Close looking at the instrument, and imagining myself in the role of an experimenter using the apparatus, directed my attention to challenges that were not immediately apparent from the text. For instance, I noted that "I was surprised at the thickness of the white caps on the ends of the needle. The judgment as to whether the needle had returned to the 0 line would need to be quite precise, but I think this would be difficult without the needle coming to a fine point at least at one end" (Connelly, reflective diary, 2 April 2015; Figure 14). Returning to the text to follow up on this point reassured me that Ohm's description called for "a delicately pointed, slightly bent, brass wire… which serves as a pointer" (Ohm, 1826a, p. 145). My understanding of the instrument grew iteratively as I travelled back and forth between material evidence and Ohm's text.

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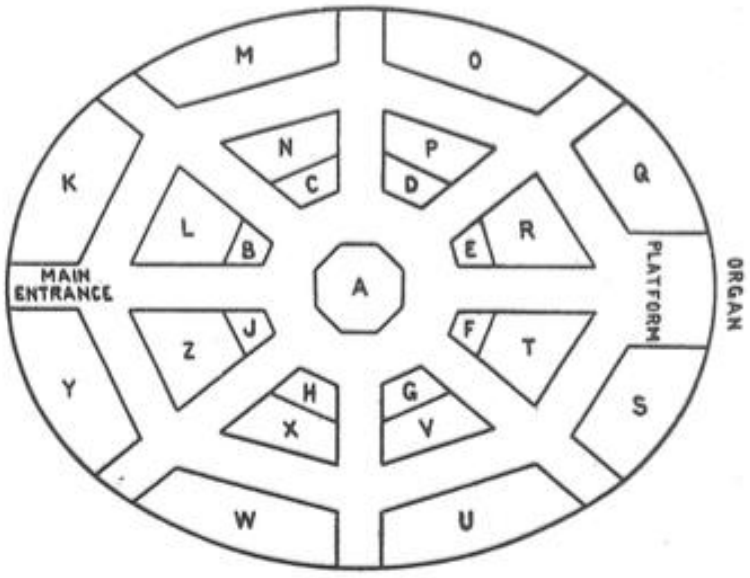
**Figure 14.** Indicator needle from Science Museum apparatus.

My thorough assessment of the Science Museum apparatus, as recorded in my reflective diary, presents the physical object. What was not recorded at the time was the cognitive shift I underwent in viewing the object. In Chapter 2 I will introduce the work of Rachel Prentice who describes how, for her, in the course of her observing several patients undergoing surgery, arthritis went from being a linguistic object to a material one. In hindsight, something similar happened to me in observing the Science Museum apparatus early in the project. I recall suddenly having a sense that the instrument was far more comprehensible. Put simply, I understood it better having seen it interpreted as a three-dimensional object, rather than described in prose and drawn on a page. Like Prentice's understanding of arthritis, my understanding of Ohm's apparatus had shifted it from a conceptual object to a material one. Later on, when working with an instrument maker to produce my own reproduction of Ohm's apparatus, it became obvious that this early exposure to a particular interpretation of Ohm's apparatus had influenced my own thinking. Having already seen a "solution" to how Ohm's description might be manifested in material form, it became quite challenging to conceive of different solutions unless the instrument maker presented an alternative (for example see Appendix p. 264).

In addition to what can be learnt from observing the instrument itself, there is also rich information in the paper files accompanying the object. From here we learn that the object was one of several made by members of The British Electrical and Allied Manufacturers' Association for display in the Faraday Centenary Exhibition. The exhibition was held at the Royal Albert Hall, London from 23 September to 3 October 1931, and was visited by around

50,000 people (Paul, 1931, p. 337). The reconstructed Ohm apparatus was displayed in section L, “Electrical Measurement and Transformers” (Figure 15) and described in the catalogue as

> L184. Replica of the apparatus used by Ohm in proving that the resistance of a conductor may be measured by dividing the difference of potential at its extremities by the amount of current passing. (Ohm’s Law), Ohm, 1826.
>
> (Vignoles, 1931, pp. 77-78)




PLAN AND LIST OF STANDS

| STAND | STAND |
|---|---|
| A. Michael Faraday. | M. Transmission and Distribution. |
| B. Electrical Science before 1831. | N. The Grid Transmission System. |
| C. Faraday’s Discoveries in 1831. | O. Electric Lamp Manufacture. |
| D. Effects of Induced Currents. | P. Electricity in Transport. |
| E. Faraday’s Researches in Magnetism. | Q. Electricity in the Home. |
| F. Faraday’s Researches in Chemistry. | R. Electric heat in Industry. |
| G. Faraday’s Researches in Electro-Chemistry. | S. Chemistry. |
| H. Faraday’s Researches in Electro-Statics. | T. Chemistry. |
| J. The Electric Discharge. | U. Electro-Metallurgy. |
| K. Generation of Electricity. | V. Electro-Chemistry. |
| L. Electrical Measurement and Transformers. | W. Broadcasting Transmission. |
| | X. Telegraphy. |
| | Y. Telephony, Radio, and Radiology. |
| | Z. Telephony and Radio. |

**Figure 15.** List of stands at the Faraday Centenary Exhibition (Vignoles, 1931, p. 3)

The description is highly anachronistic, imposing 1930s language onto Ohm's apparatus, offering another indication of the historiographical context it was produced for. The particular copy of the exhibition catalogue I referred to was used by William O'Dea (indicated by his name written in his hand on the cover), who at that time was a Science Museum curator of engineering. The electrical measurement section of the catalogue is marked up in pencil in a way that appears to indicate whether O'Dea thought that various objects should be kept for the collection. If this interpretation is correct, O'Dea was initially not persuaded that the Ohm reproduction was suitable for acquisition, because the entry for the Ohm apparatus has a cross next to it. However, on 13 October 1931, Sir Henry Lyons, then director of the museum, wrote to the British Electrical and Allied Manufacturers' Association to ask whether they would be willing to transfer a number of objects to the Science Museum, including the Ohm apparatus. The final list of donated objects is given in an acquisition form dated 13 January 1932:

Model of Electrostatic Balance, Snow Harris, 1834.
Electromagnet with 450 turns, Henry 1828. (Replica).
Working model of special spring used in Ayrton and Perry Solenoid type Ammeter.
Replica of Oersted's apparatus illustrating movement of current-carrying Conductor in a magnetic field, Oersted, 1820.
Replica of Davy's apparatus showing rotation of mercury carrying a radial current in the presence of a magnet (demonstration). Davy, 1823.
Suspended Loop, embodying a thermojunction, moving in a magnetic field over graduated dial (galvanometer). Sturgeon, 1824.
Galvanoscope, comprising strip of foil moving in magnetic field, Highton, 1846.
Replica of apparatus used by Ampere to demonstrate that a current carrying conductor exerts a force on a neighbouring current carrying conductor, (demonstration). Ampere, 1820.
Replica of the apparatus used by Ohm in proving that the resistance of a conductor may be measured by dividing the difference of potential at its extremities by the amount of current passing. (Ohm's Law), Ohm, 1826.
Collection of wires of same diameter but different metals, and of wires of different diameter and all of copper, each wire having resistance of one ohm.
Model illustrating Poggendorff's discovery that the difference in the potential of two conductors may be measured by balancing it against a known difference of potential such as that of a voltaic cell (demonstration).
Modification of Daniell cell for use as a standard, giving a difference of potential of 1.072 volt. Thomson.
Galvanometer with pivoted rectangular coil in magnetic field and hair spring connection as in modern instruments (demonstration) Varley, 1856.

(Science Museum, nominal file 3923)

This listing gives a good sense of what was considered important enough not only for inclusion in the Faraday Centenary Exhibition, but also for continuing display and preservation in the collections of the Science Museum. That Ohm's apparatus sits alongside Davy, Ørsted and Ampère is perhaps not surprising. More notable is that they were presented side by side with important technological developments in telegraphy from Thomson, Varley and Ayrton and Perry, particularly given that in today's (British) historiography we often separate science and engineering. It is also worth noting that the Ohm apparatus is not described as a demonstration object, where some of the others are. This reinforces my view that the Science Museum apparatus was never intended for use as an operational instrument, though there is no discussion in the documentation about why some objects were treated in this way and not others.

A letter of 25 November 1931 from the Association to Lyons indicates that the models were built with the assistance of R. W. Paul, an established instrument maker who founded his own company in 1891, having initially specialised in the design and manufacture of electrical instruments (Coe & Woodward, 1996). Lyons's letter continues to say that the Instrument Makers

> wish to present these models to the Science Museum so as to complete [their new exhibition] as far as possible. The cost of preparing these models is being borne by the following Instrument Makers…

This is followed by a list of nineteen firms, representing the major leading British 1930s scientific instrument makers (Science Museum archive, nominal file 3923). It is unclear what role the instrument makers played in the creation of the various reproduction instruments displayed at the Faraday Centenary Exhibition. A write-up of the electrical instruments in the exhibition by Paul himself is also tantalizingly vague (Paul, 1931). He says that there were two stands provided by The British Electrical and Allied Manufacturers' Association, and gives what could well be a first-hand account of decisions around the design of the display. There is no specific information about the Ohm instrument within the paper, and, as is to be expected, the account is written in the third person, so it is not clear what Paul's role was. Did he make the instruments? Design them, or oversee their production? What drove decisions around scaling instruments up or down or using precisely correct materials? Paul also discusses the need to "facilitate an understanding of the mode of operation of the various instruments", by which he meant that in some cases the working parts of each instrument were open to view. This interpretation of Ohm's instrument certainly bears a close visual resemblance to the diagram in Ohm's 1826 paper, and perhaps was intended as a three-dimensional illustration for the instruction of visitors, rather than as operational apparatus (Paul, 1931, p. 337; 339).

**Deutsches Museum apparatus**

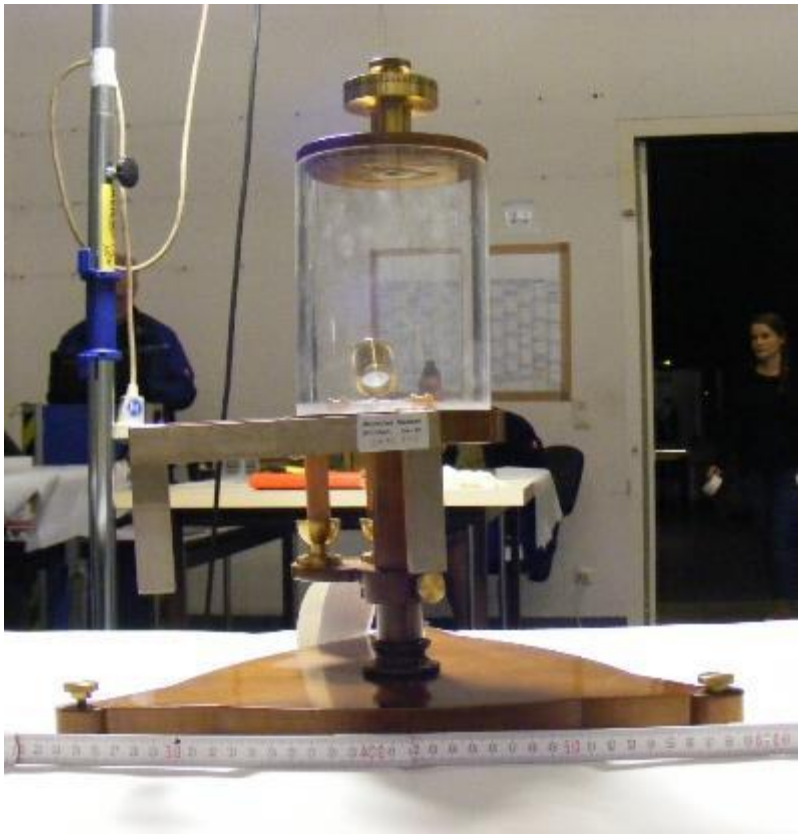


**Figure 16.** Deutsches Museum apparatus, inventory number 1976/9/9

There is very little written information available about the production of the Deutsches Museum apparatus. Its inventory number is 1979/9/9, which provides the clue that the instrument was accessioned into the museum's collection within a few years of the 1976 celebrations of 150 years of Ohm's law. Otherwise, information is scarce in the museum's collections documentation. Until autumn 2015, the Deutsches Museum apparatus was displayed alongside some of Ohm's original apparatus in a display that included Ohm's dry battery, a galvanometer and magnetic coils. The accompanying label provided a brief description of the parts of the apparatus, while a more general label, accompanied by a portrait of Ohm, gave a few key dates in his career and described him as the discoverer of the relationship between resistance, current and voltage (Deutsches Museum visited 24 August 2015). Unsurprisingly, in a label that is fewer than one hundred words long, there is no discussion of the ways this language is anachronistic, and the brief text did little to explain to visitors how the experiment worked. There was also no sign of the "rags to riches" narrative presented in Part A of this chapter, or indeed any suggestion that Ohm had a life outside of

his scientific achievements. The instrument itself appeared on the lower left-hand side of the showcase, visible but certainly not in the most prominent section of the display. The hot and cold vessels were absent from the display, and were not presented alongside the main part of the apparatus when I made a later research visit. Whether or not they were added to the Museum's collection alongside the rest of the instrument is unclear.

On observing the Deutsches Museum apparatus through the showcase glass, an intriguing feature could be seen. One of the feet of the thermocouple appeared discoloured, raising the possibility that the instrument had been operated in the past. In his *Festschrift* for the 150th anniversary of Ohm's law, Teichmann describes the apparatus in more detail, and goes on to discuss the likely voltage and amperage of the equipment in today's terms:

> Since [Ohm] only uses wires of the same diameter, and he uses the length for the external resistance in his calculations, he therefore also calculates internal resistance as a length: 20.25 inches corresponded to about 3.0 m Ω for the diameter used. In the numerical value *a*, which corresponded to the voltage of his thermocouple (approx. 8 mV), the arbitrary setting of the resistances as lengths and the current strength as graduation marks [on the micrometer] was naturally arrived at. If you use 8 mV for its highest voltage - 7.285 during the first series of measurements - you get currents between 2.4 A for wire no.1 and 0.336 A for wire no.8.
>
> (Teichmann, 1976, p. 7)

While the resistance of a length of copper of known diameter is reasonably easy to find, it is less straightforward to look up or calculate the voltage produced by a copper-bismuth thermocouple pair. In the passage above it is unclear how Teichmann arrived at his figures, whether they were mathematically derived or observed in the course of experiment. This, together with the discolouration of the object on display, led me to ask Prof. Teichmann whether the instrument had indeed been used. He responded that yes, the instrument had been tried during the 150th-anniversary celebrations and it had worked as expected. However, nearly forty years on he was unable to recall more specific details (personal communication, 21 August 2015).

In autumn 2015, the Deutsches Museum apparatus was removed to storage due to a major refurbishment project at the museum. This provided an opportunity to access the object, together with other material related to Ohm. The discolouration of one foot of the thermocouple could be more distinctly observed with the object out of the showcase (Figure 17). Notably, the Deutsches Museum apparatus is closer to the dimensions specified by Ohm

than the Science Museum interpretation. This could well be because it was built with the expectation that it might be operated, or it could simply be due to a more careful interpretation of Ohm's published description. However, despite it being a closer match to Ohm's written description than the Science Museum apparatus, there are still some features that appear to be incorrect, or at least unlikely interpretations. For example, the dimensions of the wooden base do not match Ohm's drawing well, and the use of metal for the mercury cups seems unlikely given how careful Ohm was to electrically insulate other components (more discussion in Appendix from p. 266 and p. 269 respectively). Despite these discrepancies, the instrument embodies a close study of Ohm's texts.

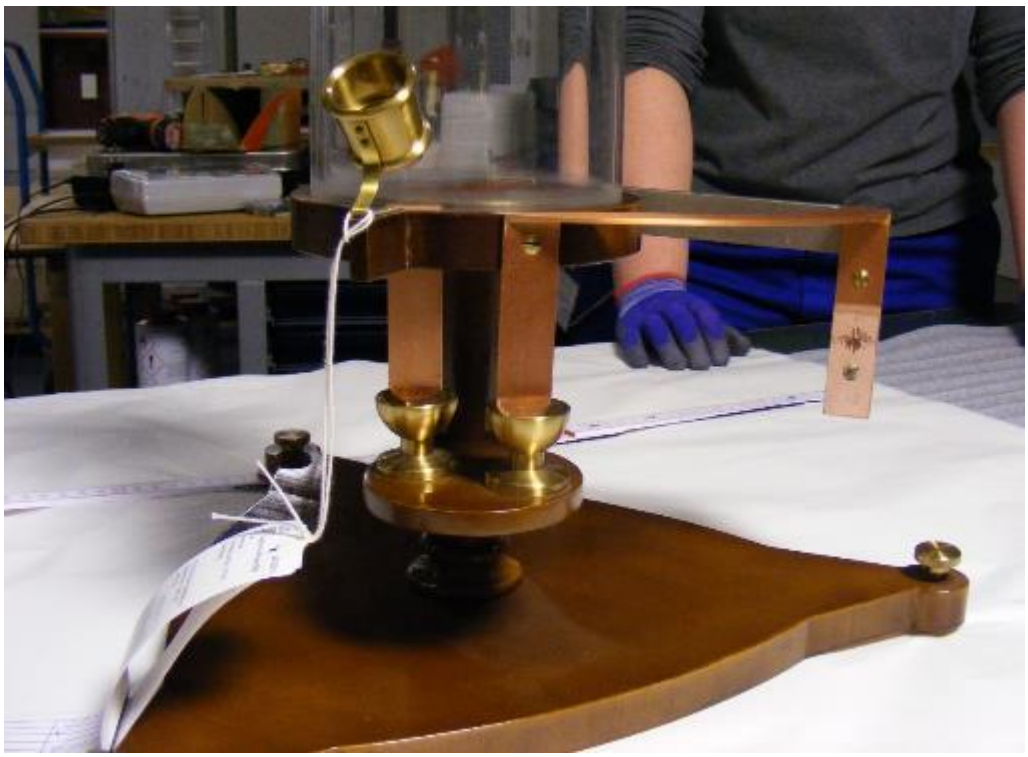


**Figure 17.** Detail showing discoloured thermocouple, Deutsches Museum apparatus.

**Oldenburg University apparatus**



**Figure 18.** Oldenburg University apparatus. (Image courtesy of Wolfgang Engels.)

In an article about the role of the history of physics in teacher training at Oldenburg University, the Oldenburg apparatus (Figure 18) is described as "G.S. Ohm's torsion balance for measurements on the conductibility of various wires (1826)" (Riess, 2000). No date is given for the instrument, though the context provided in Riess's paper suggests that it was in use in the decade leading up to the year 2000. In addition to this object, a reconstruction of Ohm's trough apparatus from 1825 appears in an undated list on the group's website, but not in the article, suggesting it was produced later ("Zur Replikationsmethode", accessed 19 July 2021, Carl von Ossietzky Universität Oldenburg). Although it can be found in several listings, more detailed information about the apparatus at Oldenburg University is limited, particularly regarding the details of its design and production.

The philosophy behind the reproduction of the instruments made by the Oldenburg group more generally is very well documented. Reproduction instruments were used as part of a

laboratory course for trainee teachers, in which five of the twelve experiments they must perform were from the history of physics and are performed with the help of a "working replica" from the University's collection. The motivations for this method of training teachers can be summarised as helping trainee teachers to gain new access to phenomena, terms, laws and theories which are "not only based on the systemic order of science but on a historical analysis of images conceived by people to understand the forces of nature. [The students] are surprized to learn that there is more than one way to explain phenomena and facts" (Riess, 2000, p. 402). In another article co-written by Riess, together with instrument maker for the Oldenburg group, Wolfgang Engels, there is general discussion of the development of reproduction Ohm apparatus, but it is unclear whether it relates to the Oldenburg apparatus or the Flensburg apparatus (Engels & Rieß, 2022)[5]. As Engels was the maker for both examples, it may be the case that this analysis could apply in general terms to both instruments. This discussion focuses on the interpretation of Ohm's descriptions, rather than the practicalities of manufacture.

In addition to Figure 18, a different image is included in a paper by Peter Heering, providing further insight into the use of the Oldenburg Apparatus. In the image, the instrument is apparently in use, with a man bending down to look through a loupe at the magnetised needle (Heering, 2009, p. 396, figure 1). The torsion balance in the Oldenburg apparatus appears to be made according to the dimensions Ohm provided, however, the hot and cold vessels are shaped differently from those depicted in the drawings in Ohm's paper. Furthermore, they appear to be made from stainless steel, which had not been invented in Ohm's time. In an addition to Ohm's descriptions, there is also a glass beaker near the hot vessel, placed to catch occasional drips from the spout of the hot vessel. This photograph also shows the most complete representation of the apparatus, indeed of any of the reconstructed examples of Ohm's apparatus. It is complete with a test wire in place between the two mercury cups; though it is not clear whether mercury was used, or an alternative with fewer health and safety implications. Finally, it is clear from the staining and other marks on the baseboard that the instrument was actively used. The object is not pristine, but instead is pictured scattered with the detritus of experimental work. Bits of wire and something that looks like a spoon are resting where they were last placed, presumably while operating the instrument. From these observations, we can conclude that, in contrast to our two previous examples, the

[5] Spelling of Riess / Rieß varies. I have used "Riess" in my narrative, but reflected the spelling used in the relevant publication for references.

Oldenburg apparatus was not designed to closely visually resemble Ohm's illustrations. Instead, it was built for operation and used. This is evident from the photographs of the object itself, without the need for extensive documentation.

**University of Flensburg apparatus**



**Figure 19.** University of Flensburg apparatus. (Image courtesy of Wolfgang Engels.)

The final reconstruction of Ohm's apparatus considered here in detail was produced in 2021 by Wolfgang Engels, following a commission from Peter Heering on behalf of the University of Flensburg (Figure 19). Heering moved to Flensburg after his time in Oldenburg, and continues the tradition of the Oldenburg group: researchers in Flensburg undertake reconstruction both for historical enquiry and for teacher training. This instrument was produced in the wake of the recent restaging of Ohm's 1825 experimental work by the same group (see Heering et al., 2020). The Flensburg apparatus was built over a similar time period to my own, and we both worked independently, without knowing that the other interpretation

was being produced. Like my interpretation, the Flensburg apparatus is intended as a historically accurate, usable instrument. As such, it provides the most direct comparison to my own interpretation. Points of difference between the two can be understood as differences in interpretation rather than differences in motivation or historiographical context. From looking at a photograph of the instrument, the differences include the use of wood for the mercury vessels, the shape of the baseboard, the form of the bent copper conductor and the location of a piece of silk used for calibration. These differences are addressed in more detail as they come up in Chapters 3, 4 and 5 and the Appendix. Engels and Riess offer some insight into the decisions behind their reworkings of Ohm's apparatus, and include a photograph of the Flensburg apparatus set up for use in their paper (Engels & Rieß, 2022). The text offers some useful insights into their process of reconstructing an operable instrument, however, the chapter is relatively short, leaving lots unsaid about the design details and what can be learnt by reproducing and operating apparatus. There will undoubtedly be more to come from Flensburg now that the apparatus has been transferred from the maker to the research group.

**Conclusions**

This introductory look at four other interpretations of Ohm's experimental apparatus shows clearly that reproducing a historical instrument is subject to the motivations and interests of those undertaking the reconstruction. The Science Museum instrument was never designed to be operated, but rather acted as a kind of three-dimensional illustration. The makers followed Ohm's drawings closely, but do not appear to have considered the practicalities of using the instrument, leading them to some particular decisions in the development of the design. The Deutsches Museum instrument was produced primarily as an act of commemoration, but also embodies detailed research made apparent by the close adherence to Ohm's dimensions and other descriptions. The instrument was operated, but that does not appear to have been its primary function. There is little documentation of the development of the reconstructed apparatus, despite there being several texts about the commemorative activities, suggesting that there were no hand-wringing discussions between the maker and curators about how to fill in the gaps in Ohm's descriptions of his instrument. In contrast, the Oldenburg apparatus was very clearly intended for use. Its primary purpose, as with the other instruments and experiments reconstructed by the Oldenburg group, was to enable a new enquiry-based model for training physics teachers. There are several compromises to be found in the instrument,

where historical accuracy gave way to ease of use or manufacture. Finally, the Flensburg model has some motivations in common with the Oldenburg example, but with a much stronger emphasis on historical accuracy. It also has far more in common with my apparatus than the other examples, in that it is intended to be both historically accurate and operated. In contrast to the Connelly apparatus, the research group at Flensburg University appear to be primarily interested in how it might be used, and comparatively less interested in the process of manufacturing the instrument. This is implied by the way it has been produced as a commissioned object, delivered ready for use in its finished form. In the following chapters, these instruments will enable a kind of comparative anatomy as I reflect upon the decision-making processes required throughout my own interpretation, production and operation of Ohm's apparatus.

# 2. Working with Experimental Objects

## PART A

This project takes Georg Simon Ohm's experimental apparatus as the starting point for understanding how he arrived at his famous electrical law. This is in contrast to many of the biographies and histories discussed in Chapter 1, which typically begin with the written record and, in some cases, pepper that record with objects and apparatus. A bias towards texts in contemporary historiography is perhaps unsurprising, especially given that in Ohm's lifetime his mathematical work was far better known than his experimental work (see Chapter 6, p. 208). However, this chapter argues that Ohm's case is far from unique as an example of materials and instruments being relatively neglected by historians of science today. Furthermore, it makes a case for addressing the literary bias in studies of experimental practice, and for extending existing methodologies of working with evidence of past experiments. I suggest a model of the distributed mind, in which scientific instruments are understood as an entangled part of the cognitive processes of the experimenter. This way of thinking about scientific instruments enables a much more explicit two-way relationship between experimental apparatus and experimental thinking. In considering experimental apparatus as extensions of the experimenter's mind, a shift occurs. Rather than understanding a scientific experiment as being thought up in the mind of a researcher, then built to that pre-conceived notion in a one-way process, the distributed mind model understands physical interaction with scientific apparatus and the phenomena they produce as leading to the iterative reconfiguring and redesign of both the apparatus and the experimenter's thinking.

This chapter also considers what the materiality of scientific instruments can tell researchers. Pamela Smith et al.'s descriptions of objects as a residue of cultural exchanges, belief systems, organised practices, networks and accumulated knowledges apply to scientific instruments just as well as any other type of made object (Smith, Meyers, & Cook, 2014, p. 20). Ohm's apparatus, as with the objects being described by Smith et al., is inscribed with the memories of previous generations' innovations and cognitions, whether following Volta's thinking by placing two metals in contact, integrating Coulomb's understandings of torsion forces in the torsion balance, or by embedding manufacturing skills and techniques within the apparatus. The conclusion of the chapter considers the extent to which these approaches apply in this particular case, given that Ohm's original apparatus is not available. In considering the value of a non-existent remnant of Ohm's apparatus, it is salient to remember

that according to the distributed model of the mind an object is a physical manifestation of a person's mind and embodies the histories and networks of the maker. An original artefact, such as the Joule apparatus held in the Science Museum's collections and inspected by Otto Sibum, can only ever offer a partial manifestation of the experimenter's mind and thought processes. Similarly, a reworked piece of experimental apparatus also offers only a partial manifestation of the experimenter's mind. There is value in both pieces of evidence, however a reworked experiment has the distinct advantage that it may be operated and handled whereas in many cases access to original artefacts is heavily moderated.

**A bias towards text-based sources**

"Historical studies are predominantly based on textual representations of the past – the notebooks, letters and papers scientists write and publish in connection with their work" (Sibum, 1995b, p. 27). This claim, that the instruments and objects of science and other histories have been neglected, has been made repeatedly by historians. Ten years later, Jim Bennett, a museum-based historian commented that "[objects] are not so easily and readily consulted, perhaps, and historians, working in a profession where a measure of bookishness is essential for success, are not always at ease with the study of objects" (Bennett, 2005, p. 605). Another decade on and Gerritson and Riello argued: "To study scientific instruments *as things*, however, is a reasonably new development, and emerged as part of a reconceptualization of what 'science' and its history might be" (Gerritson & Riello, 2015, p. 14). Even as Gerritson and Riello were writing, a "material turn" was supposed to be taking place within the history of science. This turn came soon after Appadurai and Kopytoff argued for the value of "object biographies" in order to understand objects in terms of their contexts of exchange, production and consumption. This scholarship is still referred to as influential in the development of new, object-centred studies in the history of science (see Drieënhuizen & Sysling, 2021, for a recent example of such a claim referring to Appadurai, 1986, and Kopytoff, 1986). However, it is not uncommon to see studies that centre on object biographies in which there is little or no evidence of engagement with the materials of the objects (again, Drieënhuizen and Sysling is a good recent example). Meanwhile, Lorraine Daston's work provides another very thorough demonstration that "scientific objects" need not be considered as material things in order for their biographies to be discussed at length. Her edited volume, *Biographies of Scientific Objects*, includes studies of the mind, society

and cytoplasmic particles, which, unsurprisingly, are not centred on material things (Daston, 2000).

In contemplating the claimed material turn in the history of science, Alberti points to a well-established but small cadre of scientific instrument scholars mostly, but not entirely, in museums, some of whom have eloquently called for historians of science to make better use of material culture. Alberti questions whether these calls have been successful, asking "whether the sector as a whole has materially turned" (Alberti, 2017). Tucked away in a footnote of a later paper by Alberti, we learn that

> In the last decade, of the 136 full articles in the leading Anglophone history of science journal *Isis*, fourteen were explicitly concerned with material culture and only five of those authors evidenced close study of objects… only two of the forty-one Isis 'Focus' sections in the last decade are devoted to instruments or collections.
>
> (results of a survey by Laura Volkmer, reported in Alberti, 2018, p. 451)

Put another way, fewer than 4% of the articles in *Isis* provided evidence that a *single object* had been utilised to support the article's argument. When one considers how many text-based documents are typically referred to in a research paper, the numbers of objects being explicitly consulted and referenced in the production of leading history of science scholarship is extraordinarily small. Another interpretation of the statistic above might be that the professional norms around how we cite and evidence our claims in the history of science privilege text-based sources. If object-based citations were presented consistently alongside text citations, their influence on historical research would be easier to assess. My own reference list for this work deliberately includes a list of material sources. We might also question whether *Isis* is the best journal to study. A more comprehensive study might include a more varied sample of scholarly publications, though Alberti's contention that it is the leading Anglophone journal makes it a good starting point.

Chapter 1's introduction to Ohm's life and work includes a literature review featuring an array of critical, scholarly studies; dramatic and relatively uncritical narratives; and primary source materials. They are all considered and weighed up for their usefulness in pursuit of answers to the research questions posed by this project. A standard *literature* review would stop there, however this project is concerned especially with Ohm's experimental apparatus and practice and therefore Chapter 1 also includes what might be called a *material culture*

review of Ohm and his work. It draws in apparatus used by Ohm and now held in the collections of the Deutsches Museum, together with a series of interpretations and reconstructions of the apparatus Ohm described in his 1826 paper. It also surveys other material residues of Ohm's life and work, such as his gravestone, the plaque on his childhood home and the conspicuous absence of the Ohm brothers in the Erlangen town museum. An important point here is that the two categories of evidence reviewed – literature and material culture – rarely interact or reference one another. Nevertheless, both types of material contain examples of deep scholarship. The consistently high quality of reproduced Ohm apparatus presented in Chapter 1 Part B, made at different times and with different motivations, demonstrates detailed study of Ohm's experiment at several different moments. The evidence for the detailed studies is embodied within the instruments themselves, rather than written up in scholarly texts. Returning to the quote from Gerritson and Riello, their assertion that studying instruments as *things* is a new development reveals their view, and the view of their discipline, of the status that has been given to material engagement with scientific practice by professionalised historians. To hold such a view demonstrates a lack of recognition of the care and study that has gone into the numerous reproductions and activities undertaken with surviving apparatus, often in museums or in what might be termed enthusiast communities of study where connoisseurship is prized. There is a bias, then: histories of science for scholarly audiences are almost exclusively communicated through written or spoken word, and they rely predominantly on written evidence, neglecting historical research that manifests in, for example, reproductions of experiments, acts of memorialisation, or the choices to select or reject particular objects for museum preservation or display.

This bias in favour of printable communications can also be found elsewhere. Rachel Prentice, in her ethnography of anatomy and surgery education, seeks to "correct a visual and cognitive bias that runs through academic and practitioner discussions of biomedicine" (Prentice, 2013, p. 9). To do so, she challenges the dominance of printed images and texts, and emphasises the "techniques of the body" employed by surgeons. She centres her work on their use of sensorial information that is difficult to communicate through image and word, such as changes in sounds that inform their work as they perform surgeries (Prentice, 2013, p. 12). Another anthropologist, Tim Ingold, has similarly sought to rebalance the status of materials in scholarly writings. He argues that the bias he observes can be traced back to Aristotle.

> To create anything, Aristotle reasoned, you have to bring together form (*morphe*) and matter (*hyle*). In the subsequent history of Western thought, this hylomorphic model of creation became ever more deeply embedded. But it also became increasingly unbalanced. Form came to be seen as imposed by an agent with a particular design in mind, while matter, thus rendered passive and inert, became that which was imposed upon.
>
> (Ingold, 2010, p. 92)

Ingold aims to "overthrow the model" and replace it with a model that gives primacy to the processes of formation, rather than to final products. This is perhaps more necessary in fields of work, like surgery, where the goal is to achieve a material end in contrast to scientific experiment where, perhaps, the ultimate goal is to obtain knowledge. On the other hand, within the narrow context of an experiment the focus of attention sometimes is on achieving a material end, whether observing the path of reflected light, deflecting a needle or observing cells multiply. In another example, Ann-Sophie Lehmann comments that the field of art history traditionally also followed this form-first model. "The real 'work' of art, it was commonly assumed, took place in the mind of the artist, where ideas and concepts were formed, which then merely had to be imposed onto passive matter." This model is becoming less dominant as art historians increasingly engage with the materials of the artworks they are studying (Lehmann, 2012, p. 10).

Similar arguments have been made in the history of experimental sciences, with Gooding arguing that there has been a belief that "experiment has always been primarily a form of empirical check on theories", and that historians have been overly reliant on published evidence where an experiment "usually appears in its finished and perfected form" (Gooding, 1989, p. 65). Indeed, Ohm's 1826 narrative presents an instrument, fully formed and seemingly immediately working satisfactorily to produce the results he was seeking, albeit with the acknowledgement of a false start and a modification from his 1825 attempt. Even if it was desirable to divulge not only the false starts, but also uncertainties, inconsistencies and various kinds of help needed to make such an experiment work, it would be a challenge to put into words. My own experiences of attempting to document my work make it clear how challenging it is to note down every aspect of developing and operating a piece of apparatus (for an evaluation of my own documentation practice see Appendix p. 308). Schickore's essay reflecting on responses to the recognised mismatch between what scientists do and what they state they did when communicating their findings in publications demonstrates that

this problem is not unique to Ohm, and that scholars have approached the mismatch in a variety of ways. Schickore notes that there is agreement across several scholarly disciplines that the cause is due to scientists consciously attempting to present their research with a view to audiences. However, "there is no agreement (*a*) about what the relevant audiences are; (*b*) whether the mismatch is epistemologically significant; and (*c*) what the best way to analyze it would be" (Schickore, 2008, p. 334). Gooding, similarly, notes that scientists' accounts of experiments typically reflect the planned or finished product, rather than their actual practice (Gooding, 1989, p. 64). This assessment, when applied to Ohm, takes us to the very heart of this thesis, which seeks to reconstruct some missing aspects of Ohm's thinking by accessing his material legacies.

Today's particularly abstract and impersonal way of writing scientific texts became more prevalent as scientific norms and steps towards professionalisation began to emerge in the nineteenth century. Disembodied textual descriptions of experiments that sought to remove the practitioner from the written experiment became prevalent, even though experimental science is, almost by definition, a tactile, three-dimensional, multi-sensorial and embodied activity (for a comprehensive analysis of how writing in scientific publications changed in England, France and Germany from the seventeenth to the twentieth century, see Gross, Harmon, & Reidy, 2002). There are also, however, pointers in the written record of the incompleteness of this type of record. Soirees and meetings of scholarly societies were places where experiments and experimental practice were shared. As Chapter 4 indicates, part of the reason that researchers like Ørsted travelled around Europe so extensively was to observe and share in the experimental practice of their peers, and in doing so they could gain the practice-based knowledge that is hard to put into words. Our access to historical evidence must always be partial and influenced by our own time and experience, but even with that acknowledgement it is remarkable that the study of as tactile an activity as experimental science has such a strong literary bias.

**Traditions of material engagement**

There is a long history of experiments being reworked in the service of history of science. Fors et al. point to foundational work in the 1960s, with work by Stillman Drake, Thomas Settle, James MacLachlan and others providing rich studies of mechanics and optics (Fors et al., 2016, p. 86). In the same period, Michael Polanyi was publishing philosophical work on

the notion of "tacit knowledge" which continues to provoke discussion today. His work began with the premise that "we can know more than we can tell", for instance we can recognise a familiar face in a crowd without knowing, let alone being able to verbalise, how we did it (Polanyi, 2009, pp. 4-5). Polanyi called this kind of knowledge "tacit", and argued that tacit knowing always involves two things, saying: "we know the first term only by relying on our awareness of it for attending to the second" (Polanyi, 2009, p. 10). Or, to apply this terminology to Polanyi's example of recognising a face, I know that I must have the knowledge and skills required to recognise faces, because I am able to do so with a good degree of accuracy. This ability is Polanyi's "proximal" or "distal" knowledge; we are unable to explain those skills or how we employ them. The knowledge of who that person is, or in other words the outcome of the application of my proximal knowledge, represents the second type of knowledge which Polanyi called "subsidiary" or "focal". Polanyi argues that such proximal knowledge can take the form of "muscular acts for attending to the performance of a skill" just as well as other types of knowledge (Polanyi, 2009, p. 10). This type of discussion about physical or embodied knowledges continued to be central to the work of reworking or reperforming experiments.

The next period Fors et al. point to is the 1980s and 90s when there was a rapid expansion of this mode of historical enquiry. For instance, there were attempts to rework alchemical experiments, notably by Lawrence Principe; in the same period Otto Sibum undertook his outstanding reworking of James Joule's experiment to determine the mechanical value of heat; also at that time the teacher training programme of Oldenburg University was making increasing use of reproductions of historic apparatus in the training of science teachers. Despite this breadth of activity, they note "how controversial such work actually was among historians of science in the fairly recent past… the historical technique of reproduction/reworking was often vigorously opposed" (Fors et al., 2016, p. 86). The tradition established by the programme at Oldenburg University continues to be influential for historians of science who are reworking experiments or other types of science in practice. The Oldenburg methodology is characterised by three elements working together iteratively, rather than consecutively:

1. The "material culture" of a historical experiment is designed and built. By this it is meant primarily that a reworked instrument is built, though any other aspects that can

be reconstructed should be, for example the space and conditions that an experiment is carried out in;
2. Trials with the built historical experiment are performed. Special attention will be paid to the "embodied capabilities" that are needed to make the experiment work;
3. Historical, archival exploration of the world in which these historical experiments were developed must be undertaken.

This method can be found summarised in several places (see Heering, 2005, pp. 318-321; Sibum, 1995b, pp. 27-28). The writers typically emphasise the importance of contextualising the gestural knowledge that is revealed and acquired, and demonstrating the ways that knowledge acquired through performance was important to historical actors.

For an example of this, Sibum revealed that specialist skills in taking precision readings with thermometers were needed to be able to carry out his reworking of James Joule's experiment. Joule, he argued, had access to this skilled practice through his family brewing business, whereas other natural philosophers did not. Therefore, Joule's access to what Sibum called "gestural knowledge" enabled him to perform an experiment that others could not. Gestural knowledge here is described as "knowledge united with the actor's performance of work, and it changes according to the specific kinds of performance, for example the manipulation of an instrument or the use of mathematical tools, and in ever new historical circumstances" (Sibum, 1995a, p. 76). Sibum also used the phrase "gestural collective" to describe the Manchester brewing culture that Joule had access to. This description of gestural knowledge, and in turn the gestural collective, is broad and could be more tightly defined. In his recent work, Harry Collins has reflected on his own scholarship and the work of others relating to the concept of the closely related idea of "tacit knowledge", criticising the literature on the topic over several decades as being "less than clear". In his 2010 book *Tacit and Explicit Knowledge* he spends almost 200 pages trying to categorise and map three different types of tacit knowledge, which he calls: relational, somatic and collective (Collins, 2010, pp. ix-x). Getting into the detail of what he means by these terms is unnecessary here, however his argument, that many studies have conflated different types of knowledge under the broad umbrella of tacit knowledge, echoes my concerns about the lack of clarity about the concept of gestural knowledge. In practical terms, it is hard to understand where the boundaries of a gestural collective might sit. As Marcel Mauss argued, almost every motion we make and piece of knowledge we hold is socially shaped (Mauss, 1973). Consequently, one could argue that the term "gestural collective" can be applied to almost any activity: the tools and

methods I use when I brush my hair, use a knife and fork, and tie my shoe laces are not universal or intrinsically human. Instead, they are all expressions of gestural knowledge gained throughout my life and influenced strongly by the gestural collectives I have lived within.

Another criticism one might level at the concept of gestural knowledge is that it could be understood as a one-way process – as something to be acquired to enable us to perform a particular experiment or to enhance our historical knowledge. To counteract this, in his writings Sibum offers a nuanced acknowledgement of the many ways in which natural philosophers were shaped both by the society they lived in and the work of their predecessors (for example Sibum, 1988). We can argue, then, that experimenters' socially constructed understandings of the world interface with their access to, and ability to acquire, gestural knowledge. To take this argument further, one might suggest that Joule's extensive interaction with precision thermometry in the brewing context not only provided him with physical skills, but also ways of thinking.

To briefly step away from Joule, let me share an example of a way in which my own cognition has been socially shaped. When I imagine a route for a journey, I typically perceive it as a top-down bird's eye view map. I have spent hardly any time looking down on the routes of my journeys, however I have been exposed to maps from a young age and trained in their use. For me, maps have gone from being a physical tool to a "cognitive tool", to borrow the phrase from Ryan Tweney (2004, p. 748). My cognition has been shaped by the technologies and social environment I have been exposed to. Returning to Joule, it seems reasonable to argue that Joule's experience of interacting with precision thermometry in the brewing context shaped the way he thought about temperature, and consequently influenced his theory building and experimental practice. He not only *gesturally*, or physically, worked like a brewer with his advanced thermometry skills, but he also *cognitively* worked like a brewer. Likewise, it seems reasonable to suggest that when working with his experimental apparatus, Joule developed both gestural knowledge and cognitive tools as he explored the specific requirements of his experiment.

I am keen to follow Collins's lead in recognising some of the looseness in earlier terms including the ideas of "gestural knowledge" and "tacit knowledge", and for the purposes of my work focus on the two-way relationship between a physical, tangible experiment and the

experimenter's theoretical and conceptual framework. In this particular case I seek to trace the linkages between Ohm's material interactions and the ways he used language to indicate his understandings of how electricity behaved, which in turn relates back to his instrument design. This slightly different emphasis to Sibum and his predecessors is possible in today's historiographical context where the broad concepts of tacit or gestural knowledge no longer need to be argued for, and instead there is space to drill down into the different uses we can make of them by applying different analytical lenses.

**Experiments as a mode of thinking**

> Suppose I am a blind man, and I use a stick. I go tap, tap, tap. Where do I start? Is my mental system bounded at the handle of the stick? Is it bounded by my skin? Does it start halfway up the stick? Does it start at the tip of the stick?
>
> (Bateson, 1973, p. 434)

Bateson, quoted above, was one of several anthropologists working in the mid-century grappling with understandings of how humans perceive the world, and especially the role of tools and probes. The blind man's stick has become an emblematic case in those discussions, and is still used today by scholars seeking to understand the roles of material things in influencing our thinking and perceptions. For the purposes of this discussion, the blind man's stick might well be replaced with any number of scientific instruments. Experimenters often remain blind to the phenomena they are trying to observe, requiring instruments to reveal them by some sensorial means. Vision, smell, taste, touch and hearing have all been employed in the pursuit of scientific knowledge. Polanyi, referring to Bateson's blind man's stick example, suggested that "the use of tools, of probes, and of pointers [can be interpreted] as further instances of the art of knowing" (Polanyi, 2009, pp. 7, 12). However, where Polanyi was interested in the ways that the user's ability to manipulate their probe or stick is transformed into tacit knowledge as they become familiar with the feel of it, for Bateson the scenario presents an opportunity to think about the boundaries of the user's mind.

If observation is reliant on sense organs, in the example above the stick becomes an extension of touch. Bateson's scenario is used by archaeologist of the mind Lambros Malafouris to argue that "human cognition was never entirely in the head" (Malafouris, 2013, p. 14). Malafouris argues instead that the mind has a plasticity and a distributed nature, and that by

using the stick the blind man turns touch into sight. He argues that "out of brains, bodies, and things, mind emerges" (Malafouris, 2013, p. 17). For him, the mind is situated not in the brain, as we usually conceive of it, but it is generated by the whole system of interacting bodies and things.

> For one thing, most of our evidence about the evolution of human intelligence comes in the form of material culture rather than abstract ideas and brain tissues. For another, the more we come to learn and understand about things, the more they look like a genuine element of what it means to be a human cognizer… the sort of stuff that makes up the mind can be equally found located within and outside the skin… thinking *is* interactions of brain and body with the world. Those interactions are not evidence of, or reflections of, underlying thought processes. They are instead the thinking processes themselves.
>
> (Malafouris, 2013, p. 37)

Malafouris's goal, then, is to reconceive material culture as part of the self. A crucial observation made by Leroi-Gourhan in 1964, and picked up by Malafouris in support of his argument, is that human evolution has been oriented toward placing *outside* what in the rest of the animal world is achieved *inside.* This can be related to early tool use to replace and enhance our chewing abilities as well as to recent tool use for observing natural phenomena, such as a telescope to enhance our ability to view distant objects. Scientific instruments, along with a great many other human technologies, follow this trend. Polanyi similarly argues that as we use tools, "All meaning tends to be displaced *away from* ourselves" (Polanyi, 2009, p. 13, emphasis in original). According to Malafouris's argument, then, when we look at past experimental practices and apparatus, we are not looking at the outputs of a mind, but the material residue of a mind and a particular way of thinking.

Rachel Prentice makes similar arguments in her descriptions of surgical education, reporting an example of a conversation with a surgeon who says "I take my probe. That's my finger extender" (Prentice, 2013, p. 189). Another surgeon told her trainee surgeons to "'become part of the [patient's] shoulder (or other body part)" when they operate (Prentice, 2013, p. 15). In the act of performing surgery, the surgeon's body extends to the patient, even when technology comes between them. Prentice too refers to the blind man's cane, repeating a

claim by Merleau-Ponty that the cane extends the blind man's consciousness into space. What remains unstated, she argues, is that

> like a surgeon, the blind man too needs years of practice to navigate in his world. Further, tapping does not resemble navigating by sight or by unmediated touch. Both cane and probe have structuring effects on the users' embodiment.
>
> (Prentice, 2013, p. 189)

If the cane is an extension of the blind man's consciousness, and the blind man's thinking is structured by his use of the cane, the cane and its particular form must be informing the way the blind man thinks. If we extend this argument to experimental apparatus the implications are clear: not only is the experimenter's apparatus an extension of the experimenter, but the use of the apparatus structures the thinking of the experimenter. This two-way relationship makes a study of the sensory experience of using particular experimental instruments an important element of understanding how experimental practice influences the development of theories.

**The experimenter's habitus**

The previous section explores the effects of particular material objects on an experimenter. This section investigates the influence of a wider cultural context on the ways our physicality is socially constructed. Marcel Mauss's work provides a solid foundation here, particularly his argument that we all perform everyday activities differently, whether walking, swimming or digging holes:

> I was well aware that walking or swimming, for example, and all sorts of things of the same type, are specific to determinate societies; that the Polynesians do not swim as we do, that my generation did not swim as the present generation does… During the war I was able to make many observations on this specificity of techniques. E.g. the technique of *digging*. The English troops I was with did not know how to use French spades, which forced us to change 8,000 spades a division when we relieved a French division, and vice versa. This plainly shows that a manual knack can only be learnt slowly. Every technique properly so-called has its own form.
>
> (Mauss, 1973, pp. 70-71)

Mauss went on to frame this in terms of what he called the social nature of the *habitus*, the physical characteristics of a person. Habitus, he argued, varies between societies, educations, proprieties and fashions. Applying this to experimental practice, the habitus of the

experimenter is crucial. Everything from the way the experimenter holds themselves to what they wear, where the experiment takes place, the mannerisms and gestures of reading the results, any interactions with an assistant and the ways of reporting and recording the experimental outcomes are all affected by the individual's socially constructed habitus.

Pulling the previous arguments together leads to a conundrum. If experimental apparatus is an extension of the experimenter's mind and a product of the experimenter's habitus, how can a historian usefully, let alone objectively, rework a historical experiment? At the point of reworking, the experimental apparatus is simultaneously an extension of the original experimenter's mind, habitus, processes, networks and histories *and* an extension of the mind, habitus, processes, networks and histories of the person currently performing the experiment. Not only can the claim of difference be made between experimenters now and then, but also between experimenters who were contemporaries. As Sibum states, "[reworking past experimental practice] is a form of knowledge, like that of oral cultures, which can only be communicated through active participation or enculturation. It is also one which *we cannot necessarily identify with the historical actor's knowledge*" (Sibum, 1995b, pp. 27-28, my emphasis). Every performance of the experiment is, by this argument, different and entirely dependent on the person undertaking the experiment. At one extreme reading of this situation, one might argue that reworking experiments is ahistorical and not worthy of spending time on. However, the risk of such an extreme point of view is that it can also be applied to written sources which, by the arguments above, are also extensions of the minds of the authors and interpretations of the readers. It is uncontroversial to state that historians reading historical texts are influenced by their own time and experiences. Just as with readings of written sources, some of the value of reworking historical experiments is to investigate precisely the differences in performance and interpretation of an experiment between different people at different moments.

Reflections on past reconstructions of Ohm's experimental apparatus in Chapter 1 make the case for a material historiography. This chapter suggests that there is a particular historiography in the performative and gestural aspects of working with reproductions too. Within that historiography we can identify some types of performative interaction with historical materials: there is perhaps a distinction to be found between performances that are informed by historical sources, but which are intended primarily to enhance the skills or understanding of the performer and, on the other hand, performances that seek to answer

questions about the past. We can see the implications of this distinction in the section below which asks “What kinds of answers do reworked experiments provide?” An understanding of our historiographical intentions must work in concert with an awareness of our socially constructed selves. Recognition of our particular interests and influences, and the identification of the places where our modern perspective most heavily influences our readings of historical evidence, is crucial to acknowledging the limitations of any study of the past, whether we are studying objects or texts.

Perhaps the most challenging ahistorical biases for historians to overcome are ones that are not simply gestural, but deeply embedded in their cognition. In the case of electricity, I was taught from a young age that electricity flows, that it travels in circuits that must be unbroken to work, that electricity is the flow of tiny particles called electrons, and so on. In reading two-century-old descriptions I must continually remind myself of not only the linguistic differences, for instance tension is not necessarily the same as voltage although in some contexts they are used interchangeably, but also try to unlearn the fundamental rules of electricity that are deeply embedded in how I think about how Ohm’s experiment works. In doing so I can begin to observe that Ohm’s “*elektrische Kette*”, an electrical circuit or chain, was a relatively new construct and not a central part of everybody’s thinking about electricity. These kinds of considerations are also essential when engaging in physical reworkings of experiments. In commissioning, procuring and working with apparatus, the historian has to transpose between their state of self-imposed forgetfulness and the contemporary language needed to communicate with makers, suppliers and helpers who are entirely embedded in today’s modes of thought. Part of the historian’s craft lies in being able to offer a translation between the two states.

**Visual acuity and connoisseurship**

Just as material interactions can structure the thinking of a scientific experimenter, they can also shape the thinking of a historian or any other scholar working with materials. In order to rework past experiments, it is not only necessary to unlearn things in an act of wilful amnesia, but also to learn new things. When Ulinka Rublack commissioned the making of an interpretation of an early sixteenth century garment from experts on historical dress and renaissance leather, she observed the making process. Reflecting on the specific craft skills needed, she noted

> ... the process of hammering, with complete precision and "split second determination," the holes into the leather hose to connect the leg garment with the upper garment. No remaking of the holes was possible, and these were exciting moments when expertise was tested and craftsmanship needed to be guided by confident cognition.

In the process, Rublack also gained an appreciation of the skills employed to manufacture an object, which she became better equipped to recognise on sight:

> Reconstruction trains visual acuity, as one is able to better register the intelligence of the hand that has crafted a garment or other object.
>
> (Rublack, 2016, p. 27)

In undertaking a reconstruction, one gains a degree of visual acuity that makes it possible to look at objects *other* than the ones they have directly reconstructed, and see the "intelligence of the hand" behind them. In the case of observing experimental apparatus, this kind of visual acuity applies not only to understanding the maker of the apparatus, but also the user of the apparatus. What seems quite simple in a scientific paper peppered with the matter-of-fact descriptions typically used in scientific writing, can be labour intensive, difficult or require particular skills or knowledges. This appreciation for the complexities behind simple written descriptions became clear for students involved in reworkings of early modern recipes. "When you read it [the recipe]," Marilyn Bowen recalled, "it seems very straightforward, it seems he [the author-practitioner] knows what it is written about, but when you start to do some of the 'experiments' you see he was experimenting as well and there is a lot of the unsaid and some of the little words may mean more than what you think" (Exit interview with an MA student on the 'Making and Knowing' project Bilak, Boulboullé, Klein, & Smith, 2016, p. 42). Conversely, something that appears incomprehensible and complicated when described in prose, can become almost instantaneously understandable upon exploring physical apparatus. Looking at a reconstruction of Ohm's apparatus facilitated my comprehension of his experiment, leading to a much more immediate understanding of how the experiment was operated than the written document had enabled; I reflected on this experience in Chapter 1 (p. 56). A small amount of reconstruction or reworking, then, can inform both the way a historian reads text-based evidence and also their appreciation and readings of a range of object-based evidence.

Where working closely with a single object can build visual acuity, looking closely at a wide variety of examples of a particular type of object can enable a scholar to develop their connoisseurship. Polanyi wrote, "To become an expert wine-taster, to acquire a knowledge of innumerable different blends of tea or to be trained as a medical diagnostician, you must go through a long course of experience under the guidance of a master" (Polanyi, 1958, p. 54). Similarly, as a museum curator, I can be reasonably expected to be a recognised connoisseur of the types of material in my care. Due to my many years working with particular museum collections, I can identify the artworks of a number of recognised polar artists at a glance, can confidently date early British radio receivers, and can recognise a wide range of navigational apparatus from the last 250 years. This is, as Polanyi states, due to a long period of being exposed to numerous examples of these types of artefacts. Where my view differs from Polanyi is in the requirement to be introduced to these by a "master". Part of the historian's development of connoisseurship can be through self-guided study, following the trails left by evidence and the scholars who have gone before them. Looking closely at objects and written texts is part of the development of this connoisseurship. In undertaking this project, I developed a strong sense of connoisseurship related to the design of individual components of Ohm's apparatus, and as my expertise grew I became able to assess other interpretations of the apparatus more skilfully, as seen in numerous places in the Appendix where the relative merits or disadvantages of other reconstructions of Ohm's apparatus are discussed (for example see Appendix, p. 238 and p.266).

The development of visual acuity and connoisseurship, which both lead to an enhanced ability to recognise new features of an object, is rarely explained in scientific writings, but is nevertheless central to the act of observation. For example, Rachel Prentice gives an example of learning to recognise a phenomenon from a set of physical indicators.

> …one day in the operating room, a hand surgeon opened up a knuckle to reveal the arthritis inside. She pointed out that the joint's surface was pinkish, like chicken bone, and said the discolouration was a sign of degraded cartilage. I was uncertain about whether I had seen the arthritis. "Arthritis" was a linguistic object but not yet a material object for me. Later, however, the surgeon opened up another patient's knuckle to reveal its pearly-white, healthy surface. I could see the difference and began to have some understanding of what arthritis looks like to the naked-eye. Arthritis had become a material object for me.
>
> (Prentice, 2013, pp. 16-17)

Turning to Ohm, when he observed the magnetised needle in his apparatus deflecting, then returned it back to its starting position to read a number from the micrometer, what did he think he was observing? The following chapters provide detailed answers to the many questions that underpin that seemingly simple enquiry. The point here is that Ohm's observation of the deflection of a magnetised needle revealed something to him about a flow of electrical current, just as Prentice's observation of a discoloured joint revealed arthritis. His experimental apparatus makes the linguistic or conceptual object of electrical current into a material object that can be interacted with, manipulated and observed. In both cases, the empirical observations made are intellectually linked to the theory that the practitioner is working in, however at the point of observation the theory becomes secondary to the intense observation required to understand the observable phenomena. The theoretical realm and the material realm, though strongly linked, remain ontologically distinct and as such can be studied separately. The consequence of this is that without engaging with the instrument in detail, the histories we tell can only offer a very theory heavy version of experimental practice.

The development of visual acuity, or the ability to look at an instrument and know something of the skills and challenges that were involved in the manufacture or use of it, is a facet of Sibum's gestural knowledge. It also helps to overcome a challenge that Rublack articulates with reference to historic dress: "…most historians grapple with the dilemma that most early modern dress is lost, has changed or lost color over time, or, if preserved and displayed in museums, is presented statically, with very low and uniform lighting" (Rublack, 2016, pp. 11-12). Whether studying clothing or scientific apparatus, the connoisseurship developed from interacting with a single historic object or reconstructed experiment can enable deeper readings of other material objects, even when they must remain static. A relatively small amount of work with materials of experiment can influence how a historian understands experimental practice, and rebalance their understandings to incorporate both physical and theoretical elements of experiment.

**Communicating the incommunicable**

A claim made by several historians who rework experiments is that the practice enables historians to reclaim otherwise unreachable knowledge. For example, Sibum describes the acquisition of gestural knowledges, arguing that they are incommunicable:

> Joule's consciousness of and probably his ability to control these atmospheric influences were based on long experience in constructing thermometers and developing a particular 'habit of taking measures'… But that embodied capability, that particular gestural knowledge, was incommunicable.
>
> (Sibum, 1995a, pp. 102-103)

In adopting the word "incommunicable", Sibum refers to the work of George Adolphus Wigney who described the brewer's craft as

> an imitative system of labour, in which three of the five animal senses are engaged, seeing, feeling and smelling; and in which the mental faculties have usually but little participation… but if you ask [the maltster] to state the mode by which he judges, he is quite unable to express the means by any clear and definite language, because the mind has no participation in the determination, and as his experience is incommunicable in words, so it is in practice, for he cannot impart to another by any mental instruction, the discernment of the animal faculties, which he has himself acquired by long continued observation.
>
> (Wigney quoted in Sibum, 1995a, p. 85)

Evidently the word incommunicable in these examples is an unfortunate choice as the knowledge *is* being communicated between people, if not in a verbal or literary sense. The maltsters in Wigney's discussion all used closely related techniques learnt from each other and honed over many thousands of hours spent practising their craft. Trainee physicists today learn about thermometry and measurement in a classroom through verbal, written and practical work; dedicated application of that taught knowledge to the requirements of the brewing industry could, presumably, lead to them being similarly proficient to Joule. What, then, sits behind the notion that there are "incommunicable" facets to experimental practice?

Ingold argues that, contrary to depictions like Wigney's, skilled practitioners are often inclined to expound upon their crafts "vociferously, demonstrably and at very great length." He suggests that the idea of the silent craftsman is "largely a fiction sustained by those who

have a vested interest in securing an academic monopoly over the spoken and written word" (Ingold, 2013, p. 109). Wigney's phrasing, that the maltsters use "animal" senses, and that their mental faculties have "little participation", gives the impression that he regards the maltsters as an unsophisticated and unintelligent group. While hardly conclusive, it aligns well with Ingold's assertion. Contrary to the idea that the maltsters' knowledge was incommunicable, they could in fact tell when to turn their grain from the feel and smell, and they shared that knowledge through demonstration, observation and description with novice maltsters. This kind of argument can hardly apply to Joule, however. Perhaps, where for some craftspeople the emphasis on apparently incommunicable "animal" senses may be intended to suppress a sense of intellectual mastery, for heroic figures like Joule it bestows them with an unknowable quality – an unachievable set of personal attributes, or a kind of genius for experimentation.

Perhaps a more generous understanding of the unwritten parts of Joule's work can be found in Polanyi's discussions of skills. In his language, skills can be "unspecifiable" and learnt through repetition and gradual adjustment and improvement; they can also be tacit. For instance, in learning to ride a bike, one might know that cycling faster will make the bike more stable without understanding the underlying physics of why that might be. A person's skills might also become tacit over time, as they become increasingly and subtly refined after an initial period of learning (Polanyi, 1958, Chapter 4). It is plausible to think of Joule's sophisticated use of thermometers as being developed through both kinds of influence: his basic thermometry skills may not have been described because to him they were tacit following many years of practice, and some of his activities or decisions may have been informed by tacit knowledge that he had not realised he had picked up along the way. However Joule built his particular set of skills, he could tell when and how to take a temperature measurement from a host of subtle cues in his workspace. Likewise, in Ohm's 1825 experiment it appears he could tell when the trough battery he was using was behaving properly from the presence or absence of bubbling sounds (Heering et al., 2020, p. 23). Such cues might not be explicitly communicated in the written record, though there may be clues. It is only in performing or observing a craft or practice that the non-verbal multi-sensorial information becomes clearly evident.

**What kinds of answers do reworked experiments provide?**

Given the constraints described above, what are the outcomes that might reasonably be expected from close study of experimental practice and the reworking of experiments? Although the desirability of doing so is clear from the arguments above, can it ever be possible to really regain the intangible elements of experimental practice given our socially constructed selves and our lack of craft skill? Historians in different fields have undertaken these kinds of studies with a variety of results. Bilak et al. suggest that reconstructions of early modern recipes were not useful as "test cases". They should not, they argue, be relied on to provide answers or verify hypotheses. Instead, engaging with materials encouraged them and their students to

> think with materials [and to…] critically reflect, from a maker's perspective… Our research in materials suggests the inadequacy of some modern classifications for understanding not only the variety of that early modern material world but also the ways in which practitioners thought with and through their materials and experiences to form hypotheses, taxonomies, and knowledge systems.
>
> (Bilak et al., 2016, pp. 46, 51)

This reinforces the claims earlier in this chapter that a small amount of material engagement can provide the historian with new ways of understanding the historical evidence they have access to, whether as a connoisseur or, as suggested here, enabling critical reflection on their own historiographical frameworks. For, just as the blind man's cane structures the way he thinks, our own materials of study must structure our ways of thinking and producing history.

Bilak et al.'s emphasis on learning about "Thinking with materials" as a valuable outcome of engaging with experiments and descriptions of experimental practice also encourages thinking about the labour and materials necessary for the development of apparatus. This echoes Appadurai's encouragement to study commodities in his approach to producing object biographies (Appadurai, 1986). Pamela Smith instead emphasises the availability of skills, highlighting the importance of makers and craftsmanship in studies of material culture:

> the skill of makers must be taken seriously as a form of knowledge and that collaborations between makers and scholars are one way to break down the extremely pervasive and long-enduring partitions between mind and hand and between making and knowing.
>
> (Smith, 2016, p. 5)

An appropriate rewording for historians of science might be: the skill of experimental practitioners and instrument makers must be taken seriously as a form of knowledge. The outcomes of working with experimental apparatus might be more nebulous than answering concrete questions, although those answers can occasionally be an outcome. However, just as a historian who spends long periods working in archives learns the small clues in handwriting that tell them something about an author's mood or circumstances, a historian who engages with the tangible aspects of an experiment can develop similar abilities to tell something about the practitioners and objects they are studying. In neglecting the material aspects of scientific enquiry, historians of experimental science overlook substantial and rich sources of evidence.

Despite the reluctance of Bilak et al. to call on historical reconstruction to provide specific answers, there are plenty of examples where material engagements have been asked to do precisely that. Doron Swade oversaw the construction of two versions of Babbage's Difference Engine No. 2, designed in 1847-49, but not built in Babbage's lifetime. The project sought to establish whether the failure of a machine to be built in Babbage's time was due to the limitations of nineteenth century engineering, as asserted by almost historical accounts pre-dating Swade's work. The project set out explicitly to ask, could the Engine have been built in Babbage's lifetime? And if so, would it have worked? The questions at hand required the makers of the construction to use only techniques that would have been available in Babbage's time. This they did, and the results can be seen at the Science Museum in London and the Computer History Museum in Mountain View, California. Two fully functioning difference engines were built, and the assumptions in those earlier historical accounts were shown to be incorrect (Swade, 2011). In a contrasting example, where original apparatus still exists as a reference, Peter Heering reconstructed Coulomb's use of a torsion balance, concluding that it was not possible to obtain results that fit the inverse square law of electrostatic force using the balance unless a Faraday cage was also employed. However, the Faraday cage was not invented until several decades after Coulomb had produced his results, calling into question the veracity of Coulomb's claims (Heering, 1994). Heering's work was challenged in 2006 by Alberto Martinez who carried out his own reworking of Coulomb's experiment and claimed that Coulomb could have produced the results he reported and that

they appeared typical in the context of his own results (Martinez, 2006). However, to reach that conclusion, Martinez substituted some materials in the apparatus, in particular replacing the needle in the experiment with a plastic needle. This overcame a problem with a synthesized shellac needle that "sagged a bit too much and another [that] reacted to the charge of pith balls", and simultaneously opened him up to the criticism that his experiment was not a faithful reproduction (Shech & Hatleback, 2014). Coulomb's electric torsion balance experiment has still not been successfully reworked following Coulomb's descriptions in detail. As Shech and Hatleback suggest, "we must keep learning from twisting wires and melting shellac" (Shech & Hatleback, 2014, p. 33).

Although the examples above produced answers to questions from their material work, albeit not necessarily conclusively, they also emphasise the role historical reconstruction played in developing their skills as experimenters. It led to a greater understanding of the layers of craftsmanship and expertise that went into building their apparatus, and the questions that are raised by engaging with materials and objects as well as textual sources. Part B of this chapter sets out the methodological approach I have taken in reproducing Ohm's apparatus. By looking closely at the apparatus Ohm described, questions are raised, and sometimes answered, about: Ohm's skills as a maker and experimenter, the practicability of his experiment, the commodity networks he had access to, and his particular theoretical leanings. Some of these questions are not raised by reading only the text-based sources, and they certainly are not answered by those sources.

# PART B

> … it will take a long time before I have come to terms with the cabinet; there are so many repairs necessary, so many additions necessary, and so many new things to wish for that I see no end to the thing. What I love most about it is that I am almost forced to deal with physics, and so gradually expand my knowledge of this science; and nothing contributes more to this than if you have to take care of your instruments yourself, or even have to make them yourself.
>
> (Letter from Ohm to his father, 12 May 1818 in Nürmberger, 2002, p. 199)

As Ohm observed, working with, maintaining, and even designing and making scientific instruments leads to a close understanding both of their qualities and the scientific phenomena they are used to study. In Chapter 1 we saw that there are many interpretations of Ohm's apparatus available to look at, and that the process of looking closely at objects has greatly aided my understanding of Ohm's experimental setup. The following chapters turn to my own reconstruction of Ohm's apparatus and investigate what can be learnt from the act of interpreting and building an instrument. Reconstructing Ohm's apparatus as a physical object forced me to consider questions that trying to undertake the same reconstruction process as a thought experiment would not. However, as the following chapters show, a reconstructed instrument is necessarily different from the original. In this case many parts are under-described and require imaginative design solutions, some materials are simply not available as commodities today but were available to Ohm, and some skills were once commonplace but now have to be learnt anew.

The approach to reconstruction I have taken in this project follows closely from the methodology for reworking experiments developed at Oldenburg University, introduced in Chapter 1 as requiring three interwoven steps:

1. Reconstructing the apparatus or experimental setup,
2. Re-enacting the experiment,
3. Contextualising the experience.

While using this framework might give the impression that my methodology is the same as those that have gone before, there is a difference in emphasis. In my work, the emphasis is equally strongly on the process of interpreting and reproducing the material object as on operating it. The focus on manufacturing led me to consider carefully the different types of

expertise embodied within the apparatus, whether related to its manufacture or to theories and concepts embedded within particular component parts of the instrument. In contrast, for many of the researchers involved with the Oldenburg Group, and those that have since followed their methodology, the emphasis is typically much more strongly on the process of re-enacting the experiment and using the re-enactment to make sense of what have variously been described as the embodied, tacit or gestural knowledges required to operate the instrument.

In the first part of this chapter, I argued that the form and use of material things, especially experimental apparatus, has a structuring effect on the cognition of an experimenter. In reworking Ohm's apparatus, it became clear that commodities, standards, manufacturing techniques, skills and knowledges have a structuring effect on the production of material apparatus. Stitching the two arguments together: a wide network of influences directly affects the form of experimental apparatus, and the form of experimental apparatus directly affects the experimenter and their development of concepts, therefore we can trace threads between the network of influences on scientific instrument development and the development of theories and concepts in the history of science. This theme will come up repeatedly in the following chapters. As well as supporting a historical argument, this line of reasoning also bolsters the argument that my attempts at a twenty-first century reworking of Ohm's experiment can never be understood as an "accurate replication" of his nineteenth-century experiment. This chapter, however, is primarily concerned with an introduction to my reconstruction project, which resulted in the production of a new interpretation of Ohm's apparatus, which I refer to as the Connelly apparatus (Figure 20).



**Figure 20.** Connelly apparatus, photograph taken on 10 January 2022 by the maker before the instrument was delivered to me for operation. (Image courtesy of Martin Connelly.)

**Collaboration and communication**

Historians of science have long been concerned with the invisible labour that sits behind much scientific activity (see Shapin, 1989). In Ohm's case we have a fleeting reference to the local mechanic Herr Mauch, and it is also clear from Ohm's correspondence with his father that their relationship played an important supporting role in his work. Similarly, it was clear to me that I would need to collaborate with at least one expert maker to successfully produce an operable piece of apparatus, and throughout my research I paid special attention to the dialogue between myself and the makers and suppliers I worked with. For my project I have, like Ohm, collaborated with my father, Martin Connelly. This is behind my decision to refer to the "Connelly apparatus", rather than the "Cambridge apparatus", in recognition that it was a collaboration between two people and not my production alone.

The decision to work with a family member came after a lengthy period of seeking a more independent collaborator, without success. The principal challenge was that there are very few commercial makers working today with a suitable range of skills. What this project demands, however, is more than skills, it also requires the maker to have an interest in historical manufacturing so that they understand the importance of working with techniques appropriate to Ohm's time period. A full discussion of my approach to identifying a suitable maker is given in the Appendix (Appendix, p. 232). On reflection, the opportunity to work with somebody whose primary interest in the project was not commercial, and with whom I could communicate freely and regularly, was a significant advantage.

My father worked as an engineer designing tooling for the manufacture of gas turbines, and in recent years has designed and made a number of custom parts for a variety of projects. For example, he has made bespoke parts for the current world record holding monowheel team. Throughout the design and manufacture of the reconstructed apparatus I maintained a reflective diary, taking particular care to consider how I have had to modulate my language and explanations when working with a non-historian engineer in order to collaborate effectively and convey the requirements of the project, transposing between historical understandings and contemporary pragmatism. It has been interesting observing the ways of communicating that are meaningful to a maker trained in twentieth century technical drawing techniques and computer aided design (CAD), and considering how that kind of training provides structure to ways of thinking and conceptualising a written description. In addition to thinking about my own communication with a maker, which was strongly two-way throughout the design and production of our apparatus, this reflective process has led me to consider Ohm's relationship with Herr Mauch, his own father and other friends and correspondents he may have discussed his work with. As Chapter 1 revealed, Ohm appears to have been a capable metalworker himself, which may well have made some of his communication with his local makers more fluid than it might otherwise have been.

Just as I have had to modulate my language in working with a maker, within this document I have had to modulate my language as I navigate between, on the one hand, an appropriately disinterested, third person, past tense account of historical events as expected of good academic writing, and on the other, my account of my own experimental work in which I am an entirely interested party. It would have been possible to write this account in the mould of a typically scholarly text, removing all trace of myself and my father by strictly adhering to the use of the third person. This was the approach taken by Staubermann in his 2004 account

of recreating photographic practice of the 1880s, though it is notable that two years later in his 2006 account of reconstructing F. W. Bessel's early star chart observations he introduced his actions in the first person (Staubermann, 2004, 2006). In almost total contrast to Staubermann's academic style, Elizabeth Cavicchi's "witness account" of being a participant in Peter Heering's reworking of solar microscope projections is very informal. She reports her expectations and feelings, her frustrations at the weather delaying the experience, and the dappled play of light on her fellow participants within the solar microscope (Cavicchi, 2008). Of the various texts concerning reconstructions of experiments consulted for, Adelheid Voskuhl's is most explicit about wrangling with how to understand and present her practical reworking; she describes her reworkings of Herschel's actinometer observations unflinchingly in the present tense, making it very clear which parts are her actions and which are the actions of past actors (Voskuhl, 1997, p. 354). My own choice has been to relegate the majority of my first-person account to the Appendix, which was written as part of the process of interpreting and manufacturing the Connelly apparatus. Throughout the Appendix, I typically use the first person, or on occasion refer to "we", by which I mean me and the instrument's maker. Giving myself and the maker agency in the account is important as I seek to articulate the parts of Ohm's account that are absent from his texts due, in part, to the academic style of writing he adopted. In contrast, the style of this main body of the work maintains more of the distance between source and analysis that is expected in scholarly work. It treats the work reported in the Appendix as a source, in much the same way that Voskuhl argues that a performance can be treated as a source. She suggests that the principal challenge to overcome in her work was to shift the object of research towards an analysis of the "historical context where *documents and practices* come into being" (Voskuhl, 1997, p. 340, emphasis added). For my purposes, I also include an analysis of the historical context where *objects and apparatus* come into being.

**First steps**

During initial discussions with the maker it became apparent that we needed a common language. Ohm's description of his instrument is written in lengthy prose, accompanied by two diagrams, neither of which have dimensions marked up. The maker found this difficult to interpret, and asked if a CAD drawing could be provided. To this end, I set about learning CAD in order to start building a 3D model (Appendix, p. 234). Reflecting on this need to speak a maker's language, almost before the project had started, poses a question about how

Ohm and his contemporaries related to their makers. In Ohm's case, we can see from his correspondence with his father that he understood technical apparatus, and actively undertook manual work:

> [I] also acquired a lathe, a vice with accessories, and the like, for my own amusement. (By the way, turning in wood is pretty good)
>
> (Ohm writing to his father on 12 May 1818, in Nürmberger, 2002, p. 198)

and

> I now have a complete set of tools... Filing and setting straight is easy with new files. I can drill if the thickness does not exceed half an inch, as if it were pierced with the thinnest sewing needle, and iron as well as brass.
>
> (Ohm writing to his father on 4 April 1822, in Nürmberger, 2002, p. 223)

Ohm also took the time to visit the local factories, reporting that he had already seen "steam machines, spinning machines, hydraulic presses, iron-cutting machines", and hoped to see many more (letter of 23 September 1821 in Nürmberger, 2002, p. 218). Given a working knowledge of manufacturing, and especially metalwork, it seems unlikely that Ohm would have needed to develop new skills or language to communicate effectively with local mechanics to build his instrument. In contrast to Ohm's likely ease of communication, I was translating prose from German to English, then interpreting the prose and re-presenting it in CAD drawings, before sharing the drawings with the maker to start our conversation in earnest.

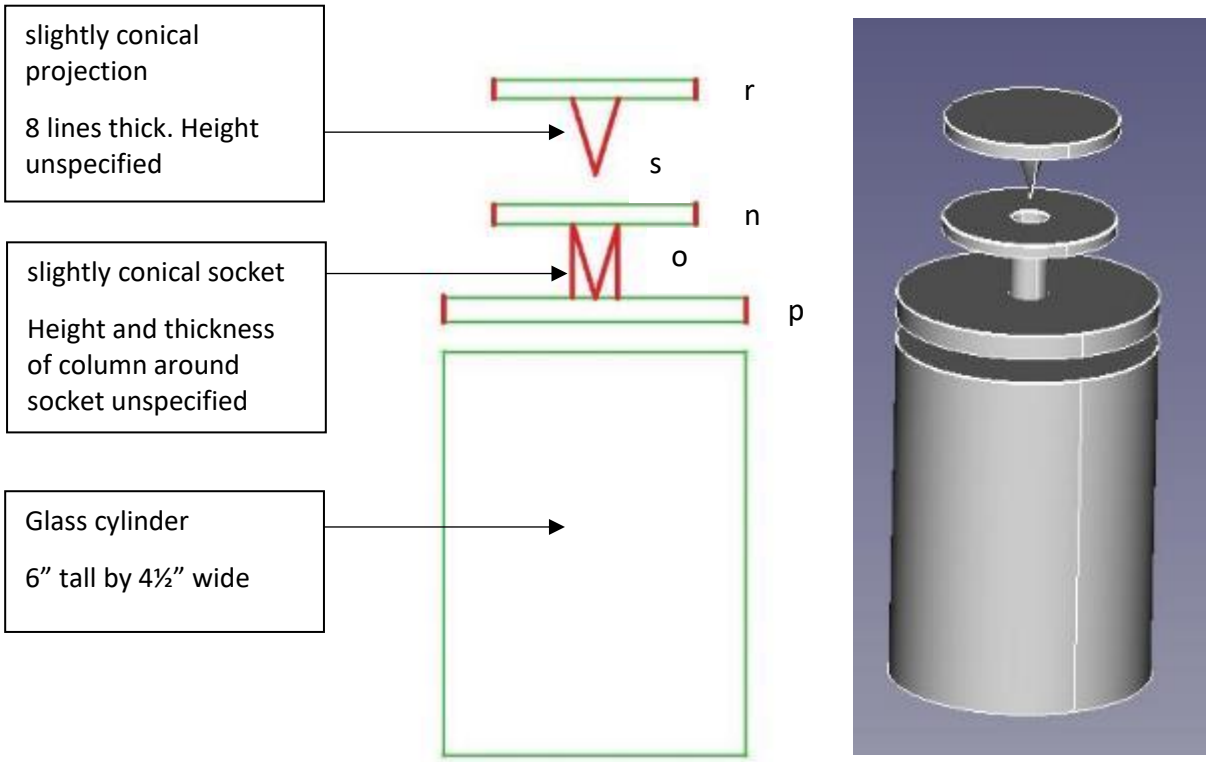



**Figure 21.** Representation of known and speculative measurements in Ohm’s apparatus. Green indicates a known measurement, and red an unspecified measurement. To the right is a three-dimensional rendering of the drawing.

The work of producing a CAD drawing immediately drew attention to two difficulties. The first was that although many dimensions were specified in Ohm’s text, there were also many that were not. Figure 21 was an early attempt to not only translate Ohm’s writings into a CAD drawing, but also to highlight the know dimensions in green, and the unknown dimensions in red. The form of some of the micrometer parts developed further as our project progressed, but the ambiguity over a range of dimensions remained. The second difficulty was that one of the first tasks when entering information into CAD software is to define dimensions. Ohm gave his dimensions in lines and inches, which at that time were not standardised. Some informed speculation was required to set a conversion from Ohm’s inches to today’s standard units. An 1830s publication provided measures from a range of different locations in the German lands, including defining the Cologne foot as 287mm in length, and indicating that dividing it into twelve inches, and then each inch into twelve lines was most

common (Niemann, 1830, p. 101). This suggested a conversion of 2mm per line and 24mm per inch (Appendix, p. 234).

These two examples, almost from before the conversations with the maker had really begun, show the role of the historian as one of translator. What was striking from my own reflections on the process was that before running into these difficulties I had believed I had a good, thorough understanding of the instrument. I had looked at the diagram, read Ohm's prose and looked closely at some other reconstructions of the apparatus. Nevertheless, on seeking to define its form well enough to reconstruct it, it became clear that my initial understanding was superficial and riddled with gaps. The following chapters, together with the Appendix, provide a detailed account of the gap-filling undertaken in the production of the reconstructed apparatus, through informed speculation and pragmatic decision making.

**The role of iteration**

The majority of my discussion of operating the Connelly apparatus is presented in Chapter 6. However, in this introduction to the process and methodologies of reconstruction, it is important to stress that there were moments of operating individual component pieces, or partial assemblages of parts of the instrument throughout its production. For example, the torsion balance was only semi-complete when the instrument maker and I tested how it behaved when a magnet was introduced into the setup (Figure 22). More detail is offered in the Appendix, but in even a small number of interactions outside the confines of a formal "experiment", it was clear that our shared understanding of how the torsion wire behaved in a magnetic field was enhanced by what I have come to think of as "focused play" with parts of the instrument. By play, I mean manipulating the instrument without any clearly defined goals, almost at random, while closely observing its behaviour to build up a picture of how it responds to different actions. Tellingly, my notes taken at the time and recorded in my reflective diary were quite scant:

> Suspended a two-part magnet from the goldlahn, tested aligning torsion balance with meridian and then deflecting the magnet with an additional magnet outside the apparatus and using the micrometer to zero.
>
> (Connelly, reflective diary, 26 June 2021)



**Figure 22.** Testing a semi-complete torsion balance, in the development of the Connelly apparatus (26 June 2021).

Despite this slim written record, a few days, and even weeks later, my sense of how the balance worked, and of the calibration steps that we might find ourselves undertaking in operating the complete instrument, were much more nuanced than before. The activity also aided the maker's understanding; he wrote a few days after the photograph in Figure 22 was taken to say:

> I think that what we were doing with our apparatus on the table over the weekend gave a good reason why [the flat torsion ribbon was helpful for Ohm]. You can tell when a flat ribbon is not wound up, it is a lot harder with a round cross section to tell if there is any twist in it at the start of the experiment and that it has returned to the original position.
>
> (Correspondence with instrument maker, recorded in reflective diary, 1 July 2021)

This focused play with the apparatus also served another purpose, in contributing to the iterative process of designing the instrument. Throughout the process of production, together with the maker, the design gradually developed as unknown details were filled in. We were unable to produce a complete design from Ohm's written description at the outset, and instead worked in phases, completing sections of the instrument before reflecting on progress and making the next set of necessary decisions. In Part A of this chapter, I cited Lehmann's work challenging the idea that artists have a fully formed piece of work in their mind which they then set about imposing on passive matter. In the case of a reconstruction like this, one

might assume that it is one instance where you do start with a fully-formed design in mind. However, this experience showed that even with a good sense of the finished product there is an iterative process in which your design development is guided by the materials. It was only after the focused play described above that together we felt able to understand where a piece of silk mentioned by Ohm might have been placed (Appendix, p. 249). It also helped us to develop our interpretation of the base of the apparatus, given our new understanding of how easily different parts of the instrument might need to be moved for calibration, and an understanding of where the person making the observations might need to stand. This same iterative process continued even once operation of the experiment began, as I developed routines and habits to establish that the instrument was stable and ready for use, and began to standardise what I recorded in my notebook for each series of observations. Each time I ran the experiment, the peripheral activities related to filling and lighting the burner or calibrating the instrument in the first place became second nature. As I shed those practical concerns, my attention became more focused on the observation itself.

**Concluding remarks**

The model of reconstruction developed by the Oldenburg group provides a good basic framework, however in my project I have deliberately given more attention to the process of manufacturing the reworked experiment, and reflections on how my own thinking was continually shaped and reshaped by the need to consider the form and operation of the material object. My motivations are subtly different from the acquisition of gestural knowledges suggested by Otto Sibum. While I am enthusiastic about better understanding Ohm's gestural knowledges and skills through the acquisition of some of my own, I am also curious about how working with materials shapes my historiographical practice, and helps me to discard my twenty-first century understandings of how electricity works.

The work started by the Oldenburg group has been continued by Peter Heering in Flensburg, where he continues using historical experimental apparatus to train teachers in tandem with his and his group's research into historical experiments using replication methodologies. In his recent work he has shifted away from the emphasis on trying to "check" the results of historical claims, and instead he focuses on understanding the prerequisites required for an experiment to work in such a way that the published result might have been arrived at. They might, he says, be related to the apparatus, to the materials used in it, or to small components or actions that initially seem irrelevant. He continues, noting that these interactions with the

experiment might lead to a different view of the sources, and in turn lead to the identification of further sources or influences upon the historical actors. He particularly emphasises experiment requires both suitable apparatus and an understanding of how to ensure it is stable and can be controlled by the experimenter. Finally, Heering notes that the person analysing the experiment is socialised differently, and that our knowledge of how to address problems in experimental apparatus cannot simply be transferred to the historical person (Heering, 2022, pp. 3-4).

My own methodology is very much in sympathy with the way that Heering's approach has developed. In particular, an emphasis on the historian acting in a "self-reflective manner" is at the heart of both our approaches to experimental reconstruction (Heering, 2022, p. 4). By continually reflecting on the process of production and design, I gained insight into how working with material objects shaped my thinking. This began long before reaching the point of finishing the design and manufacture of the instrument, let alone operating it. For example, through the co-design of the instrument, iterated with focused play with the components that make up the instrument, I developed visual and conceptual acuities that enhanced my ability to recognise and understand other experimental apparatus. This was true whether viewing a physical object in a museum, a photograph in a museum catalogue, or looking at drawings or written descriptions of experimental setups in published papers contemporaneous with Ohm's work. As evidenced by my recognition of the similarities with Ampère's experiment (Chapter 5), this can lead to novel insights from the paper record, as well as a deeper understanding of the material culture of science. The process of continual reflection also enabled me to observe the ways that my understanding of what was happening within the experiment shifted as new elements were added to the apparatus. This supports my analysis of Ohm's shifts between different phases of work, which are characterised by different conceptual framings and, in one case, happen in tandem with a transition between experimental setups. However, where I differ from Heering is that my emphasis is as much on interrogating the process of producing the material apparatus as on operating it. Chapters 3, 4 and 5 are primarily concerned with understanding the knowledge and networks that can be found embodied in the different component parts of the object, before finally in Chapter 6 reflecting on my experiences of trying to operate my apparatus.

My final observation, which becomes clearer with each chapter of this thesis, is that it is impossible to faithfully replicate Ohm's apparatus. Not only can I not think like Ohm, for the

reasons explored in part A of this chapter, but I cannot reconstruct the commodity networks that led him to purchase copper that is different from today's copper; I cannot reconstruct a regulatory system that enables unencumbered access to mercury, and its associated risks; and I cannot reconstruct the physical form of the apparatus precisely from the gaps in Ohm's descriptions. These are just a few of the examples that will come up later, and all contribute to my view that the Connelly apparatus, in common with the other apparatus described in Chapter 1 cannot be viewed as an accurate replication of Ohm's experiment. Nevertheless, they provide ways of accessing his work that are not available from the textual record alone.

# 3. Conducting Wires

## PART A

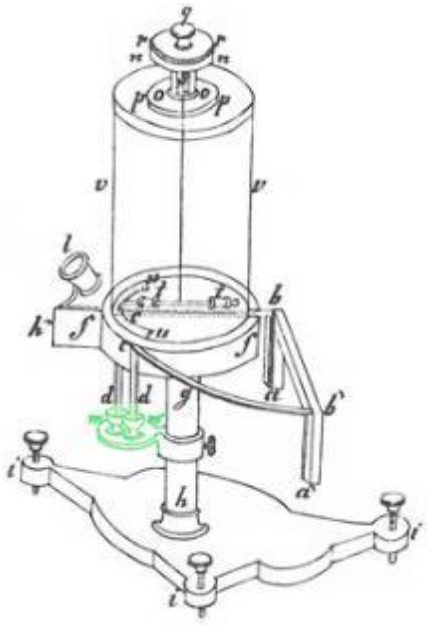


**Figure 23.** The liquid conductors in Ohm's 1826 experimental apparatus

This chapter is the first of three to look in detail at particular components of Ohm's 1826 experimental apparatus. Ohm's test wires were inserted into his apparatus by means of two mercury-filled cups, and were his means of varying the conductivity within his experiment (highlighted in green in Figure 23). It might seem surprising to focus on conductivity, rather than its inverse, resistance, given that Ohm's name is now closely linked with the concept of resistance (or *Widerstand* in German). However, he and his contemporaries thought in terms of conductivity at least as often as they did resistance. Curiously, Ohm carefully avoided the term *Widerstand* in his 1827 book, although it does appear in some of his earlier writing. Instead, he used the phrase *reduziert Länge* or "reduced length". This concept is introduced in this chapter, and discussed further in Chapter 6. Part A of this chapter explores three main questions. Firstly, what was understood about electrical conductivity when Ohm was working, and how did that influence his work? Secondly, how did Ohm work with test conductors in his experiments, particularly as his experimental setup developed over time? Thirdly, what was the interplay between Ohm's experimental changes and the changes in his conceptual understandings of conductivity?

This opening section on understandings of electrical conductivity also presents a number of claimants to priority over Ohm. Several writers have claimed that some of Ohm's predecessors either discovered what we now call Ohm's law before Ohm himself, or that they came very close. These claims typically focus on work to understand a theory of conductivity. James Clark Maxwell claimed that Henry Cavendish had provided "the first experimental proof of what is now known as Ohm's law" (Maxwell's footnote in Cavendish, 2010, p. 333). Atherton and others have claimed that "Sir John Leslie… all but stated Ohm's law in a paper written in 1791 but not published until 1824" (Atherton, 1986, p. 468). Finally, in his overview book *The Electric Current,* Turner argued that it was unfortunate that Davy "did not extend his researches further, for he approached very near to the conceptions of resistance and electromotive force given to the world a few years later by Georg Simon Ohm" (Turner, 1927, p. 76). As well as these apparent formulations or near-formulations of Ohm's Law, around the time that he published his 1825 "Preliminary indications" paper, Ohm became aware that Barlow and Becquerel were carrying out detailed research into the nature of conductivity (Ohm, 1826a, p. 139). These claims are now discussed in more detail, before reflecting on the work Ohm and his contemporaries were doing to understand the nature of electrical conduction and resistance in metals.

**Cavendish**

In their biography of Cavendish, Jungnickel and McCormmach accept Maxwell's claim that Cavendish's work could be interpreted as an early description of Ohm's law. However, Isobel Falconer has also written about the role Maxwell played as editor of Cavendish's papers and is much more critical of the claim (Falconer, 2014; Jungnickel & McCormmach, 1999). Before trying to arbitrate on these positions, it is necessary to understand the work that Cavendish was doing in the 1770s. This work predated Volta's invention of the pile by several decades, and the only electrical effects readily available to Cavendish were discharges of static electricity, principally from Leyden jars. To understand the claim that Cavendish had demonstrated Ohm's law experimentally, it is helpful to know how his experiment was set up:

> [Cavendish's] technique was to insert himself into the electric circuit by holding a piece of metal in each hand and touching one piece to the knob of a Leyden jar and the other piece to one end of a tube containing a conducting solution, while the other end of the conducting solution was connected by a wire to the other side of the Leyden jar… For the purpose of comparing one conducting solution with another, he

> prepared a series of six equally charged Leyden jars, which he then discharged alternately through the two solutions (and himself), adjusting the wire in one of the tubes until the shocks of the two solutions were as nearly equal as he could judge.
>
> (Jungnickel & McCormmach, 1999, pp. 244-245)

Cavendish's tabulated results report his findings. They are a curious mixture of qualitative, for instance "seemed rather less" or "seemed rather greater, but doubtful", and quantitative, for example a saturated salt solution conducts "1.92 times better" than a sample of fresh water (Cavendish, 2010, p. 332). Cavendish took the observations he made by using his body as a sensing instrument, with all the inherent challenges that entailed, and attributed numerical values to his observations. From those numerical values he made mathematical claims, for example:

> "Therefore resistance is as $\frac{1.1829}{1.2122} = .976$ power of velocity",

and

> "Therefore resistance is directly as velocity"
>
> (Cavendish, 2010, pp. 332, 333).

It was this second claim that Maxwell followed up with a footnote, arguing that it demonstrated Cavendish's priority over Ohm. Maxwell interprets "power of velocity" as analogous to current because it suggests the rate of flow of electricity. However, in that case we should expect it to be inversely proportional to resistance, as suggested by our modern formulation $current = \frac{potential}{resistance}$, rather than directly proportional as suggested by Maxwell.

Falconer has undertaken a detailed study of Cavendish's work and the claims Maxwell makes relating to Ohm's, and there are a few key points to draw out from her research. Firstly, Maxwell took interpretive liberties with Cavendish's work to make the analogy with potential, current and resistance work. Secondly, Maxwell overlooked errors in Cavendish's calculations which invalidate some of the claims. Finally, Falconer clearly shows that some of Maxwell's editorial decisions supported arguments that he and William Thomson were making elsewhere about the importance of mathematics in electrical theory, particularly in the context of the rapidly expanding electric telegraph industry and ongoing standardisation work (Falconer, 2014, pp. 15-17). To summarise, Maxwell's claim that Cavendish had priority over Ohm's law of conductivity is problematic. Cavendish's work was not truly quantitative, though he did ascribe numerical values to some of his observations.

Furthermore, in order to make Cavendish's experimental work fit with Ohm's law, Maxwell had to interpret Cavendish's writing anachronistically using conceptual apparatus that Cavendish simply did not have access to in the 1770s. Fifty years later, when Ohm was doing his work, the discovery of the pile and the well-known technologies for manipulating current electricity had moved electrical studies far from the work that Cavendish had been doing.

**Leslie**

Our second possible claimant to priority over Ohm is Sir John Leslie. This claim stems from a paper published by Leslie in 1824 in the *Edinburgh Philosophical Journal*, though, as Leslie writes in his introduction, it is a printing of a paper written in May 1791 "with merely slight verbal correction; but some illustrative annotations are subjoined in Italics" (Leslie, 1824, p. 2). This indicates that Leslie had the opportunity to comment on and amend the paper in such a way that brought it up to date with the state of knowledge in the 1820s. Instead, the paper and its claims are firmly based in the electrostatic theories of the 1790s, with no acknowledgement of the then-recent electrostatic advances made by Coulomb, let alone Volta's invention of the pile or the electrodynamic theories that were prevalent by the 1820s. It is perhaps notable that Volta, too, used an electrostatic framework to develop ideas similar to Ohm's Law on the rate of flow of electricity through a conductor, though constrained by his theoretical framework, and without the mathematical framing offered by Leslie (Teichmann, 2001).

The focus of Leslie's paper is on exploring the difficulties in ascertaining whether electricity was a *substance* or a *quality* held by all matter. This is similar to the discussions that were taking place around heat, which had been interpreted as both a substance in the form of an imponderable fluid or a quality in the form of the motion of bodies (for a discussion of Leslie's work on the nature of heat, see Olson, 1970).

In his paper Leslie describes a number of experiments that underpin his claims. They centre on understanding conductivity, for instance:

> Thus, if a slip of paper, sufficient to discharge a Leyden phial in about a quarter of an hour, be rubbed slightly with charcoal dust, it will perform the effect in ten seconds. If now reduced to one-half its breadth, it will require about double the time; and, if again shortened to one-half its length, the time will be nearly the same as the first. If the charcoal dust be gradually strewed thicker, the discharge will become more and more rapid, til the interval can no longer be distinguished… By comparing such

> experiments, we shall find, that the different substances differ astonishingly in their conducting qualities. Thus, when the length, thickness, and other circumstances remain the same, the electricity is transmitted some hundred times faster through linen-thread than through firewood; several hundred times faster through the metals than through water, &c.
>
> (Leslie, 1824, pp. 32-34)

Not only does Leslie refer to transmission and conductivity, but he also uses the word resistance in passing while discussing the different "celerity of transmission" of different substances. His interpretation of the range of conductive qualities is that the "cohesion of particles" is important for the ability of a substance to transmit electricity. He uses his observations to argue that "Electricity is a *state* or *condition* in which every species of matter is susceptible", rather than a substance or fluid (Leslie, 1824, p. 38, emphasis in original). Although Leslie was working with electrostatics and in a different theoretical framework to his post-1800 successors, his experimental work to demonstrate the laws governing conductivity is impressive.

Leslie went beyond qualitative analysis of experiments and used mathematical tools including geometrical analysis to argue his point. This is important in considering whether or not his work influenced Ohm because Ohm also used geometrical analysis and presented several very similar diagrams in his 1827 book (Figure 24). Leslie imagined a conductor made up of sections with different conductive qualities, and used geometrical analysis to model electricity discharging through the conductor. Only three years after Leslie's paper was published, Ohm used an almost identical approach to describe what would happen in a conductor with a variety of sections of differing conductivity.

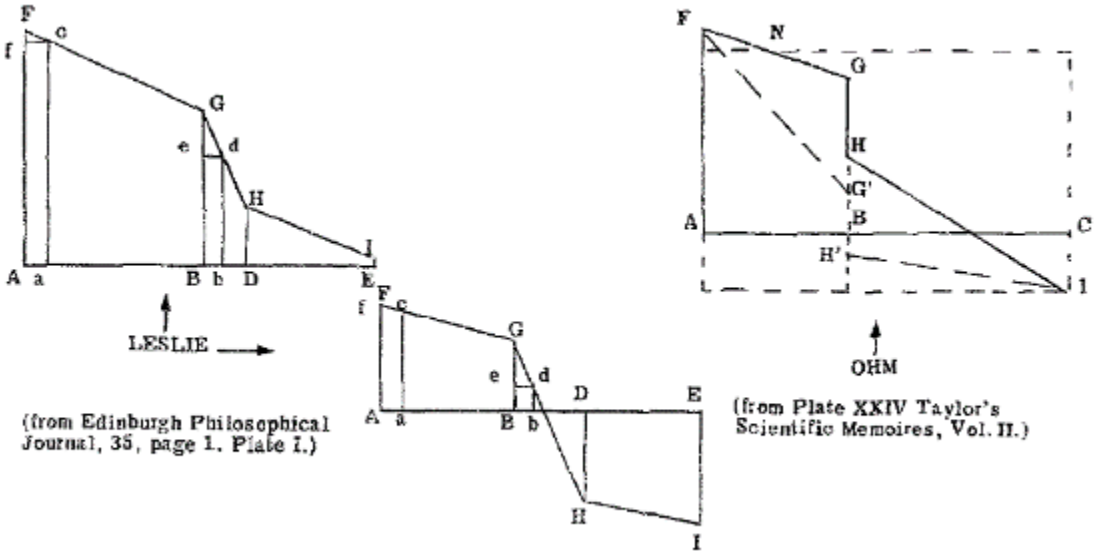



**Figure 24.** Leslie's geometrical analysis diagrams side by side with Ohm's to demonstrate the similarities in their work (Reprinted from Olson, 1969, p. 193 with the permission of AIP Publishing).

Taking a closer look at Leslie's diagrams, the *y*-axis in his diagrams indicates "intensity", and the *x*-axis represents a conductor made up of different segments. The boundaries of the segments are marked with lines *AF*, *BG*, *DH* and *EI*, and each segment has different qualities which affect conductivity. Finally, the different gradients along the line *FGHI* indicate the different rates of discharge in each segment, which Leslie showed varied depending on a range of factors that affected a segment's conductivity. Reading from left to right, then, we can see that line *FG* is at a relatively shallow gradient, suggesting that electricity discharged relatively slowly through this portion of the conductor, and therefore that it was a relatively poor conductor. In contrast, line *GH* is at a relatively steep gradient indicating electricity discharging more quickly through this portion of the conductor, which is therefore a relatively good conductor. Line *HI* is at a similar gradient to *FG*, suggesting that the two segments have almost equivalent conductivity.

The conclusions Leslie reached, and the similarities in his methods to those used by Ohm, raise two questions. Did Leslie successfully articulate what is now known as Ohm's law before Ohm? And secondly, was Ohm aware of Leslie's work? On the former question, it is helpful to refer back to Leslie's diagrams. If one chose to apply an anachronistic reading to this diagram, as Atherton has in suggesting Leslie "all but stated" Ohm's law, the

diagrammatic presentation above could be translated into a formula which reads: $I = \frac{V}{R}$ where *I* is the discharge or rate of flow of electricity indicated by gradient, *V* is the electrical potential indicated by the *y*-axis and *R* is resistance which varies in segments along the *x*-axis (Atherton, 1986, p. 468). However, it is clear that Leslie himself did not think with those concepts or terms, but instead his language and conceptual framework were firmly embedded in electrostatics. In the experiments described above, he discussed current flow as a type of discharge or leakage from a statically charged body. Furthermore, although Leslie seems frustrated by the long gap between writing his paper and finally having it published, it seems that neither he nor his peers recognised it as a ground breaking piece of work or they presumably would have put more energy into getting it published sooner.

With regard to the second question, a conclusive answer as to whether Ohm was aware of Leslie's work is elusive. Ohm referenced Barlow in his 1825 paper, demonstrating he had access to some work in English, and it is plausible that the *Edinburgh Philosophical Review* was available to him. Even if that was the case, it is not at all clear that Leslie's paper would have been an obvious choice for Ohm to read. From first glance it is very obviously written from an electrostatic perspective, with what would have been outmoded ideas and language in the 1820s. There is also nothing in Ohm's surviving laboratory notebooks to indicate that he had directly copied out any of Leslie's work, whereas there are other passages that are directly reproduced (for example Ohm copied out "On the Conductivity of Metals, by Becquerel", DM HS904, from p.2). In addition, Leslie was not well known for work on electricity, though his work on heat was highly regarded.

The remaining factor to consider is the close similarity between Leslie's and Ohm's intellectual approaches to understanding conductivity. Both were influenced by comparisons between electricity and heat, and both used geometrical analysis. In seeking to answer this question, Olson points to similarities in the two men's backgrounds: both were teachers of mathematics, and both wrote geometry textbooks. Furthermore, he argues, plotting the quantity of a substance, or the intensity of a quality, as a function of position had been a standard practice for at least three centuries. The coincidence of both men finding an analogy with heat can be explained away by recognising that Leslie had an early association with Joseph Black and John Playfair, while Ohm followed a range of people including Davy and Fourier who had also made comparisons between heat and electricity (Olson, 1969, p. 193). Furthermore, as Chapter 6 will argue, Ohm may also have been motivated to use a traditional

technique to communicate his very technical mathematical arguments to an audience with limited mathematical proficiency. These are all good points, and there simply is not enough information available to make a categorical judgement either way. What is evident is that there was also no outcry after Ohm's publication to suggest that anybody felt that Leslie had been wronged, underlining the likelihood that Leslie's paper was little known and certainly not influential. In summary, Olson's assessment that while Ohm may have been influenced by Leslie's work, it is more likely that his out-of-date approach did not feature in Ohm's reading around the topic, seems entirely justified.

**Developments in the 1820s: Davy, Barlow and Becquerel**

Our final claimant to priority over Ohm is Sir Humphry Davy, though this section will also look at a number of other people working on conductivity and resistance in the 1820s including Antoine César Becquerel and Peter Barlow, both of whom Ohm mentioned explicitly at the start of his 1826 "Announcement of Laws" paper. Keithley's wide ranging survey of the history of electrical and magnetic measurements also briefly refers to Johann Christian Poggendorff's experiments with wires of different diameters, and his finding that heavier wires gave greater deflections, and James Cumming's work investigating the effects of varying the lengths for the leads connecting a bimetallic source to his detector. In the case of the latter, Keithley nods towards the idea that Cumming could be considered to have priority over Ohm, writing "he found that, with a fixed wire to indicator spacing, the deflection of the needle indicated that the magnetic effect was in a nearly reciprocal relationship to the length of the leads. Cumming observed this effect about three years before Ohm published his findings" (Keithley, 1999, p. 78).

Davy, much like Leslie and Cavendish before him, spent time investigating the variables that affected electrical conductivity. Like Becquerel, and later Ohm, Davy reported that he had problems with the continual variations within the voltaic pile he was using. Despite the challenges he faced with his experiment, he was able to state that there was a direct relationship between the mass of a conducting wire and its conductivity:

> I found, as might have been expected, that the conducting power of a wire for electricity, in batteries of the size and number of plates just described, was nearly directly as the mass; thus, when a certain length of wire of platinum discharged one battery, the same length of wire of six times the weight discharged six batteries; and

> the effect was exactly the same, provided the wires were kept cool, whether the mass was a single wire, or composed of six of the smaller wires in contact with each other.
>
> (Davy, 1821, p. 411)

The passage above hints at another discovery that Davy highlighted in the paper, that the conducting power of metallic bodies varied with their temperature, becoming lower in some inverse ratio as the temperature was raised. Davy also went some way towards introducing the idea that the nature of the source of electricity affected the flow of electricity. This is a step further than either Cavendish or Leslie, though it falls some way short of being a complete articulation of the way current flows within a circuit:

> I made some experiments with the hope of ascertaining the exact change of ratio of the conducting powers dependent upon the change of the intensity and quantity of electricity; but I did not succeed in gaining any other than the general result, that the higher the intensity of the electricity, the less difficulty it had in passing through bad conductors...
>
> (Davy, 1821, p. 412)

In addition to any claim to priority Davy might have had over Ohm's electrical law, it is worth noting that links between heat and electricity were discussed in his paper. Davy comments that:

> Mr. Children, in his account of the experiments made with his battery of large plates has ingeniously referred the heat produced by the passage of electricity through conductors, to the resistance it meets with, and has supposed, what proves to be the fact that the heat is in some inverse ratio to the conducting power,

and later,

> This is an important experiment for investigating the nature of heat. If heat be supposed a substance, it cannot be imagined to be expelled from the platinum; because an unlimited quantity may be generated from the same platinum, i.e. as long as the electricity is excited, or as often as it is renewed. Or if it be supposed to be identical with, or an element of, electricity, it ought to bear some relation to its quantity, and might be expected to be the same in every part of the chain or greatest in those parts nearest the battery.
>
> (Davy, 1821, p. 414)

It is evident from the quotes above that Ohm and Leslie were not alone in considering heat and electricity to be analogous, and to look for ways that understanding one might help with understanding the other.

An additional refinement offered by Davy was the observation that, provided the wires were the same weight, it did not matter whether they were flattened or rounded. He took this to indicate that the surface area of the wire had no bearing on its "conducting power". Barlow and Becquerel both developed this work further in the early 1820s. Barlow's work stemmed from a project to devise an electric telegraph, but he was waylaid by needing first to understand the laws governing the "transmission" of electricity. His initial consideration was that if electric fluid dissipated somehow, the transmission should be stronger nearer the battery than further from it. He dismissed that idea after placing one compass more than 400ft from either end of a wire, and a second near one end, and found that both were "equally affected by the galvanic action" (Barlow, 1825, p. 109). Barlow dedicated several pages of his paper to describing his investigations into the effects of the length of the wire, and much more briefly comments that a heavier wire leads to greater deflection of a magnetised needle. However, the main thrust of his argument was to criticise the theory of imponderable fluids (Barlow, 1825, p. 112).

Meanwhile, Becquerel went beyond Davy's claim that the weight, and consequently surface area of the conductor was the relevant variable, to argue instead that the cross-section of the wires was the important factor (Becquerel, 1826, p. 426). Becquerel's setup was carefully designed to try to eliminate some of the problems he, and Davy, faced with continual variation of current in experiments using a voltaic pile. His solution was to insert two test wires into the experiment for comparison in order to establish "conductivity ratios", rather than studying individual test wires sequentially (Becquerel, 1826, p. 422). In practice that meant that any variation from the pile affected both test wires together, and any ratio between the two would remain consistent. By 1826 Ohm was aware of both Barlow and Becquerel's work, and by implication Davy's, which they both referred to throughout, and referred to them explicitly in his paper.

**Conductivity in Ohm's experiments**

From the context provided above it is clear that in trying to understand electrical conductivity Ohm was working in a popular field of research, and using similar techniques to his contemporaries. He also faced similar challenges to them. In particular, like Davy and Becquerel, he struggled with making an experimental setup incorporating a voltaic pile stable enough to make reliable observations. This will be investigated more thoroughly in Chapter

4. For the purposes of making sense of this chapter, there are two things it is useful to understand. The first is that after struggling with instability in his 1825 apparatus, with his 1826 setup Ohm achieved the reliability he sought. The second is that the behaviour of the apparatus changed as his setup changed. In 1825, Ohm observed a magnetised needle that oscillated wildly due to surges as the circuit was made and broken. Even when the apparatus had stabilised, the needle gradually moved back towards its starting position, suggesting there was a gradual reduction in the force causing it to deflect. In contrast, in his 1826 setup, the magnetised needle held its deflected position for as long as the temperature difference in the thermocouple remained steady. The strength of the deflection was affected by the length of the test conductor and the magnitude of the temperature difference at the thermocouple.

Not only did Ohm's experimental setup vary, but throughout the period 1825-1827 the language he used around conductivity and resistance also varied. One phrase that he used intermittently was "reduced length". He adopted the term most thoroughly for his 1827 monograph, *The Galvanic Circuit Investigated Mathematically*, opting to use "reduced length" in place of "resistance" or *Widerstand*, despite having previously referred to resistance in his 1826 "Announcement of Laws" paper. Ohm was not explicit about why his language changed, and there are several possible interpretations for his use of the phrase. One of Ohm's less famous publications offers some potential clues. In contrast Ohm's 1825 "Preliminary indications" paper sought to understand how varying the length of a test wire affected conductivity within a circuit. A later very short paper, "On the electrical conductivity of metals", demonstrated that Ohm was experimenting with changing a different variable; this time he varied the material that a wire was made of (Ohm, 1825c).[6] Again we have to wait until his 1826 "Announcement of Laws" paper for any detail about how he carried out his experiment:

> I took cylindrical wires of the same thickness and of different materials, put two at a time in the circuit, and shortened the one that most weakened the force until it had achieved approximately the same value as the other... In this way I arrived at the following ratios for the length of the various metals, where they have the same conductivity value:

[6] In this brief note Ohm also demonstrated further knowledge of the work of his peers, reporting a series from most conductive to least of copper, gold, silver, zinc, brass, iron, platina, tin, lead, and commenting that his series corresponded well with the observations of Martin van Marum, but deviated from John George Children's series, which he blamed on details in Children's experimental setup.

> Copper 1000, gold 574, silver 356, zinc 333, brass 280, iron 174, platinum 171, tin 168, lead 97.
>
> (Ohm, 1826a, p. 141)

Here we have a description of a physical reduction in the length of a wire, which enabled Ohm to describe a ratio of conductivity relative to an arbitrary conductivity, in this case of a length of copper. This method was repeated again with wires of the same material but different thicknesses:

> … I also took wires of the same material and of different thicknesses, within 0.12 and 1.40 lines, and proceeded with them exactly as I did with the determination of the conductivity of metals. Therefore, I came up with the following law: *Cylindrical conductors of the same type and different diameter have the same conductivity value if their lengths are in proportion to their cross-sections.* Barlow and Becquerel were also led to this law through their experiments.
>
> (Ohm, 1826a, p. 142)

Throughout these experiments, Ohm continued to work with the voltaic pile, and, through the two sets of observations described above, he appears to have found a similar solution to Becquerel in using a comparison between two conductors to overcome the instability of the pile.[7] For Ohm, the use of a physical reduction in length of pieces of wire translates directly into a mathematical understanding of conductivity, which feeds into his more complete mathematical work as discussed in Chapter 6. Later in this chapter, there is a suggestion that "reduced length" is means of describing resistance without recourse to problematic theory. Another possible interpretation of Ohm's use of the phrase is offered in Chapter 6, where it is suggested that length provided a useful and usable dimension for a measurement that did not have a unit of measurement (see p. 190). Possibly the more mathematical interpretation is a stronger candidate given that Ohm did not change his language until he wrote his 1827 monograph.

[7] Ohm had copied out Becquerel's paper of 31 January 1825 in his notebooks, however the note appears after an entry dated 4 July. Ohm's "On the electrical conductivity of metals" paper is dated 27 June, suggesting he arrived at his solution independently (see DM HS904, p. 2; Ohm 1825c).

*Conductivity in Ohm's 1826 experiment*

Ohm's detailed description of his use of conductors in his 1826 experiment is presented over the following pages, accompanied by my annotations (Ohm, 1826a, pp. 147-153).

> The ends of the conductors that were used for the experiments were immersed in the mercury of the bowls *m, m'*, over which a simple device was attached for safety, by means of which the ends of each conductor were always in contact with the mercury in the same way [Figure 23]. In addition, all ends of the conductors, insofar as contact with the mercury was to be concerned, were coated with resin, then the bases of the same were filed with a fine file and freshly submerged each time. A completely metallic connection of the individual parts is an indispensable condition for experiments of this type, otherwise no agreement between the observations is possible.

Although Ohm's new setup was much more forgiving than the earlier apparatus, the description shows that he continued to handle the conductors with care and consistency throughout the new series of experiments. His earlier finding that the diameter of a wire was important to its conductivity may have informed the decision to freshly file the wire end each time, while the other surfaces were coated in resin. Alternatively, this may have simply been a way to ensure a good surface between the conductor and the mercury.

> I had 8 different conductors prepared, which I will call 1, 2, 3, 4, 5, 6, 7, 8. They were 2, 4, 6, 10, 18, 34, 66, 130 inches long respectively, and 7/8 lines thick, and all cut from a piece of so-called plated copper wire and prepared in the manner described above. After the water had been boiled for half an hour, they were brought into the circuit one by one. Between two series of experiments on one and the same day, each lasting 3 to 4 hours, there was always a break of one hour, during which new, already heated water was poured in, which soon boiled, then the series of conductors came one after the other, but in reverse order, into the circuit. This is how I got the following results:

| Time of observation | Research Series | Conductor | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|
| | | 1 | 2 | 3 | 4 | 5 | 6 | 7 | 8 |
| 8 Jan | i | 326¾ | 300½ | 277¾ | 238¼ | 190¾ | 134½ | 83¼ | 48½ |
| 11 Jan | ii | 311¼ | 287 | 267 | 230¼ | 183½ | 129¾ | 80 | 46 |
| | iii | 307 | 284 | 263¾ | 226¼ | 181 | 128¾ | 79 | 44½ |
| 15 Jan | iv | 305¼ | 281½ | 259 | 224 | 178½ | 124¾ | 79 | 44½ |
| | v | 305 | 282 | 258¼ | 223½ | 178 | 124¾ | 78 | 44 |

A cursory look at Ohm's results reveals the basic pattern he found. A shorter conductor, denoted by a smaller number, resulted in a larger deflection of the needle in the torsion

balance, indicated by a larger number read off the instrument. Ohm speculated in the following section that some of the variability he observed may have been because the 8th and 11th of January were very cold days, whereas the 15th was a bit warmer. He also commented that the surge of force he had struggled with in his earlier experiments was absent with his new setup. Taking his tabulated numbers, Ohm established a new equation:

> The above numbers can be described very adequately by the equation
>
> $$X = \frac{a}{b + x}$$
>
> where *X* denotes the strength of the magnetic effect on the conductor, the length of which is *x*, but *a* and *b* denote constant quantities dependent on the exciting force and the resistance to conduction of the other parts of the circuit.

Ohm goes on to calculate figures using his observed results and the equation he has fitted to them, before using his findings to make predictions with a different set of conductors.

> … to that end I made 4 [conductors] in sequence, using 2, 4, 8, 16 inch long conductors a, b, c, d, made from the 0.3 line thick brass wire that had served me in my earlier experiments on the hydroelectric circuit; these gave in sequence 111½, 64¾, 37, 19¾ parts, while conductor 1 in the circuit gave 305 parts. If one looks for the lengths corresponding to these values according to the above equation, one finds 40¾, 84¾, 163½, 324, which numbers express in great unison that an inch from the brass wire equals 20½ inches from the plated wire.[8] After this preparation I put the 23 foot long conductor made from the same brass wire, and marked 5 in my notebook, into the circuit; it gave 1¾; and in fact you get this number almost exactly if you put 23, 12. 20½ = 5658 for *x* in the expression. From this it can be seen that this equation always adheres faithfully to experience until the force is almost extinguished by the resistance of the conductor.
>
> (Ohm, 1826a, pp. 147-153)

The accounts of Ohm's experimental work in this and the previous discussion of "reduced length" make a few things clear about Ohm's understanding of conductivity as he entered his second phase of work. Firstly, he repeatedly refers back to his 1825 work, including the short interim piece of work on conductivity in different metals, demonstrating that his knowledge and understanding came from extensive close working with a range of different metals. The paper presents a picture of a confident experimenter who is able to find solutions to

[8] Here again, we have an example of Ohm describing resistance in terms of length, or in this case a ratio of lengths. Again, this possibly provides a clue about the origins of his term "reduced length".

challenging experimental requirements, whether the requirement is ensuring a good connection between a test conductor and a cup of mercury, or ensuring that each end of the thermocouple is maintained at a constant temperature. This paper, and his earlier work, demonstrates Ohm's familiarity with the material properties of the experimental tools he was using. Across the two papers he writes about topics including the purity of the metals, and the coatings that they picked up in the process of wires being pulled. As an aside, amidst this wealth of detail Ohm makes no particular comment about his use of mercury for connections, suggesting it was common enough at the time to not be considered noteworthy. Secondly, Ohm was comfortable taking empirical observations and using mathematical tools to analyse and make sense of them. Although the full mathematical treatment of Ohm's law was not published until 1827, in his 1826 paper Ohm uses mathematical tools throughout the second half of his narrative, linking the mathematical descriptions to the empirical observations he has made as well as occasionally using mathematical descriptions in a more abstract manner. Chapter 6 offers a fuller assessment of Ohm's use of mathematics in his work.

Finally, having earlier in the paper noted his awareness and understanding of the work of a range of peers including Barlow and Becquerel, Ohm implicitly used the basic concepts of the emerging, shared understanding of how electric current behaved. His use of the pile was sufficient for him to trust that he was investigating similar phenomena to the researchers that came before and alongside him; his experiment was founded on the idea that a longer piece of wire is more resistant, or less conductive, than an otherwise similar shorter piece of wire; he recognised the importance of the cross section of a piece of wire in making a good connection; he understood that wires made of different materials or in different sizes have different values of conduction or resistance; and he recognised that differing temperatures between experiments could affect his results. In his writing, in common with his peers, Ohm flitted between conductivity and resistance depending on context, and he appeared to recognise the two concepts as interchangeable inversions of each other without any difficulty. Ohm's incorporation of this well-established set of rules describing the behaviour of electric current into the design of his experiment,was well-founded and uncontroversial.

**Changing conceptual apparatus**

This final section explores how Ohm's understanding of what electrical current was, as opposed to how it could be manipulated to flow more quickly or slowly, changed as he

moved from his first phase of work to his second. As the next chapter investigates, Ohm's changing experimental setup encouraged a different understanding of how electricity behaved. In this period, Ohm was notably silent on any theoretical ideas regarding the nature of electricity. He did not pronounce whether he thought it was a substance or a quality, or what underlying causes might govern how electricity behaved. There are times when theoretical leanings are implied, for instance Ohm referred to "contact electricity" in some of his texts, suggesting a voltaic leaning, but he did not make any use of that position. Though as previously noted, later in life he went on to vigorously defend the contact theory (p. 34). Instead, he described the observable behaviours of parts of his apparatus, which he argued were being acted on as a consequence of electrical effects. His approach was to describe the phenomena he was seeing with very little recourse to theory. Perhaps this explains why he wrote in terms of force (*Kraft*), rather than tension (*Spannung*). In so doing, he was referring to the empirical observation of the existence of force which could be detected in the form of the deflection of the magnetised needle. This, he understood, arose from the electrical effects in his circuit. Of course, this was not entirely theory-free. Ohm was by necessity invested enough in electromagnetic theory to suppose that the deflection of the needle was due to an electrical effect. Nevertheless, like the "concretizing" natural philosophers that Caneva describes preceding Ohm, in describing his experiments and the conclusions he drew from experimental practice, Ohm shied away from stating a theoretical position or overtly making claims about things that he could not observe. Where Ohm departed from the "concretizing" position was in a willingness to use mathematical analysis of his findings. Despite this reluctance to be explicit about theory, the combination of Ohm's experimental setup, and the way he drew his conclusions, betrays some of his shifting theoretical ideas.

*Current in Ohm's first phase of work*

In his first phase of work in 1825, Ohm described his experiment in terms of "loss of force". It was noted above that Leslie considered electric current to be a discharge or leakage from a statically charged body, and Heering et al. look to similarities with Coulomb's work on electrostatics, arguing that Ohm used Coulomb's third memoir on electricity and magnetism as a useful analogy (Heering et al., 2020, p. 24). This phrase, then, suggests an understanding of electricity leaking away from a source, aligned with electrostatic conceptualisations of how electricity behaves. While an electrostatic influence is one explanation for Ohm's use of "loss of force", another is that he was using language descriptively to communicate the

observable behaviour of the apparatus during his experiments. As he closely observed the magnetised needle, Ohm noted that the force that was causing it to deflect seemed to be continuously lost throughout the experiment. Furthermore, the rate that the deflecting force was lost at varied according to the qualities of the conductor being used. Paying particular attention to the effect, Ohm inserted the very long 75ft conductor *f* into the circuit, and observed that once the circuit had stabilised

> the micrometer showed 50 parts, [then] the needle went backwards gradually, but extremely slowly, and after more than a quarter of an hour, when the hissing [of the voltaic battery] had almost completely subsided, the micrometer showed 45.
>
> (Ohm, 1825a, p. 116)

We can see here that the angle of deflection of the needle gradually reduced over time. This observation, following the then well-accepted theory of torsion, leads to the conclusion that therefore the force deflecting the needle was gradually reducing over time. In the example above, this was very slow due to the long, and therefore very resistant, conductor inserted in the apparatus. With less resistant wires in the circuit, there was a greater reduction in the force.

Was Ohm's use of "loss of force" simply a description of the phenomena he observed, or was he influenced by electrostatic concepts of current leakage? To understand whether, and to what extent, Ohm may have been influenced by Coulomb's electrostatic ideas, it is helpful to understand Coulomb's work. The experiment Coulomb describes in his third memoir measures the loss of charge in statically charged pith balls over time. A reproduction of the apparatus is cared for by the Science Museum (Figure 25), and helps to illustrate Coulomb's description of his experiment in which two pith balls are charged. One is on the end of a fixed rod, seen here suspended from the lid of the lower part, and the second is on the end of a rod suspended from a fine metal wire, in turn suspended from the top of the instrument. The repulsive force of the two charged balls causes the suspended ball to move away from the fixed ball, in turn twisting the suspension wire. At the top of the torsion apparatus is a micrometer, which enabled Coulomb to twist the balls towards or away from each other by a measurable degree of turn. In the experiment the two balls were charged, then, once settled at a particular distance apart, the micrometer was turned by 10°, moving the balls further from each other. The time taken for the moveable ball to return to the initial distance was recorded and used together with the torsion measurements as a measure for charge leakage.



**Figure 25.** Reproduction of Coulomb's torsion balance made by Elliot Brothers, Science Museum inventory number: 1872-59. The fixed ball can be seen hanging from the lid of the lower glass cylinder (This image is released under a Creative Commons Attribution-NonCommercial-ShareAlike 4.0 Licence, © The Board of Trustees of the Science Museum).

There were practical similarities between Ohm's 1825 experimental setup and Coulomb's charge leakage experiments. Coulomb was observing a dynamic situation as the pith balls continuously lost charge throughout the experiment. Similarly, Ohm's battery continuously lost charge throughout his experiment, causing the magnetised needle to gradually return to its rest position. In both cases, careful timing of the magnetised needle's movement was required, as was some analysis and calibration to make sense of the observations after they had been recorded. Setting aside Ohm's and Coulomb's respective uses of a torsion balance, which will be discussed in more detail in Chapter 5, Heering et al. suggest that Coulomb's influence on Ohm offers a possible reason for Ohm's incorrect claim of a logarithmic law from his first phase of work. They argue convincingly that Ohm had a preconceived idea that he would find a logarithmic relationship from his research, and that he selectively chose results to fit. This, they note, matches with Coulomb's formulation of a logarithmic equation

to describe the loss of force in his electrostatic experiments, in which torsion is used as the measure of force:

$$\mu mt = log \frac{D}{d}$$

Here *D* is the force at the beginning of the measurement, *d* is the force at the end of the measurement, $\mu$ is log *e*, *m* represents the relative loss of force, and *t* represents time (Heering et al., 2020, p. 24). It should be noted that another possible influence in Ohm's choice of a logarithmic relationship was Biot, whose 1804 work on temperature distribution argued that temperature was logarithmically related to the distance between the source of heat and a point along a conducting bar. Biot, notably, also used Coulomb's torsion balance in his experimental work to find an electrostatic theory of the pile. While we know that Ohm had read some of Biot's work, as noted in Chapter 1, he makes no explicit link with Biot in his work here, and it seems more likely that he would have been familiar with Coulomb's very famous work than a little-known two-decade old paper from Biot.

Whether or not Ohm was applying an electrostatic framework in describing "loss of force", the similar logarithmic relationship between Coulomb's and Ohm's work is compelling, especially given that Heering et al. have persuasively argued that Ohm did not derive his logarithmic equation from his experimental data. There is insufficient evidence to say categorically whether or not Ohm's use of "loss of force" in his first phase of work suggests a conceptualisation of electric current as charge leaking away from a source, as in electrostatic understandings, or if he was instead using language to describing the behaviour of the forces acting on the magnetised needle. To me it seems unlikely that he was unaware of the differences between electrostatic and electrodynamic ideas, working, as he was, a quarter of a century on from Volta's invention of the pile, and in the aftermath of Ørsted's discovery of the electromagnetic effect. Nevertheless, Ohm was not explicit about his theoretical position, and his use of language leaves enough space for ambiguity.

*Current in Ohm's second phase of work*

In contrast, in Ohm's second phase of work he described his experimental observations of electric current in terms of "exciting force". He is silent on whether this represents a fundamental shift in his conceptualisation of how electricity works, using the new term without comment or explanation. Again, we might apply an interpretation focused on the

empirical observation of phenomena: Ohm may have been describing the "exciting force" that caused the magnetised needle to deflect more or less strongly, depending on the qualities of the circuit.

An alternative account could be to understand the change in language as indicative of a different conceptualisation of electricity, in which the electricity flows more or less freely through a conductor, rather than leaking away. This would match a shift from an electrostatic framework to an electrodynamic one. Understanding electricity flowing through a conductor in this manner is aligned with the arguments made by French natural philosophers, including Fourier. Although in his 1826 paper Ohm did not directly refer to taking any influence from Fourier's work on heat, which did not appear strongly until the following year in his 1827 book, it is possible that he was already aware of Fourier's ideas and was building them into a more electrodynamic approach to his work. If that was indeed the case, Ohm certainly did not adopt Fourier's ideas in their entirety. For instance, he was never explicit about what he meant by the terms conductivity or resistance and swapped between them at various times in this second phase of work. In contrast, Fourier had adopted "permeability" as a more appropriate term for his understanding of electrical flow, declaring "conductivity" and "conductibility" as inexact (Fourier, 1822, p. 596).

**Theories of fluids**

Another angle to consider when contextualising Ohm's understanding of conductivity and current is the discourse he was exposed to relating to the nature of fluids. As shown earlier, many natural philosophers saw analogies between the ways that electricity and heat behaved. There was a lively discourse about the existence, or not, of imponderable fluids that applied not only to heat, but also to electricity. Taking a step back from Ohm's experiment, this section looks at the broader theories of fluids, including imponderable fluids, that were at play during the development of theories of conductivity.

The most studied physical fluid was water, which had a long history of being engineered into waterways and plumbing systems. For example, ideas of resistance had developed from studies of how water behaved. In the 1760s, Euler had analysed water-flow in channels and tubes; he understood wall friction to be proportional to pressure and developed an account of the resistance of fluids. At the turn of the nineteenth century, Coulomb worked on friction and the resistance of fluids. A few years later still, working in the context of the growing

canal networks of the early nineteenth century, German engineer Johann Albert Eytelwein undertook an extensive study into the effect of the contraction of water through orifices using de Prony's formula for calculating the resistance due to friction in pipes (Grattan-Guinness, 1990, pp. 664; 1206-1207). Of these examples, Coulomb's work is particularly interesting. His interest in friction dated back to his career as a civil engineer, which pre-dated his electrical and magnetic research. In 1781, he described the law of mechanical friction, which was named after him. Taking his knowledge of mechanical friction, and using a Newtonian approach to electricity and magnetism, he developed his famous electrostatic force-law. Despite the interest in imponderable fluids at this time, Coulomb found his knowledge of working with physical systems provided a helpful backdrop to his electrical studies.

Returning to Ohm briefly, Heidelberger writes that Ohm's law did not seem intuitive enough for his contemporaries in a context where the intuitive or "*anschauliches*" quality of a theory was highly prized. He argues that had Louis Poisseulle's hydrodynamical law, developed in the 1840s to describe the flow of a Newtonian fluid travelling through a long cylindrical pipe of constant cross section, been established before Ohm's law, it might well have been considered a better, more *anschaulich* analogy than the model of heat conduction that Ohm settled on (Heidelberger, 1980, pp. 108-110). However, had Ohm's principal motivation been to find an *anschaulich* analogy, it seems likely that he could and would have borrowed from the many models of fluids in pipes that were already available when he was working. While Poisseulle's law offers more mathematical specificity than Euler, Coulomb or Eytelwein did in their work on fluids and resistance, the underlying relationships between the cross section of the pipe, viscosity of the fluid, rate of flow and length of the pipe have a lot in common. Instead, Ohm adopted heat as his preferred analogy. In making this choice Ohm was following a well-trodden path.

By the start of the nineteenth century, amid the investigations described above, the idea of "imponderable" (or weightless) fluids had gained significant traction. Different natural philosophers, with different theories, argued for up to six fluids, accounting for phenomena including cohesion, chemical affinity, heat, light, electricity, magnetism and galvanism. Usually, the argument was made that the various phenomena arose when imponderable fluids were combined with ordinary, or "ponderable" matter (Pancaldi, 2017, §9.1). The debates that pre-empted these theories as they related to electricity can be seen in Leslie's and Davy's work, where there are prominent discussions about whether electricity was a substance, usually considered to be a fluid, or a quality of all matter, analogous to a vibration. The

imponderable fluids had a number of common properties: they had undetectable weight, were attracted to matter, and were strongly self-repelling. In his paper about the extended sets of analogies at play in the "standard model" of physical sciences around 1800, Heilbron notes that Coulomb's "demonstration" that the self-repulsion of magnetic and electrical fluids both followed the same law was important encouragement for those natural philosophers who sought substantive connections among the various imponderable fluids (Heilbron, 2001, p. 14).

Heat and electricity in particular had several observable parallels. Not only could they both travel through solid bodies or be transmitted through the air, but there were more nuanced similarities too. Both theories included a sense that bodies could contain a natural amount the fluid, expressed as a quantity of electricity or a "latent" quantity of heat; and both heat and electricity could be obtained in indefinite quantities by friction (Heilbron, 2001, p. 15). As this demonstrates, together with several of the other examples given earlier, Ohm was far from alone in seeking an analogy between electricity and heat. Perhaps an *anschauliches* theory would have benefited his standing with local German natural philosophers, but as Ohm's work progressed he was decidedly in step with his contemporaries across Europe, particularly those in France.

**Internal and external resistance**

Returning at last to Ohm's experiment and his understanding of conductivity and resistance, this final section investigates the opportunities afforded by Ohm's change to a more stable experimental setup. An often-overlooked innovation by Ohm, which was fundamental to his conceptual development, was to look not only at what was happening within the test wires, but instead to look holistically at conductivity in the entire circuit, including the battery. This changing interpretation is most developed in Ohm's third phase of work, with its mathematical focus, but it began to make an appearance throughout his second phase of work.

One of the opportunities offered by the 1826 experimental setup was that Ohm could experimentally try to ascertain his value *b*, a constant value representing the "invariable" resistance of the circuit, which was separate from the "variable" resistance of the test wire as represented by *x*. A reminder of the equation and its terms is below:

$$X\ (strength\ of\ magnetic\ action) = \frac{a\ (exciting\ force\ constant)}{b\ (resistance\ constant) + x\ (length\ of\ wire)}$$

In his 1826 paper Ohm noted that *b* remained constant, even if the temperature difference in the thermocouple was varied (see p. 150). Contrary to his first phase of work, where Ohm was focused on measuring and interpreting the test wires he inserted into the circuit, in this analysis he considered the entire circuit, including the thermocouple and other assembled apparatus. Unusually, Ohm was thinking about conductivity within a wire as something that was related to the qualities of the entire circuit, and not only the single conductor being studied.

Turning to some philosophical considerations, one outcome of Ohm's work was a means to calculate conductivity. There was still no standardised scale or unit of measurement, so every measure had to be relative to some other known conductor. Related to the ability to measure conductivity was a means to measure electrical resistance. Resistance could not be empirically observed directly, only as an absence or reduction of the effects of electrical current. Heidelberger's work discusses this relationship, noting that once Ohm's law was developed any measurement of resistance presupposes a successful application of the law, stating that "'resistance' is a theoretical term relative to Ohm's theory (Heidelberger, 1980, p. 112). Furthermore, this observation, that a measured "resistance" can only ever be indirect, based upon a calculation from other observed phenomena, suggests that Ohm's law is subject to a common problem in the history and philosophy of measurement. There is often a circularity involved, in which the theory relating to a quantity is reliant on an appropriate means to measure it, and the measurement technique is reliant on the theory being correct (this is discussed further in Chapter 5). This may be behind a second interpretation of Ohm's use of "reduced length" instead of "resistance". The term could be understood as a descriptive term to articulate a unit of resistance without recourse to more abstract theoretical concerns.

These considerations may have affected Ohm's use of language. As already noted, he used the term "resistance" in this phase of work, but he later chose not to use it. Ohm's decision to refer to "reduced length" where we would use "resistance" superficially, at least, pushes the problematic circularity elsewhere by denying a link between the measurement of resistance, a theoretically loaded term, and the practical apparatus Ohm was working with to make his empirical observations. A further motivation for this might be that both Ohm and his intended

German-speaking audience of "concretizing" physicists were unenthusiastic about theory-heavy interpretation (Caneva, 1975, p. 23).

Alternatively, Ohm's changing language relating to resistance might have been in-keeping with changing conceptualisations of what happening in his experiment. If Ohm moved from ideas influenced by electrostatic theories and concepts in his first phase of work to a more clearly electrodynamic model in his second, there could have been a parallel shift in his concept of resistance. Understanding a long wire to provide more opportunity for "loss of force" than a short wire due to there being a shorter length of wire from which current leakage could take place, is quite different to an understanding of resistance as analogous to friction in a pipe where the longer the pipe, the more friction there is. If this was the case, Ohm's understanding of conductivity would have inevitably been wrapped up in his shifting conceptual tools between his first phase of research leading to his 1825 publication, and his second phase of research leading to his 1826 publication. On the other hand, if Ohm's choice of language was intended simply to describe his observations with as little reliance on theory as possible, we build up quite a different picture of a researcher who is unconcerned with *why* his apparatus behaves in particular ways, and much more concerned with *how* it is behaving and how to describe that behaviour in terms of replicable laws.

Irrespective of what was motivating Ohm, in his first phase of work there is good evidence that he expected and looked for a logarithmic law. To borrow Steinle's language, his work in this period might be described as "theory-led" experimentation. In contrast, in his second phase of work Ohm was prepared to give up his logarithmic equation when it became clear it did not match the observations made with more stable apparatus. Instead, he undertook "exploratory" experimentation, as he sought to find a law that fitted his observations (Steinle, 1997). This is unusual in that most experimenters move in the other direction, beginning with loose and unformed ideas that are explored experimentally, before gradually coalescing around a theoretical position which can be tested through experimental practice. Ohm was refreshingly willing to change his mind in the face of new evidence, again suggesting that he was not deeply invested in any particular theory of electricity.

While Ohm's ambiguity around his theoretical ideas can be frustrating from the perspective of a historian, his disinterestedness may have enabled him to disregard early ideas and respond more readily to the new evidence his improved apparatus presented him with. There is no sign that Ohm persevered with his logarithmic ideas in his second phase of work.

Instead, he threw himself into using his newly stable setup to make observations, before analysing the results mathematically. In my view, Ohm's lack of interest in theory also explains his ever-changing language, and the lack of explanation for it. It also supports an argument that he was trying to avoid theoretical terminology and instead trying simply to describe the phenomena in front of him.

# PART B

The reproduction of the conducting wires in Ohm's apparatus appeared initially to be the most straightforward aspect of reconstructing his experiment. This impression partly arose because of the limited description given by Ohm relating to the manufacture or design of the wires or mercury cups. However, beneath that apparent simplicity are some fundamental challenges relating to the reconstruction of historic apparatus.

### Commodities and standards

When reconstructing the conducting wires and associated components in Ohm's apparatus, I was forced to confront the role that the availability of commodities and their qualities, together with the ways in which commodities and measures are standardised, affected the development of apparatus. Beginning with the test wires themselves, which Ohm described as being made of copper, an immediate question was whether the copper Ohm was using is the same as the copper available today. Producing pure copper is relatively straightforward and was certainly possible at the time Ohm was working, though Schweigger did write to Ohm about the difficulties of sourcing pure copper wire (Hartmann, 1927, p. 64). Even if able to source pure copper wire, would the copper Ohm could access have the same properties as today's mass-produced copper? Ohm's wire isn't available for us to study, however the close analysis of a piece of copper from 1837, which was likely to have been subject to similar manufacturing processes, was shown to be 14% less conductive than modern high quality copper wire. This is likely to be due to the metal being full of very small cavities due to oxygen readily being absorbed into liquid copper and then exsolving from the copper as it cools. This manufacturing problem was not solved until the twentieth century when new techniques became available (Mills, 2004, pp. 464-465).

As we can see from this, even the most fundamental, elemental even, component of the instrument cannot be replicated, or at least not without setting up a copper smelting workshop. Not only is directly comparable copper impossible to purchase today, but as copper production is highly industrialised now, bare copper wire is only readily available in standardised sizes. Ohm described his wire as being $\frac{7}{8}$ of a line in diameter, which according to my conversion from the Cologne inch works out at 1.75mm. The closest match that could be found for the reworked apparatus was 1.6mm thick. It may have been possible to find a specialist wire puller to hand manufacture some wire at the diameter Ohm described,

however our search for a specialist proved fruitless and learning this skill ourselves appeared out of reach. Mills's study of the 1837 wire also revealed inconsistencies in the cross section of his wire sample, which he suggested was probably due to its handmade nature (Mills, 2004, p. 460). More details about the production and handling of the wires are presented in the Appendix, including considerations over managing long lengths of wire and a discussion of whether or not the wire would have been electrically insulated (Appendix, p. 273). The points to draw out here, however, are that commodity networks, standardisation and changing manufacturing techniques come together to force almost every aspect of the reproduction of the conducting wires into some form of compromise.

The importance of commodity networks can be seen elsewhere in the production of the Connelly apparatus. This is most clear in the lengthy description of trying to source some *goldlahn* for the torsion wire. *Goldlahn* appears to be a commodity that Ohm could purchase, but which proved very difficult to source for this project (Appendix, p. 279). Another example is described below in a discussion about alternatives to mercury, which is also no longer a readily available commodity, though for different reasons. Mercury today is unavailable due to health and safety regulations, rather than either a change in manufacturing processes or, as in the case of *goldlahn*, because it is no longer fashionable. In the reconstructed apparatus, each of these component parts embodies compromises and speculations in the attempt to interpret and rework Ohm's apparatus.

**Handling wires**

The other major consideration relating to the test wires was to consider how they were inserted into the apparatus during the experiment. A consequence of Ohm's unstable 1825 setup was that each time the circuit was broken and remade it caused huge disruption to the circuit, which then needed to settle before he could proceed with his observations. He dealt with this by avoiding breaking the circuit, placing the next test wire in the series into the circuit before removing the preceding one. This operation was achieved by using mercury as a liquid conductor. Ohm continued with this practice for his 1826 setup, possibly because there was no impetus to change the conductor, or, just as likely, because there was no obvious alternative (Appendix, p. 272).

Ohm describes coating the tips of his wire conductors with resin, before filing away the tips to reveal the circular cross section of the wire. The preparation of wires for use with mercury

was well understood at the time. In his *Popular Sketch of Electro-Magnetism*, intended for interested but non-expert readers, Francis Watkins provided a helpful overview of what was involved:

> …when apparatus is used for showing the phænomena of electro-magnetism, *perfect* metallic contact of the several repetitions of connecting wires is essential to complete success. It is necessary that all points of the wires should be cleaned, filed, or rubbed with emery of glass paper, or that they should be amalgamated, so that a current of electricity transmitted through the several wires should meet with no interruption, and that its passage should be the same as if only one wire were employed… Copper next to silver wire is the best connecter, and also the best conductor of electricity.
>
> (Watkins, 1828, pp. 12-13)

In addition to managing the cross section, Ohm's use of resin would also have ensured that the length of the test wire was not, in effect, shortened by plunging it into mercury. Although Ohm's mention of using resin is very brief in his 1826 paper, we can see from his correspondence with his father that in 1820-21 he dedicated substantial time to producing resin. He collected recipes and liquified ingredients, including shellac, mastic and sandarac, on his stove (Nürmberger, 2002, pp. 210-215). Although Ohm and his father corresponded about his endeavours over the course of several months, there is not enough detail in the texts to enable a reproduction of one of Ohm's recipes. However, we can see that he was able to access a range of resinous materials as commodities, which suggests that he would have had no difficulty sourcing an appropriate resin for his work in 1826. For the Connelly apparatus I made use of locally available materials and melted down a block of violin rosin that I already had at home; it melted quickly on the stove and coated the wire tips well (Appendix, p. 275).

One of the biggest challenges in reconstructing the apparatus as described by Ohm was due to restrictions on handling mercury. Today, more is known about the toxicity of mercury than in Ohm's time, and it is challenging to work with safely. While not impossible to access, my investigations made it clear that using mercury in a manner that would meet today's safety standards would make it very difficult to perform the other aspects of the experiment. For instance, having been awarded permission to work with it at all, it would then have to be either sealed into a closed container, making swapping wires in and out impossible, or the entire apparatus installed in a fume cupboard, which simply would not be practical given the need to look through an eye piece, monitor the burner and turn the micrometer. Instead, I sought an alternative (Appendix, p. 270). The most obvious contender is an alloy called Galinstan which is non-toxic, melts at 19°C, and has good conductive properties. However, it

is not a perfect substitution. Galinstan rapidly forms a sticky oxide layer, which interferes with the electrical connection and coats the test wire and the resin. The small bottle of Galinstan I purchased already had a clearly identifiable thick layer of Galinstan oxide on the surface, which also indicates another commodity-based challenge. Perhaps a higher quality product would have worked better, however there are very few suppliers of small quantities of Galinstan.

As efforts to work with an alternative to mercury became increasingly convoluted, they also crept further from Ohm's experiment. Other than the fact it is liquid at room temperature, working with Galinstan is almost entirely unlike working with mercury, and my substitution of one for the other did not prove to be a good one. I was forced to ask myself what I was actually trying to reproduce. Was the liquid nature of the conductor the most important thing about how Ohm inserted his test wires into his experiment, or was it more important to ensure a good connection? After a series of failed attempts working with Galinstan, the decision was taken to focus on making a good connection instead, and the maker produced a pair of screw-type mechanical clamps, not unlike some used in the nineteenth century, which tighten around the ends of the test wires in a way that enables a consistent, repeatable connection Appendix, p. 272). Of all the compromises and pragmatic decisions in the reworked instrument, this is the most substantial. By not using a liquid conductor, it is impossible to follow Ohm's practice of inserting a new test wire in the circuit before the former is removed. While my sense is that it was far more important to do this in Ohm's 1825 setup than with the 1826 apparatus, that is not a certainty. If there is a means of working with mercury in the future, it would be instructive to rerun the experiments with it in place and see how much difference it makes to the experiment in practice.

In conclusion, we can see that the commodities within an instrument play an important part in both its manufacture and use. One could continue this line of reasoning for almost any part of the instrument: the glass for the cylindrical part of the torsion balance is unlikely to be composed of the same materials as the glass for Ohm's apparatus, the fuel used in the burner is not specified and unlikely to be the same as the fuel I used, the cups that held the mercury are not well described but play an important role in the experimenter's processes as they swap test wires in and out, and so on. Details can be found in the Appendix on how a decision was made in each of these cases. Tracing some of these commodities through time, and through the networks that delivered them to Ohm, demonstrate how entangled the apparatus is with the availability of materials, and therefore how the experimental practice

and conceptual work of the researcher are influenced by the wider context in which their experimental apparatus is developed.

# 4. The Thermocouple

## PART A

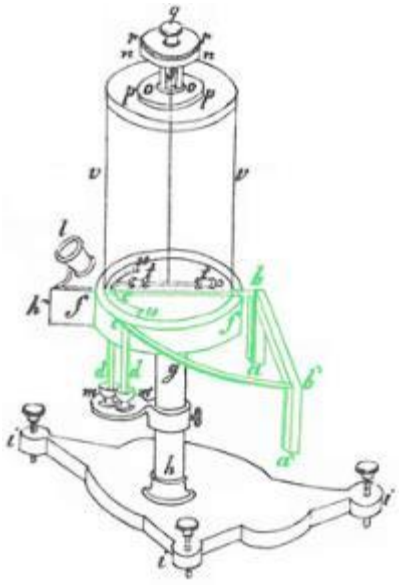


**Figure 26.** The thermocouple in Ohm's 1826 experimental apparatus

This chapter deals with Ohm's adoption of the thermocouple into his experimental setup, highlighted in green in Figure 26. As well as copper and bismuth pieces, the thermocouple included a hot vessel containing boiling water placed at *a*, and a cold vessel containing iced water placed at *a'* (further discussion of the hot and cold vessels follows in Part B of this chapter, and Appendix, p. 262). Ohm's use of the thermocouple might seem curious to us now. Thermoelectricity was never a widely used way of producing an electrical current, and today the thermocouple is generally known as a way to measure temperature.[9] Perhaps even more curious, following the arguments presented in Chapter 1's literature review, is that the thermocouple does not appear prominently in discussions of the debate between supporters of the two competing theories of the voltaic pile, the chemical theory and the contact theory. This is despite it being a means of producing an electrical current without the use of anything other than metals in contact and the application of heat; there is no need for liquid or chemical intervention in the circuit.

[9] A small number of "thermopiles" can be found in museum collections, including early designs by Nobili and Melloni in the Museo Galileo in Florence (inventory numbers 1124; 1237), and by Pouillet and Ruhmkorff in the Musée des Arts et Métiers in Paris (inventory numbers 03005; 03010).

As this chapter will show, the thermocouple was a new innovation, and still theoretically uncertain. Johann Thomas Seebeck, who discovered the effects of the thermocouple, considered it to be a "thermomagnetic" effect, and denied that there was any electrical effect evident. However, it was Hans Christian Ørsted who actively shared news of Seebeck's discovery, and Ørsted interpreted the effect as "electromagnetic". Ohm did not engage with this uncertainty in his work, and referred to both "thermomagnetism" and "thermoelectricity" in his writing (for example see Ohm, 1826a p. 143 and p. 153 respectively). In order to understand the origins of these differing views, it is helpful to know the context that both Seebeck and Ørsted were working in. Ørsted had famously announced his discovery of electromagnetism to the world in 1820, demonstrating that where there was an electric current there was also a magnetic effect. While for many the discovery of electromagnetism was a starting point for new magneto-electrical studies, for Ørsted it was an experimental observation that confirmed long held theoretical beliefs. This context provides an important backdrop to the discovery and interpretation of the thermocouple, and also helps us to make sense of Ohm's adoption of the technology, which implicitly involved adopting Ørsted's theoretical claims too.

This chapter first sets out Ohm's 1825 experimental apparatus to show what the thermocouple went on to replace, before providing a thorough introduction to Ørsted and Seebeck's work to set out the conceptual foundations adopted by Ohm. It then turns to Ohm's adoption of the thermocouple into his experiment and the impact that change in apparatus had on the conclusions Ohm drew.

**The voltaic pile in Ohm's early work**

Ohm's 1825 "Preliminary indications" paper appeared in both Schweigger's *Journal* and Poggendorff's *Annalen* and provided a description of his 1825 experimental setup. It comprised of a copper-zinc trough battery, with each end connected by wires *A* and *B* to their respective vessels *M* and *N*, which both held mercury; a further fixed wire, *C*, connected vessel *M* to a third mercury filled vessel *O*. Across *C*, Ohm suspended a Coulomb torsion meter. The ends of the test wires he was working with were placed in the mercury in vessels *O* and *N* to complete the circuit, or *Kette*, (see Figure 27 for my diagrammatic interpretation of Ohm's description).

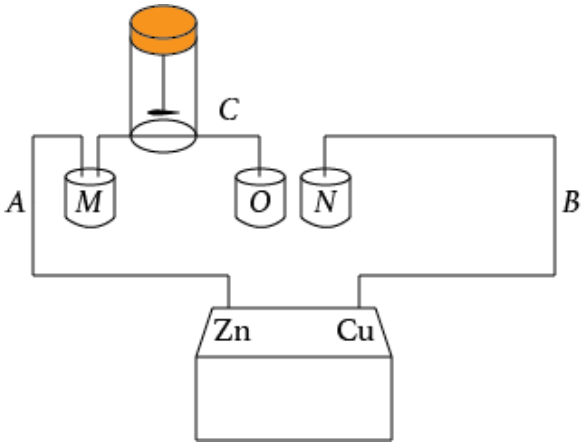



**Figure 27.** Diagrammatic interpretation of Ohm's 1825 setup.

Ohm referred to wires *A, B,* and *C* together as the "invariable conductor". This invariable portion of the circuit was 4 feet long, and $1\frac{1}{4}$ lines thick (approximately 1.15m long and 2.5mm thick). The six test wires Ohm inserted between *O* and *N* were named *o, a, b, c, d, e,* and he referred to them as the "variable conductors". They varied in length from $\frac{1}{3}$ foot long (around 95mm) to 23 feet long (approximately 6.6m). In a later series, Ohm introduced conductor *f* at 75 feet long (approximately 21.5m). With the exception of conductor *o*, which was very thick, the conducting wires were all 0.3 lines thick (approximately 0.6mm). On inserting the test piece and completing the circuit, the needle in the torsion balance was deflected due to the electromagnetic effect.

In this 1825 paper, Ohm does not describe his observations with the torsion balance in detail, saying only the "variable conductors were applied in the following order, *o a o b o c o d o e o*, and each time the force of the invariable conductor on the magnetic needle was measured" (Ohm, 1825a, p. 111). Though this paper lacks detail, he explains that by using *o* as a control he sought to calibrate his readings as the battery discharged, and, according to his paper, by the end of the first series of experiments he had produced a formula that he could test his results against:

$$v = 0.41 \log (1+x)$$

where *v* indicated the "loss of force" and *x* the length of the variable conductor. After reporting the first series of experiments which Ohm states led him to his law, he then manipulated his equation by differentiating it before seeking to fit his observed results to the values predicted by his derived equation in a second and third series of observations. As

mentioned in Chapter 3, Heering et al. have undertaken a detailed study of this experiment, and present a convincing argument that Ohm's logarithmic law is not an outcome of the data he produced experimentally.

Irrespective of the rigour with which Ohm made his observations and fitted them to a mathematical idea, it is evident that the experimental setup he was using was very challenging. Alongside the third and final series, Ohm comments on some of the difficulties he found in using his apparatus. He wrote that on inserting one of the variable conductors into the circuit, "it takes half a minute or more until the effect on the needle attains its maximum, which one must wait if they do not want to record too small a number; and *vice versa*, if afterwards the conductor is taken out of the circuit the effect on the needle is too great at first, and in order to get a reliable reading one has to wait for the needle to attain its minimum deflection to be sure" (Ohm, 1825a, pp. 113-114). Reflecting on this setup, Ohm later noted that the difficulties caused by the "hydroelectric", or voltaic, pile were very detrimental, particularly when working with conductors with very unequal values in quick succession, which caused destabilising surges within the circuit. (Ohm, 1826a, p. 143). Even without attempting to reconstruct this experiment, as Heering et al. have, it is clear that Ohm would have been juggling multiple, challenging activities to in order to make his observations. Different wires were placed in the experiment in a particular order and in quick succession, followed by close observation and careful timing of the needle's movement once each wire was in position. Post-observation, calibration against conductor *o* was required to understand the rate at which the flow of electricity from the voltaic pile was changing. Ohm also offers a hint at how familiarisation with his experiment helped him overcome some of the challenges of working with the pile, commenting briefly that he listened to the hiss (*Brausen*) of the battery to him to identify when an observation might be reliably made (Ohm, 1825a, pp. 115-116). It is clear that carrying out this experiment would have required careful planning and intense concentration throughout, largely in order to overcome the challenges introduced by the instability of the voltaic pile.

Discussing the efforts he made to manage the surging effects of the "hydroelectric pile", Ohm commented on the surge of force that occurs whenever the circuit is opened or closed, or when test wires are exchanged, which "make the experiments extremely difficult." In order to make sense of the way the force within the circuit changed over time, he closed the circuit with conductor *o*, and observed the position of the magnetised needle in the torsion balance every five minutes, gathering the following results:

> 180, 150, 135, 125 ½, 119 ½, 115, 111 ¾. 109 ½, 107 ¾, 106 ¾, 105 ¾, 105, 104 ½ [divisions on the micrometer], one hour later 100 ½, so that a total of 2 hours 5 minutes had passed.
>
> (Ohm, 1826a, pp. 141-142)

It was clear from this exercise that the circuit became more stable over time, though the deflection of the needle continued to reduce at a steady rate, albeit more slowly after the initial surge in the first 15 minutes. Ohm considered ways to minimise the surge caused by breaking and remaking the circuit, saying "as far as I know, there is only one way to make this surge of power quite harmless, which is to keep the circuit closed for a long time before the start of the experiments and never open it during the experiments themselves" (Ohm, 1826a, p. 142). In practice, this meant not breaking the circuit between observations. Ohm instead placed the test wire for a new observation into the mercury cups before removing the test wire from the previous observation. This minimised the surge, especially where the test wires had approximately the same conductivity as each other. Despite adapting this practice, the instability of the voltaic pile continued to be a major challenge for Ohm in working with his 1825 setup, and he readily took up Poggendorff's suggestion of using a thermocouple instead.

**Thermomagnetism's origins in electromagnetism**

Ohm's interpretation of what his 1826 experimental apparatus was doing included a thermocouple producing an electrical effect, which in turn caused a magnetic effect, which then caused the deflection of a magnetised needle. This section, and the next on Seebeck's work, may appear at first glance to be a digression from Ohm's story. However, they provide much needed context to unpack the conceptual assumptions Ohm was working with, and why they had the potential to be considered problematic by his peers.

As a young man, Hans Christian Ørsted, influenced by Kant's work, formed the view that the corpuscular theory then prevalent in France, as well as Denmark, was indefensible. Instead, he favoured a "dynamical system". He argued that forces belonged to qualitative categories and had to be understood in terms of sliding scales (Christensen, 2013, pp. 66-68). This understanding of forces as polarised, dynamic and qualitative can be seen throughout Ørsted's work, including his later work on magnetism and electricity. He found common ground with the work of Johann Wilhelm Ritter, who believed that heat, light, electricity and magnetism are all fundamental forces belonging to a dynamical system in which the forces

were polarised. Any particular force could be described in terms of its location on a sliding scale between two extremes: hot and cold, positive and negative, or light and dark. Within this dynamical system, Ørsted believed that the various forces were interrelated. In his research he sought evidence for links between the various types of force.

In 1804, Ritter wrote to Ørsted that he had observed that a long needle made of zinc at one end and silver at the other had aligned itself with the Earth's magnetic field, with the zinc end pointing north. This was a key moment in their relationship. The implication of Ritter's claim was that the contact between the two metals, which was known to be related to the generation of electrical effects, could now be linked to magnetism too. Ritter also intended to suspend an assembled voltaic pile in such a way that it would be free to align itself along the magnetic meridian, in the expectation of observing the same result. Had Ritter's observations been corroborated, the discovery would have been profound. It would have demonstrated the kind of relationship between two different forces that he and Ørsted had been searching for. Kragh argues that both Ritter and Ørsted were strong proponents of the chemical theory of the voltaic pile, which could have led to a further interpretation linking chemical effects to the Earth's magnetic pole, as well as the purported electrical effects (Kragh, 2000, pp. 4-9). On reading Ritter's letter, Ørsted immediately set about procuring apparatus himself to repeat the experiment. Many of the big names of Parisian natural philosophy were assembled to see his attempt: Laplace, Coulomb, Hallé, Guyton de Morveau, and Biot came together to witness Ørsted's experiments on behalf of Ritter. Inevitably they viewed the experiments through the lens of their own corpuscular theory, but nevertheless they made persistent attempts to follow the experiments. Ritter's discovery would be epoch making, if only Ørsted could verify it. However, the attempt failed, and Ritter's claim could not be verified (Christensen, 2013, pp. 150-155). Setting aside the fallout for both Ritter and Ørsted's professional standing, this episode shows Ørsted taking seriously the prospect of a relationship between electricity and magnetism some sixteen years before his ground-breaking discovery of electromagnetism in 1820.

Ørsted, in his own writing, was keen to place himself somewhere between strict empiricism and the risky abstraction of *Naturphilosophie* promoted by contemporaries including Ritter. In his 1830 article "Thermo-electricity" he not only defended Ritter enthusiastically, but also wrote extensively, and somewhat defensively, on the nature of philosophical knowledge, trying to strike a balance between two competing approaches:

> One class of natural philosophers have always a tendency to combine the phenomena and to discover their analogies, another class, on the contrary, employ all their efforts in showing the disparities of things. Both tendencies are necessary for the perfection of science, the one for its progress, the other for its correctness. The philosophers of the first of these classes are guided by the sense of unity throughout nature; the philosophers of the second have their minds more directed towards the certainty of our knowledge. The one are absorbed in search of principles, and neglect often the peculiarities, and not seldom the strictness of demonstrations; the other considers the science only as the investigation of facts, but in their laudable zeal they often lose sight of the harmony of the whole, which is the character of truth… This conflict of opinions keeps science alive, and promotes it by an oscillatory progress, though it seems to the common eye a mere fluctuation, without any definite purpose.[10]
>
> (Oersted, 1830, pp. 673-674)

Ørsted's attitude to theory is important because it underpins his search for a unified fundamental force. That search, which dominated his career, made it natural to conclude that Seebeck's thermocouple produced an electrical effect.

Just a year after Ørsted's famous discovery of electromagnetism, Michael Faraday wrote an anonymous "historical sketch" of electromagnetism for the *Annals of Philosophy.* Few discoveries can be described as history so soon after they have been made, but as Faraday pointed out:

> M. Oersted, Professor of Natural Philosophy and Secretary to the Royal Society of Copenhagen, has for many years been engaged in inquiries respecting the identity of chemical electrical and magnetic forces, and as early as 1807 proposed to try "whether electricity the most latent had any action on the magnet."
>
> (Faraday, 1821, p. 195)

Ørsted was himself at pains to establish a long historical tradition for the search for such a link. In his 1830 article he referred to historical instances where a lightning strike had affected compasses, and used this as evidence for a long-standing question about the relationship between electricity and magnetism. This desire to suggest that links between different forces had been hiding in plain sight for a long period can be read as rhetoric to back up Ørsted's position, which was far from universally accepted. In fact, when Ørsted's discovery of electromagnetism was first announced in France, the older generation of atomists was deeply sceptical. It took Arago, who was younger than many of the established

[10] Ørsted's use of "conflict" and "oscillatory progress" was typical of the dynamical understanding of the world. This can be seen again below in Seebeck's descriptions of magnetic circuits, which he considered to be in equilibrium until interrupted by a point of contact between two different metals.

figures and had a fraught relationship with Laplace, to convince the older generation of the validity of Ørsted's claims by demonstrating them in action. Although natural philosophers including Ørsted, Ritter and Schlegel may have long held the belief that magnetism and electricity must be related, at the start of the 1820s many influential figures disagreed.

Despite the scepticism from some quarters, Ørsted's discovery of electromagnetism was hugely important and a large number of natural philosophers began to work on electromagnetism and related effects. Within a matter of months, Ampère had demonstrated that conductors attracted and repelled each other depending on the direction of the currents passing through them; and Arago, Gay Lussac and Erman worked on the effects of electromagnetism on steel, which was found to retain some magnetic properties even after the electrical effect ceased (Oersted, 1830, p. 576). This flurry of activity in the 1820s, together with Ørsted seeking to reinforce his claims at every opportunity, was the backdrop for both Seebeck's discovery and Ohm's experiments.

**Seebeck's discovery**

Johann Thomas Seebeck observed that if a circuit was broken, and a new piece of a different kind of metal conductor was inserted into the circuit, making two junctions where the two different metals met, then heating one of the junctions to introduce a temperature difference caused a magnetic effect that was strong enough to deflect a magnetised needle. The greater the temperature difference, the greater the deflection. Seebeck described this as "thermomagnetism". While there was generally a muted response to this finding, Ørsted was very impressed describing it as "the most beautiful of the discoveries which has as yet grown out of mine" (Franksen, 1981, p. 14). However, in looking at exactly the same experimental setup, what Ørsted observed was not *thermomagnetism*, but rather *thermoelectricity*; he interpreted the deflection of the magnetised needle as indicative of the presence of an electrical effect. We have seen Ørsted's intellectual background, which suggests why he might have observed an electrical effect. What was different in Seebeck's background to lead him to see only a magnetic effect, remaining sceptical that there was any electrical effect implied by his discovery?

There is relatively little biographical information available about Seebeck. One of the most substantial pieces is Poggendorff's "Gedachtnissrede", presented to the Royal Academy of

Science in Berlin in 1839 after Seebeck's death. Poggendorff paints a picture of a meticulous and careful researcher whose real passions were optics and colour theory (Poggendorff, 1839). It is notable that Seebeck's work relating to the thermocouple receives relatively little space in the memorial. Poggendorff does state explicitly that it was Seebeck's discovery that enabled Ohm to first explain his "beautiful and influential theory of electrical currents", though he declines to mention that Ohm took this approach thanks to a tip-off from Poggendorff himself. Poggendorff was a member of the Royal Academy of Science in Berlin where Seebeck presented much of his work. This was valuable as Seebeck gave verbal papers and demonstrated his work far more enthusiastically than he published (see the listing at the end of Poggendorff, 1839). In the 1822-23 proceedings Seebeck presented a paper titled: "Magnetic polarisation of metals and ores through temperature difference", which is where he first described his observations of thermomagnetism. Although Poggendorff listed the dates 1822-23 for this particular paper, which is when the papers were given to the Academy, the published volumes were not printed until 1825, the same year that Ohm was carrying out his own experimental work. For natural philosophers like Ohm, who himself was never a member of the Berlin Academy, journals like those produced by Schweigger and Poggendorff were important for distributing news from proceedings of societies. This, perhaps, explains why Poggendorff was able to encourage Ohm to use a thermocouple in place of the voltaic pile. However, it should be noted that Ohm was not wholly ignorant of the idea before Poggendorff's intervention, and had made a passing reference to thermomagnetism which appeared in both published versions of his 1825 paper (Ohm, 1825a, p. 115; 1825b, p. 85).

What is less clear is why thermomagnetism was not a much more widely discussed advance, given that it further strengthened Ørsted's claims to an underlying unity in nature, and could have formed an important piece of evidence for contact theories of the voltaic pile. Seebeck's professional life coincided with the peak of the *Naturphilosophie* movement, and having lived in Jena as a young man he would certainly have come into contact with many variants of the movement. He knew of and rejected the more extreme version of the philosophy advocated by Schelling, and for a time was close with Ritter (Nielsen, 1989, p. 117). However, of all his professional partnerships, Seebeck was most heavily influenced by one formed in his early career with Goethe, with whom he worked extensively on colour theory. He followed Goethe's methodology and argued for a dynamic unity, a similar concept to that used by Ørsted, and he argued against what he considered reductionist mechanistic ideas

employed by the atomists. The network that Seebeck was part of overlapped with Ørsted's network. Although the theoretical divisions are often murky between different flavours of nature philosophy in this period, for Seebeck, Ørsted and Ritter common features of their theoretical stances were that they valued qualitative, mathematics-free, dynamic systems that were rooted in seeking polarising forces. Seebeck himself referred to both electrical and magnetic effects in terms of polarisation. Indeed, the title of Seebeck's paper, "The Polarisation of Metals and Ores Through Temperature Difference", signals his theoretical positioning. Nielsen reports that "Seebeck's spiteful and derisive remarks about the combination of experimental physics, atomistic explanations, and complicated mathematics that came to dominate French physics during the period bear witness to the intensity of his feelings" (Nielsen, 1989, p. 109).

The following section will look in more detail at Seebeck's "Magnetic Polarisation" paper, which presents factual and thorough empirical investigations. The paper opens with a claim not about the relationship between temperature and magnetism, or about a relationship between temperature and electricity, but instead with a claim about how a pair of metals in contact interact:

> In continuing investigations on the mutual relations of the electric, chemical and magnetic activities in galvanic circuits, I came across a phenomenon which seemed to me to indicate that two metals by themselves, simply joined with each other in a circuit, may become magnetic without the additional action of a liquid conductor.
>
> (Seebeck, 1825, p. 266)

This is curiously positioned between two problems. Had Seebeck opened with a claim that he could produce an *electrical* effect without a liquid conductor, this could be easily recognised as part of the discussion around competing theories of the voltaic pile. However, by focusing on the *magnetic* effects he had observed, and declining to make any argument about a link between electricity and magnetism, the paper is not explicitly positioned as part of that debate. Alternatively, had he opened with a claim that either heat and electricity, or heat and magnetism were directly linked, this would have very explicitly offered another piece of evidence to Ørsted's claims about unity in nature. The paper makes no reference to other claims by dynamists, such as Ritter's discredited 1804 claim that a zinc-silver needle had aligned itself with the Earth's magnetic field, as described above. Instead, his interpretation of his observation was focused on the disruption to the natural equilibrium of the conductors:

> … it seemed to emerge that it was not so much the action at the point of contact of the metals with each other, but rather by the inequality of the actions at the two points of contact of the metals…
>
> (Seebeck, 1825, p. 266)

The language Seebeck is employing here is very much in line with dynamic theory. Another way of saying the above would be that the circuit would be in equilibrium, if it were not for the fact that there is an inequality at the point of contact of the metals, which in turn causes a polarisation. Seebeck's experimental setup consisted of a 40ft long spirally wound piece of copper ribbon used to make a connection between two test plates, which could be varied. In the first variation, a plate of bismuth was placed on top of a plate of copper and heated initially by a warm hand, and later by a lamp to eradicate any risk of introducing a moist conductor. This caused a magnetised needle to move, and, he observed, the greater the temperature applied, the greater the effect (Seebeck, 1825, pp. 266-271).

The paper continues to be framed largely in terms of Seebeck interrogating the effects of the contact of two metals. He argues first that if both the contact points of the two metals are heated to the same extent, then "without doubt magnetism will also be excited… [but] there will be no action on the magnetic needle… because by this procedure a double and opposite magnetic polarisation will be excited in the circuit". This implies his later claim that "magnetism must be excited by the mere contact of the half circuits with each other without any change of temperature, but it remains latent, because the actions of the two metals on each other are of equal strength at both points of contact and the magnetic polarisations excited by them have opposite directions" (Seebeck, 1825, p. 273). For Seebeck, then, the contact between the two metals seems to be the point of interest, far more than the relationships between temperature, magnetism or electricity. In the remainder of the paper, Seebeck records extensive variations of his experiment. The entire paper is over one hundred pages in length, and includes lengthy tables of results. He noted that the magnetic "tension" does not increase uniformly in all cases with temperature rise, and that ores behave more erratically than elemental metals. However, the law that a greater temperature difference causes a stronger magnetic effect seems valid for most combinations of metals. Throughout the many pages of tabulated results, the observations are always qualitative rather than quantitative, as is in keeping with Seebeck's theoretical leanings. Finally, before returning to Ørsted and his interpretation of Seebeck's discovery, Seebeck's own comments about the relationship between electricity and magnetism are striking:

> The complete deviation of the magnetic series of the metals from the electrical series of metals is regarded as a new and important objection to the hypothesis that electricity and magnetism have a single identity, and, to be considered especially, an objection against the electromagnetic theories put forward since Oersted's discovery, which say that electricity, and derived magnetism, is stimulated at the points of contact of two metals.
>
> (Seebeck, 1825, p. 292)

Again, contact between the metals is consistently used as the focus of discussion. The key point for Seebeck was the difference between the magnetic series of metals that he had established, and the electrical series of metals, even while acknowledging that the position of some metals in the electrical series is still under discussion. He argued that for electromagnetic theories to work, there needs to be a fixed relationship between electrical and magnetic polarisation, writing that "it is therefore not permissible to call these circuits electromagnetic until a different source of electric excitation than that which occurs at the contact points of the metals… is elucidated" (Seebeck, 1825, p. 297).

**Ørsted's reaction to Seebeck's discovery**

In the wake of his 1820 discovery of electromagnetism, Ørsted had embarked on his third grand tour of Europe. In 1822 he visited Berlin, where Seebeck was keen to show him his results. When Ørsted saw the thermomagnetic effect he immediately interpreted it as an indication that there was an electric current present, and that the thermocouple could be an alternative kind of battery. Continuing to Paris, in 1823 Ørsted reported on Seebeck's findings, but crucially he renamed the effect thermoelectricity and placed it in contrast to galvanic current which he renamed hydroelectric. With Fourier, he carried out extensive further examination of the phenomenon, and in a joint paper they described the properties of thermoelectric batteries as analogous to the Voltaic pile (Fourier & Ørsted, 1823).

Ørsted was always careful to name Seebeck as the discoverer of thermoelectricity, despite acknowledging that Seebeck was "not inclined to consider the effect thus produced as a true electrical current, but an effect *sui generis*" (Oersted 1830). Looking back on the period in his 1830 article, Ørsted expands on the developments made in thermoelectricity. In particular he spends a good amount of space exploring the kind of electrical forces in a "thermo-electrical" arrangement, compared with a "hydro-electrical" setup:

> The thermo-electrical current has a prodigious quantity of electricity in comparison with the hydro-electrical of silver, zinc, and water, but the intensity of the electricity is much greater in the latter.
>
> (Oersted, 1830, p. 586)

In his conclusions, Ørsted frames the entire history of electromagnetism in terms of his greater scheme to unite a wide range of effects, including heat, light, electricity and magnetism, under a universal and fundamental force. His final sentence encourages stepping beyond the strict empiricism of some of his contemporaries, saying:

> The new discoveries, in short, reveal to us the world of secret motions, whose laws are probably analogous to those of the universe, and which deserve to be the subject of our most earnest meditations.
>
> (Oersted, 1830, p. 589)

Ørsted did not seek to place the discovery of thermoelectricity in theoretical frameworks other than his own. He omitted, for instance, to mention Ohm's, admittedly relatively little known, work using a thermocouple, and no mention was made of the controversy around theories of the voltaic pile. Instead, the entire article hinges on thermoelectricity as a piece of supporting evidence for his own dynamical theory. Ørsted's ability to shape the narrative to his own ends is in no small part down to Seebeck's failure to publicise his findings. Furthermore, Seebeck's choice to frame the discovery as a magnetic effect concerning contact between two metals, rather than a discovery linking heat and magnetism, hid some of its implications. This paved the way for Ørsted, as the most vocal reporter of Seebeck's findings, to interpret the observations heavily to support his own somewhat controversial theory.

Although Ørsted's reporting was influential in how the thermocouple was understood, Seebeck's findings were not wholly interpreted through Ørsted's lens; reports of the discovery made it to Britain in a brief notice in the *Annals of Philosophy* in 1822. This led most notably to William Sturgeon's investigations into thermal effects. In 1824 Sturgeon described his "comparing galvanoscope" as an "instrument I have made for the purpose of exhibiting and comparing chemico and thermo phaenomena as influenced by the magnet", demonstrating wider interest in the interrelations between different types of force (Sturgeon, 1824, p. 98).[11] In his 1831 paper on thermal effects Sturgeon praised Seebeck's work and

[11] A reproduction of this instrument is in the Science Museum's collection (inventory number 1931-536), produced as part of the same collection as the Science Museum's Ohm apparatus, showing that Sturgeon's work was considered interesting and important enough to exhibit in the Faraday centenary exhibition in 1931.

discovery as "of equal, if not superior interest, to of electromagnetism by Oersted the illustrious Dane" (Sturgeon, 1831, p. 1). Sturgeon's novel discovery, which could have confounded any claims about contact-magnetism and, in turn, contact electricity, was that under the right conditions it is possible to create a thermomagnetic effect with a single piece of metal. In his experimental work, a rectangular ring of bismuth was cast then heated at particular points on the cast to produce a magnetic effect. Sturgeon recognised that there were properties in the internal structure of the cast bismuth that were helping to achieve this effect, and observed the way that the sharp corners of the rectangle, or the gate where molten bismuth was poured for casting, could be used to consistently predict where "neutral points" would appear in the metal. Sturgeon shows no sign in his writing of thinking overtly about the contact-chemical debate, and unlike many of the other researchers engaged in research with thermocouples he consistently discussed thermomagnetism rather than thermoelectricity. Another English natural philosopher, James Cumming, undertook work on using a thermocouple, and, like Ørsted, he interpreted the effects as thermoelectricity.[12] Cumming's wide-ranging investigations appeared in a number of papers from 1823 and independently drew many of the same conclusions as Seebeck, whose own work was not available printed in full until 1825. He, too, noted the major differences between the thermoelectric series of metals and the electrical series of metals. He also found that there were some instances where there was an inversion effect with certain couples at high temperatures, as had been noticed by Seebeck (Cumming, 1823, p. 178).

**The thermocouple and contact electricity**

As described above, Seebeck was preoccupied with the contact between metals in his experiment. He wrote about his discovery of thermomagnetism as arising first from the contact between two metals, with a further disequilibrium introduced by a temperature difference. Seebeck's work, and responses to it, offer a new interpretation for the debate between contact and chemical theories of the pile introduced in Chapter 1, and, in turn, how we might interpret Ohm's inclusion of the thermocouple in his work. Had Seebeck made the theoretical claims that Ørsted jumped to, that not only were the circuits thermomagnetic, but

[12] Cumming has also been credited with independently discovering thermoelectricity by P. G. Tait (Finn, 2008, p. 497), a claim that is repeated in Haley (2002), though Bernard Finn questions the claim, having found no evidence to support it. Cumming's own description appeared in the *Annals of Philosophy,* after the original notice about Seebeck's discovery had appeared.

also thermoelectric, it might be supposed that his discovery would have been picked up by proponents of the contact theory of the voltaic pile. Interestingly, Seebeck does not get so much as a mention in Kragh's thorough exposition of the people involved in the controversy, despite potentially having produced a strong argument in favour of the contact theory, albeit described by him as a magnetic effect with no reference to electricity (see Table 1 on p. 32).

The thermocouple was also investigated by Becquerel, described by Kragh as mainly on the chemical side of the debate. While Becquerel noted that it was interesting to see the electrical effect produced without any liquid in the thermocouple, Seebeck's finding was not presented as a major blow to a passionately held theory. Instead, Becquerel's lengthy paper, like Sturgeon's, sought to compare thermal and chemical effects in thermocouples:

> We see therefore that, in a metal wire, when these two ends are unevenly attacked by an acid or when the caloric does not heat them in the same way, the state of equilibrium of the two electricities of this wire is disturbed: these are two actions which produce the same effects.
>
> (Becquerel, 1823, p. 154)

Our other key player in this discussion, Ørsted, holds an unclear position in the contact-chemical debate. Kragh placed him firmly on the chemical side of the debate, in contrast to Caneva's argument that Ørsted's use of the multiplier as an indicator stemmed from his desire to defend the contact theory (Caneva, 1978, p. 78; Kragh, 2000, p. 139). However, within Kragh's writing there are hints at some ambivalence towards the debate from Ørsted, who "tended to consider chemical effects and electricity as two manifestations of the same 'powers'" (Kragh, 2000, p. 137). On the one hand, we have Seebeck emphasising the novel evidence he has found for the role of metals in contact in generating a magnetic effect, but not being taken up by contact theorists. On the other hand, we have Ørsted, who maintained no clear public line about his position in the debate and was more interested in establishing his dynamical theories which, if anything, dismantled the debate itself by trying to show that there were unifying forces behind all phenomena, whether generated by contact between metals or chemical effects. Meanwhile, an apparently staunchly chemical-theorist, Becquerel, is interested in the comparison between chemical and thermal effects, but is not strongly defensive of the chemical theory. Given this ambivalence from some important figures, it seems that the contact-chemical debate was rather less prevalent in the minds of Seebeck, Ørsted and their contemporaries than recent historiography might suggest. At the very least, it suggests that in comparisons between the thermocouple and voltaic pile, the batteries were being used instrumentally, without much interrogation of the underpinning theories, and

instead a focus on the phenomena they produced. A stronger critique might be that this demonstrates the categories of chemical theorist or contact theorist were not actor's categories, but rather a structure imposed by historians to try to make sense of an ongoing discussion. These considerations are reflected in Ohm's apparent ambivalence around the contact-chemical debate. Although Ohm referred to "contact electricity" in the title of two of his published papers, suggesting his theoretical stance, he did not use the thermocouple in his experiment as a means of making a point about contact electricity. Instead, he followed in Ørsted's footsteps and focused on describing the differences between the "hydroelectric" pile used in his 1825 setup and the "thermoelectricity" he used in his 1826 setup. Any thoughts he may have had about the support this presented for a contact theory of electricity were sufficiently unimportant to be left out of his work in this period.

**Ohm's adoption of the thermocouple**

Here at last we return to Ohm's work, armed with an understanding of the thermocouple as a technology still under investigation, and for which the underlying theories were being actively negotiated. Nevertheless, for Ohm to rely on his experimental apparatus he had to adopt not only the thermocouple as a useful tool, but also Ørsted's interpretation that the thermocouple produced an electrical effect.

Ohm introduced the thermocouple into his 1826 experimental setup following a suggestion from Poggendorff, who included a footnote alongside Ohm's 1825 paper saying:

> It is to be hoped that the author finds the opportunity to make this and similar determinations with the so-called thermoelectric circuit. The effects are far more constant with this than with the so-called hydroelectric circuit, and therefore allow very precise measurements.
>
> (Ohm, 1825b, p. 83)

As mentioned, Ohm appears to have known something of thermomagnetism before Poggendorff's intervention, and in that same paper he makes a brief comment that his work "promises a deeper insight into the nature of thermomagnetism" (Ohm, 1825b, p. 85). However, Ohm's comment is so fleeting that it provides little insight into his level of knowledge, and he publicly acknowledges Poggendorff for his comment at the start of his 1826 "Announcement of Laws" paper (Ohm, 1826a, p. 143). Regardless of whether Ohm was reliant on Poggendorff's suggestion to make the change in battery technology, the

introduction of the thermocouple made a huge difference to his work. As he reported in his 1826 paper:

> The constant surge of force which takes place when the circuit is opened and closed, or when such conductors are exchanged, which, as the closing link of the circuit, have unequal conduction values, makes the experiments extremely difficult… I could not hope to discover the actual law of conduction in this way and therefore resorted to the thermomagnetic circuit, the stability of which has been recommended to me by Mr. Poggendorff; and since the experiences gained in this way express the law of conduction in a decisive way, I do not consider it superfluous to describe the apparatus used in it in a detailed manner.
>
> (Ohm, 1826a, p. 139; 143)

It is notable that Ohm again referred to the thermocouple as introducing a thermomagnetic circuit in these introductory remarks, rather than thermoelectric, however later on in this same paper he switches to writing about the thermoelectric circuit instead; he is inconsistent even within this one paper. Once again, we see Ohm's disinterest in theoretical concerns. He does not take the trouble to clarify his position on what precisely the thermocouple might be doing.

Ohm describes his thermocouple as taking the form of a piece of bismuth cast in the shape of a square bracket with two legs, with a piece of copper attached to each leg. The two feet of the thermocouple sat in hot and cold vessels made of sheet metal. Ohm does not describe why he chose copper and bismuth as his materials, however, as noted above, several people had produced a thermoelectric or thermomagnetic series relatively recently. It might have been the case that Ohm had selected copper and bismuth because they were both accessible and at opposite ends of a series he had seen. Alternatively, he may have followed in the footsteps of another experimenter who chose that metal pair. Either is plausible, but there is insufficient evidence to say either way.

In the hot vessel water was kept constantly boiling, and on the other side snow or fragmented ice cooled the foot (Ohm, 1826a, pp. 144, 148-149). Once again, Ohm does not describe his reasoning. However, it is likely he chose to work with boiling and freezing water for either end of his thermocouple as the most readily available pair of fixed temperatures, as was well understood at the time he was working (for a thorough history see Chang, 2004, Chapter 1). Ohm's drawing of the two vessels is much more crude than the depiction of the rest of the instrument, suggesting it was produced by a different hand (more information about the form of the thermocouple and the vessels is provided in the Appendix from p. 260). However, the

simplicity of the drawing belies the care Ohm took in using the thermocouple effectively. As the Appendix shows, there was also craftsmanship required to produce the cast bismuth piece and the bent copper pieces that attached to it. Quite how Ohm arrived at his design is unclear. It seems likely he took some ideas from descriptions of the "thermopile" as a technology, which had been investigated by a number of experimenters following Seebeck's discovery. However, as Fourier and Ørsted describe in a paper referred to by Ohm, the practical difficulties involved in maintaining a substantial effect from a thermopile meant that it was not a straightforward tool to design or use:

> To form complex circuits capable of producing a very large effect on the magnetic needle, one is obliged to make use of very short elements; this will result, it is true, in the disadvantage that the temperature equilibrium will recover quickly in the circuit.
>
> (Fourier & Ørsted, 1823, p. 380)

Fortunately for Ohm, he did not need a strong electrical effect, only a consistent one that could be detected by his sensitive torsion balance. Experiments satisfied Ohm that consistency could be achieved by maintaining a steady temperature difference by using water at boiling and freezing point. This technique was practicable and enabled him to maintain known temperature points. Ohm also tested his findings in a later series of experiments by leaving the hot end of the thermocouple at room temperature, which he measured as 7½° Réaumur. He noted that his value of *b*, or the internal resistance of the unchanged part of the circuit, which he called "conduction resistance", remained unchanged when he calculated his findings with conductors he had already tested in an earlier series.

> There are two important points to note about these last attempts. On the one hand it is remarkable that the value of *b* remains unchanged, while the force becomes more than ten times less, so that it seems to depend only on the exciting force and only on the unchangeable part of the conductor. Second, it appears from these experiments that the force of the thermoelectric circuit is directly proportional to the temperature difference.
>
> (Ohm, 1826a, pp. 153-154)

Aside from finding his 1826 apparatus much more satisfactory to use, Ohm spent some time in his paper discussing the differences between the two types of battery and suggesting reasons for some of the differences. In particular, he noted that *b* was much higher with a hydroelectric, or voltaic, battery than with the thermoelectric battery. In this and the quote above, Ohm demonstrates an interest in the internal resistance of his circuit. Incidentally, this

is also the point at which he starts speaking in terms of thermoelectricity rather than thermomagnetism.

> The value of *b* in this [hydroelectric] one is hundreds of times greater than in the other [thermoelectric] one, which evidently originates from the liquid serving as a conductor. [In experiments with trough batteries] the resistance of the liquid increases at least in proportion to their size; it follows that with one pair of plates of a few square inches of surface, a value many thousand times greater than was the case in the above thermoelectric circuit. At the same time, however, it is also emerging that the exciting force in the hydroelectric circuit is far greater than in the thermoelectric circuit.
>
> (Ohm, 1826a, p. 155)

In concluding his paper, Ohm sought to underline the universality of the equation he had found. He referred to the recent works by Becquerel and Fourier and Ørsted, and argued that his equation was not in conflict with any of their findings. In making these arguments he was not only reliant on the thermocouple as a tool for experimentation, but he also explicitly stated that the hydroelectric apparatus was not suitable for this type of study, saying "it will seem questionable to apply the above expression to a hydroelectric apparatus where *a* and *b*, as has been shown, change simultaneously with the effect, but the law of change is still unknown" (Ohm, 1826a, p. 161). He also spent several pages at the end of his paper discussing how his findings might help to explain the behaviour of the multiplier, again seeking to demonstrate the validity of his equation, and again showing an interest in internal resistance:

> … they are the reason why the multiplier weakens the effect of the thermoelectric circuit in most cases; because the case will not easily arise where one turn of the multiplier presents less resistance to the electric current than the thermoelectric circuit… A needle made entirely according to [Nobili's] instructions, with the addition of a multiplier made of 1/5 line thick copper wire of 60 turns which was an inch in diameter, gave deviations that never exceeded 20 degrees by touching the circuit with the warm hand. The same circuit, on its own with a turn of the same size as that of the multiplier, made the needle deflect by an angle which always exceeded 70 degrees by touching it with the warm hand. This device can be modified so that it is in no way inferior to the most sensitive thermoscope.
>
> (Ohm, 1826a, pp. 165-166)

**The thermocouple's theoretical role in Ohm's experiment**

Throughout his account, Ohm says nothing about how the thermocouple might work. It is unclear whether his switch from thermomagnetism to thermoelectricity part way through his paper reflects a change in his theoretical stance in the process of writing, or whether he was unconcerned by the distinction and happy to treat the two terms as good, interchangeable descriptions of what he was observing. His positive commentary about the technology is entirely focused on the improvements to his practical experimental setup, and the eradication of a source of difficulty caused by the instability of the "hydroelectric" pile in his work. Very clearly, by the end of the paper Ohm is understanding the thermocouple as a means to produce electrical effects, and he interprets the deflection of a magnetised needle as an electromagnetic effect. Ohm's unquestioning adoption of the thermocouple, and implicitly thermoelectricity, has not generally been queried; he was following the work of well-established researchers including Becquerel, Ørsted and Fourier. However, as the preceding sections showed, the technology had not been definitively adopted either practically or theoretically by his contemporaries, and using it as the basis of a law of electrical conductivity required the complete adoption of Ørsted's interpretation of the thermocouple as a device that produces a thermoelectric effect. Conceivably, had Ohm not taken this theoretical leap, an alternative reading of his experiment might have been a law that described a direct relationship between "exciting force" and magnetism, mediated by a kind of magnetic resistance within the circuit. However, he was already invested in electrical investigation before introducing the thermocouple, which came recommended by Poggendorff.

A final question remains about whether Ohm's changing apparatus influenced his conceptual ideas. In practical terms, by eliminating the instability that the pile had caused, from 1826 Ohm was able to constrain his experiment in such a way that only one variable at a time was being altered. This made for a much simpler interaction with the experiment, in contrast to the compromises and awkward procedures demanded by his 1825 setup. In his new apparatus test wires could be added or removed as required, and, provided the thermocouple temperatures remained stable, the response from the torsion balance would also remain stable. Importantly this new setup also gave Ohm the ability to isolate his variables by either changing the test wires, and thereby varying conductivity or resistance in a controlled way, or by keeping the test wire consistent and instead varying the temperature difference in the thermocouple and thereby varying the electrical force in the circuit in a controlled way. Ohm

commented that in contrast to the problematic and "hard to unravel" problems with using a hydroelectric circuit, "the thermoelectric circuit is practically created for this determination" (Ohm, 1826a, p. 151).

The impact of the improved experimental apparatus can be detected in Ohm's writing between experimental setups and, I argue, his first and second phases of work. As he moved between phases, he stopped discussing "loss of force", and instead discussed "exciting force". Thinking in terms of the phenomena Ohm observed, the transition was from a "hydroelectric", or unstable voltaic trough battery, in his 1825 setup, to the much more stable thermocouple in his 1826 setup. In the first phase of work the deflected needle gradually returned to its starting position, and the rate of this return varied depending on the conductor Ohm inserted into the experiment. In the second phase of work, with the thermocouple integrated into the apparatus, the amount the needle deflected was stable throughout the observation, deflected with more or less force depending on the temperature difference applied at the thermocouple and the qualities of the conducting wire. The "exciting force" which was causing the needle to deflect could be understood to be greater with a larger temperature difference, typically achieved by an increase in the heat applied to the hot end of the thermocouple. As already discussed in detail in Chapter 3, it is unclear whether the change in Ohm's language also reflected a conceptual shift, in which his interactions with his instrument also affected the way he thought about the fundamental nature of electricity.

In tandem with Ohm's shifting apparatus and language, he updated his equation to better match his growing set of observations, discarding the logarithmic relationship he had presented in 1825 and replacing it with a linear relationship between the length of the test conductor and the force with which the magnetised needle was deflected. If his early work might be interpreted as following Coulomb, as discussed in Chapter 3, in this period he seemed to have fewer preconceived ideas, and followed where his experimental work led him. As his work became more guided by the apparatus he was using, there was an implicit shift in Ohm's descriptions and understanding of how electricity behaves. Where the steady but weak flow of electrical current from the thermocouple encouraged an understanding of current as electricity flowing more or less freely through a material, a fluid-dynamics influenced understanding of the experiment was a good fit. In contrast, the earlier setup which suggested electricity leaking away from a source leading to a loss of force suggested a Coulomb-influenced understanding of the behaviour of electricity, with some echoes of electrostatic theory. Disentangling which of these changes drove the others is difficult.

Poggendorff's recommendation of the thermocouple, and Ohm's subsequent adoption of the technology is the only evident external driver, but it may have supported an intellectual shift that was already happening, rather than starting the process.

In summary, Ohm's experiment took place at a time when the theories surrounding thermocouples were still very much under discussion. He used a thermocouple in his experiment because it was more stable than the voltaic pile he had previously been using. However, that stability did not extend to the theoretical basis for the instrument. In the decade after Seebeck's discovery it was interpreted by Ørsted to support his own theoretical position, while seemingly being overlooked by people debating the theory of the voltaic pile. Seebeck's argument that the thermocouple did not generate an electrical effect, only a magnetic one, would, if more widely accepted, have seriously damaged Ohm's claims. Similarly, the observations by Seebeck and Cumming that the correlation between temperature difference and magnetic effect or electrical tension was not always a direct one could also have introduced a challenge, though Ohm avoided those potential criticisms by using a fixed temperature difference throughout his experiment. What is clear, is how new, uncertain and fraught with theoretical difficulties the thermocouple was. Yet for Ohm, the thermocouple was a fundamental part of the experimental apparatus that he used to make his claim to have found a new electrical law.

## PART B

This section focuses on the reproduction of the thermocouple in the Connelly apparatus. This part of the instrument particularly highlighted the extent to which Ohm's description is insufficient for a "replication", leaving many gaps for anybody seeking to reconstruct the apparatus to fill. This part of the instrument also draws attention to the ways in which materials are recycled or reused in the process of developing and using experimental apparatus.

### Interpretations and reconstructions

The principal challenge with reproducing the thermocouple and its hot and cold vessels were related to the need to invent solutions where the description of the apparatus was lacking. As discussed in Chapter 2, there are a large number of parts of Ohm's apparatus for which he did not provide any dimensions. Nevertheless, in most cases it is possible to make an informed guess based on the visual impression given by the diagrams accompanying the descriptive text. Despite written and visual guidance, interpretation is required, and it is interesting to see where interpretations have differed between different reconstructions of the apparatus. For example, Ohm presents his hot and cold vessels in vertical cross section, which does not provide enough information to know the form of the horizontal cross section. Figure 28 presents this diagrammatically, showing on the left how he presented his cold vessel, then in the middle and on the right the diagram is overlaid with a graphic to indicate two different possible interpretations for the aperture that the foot of the thermocouple sat in. As discussed in more detail in the Appendix, my maker preferred the tubular (middle) option, on the basis it was easier to manufacture than a square one. However, where the hot and cold vessels are still extant in other reconstructions, they have interpreted the opening as having a square opening (right hand side). One side effect of the circular opening in our interpretation was that it provided plenty of space for the lead shot and chopped glass that Ohm specified to pack the foot of the thermocouple in. Some of the other reconstructions appear to have the foot of the thermocouple fitting into the aperture very snugly, which makes it hard to see how any lead shot could be packed around it. There is no way to know which interpretation is correct, or indeed if there is another alternative none of us has considered.

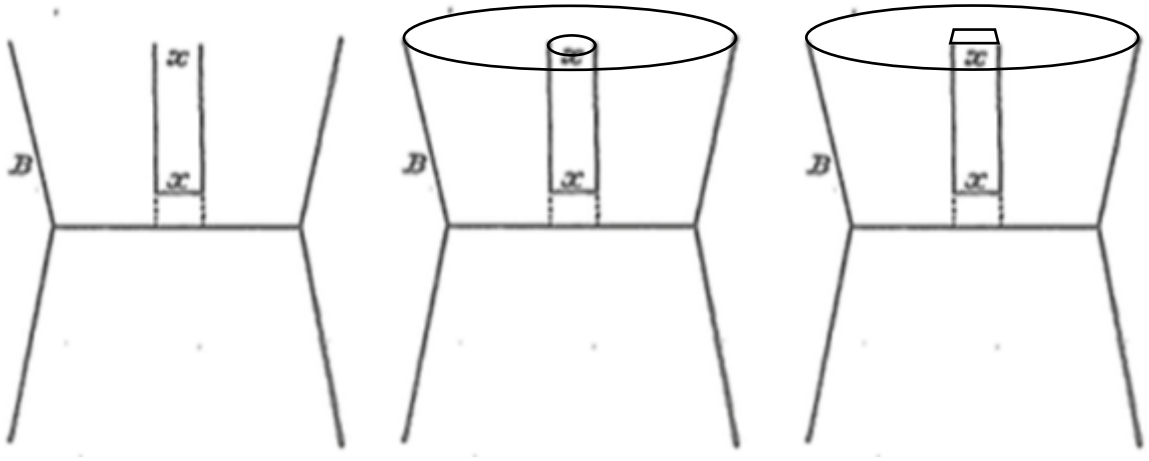


**Figure 28.** Diagram of cold vessel taken from Ohm's 1826a paper (left), overlaid with a tubular aperture (middle) and overlaid with a cuboid aperture (right).

This is just one example where Ohm's descriptions lacked necessary detail, meaning that in order to produce the Connelly apparatus the maker and I had to collaboratively develop our designs. In this instance the choice might seem trivial, however when it came to filling the aperture with lead shot, as instructed by Ohm, it was clear that a larger opening would have been preferable. Indeed, having operated the instrument, it might be the case that larger hot and cold vessels, including the central apertures, would have been beneficial for a variety of reasons including: making it easier to fill the apertures with lead shot, having a larger amount of hot water in the hot vessel and therefore having to refill it less frequently, and being able to include a burner with a larger fuel reservoir to reduce the need to either refill it or incorporate an external reservoir.

Despite the frustrations outlined above, it is unlikely they had a major impact on the way electricity flowed through the apparatus. In contrast, in the Appendix there is a thorough report of the design and manufacture of the bent copper element in the thermocouple, and how it differed from the interpretation embodied in the Flensburg apparatus (Appendix, p. 253). This is important to the functioning of the instrument because the design interpretation in the Flensburg apparatus results in the cross-section of the copper conductor within the fixed part of the circuit varying along its length (Figure 29). In my opinion, the variable cross section caused by this decision runs contrary to Ohm's intentions, particularly as we know he was aware that cross section was an important factor in resistance and he took care to control

for it in his test wires. However, Ohm does not offer clarity in his writing about which of these designs is more likely, so ultimately the decision is a matter of interpretation.

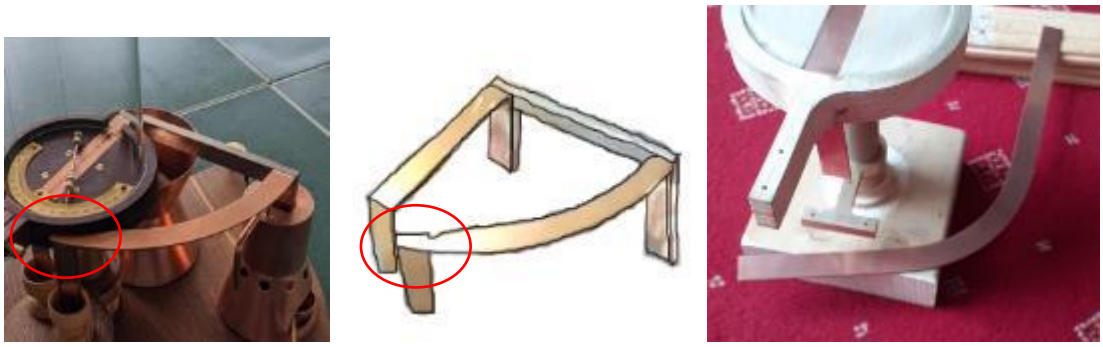


**Figure 29.** Flensburg apparatus (left) with narrowing of copper element before it is bent down to the mercury cups; diagrammatic representation of the thermocouple elements in the Flensburg apparatus in isolation (middle); work in progress image of the Connelly apparatus (right) showing the copper piece bent into a curve. (Left image courtesy of Wolfgang Engels)

A final example of Ohm's under-described apparatus is the design of the base that the apparatus sits on. In the Science Museum apparatus, very little attempt has been made to follow Ohm's diagram, suggesting that the makers simply thought it was not important to the functioning of the instrument. The Oldenburg and Deutsches Museum apparatus both incorporate variations on an equilateral triangle, but there is no documentation available to aid in understanding that decision (Figure 30).

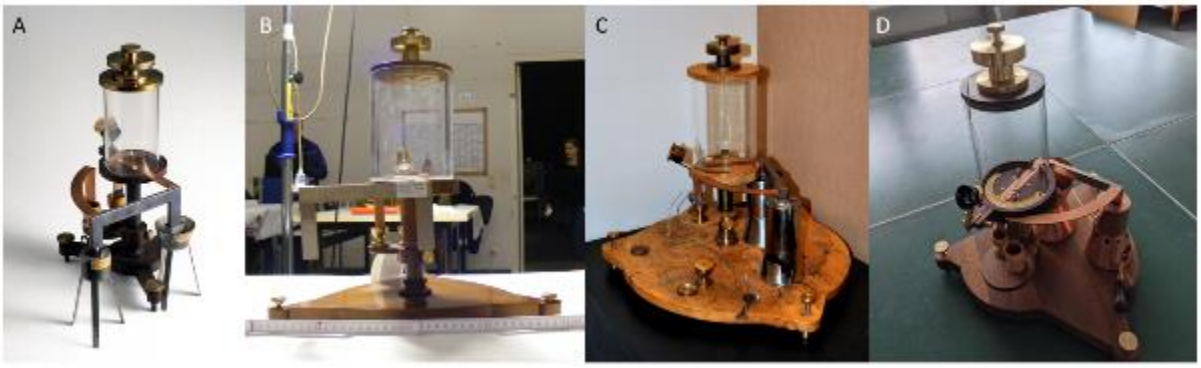



**Figure 30.** Comparative views of baseboard interpretations: (A) Science Museum apparatus, (B) Deutsches Museum apparatus, (C) Oldenburg apparatus and (D) Flensburg apparatus. (Image A is released under a Creative Commons Attribution-NonCommercial-ShareAlike 4.0 Licence, © The Board of Trustees of the Science Museum; Image C courtesy of Wolfgang Engels; Image D courtesy of Wolfgang Engels.)

Engels and Riess, whose design can be seen in the Flensburg apparatus, also arrived at a variation on an equilateral triangle. Offering a thorough analysis of their design, they start off by noting that the perspective in the drawing in Ohm's paper appears to be broadly correct despite a small number of errors, and concluding, as we did, that the shape of the baseboard was almost certainly arrived at after the other parts of the instrument were complete (Engels & Rieß, 2022, pp. 98-101). Despite broadly sharing the approach that Engels and Riess took, we nevertheless arrived at a different design for the Connelly apparatus. Engels and Riess ask why the draftsman does not depict the base symmetrically "as one might expect", suggesting a preconceived idea that the baseboard ought to be essentially an equilateral triangle, with additions (Engels & Rieß, 2022, p. 98). In contrast, in developing the Connelly apparatus we looked closely at Ohm's diagram and concluded that the base appeared to be a right angle triangle, making that assertion one of our starting assumptions. Having together agreed on a set of assumptions from Ohm's text and drawing, we then developed the shape of the base from the needs of the parts of the apparatus we already had built (Appendix, p. 249). We, too, thought there may be problems with the drawing and resolved not to try to follow it too closely, and were surprised when our end result was very close to Ohm's (Figure 31).

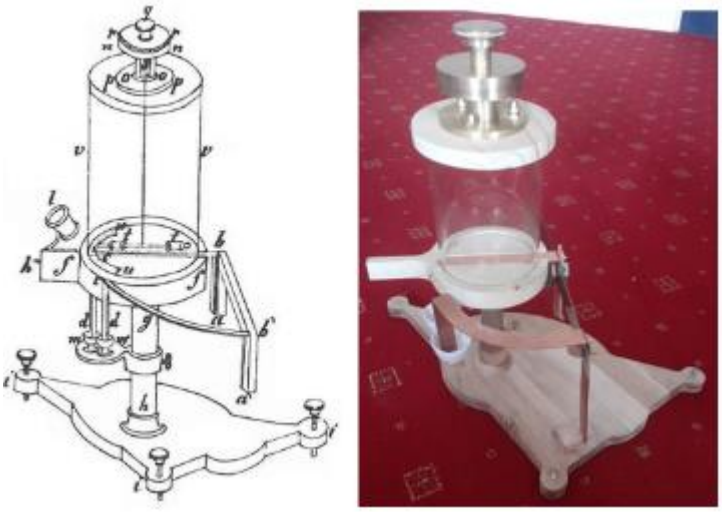


**Figure 31.** Ohm's drawing of his baseboard alongside the Connelly apparatus (work in progress) showing the baseboard. (Left image from Ohm 1826a; right image Courtesy of Martin Connelly.)

As both we and Engels and Riess report, the design of the base becomes very important when operating the instrument. Finding an appropriate form for the base is not a trivial concern. The curious bulges on the base are there to accommodate parts of the apparatus and offer

additional stability, the screws on the corners of the base are important for calibration, and, most fundamentally of all, the top-heavy instrument would be at risk of toppling and breaking without a sturdy, well-designed baseboard to sit on. We can conclude that Ohm's deliberately designed base enabled him to calibrate his instrument reliably, and use it without risking breakage. In the Appendix there are many more examples of the need to proactively make design decisions while reconstructing the apparatus, due to gaps in Ohm's description.

**Recycling and reuse**

The concluding part of this section relates to the ways an experiment and experimental apparatus develop through use, and how everyday household items become integrated as quick solutions to problems that arise. An example of recycling materials for use in the apparatus has already been mentioned; I noted earlier that some old violin rosin was melted down to coat the tips of the test wires. Just as I, and the maker I worked with, did not purchase new materials unnecessarily where we had access to something suitable already, it seems unlikely that Ohm or his maker produced everything from scratch. The importance of recycling of both materials and ideas has been investigated by historians of science, though predominantly with a focus on the early modern period (for example see Werrett, 2019).

It has been notable throughout the reconstruction project that the design of manufacturing solutions, and the apparatus itself, have been driven by a desire not to waste materials, whether by recycling and repurposing or by using off-cuts, spares and scrap materials. Examples of this sort of recycling in museum collections of scientific instruments are rare; there can be a tendency to collect the pristine and apparently, or actually, unused. Simon Schaffer, among others, argues that instruments are prone to faults and failure by nature: "Faults are defaults, yet instruments perform"; however, "states of disrepair are often not deemed worthy of display, even though – perhaps because – they show signs of use" (Schaffer, 2011, p. 707). To return to Ohm, perhaps his 1826 instrument does not survive now, despite some of his other apparatus being in the collections of the Deutsches Museum, precisely because its parts were recycled and reused for other things.

Another example of domestic repurposing arose from the challenges with the burner. On occasion the burner would suddenly start to burn very hot as the fuel reached its flash point; other times the flame would lick over the side of the burner as the fuel expanded and leaked over the top of the wick, causing the fuel or its fumes would spill over the edge onto the base

of the instrument. I was concerned that the base of the instrument would catch fire, and my solution to this was to take a piece of slate from my garden, which presumably at one time had been a roof tile, and slide it under the hot vessel. Other items from around the house were pressed into service. Meat thermometers from the kitchen were helpful for assessing the temperature of the hot and cold feet of the thermocouple, two pieces of wood from the garden were adapted as spacers for wrapping the longest test wire around to make handling practical, violin rosin was used to coat the tips of the test wires, and one of the routines I developed when performing the experiment was to light a candle to serve as a pilot light rather than using lots of matches. Recycled materials were also incorporated into the instrument itself. The hot and cold vessels were produced from unused pieces of domestic copper pipes, the magnetised needle was made from a piece of steel bike spoke and the wooden base had at one time been a shelf. It is unlikely any of these co-opted materials would make it into a formal write up of the experiment if I was not specifically paying attention to the networks of materials that influence and shape experimental apparatus. However, following the argument presented in Chapter 2, when writing about the ways in which instruments embody residues of thoughts and ideas from a range of people, from the reproduction of the apparatus it is also clear that my reconstruction carries traces of the domestic lives of both me as experimenter and my father as maker. Should some future historian try to "read" the Connelly apparatus, they may be able to detect these settings from the object itself.

# 5. The Torsion Balance

## PART A

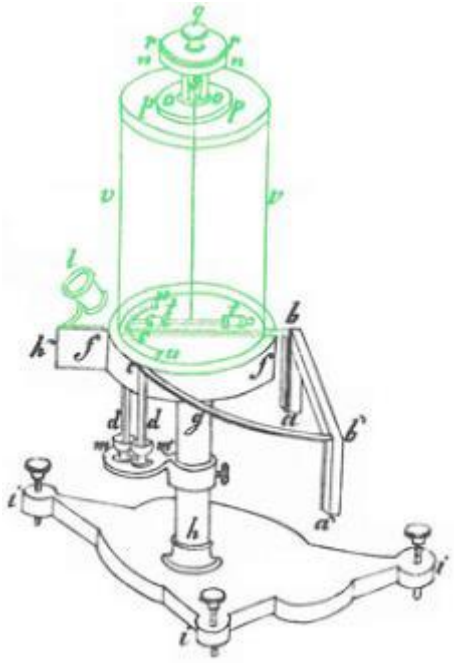


**Figure 32.** The torsion balance in Ohm's 1826 experimental apparatus

This chapter explores Ohm's use of a torsion balance in both his 1825 and 1826 experimental setups (highlighted in green in Figure 32). Opening with a description of Ohm's torsion balance, this chapter notes that Ohm's apparatus was somewhat unusual: the torsion wire was surprisingly short, and, in a break with convention, had been flattened into something ribbon-like. To understand the implications of these changes, which had the potential to undermine Ohm's entire experiment, the chapter offers a discussion of Coulomb's detailed investigations of the torsion balance, as well as his applications of it to both electrical and magnetic research. Providing further context to Ohm's use of the technology, a discussion of Ampère's work on electromagnetism in the immediate aftermath of Ørsted's discovery reveals surprising similarities between Ampère's experiment design and Ohm's. This is a novel observation, due in part to the fact that few people have looked closely at Ampère's experimental work in this period. Ampère's work, and its influence over Ohm's work, is striking. Not only did his experiment design provide a model for Ohm to follow, but Ampère also pioneered a new understanding of the electrical circuit as a whole system. Tying the

physical experiment and the intellectual ideas together, Ampère, like Ohm later, sought to make mathematical claims from his observation of the torsion balance. The chapter closes with a discussion of recent scholarship in the philosophy of measurement and the role that the torsion balance played in Ohm's measurement claims, paving the way for Chapter 6 which takes a closer look at Ohm's third phase of work and his 1827 mathematical treatise.

The torsion balance is represented in Ohm's diagram by the parts labelled *n* through *u* alphabetically. The balance consists of a magnetised needle suspended from a torsion wire, which itself is suspended from a brass piece which Coulomb called a "micromètre de torsion", referred to throughout this project as the micrometer (Coulomb, 1785, p. 570). Ohm used the word "Drehwage" to refer to both the torsion balance as a whole, and the micrometer within the torsion balance. In translating, a judgement has been made about which part he meant in each case, depending on context. In the torsion balance Ohm worked with, a loupe and lower scale were incorporated for ease of observation, and an upper scale on the micrometer enabled Ohm to read and record a number during his observations.

I have translated and transcribed Ohm's description here, interspersed with my annotations:

> The glass cylinder *vv*, on which [the micrometer] is set, is 6 inches high and 4½ inches wide.

This is short in contrast to Coulomb's design, and the other torsion balances which followed. The relative shortness might be accounted for by Ohm's use of a ribbon-like flattened wire to suspend the needle, rather than the usual cylindrical wire. However, it is unclear whether the short glass cylinder drove the need to flatten the wire, or whether Ohm had tested the flattened wire and knew that therefore he only needed a relatively short glass cylinder. The implications of flattening the wire are discussed in more detail later.

> [The micrometer] itself consists of two parts, one of which *nop* is provided with a slightly conical socket, and is cemented fast to the upper plate of the glass cylinder; the other, *qrs*, with a conical projection 8 lines thick fits exactly into the cavity of the socket, and with a plate *rr* 3 inches wide which rest on the plate *nn* of the first part, which is the same width. The midpoint of the projection *qs* was marked with great care on the lathe by a slight conical depression, and the metal was then filed off for a half inch of its length until the plane surfaces which are thus developed show the conical depression as a complete triangle. A special device clamps the thread on which the needle is attached to the projection so that its midpoint falls exactly in the apex of the triangle.

This requires some careful reading to fully understand. Essentially what Ohm is describing is that the part of the instrument that the needle is suspended from can freely rotate, enabling the user to wind or unwind the piece of metal ribbon that the needle is suspended from. Part B offers a discussion of the different interpretations of the form of the micrometer parts, showing that different reproductions of the instrument have taken different approaches. Ohm adds a footnote to this paragraph:

> This torsion balance was manufactured by the very skilled maker Mr Mauch. In the local cabinet there are two conventional torsion balances that came from Strasburg, but I soon realized that they could not be used for precise tests, as they give rise to errors of 30 or more parts.

The "local Cabinet" mentioned above is presumed to refer to the collection at the school where Ohm worked. For more information about the provenance of the school's cabinet see Quarg (1994). The main text continues:

> The magnetic needle *tt* is made of steel wire 0.8 lines thick and is not quite 2 inches long. Its two ends are inserted in cylindrical pieces of ivory, to one of which is attached a brass wire, tapered to a point and bent slightly downward. This brass pointer, which serves as an indicator, lies just above a brass arc *uu*, resting on the base and divided into degrees. At the start I made the magnet so long that its end floated just above the graduated scale; but its slow motion, as shown by the small number of its oscillations, reminded me of the experiment lately made by Arago, and led me to change to the other arrangement.

The arc *uu* is later referred to as the "lower scale" and is one of two places where the user can "read" a result from the instrument. In my experiments with the reconstructed apparatus it was mostly useful for checking that the needle was realigned with "zero" as part of the experimental procedure. It also helped to assess the needle's oscillation when waiting for it to settle. In the paragraph above Ohm writes "I made the magnet", which could either be a turn of phrase, or an indication that he was involved in the hands-on manufacture of this part of the apparatus. Magnetising a piece of steel is not terribly complicated, but nevertheless this is perhaps a clue into the level of Ohm's involvement in developing the instrument.

> The needle prepared in this way is suspended from a strip of Goldlahn [flattened gold wire] 5 inches long, which is fastened to the torsion balance exactly in the axis of rotation. These ribbon-like strips of metal are in my experience much better suited for experiments with the torsion balance than cylindrical wires. Aside from its shortness, which is so desirable for many reasons, the strip that I used in my torsion balance has all the requirements for investigations with the torsion balance to such an extent that the needle will resume its original position even after it is released from the strain of

> more than three full revolutions. Nevertheless after each experiment I examined the needle at rest, so as to be convinced that the apparatus had undergone absolutely no change. Furthermore I should remark that previous tests carried out with a similar needle of brass convinced me that small and great oscillations (I examined them from two whole revolutions down to a few degrees) always took exactly the same amount of time, so that in this respect also there is nothing to be feared.

Ohm judged that his use of a flattened wire improved the apparatus. However, it is an intervention that is not seen elsewhere in the very few surviving examples of torsion apparatus from the period, or from careful reading of Coulomb, Biot and Ampère's work with torsion balances (see further discussion of torsion balances in museum collections on p. 180). It appears that all of Ohm's immediate peers were using torsion wire with a circular cross section in their instruments. Ohm, perhaps mindful that his decision required some justification, took the trouble to explain his checks and measures to ensure that the strip returned to its original position after it had been twisted, and to check that the oscillation period of the needle was consistent. Whether or not questions would have been raised without this additional explanation is unclear, however there do not appear to have been any queries raised about this aspect of the experiment. Nevertheless, this alteration to the instrument was an unproven and untested change, and one that should not be overlooked. It introduced an unknown element into the part of the instrument where precision measurement and a linear response needed to be assured. If Ohm's change had meant that the torsion balance no longer responded in a linear way to twisting forces, none of the measurements made with the instrument would have been meaningful.

> The torsion balance was firmly cemented to the upper plate of the base in such a way that when the magnetic needle was in the magnetic meridian it was set to zero on the arc *uu* and lay in the same direction as a straight line drawn down the middle of the copper strip *bc* and through the middle part of the arc *uu*; a simple silk thread stretched perpendicularly before the arc. On a protrusion *k* of the base was carried a convex lens *l* of an inch focal length, set in such a position that the lower divided scale could be observed, and in order to avoid parallax, the eye was always so placed during an observation that the silk thread and the midpoint of the scale coincided.

The simple silk thread described here was either never incorporated into the reproductions of Ohm's apparatus now in the collections of the Science Museum and Deutsches Museum, or the threads have not survived. In Part B there is some discussion of how the physical form and location of the thread might be interpreted.

> The observation was made such that every time the needle was deflected by the electrical current in the apparatus [causing the torsion wire, or strip, to twist], the strip

> was twisted in the opposite direction [untwisting it] by turning the moveable part of the torsion balance until the brass tip of the needle stood behind the silk thread on the midpoint of the scale; then the magnitude of the rotation at the top of the torsion balance was read off in hundred-parts of a complete revolution, which number, as is known gives the force which was acting upon the needle.

It is not clear whether the degrees marked on the lower scale served a purpose, or whether Ohm used the lower scale principally for judging when the needle had returned to zero. Possibly the gradations were useful when aligning the instrument and needle with the magnetic meridian. In reworking the experiment, I found the gradations useful for observing the needle while it was still oscillating, and judging whether the micrometer needed to be turned further. Here Ohm also mentions, almost in passing, that the freely rotating part of the micrometer, which the needle is suspended from, is marked up with a scale showing one hundred equal divisions. This is where Ohm reads a numerical value from the instrument as a measure of the force which acts on the needle. Another footnote here gives a bit more detail about reading values from the instrument.

> The relationship between the needle and the strip was such that in order to advance the needle by a degree of lower scale, the torsion balance had to be rotated by ten divisions [hundredths]. With this arrangement it was not difficult to see $1/40^{th}$ of a lower degree well, and to estimate ¼ of an upper division. At the same time, however, it is clear from this that a deviation at the point of rotation of as little as half a line can lead to errors of 20 or more parts [in the upper scale]; one should not be disconcerted by this, given the anxious care with which I had my torsion balance built. Perhaps this is precisely the reason why people other than Coulomb were unable to achieve the same results with the torsion balance.
>
> (Ohm, 1826a, pp. 144-147)

In this final comment, Ohm indirectly acknowledges that the torsion balance is viewed with some scepticism by researchers as few had been able to achieve reliable results. Again, more on the challenges of using the instrument will follow, but it should always be kept in mind that Ohm was using the instrument for electrodynamic measurements while the most contested measurements made by Coulomb were of electrostatic effects. This has several implications, which the next section begins to unpick.

**How a torsion balance works**

Where Chapter 4 argued that Ohm's use of a thermocouple for producing a measurable electromagnetic effect that could be used as a proxy for electric current was in theoretically uncertain territory, this chapter will show that Ohm's use of a flattened ribbon-like piece of metal in place of a wire in his torsion balance was also theoretically unsound and had the potential to undermine his findings. To make sense of what Ohm was doing, and why it might have been problematic for him to use a flattened ribbon-like torsion wire, rather than a wire with a circular cross section, it is useful to understand how a torsion balance works and what was known in the period when Ohm was doing this work. For this, it is useful to go back to Coulomb.

Coulomb was trained in mechanical engineering before he began work on magnetism, and later electricity. His thorough understanding of forces on geometric objects, developed through mathematical training as well as the practical work of building bridges and fortifications, gave him a good background for developing the torsion balance. He demonstrated experimentally that needles suspended from silk threads and metal wires oscillated with a simple harmonic motion, and that the force of torsion was proportional to the angle through which the thread had been turned. Through his work in the 1770s and 1780s, Coulomb sought to determine the effect of the length and diameter of the torsion thread and the tension imposed on it by the load applied to the thread. He showed that torsion varied inversely with respect to the length of the suspending wire, that the torsion was proportional to the fourth power of the diameter of the wire, and that load on the wire had little or no effect. Within certain parameters, Coulomb obtained the following general equation for the "momentum de la force de torsion" or torque, where $M$ is the torque, $\mu$ a constant rigidity coefficient, $B$ the angle of torsion, and $D$ and $l$ the wire diameter and length respectively (Gillmor, 1971, pp. 140-155).

$$M = \frac{\mu B D^4}{l}$$

Because the torsion in the wire is proportional to the fourth power of the diameter, a modest reduction in the diameter of the wire resulted in a significant increase in the sensitivity of the instrument, making it quite easy for the user to adapt by substituting in a thinner or thicker wire. It should also be noted that the theory of the torsion balance was separate from theories of the force being measured. Although Coulomb initially described the instrument in a memoir on electricity and magnetism, and used the instrument to measure both types of

effect, it can be applied just as well to a range of other effects and today is best known for its use in gravimetry.

A key point with Coulomb's equation is that he explicitly only dealt with wires or threads that had a circular cross section. The formula above does not accommodate a cross section of any other shape, such as Ohm's ribbon-like "Goldlahn". Chopin and Filho's (2019) paper explores the "extreme contractility and torsional compliance of soft ribbons under high twist", demonstrating that the way ribbons under torsion behave is still the subject of research, some two-hundred years after Ohm was working. They preface their work with a historical summary, which notes not only Coulomb's celebrated formula, but, more importantly for our purposes, that it was not until 1855 that Adhémar Jean Claude Barré de Saint-Venant developed a theory that allowed for an exact formula for the twist moment for an arbitrary cross section, for example of an inextensible ribbon. Saint-Venant's work was soon followed by Wertheim, who published an extensive memoir on torsion using prisms of circular, elliptical, rectangular and tubular cross sections in a variety of materials in 1857 (Timoshenko, 1983, p. 220). This flurry of activity to understand torsion in columns of different cross sections was almost thirty years after Ohm's experiment was published. As highlighted above, Ohm provided additional description of his checks and measures when using the balance, perhaps to relieve his readers of any concerns they may have that the strip of metal he used did not perform as well as a standard wire. However, his paper falls some way short of demonstrating that his measurement tool is performing as claimed.

There is another aspect of the use of torsion balances that should be understood in order to make sense of Ohm's instrument. The torsion balance can be used in two modes: the "dynamic" oscillating mode or the "static" deflection mode. In the former, the needle is deflected by the force to be measured, and, a bit like a horizontal pendulum, its oscillations as it returns to rest are timed and used to calculate the initial force. In the latter mode, the needle is deflected to a stable position and that angle of deflection is measured either by simply measuring the angle of deflection in degrees, or, where the instrument is more sensitive, by using a set-up similar to Ohm's where the number of degrees of turn required to return the needle to its at-rest position are measured. Coulomb worked with both methods, using the balance in deflection mode to verify results previously obtained in oscillating mode. (Gillies & Ritter, 1993, p. 285). In both his 1825 and 1826 setups, Ohm was using the balance in its deflection mode, though in the first phase of work the instability of the experiment meant that

the needle was not “static”, but rather drifted back towards its starting position throughout the experiment.

Finally, just a brief comment that in reading about the torsion balance and trying to understand how Ohm might have used it, it is easy to get drawn into the lengthy and ongoing debates about Coulomb’s measurements of static electricity (see Heering, 1994; Martinez, 2006; Palmieri, 2009). For our purposes, this is a distraction as these discussions all refer to a different use of the torsion balance. Coulomb was using the balance to measure an unstable discharge of static electricity; in contrast, by 1826 Ohm was using the balance to measure a continuous and stable flow of electricity in an electrodynamic setup. The challenges that Coulomb faced in producing his controversial electrostatic law are largely not applicable in Ohm’s case. Despite this difference, it was the case that the torsion balance was perceived to be a difficult instrument to use, as Ohm hinted at in the footnote to his paper.

**Uses of a torsion balance to measure electricity**

There were just two notable users of the torsion balance for electrical measurement in the fifty years between Coulomb’s work in the 1780s and Ohm’s work in the 1820s. The first was Biot, who made use of Coulomb’s torsion balance to measure electrical forces produced when an open pile was used to charge a condenser. This formed part of his work in 1803 to collect “expériences incontestables” to demonstrate the contact force. Biot faced numerous challenges with the instrument, including problems with accidental grounding and difficulties ensuring constant contact between parts of the equipment. He designed special apparatus to make the balance work for his needs (for a detailed account see Brown, 1969, pp. 73-74). Exactly what Biot conceptually thought he was measuring is hard to define. As Friedrich Steinle notes, “Biot’s notion of tension (and the notion prevalent in the several decades to follow) was neither particularly sharp or congruent with our contemporary concept, as indicated already by the fact that Biot speaks interchangeably of ‘quantity of electricity’ (*quantité de électricité*) and ‘electrical state’ (*état électrique*)” (Steinle, 2016, pp. 45-46).

Our second notable user of a torsion balance after Coulomb was Ampère, who made careful use of the torsion balance in his early experimental work on electrodynamics. Importantly, Ampère’s introduction of the torsion balance into his experimental setup was his attempt to move beyond qualitative indicating, and instead to produce quantitative numerical results that could contribute to a mathematical analysis of his electrodynamical findings. Ampère’s

particular use of the torsion balance was within a complex experimental setup pictured below in Figure 33 (taken from Ampère, 1820). The paper where the instrument appeared was one of several that he published in quick succession in the wake of Ørsted's announcement of his discovery of electromagnetism. He described the instrument as a "current balance", and used it to investigate forces between two different electrical currents or conductor segments.

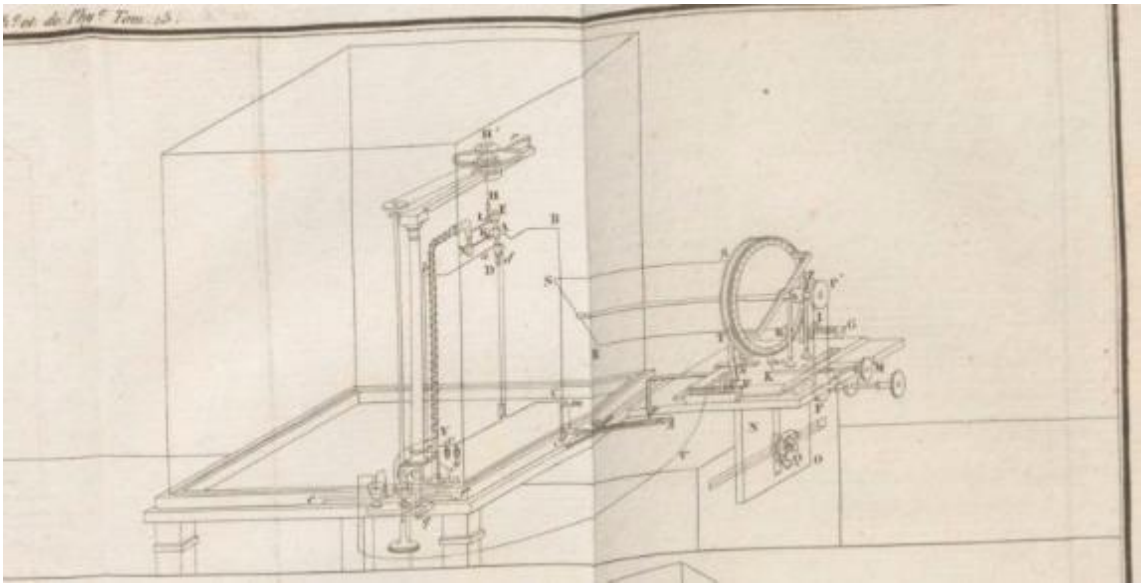


**Figure 33.** Diagram of Ampère's "current balance". (Plate 4 from Ampère, 1820. This image is made available for non-commercial use, source: gallica.bnf.fr. © Bibliothèque Nationale de France.)

The image above is not straightforward to interpret, so an annotated version is included in Figure 34. The experiment hinges on the observation of the interplay between a mobile conductor *BC*, and a fixed conductor *RS*. The complicated arrangement included a wheel that enabled the user to precisely control the orientation of the fixed conductor. The fixed conductor segment was connected simply to a battery via trough-shaped mercury-filled beakers, *G* and *F*. The arrangement for the mobile conductor segment is more complicated. It formed part of a larger loop, all of which is suspended from a torsion thread *H'H*. The whole segment was free to oscillate around the axis provided by the vertical torsion thread, and the design of the double loop incorporating *BC* was intended to neutralize the effects of terrestrial magnetism. Once again, mercury beakers were used for connecting up a battery, though this time they are presented as egg-cup shaped vessels. Steinle summarises Ampère's description of using the instrument, saying:

> Measurements using this rather complex device were to begin with calibration in its inactive state, ensuring the vertical alignment of the mobile conductor above point m. When the current was switched on, it would move from this default position, toward or away from RS, and could then be returned to it by twisting the torsion wire [using a micrometer]. The degree of twisting necessary for this purpose could be read off scale r, yielding the magnitude of the electrical force now exerted between the two wire segments.
>
> (Steinle, 2016, pp. 157-158)

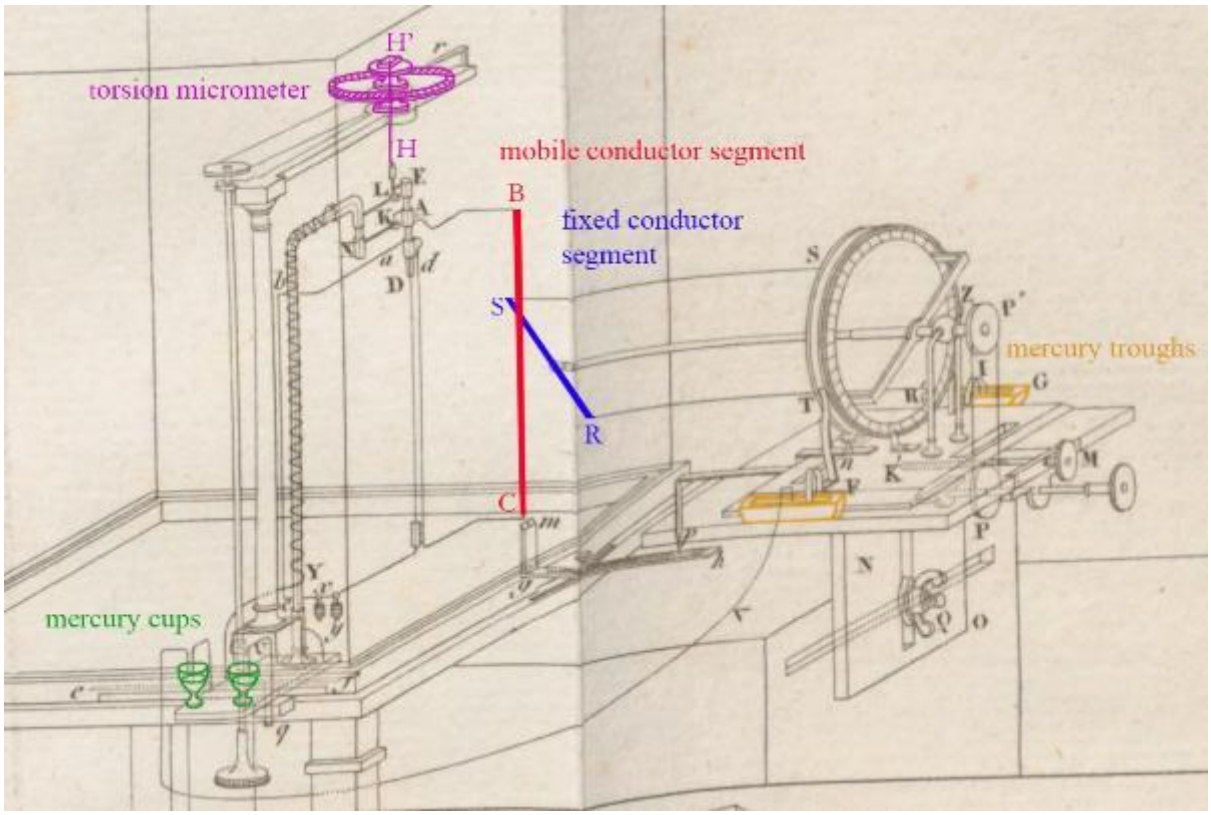



**Figure 34.** Annotated diagram of Ampère's "current balance". (Plate 4 from Ampère, 1820. This image is made available for non-commercial use, source: gallica.bnf.fr. © Bibliothèque Nationale de France.)

We can see a number of similarities here between Ampère's equipment and Ohm's set up which have not previously been highlighted. Indeed, in his reflections Steinle comments that Ampère's use of the torsion balance has "thus far escaped the attention of historical scholarship" (Steinle, 2016, p. 158). Steinle comments further that Ampère's measurement protocol was a direct adoption of Coulomb's procedures. The particular similarities that we should note between Ampère's and Ohm's setups are: that they both used a graduated micrometer in order to take a numerical reading from a torsion balance, which in turn was

intended to stand for a value for the force deflecting the mobile part of the balance; that they both used the torsion balance to measure electric current; that they both used the torsion balance with a closed pile, unlike either Coulomb or Biot who were working with open piles, though in his 1826 setup Ohm substituted his pile for a thermocouple; and finally that there is a visual similarity in the form of the egg-cup shaped beakers for holding mercury depicted in the fixed conductor segment, though this was common practice and of less importance. They also both show clear signs of being influenced by Coulomb's work. As well as the similarities in the instrument design, they both had the same problem to overcome with the use of the pile. Again, Steinle provides a succinct and clear explanation of problems which are familiar after reading in earlier chapters about the difficulties Ohm faced with his 1825 apparatus.

> In addition to the notoriously difficult problem he had encountered in his first electromagnetic experiments, that of maintaining the best possible electrical contact without sacrificing mobility, he now had to contend with the fact that the strength of voltaic batteries did not remain constant over time but decreased constantly and rapidly. Whereas the first of these problems could at least be mitigated by submerging the wire ends in beakers of mercury, there was no technical way of eliminating the latter. Instead, he was forced to consider measurement and interpolation protocols by which the effect could be calculated from the experimental results… In a series of measurements with incrementally varying angles between the two conductor segments, every measurement for a given angle setting had to be preceded by a calibration step in which the two segments were realigned in parallel. This yielded a curve for the decrease in battery strength over time, by which measurements could be renormalized to constant strength.
>
> (Steinle, 2016, pp. 158-159)

The descriptions of using beakers filled with mercury to mitigate problems with making good electrical contact, and the alternation of measurement and calibration steps in order to interpolate and calculate experimental results are virtually the same as those described by Ohm. This relationship between Ampère and Ohm's experimental setups is all the more notable given that the majority of electrical experimenters in the early 1820s were focused on developments in galvanometers, a different type of instrument that relied on the angle of turn as a measure rather than the force of deflection. Not only has Ampère's experimental work been neglected by historians, likely because it was a dead-end for his own research, but this is also the first time that the similarities between Ampère's work and Ohm's have been noted.

**A theory of the circuit**

Having looked at the detail of Ampère's current balance experiment, this section seeks to place it in the context of his wider work to investigate electricity. It is worth noting that Ampère had not been particularly involved in studies of electricity before Ørsted's announcement in 1820, which inspired Ampère to undertake an extraordinary period of experiment and the production of numerous publications over the following year. Several historians have noted the unexpectedness of this sudden enthusiasm, which does not directly follow from Ampère's work preceding Ørsted's announcement (see Blondel, 1982, pp. 65-66; Caneva, 1980, p. 123; Steinle, 2016, p. 82). Just as Chapter 4 described how Ørsted's discovery had been met with a mixed reception in Paris, Ampère's work was not universally warmly received either. However, Ampère found allies in a group of researchers that Caneva has called the "etherians". The group included Arago and Fresnel, and was characterised by their commitment to the idea that ether played a unifying conceptual role. For many of the etherians, this framing encouraged theorising about the interplay between phenomena including electricity, magnetism, heat and chemical reactions (Caneva, 1980, especially pp. 123-126). As this characterisation hints, although Ampère may not have worked extensively on electrical research before, he was sympathetic to the idea of a relationship between electricity and magnetism as well as other phenomena. Indeed, Blondel suggests that unifying electricity and magnetism was a youthful ambition articulated by Ampère as early as 1801 (Blondel, 1982, p. 66). However, Steinle cautions that contrary to these historical analyses, his reconstruction of Ampère's earliest work does little to justify the claim that he was working towards any such single well-defined goal (Steinle, 2016, p. 86). Unpicking Ampère's wider motivations here is beyond the scope of this thesis, but Steinle's thorough analysis of Ampère's work in this period provides an excellent starting point. The final point to make here on Ampère's views of the interrelatedness of different phenomena is about his understanding of the relationship between electricity and galvanism, which in 1820 were not universally understood as manifestations of the same phenomenon. Relatively early in his research, Ampère set out to establish whether the effects observed in Ørsted's electromagnetic discovery genuinely were *magnetic* and *electrical*. This was complicated by the fact that there was still no agreed consensus on whether electricity and galvanism were the same effects, or, to put it another way, whether the observed effects from an open pile had the same root cause as the observed effects from a closed pile. Initially Ampère was cautious about suggesting the two terms and associated phenomena were interchangeable, particularly after some failed experiments with electrostatic generators which he intended to compare

with electrodynamic batteries. Initially, he referred only to “galvanic” currents in his experimental work. This would change later as his understanding deepened (Steinle, 2016, pp. 96-97).

Perhaps a bigger departure for Ampère from his previous work was not his choice of topic, but that his foray into electrical research also marked his first venture into experimental practice. His initial experiments used the magnetised needle as an indicator; he produced textual descriptions of the needle turning left or right in a particular plane, but did not go so far as to use the magnetised needle as a measuring device that enabled quantitative data to be recorded. However, for Ampère and his peers in Paris there was a strong mathematical tradition in physical research, and he sought to introduce numerical values and mathematical analysis into his work. The galvanometer, which had been independently invented by several people following Ørsted’s discovery, was still not developed into a precision measuring device. Whereas following Coulomb’s famous work, the torsion balance was emblematic of precision measurement. Ampère adopted it into his apparatus, using Ørsted’s electromagnetic effect to drive the deflection of the magnetised needle.

An early finding made by Ampère in the course of his experimentation was accidental. He noticed that it was not only the wire connecting the battery’s terminals that was magnetically active, but also the battery itself. Although the necessity for a circuit to keep current flowing in electrodynamic setups had been known since Volta’s introduction of the pile in 1800, theories of the battery and theories of conduction had been dealt with separately. We saw in Chapter 3 the work of natural philosophers including Leslie and Davy trying to understand how the properties of a conductor affected current, without any detailed thinking about the interactions of the Leyden jar or pile, respectively, on what they were observing, beyond the practicalities of making the experiment work. Similarly, there were lengthy debates about what was going on within the pile, which paid little heed to the conductors joining the two terminals together. This was true from Volta’s initial announcement of the pile, in which the connector was usually himself, enabling him to report on the sensations he felt from being part of the circuit. Even where he introduces the idea of an electrical “circle” in presenting the crown of cups, the chief observation is that a break anywhere within the circle causes the flow of current to cease everywhere. However, the crown of cups is effectively discussed as a deconstructed pile, while the wire that connects the first and last cup of the crown receives little attention (Volta, 1800, p. 414). Ampère’s innovation, preceding Ohm’s similarly pioneering work, was to investigate the battery and the connecting wire as a whole system,

understanding that changes made anywhere in the system, whether in the battery or the conducting wire, affected what took place throughout the entire circuit.

It was only with this work by Ampère that the word current began to refer to something close to today's conception of the term (Brown, 1969, p. 62; Steinle, 2016, p. 91). Before that, the term "courant electrique" had been used by researchers including Volta and Biot, usually in reference to specific theoretical processes taking place within a wire. At the start of his frenzy of publishing on electromagnetism, Ampère carefully used the expression "courant galvanique", following the lead of others to mean a directional current between the poles of the battery. His decision to talk about *galvanic* rather than *electric* current, together with his careful and often complex descriptions of the magnetised needle turning "left" or "right" within a particular plane, appears intended to be as unencumbered by theory as practical, and set him apart from researchers who were using the language of current to make claims about what was happening on a micro-scale within the wire. Furthermore, Ampère was explicit about using the term instrumentally as "a convenient term with which to define the directions at issue. This expression may be used as an expedient, so long as one does not admit the real existence of any such current." Similarly, he took care to use the phrase "galvano-magnetic" rather than using the language of electromagnetism introduced by Ørsted (Steinle, 2016, pp. 92-95). This very particular use of language makes it all the more notable that a few months later Ampère began using different terms. Influenced by his new concept of an electrical circuit comprised of both a battery and the connecting conductive materials, and an investigation that showed water or saline solutions would be decomposed if they were inserted at any point within the circuit, Ampère began to think about the micro-scale processes within the circuit (Steinle, 2016, p. 98). He stopped using the terms "circuit galvanique" and "circuit voltaïque", and instead began using the term "circuit électromoteur", or "electromotive circuit." Steinle explains this conceptual shift (noting that tension and current remained imprecise terms):

> As the two essential effects of "electro-motive action," Ampère lists, first, the familiar, properly electric effects of the open pile, using the established concept of "electrical tension," and, second, the chemical effects observed when the pile is closed. By seeking to understand both kinds of effect as caused by one and the same "electro-motive action," he establishes for the first time a tight connection between the effects of open and closed piles, or between those attributable to tension and to current.
>
> (Steinle, 2016, p. 125)

At this point in his work, Ampère also stopped using the term "galvanic" current and began instead to talk of "electrical" when referring to a current from a closed pile, marking a shift from the strictly instrumental and, as far as he could manage, theory-free language to a much more explicit adoption of a theoretical stance.

There is much more that one might say about Ampère's remarkable work in 1820-21, but for the purposes of this project the important points are that Ampère pioneered what is now our everyday understanding of an electrical circuit as a whole system with current flowing throughout; that Ampère went to great lengths to experimentally demonstrate the equivalence of galvanic (closed pile) and voltaic (open pile) effects, and to demonstrate that the effects Ørsted had observed really were electrical and magnetic effects; and finally that Ampère demonstrated a means of making a numerical measurement of electric current using a torsion balance. This final point is most useful for interpreting the influence Ampère may have had on Ohm's experiment design, particularly given that Ampère's experimental work was fraught with the same technical difficulties that Ohm would encounter in his 1825 experiment before swapping his voltaic pile for a thermocouple. For Ohm to make use of elements of Ampère's experiment design he must have read and understood Ampère's work, which included the holistic approach to understanding what an electrical circuit is.

**Linking observations with mathematical claims**

Before finally returning to look at Ohm's use of the torsion balance to observe and read a numerical value for either "loss of force" (phase one) or "exciting force" (phase two), this section provides an introduction to some key discussions in the philosophy of measurement.

Since the early 2000s, a research programme exploring the philosophy of measurement has emerged, emphasising the material and historical aspects of measurement and offering new ways of analysing the relationships between measurement and theory claims in the history of science. A classic problem in the philosophy of measurement is "the problem of coordination", which is concerned with finding a way to break a problematic circularity when measuring quantities where the theory relating to the quantity is reliant on an appropriate means to measure it, and the measurement technique is reliant on the theory being correct. In a break with twentieth century philosophical studies of measurement, recent scholarship has explored the ways in which the circularity is an integral part of the process, rather than a problem to be ignored or overcome; Chang (2004) and van Fraassen (2008) suggest that

quantity concepts emerge as part of an iterative process in which the development of a quantity concept and the standardisation of its measurement are co-dependent (for a comprehensive survey of developments in philosophy of measurement in the early twenty first century see Tal, 2013). As Tal summarises, "The questions 'what counts as a measurement of quantity X?' and 'what is quantity X?', though unanswerable independently of each other, are addressed together in a process of mutual refinement" (Tal, 2013, p. 1162).

This is certainly a challenge for Ohm's experiment. As the preceding chapters have shown, Ohm could only read his measure of "loss of force", or later "exciting force", because he understood his apparatus to be working according to a suite of theories. Ohm's experimental setup, his language, his understanding of what was happening in his setup, and consequently what he was measuring, change between his first and second phases of work. Even though the physical torsion balance appears unchanged, Ohm's interpretation of what it is showing him changes. Luchetti's work investigates the problem of coordination within Ohm's work, and extends the problem of coordination beyond Ohm's experimental work and into Ohm's mathematical and theoretical argument that tension is equally distributed between every two points on a circuit (made in Ohm's 1827 treatise). This, he argues, provides part of the justification for a more general form of coordination between Ohm's mathematical formulation of his law and the empirical regularity it expresses (Luchetti, 2020, p. 120). For Luchetti, Ohm's work highlights two problems. Firstly, Ohm's experimental observation is a problem of "measurement coordination, which refers to the specific problem of how to justify the attribution of values to a quantity by using a certain measurement procedure". Subsequently, he argues, there is a second problem "which refers to the broader issue of justifying the representation of an empirical regularity by means of abstract mathematical tools" (Luchetti, 2020, p. 119). The remainder of this chapter deals with both of those types of coordination problem as they relate to Ohm's work, and integrates not only Luchetti's analysis of the documentary evidence left behind by Ohm in the form of archive papers and published works, but also the experimental practice and material culture that emerges from Ohm's research.

**Measurement and the coordination problem in Ohm's 1826 apparatus**

Let us consider what is happening within the torsion balance in Ohm's 1826 experimental setup. A description rooted in the phenomena Ohm observed shows that when two things are

in place, firstly the circuit is fully connected, and secondly a temperature difference is established between the two feet of the thermocouple, then the magnetised needle suspended from the torsion wire is deflected. Moving beyond the empirical observations, there are also several theoretical claims at play within the instrument: (1) the temperature difference in the thermocouple causes an electric current to flow, which (2) causes a magnetic effect; (3) the magnetic effect deflects the needle and twists the torsion wire, and (4) the force of deflection can be measured directly using the torsion balance with its flattened torsion wire. Only with all of these theoretical assumptions in place can a measurement of the force deflecting the needle be understood as a measurement of electric current. The final theory required to complete the puzzle is that the deflection force is understood to be in direct proportion to the "exciting force" within the circuit. Taken together, these add up to the strongest understanding we can gain about Ohm's concept of the nature of electricity.

As has been noted elsewhere in this project, Ohm was reticent about aligning himself with strongly theoretical claims, tending towards careful empirical description rather than using heavily conceptual language. In his 1825 attempt to derive his equation, he also made use of the torsion balance but wrote very little about its use other than saying it was used for the "determination of force" (Ohm, 1826a). Ohm's second 1825 paper on the conductivity of metals offered slightly more indication of his use of theory, saying "the magnetic effect of the galvanic circuit must be an excellent means of determining the conductivity of metals through numerical ratios" (Ohm, 1825c). In his 1826 paper, Ohm continued to refer ambiguously to "the force" of rotation acting on the torsion needle, without making a theory claim about the causation of that force. He also managed his language carefully in introducing his new equation:

$$X = \frac{a}{b + x}$$

> where *X* denotes the strength of the *magnetic effect* on the conductor, the length of which is *x*, but *a* and *b* denote constant quantities dependent on the exciting force and the conduction resistance of the other parts of the circuit.
>
> (Ohm, 1826a, p. 151, emphasis added)

Although Ohm's language is ambiguous, the way he framed his argument strongly suggests that he saw a direct relationship between the strength of flow of electricity, strength of the magnetic effect, and the force required to return the needle of the torsion balance back to its starting position. For instance, as well as his comments on the hydroelectric circuit, he refers

to the work of Becquerel and Davy on electrical conductivity, saying "the change in force is proportional to the increase in length of the conductor wire. This law is the same which Becquerel obtained…" (Ohm, 1826a, p. 156). The nearest Ohm comes in his 1826 paper to explicitly saying that he is measuring electrical current or electrical force is in a brief reference to the "force of the thermoelectric circuit" (Ohm, 1826a, pp. 153-154).

Returning to the problem of coordination, then, what is the measurement claim that Ohm is making here, and is it well founded? The word that Ohm uses repeatedly is *Kraft*; he is measuring a force. He strongly implies that the force is a product of flowing electricity, though he is not explicit. However, by adopting the term "thermoelectricity", he also adopts the theory put forward by Ørsted that the thermocouple causes a flow of electricity, and in turn a magnetic effect in direct proportion to the strength of the flow. Using that theory, Ohm interprets the force of deflection he observes in the magnetic needle as a measure of the strength of the flow of electricity, and he measures the deflection by counting the number of divisions he has to turn the micrometer through in order to return the needle to its starting position. In this, Ohm is assembling ideas that have been tested by others before him, rather than introducing any radical new innovations.

Here the problem of circularity described above shows itself. According to the theory Ohm is working within, the greater the electrical force, the greater the deflection of the needle within the torsion balance. The torsion balance is also used to argue for the theory of conductivity that Ohm is presenting. The two claims are interdependent on one another. However, as has been shown in this and earlier chapters, Ohm's claims about what the torsion balance is showing is built upon decades of earlier research. Other tools for observing electrical force had been used, including the use of taste or observing the rate of bubbles forming as water decomposed, and Coulomb had shown that the torsion balance could be used for accurately measuring magnetic forces. Taken in this context, Ohm's 1826 claim that the magnitude of rotation required to return a deflected needle to its starting position is a good measure of the force of a thermoelectric circuit appears to be contingent on theories that were generally well accepted and robust. By taking numerical readings and using them to feed into mathematical equations, Ohm integrated mathematics into the measurement procedures in his experimental work. As Steinle has commented,

> equations relating measurable variables never follow necessarily or directly from empirical data… It would be more accurate to speak of the accommodation of data to specific, seemingly plausible formulas. The reasons why certain formulas and other

> mathematical formalisms were closer at hand than others can be traced to fundamental, dare I say metaphysical presuppositions regarding the underlying structure of matter.
>
> (Steinle, 2016, p. 25)

In the case of Ohm, this raises the question of what presuppositions he held, and what for him was a seemingly plausible formula. As earlier chapters have shown, and as Luchetti (2020) has described, Ohm's understanding of what was happening in his experiment changed between his first phase and second phases of work in 1825 and 1826 respectively, from an understanding of "loss of force" and a logarithmic mathematical relationship to one of "exciting force" and an inversely proportional mathematical relationship.

Amidst his shifting conceptual tools, Ohm's use of the torsion balance for measuring a force, and the physical process of recording the force, remained consistent throughout his experimental practice. Yet, what he could be said to be observing and recording changed. This illustrates that even with a trusted and well understood measurement tool, in this case the torsion balance, the act of measurement is a combination of instrumentation, process and theory. Chapter 6 contrasts this measurement and observation-based practice with Ohm's third phase of work, which is principally mathematical in basis, with only passing reference to Ohm's previous empirical work. Heidelberger has argued that the shift in understanding that Ohm goes through as he progresses through his two experimental phases and into the mathematics-led work of his third phase is akin to a Gestalt switch. Over those few years, he argues, Ohm went from working within a "Coulomb-type" theory to an "Ohm-type" theory, moving from a conceptualisation based in electrostatic force to one based in Ohm's own idea of electroscopic force. Throughout this transition between phases, the material instrument used for measuring the two types of magnitude stayed the same, however the theoretical understandings and presuppositions were different to the extent that the experiment itself could be said to be different (Heidelberger, 1980, pp. 119-120). Despite Ohm's shifting understanding of what his experiment was measuring, the torsion balance remained central to his experimental practice as the iconic tool for measuring electrical effects.

**Understanding the torsion balance in "received" history**

The conclusion of this chapter reflects on the conspicuous absence of the torsion meter in our historical interpretations of how electrical current, as opposed to electrical charge, has been

measured. Coulomb's canonical experiments with the torsion balance, together with the controversy surrounding his claims to have demonstrated the inverse square law of electrical charge with the instrument, dominate historical discussions of the torsion balance. Both the instrument and its use have been replicated on numerous occasions, and those replications have in turn become some of the foundational works relating to the replication method in the history of science, and the pitfalls of attempting to repeat experiments from a different time and place (see Heering, 1994; Martinez, 2006; and Palmieri, 2009 for the ongoing debate surrounding the replication of Coulomb's torsion experiment). These historical narratives and modern-day reworkings foreground the extreme difficulty and the skills required to operate the torsion meter in the way that Coulomb claimed; they also foreground a particular context of use, to measure extremely small forces resulting from the repulsion between small quantities of electrical charge. However, an implication of this is that *all* uses of the torsion balance are considered extremely challenging, verging on unrepeatable, and consequently unreliable for making empirical claims. In fact, the instrument is far more straightforward to use in the setups that both Ampère and Ohm employed, in which they used the balance to observe the amount of turn required to return a magnetised needle to its starting position after it has been deflected by the magnetic effects of a nearby flowing electrical current. Both Ampère and Ohm had difficulties with their experiments, but neither reported the torsion balance to be the cause of those difficulties, and Ohm shows no signs of having tried alternative measurement techniques even as the conceptual framework and key parts of the experimental apparatus changed around the balance.

The torsion balance has been omitted from histories of measuring electrical current, except occasionally as part of a preamble alongside the electrometer or electroscope and other pre-electrodynamic ideas. A review of torsion balances dating from the period Ohm was working in held in museum collections also revealed remarkably sparse holdings of an instrument that was likely to be present in teaching collections in schools and universities across Europe.[13] Indeed, as noted earlier in this chapter, there were two torsion meters available to Ohm at the

[13] Searches of online catalogues were carried out, using search terms "Coulomb" and "torsion" (or equivalent). Boerhave Museum, one object (inventory number V13317); Museum of the History of Science, Oxford, one object (inventory number 94227); Deutsches Museum, one object (inventory number 39094); Smithsonian, one object (catalogue number 323667); Arts et Métiers, one object: (inventory number 05108); Hunterian Museum, Glasgow: no results in scope; Science Museum, London: no results in scope; National Museums Scotland: no results in scope; Whipple Museum, Cambridge: no results in scope.

local cabinet of his school, though he considered neither of satisfactory quality. Could it be possible that the instrument made the transition from near-impossible to use, as in the case of the hard to verify claims made by Coulomb, to being so ubiquitous and integrated within a larger instrument that it is barely worth mentioning or including in museum collections? Another consideration is that because the technology of the instrument had not changed much in the intervening three decades between Coulomb and Ohm, it is not seen as having developed enough to be worth writing further about or adding to museum collections, even though the concepts the instrument helped to reveal were changing rapidly.

Without fully understanding how the torsion balance was understood by Ohm's contemporaries, it is hard to appreciate how its inclusion in his experiment might have either lent credibility to the work or undermined his conclusions. The footnote Ohm includes in his 1826 paper suggests that he felt his readers needed some reassurance: "one should not be disconcerted by this [potential source of error], given the anxious care with which I had my torsion balance built." However, Ampère had used a torsion balance without attracting much comment, followed by Ohm in a similar fashion. None of the responses to his work offered up criticism of the instrument or presented alternatives, and even his modification of using a flattened torsion wire seems to have escaped notice or concern. Despite the philosophical uncertainties that had the potential to undermine Ohm's use of his modified torsion balance, it seems reasonable to conclude that in a context where researchers were trying out a wide range of different techniques for studying the deflections of magnetised needles, most of Ohm's peers were quite comfortable with him using the torsion balance within his instrument in the way that he did.

## PART B

In reconstructing Ohm's apparatus, the micrometer at the top of the torsion balance marked the starting point. It was the first part of the instrument that the maker and I produced, partly because the micrometer itself is well described, even if other parts of the torsion balance are not, and partly because there seemed to be a logic to starting at the top of the instrument and working down. In many respects the torsion balance is the heart of Ohm's apparatus. It is where the theoretical concept of electrical current could be observed via the proxy of the deflection of a magnetised needle, it is where that deflection was converted into a numerical value, and it is where the sensitivity and accuracy of the instrument becomes apparent. Given its centrality to the instrument, it is remarkable how poorly the majority of the balance is described. This presents challenges for interpreting the instrument, and, as we have seen in previous chapters, raises the question of what precisely it is that we are trying to replicate in reconstructing apparatus of this nature. The development of the torsion balance particularly focused our attention on the role of calibration in the instrument, and the many different variables that can affect the instrument's sensitivity.

### Calibration, variables and sensitivity

In contrast to the discussion in the previous chapter, the basic form of the torsion balance was reasonably well defined. There were nevertheless challenges to overcome including finding a means of attaching the torsion thread to the micrometer and the needle (Appendix, p. 243 and p. 278). There was also a commodity issue, in that the ivory that Ohm described using at the ends of his magnetised needle is no longer available to purchase. Instead, plastic with similar hardness and density was substituted (Appendix, p. 278). While these required consideration, they were relatively straightforward problems to solve and seemed almost insignificant in the face of the other difficulties encountered in reworking the torsion balance. The first of those was identifying a suitable material for the torsion thread. A fuller account of the search for Ohm's *Goldlahn* is provided in the Appendix, but the key point for this analysis is that it is unclear precisely what he meant, or what particular qualities it ought to have, other than it was a flattened band of metal (Appendix, p. 279). The thickness, hardness and shape all have a bearing on the torsion qualities of the thread, and the hardness can depend on the metal alloy and whether or not it has been annealed. Within this single component there are multiple variables that affect the fundamental operation and sensitivity of the instrument, and

getting this very specific set of qualities to match what Ohm was using is impossible without a more thorough description.

While the qualities of the torsion wire provide one control on the sensitivity of the instrument, the other major control is the magnetised needle that hangs from it. Ohm provided dimensions for the size of the needle (0.8 lines thick, a little under two inches long) and the material it was made from (steel). However, he did not, and indeed could not, provide any indication of how strongly it was magnetised, as there was no standardised measuring system for magnetism then in use. He also did not note a distance between the magnet and the copper conductor running beneath it, which also makes a substantial difference to the sensitivity of the instrument; a magnet very close to the conductor will be more strongly deflected than one which is further away. These three variables, (1) the qualities of the torsion thread, (2) how strongly the needle was magnetised, and (3) the distance between the magnetised needle and the copper conductor mean that even if one was to precisely reproduce Ohm's results experimentally, it still would not be possible to know whether the qualities of individual component parts were a close match to Ohm's original apparatus. The Connelly apparatus consistently performed far less sensitively than Ohm's, never requiring multiple complete turns of the micrometer dial. However, it was clear that changing the torsion thread or raising the height of the needle did indeed affect the outcomes of the experiment, as did an array of other factors including the ambient temperature of the room and the location of household magnetic objects. It seemed clear that if the sole goal was to improve the sensitivity of the instrument, that could be achieved with some adjustments, though it would take a great deal of trial and error with multiple components to match the response that Ohm recorded. However, as we will see in the next chapter, what our relatively insensitive apparatus could do was produce results that demonstrated the expected inverse linear relationship between the length of the test wire pieces and how strongly the magnetised needle was deflected (Appendix, p. 288).

Calibration of the instrument not only takes place within its material components, but also through the way it is used. The very first thing that was required on first setting up the instrument was to align the magnetised needle with the Earth's magnetic field. The more sensitive the instrument was, the longer it would take for the needle to stop oscillating and settle, giving the user the opportunity to check its alignment. Once the needle was reasonably stable, the instrument could be gently shifted beneath it until it was aligned with the meridian. This was followed by using the levelling screws on the three corners of the baseboard to

ensure that the needle was at rest centred over the copper conductor beneath it, that it was horizontal and running parallel to the conductor and that it was not touching the inside of the glass cylinder. The final task was to untwist the torsion thread as far as possible by setting the micrometer to zero, then twisting the glass cylinder until there were no visible twists in the torsion wire. This was easier to observe when using metallic *lahn* for the torsion wire as it could be observed through the glass to see if there were any twists. When, in a later series, a hair was substituted for the *lahn,* it became much harder to identify whether there was a twist because it was much finer. Instead, the instrument's base was set up without the torsion balance, using a compass needle to align it with the Earth's meridian. Then the torsion balance was placed on it, and when on occasion it was clear that the torsion thread was twisted enough to be forcing the magnetised needle away from the Earth's magnetic field, the micrometer was then turned in the opposite direction, untwisting the thread so that the needle returned to the zero position.

It was noticeable that after a few attempts to operate the instrument I had developed a routine for getting the instrument ready for use. After the steps outlined above, ice was placed in the cold vessel and recently boiled water in the hot vessel over a burner, which I then lit. Then there was nothing to do but wait for the two sides of the thermocouple to reach the desired temperature.[14] Among the routines developed early on was a process for refilling the burner when it had run out of fuel. Using a pair of pliers, it was possible to quickly and safely lift the still-hot burner out of the instrument and to a safe place for it to cool and be refilled. The pliers could then be used to return the burner to its location within the instrument. This worked smoothly, and solved a practical problem. However, after several runs of experiments with almost random results which caused me to tinker with almost every aspect of the instrument, especially the burner itself, I realised that the pliers being used to remove the burner were very slightly magnetic themselves (Appendix, p. 291). Every time I had used them, I had then put them down somewhere – anywhere – before proceeding with my observations. The frustration this caused was much like that felt after spending time hunting for a pair of glasses that you are already wearing, or searching for a pen already in hand. Happily, the experiment ran much more as expected after removing the pliers from my

[14] My usual practice was to boil enough water for the hot vessel and for a hot drink. This might sound like a flippant aside, but I report it to make a serious point. Being on my feet, bent over and making close observations was physically demanding work. On several occasions I commented in my notes that it was time to stop and take a break because I was becoming tired, clumsy, and no longer working effectively.

workspace, and I learnt a useful lesson about the need to not only ensure the instrument itself is stable and ready for use, but its surroundings as well. In the meantime, I had refined my processes for ensuring the hot and cold ends of the thermocouple were at stable temperatures even as I stopped to refill the burner or top up the ice. This was essential because the biggest cause of instability in the instrument, particularly in the early attempts to use the apparatus, was keeping the hot end of the thermocouple consistent as I wrangled with fuels that ran out quickly or burned inconsistently (Appendix, p. 297).

**Making sense of measurement tools**

My final remarks on building and using the torsion balance relate to the challenges of taking a precision reading. Once the instrument was in operation, a new test wire would be inserted, causing the magnetised needle to deflect. After the initial force of deflection the needle oscillated for a time, before coming to rest. The damping on the deflection comes from the air in the cylinder, the newly changed or changing magnetic effects, and any resistance to twisting from the torsion thread. In the latter case, the torsion thread was specifically chosen to twist easily, so this was minimal. The more sensitive the instrument, the longer it would take for the needle to come to rest, and the more likely it was that the burner would run out of fuel in the process. Here it became clear that the lower scale performed a useful function for making observations. On looking through the eyepiece, I lined my eye up so that the piece of silk that was laid across the lens was aligned with the zero line on the lower scale and then watched for the needle to come into alignment too (Figure 35). Using the lower scale, I could observe whether the needle was travelling equally far in each direction, counting the number of markings it was swinging to on either side. In the audio recording of my second series of experiments I can be heard saying “plus two, to minus one”, meaning that the needle was skewed to the right based on my view through the eyepiece, and again “plus two, to minus three” as I adjusted the micrometer too far the other way (Connelly, reflective diary, 3 June 2022). When the swing was peaking at the same number of divisions on each side of the zero line, it was time to leave it to settle down before any final tweaks, and then take the reading.

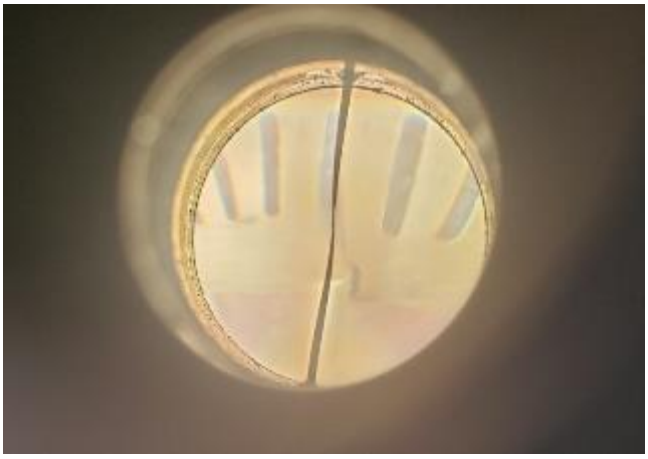


**Figure 35.** View through the eyepiece of the Connelly apparatus. The piece of thread across the lens can be seen, as can the divisions on the instrument's lower scale. The tip of the needle can just be made out behind the piece of thread, covering the number 0.

My use of the lower scale was very useful, though admittedly not essential. However, it seems to suggest that Engels and Riess may not be wholly correct in their assertion that the lower scale "is only used to find the starting position with or without current flow" or that "the [lower] degree scale becomes superfluous because, taking into account the other settings described for parallax-free reading, only the zero position, i.e. a central line on the copper strip, has to be defined. The scale could therefore be an obsolete remnant of previous considerations and experiments" (Engels & Rieß, 2022, pp. 96, 98). Their argument also includes a different interpretation of the location of the silk thread used to avoid parallax errors. Rather than placing it across the lens of the loupe as we had, they placed it across the surface of the glass cylinder. This shows once again that there are multiple ways to interpret Ohm's instructions.

As this section has shown, understanding how an instrument works in principle and operating it in practice are very different things. It is also clear how partial a written account is, even when striving to be comprehensive. It was instructive, for example, comparing my audio recordings with my reflective diary, and noting just how much detail I had already subconsciously edited out. Subsequently, when writing up my processes in the Appendix as a more formal report, further detail was jettisoned in pursuit of clarity. Reflecting on my own practice, most interesting is that during the experiments where the problems caused by the pliers caused instability in the experiment, I simply did not record my results. I had an idea of what results I expected, and when they did not match my expectations I manually tinkered

with the instrument, making a mental note of what was happening, but not taking the trouble to write everything down as I sought to solve a problem. There was a clear hierarchy between things that I felt were sufficiently useful to be worth recording in writing and those which I did not, even as I observed myself in the act. This forces me to reflect upon the gaps between Ohm's texts and his practice. Of his notebooks as they survive in the Deutsches Museum, his personal correspondence and his published papers, each had a different intended audience, and contained different types of content and different degrees of detail.

# 6. Measurement and Mathematics

## PART A

In approaching the conclusion of this piece of work, it is time to reconstruct the different parts of Ohm's experiment and see how it informed Ohm's subsequent mathematical treatise. Chapters 3-5 have presented a dissected view of Ohm's apparatus and looked in isolation at the three key parts of his instrument, together with their associated histories and theories. This chapter picks up where Chapter 5 left off to look at how Ohm shifted from his experiment-led second phase of work into his mathematics-led third phase. Where Chapter 5 described the theoretical underpinnings of the torsion balance, and how Ohm used it as a tool for measurement, it stops short of exploring exactly how Ohm's observations and measurement claims supported his mathematical arguments.

There was a growing shift towards quantification in natural philosophy in the period leading up to Ohm's research. John Heilbron described how the second half of the eighteenth century saw many parts of physics fall under the "yoke of mathematics". However, he argued, this turn towards quantification "owed nothing to the progress of mathematics; not until the turn of the nineteenth century did the electrician or thermodynamicist begin to require mathematical techniques not fully available a hundred years earlier" (Heilbron, 1979, p. 73). Meanwhile, thinking specifically about electrical studies, Olivier Darrigol has reflected on the role of quantification in understanding developments in theories of the pile and electricity. Noting that the voltaic pile discharged whenever the two poles were connected, seemingly unceasingly no matter how many times the pile was reconnected, Darrigol writes: "In this picture only the state of the pile before discharge seemed amenable to quantitative studies. This may in part explain why quantitative studies of the galvanic current were so scarce before the 1820s" (Darrigol, 2000, p. 2). By the time Ohm was studying electrical circuits, not only was quantification a well-respected goal of empirical studies, but newly developed mathematical tools presented new opportunities for interpreting the quantified observations. Ohm's knowledge of French mathematics, where the move towards quantification was most prevalent, together with the new discoveries of electromagnetism and thermoelectricity and the availability of a good quality torsion balance, put him in a strong position to undertake quantitative research.

The first part of this chapter provides an overview of Ohm's 1827 mathematical treatise, *The Galvanic Circuit Investigated Mathematically*, produced during his third period of work. The

chapter continues with a look at the wider mathematical context for Ohm's work during the period, and how the work fitted into the culture of scientific research at the time. In particular it focuses on the extent to which Ohm followed Fourier, who has often been cited as Ohm's principal influence for this work (Archibald, 1988, p. 148; Caneva, 1978, p. 116; Jungnickel & McCormmach, 1986a, pp. 54-55).

### *The Galvanic Circuit Investigated Mathematically*

Published in 1827, Ohm's mathematical treatise is over one hundred pages long and sets out a mathematical derivation of Ohm's law.[15] It was written during a sabbatical from teaching, during which Ohm moved to live in his brother's house in Berlin where "he had a small apartment with space for doing experiments", suggesting that he was not entirely focused on mathematical workings as he wrote (Jungnickel and McCormmach 1986, p. 53).The introduction states that the purpose of the book is to work from a few key principles obtained by experiment to deduce the "rationale of those electrical phænomena which are produced by the mutual contact of two or more bodies, and which have been termed Galvanic" (Ohm, 1841, pp. 401-402). The first thirty pages are framed as an introduction and are predominantly written in prose, relying on geometrical analysis and accompanying diagrams to introduce Ohm's themes without recourse to complicated mathematics. As described in more detail in Chapter 3, the diagrams themselves give a basic sense of Ohm's theoretical claim that the tension brought about by electrical force drops linearly along the length of a homogeneous metal wire. This is relatively straightforward, and aligned with claims made by

[15] A note on translations: Ohm's original work in German is relatively hard to access in print in the UK, though available in full online (Ohm, 1827). In German the printed form can most easily be found within Eugene Lommel's collected works of Ohm (Ohm, 1892). The easiest English translation to find, particularly available in print copy, is an 1891 print of William Francis's translation, edited by Thomas D. Lockwood (Ohm, 1891). This translation purports to be a reprint of the translation William Francis made for publication in Richard Taylor's *Scientific Memoirs* (Ohm, 1841). However, close reading reveals anachronous language, and a direct comparison between the original Francis translation and the later Lockwood edition shows that in addition to overt edits to the later edition, which are presented in square brackets, the language has also been edited throughout. This was presumably in order to make Ohm's work comprehensible to an 1890s readership, who were using new vocabulary and conceptual interpretations developed in the intervening decades. The result is a translation that provides a fascinating tool for comparison between the state of electrical knowledge in 1841 and 1891, but is rather unhelpful for understanding Ohm's intentions.

others including Davy and Becquerel, though it is notable that Ohm is prepared to make a theory claim unfounded by experiment where it is supported by mathematical argument. In introducing his themes, Ohm's language changed again in producing his book. Whereas in his first phase of work he described "loss of force", and in his second phase of work he described "exciting force", in this third phase of work he introduced "electroscopic force" (*elektroskopische Kraft*). Ohm did not acknowledge the change in language, though unlike in his earlier work it is possible to discern more about the theoretical ideas underpinning his use of the phrase. The term itself was in use elsewhere. It echoes Ampère's phrasing, and it was used by Erman in writing about Ohm's "interesting" studies of "electroscopic phenomena" when asked to assess Ohm's application for research leave (Hartmann, 1927, p. 73). Whittaker has provided a thorough discussion of Ohm's use of the term, suggesting that Ohm was still using some electrostatic ideas, and that he seemed to have "thought of the electroscopic force at a place in a circuit as being proportional to the volume-density of electricity there" (Whittaker, 1910, p. 97).

Ohm is more assertive about his use of the term than he had previously been with either "loss of force" or "exciting force". In *The Galvanic Circuit* he discussed the idea that any two heterogeneous bodies in contact will have a consistent electroscopic force between them, "which we are accustomed to designate by the expression *electric tension*, or difference of bodies" (Ohm, 1841, p. 447). The electroscopic force, he argues, will settle into an equilibrium, being equal at all points in the circuit. It would, in principle at least, be possible to measure the electroscopic force within any part of the circuit and find it to be equal with any other part. For Ohm in his third phase of work, electric current is a continual change in the way electroscopic force is distributed as the circuit. One might imagine the circuit to be made up of many individual conductor segments all in series, in which the electroscopic force tends towards an equilibrium despite the continual disruption caused by, for example, a pile or thermocouple. Although Ohm's language changed in this phase of work, there is more continuity between Ohm's second and third phases of work than between the first two. Archibald goes so far as to say that "In the 1826 experimental paper what Ohm termed the 'exciting force' was already the 'electroscopic force' of his later work" (Archibald, 1988, p. 145).

One other linguistic peculiarity, which we first encountered in Chapter 3, is that Ohm uses is the term "reduced length", $L$, in places where he had previously referred to resistance (introduced on p. 414 of Ohm, 1841). Here again, we have to look at Ohm's use of the term

to try to make sense of why his language has changed because he offers no direct explanation for altering his language. Thinking in mathematical terms, it is notable that there was not a unit of measurement for resistance or conductivity when Ohm was working. Using a "length" of a known material such as a specific copper wire is perfectly adequate for experimental work where finding ratios is sufficient for defining a law. When working more mathematically, a better-defined unit of measurement is useful. As noted in Chapter 3, Ohm was resistant to theorising, and using length as a dimension could help to avoid introducing a theory-based unit of measurement for resistance (see p. 125). This could provide yet another explanation for Ohm's adoption of the language, which he used much more consistently than he had used either "resistance" or "conductivity" in his earlier work.

Returning to Ohm's introductory section, there are some mathematical equations, but they are always framed around the relatively simple geometrical analysis that Ohm had already introduced. Part way through his introduction, Ohm first introduced his equation in a form we can recognise (where $S$ is current, $A$ is magnitude of tension, and $L$ is reduced length):

$$S = \frac{A}{L}$$

(Ohm, 1841, p. 424).

Contrary to the perceptions of Ohm's work as being entirely mathematical, there are several pages of discussion on the difference between thermoelectric and hydroelectric circuits, sowing seeds for an appendix to the work titled "On the chemical power of the galvanic circuit". There is also some discussion of the impact of a multiplier within a circuit. Ohm argues that although adding loops within a circuit to act as a multiplier around an indicating needle increases the magnetic field, and therefore deflection of a needle at the centre of the multiplier, each additional loop also increases the reduced length of the overall circuit and reduces the deflection of the needle. Here, Ohm built on a theory of multipliers put forward in his 1826 "Announcement of Laws" paper (Ohm, 1826a). Ohm's understanding of an electric circuit as a whole system shines through in his discussion of multipliers. A consequence of his holistic view is that he recognises that a single component, whether in the form of a multiplier or a voltaic pile, can impact on the overall reduced length, magnitude of tension, and electroscopic force throughout the entire circuit.

Following these introductory pages, Ohm begins the serious work of a mathematical derivation of what he calls "galvanic circuits whose state is permanent", by which he means

circuits which are not also subject to chemical change.[16] He begins by offering up "General observations on the diffusion of electricity" (Ohm, 1841, p. 437). The use of the word "diffusion" immediately suggests a resonance with Fourier and his contemporaries in France, and, following a few pages of introductory remarks and scene setting, Ohm introduces differential calculus using techniques pioneered by Fourier and the French mathematical physicists. The argument follows the standard form of differential calculus, where one imagines the subject of the study cut into small slices:

> If we imagine this body divided by a number of such sections into disks of infinitely small thickness, and so that in the whole circumference of each disk the electroscopic force does not vary sensibly for each pair of such disks, the expression [given previously] can be applied to determine the quantity of electricity passing from one disk to the other
>
> (Ohm, 1841, p. 448)

Ohm's work considers the physical form of wires, and in setting out his arguments he specifically uses equations that work for a series of connected prismatic bodies as an analogy for wires in a circuit.

Following on from his general discussion of the diffusion of electricity in wires, Ohm then turns his attention to what he calls "Electroscopic Phænomena". This progresses his argument from the discussion of stand-alone prismatic bodies to one about curved conductors, noting that "galvanic circuits, by their very nature, can but seldom be extended in a straight line" (Ohm, 1841, p. 455). Throughout this section, Ohm is concerned with the forces acting within a circuit or a section of a circuit, and how the tension or force at any point in a circuit might be calculated. He works through a variety of scenarios, considering the extent to which the atmosphere outside a wire affects the forces, what happens if a section with a large "reduced length" is incorporated into the circuit, and so on. As before, this is heavily mathematical in nature, and, with reference back to the calculus developed in the previous section, uses the different physical scenarios to either simplify or add complexity to his workings as he moves through his derivations.

[16] Ohm's Appendix provides an analysis of circuits which are not "permanent", typically because they incorporate a voltaic pile. The implication is that Ohm saw his 1825 experimental setup as flawed because the voltaic pile introduced chemical changes. In contrast his thermocouple-based setup in 1826 fits into his definition of a "permanent" circuit.

This leads to the final section of the book, which draws together arguments from the previous section, together with some new derivations, to reflect on “Phænomena of the Electric Current.” Ohm states some of his key arguments in this section, which can be summarised as:

> Since *A* represents the sum of all the tensions in the circuit, and *L* the sum of the reduced lengths of all the individual parts, from the equation we can find the following general properties of electric current in a galvanic circuit.
>
> > I. The electric current is of equal magnitude at all places in a galvanic circuit, and is independent of the value of the constant *c*, which fixes the intensity of the electroscopic force at a determined place. In the open circuit the current ceases entirely because the reduced length *L* of the circuit becomes infinite.
> >
> > II. The magnitude of the current in a galvanic circuit remains unchanged when the sum of all its tensions and its entire reduced length are varied in the same proportion; or, assuming the reduced length stays the same, it increases in proportion to any increase in the sum of the tensions; or, assuming the sum of the tensions remains the same, it increases in proportion with any reduction in the reduced length of the circuit.
>
> It follows that:
>
> 1. A difference in the arrangement and distribution of the individual points of excitation, by reordering component parts within the circuit, has no influence on the magnitude of the current, provided the sum of all the tensions remains the same.
>
> 2. The intensity of a galvanic current is consistent even when a part of the circuit is removed, provided another prismatic conductor with the same reduced length is substituted and provided the sum of the tensions in both cases remains the same; and *vice versa*, when the current of a circuit is not altered by the substitution of one of its parts for an alternative prismatic conductor, and we can be convinced that the sum of the tensions has remained the same, then the reduced lengths of the two exchanged conductors are equal.
>
> 3. Imagine a galvanic circuit which is kept consistent so that it is constructed of the same number of parts, that the substance they are made of remains consistent, and that it is consistently arranged in the same order so that the individual tensions may be regarded as constant. Then, the current in the circuit increases or decreases in proportion to any increase in the size of the parts, provided their lengths remain unaltered and all of the parts are increased in a similar manner. It also follows that if the reduced length any one part of the circuit far exceeds that of the other parts, the magnitude of the current will principally depend on the dimensions of this part. The calculation using the law will assume a much simpler form if attention is solely directed to this one part.
>
> (Ohm, 1841, pp. 478-479)

Underpinned by experimental observation, and reinforced by a complete mathematical derivation, in the passage above Ohm has made a comprehensive series of novel statements about the nature of a circuit as a whole. In the concluding pages of the book Ohm expands on his ideas further, discussing how the statements above are affected in the case of "multiple conduction", or what we would describe as parallel circuits, and also how heterogeneous components impact on the tension, intensity and reduced length at different points in the circuit, as well as when taking the circuit as a whole system. As before, this analysis is accompanied by mathematical workings, this time represented by a combination of infinite series together with calculus.

Finally, Ohm included as an appendix a few pages about the chemical changes in a galvanic circuit. This section takes into consideration "the action of the current on the bodies subjected to it, and the alterations in their chemical constitution thence resulting in any possible manner, as also the changes of the current itself produced by reaction" (Ohm, 1841, p. 488). Although the appendix includes some mathematical workings, it is dominated by prose which argues that within a chemical battery there is a "decomposing force" which acts when the circuit is in operation and causes a voltaic battery to become unstable. Ohm's work here is undoubtedly influenced by his challenging first phase of experimental work, in which his unstable voltaic pile made it difficult to use. The appendix concludes with a series of mathematical equations to indicate the nature of the change of current produced by chemical alteration, which also coincides well with Ohm's observations of fluctuating current in his early research. The innovation here, again, is seeing the voltaic pile and its components as a part of the wider circuit. Earlier on Ohm states that "The voltaic pile, which is a *combination of several similar simple circuits*, merits peculiar attention in this place, from the numerous and varied experimental results obtained by its means" (Ohm, 1841, p. 481, emphasis added). This statement is followed by mathematical workings relating to the interaction of electricity and the individual plates within the pile, and describes the effects of what we would call internal resistance and the ratios of different elements within the pile that best overcome it.

The way Ohm wrote his 1827 book raises a question about who he thought the book's audience was. The first thirty pages shy away from maths, and offer up instead rather technical prose and geometrical diagrams. The remainder of the book is heavily mathematical in nature, in a way that would have made sense to some French mathematical physicists, but few German speaking natural philosophers. His appendix presents his theories on the voltaic pile, making use of contact theory, which was likely to appeal to a slightly different group of

people again. Furthermore, the text was only made available in German, which means that the central core of the book was less easily accessible to the French mathematical physicists who may have been able to understand it.

**Fourier's differential calculus and the wider mathematical context**

As we have seen, electricity, magnetism and the associated mathematics were the focus of a great deal of attention in the early nineteenth century. In his assessment of the work of George Green, who published *An Essay on the Mathematical Analysis of Electricity and Magnetism* in 1828, only a year after Ohm's *The Galvanic Circuit*, Grattan-Guinness gives a helpful overview of the state of physical mathematics in the preceding period.[17] He highlights several important strands of work including studies of the attraction of spheroids to an external point, which made use of Newtonian ideas together with partial differential calculus, and which developed from studies of the properties of equipotential surfaces. The studies of spheroids incorporated corpuscular theories, which, for our purposes, importantly include Coulomb's electrostatic studies, and Siméon-Dénis Poisson's subsequent analysis which made use of partial differential equations (Grattan-Guinness, 1995, p. 388). The mathematical techniques available for solving partial differential calculus were greatly improved by Fourier's work as he developed his *Analytical Theory of Heat*, published as a monograph in 1822 but preceded by work taking place over nearly two decades.

Fourier's major work on heat was carried out in a context dominated by Pierre Simon Laplace. As Robert Fox has described, "Laplacian Physics" was a style of physics that sought to account for all phenomena in terms of central forces between particles. These were considered analogous to Newtonian forces, though they could be either attractive or repulsive. Attempts to refine and quantify a theory of the imponderable fluids characterised much of the work the Laplacians carried out. Fourier's work presented a challenge to the

[17] There are a number of striking similarities between Ohm's work and Green's. Green's interest in using mathematical physics was focused on the use of volume and surface integrals to analyse electricity and magnetism, and it the opening statement of his *Essay* he refers to use of calculus for analysis. He explicitly mentions that he will take a similar approach to Fourier but does not mention Ohm. As Ohm's work was published a year earlier in German, and Green had limited access to up-to-date literature, this is not a surprise. The essay also opens with observations that emphasize the central role of potential, a term Green coined to denote the results obtained by adding the masses of all the particles of a system, each divided by its distance from a given point. Green then applied these general properties to electricity and magnetism (Grattan-Guinness, 1995, p. 391; Green, 1828; Wallis, 2008, p. 199).

Laplacian style of working as early as 1807, with his mathematical treatment of the distribution of heat in solids. In 1812 he was awarded a prize for a revised version of the paper, which suggests some acceptance from the scientific establishment, however the paper was only published as an abstract, received criticism from the judges and went unpublished until it appeared in edited parts between 1824 and 1826 in the *Mémoires* of the Royal Academy. By the time the work emerged in public, Laplacian physics had lost its firm grip on French science, and traditional rational mechanics was re-established as a key influence on a more positivist approach to physics (Fox, 1974, pp. 91-95; 111, 128).

Unlike his Laplacian peers, Fourier adopted an agnostic position about the nature of heat, not publicly adopting either the caloric theory, which argued that heat was an imponderable fluid, or the wave theory, which argued that heat was a phenomenon caused by internal motion within a body (Grattan-Guinness, 1990, p. 588). Instead, Fourier's contribution to physics is often seen as primarily mathematical. This is played out in some of the more accessible histories. For instance, Ravetz and Grattan-Guinness's short biography of Fourier is heavily focused on his mathematical work. It offers only a brief statement about experimental practice, where they argue that Fourier produced mathematical workings first, and only used experiment to verify them:

> For Fourier, every mathematical statement (although not all intermediate stages in a formal argument) had to have a physical meaning, both in exhibiting real motions and in being capable, in principle, of measurement. He always interpreted his solutions so as to obtain limiting cases which could be tested against experiment, and he performed such experiments at the earliest opportunity.
>
> (Ravetz & Grattan-Guinness, 2008, p. 97)

In contrast, Grattan-Guinness's enormous multi-volume *Convolutions in French Mathematics* provides a much more detailed account of Fourier's work, and spends a great deal of time on his experimental practice. As above, he comments on Fourier's philosophy of experiment, which was "in order to check on truths already divined, not to test hypotheses humbly proposed" (Grattan-Guinness, 1990, p. 607). However, Grattan-Guinness writes that generally, despite Fourier taking a lot of trouble to carry out experiments as well as possible, the correlation with theoretical predictions was not impressive. Grattan-Guinness also indicates that Fourier's approach led to some rigidity in thinking. His calculations of heat diffusion in solids for his 1807 paper, which were strongly influenced by Newton's law of cooling, do not consider different types of solids and how they might affect results, or the

environment in which the cooling was taking place (Grattan-Guinness, 1990, p. 607; 631). Nevertheless, in his *Analytical Theory of Heat*, Fourier commented on the good agreement between his measured results and the results he had calculated, saying for example that "the experiments fully confirm the results" (Fourier, 1822, p. 105). Again, reinforcing perceptions of his mathematically driven approach, he did not describe his experiments in detail in his book. One place Fourier's experiments were thoroughly described was in his 1807 paper. However, the paper was not publicly known; the manuscript was lost for a period, and only published in the 1970s. Aside from the close similarities between Ohm's mathematical derivation of his law and Fourier's *Analytic Theory of Heat*, two other things are notable. The first is that, like Fourier, Ohm's most famous work only referred to his experimental work in passing, foregrounding the mathematical work instead. The second is that Ohm remained agnostic about the nature of electricity, just as Fourier had about the nature of heat. He was not explicit about whether he considered electricity to be a fluid or a quality of all substances.

In addition to the mathematical style used by Fourier and some of his contemporaries, his philosophical style is also worthy of comment. Fourier has been described as a "positivist" by writers including Kargon:

> Positivism, following August Comte, insisted upon the primacy of facts, and of laws derived directly from them. Whereas Laplace, Poisson, and the others introduced hypothetical molecular considerations for the purpose of rendering discussions in mathematical physics more physical (i.e., more closely tied to characteristics of matter), Fourier's aim in science was precisely the opposite. The concern of Fourier was not causes but laws, mathematically expressed.
>
> (Kargon, 1969, p. 427)

Kargon goes on to describe Ohm as one of the first researchers to take up the "positive" or analytic approach following Fourier. A similar sense is expressed in a different way by Grattan-Guinness who commented on Ohm's similar "agnostic" view of electricity to Fourier's agnosticism regarding heat (Grattan-Guinness, 1990, p. 1177). Throughout this project it has been noted that Ohm did not enter into theoretical speculation, instead using descriptive language to relate his experimental findings. As in Kargon's characterisation of positivism, Ohm's use of mathematics was rooted in empirical observations rather than speculation. However, there are some assertions in his mathematical work about the distribution of electricity in a circuit that are not backed up by direct observation. Ohm was, it seems, prepared to make such claims when supported by a mathematical argument. He does

not state whether or not he thinks those assertions refer to something real within the circuit, or if he is simply being instrumentalist and setting up rules that enable him to undertake his mathematical analysis.

Throughout this period, the use of mathematics as an abstract analytical tool was gaining traction in France, albeit against the challenging background of the shifting political power held by the Laplacian school and its successors. However, it was still relatively new when Ohm was working, and importantly it was far from universally accepted as an appropriate means of interpreting physical phenomena, even in France. Further field, including in Ohm's German context, it was an alien way of working and utilised mathematical techniques that were impenetrable for most people. Before looking at the reception of Ohm's mathematical work, the next section goes into some detail examining Ohm's direct influences and the techniques he used.

**Making a mathematical argument**

In his published papers throughout 1825 and 1826, Ohm was restrained in his use of mathematical arguments. Nevertheless, he did use equations to articulate some of his reasoning. Sometimes his use of mathematical notation was directly linked to measurements and empirical observations he had made (see Ohm, 1826a, p. 151). In other cases he used mathematical reasoning as a tool to develop a written argument (see Ohm, 1826b, p. 46). However, for the most part, the papers he published in journals were descriptive accounts of experimental work, with a combination of qualitative and quantitative written analysis. His 1827 monograph was very different and presented a mathematical derivation of his law based on a few underpinning claims about the nature of electricity and its observed behaviour in metal wires. *The Galvanic Circuit* referenced Ohm's experimental work in passing, but did not describe the experiments or his results. Ohm was not alone in keeping his two styles of work separate. Keithley comments on Fourier's separation of his experimental and mathematical works:

> Fourier frequently commented in his "The Analytical Theory of Heat" on the good agreement between his measured results and his computed results. In it he did not, however, describe his experimental equipment, how he used it, or the data he took.
>
> (Keithley, 1999, pp. 85-86)

Steinle, too, comments on Ampère seeking a mathematical treatment without measurement, suggesting that Ampère worked hard to hide his research trajectory to conform to prevalent methodological ideals.

> In addition to the elision of exploratory work, Ampère's self-presentation was significantly shaped by his tendency to adapt his research to the epistemological standards of the tradition of mathematical formalization. He thus emphasized that his work "naturally" divides into two parts, one devoted to experimental facts and the other, to theory. This emphasis, as discussed above, was certainly a matter of the retrospective rhetorical adaptation of his work to a preexisting understanding of scientific process.
>
> (Steinle, 2016, p. 199)

We know that Ohm read and was influenced by both Fourier and Ampère, and it is unsurprising therefore that he followed their lead in adopting the separation of experimental work and mathematical work in his published writings. It is also worth considering the work of some of Ohm's other predecessors and contemporaries.

In 1804, alongside a number of electrical researches into the theory of the electric pile, J.B. Biot wrote a paper that recounted experiments on measuring the steady-state distribution of temperature in a homogeneous iron bar that was heated at one end. The resulting observations led him to formulate a simple theory of heat flow, which accounted for both internal and external effects, and in which he argued that the temperature was related to the distance along the bar by the "logarithmic curve". Biot regarded the law as applicable to heat conductivity both within the iron bar and the wider environment (Cardwell, 1971, p. 66; Grattan-Guinness, 1990, p. 591). The relevance of considering heat flow in a metal bar will become apparent later, as both Fourier and Ohm used similar devices in putting forward their theories. As mentioned in Chapter 1, Ohm read widely among the French mathematicians earlier in his career, explicitly noting Biot's work alongside others.

Fourier himself seemed to take Biot's work as a starting point, though, as Grattan-Guinness observes, Biot's name was conspicuously absent from Fourier's 1807 paper. "Indeed, for some reason Fourier held Biot's work in contempt" (Grattan-Guinness, 1990, p. 593). Fourier's failure to mention Biot likely stems from jostling between the Laplacian school, which Biot was part of, and new approaches from natural philosophers including Fourier and Ampère (for more background see Steinle, 2016). Regardless of the reasons, there is nevertheless a clear relationship between the two pieces of work. Where Biot's analysis

began with an iron bar heated at one end, Fourier follows with a series of chapters titled: “Propagation of Heat in an infinite rectangular solid”, “Of the linear and varied Movement of Heat in a ring”, “Of the Propagation of Heat in a solid sphere”, and so on (Fourier, 2009).

The style of analysis that Fourier and others put forward was described by Maxwell as reducing physics to analysis, “stripping away the visible and concrete like bark from a tree” (Maxwell cited in Kargon, 1969, p. 423). This stems from the way that Fourier and other researchers separated their experimental work from their mathematical work. This case happens to be particularly extreme because, as noted above, the details of Fourier’s experimental work were not published during his lifetime. In his *Analytical Theory* itself, Fourier includes statements based on empirical observation such as: “It has been observed that if two bodies of the same volume and of different nature have equal temperatures, and if the same quantity of heat be added to them, the increments of temperature are not the same; the ratio of these increments is the ratio of their capacities for heat” (Fourier, 2009, p. 19). While the underlying observations are not clearly explained, there is enough information there to design and carry out an experiment, and the strong implication is that the observations are reliable enough to base a factual statement on. However, Fourier also argues that “mathematical analysis has outrun observation”, and is prepared to rely on his mathematical analysis to make claims and arguments that go beyond existing empirical observations (Fourier, 2009, p. 24). In doing this, Fourier reduces some of the physical questions presented by empirical observation to purely analytical problems. Grattan-Guinness went further still, arguing that Fourier only used experiments for checking truths that had already been worked out by theory (Grattan-Guinness, 1990, p. 607).

There are a few clear comparisons to draw between Ohm’s *Galvanic Circuit* and Fourier’s *Analytical Theory*. They both argued that in a continuous setup with a source of electricity or heat, respectively, at one end of a homogeneous linear conductor, a uniform reduction in the quantity of the phenomenon they were studying could be observed along the length of the conductor. They both made their arguments using differential calculus, bolstered by some basic observable statements as the starting point for their mathematical reasoning. As well as similarities in overall approach, there is also some close resemblance in the detail of the two works. One obvious example on reading the books is that they both begin their reasoning by giving the example of a uniform metallic ring. After a brief opening statement, Fourier begins his introduction saying:

> If we expose to the continued and uniform action of a source of heat, the same part of a metallic ring, whose diameter is large, the molecules nearest to the source will be first heated and, after a certain time, every point of the solid will have acquired very nearly the highest temperature which it can attain…
>
> (Fourier, 2009, p. 15)

Ohm, meanwhile, begins his line of reasoning by saying:

> Imagine, for instance, a ring everywhere of equal thickness and homogeneous, having, at any one place, in its whole thickness, one and the same electrical tension, i.e. inequality in the electrical state of two surfaces situated close to each other, from which causes, when they have come into action, and the equilibrium is consequently disturbed, the electricity will, in its endeavour to re-establish itself, if its mobility be solely confined to the extent of the ring, flow off on both sides. If this tension were merely momentary, the equilibrium would very soon be re-established; but if the tension is permanent, the equilibrium can never be restored; but the electricity, by virtue of its expansive force, which is not sensibly restrained, produces in a space of time, the duration of which almost always escapes our senses, a state which comes nearest to that of equilibrium, and consists in this; that by the constant transmission of the electricity, a perceptible change in the electric condition of the parts of the body through which the current passes is nowhere produced…
>
> (Ohm, 1841, pp. 401-405)

Although superficially similar, in these examples Fourier is considering the action inside the ring at a particulate level, while Ohm is analysing the idea of tension causing a disruption to electrical equilibrium across the whole system. In Ohm's work, the consideration of the physical form of a ring or a bar is followed by his use of geometrical analysis as a method for articulating the way electricity flows through a range of different materials. For example, Figure 36 takes the example of a ring composed of two heterogeneous parts, open at one of its "places of excitation" and stretched out to form the line *ABC*. The lines dropping from left to right are representative of the drop in electrical tension observed within the linear conductor, with the sudden drop from *G-H* or *G'-H'* indicating the join between the two heterogeneous parts. Füchtbauer hints that Ohm's geometrical presentation at the start of his book was intended to make it accessible, knowing that few physicists in Germany would have sufficient mathematics to follow the arguments he was putting forward (Füchtbauer, 1947, pp. 157-158).

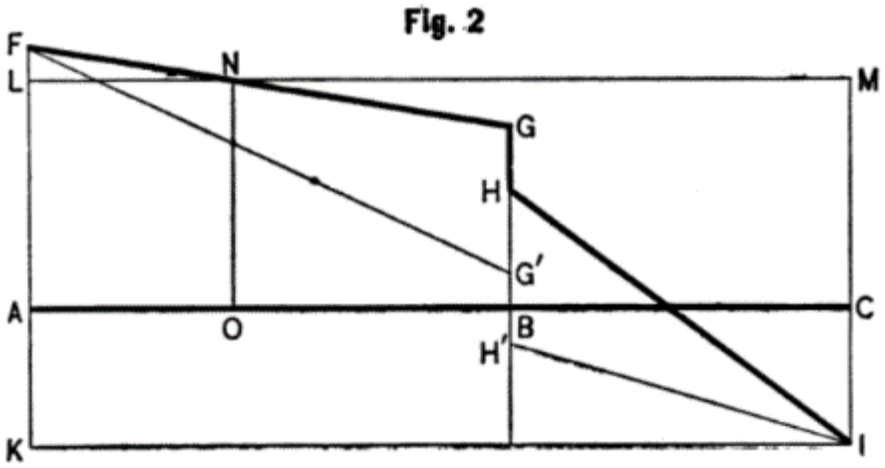



**Figure 36.** Diagram of Ohm's Geometrical Analysis (from Ohm, 1891, p. 96)

Fourier also used a very limited form of geometrical analysis, but was generally far less reliant on it for making his argument, and he offered much less detail in his figures. Nevertheless, he did present a small number of similar diagrams.

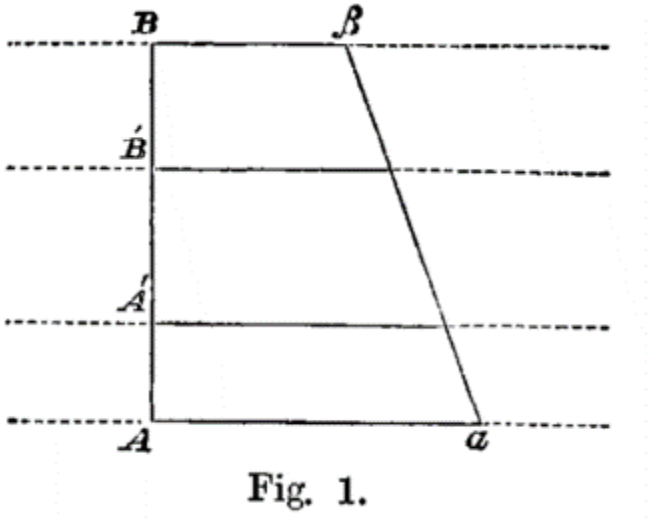



**Figure 37.** Diagram of Fourier's Geometrical Analysis (from Fourier, 2009, p. 46; Reproduced with permission of The Licensor through PLSclear)

Figure 37 offers an interpretation of temperature within a cross section of a solid, and accompanies an argument that the temperature decreases in arithmetic progression from the base to the upper plane. In the diagram the higher temperature at the point marked $\alpha$ is denoted by it being presented further to the right of the diagram than the lower temperature of

the point marked $\beta$, which is shown further to the left of the diagram and also further from the base of the object. While Fourier did not go to such detailed lengths with his use of diagrams as Ohm, he did write out in prose the same kinds of geometrical analysis that are presented within Ohm's diagrams (for example see Fourier, 2009, pp. 15-16).

Perhaps the most important point of similarity to consider is that both Fourier and Ohm relied on calculus to describe and analyse the flow of heat or electricity through metal solids, and specifically they relied on partial differential equations. Although the underlying argument is essentially the same, the presentation of the maths is quite different in the two books. A large portion of the *Analytical Theory of Heat* is given over to long form workings of partial differential equations and their respective integrations, using techniques pioneered by Fourier, in order to work out mathematical derivations of how heat might be expected to flow through three dimensional solids of different shapes (for a good overview of Fourier's advancements in mathematical physics, and specifically with regard to solving partial differential equations, see the introduction to Grattan-Guinness, 1972, pp. vii-viii). In contrast, Ohm did not need to derive the equations from first principles. Instead, he could simply refer to calculus. Ohm explicitly commented on the similarity between his work and Fourier's and Poisson's, saying:

> The form and treatment of the differential equations thus obtained are so similar to those given for the propagation of heat by Fourier and Poisson, that even if there existed no other reasons, we might with perfect justice draw the conclusion that there exists an intimate connexion between both natural phænomena.
>
> (Ohm, 1841, p. 403)

However, having drawn out the similarities with some of his predecessors, Ohm then set aside the need to undertake lengthy calculations, arguing that electricity's peculiar nature eradicated many of the difficulties:

> …it is therefore a fortunate chance, that in a not unimportant part of the propagation of electricity, in consequence of its peculiar nature, those difficulties almost entirely disappear.
>
> (Ohm, 1841, p. 403)

What Ohm is referring to here is that typically electricity propagates in just one dimension, along a wire for instance, but not escaping into the atmosphere in the same way that heat does. It is clear here that if he held any idea that electricity leaked out of wires in his first

phase of work, that idea had been comprehensively dismissed by this third phase of work. In addition, he argued that typically electricity moves with great velocity and has a "constantly acting source", and that within a given setup the "character does not vary over time" (Ohm, 1841, p. 403). Ohm's constantly acting force is similar to the steady state heated bar that Biot proposed in 1804, enabling careful measurements to be taken from a stable experimental setup. The consequence is that although Ohm has clearly taken inspiration from Fourier's work, and he uses some of Fourier's equations, he does not derive them from first principles in the same way that Fourier did, making the two works appear quite different at first glance.

Despite these superficial differences, both are using a differential model and Ohm follows the familiar form developed by Fourier of dividing a conductor into thin disks to calculate the quantity of electricity transmitted into and out of each disk per unit of time. In working in that way, Ohm showed that the flow of electricity could be represented mathematically by substituting electricity into Fourier's heat equation. Specifically, he showed that over a difference in time *dt* the electroscopic force *u* at a particular differential section of a straight wire *dx* was exchanged with its immediately neighbouring sections, with negligible amounts of force lost to the atmosphere via the surface of the wire. The equation that results from this process is a partial differential equation representing Ohm's theory.

> If we assume the two expressions just found for the entire change in the quantity of electricity in the disk *M* during the moment of time *dt* to be equal, and divide all the members of the equation by *ωdxdt* we obtain
>
> $$\gamma \frac{du}{dt} = \chi \frac{d^2u}{dx^2} - \frac{bc}{\omega} u$$
>
> from which the electroscopic force *u* has to be determined as a function of *x* and *t*.
>
> (Ohm, 1841, p. 451)

Ohm was then able to solve this to arrive at the same formula he had presented in 1826 during his second phase of work. The re-appearance of Ohm's law in the form we now recognise comes some thirty pages later:

> we then very simply obtain
>
> $$S = \frac{A}{L}$$
>
> where *L* denotes the entire reduced length of the circuit, and *A* the sum of all its tensions. [*S* is defined earlier as the magnitude of the electric current.]
>
> (Ohm, 1841, p. 478)

Using calculus in this way made use of Fourier's heat equations and provided Ohm with a tool for deriving his law of electrical conduction. Despite the differences in the general tone and usage of maths in the two works, ultimately Ohm relies on Fourier's equations, with the mathematical expression that Ohm uses for his analogy between electrical conduction and heat conduction following the precise form of Fourier's. In Ohm's case he simply replaces "temperature" with "electroscopic force".

One final comparison to draw between Ohm and Fourier stems from Friedman's argument that Fourier was able to make a conceptual break from tradition, leading to a new science of heat. By removing the study of heat propagation from the broader study of heat he was able to form an autonomous science. Ohm, following in Fourier's footsteps, was also making a break from the traditions held within the wider study of electricity (Friedman, 1977, p. 74). Pourprix also makes a similar argument, that the conceptual break Ohm made was in treating the propagation of galvanic electricity independently of a general theory describing matter and the forces between particles (Pourprix, 1989, p. 140). This fits with his willingness to describe how electricity is distributed in a circuit, but not what the nature of electricity itself might be. It should also be noted that Pourprix argued that Fourier's influence on Ohm's work has been overstated, suggesting that instead Ohm was influenced by dynamism and Kantian ideas. There are certainly hints at a more dynamistic way of viewing the system than Fourier offers, for example in the way Ohm approaches his analysis of what happens in an iron ring as being based in notions of equilibrium, in contrast to Fourier's particle-based analysis. However, as shown, Ohm is absolutely reliant on mathematical tools developed by Fourier.

**Responses to Ohm's work**

Ohm was aware when writing his book that he was using some very difficult mathematics, and that his German readership would struggle to understand his work. His fears were borne out when it was published. The Prussian minister of culture sent a copy to Kämtz, a colleague of Schweigger at Halle for review, which when published was very cautious and made clear that Kämtz was unable to follow the mathematical derivation (Kämtz, 1828). Another prominent review came from Pohl who was not a typical Berlin physicist, or a mathematician. He reviewed the book in a journal for scientific criticism which encouraged reviewers to judge scientific works from a "higher point of view", and to use reviews as a

starting point for an essay on the reviewer's own theories and views (Jungnickel & McCormmach, 1986b, p. 56; Schagrin, 1962). These two reviews, one lukewarm and another very negative, albeit without seriously engaging with Ohm's work, reflect the challenges of engaging a German readership with a heavily mathematical work. One final German-language review did show a remarkable comprehension of Ohm's work. Ohm sent the book to Kastner in Erlangen, hoping the book would be reviewed in his journal. Kastner invited Pfaff to write the review, but as Pfaff felt ill-equipped to undertake the work it appears from correspondence that Kastner, Pfaff and Ohm himself all contributed to the review, which takes the form of a thorough summary of the book itself (Füchtbauer, 1947, p. 171; Hartmann, 1927, p. 90). As Caneva writes, "it is less a review than a complete recapitulation and reformulation of the full mathematical theory, an undertaking which probably only Ohm would have ventured. As such it should perhaps be numbered among Ohm's works" (Caneva, 2008a). Irrespective of the third, very positive review, the book's sales were unimpressive. Not only was the reception muted, but Ohm's future career aspirations were also set back. His search for a position that would enable him to continue his research, and which consequently required access to apparatus, proved fruitless (Hartmann, 1927, pp. 118-119, 129; for a more detailed summary of the response to Ohm's book, see Jungnickel & McCormmach, 1986b, pp. 56-58).

In the years following Ohm's publication, Gustav Theodor Fechner carried out a series of experiments that confirmed Ohm's theory, published as a collection in 1831 (Fechner, 1831). Fechner's work on Ohm's law took two years. It was extensive, meticulous, and he reported his experiments in great detail so that they could be repeated. Figure 38 gives an idea of the number of different experimental setups he employed, and the book is filled with page after page of carefully compiled tabulated data. Ohm was pleased to see the extraordinary scale of Fechner's work, which he contrasted with his own limited experiments due to constrained means and time (Füchtbauer, 1947, p. 173).

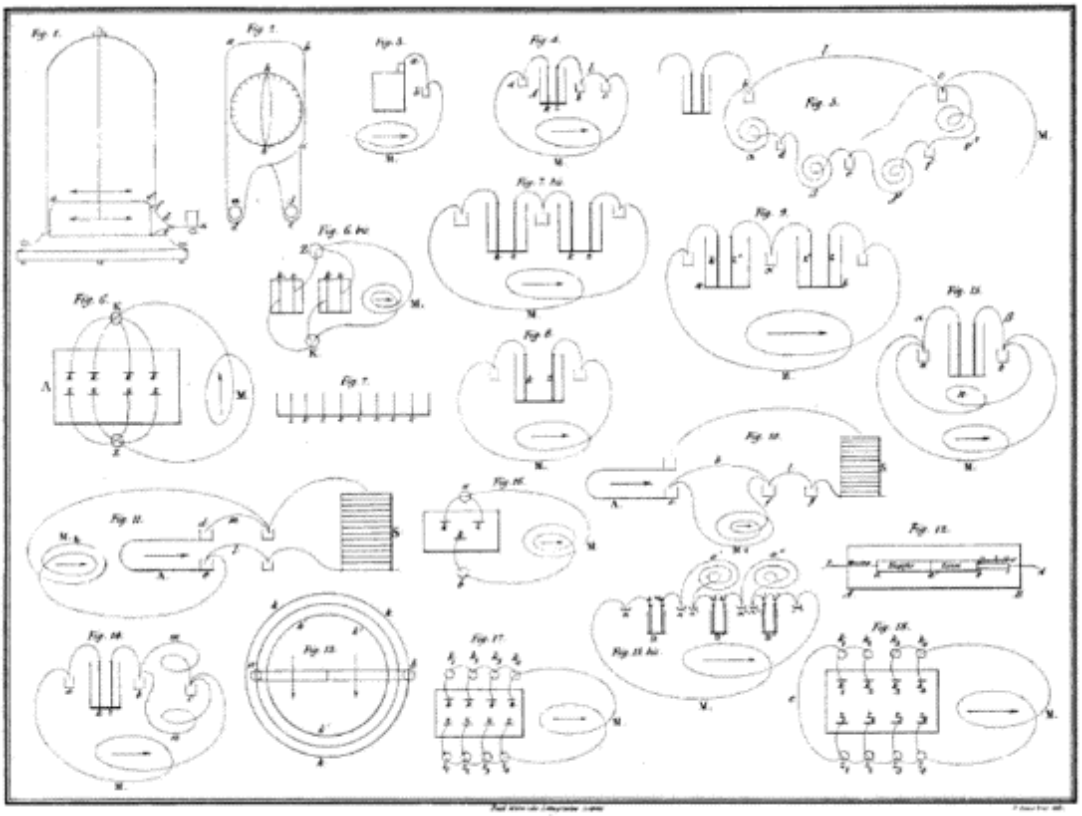


**Figure 38.** Plate from Fechner (1831) illustrating the many experimental setups he used.

While Fechner gently criticised the complexity of Ohm's solutions, saying his theoretical derivation could have been "much more elementary", he credited Ohm with bringing clarity to his understanding of the circuit as a whole (Jungnickel & McCormmach, 1986b, p. 59). Fechner's confirmation of Ohm's law does not appear to have had much of an effect on the wider acceptance of the law in Germany, although it was well enough recognised to help Fechner progress in his career.

Perhaps surprisingly, Ohm's work did not make a great impression in France either. In his comparison between Ohm and Green, Grattan-Guinness notes that both were overlooked and, surprisingly, neither were picked up by Ferrusac's *Bulletin*. In Green's case, Grattan-Guinness blames his complete failure to publicise his work, however no such reasoning applies to Ohm who sent free copies to everybody he could think of who might help him (Grattan-Guinness, 1990, p. 1176). Some of the histories of Ohm specifically report that he sent a copy of the book to the Paris Academy of Science, though they fail to report whether it was read by anybody influential (for example see Atherton, 1986, p. 471). It is possible that a book written in German was overlooked amid the wealth of other electrical research

becoming available at the time. Whatever the reason, it would be several years before Ohm's law made any meaningful appearance in France. Ironically, when it did appear Ohm was disadvantaged by having separated his mathematical arguments out from his experimental work, which, as noted above, appeared to follow norms more typical in France. His break came when in 1831 and 1837, five and eleven years after Ohm's initial publication of his law, Pouillet published articles claiming the experimental discovery of a new law, which we would recognise as Ohm's law. Following up his work in the second paper, Pouillet claimed to be the real discoverer because he had found the law experimentally, while Ohm had only derived the law mathematically. This idea was repeated in French works for many years (for an example a decade later see de la Rive, 1856, p. 77). Writing his biography of Ohm in 1891, Lommel expressed incredulity that this idea was still pervasive:

> In France the belief arose that Ohm found his law by simple deduction based upon an hypothesis, and then subsequently verified it by experiment. This belief remains to the present day in spite of frequent contradictions. It is found to-day not only in French treatises, but most inconceivably, even in widely used German text-books.
>
> (Lommel, 1891, p. 248)

Despite the pervasiveness of Pouillet's claim to priority, it was not entirely detrimental to Ohm as it also brought his work to a wider audience and shortly after Pouillet's 1837 paper was published, international readers became aware of Ohm's discovery. An English translation of the work was published in *Scientific Memoirs* in 1841, and editor Richard Taylor stated that the publication was essential for "progress and advancement of science in this country." The essential nature of the publication was reinforced in an announcement of the launch of *Scientific Memoirs*, where it was commented that "had Ohm's and Fechner's work been known in England, Dr William Ritchie would have been spared 'a world of trouble in experimenting, as well as in propounding a theory... demonstrated to be untenable'" (Olohan, 2014, pp. 436-437). Ohm's new found prominence also led to the Royal Society's prestigious award of the Copley Medal in the same year, which was instrumental in turning Ohm's fortunes around at home and helped him in his career. This award is often presented as an important moment in which Ohm finally received the recognition he deserved ("The triumph of Ohm came in 1841", Appleyard, 1930, p. 193; Cajori, 1962, p. 238; Kipnis, 2009, p. 363).

**Conclusions**

Ohm's mathematical treatise represents the culmination of an intense period of work. In the space of three years, we have seen how Ohm's work went through three distinct phases, each one taking in new conceptual ideas and theoretical developments. Although the "rags to riches" story introduced at the start of this thesis is far from representative of Ohm's true position, he was nevertheless something of an outsider for most of his career. This is reflected in his continual shifting between styles of working. He shifted between electrostatic and electrodynamic models, experimental and mathematical work, and Laplacian and anti-Laplacian approaches. His lack of adherence to any one school or tradition gave him the freedom to change course, but also caused confusion among his readership, making it difficult for him to garner support from contemporaries.

Despite these challenges and the complexity of Ohm's mathematical derivations, he produced an extremely simple and easy to use law together with some clear rules about how an electric circuit works. The strength of the law lies in its broad applicability, which enabled Ohm to analyse sections of a circuit, components within a circuit and the circuit as a whole system. Setting aside the mathematical derivations and sometimes convoluted underlying concepts and theories, from the perspective of empirically observed phenomena Ohm's law is simple. The statements he makes at the end of *The Galvanic Circuit* are at their heart simple: tension falls linearly from the source of tension to the point furthest from the source of tension; in what we would call a series circuit, the electrical force or current is the same at all points of the circuit; in what we would call a parallel circuit the current is inversely proportional to the reduced length, or resistance, in each parallel section of the circuit, and so on. Each statement makes sense alongside the others, and makes use of and reinforces the basic law. However, the simplicity here is best viewed in hindsight. Today's electrical equipment is stable, standardized and, above all, predictable. By 1841, Ohm's triumphal year, electrical equipment was already much more user friendly than fifteen years previously when Ohm was experimenting. The stable "self-depolarising" Daniell cell was invented in 1836, telegraph cables were the subject of growing industrial research and "black box" electricity meters would soon be introduced in the form of "Lineman's" devices for telegraph workers (Connelly & Chang, 2019; Dunsheath, 1962, p. 45).

In taking all this into account, it is little wonder that Ohm's story is so poorly known despite his famous name. The theoretical basis of his work was easily contested, yet within a few years electrical equipment was so stable as to make his simple law easily verified and, in

contrast, his extremely complicated experimental setup and mathematical derivations were almost redundant. Nevertheless, Ohm's innovative understanding of the circuit as a holistic system offered a new way of understanding electrodynamic systems and rapidly proved invaluable, leading to his name being given to the standard unit of resistance, formally agreed at the International Electrical Exposition held in Paris in 1881 (Dunsheath, 1962, p. 302).

# PART B

This final section on the Connelly apparatus, and the restaging of Ohm's experiment, pulls together the different component parts of the apparatus discussed in previous chapters. It focuses on the act of using the assembled instrument to take a reading, which can be translated into something that can be used as evidence to support a scientific law. It highlights the role of the experimenter in observing and recording a numerical value, and the difference between making observations to match a known outcome, as I did, and taking observations in the hope that they can then be made to fit an unknown law. Finally, it considers the analytical tools we use to think about results, and how the ways we cognitively understand and process data are socially constructed.

## Reworking Ohm's experiment

In Chapter 5, the steps required to get the instrument into a state where an observation could be made were described. The apparatus must be connected up, with the test conductor completing the circuit; the needle deflects, and then the micrometer must be adjusted until the needle comes to rest over the zero mark on the lower scale. In my restaging of the experiment, when all was working smoothly this took around five minutes for each test wire (for example see Appendix, p. 295). Having completed that process, the next task was to look at the micrometer and read off the figure indicated by a line. Sometimes this required a judgement because the line fell between two division markings, leading me to record quarter, third or half fractions of divisions (Figure 39).




**Figure 39.** Reading a numerical value between two divisions on the micrometer.

Ohm also recorded half and quarter fractions, presumably also judged by eye in this manner. Having read the value from the micrometer, I then wrote the result in my notebook, sometimes also noting the time of the observation, and, on later runs of experiments, the temperature at the hot foot of the thermocouple. This temperature observation provided a guide as to whether any anomalous results were likely to be due to instability in the thermocouple, or something else (Appendix, p. 302). While in the process of recording a run of observations, I initially tried to be as disinterested in the values I was noting down as possible, and not analyse the results as they were being made. This was an effort not to be swayed by my own expectations of what the outcome ought to be, and worked well for the first series of observations, which was remarkably close to the expected outcome (Figure 40).

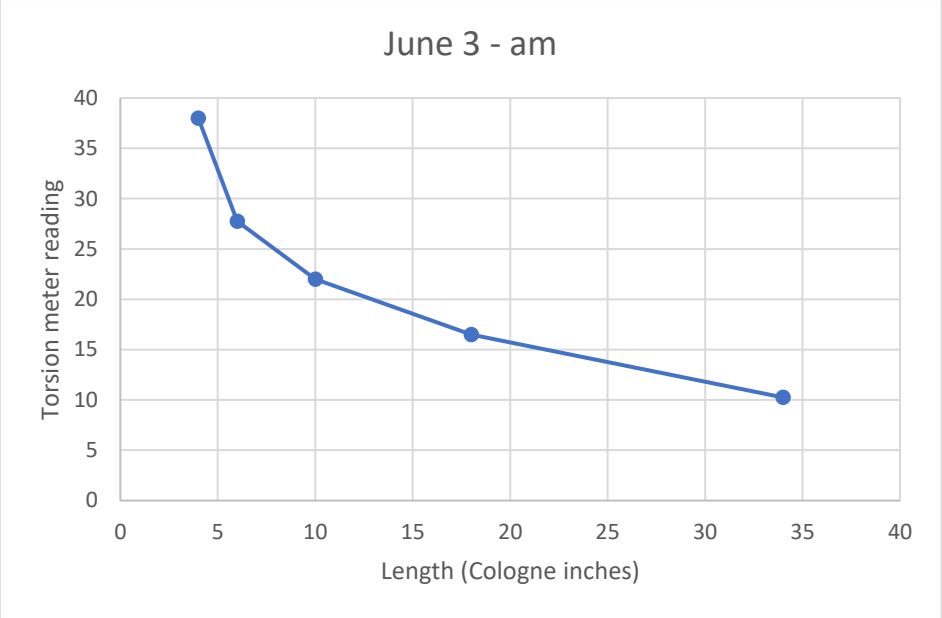



**Figure 40.** Chart showing results from experimental observations, Series 1: 3 June 2022.

For a first attempt at using the instrument, I was pleased to see that there was at least an inverse correlation between length of wire and the torsion reading. Due to my haste in starting to work with the instrument, and being more interested in handling the object than carefully thinking through the theory, I had mistakenly expected the torsion meter reading to indicate resistance rather than conductivity. Consequently, I was anticipating a straight line to suggest a negative direct correlation between length of wire and the observed results. It is possible that a significant amount of time might have been spent on trying to achieve this. However, in the following few sets of observations simply reproducing my results turned out to be a challenge until I solved the problem of what was disrupting the setup. As mentioned in Chapter 5, in improving the way I handled the hot burner in the apparatus, I had unwittingly introduced a pair of pliers which happened to be magnetised and were disrupting the setup. By the time I had realised what was causing problems with the apparatus, I had also realised my error in expecting a linear curve. To get to the linear curve, I had to plot the inverse of my observed results to convert a measure of conductivity to a measure of resistance. If these processed figures are plotted against the length of the test conductor on the *y* axis, then the results are very close to the direct correlation between "reduced length" and "exciting force" that Ohm's law predicts (Figure 41).

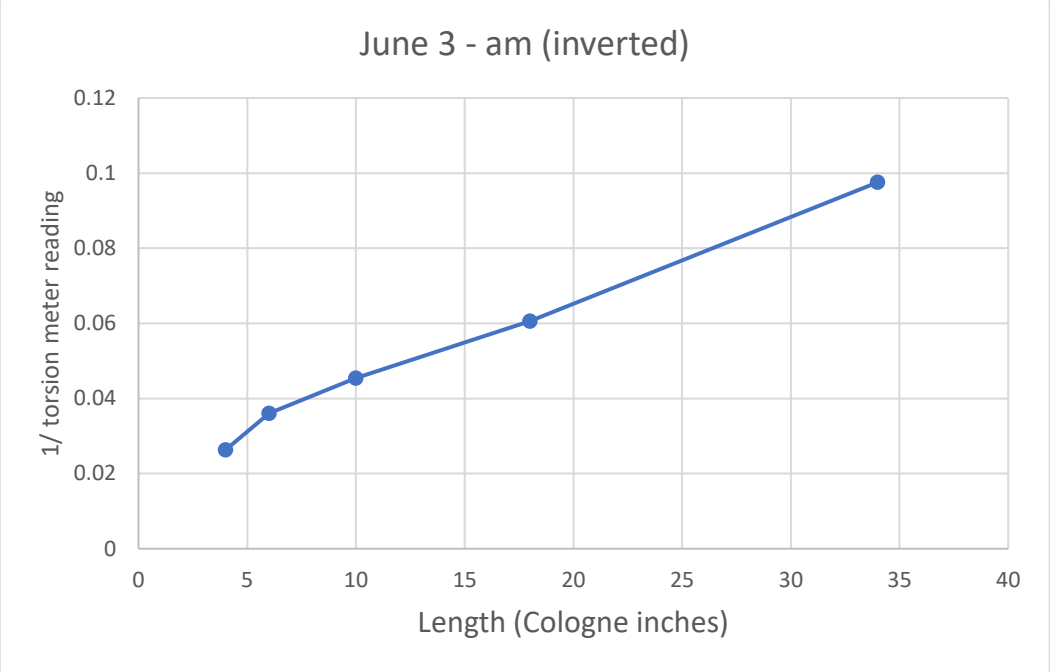
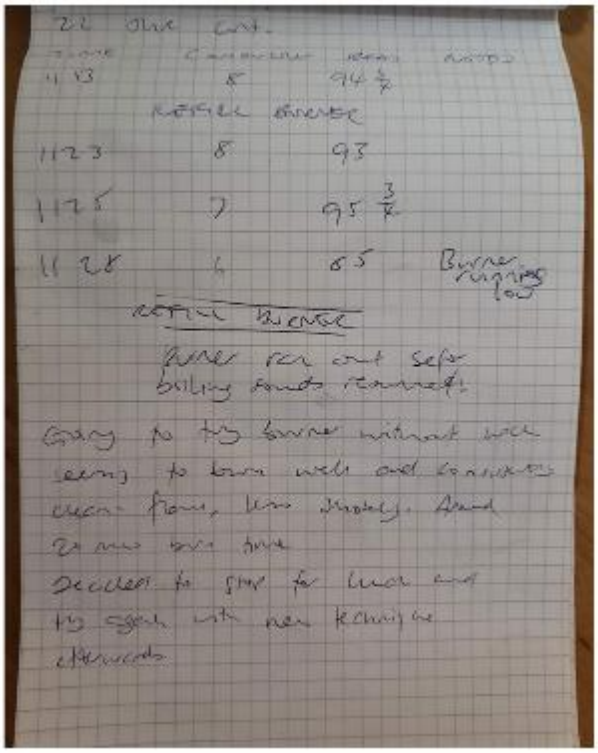



**Figure 41.** Chart showing inverse results from experimental observations, Series 1: 3 June 2022.

In the subsequent series, as it was becoming apparent that the apparatus was not behaving as expected, I briefly paused my attempt at disinterest in each result and paid more attention to the individual readings as they were made and tried to understand what was causing them to be so unpredictable. My notebook in this period of work reveals much messier note taking, with scribbled in additions about factors that might or might not be relevant, and backtracking as I sought to repeat a result that I had observed only a few minutes previously. On several occasions I chose not to complete a series of observations because the experiment was evidently not producing the expected outcome. Figure 42 provides an example of this from my notes, showing that results were inconsistent; consecutive test wires resulted in first a lower numerical result, then a higher one, then a lower one again. This was not the result I was expecting, and the excerpt shows that my response was to stop the observations and try adjusting the apparatus before continuing.



22 June cont.

| TIME | CONDUCTOR | READ | NOTES |
|---|---|---|---|
| 11 13 | 8 | 94 ¾ | |
| | REFILL BURNER | | |
| 11 23 | 8 | 93 | |
| 11 25 | 7 | 95 ¾ | |
| 11 28 | 6 | 85 | Burner running low |

REFILL BURNER
Burner ran out before
boiling sounds resumed!

Going to try burner without wick.
Seems to burn well and consistently
clean flame, less smoky. Around
20 mins burn time

Decided to stop for lunch and
try again with new technique
afterwards.

**Figure 42.** Excerpt from notes taken while running experiment (transcript to right). Only conductors 8-6 used before stopping for a break and to try a new method (Connelly, notebook p. 10).

When I eventually understood that the pliers being used to remove and replace the burner were interfering with the magnetic effect, I returned to my original procedure of deliberately avoiding analysis of the readings until after each series of observations was completed.

The processes outlined above demonstrate that undertaking an experiment when there is an expected outcome necessarily impacts upon the way the experiment is run. In my case, my strongly held conviction that my experiment should corroborate Ohm's law led to the assumption that either the apparatus or the experimental setup was at fault when the results did not match expectations. Indeed, my expectations were so strongly held that I simply did not continue with a series of observations when they appeared incorrect. In contrast, Ohm did not have the luxury of knowing the "correct" outcome of his work. As noted previously, Heering et al. have shown that for his experiments in 1825, Ohm appeared to have an idea that the relationship between conductor length and the deflection of the needle might be logarithmic. However, by the end of his experiments with the thermocouple in 1826 he had changed his mind and argued for an inversely proportional relationship instead.

Holding a preconceived idea of what results are expected can, consciously or subconsciously, lead an experimenter to read the results in a way that confirms their expectations. With the Connelly apparatus, there were times when the problems with my experimental setup led to inconsistent results. On occasion the deflection of the needle using a particular test wire was observed several times in quick succession, leading to multiple outcomes due to the unstable setup. Given the choice between these varying options, there was a temptation to select the result that best matched my expectations. My decision to try not to analyse as I proceeded through a series of observations was due to my recognition of this temptation to select the most appropriate fit. The same temptation inevitably arose again when reviewing the results after finishing the practical work. Heering et al. have argued that Ohm succumbed to this temptation with his 1825 experiments, deliberately selecting results that supported the logarithmic interpretation he went on to make (Heering et al., 2020, p. 20).

**Conceptual, paper and material tools**

This section concludes with some comments on the ways that mathematical training, and the tools we are comfortable with, structure our thinking. The first thing I did after completing a series of observations was to plot the tabulated results onto a line chart. My high school mathematics taught me, as a fundamental skill, how to recognise what a variety of formulae look like when plotted on a graph, whether they represented direct relationships, quadratic relationships, exponential relationships, and so on ("Graphs" are still taught in this way as part of the UK's GCSE mathematics curriculum. See Department for Education, 2013, pp. 6-7). While I could look from one result to the next and judge whether or not the number should go up or down based on my knowledge of Ohm's law, the quickest way for me to assess whether the relationship between the variable and the outcome was a direct one or not was to plot the results on the graph. Using spreadsheet software made this very rapid, but plotting by hand on graph paper would still have been far quicker for me than analysing anything but the most straightforward series of results algebraically. For instance, after plotting my first series of observations on a graph, it was immediately clear that my results were at least broadly in the anticipated direction but were not the direct relationship I was expecting to see. My own incorrect preconceived ideas came into play when making an initial assessment of the results. In my notes from the time, I try to explain away the curve, suggesting that the thermocouple may have still been heating and cooling at its respective ends, causing the relationship to not be perfectly linear (Appendix, p. 290).

The next few sets of observations were unusable for analysis as a result of the interference of my magnetised pliers, but once that was rectified the subsequent results, barring some anomalies, also closely matched the relationship between the length of test wire and deflection force that Ohm described (Appendix, p. 302). Using the inverted results to plot a direct linear relationship also enabled a graphical mechanism for identifying a value for *b*, Ohm's constant for describing the resistance of the fixed portion of the circuit, by observing where the plotted line crosses the *x* axis. Using this method, one can find *b* to be equal to the equivalent of a length of test wire that was around 8 Cologne inches long (Figure 43). Here another plausible interpretation of Ohm's use of the phrase "reduced length" can be seen. The phrase could be understood as a description of a unit of resistance that is equal to a unit length of the particular wire being used. Though as we have seen elsewhere, Ohm might simply be trying to use descriptive language to report empirical observations rather than relying on the terminology of "resistance".

One benefit of using spreadsheet software is that it will automatically generate the equation for a trendline for any given set of data. For this set of results, this produced the equation $y = 0.23x + 0.02$, which, using Ohm's equation, results in a value of *b* of 8.15.

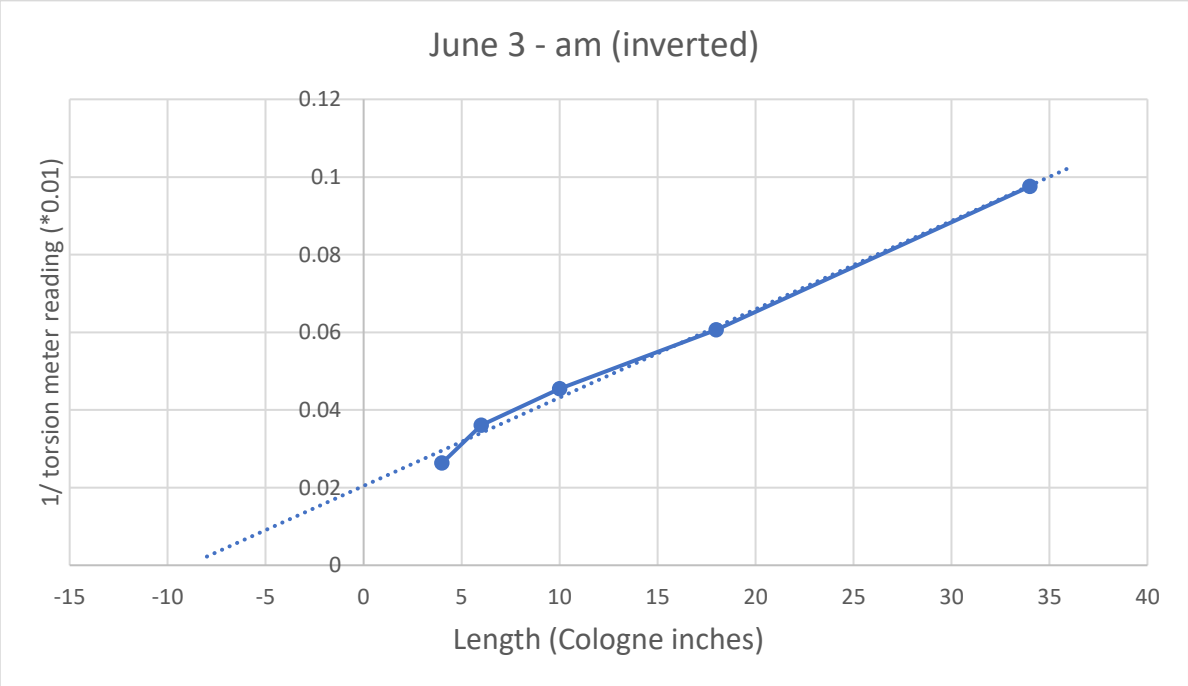



**Figure 43.** Chart showing inverse results from experimental observations, Series 1: 3 June 2022, including a trendline to indicate a value of *b* (constant representing fixed resistance) of approximately 8 Cologne inches.

These same values can all be derived mathematically by various means. Simultaneous equations offer one accessible mechanism for finding the value of *b*. For example, one can take a formulation of Ohm's equation as the starting premise, where *T* is the torsion meter reading, *a* is the constant associated with the exciting force, and *b* is the constant associated with the reduced length of the fixed portion of the circuit:

$$\frac{1}{T} = \frac{b + x}{a}$$

By substituting in two of my figures for *x* from my results in two simultaneous equations we get:

$$\frac{1}{16.25} = \frac{b + 18}{a}$$

and

$$\frac{1}{10.25} = \frac{b + 34}{a}$$

If both equations are made equal to one, then:

$$\frac{16.5(b + 18)}{a} = \frac{10.25\ (b + 34)}{a}$$

Once the constant *a* is eliminated, solving the equation is straightforward

$$16.5b + 297 = 10.25b + 348.5$$

So,

$$b = 8.24$$

This very simplistic analysis only took into account two of my readings, rather than the full series, but nevertheless resulted in a value of *b* that is close to that that derived from plotting the results on a graph.

For me, the line chart is valuable as both a mathematical technology and a thinking aid. It is almost essential for me to comprehend my results, and enables me to see at a glance whether the results are as expected. To borrow Ursula Klein's language, plotting results onto a graph is a paper tool (Klein, 2003, p. 3). Although I can follow the algebraic presentation of the

same data, I find it much less intuitive to comprehend. The graph, for me, is also a cognitive tool that is not only an aid to my comprehension, but an intrinsic part of how I think about and make sense of mathematical relationships of this type. In his brief chapter on Ohm's apparatus, Achilles also plots Ohm's results onto a line chart, noting that the results "hardly scatter". Describing Ohm's results in terms of how they would be presented when plotted indicates a contemporary bias towards graphical comprehension of data (Achilles, 1996, p. 89). Ohm, in contrast, leaves no trace of plotting his results on a chart either in his notebook pages or his published work. Instead, he used algebraic methods to analyse the relationship between the test conductors, fixed conductors and the deflection of the needle (DM HS904). This is unsurprising, as graphical representations of this type were uncommon at the time. The reason for drawing attention to this is to once again demonstrate that it is impossible to replicate the work of a historic experimenter. Even if every single part of Ohm's instrument and its physical setting could be accurately reproduced, my life experiences are different to those of Ohm's and consequently I cannot think in the same ways that he did. Nevertheless, my results have shown that his apparatus could have been operated as he described to derive the results he gave. My very first set of observations, without any of the refinement that comes from repeated usage and the development of systems, produced a set of readings that were a close match to those predicted by Ohm's law. Ohm was able not only to conceive of the apparatus and operate it, but also to apply mathematical tools to make enough sense of the results to produce his law.

# Conclusion

There are two key strands of work in this project. The first is a detailed study of the experimental, theoretical and conceptual developments of Ohm's work as he established and then set about making a case for his law of electrical conduction. In that first strand of work, I describe Ohm as a natural philosopher who was prepared to change his mind, and whose physical apparatus and mathematical tools contributed to his shifting understandings of how electricity behaves. I have described Ohm's work in three phases, which clearly show his evolving understandings of the phenomena he was dealing with. Those different understandings are in some cases most clearly understood by looking at how his experimental apparatus behaved, changing as Ohm swapped out a voltaic pile for a thermocouple. This leads neatly to the second strand of work in this project, namely an investigation into the ways in which material things can provide useful evidence for studies in the history and philosophy of science, using Ohm's work as a case study. Throughout the project I have used a number of different approaches to making sense of material objects or descriptions of material things. For historians of science, each one of these approaches offers the possibility of new understandings and findings, whether about an experimenter and experiment, or about different historiographical approaches to a topic over time.

### Categorising Ohm

Several historians have sought to categorise Ohm as, variously: a contact-theorist (Kragh, 2000, p. 139), a lone genius disconnected from his peers (Cajori, 1962, p. 234), not Hegelian enough for the prevalent view (Winter, 1944), ahead of his time (Meya & Sibum, 1987, p. 191), too mathematical (Olesko, 1991, p. 12), or caught between paradigms (Caneva, 1978, p. 65). What has become clear from getting to know Ohm's work, particularly as he traversed the different phases of work I have described, is that he was prepared to change his mind and to change his approach. In my view, none of the categories above does a good job of describing Ohm and his position in the history of science. In his first phase of work he may have been influenced by electrostatic ideas of current leakage and logarithmic relationships, but by his second phase of work his understanding of electricity as a current flowing, apparently bounded, in wires had become dominant. These two phases are strongly experimental, and by his third phase of work Ohm had turned to mathematics, using tools including geometrical analysis and the more advanced differential calculus to make his

arguments. This continual shifting between ideas and frameworks makes Ohm extremely difficult to categorise. This is true for me today as a historian, but arguably was equally true during Ohm's lifetime. His work had no clear audience that could read and understand it, and Ohm failed to build up a strong network of intellectual allies who could help him to promote his work. Consequently, it took many years for his work to be recognised, and even then the dominance of his mathematical text meant that he was not widely recognised as an experimenter and, as a result, was sometimes not considered to be the "true" discoverer of his law (Lommel, 1891, p. 248).

Studying Ohm's apparatus and laboratory texts, we can see a range of influences upon his work, not all of which are apparent from his published work or correspondence. For instance, in his instrument we can see traces of Coulomb's ideas embedded in Ohm's use of the torsion balance and, perhaps, his presupposition that he was seeking a logarithmic law. In the thermocouple we can see remnants of Seebeck and Ørsted's ideas, while in his use of test wires ideas developed by a range of people including Davy, Becquerel and Cavendish can be discerned. In his overall experimental setup, there are clear similarities with Ampère's "current balance", which itself drew on Coulomb's work. As shown in Chapters 3-5, these pieces of Ohm's apparatus tell us about his theoretical commitments. He could not claim he was observing an electric current if he did not trust that the thermocouple was producing an electrical effect, he could not claim that the torsion balance was indicating a force if he did not trust Coulomb's torsion laws, and he could not claim that doubling the length of a wire halved its conductivity if he did not already trust work on conduction by several peers and predecessors including Davy, Becquerel and Cavendish.

Taking a materials-led approach to studying Ohm's work has enabled me to make several novel arguments. The first is that Ohm's texts are insufficient for understanding his work, and that many of the scholars who have tried to place Ohm in a particular school of thought have fallen short. Ohm is not easy to categorise because he did not adhere consistently to any particular school of thought or practice, instead he picked and chose from the materials he had access to and was prepared to change his mind. The second is the observation that for Ohm's apparatus to work in the way he described, he had to put his faith in several concepts that were not widely adopted. For example, he used a flattened torsion wire, even though it had not been demonstrated that it behaved in the same way as a wire with a circular cross section; secondly, he made use of a thermocouple, even though it was not universally agreed that the magnetic effect produced by a thermocouple was due to a flow of electric current. A

third novel argument is that Ohm was strongly influenced by Ampère, primarily in his experiment design, which bears a striking visual resemblance, and possibly more fundamentally in the way his concept developed. Finally, my fourth novel argument is that Ohm's conceptual understandings went hand-in-hand with changes in his material apparatus, suggesting that the materials shaped his thinking just as much as his thinking shaped the materials.

## Tools for a material-led history of science

Throughout this project, I have sought to foreground materials. Sometimes that was done by looking at historic objects, and other times by reading texts specifically to understand objects. This project also undertook to reconstruct and rework Ohm's experimental apparatus. Ohm's original apparatus is believed to no longer exist, which made reconstructing the apparatus a more important part of my methodology than it might otherwise have been. Working with material objects in this way feels natural to me as a museum curator who interacts with three-dimensional historical objects as part of my daily work. However, I can anecdotally report that it is quite common for me to speak with historians and other researchers who admit to not knowing how to read objects and materials. Before describing some of my findings through working in this way, I will first briefly describe a number of the tools and techniques used in this project under the broad umbrella of a material-led study of Ohm's work.

### *Material culture review*

Just as a historian embarking on a new project might undertake a literature review, for this project I undertook what I referred to as a *material culture review*. I did broad, sweeping searches of online museum collections, heritage sites of interest and artworks that might relate to the project. In some cases, I travelled so that I could consult them, for instance visiting museums and memorials in Germany. In other cases, I simply noted that they existed and saved them as possible things to explore later. A list of objects and sites cited in this work is provided in the reference list, alongside texts consulted.

*Close looking*

Looking at objects can have a profound impact on how they are understood. I noted earlier in the thesis, for example, that having looked at the Science Museum apparatus, I very quickly developed an understanding of Ohm's instrument and what it did. I had struggled to make complete sense of the instrument just from Ohm's lengthy prose description and the accompanying diagram. Three dimensional things, looked at in-person rather than in a photograph or on a screen, can be transformational in trying to make sense of a piece of apparatus and how it might have been used. As well as getting an overall sense of how an instrument works, studying the object might reveal later additions, pencil markings, damage, differences when compared to written descriptions, or other unexpected features that are hard to identify from the texts or photographs alone.

*Material historiography*

By looking at a broad range of material things, I was able to draw conclusions about historiographical trends in interpreting Ohm. The way he was presented in statues and memorials, his absence from Erlangen town museum, and the different interpretations of his instrument in a variety of reconstructions were all helpful. Just as a historian can understand how a historical actor has been interpreted differently over time by reading and comparing texts, they can do this by comparing material interpretations.

*Object biography*

Though relatively little used in this thesis, an example of an object biography can most clearly be seen in my description of the Science Museum apparatus, and to a lesser extent the Deutsches Museum apparatus. Although my project was mostly concerned with making sense of an instrument that is no longer believed to exist, where reconstructions of Ohm's instrument have been produced, their lives and biographies can be traced. In the case of the Science Museum apparatus, we find it was produced for the Faraday Centenary Exhibition, then acquired by the Science Museum – perhaps against the initial judgement of curator William O'Dea – and then after a period of display was moved to the museum's reserve collection. From this we can understand that at one time it was valued highly enough to take up valuable display space, and that today its display for general visitors is not a priority.

Nevertheless, it continues to be cared for as part of the museum's collection, and can be accessed by researchers in-person on request or via an online catalogue entry.

*Replication and reworking*

The process of reconstructing and reworking Ohm's apparatus enabled me to better understand his written texts. It also built my intuitive sense of how Ohm's apparatus worked, and also how some other contemporaneous instruments may have behaved. Furthermore, it gave me insight into some skills he and his maker would likely have had. The process caused me to move from a superficial understanding of the instrument to a deep understanding of how it behaved. It also helped me gain a sense of what may have been late additions or developments within the instrument. For instance, it seems very likely that the baseboard design as it appears in Ohm's paper was developed after he started using the instrument and realised that the ideal shape had a particular form. Another useful outcome of reworking the experiment was that I gained a much better idea of the calibration steps required, and the difficulties of taking a reading or ensuring that the local environment was not introducing any unwanted effects. These would have been difficult insights to gain without operating the reconstructed apparatus.

*Commodity histories and standards*

In reconstructing Ohm's apparatus, I was forced to confront the materials available today in comparison with those available in the past. Reconstruction focused my mind, but is not a necessary step. In looking at an object, one can look at the different component parts and consider what resources, whether commodities, skills or trade networks, would have been required to bring them together. It can also help the "reader" to consider which components may have been standardised or off the shelf, and which were bespoke to this object.

*Manufacturing histories*

Similar to commodity histories, reconstructing Ohm's apparatus caused me and the maker I worked with to think in depth about the kinds of manufacturing techniques available in Ohm's time. It helped us consider whether there were parts of the manufacture of his instrument he might have taken on himself, and which parts he would have needed expert

help with. It is unlikely, for instance, that he blew and shaped the glass cylinder himself. However, it is possible that he cut the wooden baseboard, or magnetised the needle himself. By looking methodically at parts of the instrument, perhaps in the company of somebody with some manufacturing skills, a historian can begin to build up a picture of the types of knowledge and expertise embedded in an object.

*Developing visual acuity or connoisseurship*

By looking closely at an object or a selection of objects, and ideally having the opportunity to see them in three dimensions and even manipulate them, a sense of "visual acuity" can be built up. This makes it possible for similarities and links with other objects to be recognised, as I did when reading Ampère's paper about his current balance.

*Thoughts embedded in things*

Perhaps the most valuable information I took from looking closely at Ohm's apparatus came from the theories and ideas that are embedded within the object's design and materials. Within the thermocouple we can see Seebeck's ideas, and by understanding how Ohm was making use of the thermocouple we can also see Ørsted's thinking. The torsion balance is a manifestation of Coulomb's theories and designs, with Ohm's innovation of using a flattened torsion wire embedded within it. By using the torsion balance to measure electric current in a steady system, Ohm took Ampère's ideas and translated them into a new physical form. Even more fundamentally, there are theories of heat embedded in the hot and cold pots, there are ideas about electrical conduction in all the metal elements, there are understandings of magnetic attraction and repulsion within the torsion needle, and so on. Going beyond scientific theories, we can see the knowledge and practice of a variety of manufacturing crafts embedded in the object, from a wood-turned pillar to sit the torsion balance on, to a torsion micrometer marked up with 100 equal divisions. These components require metalworking, woodworking, glass working, the production of copper wire, the production of pure mercury for the liquid conductors, the production of *Goldlahn*, and so on. We can read all of these in Ohm's apparatus, and through them we can understand a little more about Ohm's theoretical leanings, his own craftsmanship and that of his maker.

*Understanding objects as extensions of the experimenter's mind*

One of the claims I make in this thesis is that a change in Ohm's apparatus caused his conceptualisation and description of electrical flow to change. Between his first and second phases of research he changed his language from "loss of force" to "exciting force" which, I argue, can be understood as descriptions of the new phenomena he was confronted with. An important part of reading an object is to either physically try using it, if permitted, or to imagine oneself using it. Was it heavy? Would it have been operable by a single person? Would it have to be mounted at a particular height to be easy to use? Is it designed for a large man, or a petite woman, or a child? Would it have been usable if mounted on wooden floorboards that bounced every time somebody walked past? How would the instrument have behaved in use? By placing oneself in the role of the user, it becomes easier to understand not only how the instrument worked, but also how it may have affected the way the user understood a particular phenomenon. My philosophical argument is that instruments can be understood as entangled parts of the experimenter's mind, and that they have a structuring effect on the way the user thinks about a particular concept. My practical argument is that there is some merit in the adage that to really understand the way somebody thinks, you have to walk a mile in their shoes. If a historian is trying to understand an experiment, then looking at the apparatus and the way it was interacted with can be very helpful indeed.

**Novel findings from a material-led approach to Ohm's work**

The sub-title of this work is "investigating the flow of electrical ideas through the instruments of their production". In studying Ohm's apparatus via texts, objects and the process of reproducing and reworking his experiment, we can understand this subtitle better. In Ohm's apparatus we can see embedded the thoughts and ideas of a number of other electrical theorists. It also follows that later electrical instruments make implicit or explicit use of Ohm's law. Ideas, theories and concepts are products of human thought, and their residues can be found embedded in material things. A number of my research findings were only accessible through close study of materials related to Ohm's electrical studies in the 1820s. To give just one example, early on in my project it became clear that the deep studies of Ohm's texts that were embodied in the numerous reworkings of his apparatus were almost entirely ignored by historians writing text-based histories of Ohm's law. The two types of study, material and physical, occupied different realms and were seen and interacted with by different communities of people.

What, then, did I learn from looking at physical objects? There are a wide range of findings that could be considered incidental to my reworking of Ohm's work, in the sense that I could, conceivably, have asked the same questions without undertaking a detailed reconstruction of his apparatus. For example, I have explored the commodity networks Ohm had access to, and the manufacturing techniques that were available in his time. I have used the apparatus to make informed speculations about some of the skills Ohm might have needed to operate the instrument, or the kind of laboratory setting that might have been useful. I have used my thorough understanding of how Ohm's experiment looked and operated to recognise similarities in Ampère's experimental setup. These kinds of investigations and interpretations could have been made via a kind of thought experiment, in which I carefully read Ohm's descriptions, looked at his diagrams and considered step by step what it would mean to operate Ohm's instrument. However, the fact is that I believed I had a good understanding of Ohm's apparatus, and it was only when I seriously set about reconstructing it that I discovered that understanding was relatively superficial as I found myself wanting to answer explore such questions in more depth (see p. 98). I suspect that if confronted with another piece of 1820s electrical apparatus now, I would be better equipped to ask those same research questions without having to undertake any reconstruction work. However, this experience of working with physical materials has shaped my thinking as a historian, just as I argue it shaped Ohm's thinking as a physicist.

There are more explicit findings that I could only have arrived at by working with the materials. The first is an understanding of the procedures involved in taking a measurement, and knowing for sure that it was possible to operate the instrument single-handed. Ohm does not report having any assistance, and it is apparent to me now that there was no necessity for him to have any help during his observations. A second finding is a greater appreciation of the degree of improvisation involved in getting an instrument working. Unlike Ohm, my maker and I had a reasonably good, if not entirely complete, description to work from. Nevertheless, I found myself adapting the experiment and introducing new materials such as a roof slate to operate the instrument safely. It is very likely that Ohm and his maker had to adapt many more elements as they produced the instrument. This all goes unrecorded, however, and we are presented with a finished product in Ohm's 1826 "Announcement of Laws" paper.

The most important argument I have been able to make as a result of close study of Ohm's instrument relates to how the instrument shaped his thinking. Between Ohm's first and

second phases of work he shifted from a concept of "loss of force" to one of "exciting force". This is clear in his texts, though not explained. Various historians have suggested that the shift represents an outmoded, even misguided reliance on old electrostatic ideas before in his second phase of work Ohm became enlightened and adopted new electrodynamic ideas. This, in my opinion, does Ohm a disservice. While it is true that he appears to have had a logarithmic idea in mind for his first phase of work, it seems to me that the simplest explanation for his notion of "loss of force" is one rooted in empirical observations. Furthermore, in reporting his experimental work Ohm almost goes out of his way to avoid getting mired in theory, using descriptive language to carefully report the phenomena he sees. When he connected his apparatus up to a voltaic pile, the magnetised needle in the instrument's torsion balance was initially deflected by the force this introduced, and then the amount of deflection gradually reduced suggesting that the force deflecting the needle was reduced. This action can be described as a "loss of force" without any significant reliance on theory. Of course, that does not preclude from Ohm holding electrostatic ideas, however, given how widely read he evidently was in French mathematics and physics, and given that he had a good enough understanding of electromagnetic theory to design his experiment, it seems very unlikely to me that he would still be clinging on to electrostatic ideas so firmly. Turning to Ohm's second phase of work, he swapped out the voltaic pile in his apparatus for a much more stable thermocouple, and consequently the behaviour of the magnetised needle changed markedly. On connecting up the apparatus to the thermocouple, the needle was deflected. The force of the deflection varied according to the qualities of the conductors in the apparatus. Ohm was able to observe the force of deflection by the amount of turning of the torsion micrometer required to return the needle to its starting position. The force affecting the needle could be described as the "exciting force" without any significant recourse to theory. Again, in my view, this is an attempt at an observation-based description. Of course, there were plenty of theories embedded within the apparatus, and in Ohm's interpretations of what it was doing, but my argument here is that the apparatus itself shaped Ohm's conceptualisation and use of descriptive language, and that only by studying the apparatus and its operation could I fully understand that. A closing observation is that in his third phase of work Ohm turned to mathematics, and in contrast to his earlier work was much more prepared to make assertions about, for example, the distribution of electrical tension in a circuit, provided it was supported by his mathematical derivations.

**Ohm's legacy**

Just as I have found it instructive to understand Ohm's experimental practice, so too his contemporaries might have done. However, Ohm's most widely publicised work was his mathematical treatise which, as we have seen, was written for one type of audience but shared with another. As a result, his work passed relatively unregarded for several years before finding a new audience. When recognition finally arrived, most people were only introduced to his mathematical work, and his experimental work received barely a mention. In between Ohm's initial publication of his findings, based on complex mathematics and an equally complex experimental setup, and the recognition of his work, the age of the electric telegraph had arrived. In the space of a few years, electricity had become big business, and electrical technologies were stable enough to be reliably and repeatably used. With those technological developments, a monograph-length mathematical derivation of Ohm's simple law was hardly needed. The results Ohm obtained with his convoluted experimental setup could be achieved with readily available telegraph equipment. By the 1860s, when the unit of resistance was being named the "ohm", the equation and the associated rules about how circuits in parallel and in series behave had become an everyday tool that trainees were taught as a matter of course in the rapidly professionalising world of telegraph engineering (Bourne, 1991; Connelly & Chang, 2019, p. 174).

Ohm's law continues to be fundamentally important to technologists in today's digital world, and Ohm's ideas are embedded in almost every device we own. From my desk here, as I type this, the power for the light above me is delivered by the national grid, my mobile phone is charging by my side on a wireless induction charger, and the microprocessor in my computer is sending and receiving millions of tiny "bits" of electronic data. The legacy of Ohm's law can be found in its ubiquity; Ohm's ideas are embedded in the things that surround us.

# Appendix: Report on the production and use of a reproduction of Georg Simon Ohm's 1826 experimental apparatus

## Introduction

This report details the process of producing and using an interpretation of Georg Simon Ohm's 1826 experimental apparatus. To aid comprehension, the work is presented as a progression through the instrument from top to bottom. Unlike the actual production, in which different parts of the instrument were produced in tandem, or still being designed as another part was being manufactured, this report works through one component at a time. It should be noted, however, that the progression was iterative, and individual decisions were often dependent on how another part of the instrument developed. The base of the instrument provides the best example of this iterative way of working, but examples can be found throughout this report.

The account here is primarily drawn from a reflective diary and a log of correspondence via email and WhatsApp messenger between me and the maker, Martin Connelly, kept throughout the process of manufacturing the instrument. After the maker delivered the almost complete instrument to me, I produced and added the final parts of the apparatus before operating the instrument alone. My experimental work with the instrument was documented using a combination of written notes, photographs and audio recordings taken while I was operating the instrument. This document provides an abstracted version of the materials described above, one step removed from texts and documentation which were produced without an audience in mind, and translated into a piece of writing with the intention of making them comprehensible while incorporating detailed accounts. Rather than being an account produced after the fact, it was written as part of the process of reworking Ohm's experiment and can be understood not simply as an account of the reworking, but as a manifestation of the process itself.

I have made the deliberate decision to write the report in the first person, and occasionally refer to "we", meaning myself and the instrument maker. The convention in scientific reports is to write in the third person, introducing a level of abstraction between what was done and who was doing it. On this occasion, I want to re-insert my agency and the agency of the maker as interpreters, designers, builders, decision makers and users of the instrument. This report highlights the multiple places where there were different ways of proceeding, and

where we made decisions based on our personal knowledge, experience or biases. This report makes very clear just how many decisions were required to interpret Ohm's, apparently thorough, written account and turn it into a physical object. In doing so, it emphasises how conditional this kind of work is on the knowledge and skills of the people involved. In this case we were a historian and a maker, doing both the interpretive work and the technical work of production.

## Approach

As my research makes evident, it is not possible to accurately recreate every aspect of a historical experiment. People alive today have different types of knowledge and understanding to those in the past; changed commodity networks mean that even the most fundamental materials, for example copper, an elemental metal, cannot be assumed to be consistent over time; and different tools and techniques are available at different times. The aim of this project, then, is not to "accurately" recreate Ohm's experiment, because that is unachievable. Instead, it seeks to follow the spirit of Ohm's experiment, acknowledging the numerous places where interpretation is necessary in a reworking, and providing some rationale for each decision made.

The purpose of undertaking the reworking, and of documenting that work, is partly to observe the processes of reconstructing an experiment from written, experiential and practice-based evidence. The process of producing the instrument is perhaps even more interesting than the outcome of the experiments with the apparatus. The "success" or "failure" of the reworked experiment is secondary to the many things learnt in the process of getting to know the experimental apparatus and its workings in detail by reproducing it. This process also serves to illustrate the substantial role played by an instrument maker. Ohm, like many other researchers, does not make explicit the role that his maker, Herr Mauch, played in the production of his apparatus. He is silent on which decisions he made himself, and why, in the development of the instrument design, leaving today's historians with a challenge to plug the gaps in his descriptions. What this experimental reworking makes clear is that no part of the design of the instrument could have been made without some deliberation. To name just a few examples: how many divisions should be marked on the micrometer? How should the torsion thread be connected to the micrometer? How tall should the glass cylinder be? How thick, or thin, or long, or strongly magnetised should the needle be? If the fixed conductor is

made from bent copper, does it matter if the cross section of the piece changes along the bend? Which parts should be fixed and which should be moveable to enable good calibration to take place? How will the user ensure the instrument is properly levelled? And so on. At each of these decision points, Ohm or his maker selected a way to proceed, introducing new design features or protocols for using the instrument. Perhaps they sometimes backtracked, or perhaps they got the design right the first time. In constructing an interpretation of this apparatus, and the experimental processes required to use it, some of those design questions, together with the underlying theories or types of knowledge required to answer the questions, are revealed. Many more are left unresolved, and pragmatic solutions were adopted in order to build the apparatus.

## Identifying a maker

Before getting into the detail of producing the instrument itself, a brief note on identifying a maker to collaborate with is needed. Returning to my diary to write this report, I discovered that my diary-keeping was lacking. While I had taken care to note the many technical developments involved in producing the instrument, the process of selecting a maker went unrecorded. This short note is, therefore, based on my recollections several years after the fact. At the very start of this research project in autumn 2014, and even earlier when writing an initial funding application, the idea of replicating Ohm's experiment was central to the project. Early on I began investigating possible makers to collaborate with, and likely sources of funding for paying a maker to undertake the work. Initial investigations were not promising. There are few commercial enterprises today that have the range of skills and experiences needed to produce a nineteenth century scientific instrument. It was also clear that the project not only required somebody with the right technical skills, but also the time and interest to engage in dialogue, and to be prompted to consider whether and how they were thinking like a twenty-first century maker rather than a nineteenth century maker. This intellectual workload proved to be substantial, though hard to quantify or work into the project design. Despite occasionally returning to my search, and asking friends and colleagues for recommendations, there was no readily identifiable maker available to collaborate with.

Throughout this early period, I anticipated working in the same way as I would for any commercial project that required a creative response: producing a design brief in order to

seek quotes and develop a project plan. I spoke to a range of people about what might go into such a document, including my father, Martin Connelly, who at that time was working as a production engineer for Siemens. As the project progressed, in the background there were numerous changes in my personal life. The most significant for this project was taking up a full-time role as the Museum Curator of the Scott Polar Research Institute, which impacted on my doctoral research in several ways. One was to transfer it to a part-time project, stretching the deadline into the future. Another was that I had disposable income which could be spent on manufacturing the instrument, removing the need to seek external funding with, most likely, specific timeframes and reporting requirements. As well as my own life changes, my father retired from work. That meant that rather than offering advice on the development of a design brief for the instrument, he had the time to take on the production of the instrument. With a long career of both designing and making bespoke parts and assemblages, and training in both traditional metalworking and contemporary computer aided design, he was well placed to support the wide-ranging requirements of recovering and interpreting the design of the instrument, alongside manufacturing our joint interpretation of the instrument.

As a researcher, I was initially uneasy about collaborating with a family member, wondering whether it would be possible to maintain the necessary level of disinterestedness. However, the close relationship we share had multiple benefits throughout the project. On a very pragmatic level, it was beneficial not to have a tightly constrained commercial relationship. We could have long drawn-out conversations without worrying about how they were eating into a set budget. We could also take time to reflect on progress before moving on to the next stage of work, or try some options without being concerned about following a dead-end, as we were not working to a tight deadline. Another unexpected benefit of working with somebody with whom I already had a good relationship was that there was no client-business relationship to navigate. While we both brought different expertise to the project, and were respectful of that, the typical hierarchies introduced by a commercial relationship were not present. That meant I could push back against manufacturing techniques that assumed modern-day knowledge. In turn I was challenged to select methods that might have been preferable because they limited materials wastage or would have been less labour intensive with nineteenth century tools. Overall, the experience has been a positive one. Indeed, it is hard to imagine how the project would have been completed otherwise. One solution might have been to work with different makers for the glassware, woodwork and metalwork, and hope that the communication was good enough for all parties to produce a coherent whole.

Another would have been for me to undertake a much more involved study of these manufacturing crafts so that the intellectual choices were mine alone, putting me in a position of more straightforwardly instructing a manufacturer, rather than having a fruitful two-way dialogue. It seems unlikely that either of these scenarios would have been easy or resulted in a high-quality reconstruction of Ohm's experimental apparatus.

## Instrument design and production

Having identified a maker to work with, an early barrier presented itself. After initial discussions with the maker about what he needed to know, it became apparent that a common language was necessary. I had initially shared Ohm's lengthy prose description of the torsion balance from his 1826 paper, leading the maker to comment that there were a lot of things missed out of the description that would need addressing. What he ideally wanted was a computer aided design (CAD) drawing to work from; a partial drawing would be enough to start the conversation in a meaningful way. To this end, I began producing drawings initially with a free software package called FreeCAD, and after some challenges with that, using the industry standard package AutoCAD.

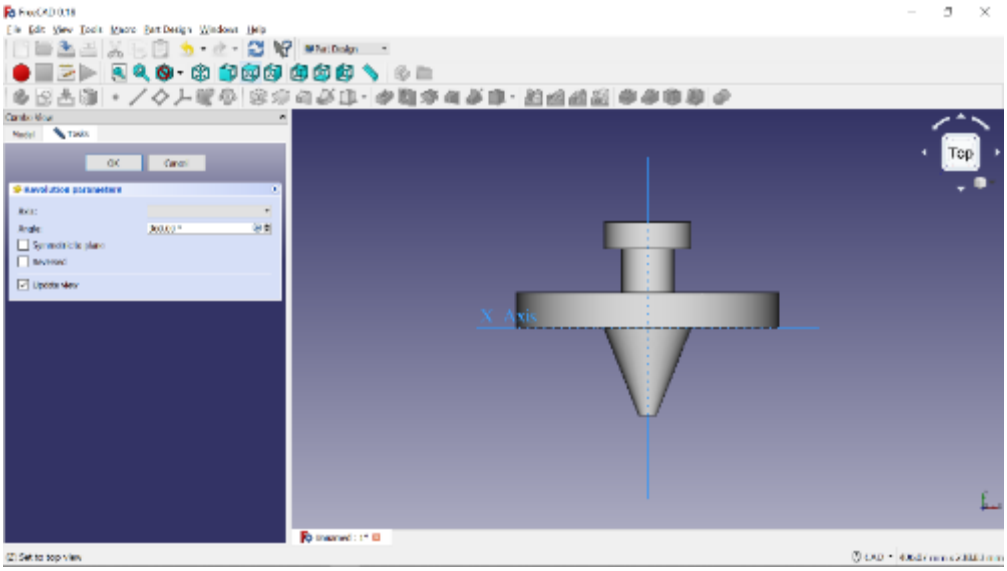



**Figure 44.** Screen shot of an early mock-up of the micrometer from Ohm's apparatus in FreeCAD. 1 October 2020.

CAD software requires the user to input spatial data: length, diameter, location, etc. This immediately raised the question of what units of measurement Ohm was using. He referred to

*Linie* and *Zoll*, or lines and inches, which were not standardised units at the time. An online search unearthed a handbook of "coins, masses and weights" dating from 1830, with a table of inches for different parts of the German speaking world converted into millimetres. At the time of Ohm's experimental work he was living in Cologne, then a part of Prussia, leading me to adopt Niemann's definition of the Cologne foot: "Cöln [Cologne] am Rhein, Prussia, Fuß, 287 mm" (Niemann, 1830, p. 28; "Obsolete German units of measurement", 2020).

The same source suggests that an inch, or *Zoll*, is usually $^1/_{12}$ of a foot, but also sometimes $^1/_{11}$ or $^1/_{10}$. I took the most common, $^1/_{12}$. Similarly *Linie* or line is usually $^1/_{12}$ of an inch, but sometimes $^1/_{10}$. Again, I took the more common $^1/_{12}$ of an inch. This led me to adopt the following conversation table in all workings for the instrument.

**Table 3.** Conversion table: Ohm's inches to metric units

| | |
|---|---|
| Foot / *Fuß* | 287 mm |
| Inch / *Zoll* | 24mm (rounded from 23.916666666666) |
| Line / *Linie* | 2 mm (rounded from 1.993055555555) |

**Torsion balance, basic form**

Having ascertained the scale being used, the next step was to begin drawing the apparatus in AutoCAD, starting with the factual information provided in Ohm's written description. The order of developing the instrument emerged quite naturally as a top-down process, starting with the micrometer at the top of the instrument, then the glass cylinder and torsion thread and so on. This sequence partly emerged from following the order that Ohm presented his work in, but we also started with the part of the instrument where the most detail was provided:

> It [the micrometer] itself consists of two parts, one of which *nop* is provided with a slightly conical socket, and is cemented fast to the upper plate of the glass cylinder; the other, *qrs*, with a conical projection 8 lines thick fits exactly into the cavity of the socket, and with a plate *rr* 3 inches wide which rest on the plate *nn* of the first part, which is the same width.
>
> (Ohm, 1826a, p. 144)

The diagram below shows an elevation cross-section of a drawing of the instrument designed to the specifications above. The measurements that Ohm provides explicitly are marked in green, with his measurements in inches and lines. The dimensions of the red sections are not

specified in Ohm's writing, but instead are subject to interpretation. They were judged approximately by eye to resemble the diagram that accompanied Ohm's paper. The labelled letters are copied from the diagram in Ohm's paper.

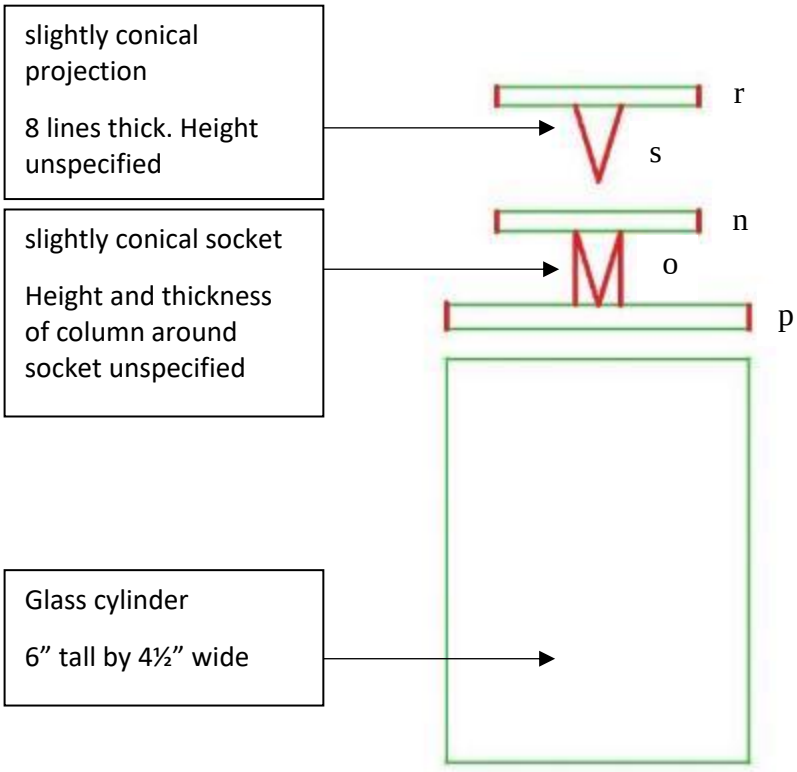



**Figure 45.** Cross section of torsion balance and glass cylinder showing known dimensions in green, and speculative dimensions in red.

Ohm continues:

> The midpoint of the projection *qs* was marked with great care on the lathe by a slight conical depression, and the metal was then filed off for a half inch of its length until the plane surfaces which are thus developed show the conical depression as a complete triangle. A special device clamps the thread on which the steel needle is attached to the projection so that its midpoint falls exactly in the apex of the triangle.
>
> (Ohm, 1826a, p. 144)

This description requires a bit of interpretation, and at this point it becomes useful to refer back to Ohm's diagram, which shows that *q* marks a knob at the top of piece *qrs*.

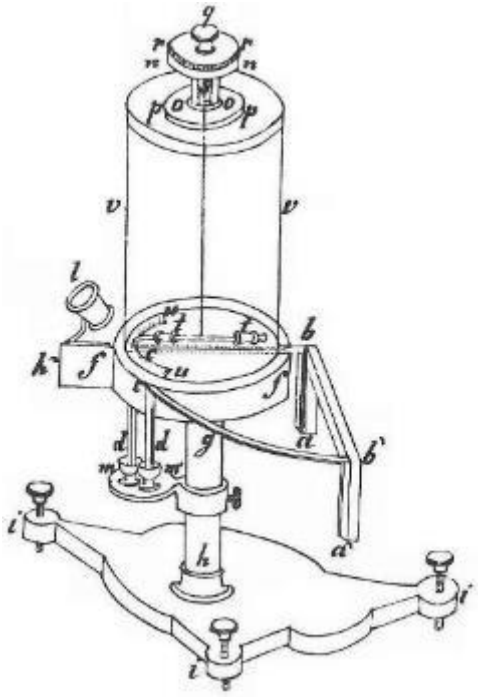


**Figure 46.** Diagram of Ohm's apparatus from his paper (Ohm, 1826a).

My initial reading of the description was that the midpoint of projection *qs* referred to the midpoint of a line drawn between *q* and the tip of *s*. That indicated that a decision was needed about the height of the piece, which was not specified by Ohm. Using the drawn diagram as a visual guide, I estimated the height of the cone shaped lower part of *qs* at 1". This in turn established the size and shape of the socket in the lower piece. My next line of reasoning was that:

- There's only one plane that could be filed away to reveal a triangle
- For the wire to stay in one position, aside from twist, when in use it will need to pivot around the centre line of the conical shape. That means the shape must be filed as pictured below.

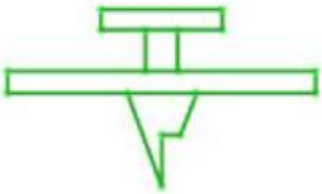


**Figure 47.** Cross-section of an initial drawing of the upper part of the torsion micrometer.

In coming to this conclusion, I was influenced by the reworkings of the apparatus by the Science Museum and the Deutsches Museum (Figure 48 and Figure 49).

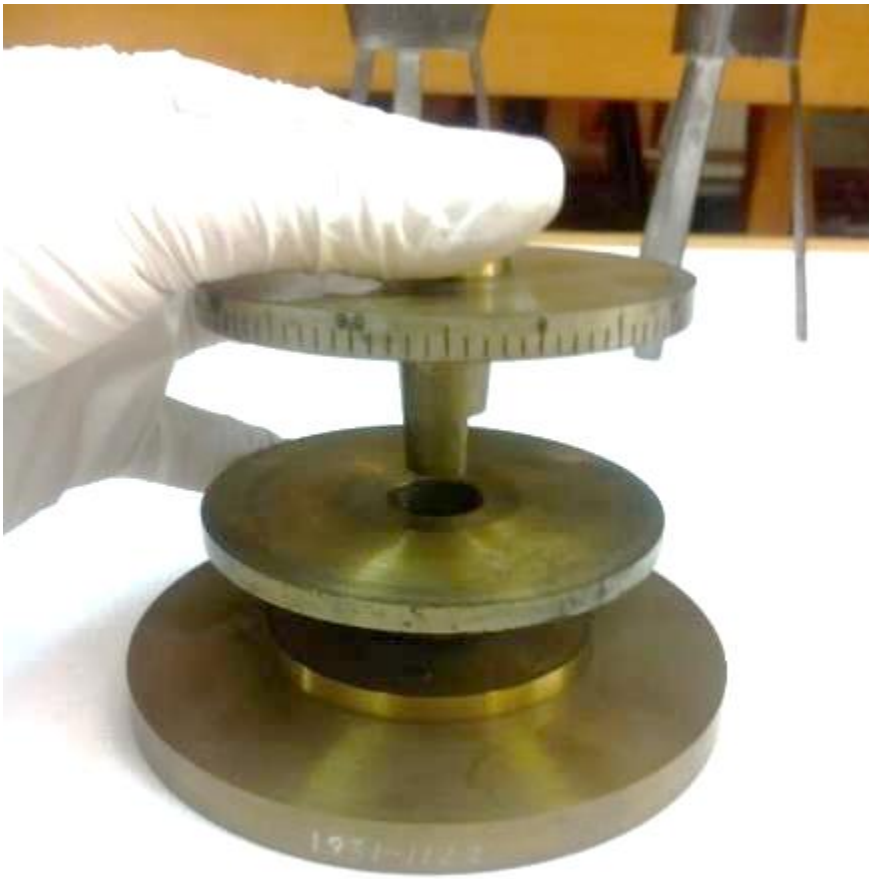


**Figure 48.** Science Museum apparatus micrometer(inventory number 1931-1122).

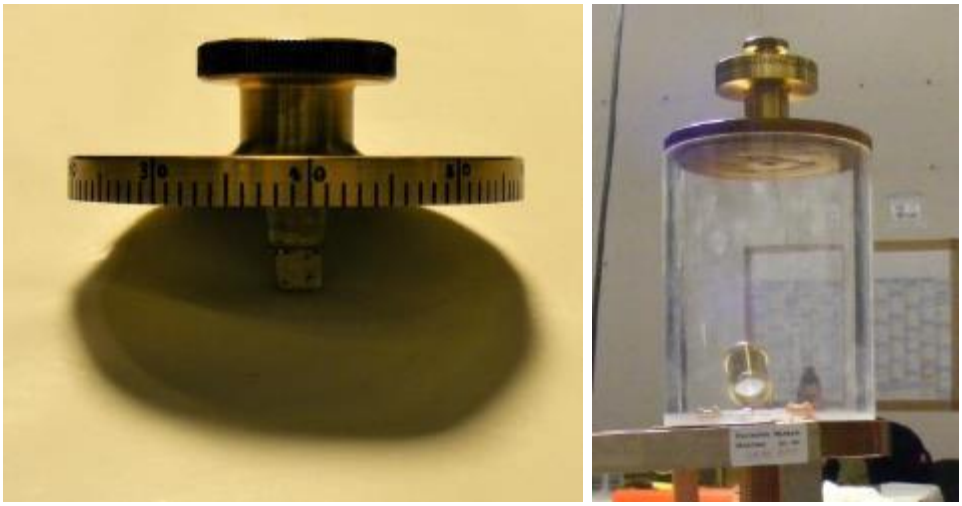


**Figure 49.** Deutsches Museum apparatus micrometer (object number 1976/9/9).

In both examples, the point has been filed off the conical shape, and the side filed away to reveal a flat surface. However, neither example revealed the triangle shape Ohm discussed. From here, I continued developing a CAD drawing to share with the maker. Having designed the size and shape of the conical projection, the next task was to see whether it worked with the lower part, *nop*. Ohm's written text left out detailed description, including failing to discuss the need for an opening in the lower piece for the torsion wire to pass through.

My estimated height for the conical projection of 1" enabled me to draw up what the lower piece, with a conical socket, could look like (Figure 50). Each decision point acted as an opportunity to check previous decisions and laid the foundations for future decisions. This iterative process proved to be characteristic of the process of developing the instrument.

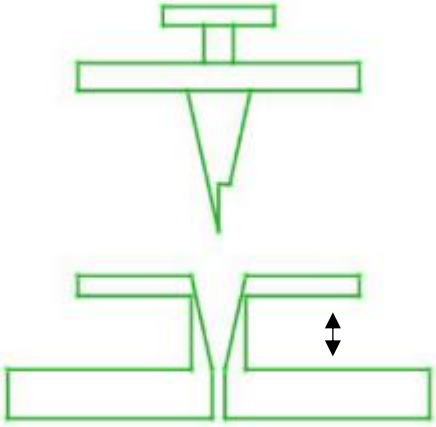


**Figure 50.** Initial sketch of lower piece *nop* following early drawing of *qs*, the conical projection.

In this instance, producing the drawing of the next piece of the instrument, based on the assumptions and decisions made in the first, raised two immediate problems:

- The height, marked up with a double ended arrow, is much smaller than it appears in the diagram in Ohm's paper.
- The tip of the projection will stick out of the bottom of the lower part. This is an aesthetic problem rather than a functional one.

In this instance the 1" estimate had been arbitrarily made and could be changed. The design of the conical projection, however, required more consideration. On sharing the designs with the maker, he interpreted Ohm's text differently, writing that the "midpoint of the projection was probably centre drilled to give a slight conical depression pointing up from s towards q. When half is filed away this conical dent in the end will be triangular" (personal correspondence, 4 November 2020). This comment in an email was accompanied by the sketched diagram in Figure 51.

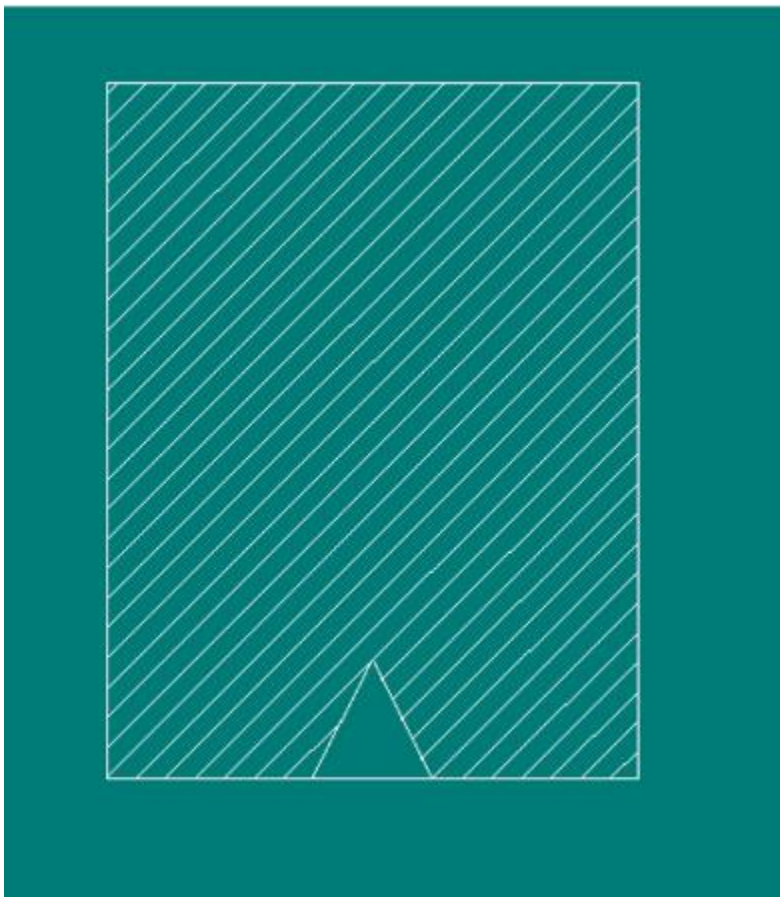


**Figure 51.** Illustrative diagram sent to accompany description of the "midpoint of the projection" being drilled to reveal a triangle shape. (Image courtesy of Martin Connelly.)

The Engels and Riess interpretation is similar to the Connelly interpretation. The only difference is that the conical shape looks closer to a cylinder in their example, however it is hard to say with certainty whether that is the case from the images in their paper (Engels & Rieß, 2022, pp. 94-95). In the Connelly apparatus there was a deliberate decision to make the taper of the conical project a reasonably steep angle to avoid producing a self-holding taper, as discussed below.

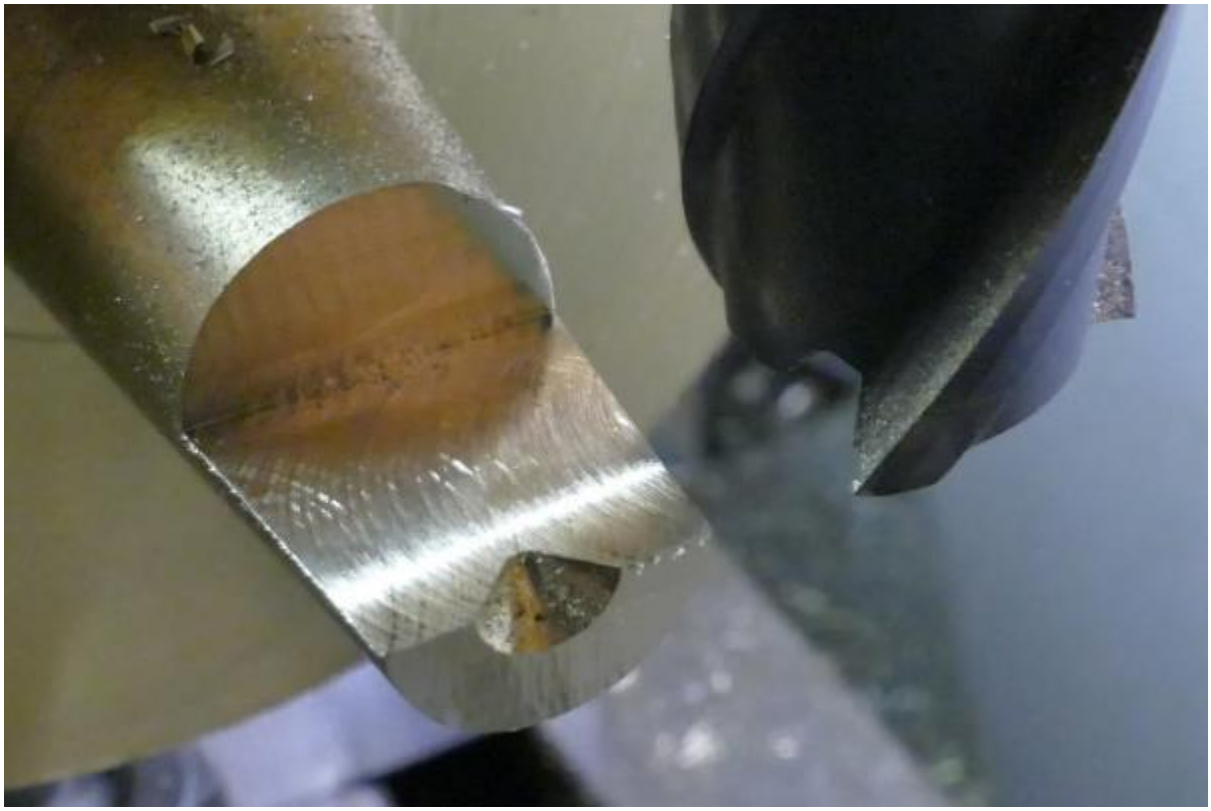


**Figure 52.** Photograph of the indented part filed away to reveal a triangle shape (Image courtesy of Martin Connelly.)

The reason for providing such a detailed description of the level of interpretation required in this piece of design is to illustrate clearly that, even where there seems to be a high level of detail in Ohm's prose, there are still multiple possible interpretations. In this case, the decisions taken could impact directly on the behaviour of the torsion wire, and therefore on the basic functioning of the instrument as a tool for measurement. Decisions around the design of this part are, therefore, not trivial. As we will see, there are a multitude of places where different decisions can be taken, some with more or less influence on the functioning of the instrument overall.

A final point raised by the instrument maker in the design of the upper part of the micrometer related to agreeing an angle for the taper, or the conical protrusion. He wrote:

> The subject I think you need a bit of background info on is self holding tapers and the issues of designing with a slight taper that is too slight… I can't find a coefficient value for brass on brass. This is probably as there are many different types of brass. If the taper on the top part of the apparatus is too slight it will not give smooth movement. If it is too coarse it will not centre the torsion wire as well as it could. The

> point is that designing the parts needs some understanding of the result of picking the wrong taper angle.
>
> (Connelly, personal correspondence, 12 November 2020)

Put another way, depending on the angle of the taper the two pieces might lock together in a "self-holding taper" when turned due to friction, or alternatively the angle might be too wide for the pieces to sit snugly together to enable accurate calibration with each usage. Trial and error with some scrap pieces enabled the maker to find an angle that was not problematic.

Having designed the taper and filed away a section of the taper to reveal a triangle, the last task to complete the physical form of this piece was to design and produce a "special device" to clamp the torsion thread to the micrometer (Ohm, 1826a, p. 145). No further explanation was provided, meaning that once again the maker was free to find his own solution. He proposed a small plate which could be screwed into a threaded hole in the flattened surface of the micrometer, shown in Figure 53 with and without the plate in position. A small piece of leather is attached to the clamp so that the torsion wire is not damaged when inserted.




**Figure 53.** Design of the "special device" to clamp the torsion thread, on the left a photograph of a small hole drilled into the flat surface of the tapered piece, and on the right the same view with the plate screwed down and a sample thread held in place. (Images courtesy of Martin Connelly.)

Our solution was similar to that incorporated into the Deutsches Museum apparatus (Figure 54). In contrast, the Science Museum instrument did not employ any clamping device, but

instead simply trapped a long ribbon of metal between the upper piece of the micrometer and the lower piece (Figure 55). This is simple, but effective, provided the thread is very slim and the user can be sure that it is properly centred as it hangs down. However, it would not be possible to calibrate such a setup reliably, adding another piece of evidence to my argument that the Science Museum apparatus was never intended to be a working reconstruction.

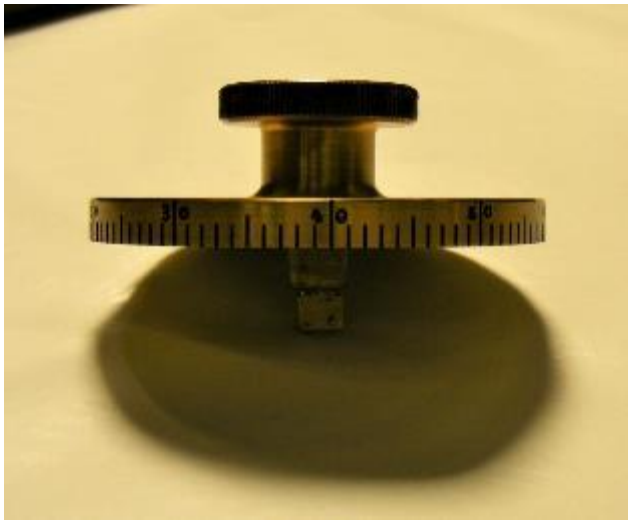


**Figure 54.** Photograph of the Deutsches Museum's interpretation of the "special device", a similar plate arrangement, with two screws instead of one.

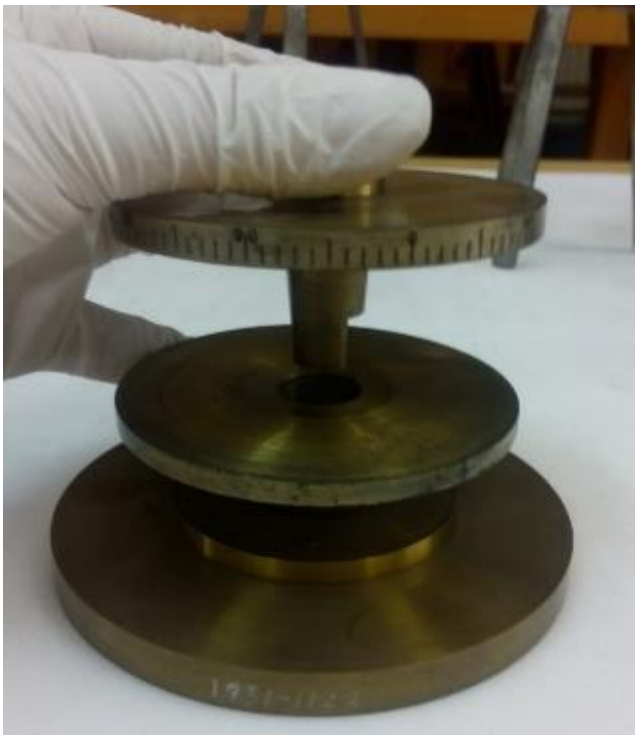


**Figure 55.** Photograph of the Science Museum's interpretation of the micrometer.

**Glass cylinder**

To produce the glass cylinder, a local company, Cambridge Glass Blowing, were approached with the drawing in Figure 56. The height and radius of the cylinder were specified according to Ohm's text, but the thickness of the glass and type of glass were unspecified.

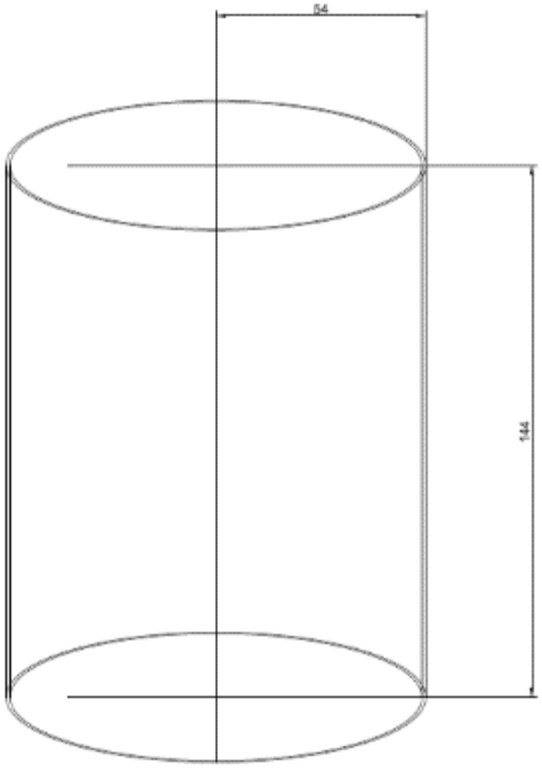



**Figure 56.** Dimensioned drawing of glass cylinder shared with Cambridge Glass Blowing.

They replied, saying "The closest standard tube size we can offer is 110mm ±1.40mm Outside Diameter. Would this be suitable for your application?" (personal correspondence, 29 October 2020). The order was confirmed with the following specification: "Glass Tube 110mm OD [outside diameter] x 3mm Wall x 144mm +/-2mm Long – Ends cut and flame polished." At the time I noted in my diary:

> Given the imperfection involved, and the fact that the dimensions of the glass cylinder are (a) unlikely to be critical to the experiment and (b) would have been hard to produce with precision in 1820s Cologne, I considered that using a standard tube of 110mm diameter (±1.40mm tolerance) was a close enough approximation to the 108mm diameter tube (±1.40mm tolerance) that I had originally specified.
>
> (Connelly, reflective diary, 4 November 2020)

The overall cost was £30 per unit, though a minimum spend of £150 meant that it made sense to purchase two cylinders for £75 each, meaning there is a spare in case of breakage. The pieces were delivered on 21 December 2020.

As the correspondence above reveals, today's commercial glassblowing businesses stock a range of standardised tubes to cut pieces from as needed. It is far more likely that in 1820s Cologne an individual item like this would have been hand-produced as a bespoke product. However, Ohm does not provide details, and, as the likely function in the instrument was to hold the micrometer in place and prevent draughts from affecting the torsion wire, understanding and perfectly matching the detail is not necessary. Perhaps understandably, detail is scarce for parts of the instrument that had limited bearing on the functionality of the equipment in use.

Because the glass cylinder had been specified to be produced within set tolerances, the final base piece of the micrometer was not manufactured until the glass cylinder was available. This enabled it to be produced with a snug fit. At this point it is useful to think about which parts of the instrument were cemented or otherwise fixed together, and which parts were free to move. Ohm writes:

> It [the micrometer] itself consists of two parts, one of which *nop* is provided with a slightly conical socket, and is cemented fast to the upper plate of the glass cylinder; the other, *qrs*, with a conical projection 8 lines thick fits exactly into the cavity of the socket, and with a plate *rr* 3 inches wide which rest on the plate *nn* of the first part, which is the same width.
>
> (Ohm, 1826a, p. 144)

At the time, this seemed incorrect. The maker and I agreed that the micrometer should be removable from the glass cylinder to enable the torsion wire to be easily accessed at the point where it attached to the micrometer. The maker proposed using some small pieces of leather to enable a tight but removable fit between the cylinder and the upper part of the instrument. Meanwhile, the upper part of the instrument, by which we meant the micrometer and the wood it sat on, was connected by three screws. On reflection, perhaps where we have used screws, Ohm used cement and we have misunderstood what the "upper plate" of the glass cylinder meant (see Figure 57). This misunderstanding is unlikely to have any meaningful effect on the functioning of the instrument, but does suggest that there are other plausible interpretations of how the top of the cylinder was made. The other reconstructions noted in this work variously used either brass or wood for the disk that sits immediately on top of the

cylinder, demonstrating some degree of variation in contemporary reworkings of the instrument.

**Torsion balance, marking up a scale**

Having agreed on a physical form for the micrometer, the next step was to agree on appropriate markings. Ohm wrote that there were one hundred divisions on the micrometer, which seems straightforward enough. However, the maker wanted to know whether the scale was likely to have had a Vernier scale as standard. He argued that Ohm might not have been considered this worth mentioning as it presents a way to improve the precision of a reading, but would not contradict Ohm's report of using one hundred divisions. Vernier's innovation predated Ohm's work by almost two centuries, so the knowledge was certainly available (Vernier, 1631). The instrument maker's opinion was that "from a manufacturing point of view [it was] no harder than making the scale without a Vernier" (Connelly, reflective diary, 6 January 2021).

Looking at Ohm's results, he sometimes reported halves or quarters of a division, but no more granularity. A Vernier scale would have easily made reading 10ths possible, whereas halves and quarters are possible by eye. Given this, and the fact that Ohm makes no mention of using a Vernier scale, I agreed with the maker that the upper piece would be marked up with one-hundred divisions, and a simple line to read against would be marked on the lower piece of the micrometer (Figure 57). This was the first of a number of conversations with the maker where his natural inclination was to make the instrument as good as possible, albeit within the bounds of Ohm's nineteenth century manufacturing constraints. In cases like this, my role in the discussion was to resist the urge to innovate where there was no evidence that Ohm had done so, and to keep returning to the source material to consider what evidence was available for or against particular techniques or designs.



**Figure 57.** The completed micrometer on top of the glass cylinder. (Image courtesy of Martin Connelly.)

One contribution by the maker which did make it into the final production of the instrument was to add a bit of texture to the upper knob for turning the micrometer. Grooves around the outer edge of the knob provide a bit of grip for the user. This was not specified in Ohm's text, nor does it have any substantial bearing on the functioning of the instrument for measuring. Therefore, as with the rest of the dimensions of the turning-knob, the maker had the freedom to design it as he thought appropriate and he chose to add this feature.

**Lower scale**

We continued to work down through the physical form of the instrument. The next piece to design was the lower scale *uu*, which sat immediately beneath the magnetised needle. Ohm provided some explanation of how this lower scale was used:

> The relationship between the needle and the "lahn" [torsion wire] was such that, in order to advance the needle one degree of the lower division, the turntable had to be turned ten parts. With the arrangement made, however, it was not difficult to see 1/40 of a lower grade and therefore to feel ¼ of an upper part perceptibly. At the same

> time, however, it is clear from this how a deviation of the point of rotation by not yet half a line can lead to errors of 20 or more parts.
>
> (Ohm, 1826a, p. 147)

There is not enough information here to know how the lower scale was spaced. Following conversation, the maker and I agreed that a sector of a circle divided into $^{1}/_{100}{}^{\text{ths}}$, as in the micrometer scale, was appropriate. The size of the sector was judged by eye, referring to the drawing in Ohm's paper, resulting in a lower scale of ±20 divisions (Figure 58).



**Figure 58.** Lower scale shown with roughly assembled upper parts of the instrument, including a test magnet hanging from a plastic strip in place of the "Goldlahn" torsion wire. (Image courtesy of Martin Connelly.)

**Base of the cylinder and loupe**

While there had been some discussions about the nature of the torsion thread and needle, as can be seen from the test pieces shown in Figure 58, the next decisions related to the production of a base for the cylinder to sit on. It was quickly agreed that wood was the most likely material for this, as it is cheap, easily workable and an electrical insulator. Ohm

described how on a protrusion *K* from the base, a loupe with a one-inch focal length was attached. This enabled the needle to be observed relative to the lower scale. In order to avoid any parallax, “the eye was always positioned during the observation so that the silk thread and the middle division of the arc of the degree coincided” (Ohm, 1826a, pp. 146-147). Getting this assembly right is very important for the operation of the instrument. Ohm explained that after the needle had been deflected by the electric current, the micrometer at the top of the instrument was twisted in the opposite direction until the brass point of the magnetised needle was aligned with the middle of the lower scale and the silk thread. The magnitude of the rotation could then be read off at the top of the instrument. Ensuring that the user could be sure that they were always looking in the same place was vital to know that the readings were consistent.

Procuring an appropriate loupe initially appeared very straightforward. A relatively cheap loupe with a one-inch focal length was purchased from an online seller which also came supplied with a clip-on “reticle”. The reticle, also sometimes known as a reticule or graticule, is a pattern of lines, for example a crosshair or graduated markings, which can be used with an optical device. In this case, the clip-on reticle was designed to enable a user to measure items viewed through the loupe at a high level of magnification. In practice, the one-inch focal length was impossible to use with Ohm’s setup: the end of the needle would need to be so close to the lens that the loupe would have to be embedded within the glass cylinder. Not only was this not practicable, it also was not reflected in Ohm’s diagram. Frustratingly, here was a place where Ohm gave a clear direction, which we followed, and which then proved to not be a usable solution.

As mentioned on p. 247, one source of ongoing discussion between me and the maker, was his urge to innovate and make the instrument as well as possible, in contrast to my wish to follow Ohm’s directions as far as possible and not innovate unnecessarily. In the case of the loupe, the maker produced an optical piece that can be focused by each user, and focused on different distances, with an internal vertical thread of plastic tape in place of Ohm’s piece of silk (Figure 59). The loupe can also be adjusted for height and position, though this is largely a function of the scrap brass the maker had available, rather than an immediate design choice. It is likely that Ohm or his maker, Herr Mauch, would have been similarly motivated by the materials immediately to hand, and it is impossible to know without access to detailed notes or the original instrument to what extent parts of the instrument were repurposed from other things. Regardless of the underlying causes, these design innovations are undoubtedly useful

for me and others as the user of the loupe, but also incorporate speculation well beyond Ohm's description of his instrument. Without a more simplistic loupe to compare with it is hard to know how much meaningful difference this makes to the operation of the instrument. It would be a relatively simple amendment to the instrument in the future if so desired, but my sense is that it has a trivial bearing on the operation of the instrument in practice.



**Figure 59.** The loupe viewed externally (left) and through the viewfinder to reveal the internal "reticle" in place of Ohm's silk threat (right). (Images courtesy of Martin Connelly.)

In conclusion to this section, it may be helpful to spend a bit of time considering Ohm's inclusion of a silk thread. Ohm wrote that:

> a straight line, drawn through the centre of the copper strip *bc*, with the centre line of the degree curve *uu* and a simple silk thread stretched perpendicularly in front of this curve, lay straight in one direction.
>
> (Ohm, 1826a, p. 146)

Figure 60 presents a top-down view of the instrument that I sketched to help make sense of this statement while in conversation with the maker. We were trying to understand the fundamental form of the instrument to develop the designs for the base of the cylinder, the location of the loupe and, therefore, the piece of silk thread.

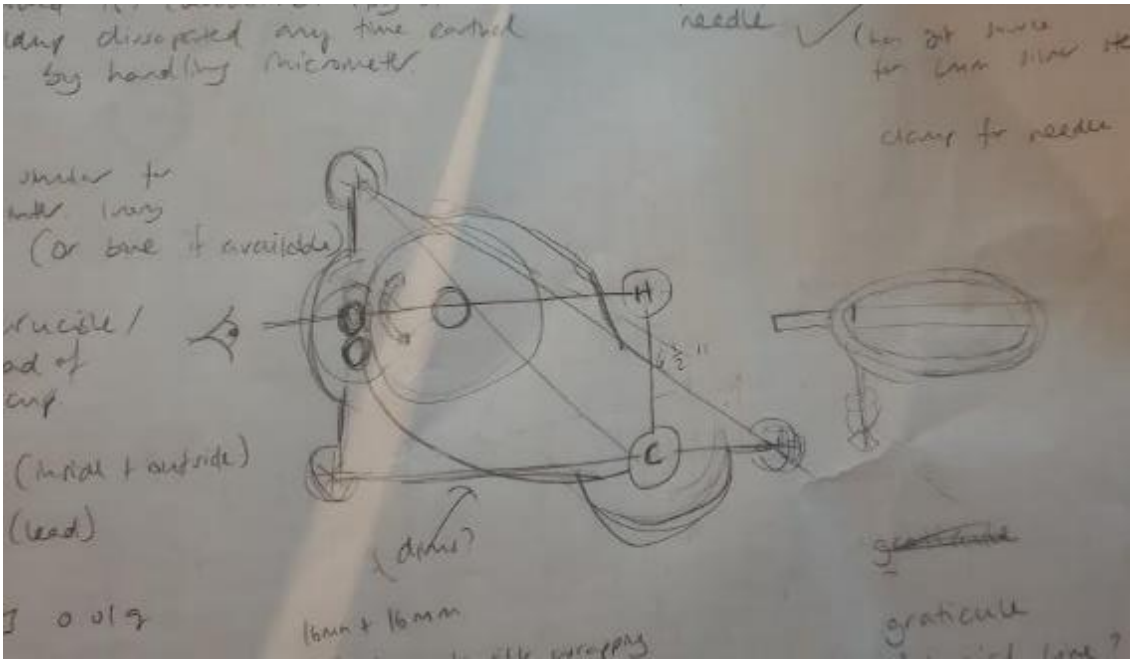



**Figure 60.** Sketch of top-down view of the instrument, made while in conversation with the instrument maker.

Perhaps biased by a knowledge of loupes with clip-on reticles, both the maker and I assumed the thread sat over the end of the loupe. The closer the thread is to the user, and therefore the bigger the gap between the silk thread and the tip of the needle, the greater the parallax effect will be and the greater precision is available to the user when attempting consistency in readings. I was surprised to see the Flensburg instrument had interpreted the piece of silk differently. It can be seen hanging vertically down from the top edge of the glass cylinder in Figure 61, and is presumably secured so it is taut when it is in use. Engels and Riess provide some further description of their interpretation, writing:

> Ohm used a thread stretched inside the torsion balance as a special device to avoid parallax errors. The thread runs centrally over the metal strip, runs under the scale and is deflected vertically upwards at its front edge (seen from the magnifying glass); the horizontal and the vertical part of the thread thus span a plane. When reading in the zero position, both the magnetic needle, the suspension point, the axis of rotation, the magnetic meridian and the zero point of the scale must lie in this plane.
>
> (Engels & Rieß, 2022, p. 97)

This solution would also work, and perhaps the addition of the silk running along the copper conductor also has the advantage of some extra indicators to ensure good calibration of the magnetised needle before observations begin. The disadvantage is that the setup described is

less sensitive to minor variations in the user's position than in a setup with the thread close to the end of the loupe. Ohm, however, is ambiguous about how he designed his instrument, making all of these discussions speculative.



**Figure 61.** Flensburg apparatus. (Image courtesy of Wolfgang Engels.)

### Copper elements

As can be seen in Figure 60, the basic form of the instrument was emerging in our designs, but there were still some details missing. Ohm wrote:

> A piece of bismuth *abb'a'* was cast in the shape of a square bracket was 9 lines wide and throughout 4 lines thick, the longer side *bb'* of 6½ inches and the shorter legs *ab*, *a'b'* each of 3½ inches. Using two screws, I attached copper strips *a'b'c'd'*, *abcd* to each of these legs, which were 9 lines wide, 1 line thick and, together, 28 inches long and bent in such a way that their free ends *cd*, *c'd'* were in two mercury-filled bowls *mm'* attached to the wooden frame. *fghi*, protruded when the whole device was on the frame.
>
> (Ohm, 1826a, p. 144)

The sketch shown below (Figure 62) presents the factual information presented in Ohm's prose diagrammatically. Many of the dimensions are known or relatively easy to estimate

from Ohm's text and drawing of the instrument, combined with the conversion table produced earlier (see p.235). The facts as given by Ohm can be summarised in a few brief points:

- Total length of copper = 28"
- *ab* and *a'b'* both 3½"
- diameter of the glass cylinder = 4½ ", which together with Ohm's diagram enables us to estimate the distance *cb* as 5".
- because we know the dimensions of the bismuth, we know that the distance between the outer corners of *b* and *b'* is 6½"
- We can assume, from looking at Ohm's diagrams, that *cd* and *c'd'* are both 3½" to match the given heights of *ab* and *a'b'*.

When these various lengths were subtracted from the total length of copper given by Ohm of 28", a length for the curved piece worked out as 9". Using Pythagoras's theorem, a simple right-angle triangle suggested that if the piece was not curved the length would work out at 8.2", so a slightly longer piece with a curve seemed to be a good solution. There is undoubtedly some speculation in this, but reassuringly the end result looked very similar to Ohm's diagram.

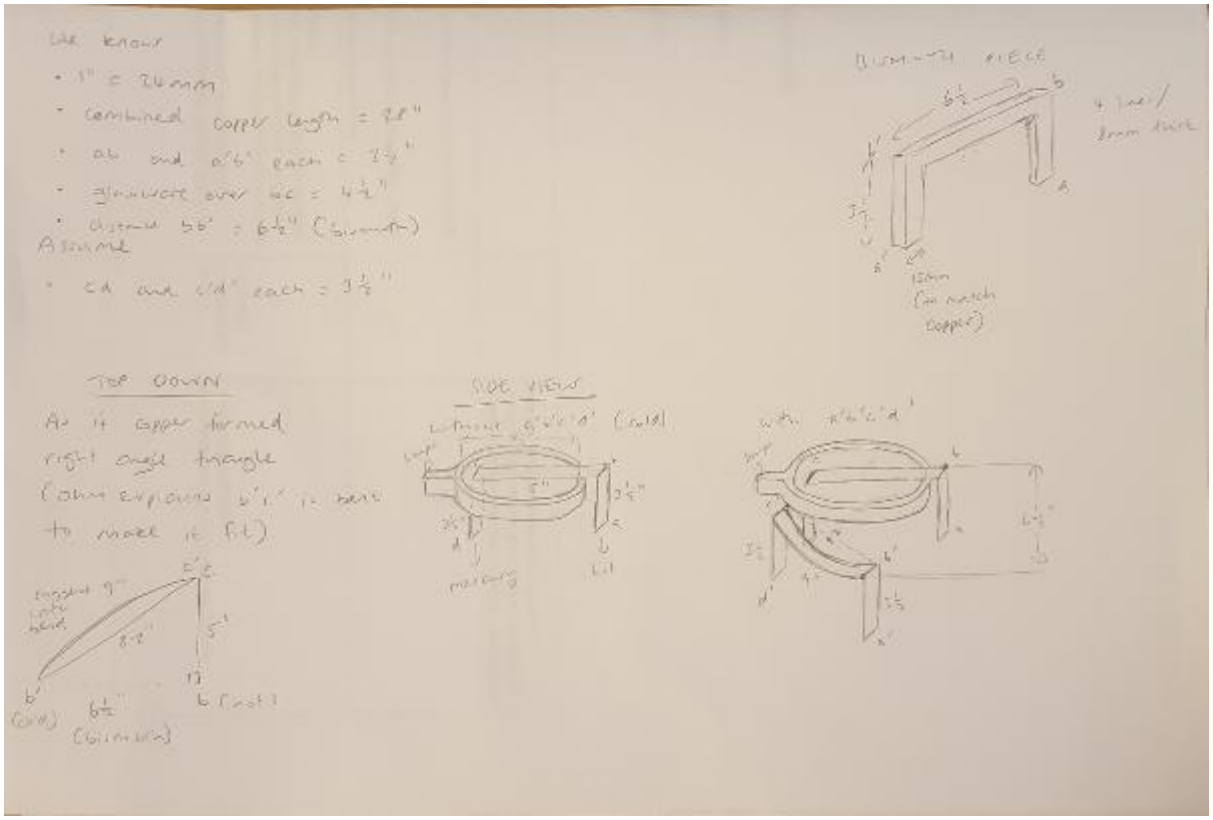


**Figure 62.** Notes from determining possible dimensions and form of copper pieces.

Having agreed a set of dimensions, the next challenge was to agree a means of production. Our first thought was to buy off-the-shelf copper bars. Ohm specified 9 lines wide (18mm) and 1 line thick (2mm), and altogether 28 inches long (672mm). The nearest ready-made options were 20mm x 3mm, or a choice of either 15mm or 20mm wide by 2mm thick. One option we explored was to order a 2mm wide piece and investigate machining it back. A piece of sample copper was purchased with this in mind, but proved not to be suitable: it was delivered directly to the maker who wrote back that "the copper you have supplied has rounded edges so that the contact face would be less than the maximum width. I would think in the region of 12mm wide, hard to tell where the flat and curve meet exactly" (personal correspondence, 9 July 2021). We discussed buying a wider piece and machining it back, but in the end the maker decided that purchasing a 2mm thick sheet and cutting it to size was a better option. This was not his first choice as "copper is sticky", though not impossible to machine (personal correspondence, 15 July 2021). Alongside discussions about the challenges of machining copper, we also discussed the way that copper work-hardens, and what is involved in annealing it so that it can be worked again. It is not necessary to understand the detail of this to follow the decision-making process here, so I have not

described copper's qualities, but it is worth noting that the material qualities of copper as a metal had a meaningful bearing on the techniques pursued in the production of the instrument.

The next challenge with the copper pieces was to bend them into shape after suitable lengths had been cut. The ninety-degree bends at, for example, *b'* and *b* were relatively straightforward: the metal was heated then the copper was bent around an anvil or other similar form. The curve between *b'* and *c'* was harder to achieve. In part this is due to trying to match Ohm's work, whereas presumably he and his maker simply did what worked for them without any strong pre-formed idea of the precise angles of bend they wanted. To produce the bend, the maker produced a shaped former to bend the copper around. In an initial drawing made in a CAD package, the maker suggested a simple arc, which resulted in a total length that was 14 mm short of the total length of copper we needed Figure 63.

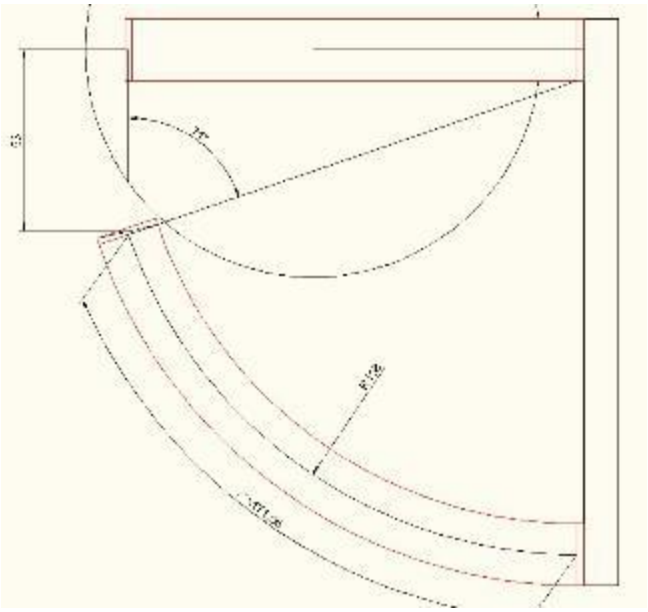


**Figure 63.** Initial drawing of the form of the bent copper. (Image courtesy of Martin Connelly.)

To amend the length to match Ohm's description, short straight sections were introduced at either end, increasing the length by 7mm (Figure 64).

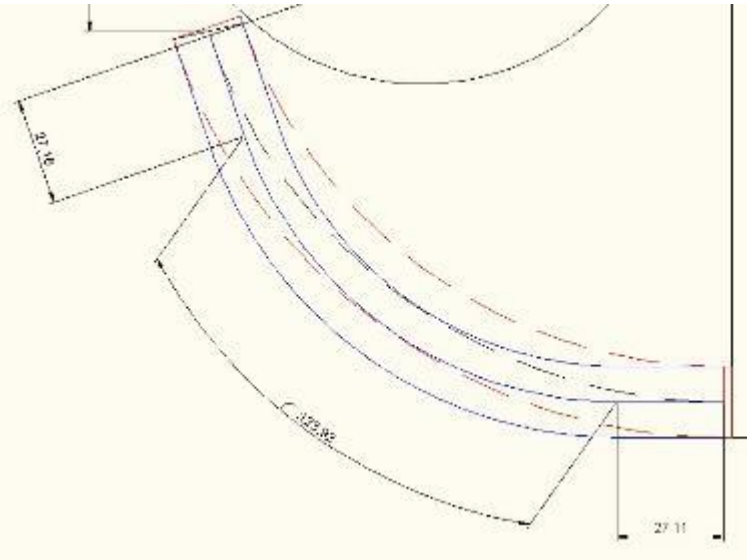


**Figure 64.** Revised drawing of the form of the bent copper. (Image courtesy of Martin Connelly.)

As we entered into detailed discussions it was apparent that there was some ambiguity about what Ohm was measuring when he gave his 28” figure for the length of the copper. My assumption was that it was the length before it was bent, but it might have been the outside edge measured with a tape afterwards, or some other variation. As Figure 65 shows, the maker applied the curve to the piece before the two ends were bent down at appropriate angles. It would have been harder to bend the piece into the curved shape after the legs had been bent down.



**Figure 65.** Work in progress image of bent copper element in production. (Image courtesy of Martin Connelly.)

We continued to return to the historical material in our discussions, for example looking closely at the drawing in Ohm's paper and noting that the presentation of the two vertical copper pieces *d* and *d'* is different (Figure 66). This reassured us that it was not problematic for the two feet in the mercury cups to be at different angles to one another.

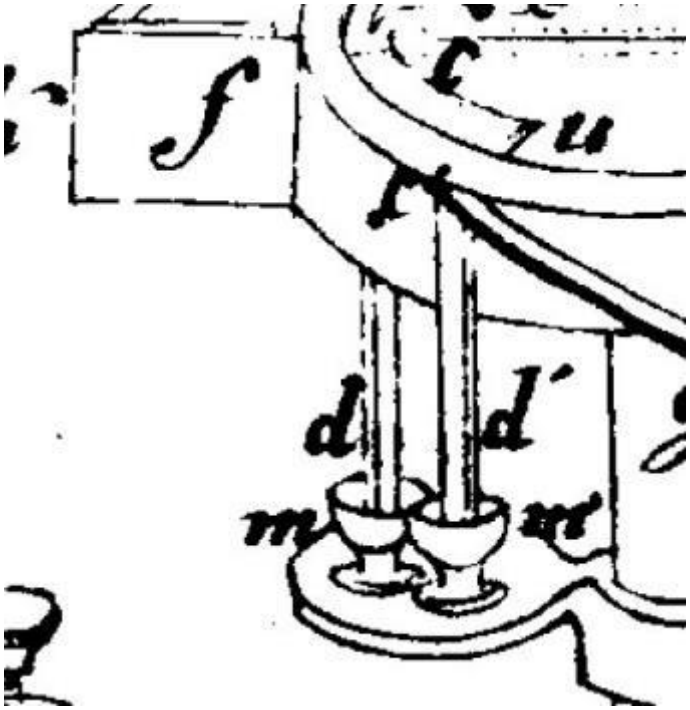



**Figure 66.** Detail of copper pieces *d* and *d'* from Ohm (1826a).

This interpretation of Ohm's design is in contrast to the Flensburg apparatus where the curved shape was cut from a piece of copper, rather than bent from a straight piece (Figure 67).



**Figure 67.** Diagram showing Flensburg interpretation of copper elements. Drawn following video call with Wolfgang Engels (29 September 2021).

One effect of that decision is that at point *c'*, where the copper is bent through ninety degrees in order to place the foot in the mercury cup, has to be cut into a less uniform shape. In the diagram above, it can be seen that the copper is narrower immediately before the bend. Therefore, the cross section of the copper piece is not uniform along its length in the Flensburg model. We had discounted this approach for two reasons: firstly, that it was a

wasteful approach to the materials as the arc has to be cut from a large sheet of copper, and consequently it seemed an unlikely approach at a time when copper was a less readily available commodity, and secondly that it would result in a variable cross section throughout the piece which seemed to run contrary to Ohm's intentions. Engels and Riess do not offer any commentary on this part of the apparatus in their paper, and Ohm does not offer clarity in his writing about which of these designs is more likely, so ultimately the decision is a matter of interpretation.

**Bismuth element**

Early on in this project, in February 2016, I undertook some tests with bismuth casting as part of a feasibility test for producing a reproduction of the instrument. Although the pieces produced at that time did not go on to form part of the final instrument, there were nevertheless useful lessons to learn. These included that bismuth pellets are easy to purchase, and non-toxic to work with. They can be heated to melting point over a kitchen gas burner and then poured into a mould without special equipment. The challenge to overcome was that if the bismuth cools too slowly it begins to form crystals. Incidentally, this feature of the metal meant that there were numerous guides on the internet to working with bismuth in a home workshop, though many of them focused on producing the right environment for forming decorative crystals, rather than preventing them. The solution we found for preventing crystallisation was to use a mould that could be heated up to close to the temperature of the liquid bismuth, so the mould and the bismuth cooled together evenly; secondly, we chose a metal as the material for the mould because it is a good conductor of heat and would cool relatively quickly.

Ohm gave clear dimensions for his bismuth piece, as being 6" wide across *b'b*, 3½" tall, with legs that were 9 lines wide and 4 lines thick. Returning to make the piece to this specification more than five years after a first foray into working with bismuth, the maker wrote:

> I'm making a mould for the bismuth. Most of it is steel and the central piece is aluminium. The thermal expansion of bismuth is less than aluminium but greater than steel. I think this will allow it to be extracted from the mould when set. All the pieces will be bolted together for ease of extraction.
>
> (personal correspondence, 11 October 2021)

This was accompanied by a photograph of the pieces (Figure 68). We do not have conclusive information about how the bismuth piece in Ohm's instrument was cast, however it is likely that it was cast in compacted sand as a more common casting method. The decision to use machined metals in our case was purely pragmatic. We weighed up the risk of using a less historic method against the difficulty of doing so, and opted for a method that was more readily available to us. If at a future date it was desirable to compare the two methods, the bismuth piece is relatively easy to swap out for an alternative. It should be mentioned that there is evidence that bismuth was used by tin foundries and some other trades at the time that Ohm was working, and it was likely to have been relatively easy for him or his maker to access (Blumhof, 1822).



**Figure 68.** Mould for bismuth element. (Image courtesy of Martin Connelly.)

After moulding, the piece was filed down to enable a smooth surface to be presented to the respective copper parts (Figure 69). Ohm wrote that he used two screws to attach the copper strips to the legs of the bismuth piece, though he did not specify what the screws were made from. It was, however, known from previous studies of thermocouples that the qualities of the screws were not important to the functioning of a thermocouple, provided the screw was the same temperature throughout, and that its temperature matched the portion of the thermocouple it was part of. The maker chose to use brass screws, but the decision was quite arbitrary beyond considering any possible magnetic effects.

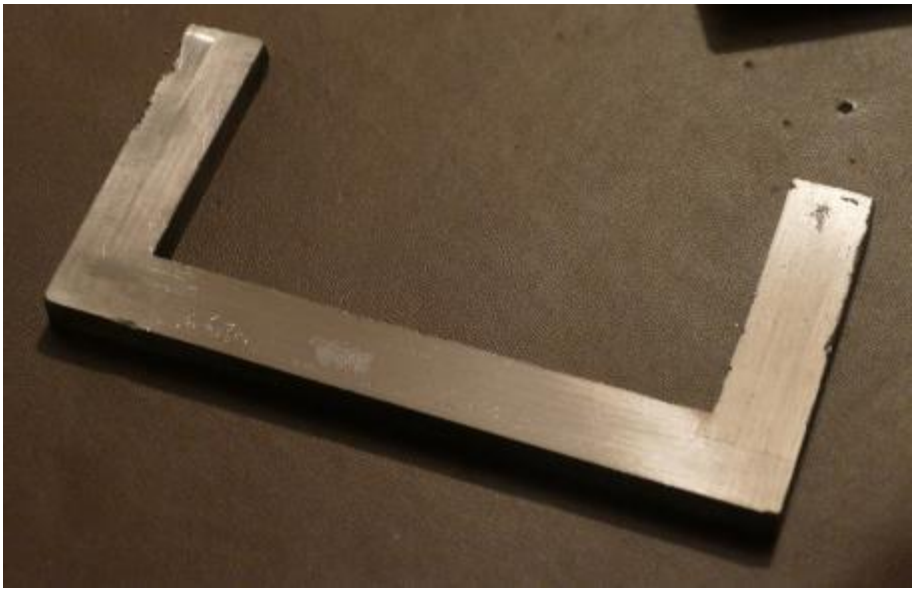


**Figure 69.** Photograph of cast bismuth piece. (Image courtesy of Martin Connelly.)

**Hot and cold vessels**

While the copper and bismuth pieces were being produced, the instrument was placed on a temporary stand. In order to know how tall to make the final stand, in the form of a wooden post, it was first necessary to agree the height and dimensions of the hot and cold vessels *A* and *B* which sit at either end of the thermocouple. There were very few dimensions provided by Ohm for these vessels, and they do not appear in the main drawing of the instrument in his paper. Instead, there was a diagram provided as a separate figure accompanying the paper which was used as a visual guide (Figure 70).

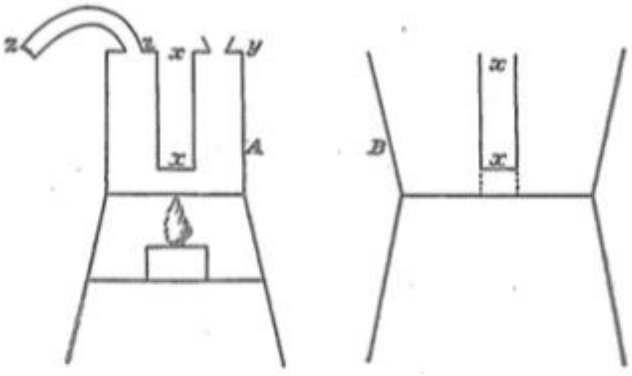



**Figure 70.** Copy of figure from Ohm's paper (1826a)

Ohm also provided a brief written description:

> Finally, in order to give the parts of the apparatus where bismuth and copper touched a constant difference in temperature, I had two vessels made of sheet metal, the diameters of which are shown on a larger scale ([Ohm's] fig. 2). Inside each had a cavity *xx*, open at the top, closed all around and intended to receive the parts from, *a'b'*. In one, *A*, water was constantly kept boiling; this therefore had an opening to be closed with cork at *y* in order to be able to bring water into the vessel, and on the other side a pipe *zz* to discharge the steam. Snow or fragmented ice was brought into the other vessel.
>
> (Ohm, 1826a, pp. 148-149)

The only dimensions that were available for the design of the two vessels relate to the height, width and depth of the thermocouple legs. Figure 71 is a photograph of my notebook workings to try and establish appropriate dimensions for each piece. As well as seeking to maintain visual similarity with Ohm's drawing, I also rounded measurements to the nearest inch or half-inch.

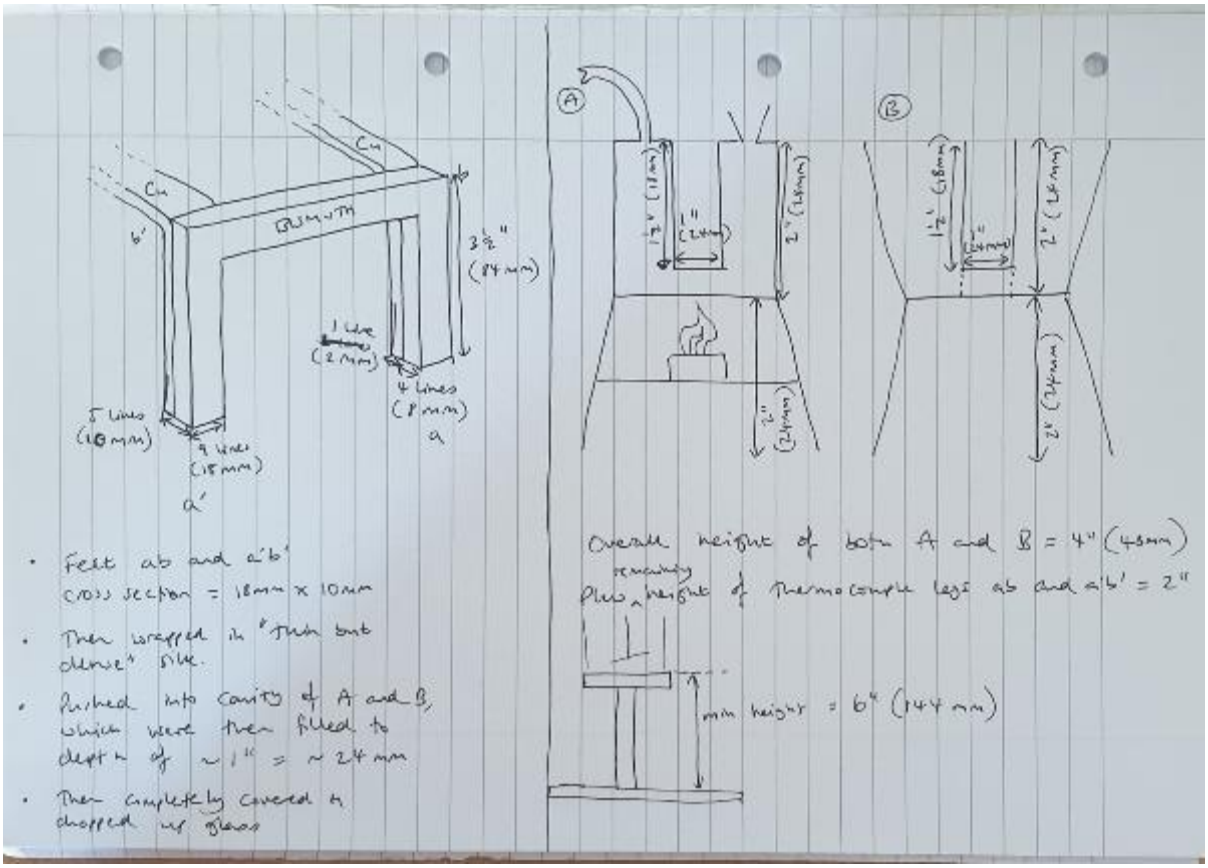



**Figure 71.** Sketch of hot and cold vessels with suggested dimensions.

Having agreed the basic dimensions, there were some nuanced decisions to make. First was choosing a material for the vessels. I had translated “Blech” as “sheet metal” as the material for these vessels. The word is ambiguous and could have also been viably translated as tin, brass or scrap. The maker opted to make them from copper which is readily available and not magnetic. Where sheet metal was required, it was made by flattening out a piece of tube, as pictured on the right of Figure 72. Another question was how the vessels would be placed into the apparatus or removed, which is necessary to empty water and lead shot from them at the end of an experiment. This led to the decision to make the vessels in two pieces: the top half was separate from the lower stand, and could be slid on or off the bottom piece, making it possible to remove the pieces from the instrument without having to raise or lower the thermocouple or base of the whole instrument.



**Figure 72.** Copper tube flattened as raw material for the hot and cold vessels. (Image courtesy of Martin Connelly.)

Another decision related to the shape of the internal cavities of the vessels. The drawing Ohm provided showed the vessels in cross section, which does not tell the viewer whether the cavities were circular or square. As can be seen in Figure 73, the makers of both the Science Museum apparatus and the Oldenburg apparatus chose to make square cavities for the feet of the thermocouple to sit in. The Deutsches Museum apparatus did not appear to include the vessels at all, based on the information available to me. The maker challenged this

interpretation, pointing out that in all cases there was an assumption that the main body of the vessels was circular, and that he had no reason to read the internal cavity any differently. He also suggested that making pipe was more routine, and a standard rounded pipe would have been a more natural thing to produce by hand than a pipe flattened to have sharp corners. Finally, we noted that Ohm's instructions include surrounding the feet of the thermocouple with lead shot and crushed glass, suggesting that a tight fit for the feet was not desirable.



**Figure 73.** Science Museum apparatus vessel and Oldenburg apparatus, right (Right image courtesy of Wolfgang Engels.)

Further decisions were quite practically-minded. How long should the downspout for topping up the hot vessel be? The maker's decision was to make it long enough that in use it sat below the upper water level. "Then if the water level falls below the end of it steam will escape giving an indication that a top up is required" (personal correspondence, 29 August 2021). Another decision by the maker was to fit the top of the hot vessel as a snug cap, but not soldered on, so that in the event of a build-up of pressure the top would pop off before there was any risk of a more dramatic explosion. The size of the funnel for topping up was largely driven by what looked right, rather than specifying measurements, as was the form of the overflow pipe on the hot vessel. The vessels are pictured near completion in Figure 74.



**Figure 74.** Work in progress picture of the hot and cold vessels. (Image courtesy of Martin Connelly.)

To complete the hot vessel, a burner was needed. The maker suggested looking into burners made for use with model steam engines. A good sized spirit burner made of brass was available second hand, which seemed the ideal solution. However, as discussed later, when the instrument began to be used, this seemingly trivial part of the design proved to be crucial to operating the instrument effectively.

**Baseboard**

The process of designing the hot and cold vessel included specifying the height of the central pillar which held the instrument up off its base. This led directly on to the form of the base piece that supported the central pillar and the two vessels. We assumed it was made of wood, although Ohm was not explicit. It seems highly unlikely to be any other material available at the time, and wooden bases were conventional in other scientific instruments contemporary to Ohm's.

Before starting work on the detailed design of the base, the maker and I had both independently come to similar conclusions. In an email he had written to me, "I wonder if what I thought of as a poorly drawn base is not instead an attempt to draw a base that allows

the hot and cold junctions to be positioned beyond it" (personal correspondence, 21 January 2021). Later, after a visit to the maker on 26 June, I wrote in my diary:

> Understanding the layout of the base is another challenge. Ohm's drawing in his paper makes it look like the base is poorly drawn. However, trying to work out the positions of the hot and cold vessels relative to the torsion balance suggests a triangular base with a right angle located where the drawing indicated… working out dimensions is the next job.
>
> (Connelly, reflective diary, 26 June 2021)

Having both decided that we did not trust the draughtsmanship in the drawing alongside Ohm's paper (Figure 46), the only way forward was to glean what information we could and start from scratch. We agreed that the image appeared to show a right-angled triangle, with the right angle somewhere beneath the curved piece of copper. As on other occasions, we also rounded to the nearest inch as a convention, to help us narrow the options. We considered that the torsion balance would be very top heavy due to the brass micrometer, which suggested that the base would be quite large.

With those basic rules in place, we took the pieces that had already been made and began laying them out on a large piece of paper in the approximate positions they would appear in the finished instrument Figure 75.

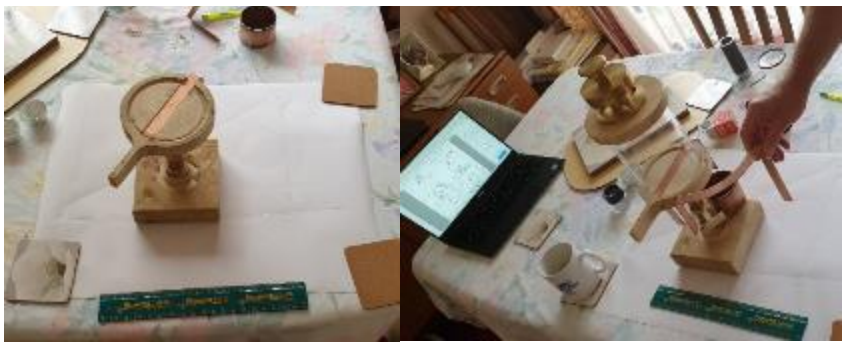


**Figure 75.** Laying out incomplete instrument to suggest appropriate dimensions for base

With the pieces in position, a rough sketch of the base was produced. Slight outward bulges in the triangle can be seen on Ohm's drawing, and these correspond with the locations of the two vessels and where the area above the mercury cups. Having sketched by eye, we then measured the different sides of the base, which was very close to an 18" by 12" right-angled

triangle, making the hypotenuse a little over 21” (taking an inch to equal 24mm, as per the conversion table on p.235).

The maker took the measurements and used them to produce a 3D model which we could look at before the wood was cut to size. Despite our initial scepticism about the quality of Ohm’s drawing, we had produced a base which, viewed from the correct angle, showed remarkable similarity to the drawing in Ohm’s paper (Figure 76). The main discrepancy is the position of the torsion balance which has been turned through several degrees. This may mean that the instrument had been pivoted slightly on its wooden support pillar before it was drawn, or perhaps the adjustment was made for clarity in the drawing. The side-by-side drawing also demonstrates quite clearly that Ohm’s drawing was not to scale, as the known dimensions are not in proportion. Nevertheless, the similarity was a surprise to us, after our initial sense that the base did not look right, so must have been poorly drawn. Perhaps Ohm and his maker undertook a similar process to agree the size and shape of the base needed for the instrument.

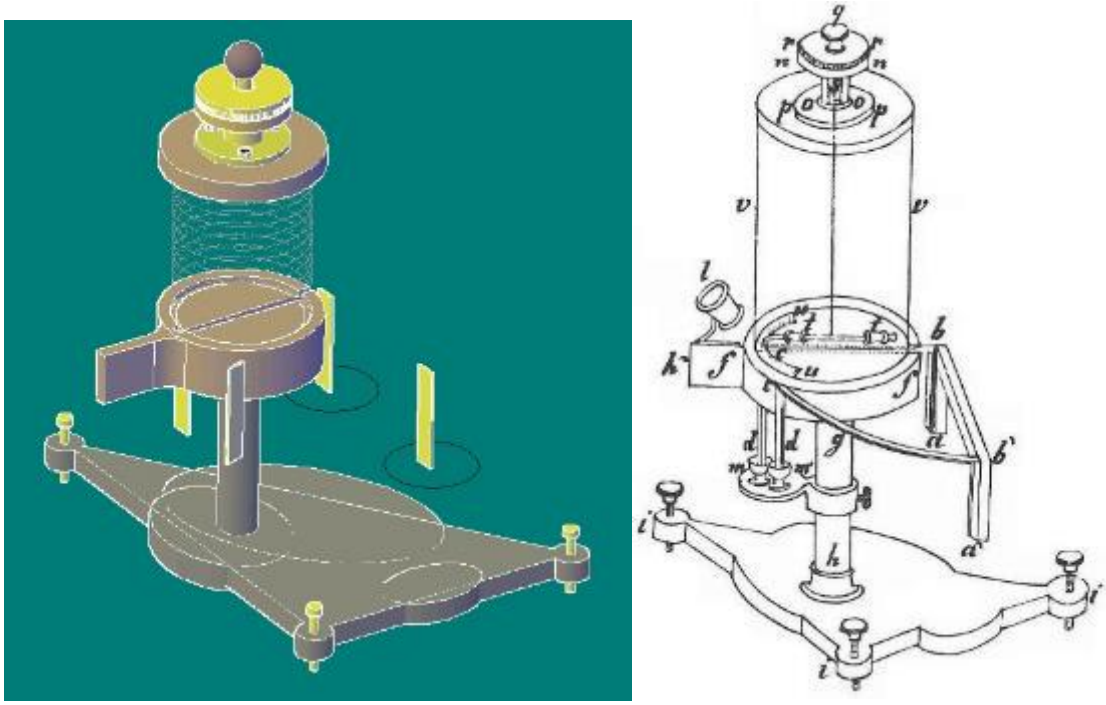


**Figure 76.** 3D drawing of reconstructed base alongside Ohm’s drawing of the instrument. (Left image courtesy of Martin Connelly, right image from Ohm 1826a.)

As suggested by Ohm’s drawing, the base is fitted with levelling screws at each corner. This proved to be essential for calibrating the instrument, as discussed later.

**Mercury cups**

Ohm used "two mercury-filled bowls, attached to the wooden frame" as a liquid conductor to join the variable copper lengths to the main apparatus (Ohm, 1826a, p. 144). Other than this very brief description, he did not provide any detail about the form of the bowls. In the diagram that accompanied the paper, they look a little like egg cups, and I adopted the language of calling them "egg cups" for a time, before realising this was an interpretive layer I had added, rather than something Ohm had written explicitly. I even searched for German egg cups from the period to see if there was a common style or design that was in use that might inform our apparatus, but this did not reveal any likely solutions. Once I had stopped using the phrase "egg cup", with scarce information available I instead sought similar vessels specifically for liquid conductors in museum collections. There were no obvious parallels that I could find by searching through various online catalogues. In a brief conversation with Professor Malcolm Longair, curator of the Cavendish Collection at the University of Cambridge, he mentioned that there were some examples of mercury being used as a liquid conductor in the collection. He believed they were basic test tubes that were sealed at the top, however in July 2021, when we spoke, he was unable to access the collection to confirm this as a result of restrictions due to Covid-19 (Connelly, reflective diary, 15 July 2021).

Without clear examples to follow, the maker and I instead discussed the options with some reference to the other existing reconstructions of the apparatus. We ruled out metal cups, as were used in the Oldenburg apparatus, as the cups themselves would be conductors and could easily touch and short circuit the instrument. We also ruled out wood, even though it would be easily manufactured to the shape Ohm indicates in his drawing. That was partly a pragmatic decision because it was unlikely to be easy to clean, particularly if we used a sticky metal alloy in place of mercury. Glass seemed unlikely too, as a search of early nineteenth-century German glassware did not reveal anything that appeared visually similar to Ohm's drawing. In the end, we settled on ceramic. It is an insulator, easily cleaned, easily manufactured in a range of forms, and relatively accessible. I was able to purchase two crucibles from Fisher Scientific, a supplier of laboratory equipment, each with a 25mL volume. They looked approximately correct and fitted the width of the copper pieces well.

The wooden stand for the crucibles is a relatively straightforward design. The maker paid special attention to the design of the clamp screw, and suggested making the ring out of brass, to avoid it becoming loose or coming away due to the grain causing weakness (personal correspondence, 31 July 2021). Ensuring the stability of the platform was an important part of

our risk assessment for the instrument, as mercury is toxic and must be handled with care. Any reasonable measure to avoid risk of it spilling was taken. The maker also fitted the clamp screw with a metal shoe so that the screw was tightening the shoe against the wood, and spreading the load, rather than screwing into the wood directly (Figure 77).



**Figure 77.** Brass ring for supporting the wooden platform for mercury cups showing the ring and separate metal shoe (left), the ring with the brass shoe fitted (centre) and the ring fitted on the wooden upright pillar (right). (Images courtesy of Martin Connelly.)

Despite the care we took to ensure that the instrument was as safe as possible when working with mercury, sourcing the metal for private use is not possible and explorations of working with it through the University's scientific laboratories proved unsuccessful. Following lengthy explorations with departments in the University of Cambridge together with our thorough reading of the various UK regulations regarding the use of mercury, we came to the conclusion that in the first instance we would make the instrument in such a way as it could be operated without mercury as a liquid conductor, and troubleshoot any issues that arose. The hope was that as a final step, once other issues had been ironed out, a means of working with mercury would be sought. We adopted this strategy because it was clear that working within a university laboratory setting would require a great deal of persuasion, and that effort would almost certainly be unsuccessful if the rest of the instrument was not already demonstrably functioning.

The first alternative to mercury we considered was to purchase some Galinstan. Galinstan melts at -19°C, and is liquid at normal room temperature. The alloy has good conductive properties, and, because it is non-toxic, is attractive as an alternative to mercury in a number of applications. However, it is sticky compared with mercury which, for example, means that

its use in glass capillaries for thermometry requires a coating of gallium oxide (Surmann & Zeyat, 2005). I sought the advice of Peter Heering and, on his recommendation, Wolfgang Engels, who has made several reconstructions of historic apparatus and was working on an interpretation of Ohm's apparatus at the time of our conversation. Engels described Galinstan as very sticky, and therefore requiring careful cleaning after every use, and quick to oxidise leading to an oxide layer forming on the surface. This is particularly important, he added, because it can make measurements tricky as the ends of the wire have to be cleaned and prepared first, and then the Galinstan poured in (personal communication, 29 September 2021). This was borne out in my tests with Galinstan; it was challenging to use effectively as a conductor, and very difficult to clean up. The biggest problem, however, is the oxide which is extremely sticky and viscous, and appears to not be a very good conductor. In principle it is possible to coat the Galinstan with a layer of hydrochloric acid to prevent oxidisation, but this causes a reaction and changes the makeup of the substance (Kim et al., 2013). The applications where this has been developed are typically very small scale, for example where Galinstan is used as a liquid conductor in microelectronics components. Other modifications to my operation of the instrument to try to make it work with Galinstan included placing the wires in the cups before filling the cup with Galinstan. This avoided the tips of the wire being pushed through the oxide layer, but it only worked for the first test wire in a series. To repeat this process would require emptying and refilling the cup between each test wire while also maintaining the temperature difference in the experiment. It became clear that although Galinstan is like mercury in that it is a liquid metal, there are also plenty of ways in which it is not at all like mercury. It was certainly not a demonstrably better substitute than a non-liquid conductor.

Even before my first test with Galinstan, we had decided to work with a mechanical connector. Initially as we iteratively developed the instrument, we were trying things out using basic modern-day technologies such as crocodile clips with good results. We decided to continue with this sort of technology in the first instance, anticipating a later switch to Galinstan and then to mercury if it could be achieved within health and safety regulations. To that end, the maker produced some connectors that we could slide over the copper feet and attach a wire to, to ensure a similarly good connection for each test wire. The metal wires push in to a fixed point and can then be tightened up firmly and evenly by screwing a cuff over the connector (Figure 78). Due to the problems described above, this became the principal means of making a connection during the experimental phase of working.



**Figure 78.** Connectors for test wires produced by the maker. The wire is inserted to a fixed point, then the outer cuff is tightened to grip the wire in place.

In thinking about the sorts of mechanical connector to incorporate into the instrument, it was clear that Ohm would not have anything like a crocodile clip, with the most widely reported pre-cursor to the crocodile clip being the “test-clip” invented and patented by Ralph Mueller almost a century after Ohm’s work (Mueller, 1920). There is, however, some evidence of screw-type connectors being used throughout the first half of the nineteenth century, for example in a copy of Ørsted’s apparatus for showing the effect of an electric current on a magnetic needle, believed to have been supplied by Watkins and Hill of Charing Cross in 1828 (Science Museum, inventory number 1983-478), or in telegraph equipment such as Cooke and Wheatstone’s 1838 four needle telegraph (Science Museum, inventory number 1876-1274) or William Reid’s 1846 single needle telegraph (Science Museum, inventory number 1949-323). This suggests a screw-type clamping conductor is an appropriate substitution. It should also be mentioned that other liquid metal conductors were not available to Ohm. Galinstan and similar alloys were developed by a range of people seeking better thermometry or thermo-conducting technologies at the end of the twentieth century (for an example see Speckbrock, Kamitz, Alt, & Schmitt, 2000, one of a wide range of applications

for similar patents around the year 2000). The disadvantage of using the screw-type connector is that it was not possible to place more than one conductor in the circuit at a time. Had the torsion balance been more sensitive, for example requiring the micrometer to be twisted through several complete revolutions as part of the observation, this may well have proved to be a problem. Achilles notes that on occasions where complete revolutions are made with the reconstruction he had access to, breaking the circuit causes substantial oscillation of the needle which means it takes a long while to return to rest (Achilles, 1996, pp. 90-91). However, the relative stability of the apparatus means that spending longer waiting for the needle to come to rest does not affect the overall result in the same way that it did in Ohm's 1825 apparatus, where the "loss of force" continued throughout the observation.

**Variable conductors**

Ohm described his test wires as being made of copper. He called them "1, 2, 3, 4, 5, 6, 7, 8, they were 2, 4, 6, 10, 18, 34, 66, 130 inches long respectively, and $\frac{7}{8}$ of a line thick" (Ohm, 1826a, p. 149).

A first step in reproducing the conducting wires was to convert the lengths given by Ohm to modern measurements, using the table introduced on p.235. The thickness of the wires, given by Ohm as $\frac{7}{8}$ of a line, works out as 1.75mm thick. The various lengths of the wires are presented below.

**Table 4.** Conversion table: Ohm's variable conductor lengths in millimetres.

| **Conductor number** | **Length in inches** | **Length in mm** |
|---|---|---|
| 1 | 2 | 48 |
| 2 | 4 | 96 |
| 3 | 6 | 144 |
| 4 | 10 | 240 |
| 5 | 18 | 432 |
| 6 | 35 | 840 |
| 7 | 66 | 1,584 |
| 8 | 130 | 3,120 |
| | **Total length required** | **6,504** |

Perhaps the most fundamental challenge in replicating Ohm's use of high purity copper, is that even pure copper in 1826 was almost certainly different to the pure copper available today. Although Ohm's wire isn't available for study, the close analysis of a piece of copper from 1837, which was likely to have been subject to similar manufacturing processes, was shown to be 14% less conductive than modern high quality copper wire. This is likely to be due to the metal being full of very small cavities due to oxygen readily being absorbed into liquid copper and then exsolving from the copper as it cools. This problem was not solved until the twentieth century when new manufacturing techniques became available (Mills, 2004, pp. 464-465). Copper production is highly industrialised today, which means that purchasing bare wire at standard sizes is very easy, but purchasing bare wire at a non-standard size, such as 1.75mm (or $\frac{7}{8}$ of a line) is much more challenging. The closest that could be readily found was 1.6mm thick. The 1.6mm diameter wire available today works out as 83.6% of the cross-sectional area of Ohm's 1.75mm diameter wire. The 16.4% reduction in surface area and therefore conductivity is relatively close to the approximately 14% reduction in conductivity likely in Ohm's wire compared to today's materials, due to differences in manufacturing techniques. Another inconsistency is that modern copper wire is machine manufactured, where the wire Ohm was using was likely to have been hand pulled with potential inconsistencies in cross section as a result of the manufacturing method. This was also observed in the 1837 wire studied by Mills, who speculated that a slightly non-circular cross section could have resulted from drawing a wire through a circular die that was not fixed exactly perpendicular to the direction of motion (Mills, 2004, p. 460). Taking all these things into consideration, some variation between what is achievable today and what Ohm worked with is to be expected. However, in understanding the role of the test wires in the experiment, the most important thing is to ensure that the test wires are produced and used as consistently as possible. I purchased the closest approximation to the wire Ohm was likely to have used. After that it was a simple task to measure, cut to length and file the ends of the wire. It took around an hour to prepare the wires for use.

Another challenge with managing and handling the wires was physically handling different lengths of wire, and ensuring that there were no short-circuits introduced by touching parts of the test wire together. While the short test wires could have relatively simply been bent into place and dropped into the mercury cups, the longest wire, at over 3m long, may have either required some winding for practical use or a workspace that enabled Ohm to drape the wire around a large area. This led to another question about how they might have been handled,

and whether the wires themselves were insulated. Ohm comments on the steps he took to ensure a consistent area of contact between his test wires and the mercury. He coated the tips of his wire conductors with resin before filing the tips away to reveal a smooth circular cross-section. I already had some violin rosin at home, and by heating it over a flame and dipping the tips of the wires into it I was able, with a small amount of practice, to get a good coating of reasonably consistent thickness (Figure 79). Once the resin had hardened it could be easily filed to reveal a good, clean cross section of copper wire. Some care was required not to chip off any additional resin, but recoating the wires was a quick job for the small number of pieces where the end did not file away smoothly.

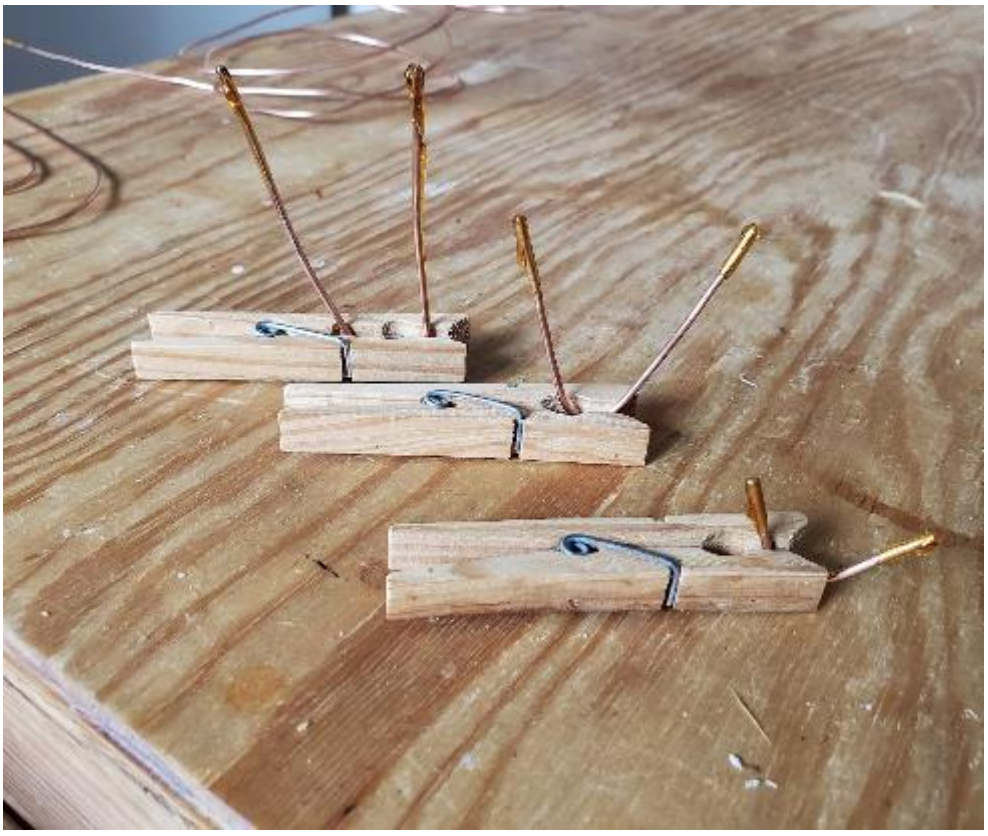


**Figure 79.** Wire conductors cooling after being dipped in molten violin rosin.

The problems I encountered when trying to make a good connection using Galinstan meant that the coated wires were not used, nevertheless it was helpful to see that it was a straightforward process with a readily available material. While Ohm was unlikely to have used pre-manufactured violin rosin, the essential materials are wood-resin based and were available to buy. Ohm had written to his father about working with a variety of resinous

materials in the early 1820s, showing that he had access to a range of different suitable sources of resin (Nürmberger, 2002, pp. 210-215).

Whether the rest of the wires were insulated at all is an open question. Conductor numbers 1-6 are relatively easy to handle without needing to worry about introducing any coils. Conductor 7 is a little unwieldy, but can be manipulated into a position where there are no short circuits and there is no substantial coiling. Conductor 8, however, at over 3m long is a challenge to handle without some support for the wire. I took the decision to coil the wire loosely around two pieces of fallen wooden branch from my garden. This makes them much easier to handle, and avoids unwanted contact between different parts of the wire. However, it does also introduce a coil into the system with the attendant risks of introducing an unwanted magnetic effect. Ohm does not explain what he did to work effectively with such a long test wire. He may have had a setup in his workspace that made it possible to drape the long piece of wire on furniture or pegs in a way that is not achievable for me in my workspace.

As for whether the rest of the wire was insulated, it seems unlikely. Mills is informative on the history of insulated copper wire, which was not commercially available as a commodity for electrical work until the 1830s. He notes that it was possible to purchase "silked wire", which was "fine silver, brass, or copper wires wrapped with silk that were used for tapestry and furnishing trimmings, although a textile core covered with metal foil was the more usual construction" (Mills, 2004, p. 455). Although Mills is writing particularly about the British context, he cites Poggendorff as a user of silked wire in an early galvanometer. Therefore, it is possible that Ohm used something like this, which would have made winding the copper without risking a short circuit a bit more straightforward. However, Ohm does not mention silked wire. This seems an unlikely omission given the detail he provided on the purity and preparation of the metal wires he used in his 1825 experiments for comparing the relative conductivities of different metals:

> Gold and silver for the above experiments were chemically pure, the other metals as they occur in trade. I had prepared the zinc wire used for the experiments from rolled zinc sheets, which, as is well known, contains many foreign substances within it; but this zinc wire as it was, was definitely above the brass in the ratio of 6:5.
>
> (Ohm, 1825c, pp. 246-247)

An alternative to purchasing silked wire might have been to insulate the wires by hand, as Mills describes Faraday having done using calico and shellac (Mills, 2004, p. 454). This

would have been labour intensive, and as Ohm commented readily on how time-pressured his work was it seems unlikely that this took place without Ohm reporting on it. Using resin, as described for the tips, would not have been a good solution for lengths of wire that were being bent and handled. It is very brittle, and would not withstand any flexing of the wire.

In summary, then, the pure copper conducting wires which at first glance are very straightforward to reproduce, are actually very hard to replicate faithfully. Commodity networks, standardisation, changing manufacturing techniques and under-described experimental practice come together to force almost every aspect of the replication into some form of compromise.

**Torsion needle**

The maker and I had long conversations about the qualities of the torsion wire, but were left with an enormous amount of uncertainty. Ohm provides some basic information:

> The magnetic needle *tt* is made of 0.8 line thick steel wire and is not a full 2 inches long, its two ends sit in cylinder-shaped pieces of ivory, one of which has a delicately pointed, slightly bent, brass wire inside. This brass tip, which serves as a pointer, lies just above the brass arc…
>
> (Ohm, 1826a, p. 145)

Ohm's diagram provides a little more detail about the shape of the needle (Figure 80). The ivory pieces are likely to have been weights, though it is unclear.

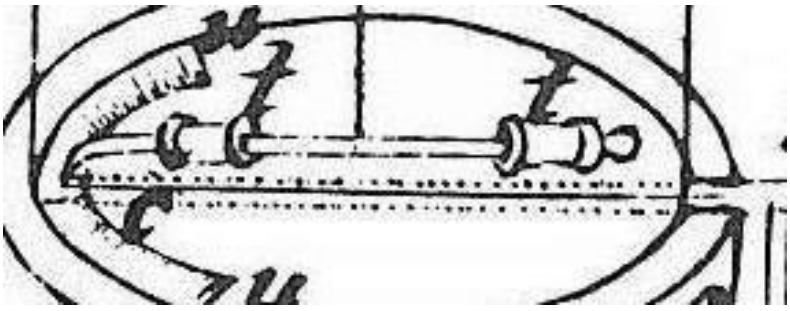


**Figure 80.** Detail of the torsion needle from Ohm's diagram. (Ohm 1826a)

What this description does not reveal is the strength of the magnet or its distance from the copper conductor when suspended. The main controls on the sensitivity of the instrument are

how strongly the needle is magnetised, how far it is from the copper conductor beneath it, and how easily the torsion wire twists.

The piece of steel we used was cut from a bicycle spoke, which happened to be the right diameter. It was magnetised by hand, and tested semi-regularly to check that it retained its magnetism. Instead of ivory, which can no longer be legally purchased, some white plastic dowel of an appropriate diameter was used for the pieces at the tips. This substitution might have been problematic in an electrostatic setup, but we could not identify any likely problems with using plastic in this electrodynamic setup.

The maker produced a small cuff to clip over the needle, which could be clipped over the top of the torsion wire to keep it in place, and slid along the length of the needle to help balance it. Where the torsion wire was very thin, it was necessary to pack the clip with paper or leather to enable it to stay in place. Blu-Tac also worked well, but was clearly not something Ohm would have had access to (Figure 81).




**Figure 81.** Detail of the torsion needle, showing torsion thread held in place by a cuff with a piece of Blu-Tac to pack the torsion thread in place.

**Torsion wire**

As discussed in the previous section, the torsion wire together with the magnetised needle determines the sensitivity of the instrument. It is a crucial part of the apparatus, and the one that we found the hardest to develop. Ohm describes the torsion wire with the needle:

> The needle prepared in this way is carried by a 5-inch-long strip of gold 'lahn', which is attached to the rotating micrometer at the exact centre of the rotation. These band-shaped metal strips are used after my experience in experiments with the torsion balance. They are far more sensitive than cylinder-shaped wires. The lahn that I use on my rotating balance meets all the requirements for the attempts at deflection, despite its shortness, which is so desirable for many reasons, so that the needle takes its original position again when the tension is released even after the lahn has undergone a tension of more than three full revolutions. After every attempt I was able to calmly observe the needle in order to be convinced that the apparatus had undergone absolutely no change.
>
> (Ohm, 1826a, pp. 145-146)

The particular type of wire he used was called Goldlahn, which appears to have been a commodity available readily available for purchase in nineteenth century Germany. As my reflective diary records, finding Goldlahn today proved to be impossible.

> I also spent time researching "Goldlahn", the flattened gold ribbon Ohm used in his experiment. Lahn is described in the German dictionary Duden as "flat rolled metal wire made of gold, silver, copper etc… from the French lame." while the Pons German-English dictionary gives the definition "pressed golden wire [often used in the Middle Ages]", (content in square brackets copied from the original).
>
> Despite… searching sites such as the University of Leiden's textile research centre website, I had little luck finding out exactly what Goldlahn is or how it is made. I had been hoping for a good English translation to work from, but couldn't find anything suitable.
>
> A search of eBay did find some examples available to purchase, with little to no provenance or information (and a high price tag). Described as "vintage, from the 1940s" the lahn isn't the right time period. However, it does predate the likely use of plastics in an embroidery thread and seems like a good compromise in the absence of a ready alternative.
>
> (Connelly, reflective diary, 19 January 2021)

Some "1940s vintage Goldlahn" was purchased from a German online shop, however the product appeared to have a plastic coating on it providing structure to what appeared to be a metal foil. The 1940s date seemed unlikely on close looking at the material. Sourcing a piece of flattened metal as described, without reference to "Goldlahn", also proved to be a

challenge. When speaking with Wolfgang Engels about the Flensburg apparatus, I specifically asked about the torsion wire and recorded the following notes on the conversation in my reflective diary.

> [Engels] said the original Oldenburg maker had been in touch with a clockmaking company who had sent him some samples of spring ribbon. He'd tried through trial and error until he found one with the right sort of torsion moment – not too stiff, but not so flexible that the needle turns repeatedly and rises up in the process causing other effects.
>
> (Connelly, reflective diary, 29 September 2021)

This suggests that when the original Oldenburg instrument was made in the 1990s, Goldlahn was no longer a readily available commodity and an alternative was found. It also indicates how informal networks contribute to the building of bespoke instruments. In the case of the Connelly apparatus, a similar informal relationship offered a potential solution. A family friend who repairs historical music boxes as a hobby provided some flat steel wire used for damping music box comb teeth, which was 0.06mm thick and a fraction of a millimetre wide (reflective diary, 28 November 2021). The maker took delivery of the wire and tested it, concluding that the steel was too stiff in torsion, whereas some plastic lacing tape he had was better: "You have to turn the top through 65% to get it to swing around back to North South just from Earth's magnetic field" (correspondence from maker, 24 December 2021). The plastic lacing tape was a woven construction, around 3mm wide, and was installed in the instrument when the maker delivered it to me.

As the plastic lacing tape was so clearly different to what Ohm had used, and in my view unlikely to behave in a similar way given the woven fibres it was made from, I spent some time visiting jewellers in Cambridge and asking for advice. None of the craftspeople I spoke to recognised either the word "lahn" or my description of it. It seemed evident from this that it is not a commodity that can be purchased. One jeweller suggested that a piece of wire might be hammered flat, but they would want me to specify a thickness and purity of gold first. I had neither of these facts at my disposal, so at best could make an educated guess.

As I was impatient to begin my experiments, I chose to make use of the supposedly 1940s Goldlahn as the closest material I had been able to purchase. It is very slender and twists easily, whereas all the pure metal solutions I had been able to source or observe had been very stiff and resistant to any twisting. In the course of reworking the experiment it became clear that the torsion thread I was using was limiting the sensitivity of the experiment. I

wanted to see how the balance operated with something much finer in place, and used a piece of my own hair for the torsion thread. This resulted in a much more sensitive instrument, though still less sensitive than Ohm described.

**Insulating the thermocouple**

The final part of the instrument to be developed before its operation was the area surrounding the feet of the thermocouple. These had to be in good enough contact with boiling water or ice bath to be heated or cooled effectively, but they also had to be electrically insulated. Ohm wrote:

> The parts *a'b'* were sewn around with thin but tight silk, then pushed into the cavity *xx* and these were finally filled up to a height of about an inch with small shot and then completely covered with chopped up glass. In this way, all points of contact between bismuth and copper were within the lead-filled cavity, which was a good conductor of heat, and the glass cap protected the space from a rapid change in temperature due to the surrounding air.
>
> (Ohm, 1826a, pp. 148-149)

On paper, these instructions sound simple enough. My first task was to purchase some lead shot, which proved to be more challenging than anticipated as lead is no longer easily available due to its toxicity. Nevertheless, I was able to purchase some 1mm diameter lead shot from a science supplier. Ohm's suggestion of covering the shot with chopped up glass was less straightforward. Chopped glass could mean a wide range of different things from something resembling coarse sand to sizable chunks of glass plate. There is no way of knowing which of these options Ohm meant, and keyword searches and reading of other experiments by Ohm's contemporaries did not reveal any clues. I chose to purchase some decorative granulated glass pieces. They were small enough to sprinkle over the top of the lead shot, and big enough to handle individually. Another factor was that I knew they would be smoothed off enough that I would not risk cutting myself while working with them.

Finally, Ohm described the parts as being sewn around with thin silk. I was able to purchase some offcuts of silk quite cheaply, and tested them by feel to see if they allowed me to feel temperature through the silk. This worked well and I set about sewing two silk socks for the feet of the thermocouple. I sought to minimise seams at the bottom of the socks, and used a

hand-sewn blanket stitch to prevent fraying edges. The finished socks had a bit of stretch in them which allowed them to be carefully pulled up over the thermocouple feet, where they fitted snuggly and stayed in place (Figure 82).

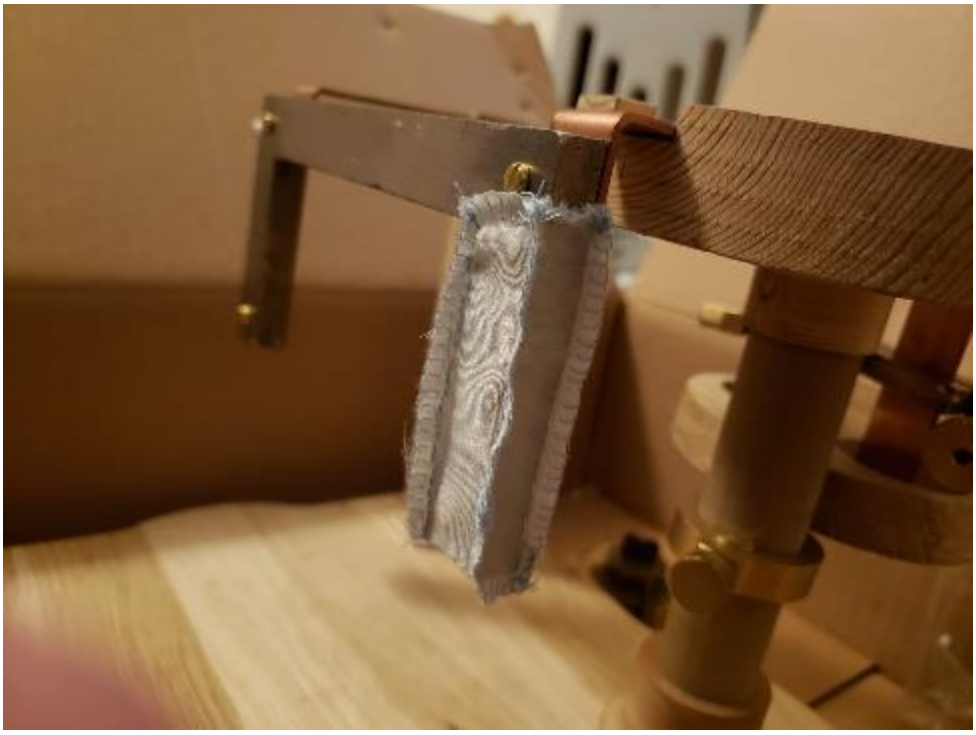


**Figure 82.** Silk sock on one of the thermocouple feet.

## Assembling the instrument for use

The maker delivered an almost complete instrument to me in early 2022. I then made the silk socks and cut the test wires to length. I operated the instrument alone, usually with it placed on top of a low cabinet in the conservatory of my house. The conservatory benefits from a solid tiled floor which does not discernibly vibrate when somebody walks across it. However, it also suffered from being excessively hot in the summer, leading me on one occasion to operate the instrument on my dining table. This was a bit more spacious, and the height was more comfortable to work at, however it was easy to knock the table causing vibrations. This setup was also prone to vibrations from me walking around and making floorboards bounce, and from heavy traffic passing by outside the house.

On my first attempt to work with the instrument, the process was made more time consuming by my failure to successfully think through all the steps in advance. This was anticipated; I

had chosen to do a test run of operating the instrument as a stand-alone exercise, not expecting to make any observations on that first occasion.

**Attaching and calibrating the magnetised torsion needle**

The first task when setting up the instrument was to swap the torsion wire from the plastic tape the maker had installed, replacing it with the "vintage" gold lahn I had purchased. Unscrewing the clamp at the top of the instrument and replacing it with an alternative is straightforward, if a bit fiddly. There is a knack to connecting the needle to the torsion wire, and with a bit of practice I developed my preferred method which involved first turning the whole torsion balance upside down, supporting the heavy brass micrometer between my knees, and clipping the needle in place so that it was approximately centred. Next, I carefully turned the balance the right way up again, supporting the needle against the inside of the glass cylinder so it did not move around excessively. Finally, I perched the glass cylinder over the edge of a table so that I could reach inside and gently move the clip left or right to balance the needle, or carefully tug on the excess piece of torsion wire to shorten the overall length (Figure 83). This was a tricky manoeuvre as the torsion balance is very top heavy due to the large brass micrometer on top, and the needle must sit inside the cylinder which means the user has to reach inside to manipulate it. There is not enough space inside the cylinder to put two hands inside, so I found myself stretching my fingers into peculiar positions and trying to adjust the needle and thread with one hand. More than any other experience in putting together the apparatus, this exercise highlighted the development of physical technique. My initial efforts were clumsy and resulted in several snapped torsion wires and a great deal of frustration as I tried to level the needle so it was horizontal. However, I am now able to swap wires in and out relatively quickly, although balancing the needle so that it sits horizontally can take some trial and error.

My reason for articulating the procedures above is to demonstrate that there was a quick development and refinement of a method of physically interacting with the apparatus, which after just a few iterations became easy to repeat. Unsurprisingly, Ohm did not describe his method for attaching and levelling the needle as it does not aid understanding of how the instrument works, and in likelihood each person to try setting up the apparatus would find a slightly different way that works for them. Nevertheless, Ohm surely had to find his own solution to affixing the needle, with the same or similar constraints to the ones I faced. An

alternative might have been to make the upper wooden plate removable, however we understood from Ohm's text that it was "cemented" to the glass. As noted previously, there is some ambiguity within the text, and with hindsight I increasingly think we may have misunderstood which parts were cemented together and which were free standing.




**Figure 83.** Balancing the torsion balance and adjusting the needle until it was horizontal.

The next task was to place the torsion balance on the instrument and let it settle in line with the Earth's magnetic field. The needle must start in a position where the micrometer is at zero, there are no twists in the torsion wire and the needle is in line with the magnetic meridian. One benefit of the flattened wire, or *lahn* used for the torsion wire was that it was very easy to see whether it had a twist in it or not. Because the needle oscillated back and forth every time the instrument or micrometer was moved, this alignment process is often time-consuming. As well as ensuring that the needle is aligned with the magnetic meridian, it must also be horizontal and centred lengthways relative to the copper conductor. The maker

had included adjustable feet on the base of the instrument, following Ohm's drawing, and it quickly became clear how useful they are in calibrating the instrument's starting position. Screwing or unscrewing the feet enabled me to position the needle over the copper conductors precisely. Meanwhile, the loupe enabled me to observe the oscillation of the needle closely and more easily judge when it was oscillating over the "zero" mark as I waited for its movement to dampen.

**Working with the hot and cold vessels**

Establishing the temperature difference in the thermocouple involved putting the hot and cold vessels in position. The thermocouple feet were placed in the cavities of the vessels, which were then filled with lead shot and then covered over with chopped glass. It proved to be challenging to get the shot in the opening, there was not enough space to empty the small scoop I was using from all angles, and the seams on the socks made it hard to push the shot around from the more accessible side. With some perseverance, and a small flat tool, the lead shot was carefully packed in around the thermocouple feet. It was apparent that this was not a trivial part of setting up the instrument. It was best to do it well once and then leave it for as long as possible rather than dismantle the instrument between uses. Placing the chopped glass over the top of the lead was also a challenge for the same reasons. Having been through this process, it seems likely that Ohm's cavities, where the lead shot and glass were packed, were bigger than in our design. The chopped glass hardly seems necessary with such a small amount of surface area as in the Connelly apparatus, and physically getting it and the lead into place is very awkward with our setup.

With the thermocouple feet firmly packed into place I filled up the cold vessel with ice and the hot vessel with recently boiled water, then placed the spirit burner under the hot water. This very quickly revealed a number of things. The first, and most obvious, was that I should have been more careful in reading Ohm's instructions. I had placed the vessels the wrong way around, with the result that the flame from the burner was underneath the torsion balance. I was concerned about setting the wooden platform it sits on alight with a lively flame from the burner, and was also causing the glass cylinder to become sooty so that it was increasingly difficult to see through it (Figure 84).



**Figure 84.** First test run operating the instrument, hot and cold vessels the wrong way around (16 May 2022)

This first run also revealed that an overfull hot vessel would overrun and pour hot water onto the work surface and the burner. The burner itself was also a cause for concern. After a few minutes it appeared that the fuel reached its flashpoint where the gases from the fuel ignited, leading to a large and very hot flame which burnt itself out very quickly as the fuel ran out. After this initial attempt to set up the instrument a few changes were made. The hot and cold vessels were swapped to their correct locations, a piece of slate from the garden was inserted underneath the hot vessel to mitigate against the risk of the wooden base igniting, and an alternative burner and an alternative fuel were ordered. I had initially been using a fuel for camping stoves which was very sooty, instead I purchased some methylated spirits which burns less hot and is easier to control, though requires a bit more safety management due to its flammability and barely visible flame.

At the end of this test run, I dismantled the apparatus. The chopped glass and lead shot were all mixed up together, which caused me to spend some time with tweezers separating the small lead balls from the glass. I found myself wondering what Ohm did. There were not legal requirements to dispose of lead carefully when Ohm was working, so he may have simply disposed of the small amount of material generated, though this would be a costly

solution. Alternatively, he may have used a much larger piece of glass that could be more easily removed from the apparatus before dismantling it. In my case, I decided to forgo the glass in future iterations of the experiment. The surface area for heat to be lost from is very small in the Connelly apparatus, and not including it seemed unlikely to cause major difficulties for the experiment.

*Attaching the conducting wires*

In the initial tests with the instrument, I tried both methods of connecting the wires that we had developed. To begin with, as the wire tips were already coated with resin, I tried using Galinstan as a liquid conductor. I had purchased a small bottle of Galinstan, and this was the first time I had opened it. When I initially tipped it up to pour some into the first crucible, it did not come out. Using a small splint, I ascertained that the surface of the metal was very sticky and had formed a seal in the neck of the bottle. Looking at the bottle side-on, I could see a distinct line around a quarter of the way down the bottle between the sticky layer, which I presume to be an oxide layer, and what proved to be the liquid metal beneath it. I was able to break the seal caused by the oxide and pour the metal into the crucibles, sharing it evenly between them. By raising and lowering the platform that the crucibles sit on, I could place the bottom of the two copper feet in the metal, though they were less well submerged than I had hoped due to the reduced quantity of usable liquid metal. I was able to place the test wires into the crucibles relatively easily, though I was concerned with the longer unwieldy wires about how easy it would be to accidentally tip one over and spill Galinstan over my work area. The biggest problem, however, was that Galinstan in the crucibles was oxidising quickly. The sticky layer on top of the metal made it hard to submerge the test wires and make a good connection. Furthermore, as it oxidised the amount of usable Galinstan reduced. Not only were my tests not particularly effective, but cleaning up the apparatus afterwards was very time consuming.

The other method of connecting wires was to clamp them in place using the specially made clamps described previously. A butterfly screw at the top of each connector makes it possible to change the angle between them, which proved to be essential for the shorter wires. Even with this, it was very difficult to insert Conductor 1, and due to its short length I was not sure that it was reliably inserted to the same distance each time. Consequently, on some of my

series of observations I left Conductor 1 out, and started with Conductor 2 instead. In comparison to working with Galinstan, this connection method was quick and easy.

## Making observations

### Series 1: 3 June 2022, morning

Having set up the instrument, calibrated the needle and filled the cold and hot vessels with ice and freshly boiled water respectively, I lit the new, smaller burner, filled with butane-based fuel, and placed it beneath the hot vessel. It took around five minutes for the water in the hot vessel to begin hissing followed shortly by audible bubbling. It was also possible to look into the spout and see bubbles.

It was hard to tell whether the cold end was still getting colder, however it felt cold to the touch. Around half an hour after first lighting the burner, I placed Conductor 2 in the circuit. There was a quick and visible deflection of the needle when the circuit was connected. It then took around five minutes to return the needle to the "zero" position. I could quickly get the needle to approximately the right position, but then time was required for the oscillation of the needle to dampen. It was possible to take a preliminary reading while the needle was still oscillating, as looking at the lower scale through the optic enabled me to judge when the needle was oscillating evenly on either side of the zero mark. I then left the instrument for a few minutes to dampen completely before confirming the reading. For this first observation, my preliminary reading was also my final recorded reading because immediately afterwards the small burner ran out of fuel.

Finding a suitable burner proved to be my biggest challenge in operating the instrument. When the burners I was using ran out of fuel they needed to be cooled before they could be refilled. Meanwhile, the hot end of the thermocouple was also cooling. I filled both the older, large burner and the new, small burner with the intention of swapping them in and out as needed. The ferocity of the burners also contributed to the ice melting, which meant regular trips to the freezer. The rate at which my experiment consumed ice led me to wonder about Ohm's access to ice throughout his experiment, as I worked my way through more than I had anticipated on this first run of observations. His experiments took place in winter and he reported using snow, which suggests that he could readily go outside and collect more snow as needed.

I continued this run of observations, regularly refilling the burner, topping up the ice, topping up the hot water, and hoping that the thermocouple was at something like a stable temperature throughout. I decided to stop the series before taking a measurement with conductor 8 as there were clear problems with the setup that I felt could be rectified for a fresh series of observations.

The final thing to note is that it quickly became apparent that the micrometer scale ran in the wrong direction. A deflection of 90° would be read as 75, rather than 25 (out of 100). This is easily adjusted for by subtracting every reading from 100. The tabulated results presented in the following tables give the reading as observed, however the graphs present the adjusted reading.

My final measurements were as follows:

**Table 5.** Series 1: 3 June 2022, morning.

| Conductor number | Length (inches) | Reading | Notes |
|---|---|---|---|
| 1 | 2 | - | No reading taken, could not easily fit conductor into connectors |
| 2 | 4 | 64 ½ | |
| 3 | 6 | 72 ¼ | |
| 4 | 10 | 78 | |
| 5 | 18 | 83 ½ | |
| 6 | 34 | 89 ¾ | |
| 7 | 66 | 90 | |
| 8 | 130 | - | |

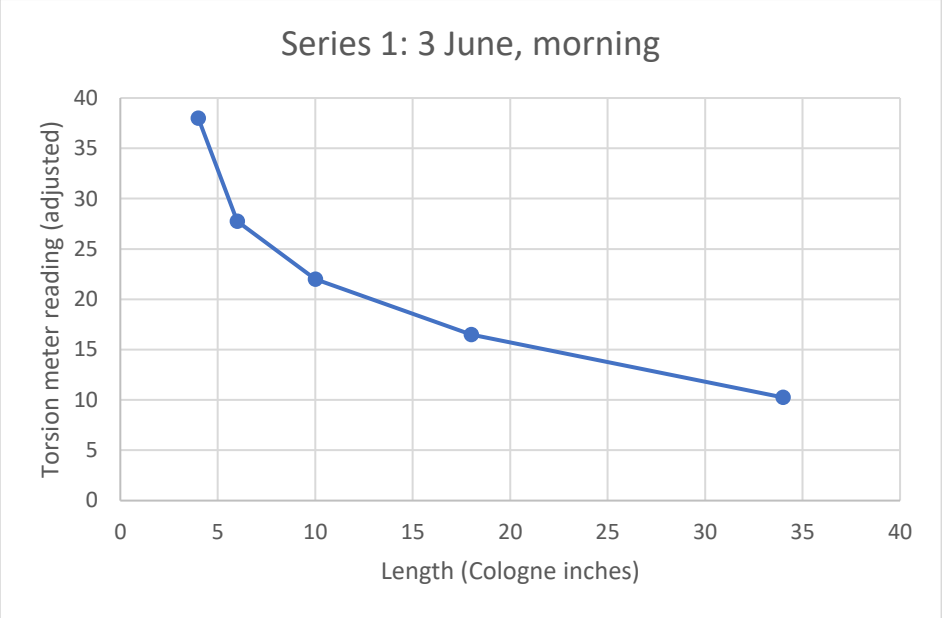



**Figure 85.** Chart showing results from experimental observations, Series 1: 3 June 2022, morning.

The first series of experiments suggested clearly that a short conductor caused greater deflection of the needle than a longer one, which was the expected result. However, my initial response to seeing the graph above was to think it showed the experiment had not worked perfectly. I had, incorrectly, expected a straight line to represent a direct correlation between resistance and the length of the test wire. I speculated that the temperature difference in the thermocouple had still been changing as the experiment progressed. It was only later that I realised my mistake, and that the graph above actually shows the correlation between conductivity and the length of the test wire. Conductivity is, as Ohm knew, the inverse of resistance. Therefore, if one takes the inverse of the results above, they can be plotted onto the following chart.

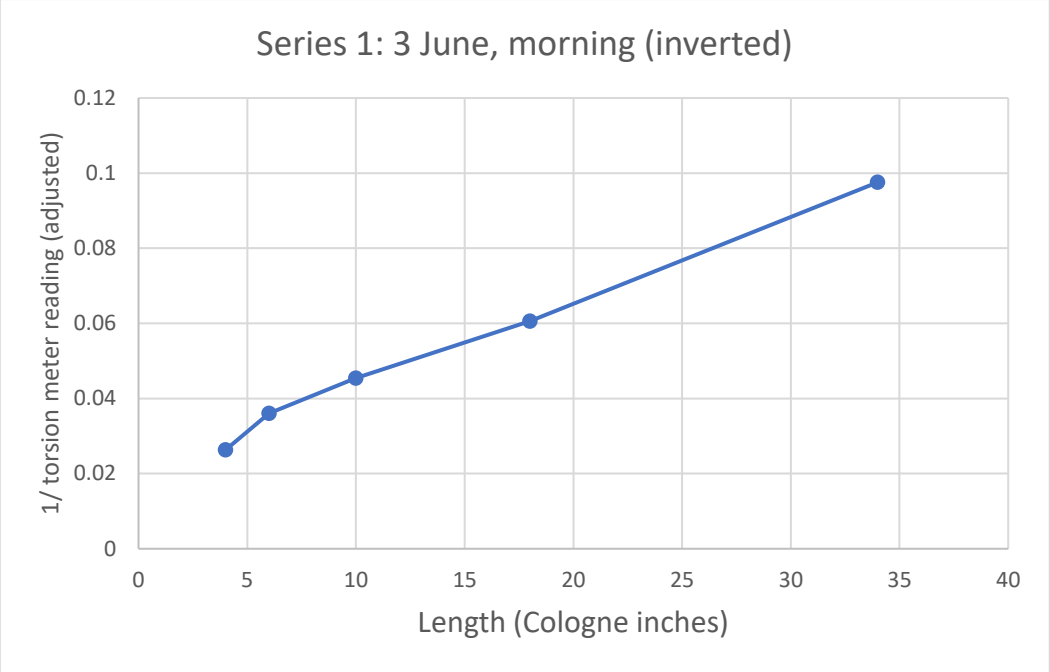



**Figure 86.** Chart showing inverse of results from experimental observations, Series 1: 3 June 2022, morning.

This is remarkably close to a direct correlation. My first set of observations had, it seemed, corroborated not only Ohm's law, but Ohm's account of his instrument producing the data needed in order to produce Ohm's law.

**Series 2: 3 June 2022, afternoon**

Although my first series of observations was promising, there were various challenges with the apparatus. The first of those was that the burner ran out of fuel very quickly and burnt very hot. While I had a risk assessment in place to manage the risk of fire, it nevertheless seemed sensible to seek an alternative fuel that was a little more controlled. To that end I purchased some methylated spirits (meths) to use for the second series of observations. As with the butane-based fuel, the burner lasted for about ten minutes before it ran out of fuel, and as before, it had to cool down to a safe temperature before being refuelled. However, the flame generally was more controlled and safer to work with. Because of the need to regularly change the burner, I began using a pair of pliers to extract the hot burner from the apparatus in order to quickly cool it and refill it. Only later did I realise that this addition to my experimental setup was causing me problems because the pliers had become magnetised. The effect was very weak, barely enough to notice unless the pliers were placed right next to the

needle. Nevertheless, it was enough to disrupt the observations. The situation was made worse by my ignorance of the problem I had introduced, so that every time I swapped the burners in or out, I dropped the pliers down somewhere nearby more or less at random.

Perhaps this knowledge helps to understand why, despite noting the time that the water first started boiling at 20:10, thirty minutes later at 20:40 I still had not been able to get the experiment stable enough to start making observations. I noted that whenever I got to the point of being ready to make an observation, with the needle stable and no longer moving, the burner would run out of fuel. I would then have to wait for it to be cool enough to refill, and then would have to get everything up to temperature again before making another attempt. I continued my attempts to make observations, but noted the challenges throughout and rushed the process to try and make some useful observations in between refuelling. I eventually stopped when daylight failed at around 21:45.

**Table 6.** Series 2: 3 June 2022, afternoon.

| Conductor number | Length (inches) | Reading | Notes |
|---|---|---|---|
| 1 | 2 | - | No reading taken, could not easily fit conductor into connectors |
| 2 | 4 | 71 ¼ | |
| 3 | 6 | 88 | |
| 4 | 10 | 82 | |
| 5 | 18 | 85 ½ | |
| 6 | 34 | - | Ceased observations due to failing light |
| 7 | 66 | - | |
| 8 | 130 | - | |

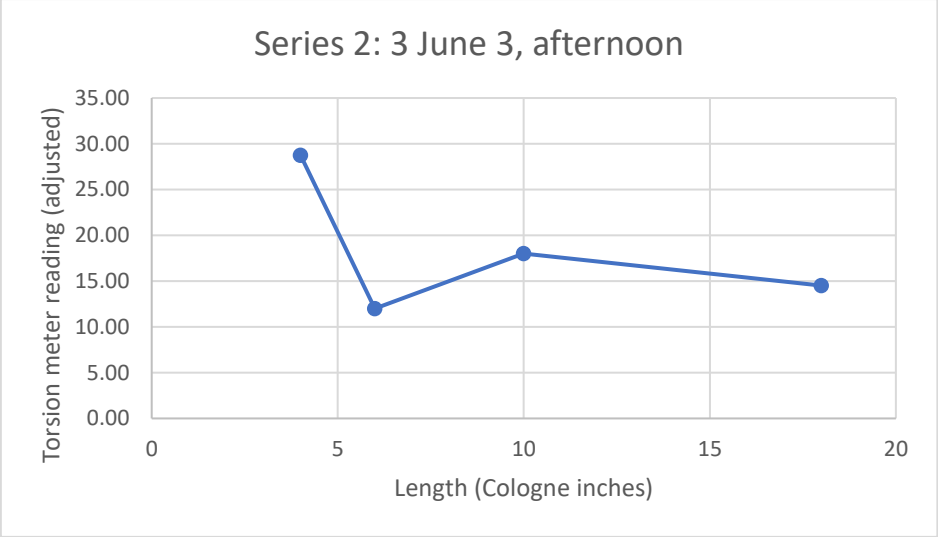



**Figure 87.** Chart showing results from experimental observations, Series 2: 3 June 2022, afternoon.

As can be seen, there is no clear pattern from the four observations I was able to make. Arguably, Conductor 3 could represent an anomaly, but with so few data points it is hard to make any conclusive arguments from this series of observations.

**Series 3: 4 June 2022**

To address the challenges of the unstable burner, I bought some candles. However, none proved to be suitable for the instrument. They either did not fit well, or did not produce a hot enough flame to sustain boiling water. Instead, I continued my attempts with meths, but made a more systematic plan for swapping the two burners in and out for each other.

I also took the decision to start my series of observations with the longest conductor first, then working towards the shorter conductors in turn. This was because of my observation that when the circuit is first connected up with conductor 1, the needle is strongly deflected and takes a long time to settle and become stable enough to take a reading. The longer conductor would cause a smaller deflection for the first reading.

Throughout this and the previous series of observations, I was concerned that the flames from the burner were licking around the side of the hot vessel and heating the copper elements directly. If that is the case, the temperature difference will be wider than the fixed gap between freezing water and boiling water.

**Table 7.** Series 3: 4 June 2022.

| Conductor number | Length (inches) | Reading | Notes |
|---|---|---|---|
| 1 | 2 | - | |
| 2 | 4 | 61 ½ | Refilled burner after conductor 3 |
| 3 | 6 | 83 ¼ | |
| 4 | 10 | 95 | |
| 5 | 18 | 91 ½ | Hot water and ice refilled, burner refilled after conductor 6 |
| 6 | 34 | 91 ½ | |
| 7 | 66 | 94 1/3 | Burner refilled after conductor eight |
| 8 | 130 | 94 1/3 | |

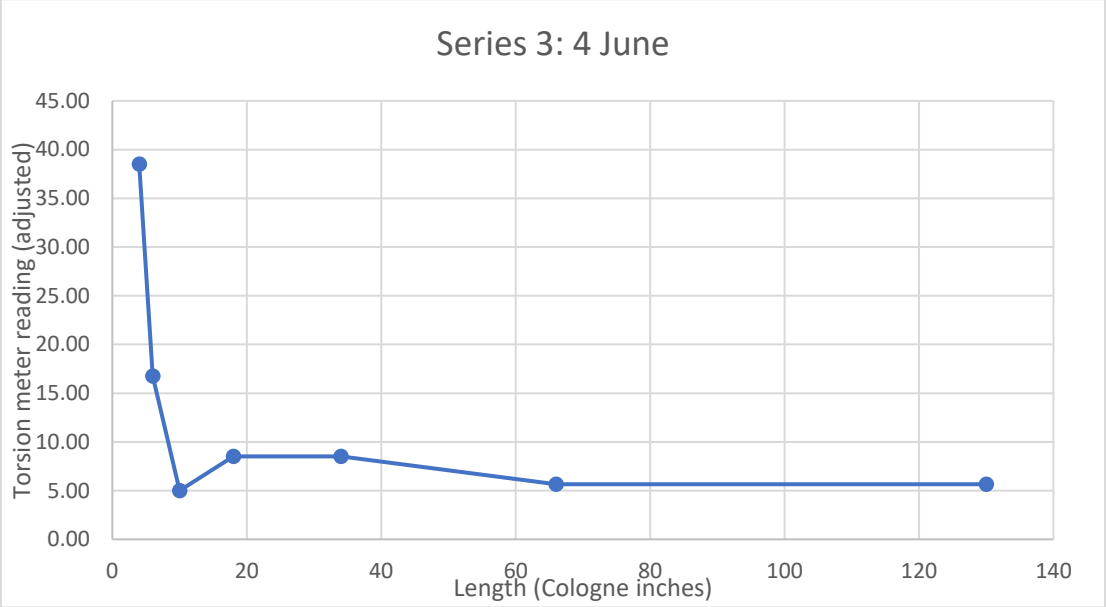



**Figure 88.** Chart showing results from experimental observations, Series 3: 4 June 2022.

As with Series 2, this set of observations was severely hampered by efforts to swap out burners and keep the experiment stable. The general trend is in the right direction, with the exception of conductor 4, however at each moment of refilling the various consumable parts of the experiment (ice, hot water or fuel), there is an anomalous result.

**Series 4: 10 June 2022**

For this series of observations, I used solid fuel “Hexamine” tablets in a metal tray. The tablets and tray are readily available from suppliers of model steam engine kits, and are sold as a safe alternative to meths. The fuel lights easily, and burns hot for a few minutes. However, the burn is inconsistent. Out of an approximately ten-minute burn time for each tablet, the flame is strongest for around three minutes before reducing significantly. While much more user friendly than the liquid fuels, as I could simply top up the tray with a new tablet as required rather than having to wait for the burner to cool, the results I collected were very inconsistent.

One additional observation made in this series of observations was that the spout for the hot vessel is underneath the bismuth piece. Ohm referred to a cork bung for the spout, which I had forgotten amongst the other complexities of designing and building the instrument. For the purposes of this series I placed a cap over the spout, repurposed from the small burner. The effect of this on the heating of the thermocouple was unclear, but from a very practical perspective it made it difficult to hear whether the water in the hot vessel was actively boiling, and in turn whether it had run out entirely. There were two occasions during this run of observations where I went to top the hot water up and heard a sizzle as the newly added water hit hot copper at the bottom of the empty vessel. Running out of water meant that the temperature was not being held at a stable point. These challenges suggest why, again, the results are inconclusive.

For this series of observations, and all subsequent ones, I stopped using audio recordings to capture my thoughts and instead swapped to taking notes by hand. The tabulated results are presented as I recorded them in my notebook.

**Table 8.** Series 4: 10 June 2022

| Conductor number | Time | Reading | Notes |
|---|---|---|---|
| 8 | 19 53 | 96 1/3 | |
| 7 | 20 01 | 97 1/2 | |
| 6 | 20 06 | 91 | |
| 5 | 20 10 | 88 2/3 | |
| 4 | 20 13 | 83 2/3 | |
| 3 | 20 33 | 89 | Refilled water |
| 2 | 20 37 | 93 1/4 | |

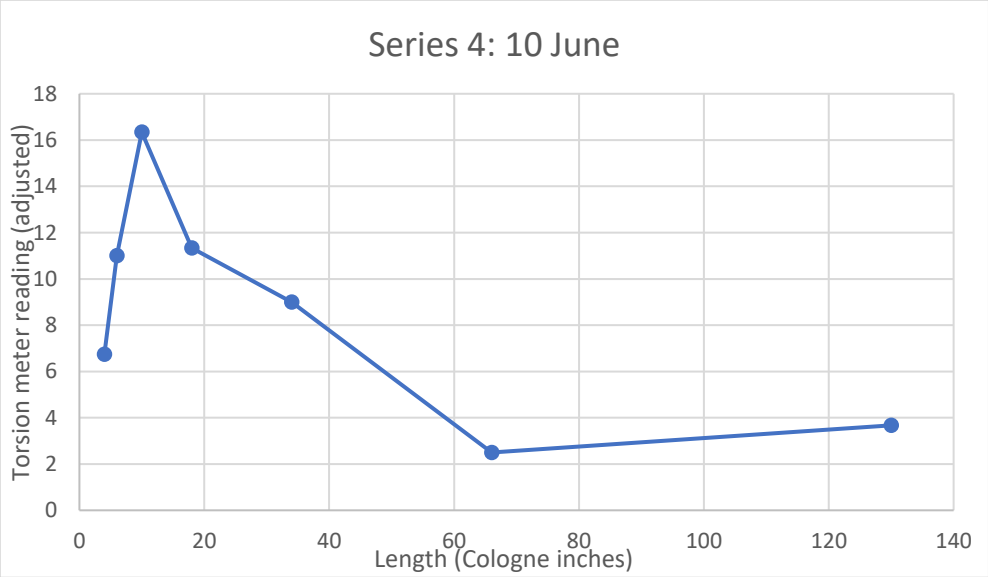
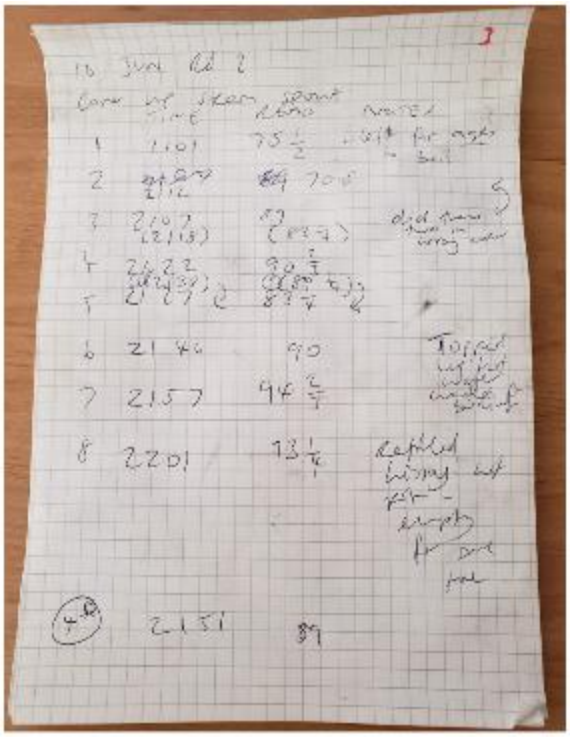



**Figure 89.** Chart showing results from experimental observations, Series 4: 10 June 2022.

I carried on operating the instrument and making observations into the evening, not finishing until almost 10pm. My frustration with the instrument not behaving as expected can almost be seen on the page of my notebook, as I backtracked, crossed out results when they had been changed, and became clumsy, doing things in the wrong order (Figure 90). For example, after taking a reading of 83½ with Conductor 5, I had to top up the water in the hot vessel and wait for it to boil. I repeated the reading with the same conductor and recorded a reading of 80¼ for the same Conductor. It often seemed to be the case that after refilling the burner, the reading changed even though the conductor had not. Because the burner was generally proving troublesome, I put this down to the temperature in the thermocouple being

inconsistent, and set about trying to stabilise that hot vessel as much as possible. In hindsight, it is clear that the readings changed after the burner was refilled because that was the point when the pliers I was using for handling the burner changed location and therefore changed the magnetic field around the instrument.



10 June [Round] 2.

Cover up steam spout

| | | TIME | READ | NOTES |
|---|---|---|---|---|
| 1 | | 21 01 | 75 ½ | left for ages to boil |
| 2 | ~~21 04~~ | 21 12 | ~~89~~ 70.5 | |
| 3 | | 21 07 | 89 | |
| | | (21 18) | (82 ¼) | did these two in wrong order |
| 4 | | 21 22 | 90 2/3 | |
| (5) | | (21 38) | (80 ¼) | |
| 5 | | 21 27 | 83 ¼ | |
| 6 | | 21 46 | 90 | topped up hot water waited for boiling |
| 7 | | 21 57 | 94 2/3 | |
| 8 | | 22 01 | 93 1/4 | Refiled hissing but not empty for some time |
| (4*) | | 21 51 | 89 | |

**Figure 90.** Excerpt from notebook. 10 June, p. 3.

**Tests with adjustments to instrument: 12 June 2022**

The burner continued to be the principal problem with running the experiment. I purchased some “lamp oil”, a paraffin-based fuel, which is known for burning bright yellow and cooler than the other fuels I had tried. The fuel seemed promising, it burnt less hot but for longer, lasting for over thirty minutes on the first burn. Like the other fuels, after some time the flames began to escape the side of the burner. The flame from this fuel is easier to see than the other fuels I tried, making it easier to see clearly that as the fuel hit its flashpoint vapours spilt out of the burner causing the flame to burn more ferociously.

As a safety precaution I poured a small amount of water onto the slate, which steamed and smothered the flame. When the burner was re-lit, it had a steady and well contained flame. From this I speculated that keeping the burner cool by regularly cooling the slate with water might help. This worked over a period of nearly 30 minutes, keeping the flame steady.

However, the lamp oil with the burner being regularly cooled did not burn hot enough to keep the water boiling, suggesting that one of the other, hotter-burning fuels might work well with this adjustment to the experiment.

The challenges with the burner may at least in part be down to my inexperience with open flame burners. In the 1820s they were common in domestic as well as laboratory use, and Ohm would almost certainly have been knowledgeable about the best way to trim a wick, for example, or which fuels were best suited to the task. It is also reasonable to imagine that he and his maker were familiar with the burners that were readily available to them, whereas in our instrument design it was almost an afterthought.

**Series 5: 16 June 2022**

In advance of this series of observations, I purchased a new wick for the burner. It was not such a snug fit as needed, and there were immediately flames burning around the edge of the burner where fuel had overflowed away from the wick. The observations took place at the end of a very warm day, which made it hard to cool the cold end of the thermocouple sufficiently without very regular trips to top up the ice. It is clear from the observed results that there was very little deflection of the needle for several of the observations.

**Table 9.** Series 5: 16 June 2022

| | Conductor number | Reading | Notes |
|---|---|---|---|
| 19 49 | 8 | 98 1/3 | |
| 19 51 | 7 | 99 ½ | |
| 19 55 | 6 | 99 ½ | |
| 19 58 | 5 | 95 ¾ | |
| 20 02 | 4 | 96 ½ | |
| 20 04 | 3 | 91 2/3 | |

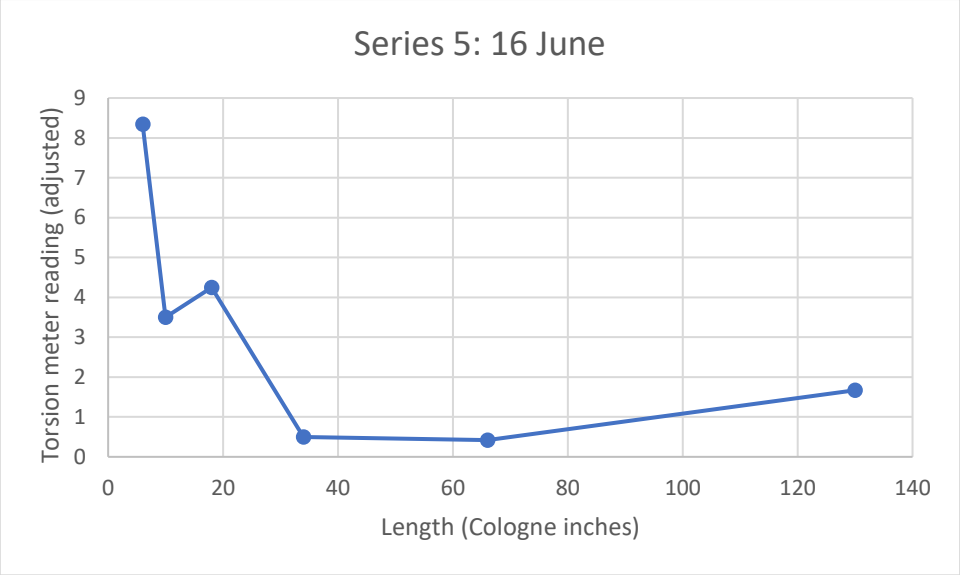



**Figure 91.** Chart showing results from experimental observations, Series 5: 16 June 2022.

I speculated that the very small readings were partly due to the high temperatures in my workspace, which made it difficult to cool the cold end of the thermocouple sufficiently to introduce a consistent or substantial temperature difference.

**Series 6: 22 June 2022**

Once again, it was very warm. I moved the apparatus inside to my dining table, removing the torsion balance for the move but otherwise leaving the instrument assembled. The motivation for changing location was to work somewhere cooler, which would hopefully enable a more stable temperature difference to be established in the thermocouple. This alternative workspace was more prone to vibrations, which required more care when working with the instrument. Before starting, I used an ice pack to cool down the cold end of the instrument. This was to counter a problem in the previous series of observations where the first ice placed in the instrument had melted very quickly, resulting in me having to scoop water out of the cold vessel to make space for more ice.

I noted that “yesterday I took my time to carefully trim a new piece of wick. May be a bit too long still inside burner. Kept v. short outside burner” (Connelly, notebook, p. 9). This illustrates again that my attention was almost entirely on the burner. I also began to record the times when I refilled the burner alongside my observations.

**Table 10.** Series 6a: 22 June 2022, partial series of observations

| Time | Conductor | Reading | Notes |
|---|---|---|---|
| 11 13 | 8 | 94 ¾ | |
| REFILL BURNER | | | |
| 11 23 | 8 | 93 | |
| 11 25 | 7 | 95 ¾ | |
| 11 25 | 7 | 95 ¾ | |
| 11 28 | 6 | 85 | Burner running low |
| REFILL BURNER | | | |
| Burner ran out before boiling sounds resumed! | | | |

After this very partial run of results, which was very inconsistent, I wrote:

> Going to try burner without wick. Seems to burn well and consistently clean flame, less smoky. Around 20 mins burn time. Decided to stop for lunch and try again with new technique afterwards.
>
> (Connelly, experimental notebook, p. 10)

The benefit of using the burner without a cap and a wick was that it was far quicker to refill. Extra care had to be taken not to risk a flash fire from pouring fuel onto hot metal, however cooling the burner down in cold water was quick and easy. With this quicker refill there was less time for the hot end of the thermocouple to cool between refills. Nevertheless, the initial tests with the apparatus showed that there were still inconsistent results before and after the burner was refilled and replaced. While the problems with the burner were not completely eliminated, after all it still needed regular refills, they were mostly resolved. For the first time I considered that there might be another problem with the setup that was not due to the practical problems with using the burner. Before starting to swap parts out of the instrument, I sought to check the stability of the thermocouple. I took two cooking thermometers from the kitchen and put one each in the lead shot on either end of the thermocouple. After a few minutes observation I noted that "+ 80 and + 10 C pretty consistent" (Connelly, notebook, p. 12). I took a complete set of observations, this time also noting the temperature at the hot end of the thermocouple. I did not record the temperature at the cold end because it seemed stable.

**Table 11.** Series 6b: 22 June 2022, complete series of observations

| Time | Conductor | Reading | Temperature at hot end of thermocouple (°C) |
|---|---|---|---|
| 13 59 | 1 | 78 | 79° |
| 14 04 | 2 | 76 | 79° |
| Noticed temp going up into high 80s – checked water, all run out. Refilled burner. | | | |
| 14 11 | 3 | 72 ¾ | 80° |
| 14 17 | 4 | 88 | 79 ½ ° |
| ~~14 22~~ | ~~5~~ | ~~86~~ | ~~82°~~ |
| Burner ran out | | | |
| 14 31 | 5 | 86 | 78 ½° |
| 14 40 | 6 | 89 | 79° |
| 14 49 | 7 | 97 | 79° |
| 14 54 | 8 | 95 | 80 ½° |

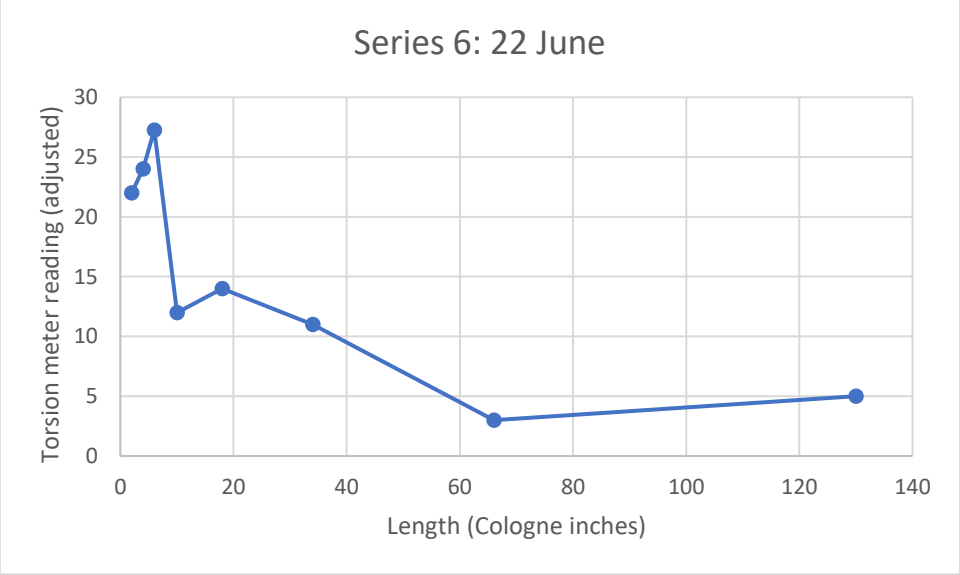



**Figure 92.** Chart showing results from experimental observations, Series 6: 22 June 2022.

Even without plotting the points on a chart, it was clear that the apparatus was not producing consistent results. That evening I began the process of swapping parts in the instrument to see whether the problem could be identified, my first change would be the torsion wire. Fortunately, this helped me to identify the problem with the pliers.

In the course of swapping the torsion wire for a new piece, I used a pair of scissors to snip some excess wire from near the needle. This caused the needle to swing unexpectedly, and led me to think about what else might be unexpectedly magnetic. I took my small compass and moved it around the various parts of the apparatus and the tools I had been using, and identified the pliers as weakly magnetic. When the needle, which is relatively strongly magnetised, was free hanging inside the torsion balance, it was possible with the pliers alone to pull the needle around in a circle from outside the glass cylinder.

**Series 7: 27 June 2022**

Having found a potential source of interference with the experiment, I returned to my original setup using the same torsion wire I had been using for series 1-6, noting: “pliers in other room, as are scissors” (Connelly, notebook, p. 14). I continued to use the thermometer to monitor the temperature at the feet of the thermocouple throughout the experiment, using it as a guide to when the instrument was up to temperature and ready to use as well as for monitoring purposes throughout the experiment.

**Table 12.** Series 7: 27 June 2022

| Time | Conductor | Temperature | Reading |
|---|---|---|---|
| 19 51 | 8 | 82° | 95 ¾ |
| 20 00 – REFILL BURNER | | | |
| 20 02 | 7 | 81 ½° | 95 ¾ |
| 20 07 | 6 | 86° | 92 ½ |
| 20 13 – REFILL BURNER | | | |
| 20 17 | 5 | 82° | 91 |
| 20 23 | 4 | 90° | 84 |
| 20 28 | 3 | 86° | 77 ¼ |
| 20 30 – REFILL BURNER<br>20 35 – TOPPED UP HOT WATER<br>Realised needle touching glass, resisting turning. Adjusted feet | | | |
| 20 41 | 2 | 82° | 74 |
| 20 47 REFILL BURNER | | | |
| 20 50 | 1 | 82° | 75 |

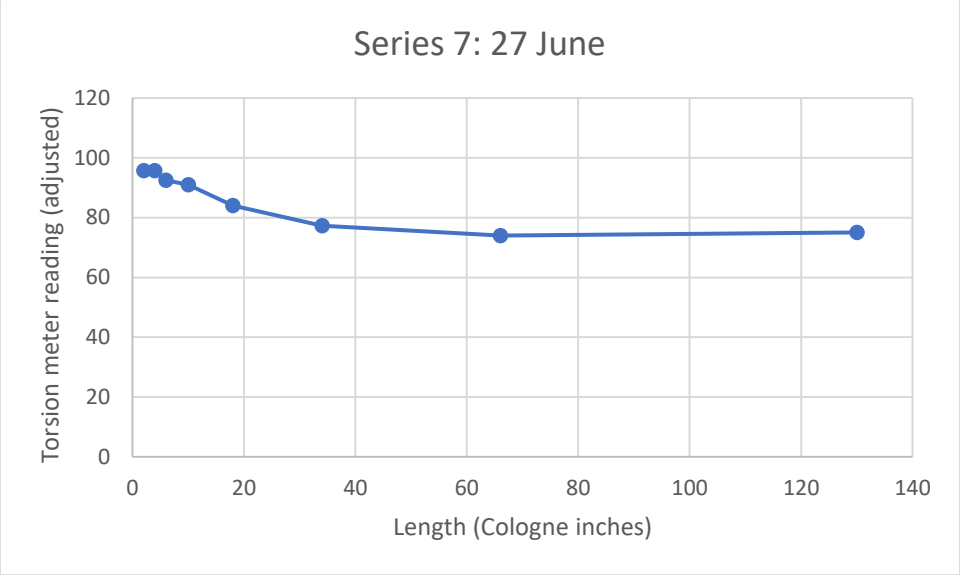



**Figure 93.** Chart showing results from experimental observations, Series 7: 27 June 2022.

This series resulted in what, on the face of it, appeared an unpromising result. However, the results are at least loosely correlated with the expectation that a longer conductor results in a smaller deflection. This was an improvement on Series 2-6, which had resulted in an almost random set of readings. I made two comments about the readings. The first was that Conductor 1 is not reliably useable. I had noticed that it was hard to get it into the connectors fully, and was not surprised therefore to see that it went against the trend of the other observations. The other comment was that with a meths burner, as opposed to my original butane-based fuel, together with the burner now being more controlled meant that the temperature difference was smaller in this series than in Series 1. This, I speculated, might mean that for the longer lengths of wire (and therefore more resistant) the instrument was reaching an equilibrium point, leading to the almost flat results for the longest conductors.

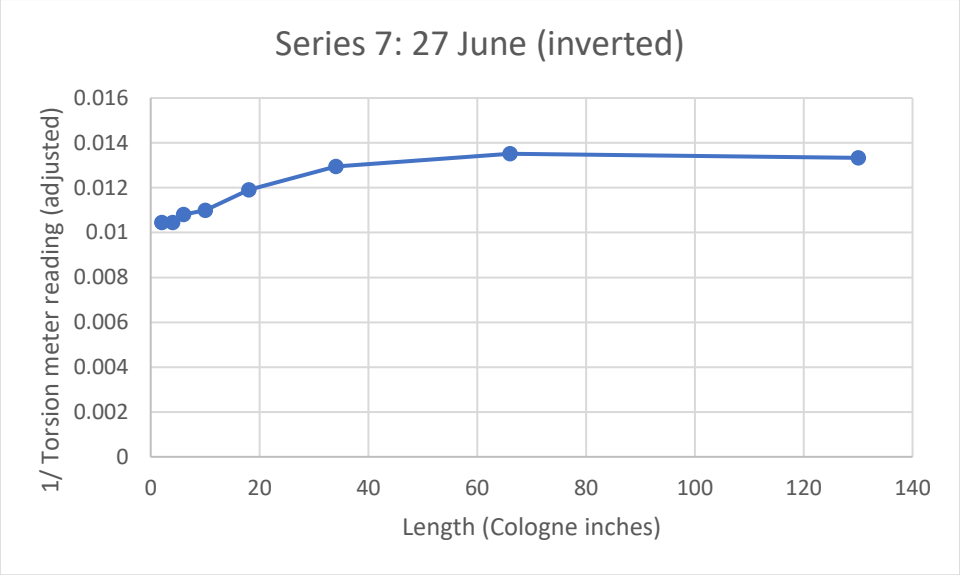



**Figure 94.** Chart showing inverse of results from experimental observations, Series 7: 27 June 2022.

Nevertheless, the inverted results show what could be interpreted as a direct correlation for the shorter wires. However, there are too few data points to be certain. For my next series I sought a way to make the instrument more sensitive.

**Series 8: 1 July 2022**

Having failed to source something matching Ohm's description of *Goldlahn* that also had anywhere near the right level of sensitivity, I considered other options and eventually decided to try the finest, strong material to hand that was not already spun or woven: my own hair. This seemed appropriate as not very different to the silk that Coulomb used in his early experiments, and had the benefit of being readily available. This was fortunate, as it broke easily when fitting it in the balance. I do not have easily available means to analyse the cross-section of a piece of my hair, but it is likely to be somewhat flattened as it has a natural curl; straight hair has much closer to a circular cross section. Because it is so fine, I was unable to see by eye whether the strand of hair was twisted in the instrument as I had been doing with the flattened "lahn". Instead, I aligned both the magnetised needle and the clamp at the top of the torsion balance with the meridian. I then used an external magnet to turn the needle through several twists and let it untwist itself to find a natural equilibrium point. The new torsion thread did indeed result in a more sensitive instrument. I found it difficult to get the needle to stay vertically in place, and ended up with it raised higher from the copper

conductor beneath than in some of my earlier series of observations. An alternative clamping mechanism may help with this for any future attempts.

**Table 13.** Series 8: 1 July 2022

| Time | Conductor number | Temperature at feet of thermocouple | Reading |
|---|---|---|---|
| 19 50 | 2 | 12° and 87° | 100 + 61 ¾ |
| 19 55 – REFILL BURNER | | | |
| 20 01 | 3 | 12° and 86° | 28 |
| REFILL BURNER | | | |
| 20 19 | 4 | 12° and 88° | 42 ¼ |
| 20 25 | 5 | 12° and 80° | 34 ½ |
| REFILL | | | |
| 20 32 | 5 (again) | 12° and 82° | 32 ¼ |
| 20 36 – REFILL WATER | | | |
| 20 49 | 6 | 12° and 84° | 39 ½ |
| 20 58 | 7 | 12° and 80° | 48 |
| 21 05 | 8 | 12° and 83° | 54 |

As I made the initial reading, I was unsure I had got the instrument correctly calibrated, with the torsion thread fully unwound. It was not a surprise, therefore, to see that the first reading seems anomalous alongside the others. There was no indication that the reading for conductor four should be incorrect, however. An attempt to redo just the observation for Conductor 4 failed, as the needle oscillated a lot and left me unsure whether it had been through a full revolution. While this happened, the water in the hot pot had all evaporated causing the temperature on the hot side of the thermocouple to soar well beyond 100°, causing some of the solder in the hot vessel to melt before I realised my error. Consequently, my second reading of Conductor 4, which I noted as 100+62.5 (137.5 adjusted), is even more out of step with the other results than the first reading.

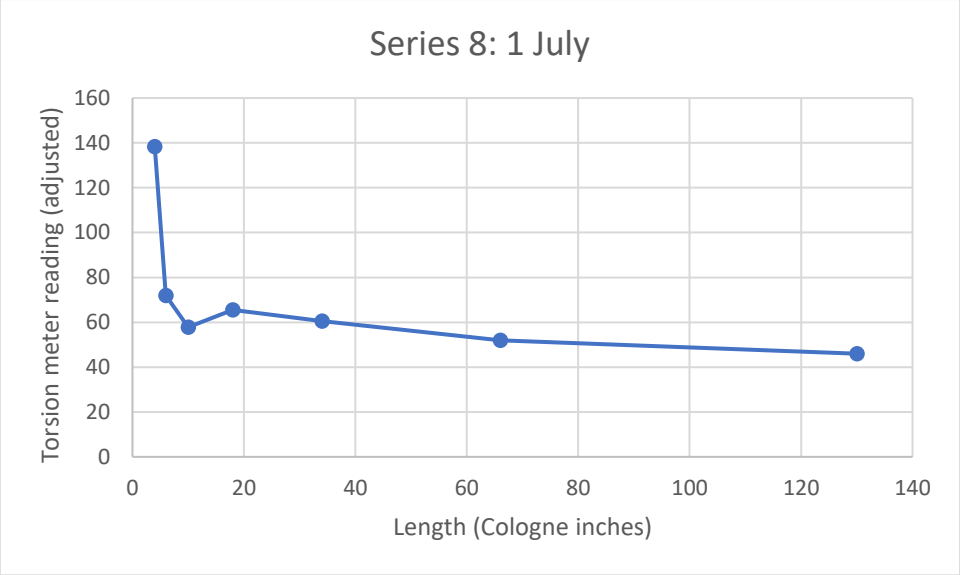



**Figure 95.** Chart showing results from experimental observations, Series 8: 1 July 2022.

Despite the two results that do not fit the trend, the remaining five points map quite well onto the predicted line.

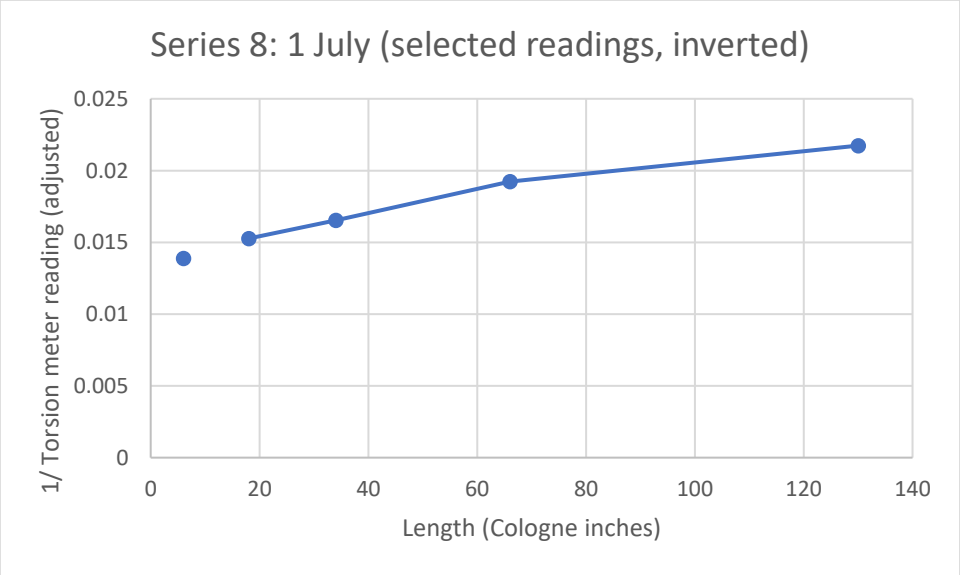



**Figure 96.** Chart showing inverse of selected results from experimental observations, Series 8: 1 July 2022.

## Evaluation and future developments

The process of producing this reworking of Ohm's experimental apparatus and practice has led me to several conclusions. Firstly, it has shown that this kind of deep engagement with material apparatus is a valuable tool for understanding science in practice and prompting new avenues for research. Secondly, it has demonstrated to my satisfaction that Ohm's apparatus could have been used in the way he described to produce the results he claimed. And thirdly, it has shown just how much goes unsaid and unwritten in the process of developing and using experimental apparatus. To the extent that these three statements are true, the project can be said to have been a success. Nevertheless, there are inevitably things that I might do differently if I repeated the exercise.

In terms of the instrument itself, there are a number of places where the production methods could be closer to those likely to have been used by Ohm. For example, the loupe could be produced more simplistically, and the bismuth part for the thermocouple could be produced using a sand-casting method. While these would make the instrument closer to my understandings and expectations of what Ohm's instrument would have been like, both changes would still require some degree of speculation. It seems unlikely that these relatively small adjustments would make much difference either to the use of the instrument or to the range of historical questions that were prompted in the process of development. A much more substantial change would be to consider the burner more seriously as part of the instrument design. Instead, it was something of an afterthought and one that proved very time consuming as I spent many hours trying to find the best fuel, burner and processes. Another change related to the burner is that I might consider how to heatproof the base of the hot vessel, or at least make it enclosed so that the risk of fuel spilling onto the wooden base and catching fire was lessened. However, using a piece of slate as a heatproof surface worked well.

The biggest failing in this reworking of Ohm's experiment was my inability to find a means of using mercury as a liquid conductor. As described earlier, this is very challenging within today's health and safety regulations, and was not something I was able to arrange within the scope of the project. There may be future opportunities to work safely with mercury, in which case I would be happy to rerun the experiment with my apparatus and see what differences it makes to the processes of swapping test wires in and out and taking readings.

The other key learning point regarding operating the experiment is to note that the suitability of the workspace is crucial. I was hampered throughout my observations by external factors

including the temperature in my home conservatory, the bounciness of the floorboards beneath my dining table, the unsuitable height of the instrument that caused me to bend down uncomfortably to take readings, and the location of magnetised objects. In the case of the infamous pliers that caused series 2-6 to be disrupted, that was a problem that could be solved. However, there were almost certainly other magnetic effects in my home that I could not easily identify or remove. In short, the laboratory setting, or at the very least a good solid workbench, is clearly very helpful for carrying out experiments.

Next, I would like to move away from evaluating the instrument itself, and consider the effectiveness of my tools for recording and reporting on my processes. Despite setting out to document my processes thoroughly throughout, there were a number of moments where I failed. Until the point of beginning to operate the instrument, I was disciplined about noting down my thoughts and ideas in a reflective diary. I saved receipts from purchases, and carefully filed away correspondence with a variety of people. Nevertheless, when I came to write this Appendix, I found that there were things I remembered very clearly, but had not written down at the time (for example, searching for an instrument maker, p. 232). This was not due to a lack of diligence, but rather a function of my diary-writing approach. I noted "progress", but rarely recorded when I had had conversations that had not developed into anything. Should I undertake a similar process again, I would consider developing a set of prompts for myself, so that I regularly note down my "big picture" thoughts about the progress of the project, in addition to the particular details of individual purchases or decisions.

When it came to operating the instrument, I initially began by making audio recordings of my activities. This was useful in capturing the time taken to do things, and I deliberately vocalised my thoughts as I worked with the instrument. When I later wrote my diary notes, it was noticeable how I had already subconsciously self-edited what had happened. My memory as written down showed that I had simplified a number of tasks and processes, and only on listening back to the audio recordings did that become apparent. Inevitably, the material presented in this appendix is a further abstraction, written with an audience in mind and the need to be comprehensible. The audio recordings made in the first few experimental runs were rather unwieldy to work with, so once I had established some basic processes for the experiment and established that I had time to make written notes as well as operating the apparatus, I stopped making them. Instead, I tried to record my thoughts on paper. At this stage my note-taking as part of the experiment seemed sufficient and my diary keeping

ceased, being replaced entirely by the notebook in my workspace. This meant that activities between series of observations went unrecorded, and were reconstructed from memory for this document. For example, between Series 5 on 16 June and Series 6 on 22 June, I replaced the torsion wire in the instrument and realised that my scissors were slightly magnetic. This led to the realisation that I had been causing magnetic interference in my experiment. The only record of this on paper is a note at the start of Series 6 that the (magnetised) pliers were in another room for the purposes of the experiment. As I became more practised at working with the apparatus, my note keeping also became more formalised. I stopped making many additional notes, and had a standard set of data that I collected each time I ran the experiment. My notes no longer presented a reflective diary, and much more closely resembled a laboratory notebook. At the time, I was so involved in working with the instrument that, even though I knew that documenting my experiences was a major part of my process, I could not effectively record my every action and operate the apparatus. It was no more possible than performing a piece of music while stopping to make notes on my feelings every time I had an emotional response. Making notes would ruin the response, and get in the way. Similarly in this case, my attention was entirely on the physical work at hand and noting down the vital data. Here a little more discipline would have been useful, again perhaps with some set prompts to encourage me to think about more than the immediate work of operating and observing the instrument.

Even though my own self-documentation is lacking in places, it serves as a useful reminder of human nature and the kinds of pressures on researchers, like Ohm, who leave only partial records behind. As in my case, breakthroughs are just as likely to happen outside of the main experimental activity as within, and may be less likely to be recorded when that happens. It is also instructive to look back at my own writings and think about when I had written with an audience in mind, and when I had not. My notebooks were intended only for me, and are full of short-cuts, scribbles and arrows making connections. My reflective diary, in contrast, was written with the expectation that I would quote passages from it. I already had an audience in mind as I wrote it, even though it was unlikely that anybody other than myself would ever access the whole document. Whether writing for myself, writing for an imagined audience or corresponding with the maker, I did not try to write absolutely everything down that I had undertaken or experienced, but rather I always selected the material that I thought would be useful to my future audience. We tend to think about published works, such as Ohm's published papers, as written with an audience in mind. What this process reminded me of was

the extent to which I myself am my own audience, and Ohm too was sometimes making his notes principally for himself. He no doubt, just as I did, omitted anything he thought he would remember easily, or that simply did not seem important enough to recall.

The final part of the process of reworking Ohm's apparatus that must be underlined is the essential role played by the maker. As can be seen in the early part of this Appendix, I enjoyed a fruitful two-way dialogue with the instrument maker, enabling us both to challenge and be challenged by each other. Even with Ohm's clear interest in mechanical craftsmanship, it seems highly unlikely that he simply instructed a maker in what to produce. More likely is that they discussed Ohm's ideas until they came to a shared agreement about the parts for manufacture. In my case it seems obvious that I could not have completed the project without first identifying a capable and interested maker to work with. I have learnt an enormous amount about metal working, and also become very aware of how much I do not know. Collaborating with somebody who has many decades of experience was invaluable. My father not only brought practical manufacturing skills, but also an intuitive sense of how to interpret drawings and descriptions and turn them into something that can be made. These skills were as vital to the successful completion of the project as being able to interpret German was to the text-based study.

To conclude, there are some changes that could be made with the intention of achieving a closer match to the instrument used by Ohm. However, as I hope I have successfully demonstrated, no amount of refining could ever produce an accurate replica of Ohm's instrument. At best it will only ever be an interpretation of the evidence he left behind. With the exception of not being able to integrate mercury as a liquid conductor, as Ohm had used, I feel assured that the Connelly apparatus is a close enough interpretation of Ohm's instrument to glean an understanding of some of his skills, knowledge and networks, as well as the opportunities afforded by the availability of particular commodities and craftspeople at the time he is working. For that reason alone, the reworking of his experiment has been a valuable exercise. The cherry on the cake is the demonstration in the very first set of observations, Series 1, that the instrument performed as expected and could have been used as described by Ohm as he developed his law.

# References

## Objects

Where possible, descriptions have been taken from museum catalogues, however where the catalogue was not available for consultation the descriptions are taken from interpretive labels presented in gallery or exhibition spaces. The inconsistencies in descriptions between organisations reflect the different information provided in labels or catalogues.

*Arts et Métiers, Paris*
03005 “Pile thermo-électrique avec deux écrans”, undated (entered the museum collection in 1844), made by Heinrich Daniel Ruhmkorff.
03010 “Pile thermoélectrique”, made 1845, made by Jean Gustave Bourbouze to a design by Claude Servais Pouillet.
05108 “Balance électrométrique de Coulomb”, made before 1852, no maker named.

*Deutsches Museum. Munich*
2155 “Messinggestell mit Drahtspule von Ohm”.
2156 “Tauchelement aus dem Nachlaße von Ohm”.
2159 “Galvanometer aus dem Nachlaße von Ohm”.
2163 “Magnetspule auf Gestell von Ohm”.
2164 “Trockensäule aus dem Nachlaße von Ohm”.
3919 “Elektrisiermaschine von Ohm” used c. 1840 by Georg Simon Ohm.
3920 “Batterie aus Leidener Flaschen” used c. 1840 by Georg Simon Ohm.
39094 “Elektrische Torsionswaage von Ohm”, undated, maker Georg Simon Ohm.
1976/9/9 “Torsionsdrehwaage” made 1976, maker Deutsches Museum.

*History of Science Museum, Oxford*
Inventory number 94227, accession number 1928-56 “Coulomb Torsion Balance Electrometer”, made c. 1850, maker Watkins & Hill. Provenance: Presented by H.Minn.

*Museo Galileo, Florence*
1124 “Nobili’s thermopile with micrometers”, made c. 1834, maker unknown. Inventor Leopoldo Nobili. Provenance: Lorraine collections.
1237 “Nobili-Melloni radiant-heat thermopile”, made c. 1831, maker unknown. Inventor Leopoldo Nobili, Macedonio Melloni.

*Rijksmuseum Boerhaave, Leiden*
V13317 “Torsiebalans” dated 1825-1875, associated with Coulomb, “reproductie” [reproduction].

*Science Museum, London*
1872-59 “Coulomb’s torsion balance” made 1872, maker Elliot Brothers after Charles Augustin Coulomb. Credit: Elliot Brothers.
1876-1274 “Cooke and Wheatstone’s four needle telegraph, 1838”, made 1838, maker William Fothergill Cooke. Credit: Donated by HM Postmaster General.

1931-536 “Replica of William Sturgeon’s rotary electromagnetic engine”, made 1832, maker William Sturgeon. No credit line provided.
1931-1122 “Replica of apparatus used by Ohm to prove Ohm’s Law”, made 1826, maker unknown. Credit: British Electrical and Allied Manufacturers Association.
1949-323 “Single needle telegraph, 1846”, made 1846, maker William Reid. Credit: Lent by King’s College London.
1983-478 “Oersted Compass Needle, 1828”, made 1828, maker Watkins & Hill. Credit: Donated by University College London, Dept. of Physics and Astronomy.

*Smithsonian Institution, Washington D.C.*
Catalogue number 323667, accession number 251332 “Coulomb Torsion Balance”, undated, associated with Coulomb. Credit: from Case Institute of Technology, Department of Physics.

**Sites and museum displays**

Cologne, Marzellenstraße, Georg Simon Ohm’s former school site, including a commemorative plaque to Ohm. Visited 7 September 2015.
Cologne, Rathaus, statue to Ohm appears on the north face of the town hall’s tower. Visited 7 September 2015.
Erlangen, 11 Fahrstrasse, Georg Simon Ohm’s family home and the nearby University of Erlangen. Visited 15 August 2015.
Erlangen, Ohm Platz and Ohm Gymnasium, square and school commemorating the Ohm family. Visited 15 August 2015.
Erlangen, Stadtmuseum, including displays relating to the town’s electrical supply, but no mention of Ohm brothers. Visited 15 August 2015.
Munich, Alter Südlicher Friedhof, Georg Simon Ohm’s grave. Visited August 2015.
Munich, Ohmstraße. Visited August 2015.
Munich, Technische Universität München, large statue to Ohm. Visited August 2015.
Munich, Deutsches Museum, several gallery spaces including the “hall of fame” featuring a bust of Ohm, and history of physics galleries featuring his electrical work and work on optics and acoustics. Visited August 2015.
Nuremberg, City Museum Fembohaus, display of local trades including locksmithing and wire drawing. Visited 14 August 2015.

**Unpublished texts**

Ohm, Georg Simon. “Versuchungsaufzeichnungen zur galvanischen Kette”. Deutsches Museum DM HS904.
Science Museum. Materials relating to The British Electrical and Allied Manufacturers’ Association. Science Museum nominal file 3923.

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