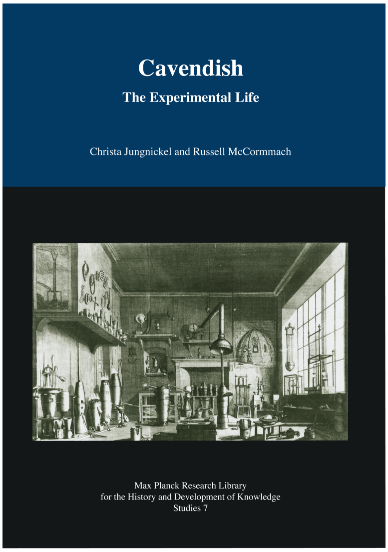
Christa Jungnickel and Russell McCormmach:

Early Researches



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Chapter 8
Early Researches

William James's observation that "in most of us, by the age of thirty, the character has set like plastic"[1] applies to Cavendish, if we take his "character" to include a narrow focus on science. His earliest known extended series of experiments were in chemistry and heat, specifically on arsenic and on specific and latent heats. This was around 1764,[2] twelve years after he had left the university and four years after he had been elected to the Royal Society. His first publication came two years later, on the chemistry of air, when he was thirty-five; this was rather late for a scientific researcher to begin, but in this as in other ways he was not typical. Never in a hurry to bring his work before the world, he was concerned to perfect it before communicating it.

Cavendish's Correspondent

The earliest contributions to the *Philosophical Transactions* were letters to its founder, Henry Oldenburg. Over time, the pretense of letters was dropped, and the genre of the scientific paper emerged as authors increasingly wrote for their readers instead of to the editor. With the introduction of a committee of papers in 1752, the editor withdrew further.[3] Still, during the time Cavendish was a student and beyond, publications in the *Philosophical Transactions* commonly took the form of "letters" addressed to the president of the Society or to a member who was knowledgeable about the subject. Sometimes a letter by an author would be published as a preface to a paper. The practice of sending letters to the journal is the background of Henry Cavendish's papers written to be read by a person referred to as "you." Given Cavendish's habits of privacy, a correspondent draws our interest.

"You" might have been his father, who was convenient, though here an informal way of communicating would have been more natural. Among other possibile correspondents is the longtime family friend William Heberden, who having lectured on chemistry at Cambridge would have been a competent reader; Cavendish's first published chemical research was carried out at Heberden's request. Another possibile correspondent is another family friend William Watson, who together with Heberden signed Cavendish's certificate at the Royal Society. Others are the London apothecary Timothy Lane, the London schoolmaster John Canton, and the Cambridge fellow and Anglican minister John Michell.

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[1] Paul T. Costa, Jr., and Robert R. McCrae (1994, 21–22).

[2] Cavendish's editor Thorpe refers to "an interpolation table calculated by Cavendish, from the results of measurements made in conjunction with his father on the Tension of Aqueous Vapor…. They appear to have been made about 1757 and are based upon a number of observations over a considerable range of atmospheric temperature and probably, therefore, at various seasons of the year." If Thorpe is correct about the year, they are the earliest experiments of Henry Cavendish's we have record of. *Sci. Pap.* 2: 355.

[3] Charles Bazerman (1988, 130, 137).

Timothy Lane published papers in the *Philosophical Transactions* on an electrometer in 1766 and on mineral water in 1769, which were Cavendish's interests around the same time. In 1766 Cavendish informed himself on electricity,[4] later making use of Lane's electrometer in his researches, and in 1767 he published a paper on mineral water. Lane took up the problem of mineral water where Cavendish left it, tying it closely to pneumatic chemistry and submitting his experiments privately to Cavendish for his opinion before publishing them. Lane and Cavendish had a similar aptitude for accuracy: Lane spoke of Cavendish's known "accuracy," and his own electrometer introduced a "much greater degree of precision" in the field of electricity, being capable of measuring the quantity of electric fluid stored in a Leiden jar with "tolerable accuracy."[5] In 1769 Cavendish invited Lane to five meetings of the Royal Society before his election the following year, Cavendish having signed his certificate along with John Canton, Watson, and Heberden.[6] The Royal Society extended a scientific exchange that had already been established between Lane and Cavendish, which may have included Cavendish's sending him papers to read.

A variety of evidence points to John Canton, a schoolmaster in Spital Square, as Cavendish's correspondent. Thirteen years older than Cavendish, Canton was elected fellow of the Royal Society in 1749, and he began publishing his experiments in the *Philosophical Transactions* four years later. Cavendish had a connection with Canton through his father, who in 1762 confirmed Canton's proof of the compressibility of water, discussed earlier. In 1766 Cavendish wrote to Canton about a book on electricity, establishing that the two had a connection by then; electricity was a major interest for both of them. The second possible evidence is an undated manuscript by Cavendish, "Paper Communicated to Dr Priestley," in which Cavendish referred to what Priestley wrote about mephitic air in 1767, which he would have got personally from Watson or Canton, probably the latter.[7] In his manuscript "Experiments on Heat," Cavendish left a clue concerning the identity of a correspondent "you," which fits Canton. Cavendish said that a certain substance differed from other substances by not transmitting heat as fast, commenting on his choice of the word "transmitting": "I forbear to use the word conducting as I know you have an aversion to the word, but perhaps you will say the word I use is as bad as that I forbear."[8] Fluids are conducted; if heat, as Cavendish thought, is not a fluid, "conduction" conveys a false idea, implying that his reader "you" accepted the idea of heat as the motion of particles, narrowing the circle of potential correspondents. In a paper in 1768, Canton showed that he regarded heat as the agitation of the parts of bodies.[9] Canton was generally interested in Cavendish's subject, heat, studying its effect on diverse phenomena: magnetic strength, electrical conduction in

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[4] Roderick W. Home (1972)

[5] Timothy Lane (1769, 216; 1767, 451); "Description of an Electrometer … with an Account of Experiments …," *PT* 57 (1767): 451–460.

[6] On 20 Apr., 4 and 11 May, 8 June, 9 Nov. 1769, JB, Royal Society 26.

[7] Henry Cavendish, "Paper Communicated to Dr Priestley," Scientific Mss, Misc. The paper is directed to "you," who is either Canton or Watson, most likely the former, who would have passed it along to Priestley. At this time, Cavendish did not know Priestley, who lived in Leeds, and Canton who knew Priestley lived in London. Two letters Priestley wrote to Canton in 1767 refer to Priestley's experiments on mephitic air. Joseph Priestley to John Canton, 27 Sep., 12 Nov. 1767, in Joseph Priestley (1966, 58).

[8] Henry Cavendish, section of "Experiments on Heat," entitled "Experiments to Shew That Bodies in Changing from a Solid State to a Fluid State Produce Cold and in Changing from a Fluid to a Solid State Produce Heat," *Sci. Pap.* 2:348–50, on 350.

[9] John Canton (1768, 342–343).

solids and air, absorption of electric fluid in solids, and emission of light in phosphorescence and luminescence.

The persons mentioned so far were capable of serving as a sounding board for Cavendish's experiments but probably not for his mathematics. At the bottom of the last page of a carefully drafted paper on the motion of sounds, Cavendish added a note addressed to "you," mentioning a demonstration, "which if you have a mind I will show you."[10] A possible mathematical reader for this paper was John Michell, with whom Cavendish later had a known connection, but the paper is undated and Cavendish had many Cambridge acquaintances who understood mechanics and mathematics.

As a special case, we consider one more possibile correspondent, John Hadley. He died suddenly in November 1764, the year Cavendish began saving his experimental papers, but in his writings that year, Cavendish could have had him in mind. Latent heat was one of Cavendish's first subjects, and we know about an experiment Hadley performed on latent heat. Chemistry, Cavendish's other early subject, was also Hadley's subject. Born the same year as Cavendish, Hadley entered the same college in Cambridge in the same year, and like Cavendish, he was good at mathematics, graduating fifth wrangler in the mathematical tripos examination.[11] Elected to the Royal Society before Cavendish, Hadley signed the certificate for Cavendish's membership, suggesting that he knew about Cavendish's work before Cavendish had published anything. Both were members of the Royal Society Club, and Hadley was a guest at the Cavendish home in London, so they had opportunity to keep in touch. When in 1756 a proper chair of chemistry at Cambridge was endowed, Hadley was appointed to it. He published a plan of chemical lectures in 1758, and that year and the next he lectured in the chemical laboratory at Cambridge.[12] He based his course largely on the work of foreign chemists, including the same ones Cavendish took his first chemical problems from, and he also included the British chemists Hales and Black, whose work was the starting point of Cavendish's first published paper. In an unpublished part of his first paper Cavendish mentioned Hadley's account of the distillation of a salt with a metal as support for his own experiments on the distillation of various substances.[13] Hadley gave close attention to mineral water in his lectures, even beginning his own investigation of a mineral water, which he broke off when it became too difficult.[14] Cavendish's second publication was a chemical analysis of a mineral water. Cavendish addressed his earliest preserved chemical research, in 1764, to "you." If he had been in the practice of writing for Hadley, he may have continued to write for him even after 1764, *as if*.

Given the range of his researches, Cavendish likely had more than one correspondent. Considering that his scientific manuscripts contain no responses to his early researches, it is conceivable that he did not send his work to anyone but simply adopted the form of the letter-

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[10] Henry Cavendish, "On the Motion of Sounds," Cavendish Mss VI(b), 35:10.

[11] "Hadley, John," *DNB*, 1st ed. 8:878–880, on 879.

[12] John Twigg (1987, 212–213). John Hadley (1758). At Trinity College, Cambridge, there is a two-volume manuscript of Hadley's lectures: "An Introduction to Chemistry, Being the Substance of a Course of Lectures Read Two Years Successively in the Laboratory at Cambridge by John Hadley …." "Hadley, John," 879.

[13] Hadley's work is referred to in a footnote to the unpublished fourth part of Cavendish's paper on factitious air in 1766. "Experiments on Factitious Air. Part IV. Containing Experiments on the Air Produced from Vegetable and Animal Substances by Distillation," *Sci. Pap.* 2:307–316, on 313.

[14] Hadley wrote to the secretary of the Royal Society that the analysis of mineral water was "very difficult & would lead into very extensive chemical inquiries, "and his own papers on it were "not of consequence enough to be printed." John Hadley to Thomas Birch, 13 Sep. 1762, BL Add Mss 4309, f. 9.

report from the *Philosophical Transactions*. In the absence of more revealing documents, we can only speculate about his correspondents.

Chemistry

By all accounts Cavendish cut an awkward figure in public. He did not do so at home, where everything was made to fit. Furnished with instruments and books, his home was the principal location of his chosen life. The gentleman's double house on Great Marlborough Street, with its elegant stairs leading off the entrance and its rooms for entertaining, was unlikely to have been used also as a chemical laboratory. If Cavendish carried out his chemical researches at home, as he no doubt did, the location would have been either the stables or the separate apartment on the grounds behind the main house, and most likely in the former. Since we know that his father had chemicals, a laboratory in some form might already have been in place for Henry. In any case, by the time he wrote his earliest surviving papers on chemistry, he had a substantial chemical laboratory. We have no description of it, but we know in general what it had to be like (Figs. 8.1–8.2). It would not have been located in the underground rooms of the apartment behind the main house (if he was living there then), for in the dampness, metals would have rusted, furnaces collected mold, salts turned watery, and labels fallen off bottles. The laboratory would have been in a ground-floor room or in a room in or above the stables, with openings to the outside at each end for admitting fresh air and clearing away poisonous vapors. We suppose that there was a chimney high enough to walk under and wide enough to walk in front of. Beneath it we picture various furnaces and probably a double bellows to fan the flames from gentle heat to red hot. Ready at hand, suspended on hooks, would have been pokers, pincers, tongs, shovels, and pans, much as in a kitchen of that day. Near the chimney was an anvil along with hammers and a range of other tools. Lining the walls were shelves for containers and chemicals, near which were bins for storing bulk charcoal, sand, and quicklime. Since acids, alkalis, metals, and earths had to be as pure as possible, standing in a corner of the laboratory was a lead or stone "fountain" with a drain pipe for cleaning vessels after each use, no doubt by an assistant. In the center of the room was probably a large table for chemical operations not requiring a high heat, on which were laid out scales, mortar and pestle, filtration paper, corks, stirrers, pencils, pens and ink, and a stack of small sheets of paper for keeping notes.[15] From Cavendish's manuscripts, we can be specific about what he required to carry out his early researches. Heat entered into most of his operations: roasting, calcining, dissolving, subliming, evaporating, and distilling. His sources of heat were lamps, a forge, and a reverberatory furnace, designed to direct the flame back on the heated substance, placed high into the chimney in anticipation of "obnoxious" fumes. There was a sand pot for distilling at "sand heat" and for holding bottles. Other operations included precipitating, crystallizing, filtering, deliquescing, and weighing. At some time Cavendish acquired a cabinet containing scales of high quality. He had an elaborate collection of containers, some made of metal, some earthen, most of

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[15] We have been guided in our sketch of Cavendish's laboratory by the entry "Laboratory (Chemical)" in Pierre Joseph Macquer's *Dictionary of Chemistry*, originally published in 1766, just after Cavendish had begun his known chemical experiments. Macquer's laboratory was intended for the "philosophical chemist," and together with his list of reagents, it sufficed for "any chemical experiment." P.J. Macquer (1771). A more detailed itemization of apparatus divided into items used in preparation of operations and items used in operations is given in Peter Shaw and Francis Hawksbee (1731, 19–21).

glass. There were open flasks, Florence flasks (having long, narrow necks), retorts (having downward bending necks for distilling), receivers (flasks for retaining condensates and distillates), adapters (for connecting retorts and receivers), pipkins (small pots and pans), bottles of various sizes, glass tubing, and copper pipe. There was a lead crucible for keeping the bottom of another crucible placed in it cooler than the top. There was another crucible designed by Cavendish for use in the reverberatory furnace, complete with a set of aludels (pear-shaped pots open at the bottom as well as at the top and made to fit over one another for subliming). Cavendish's apparatus was made for the purpose, to which he added a humble coffee cup for calcining. His materia chemica included solvents, acids, solutions of metals and acids, alkalis, neutral salts, and solutions and treated papers for testing acids and alkalis. Cavendish's chemical experiments depended on a sizable investment in chemical apparatus and supplies. The chemist James Keir may have had Cavendish in mind when he gave as one reason for the emergence of chemistry as a science its recent cultivation by "persons who employ the advantages attending rank, opulence, leisure, and philosophical minds."[16]

Ever since Wilson's biography, Cavendish's mind has been likened to a calculating engine, and although it is a caricature, he was an experimenter who made copious quantitative observations and calculations. He filled his laboratory notes with numbers standing for proportions of reactants and weights expressed in ounces and their breakdown into drams or grains. In combination with his measurements, he expressed in numbers various aids such as standards, equivalents, and saturation (the point at which acids in combination with other substances lose their acidity or at which solutions have dissolved as much solutes as they can). Cavendish's skill in quantitative work is evident in his early chemical research, in which he worked with uncommonly small amounts of substances, ounces instead of the familiar pounds.

Cavendish typically began an experiment with carefully weighed quantities of substances, which he then combined and performed various operations on, and the products he obtained he would again weigh. He might then put the products through a series of tests, "small experiments" as he called them, in which he did not record, and probably did not measure, the quantities involved. As he proceeded, he described as well as measured: in his investigation of neutral arsenical salt, he witnessed fuming, shooting of crystals, and other manifestations of chemical and physical activity. By smell, he distinguished between acids and their products. He observed textures: dry, hard, thin jelly, gluey, thick, stiff mud, and lump. With colors, he made the most distinctions: milky, cloudy, yellow, pale straw, reddish yellow, pale madeira, red, reddish brown, dirty red, green, bluish green, pearl colored, blue, and transparent. His account of arsenic was the record of a complete investigation, if under "complete" we include the activity of a thinking mind. Cavendish's goal was understanding, which involved hypotheses and explanations.

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[16] James Keir, "Preface," iii, in his translation in 1771 of Macquer's *Dictionary of Chemistry*.

Chemical Apparatus and Laboratory

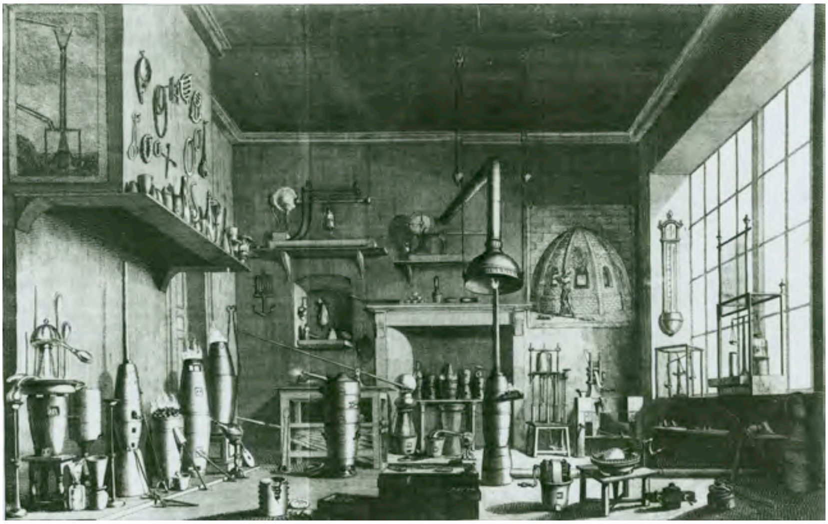

Figure 8.1: Chemical Laboratory. This idealized laboratory with metallurgical furnaces is from William Lewis, *Commercium Philosophico-Technicum* (London, 1756). Courtesy of Smith Image Collection, Van Pelt Dietrich Library, University of Pennsylvania.

Figure 8.2: Chemical Laboratory. From Denis Diderot, *Dictionnaire raisonné des arts et des métiers*, 1780. Courtesy of Smith Image Collection, Van Pelt Dietrich Library, University of Pennsylvania.

Chemistry in the middle of the eighteenth century was still closely tied to pharmacy, medicine, metallurgy, and manufactures, but it had a strong scientific direction too. A major scientific source was the work of Johann Joachim Becher and Georg Ernst Stahl, who introduced an oily earth given off in combustion and presumed to be present in every combustible body. "Phlogiston," the name given it by Stahl, the Greek word for "inflammable matter," was one of four elements (the other three being water, mercury, and another kind of earth), but because of its common presence in chemical processes, his chemistry came to be identified with *phlogiston*. Stahl and his followers took little notice of the physical properties of substances, and they denied that chemistry had mechanical foundations. The other scientific source of chemistry was Robert Boyle (Fig. 9.2), Newton, and Boerhaave, who regarded chemistry as a branch of physical science that made use of mechanical concepts.[17] Because merit could be seen in both approaches, the chemical and the physical, attempts were made to bring together the "chemist" Stahl with the "physicist" Newton or Boerhaave, a route to a unified chemistry advocated by Macquer, Macquer's collaborator Antoine Baumé, and L.B. Guyton de Morveau.[18] By Cavendish's time, the physical approach to chemistry had incorporated the combustible principle from Stahlian chemistry. Cavendish's approach was physical, and he was a phlogiston chemist.

An advantage of phlogiston chemistry was its unified explanation of combustion and of the calcination of metals (the transformation of metals by intense heating or by chemical combination into a powder having the properties of an earth). When combustibles such as charcoal burn, their phlogiston separates and flies off, the evidence for which is obvious to the senses. When metals, which like combustibles contain phlogiston in combination with another constituent, are calcined they lose their phlogiston, and when the calces are heated with charcoal they reacquire phlogiston, returning to pure metals. Phlogiston, by its presence or its absence, affects most chemical reactions, and by keeping a balance, the chemist could foresee the outcome. The experimental proof of phlogiston seemed incontravertible, the reason why the physical school of chemistry accepted it. However indispensable it was in understanding chemical operations, phlogiston by itself was elusive, thought to be the "least accurately known" of chemical substances or principles and incapable of being isolated and studied on its own.[19] Cavendish would disagree on this important point.

When Cavendish took up chemistry, phlogiston was familiar in Germany, but in Britain and France it was just taking hold. Interest in phlogiston in France was stimulated especially by translations of Becher's and Stahl's writings by Guillaume-François Rouelle and his group in Paris.[20] Rouelle's student Macquer's text on theoretical and practical chemistry in 1758 and Casper Neumann's lectures on chemistry in 1759 were the first accounts of phlogiston in English.[21] Cavendish's colleague Hadley, an early English advocate of phlogiston, said that in preparing his lectures in Cambridge he was "much beholden" to Becher and Stahl. In his lectures in 1758 and 1759, he used the word "phlogiston" throughout.[22]

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[17] Maurice Crosland (1963, 408, 440).

[18] Mi Gyung Kim (2003, 203). Antoine Baumé (1763, 41–44). Crosland (1963, 408).

[19] Thomas Thomson (1830–1831, 2:257–260). Macquer (1771, 2:516).

[20] Thomas L. Hankins (1985, 95). Henry Guerlac (1959, 103).

[21] W.A. Smeaton (1975, 619). Macquer's *Élémens de chymie théorique* (Paris, 1749) and *Élémens de chymie practique* […] (Paris, 1751) were brought out in English translation by Andrew Reid in 1758 as *Elements of the Theory and Practice of Chemistry*. Casper Neumann (1759). Nathan Sivin (1962, 73).

[22] Quotation from p. 8 of Hadley's lectures. L.J.M. Coleby (1952a, 295).

Arsenic

Cavendish's earliest completed chemical research was an experimental study of "arsenic," our arsenious oxide. (His paper was described ominously by one commentator as "Notes on some experiments with arsenic for the use of friends.")[23] Halfway through his laboratory notes the date December 1764 appears.[24] An unnamed reader is referred to in a carefully written draft of his paper on arsenic as "you," who worked with the same substance, "as you tell me you have tried yourself," and who evidently visited Cavendish's laboratory, "particulars of this exper. which I showed you before."[25] Hadley could have been this person, especially since his Cambridge lectures contained an extended discussion of arsenic among the "semi-metals,"[26] qualifying him as an informed reader.

By the time of his experiments on arsenic, Cavendish had been coming to meetings of the Royal Society for about seven years, five years as a member, during which time he had heard few reports or read few papers dealing with chemical topics in the *Philosophical Transactions*, and none relevant to the work in question.[27] The Londoner Cavendish, who was just then setting out on chemical research, would have consulted books and papers from abroad, written in the foreign languages he could read, Latin, French, and German, or else in English translation. His point of departure was the French chemist Pierre Joseph Macquer's discovery and naming of "neutral arsenical salt" (potassium arsenate), which appeared in two papers published by the Paris Royal Academy of Sciences in 1746 and 1748. Macquer's work on arsenic was noticed in Britain; Hadley, for example, took an interest in it.[28]

In this, his most important early work, Macquer distilled arsenic with nitre (potassium nitrate), leaving as residue a compact, white, soluble, mild salt, the neutral arsenical salt. The salt had obvious value for scientific chemistry, and it probably had practical uses, though Macquer doubted that these included medicine despite its actual mildness, since the "name

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[23] Quoted in John Pearson (1983, 118).

[24] The earliest chemical work by Cavendish for which there is an apparently complete record consists of the following: a bundle of 59 numbered pages of laboratory notes on arsenic, with index; a carefully written 25 page version of the account; and 19 unnumbered pages constituting a rough draft. Cavendish Mss II, 1(a), 1(b), and 1(c). A brief description and analysis of these papers is given by Thorpe, in Cavendish, *Sci. Pap.* 2:298–301.

[25] Henry Cavendish, "Arsenic," Cavendish Mss II, 1(b):20, 25.

[26] It was probably sometime after December 1764 that Cavendish wrote or at least completed the paper for "you." To give an idea of the extensiveness of Hadley's familiarity with arsenic, the topics he addressed under "Of Arsenic" in his lectures were: "The Orders of Arsenic; Cobalt, white Pyrites, Orpiment, Realgar. – Of *white*, *yellow*, and *red* Arsenic, and the Method of procuring them – Artificial Realgar, Orpiment fused – Regulus of Arsenic procured from Cobalt by Distillation – Zaffer and Smalts – Sympathetic ink made with Zaffer – Glass rendered Blue by fusing it with Zaffer – Acid of Niter procured by distilling Nitre with Arsenic – The Residuum considered – Arsenic fixed by fusing it with Nitre – Regulus of Arsenic deflagrated with Nitre – White Enamel of Arsenic – Reduction of Arsenic to its Reguline form – Butter, Oil, and Cinnabar of Arsenic, procured by distilling Orpiment with Corrosive Sublimate – Sympathetic Ink from Orpiment and Lime, and its use in discovering the adulterations of Wine by preparations of Lead." Hadley (1758, 17–18).

[27] In the years 1755–64, the *Philosophical Transactions* contained eight papers on "chemical philosophy" and two on "chemical arts," according to the classifications used in the abridgment of the journal, which lists all papers appearing in the full journal. Five other papers were about natural waters, the subject which Cavendish would take up in his second published paper on chemistry.

[28] Pierre Joseph Macquer, "Researches sur l'arsenic. Premier mémoire," and "Second mémoire sur l'arsenic," *Mémoires de l'Académie des Royal Sciences*, 1746 (published 1751), 223–236, and 1748 (published 1752), 35–50. Macquer described this work in 1766 in his *Dictionary of Chemistry*, translated in 1771. The article "Neutral Arsenic Salt" is in vol. 2, 666–667. Shortly before Cavendish's researches on the subject, Macquer's work on arsenic was described in English in an annotation by William Lewis to the translation of Casper Neumann (1759, 143). Coleby (1952a, 301).

of arsenic is so terrible."[29] The agonizing symptoms and fatal consequences of arsenic were mentioned in every book of chemistry. The German chemist Caspar Neumann cautioned that arsenic is a "most violent poison to all animals," so that the "utmost caution is necessary in all operations upon arsenic, to avoid its fumes," which have a "strong fetid smell resembling that of garlic"; and in solution, it has a nauseous taste. Arsenic, it seemed, had no attractive qualities. Little wonder that it, Neumann said, had been "so little examined" by the chemist.[30]

When Cavendish took up the study of arsenic, chemists had not been able to "determine what it really is, or to what class of bodies it belongs."[31] Independently of its noxious properties, arsenic has "singular properties, which render it the only one of its kind." It was the "very singular and extremely different" properties of arsenic from those of other metallic calces that led Macquer to investigate this little-known calx in the first place.[32] Neither fish nor fowl, but something of a flying fish, arsenic behaves like a metal in some states and like a salt in other states. On the one hand, like every metallic calx, "arsenic" can be changed into a metallic form, a "true semi-metal," or "regulus of arsenic," by combining it with phlogiston. On the other hand, like salts, arsenic is soluble in water. Even when it is regarded as a salt, arsenic is uncommon, neither acidic nor alkaline, yet it behaves as if it were an acid.[33] When it is considered as a calx, arsenic differs from other known calces: it is volatile with a strong smell, it is fusible, it unites with metals and semi-metals, and—the difference that Macquer and Cavendish picked up on—it decomposes nitre when distilled with it.[34] From the standpoint of its readiness to unite with other substances, arsenic is exceptional too.[35] Cavendish did not say why he investigated arsenic, but from the state of chemistry at the time, we get an idea of its considerable interest, at once dangerous, difficult, unique, scientifically puzzling, and incompletely known.[36] Its study demanded manipulative skills of a high order, a stiff challenge and testing ground for a young chemist.

In practice, chemistry looked complicated because it dealt with all kinds of matter with a large repertoire of operations. In principle, chemistry looked simple, though this appearance was changing. "Neutral salts," Cavendish's starting point, are a case in point. These were salts composed of acids and other substances that were without acidity, usually alkalis. Not long before, neutral salts could be arranged in a compact table of twelve entries, but when Cavendish began to work with them, the table of neutral salts was fast expanding.[37] The

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[29] Macquer (1771, 1:100, 2:666–667).

[30] Neumann (1759, 145).

[31] Ibid., 140–141. What Neumann, Macquer, Cavendish, and their contemporaries called "arsenic" is a dense, brittle substance with a crystalline or vitreous appearance; this substance, arsenious oxide, is a common byproduct of roasting metallic ores. Another name for it then, as now, is "white arsenic," the calx of regulus of arsenic, the white, shiny semi-metal.

[32] Pierre Joseph Macquer (1758, 1:96).

[33] Macquer (1771, 2:634).

[34] Ibid. 1:99–100.

[35] Arsenic has the least, or next to least, affinity of the soluble substances for the several acids, with the exception of aqua regia. Gellert's "Table of the Solutions of Bodies," at the end of vol. 2 of Macquer's *Dictionary*.

[36] For example, arsenic was soluble in acids, and the results had "not yet been sufficiently examined." Macquer (1771, 1:103).

[37] The Scottish chemist William Cullen's table of twelve neutral salts was reproduced in Donald Monro (1767). Monro, on page 483, pointed out that a table had been published in Germany giving three or four more of these salts, and that there were actually many more because vegetable acid was in reality many acids each with its own neutral salts.

subject of salts in general was recognized as highly undeveloped, with so many “little known, or not even thought of.”[38]

Cavendish examined the action of several acids and alkalies on arsenic. He procured Macquer’s neutral salt using Macquer’s method of distilling arsenic with nitre, noting the misnomer: the salt was slightly acidic, not neutral. He dissolved arsenic in spirit of nitre (nitric acid), and then by adding the alkali pearl ashes (potassium carbonate), he made a discovery: the change that arsenic underwent when dissolved by spirit of nitre made it acidic. To see if he could isolate the acid, he dissolved arsenic in concentrated spirit of nitre (which he called aqua fortis, another name for nitric acid) and then drove off the acid by heat. The experiment succeeded: the residue dissolved in water, which turned acidic (arsenic pentoxide). To be certain that he had an acid, he tried it on other alkalies, calcareous earths, earth of alum, and magnesia, and he tested it with syrup of violets, which turned red, the color of acid. What combined with an alkali to form the neutral salt was not any known acid but “arsenical acid” (“if you will allow me to call it by that name“). The product had “all the properties of an acid,” a conclusion Cavendish qualified with an implicit acknowledgment of the fatal reputation of arsenic, “unless perhaps it should fail in respect of taste which I have not thought proper to try.” He showed that the crystals formed by dissolving a fixed (non-volatile) alkali in arsenical acid resembled Macquer’s neutral arsenical salt. The discovery of an acid was the high point of Cavendish’s researches on arsenic.[39] A new acid was important, for few acids were known at the time, and each was a valuable reagent for the chemist.[40]

In going from a first draft to a revised draft of his paper on arsenic, Cavendish made revealing changes of wording. Whereas in the first draft he expressed his opinions such as his differences with Macquer forcefully, in the revised draft he toned them down. Even in the semi-privacy of a correspondence, Cavendish was cautious. In the revised draft, he combined his experiments with a “hypothesis” that explained them; it is significant that he presented the experiments before the hypothesis, for by this time a priori conjectures were not regarded as the way to advance chemistry. The hypothesis was that all metals including the perfect metals are deprived of their phlogiston when dissolved in acids. Associating arsenic with other “metallic substances,” which by the phlogiston theory are rich in phlogiston, Cavendish accounted for the changes that arsenic undergoes by the readiness with which the attacking acid, spirit of nitre, unites with the phlogiston in arsenic.[41] In keeping with this explanation, Cavendish concluded that “the whole difference” between arsenic and arsenical

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[38] Macquer (1771, 2:642, 649).

[39] Cavendish, “Arsenic,” 1(b), 10, 13. Thorpe, in Cavendish, *Sci. Pap.* 2:299. A.J. Berry (1960, 46–47).

[40] We see the chemist’s dependence on many reagents and testing materials in Cavendish’s study of arsenic. From his well-supplied laboratory, he made use of (in his spelling) distilled vinegar, spirits of salt (hydrochloric acid), oil of vitriol (sulfuric acid), spirit of nitre (nitric acid), aqua fortis (concentrated nitric acid), nitre, syrup of violet (a botanical extract that changes color when exposed to acids or alkalis), tournsol paper (litmus paper, a mix of dyes that turns color when exposed to acids or alkalis), blue vitriol (copper sulfate), green vitriol (ferrous sulfate), solutions of silver, mercury, copper, and iron in nitric acid, solutions of mercury, copper, and iron in concentrated nitric acid, solution of tin in hydrochloric acid, solutions of gold and nickel in aqua regia (mixture of nitric and hydrochloric acids), solution of regulus of cobalt, sope leys (potassium hydroxide), pearl ashes (potash), fixed alkali (potassium carbonate), calcareous earth (whiting, or carbonate of lime), volatile alkali (ammonia), magnesia, earth of alum, sedative salt (boric acid), white flux, sulphur, linseed oil, and charcoal. Cavendish also had at hand pure “rain” water.

[41] Macquer wrote: “Nothing can equal the impetuosity with which nitrous acid joins itself to phlogiston” (1771, 1:11). Cavendish, “Arsenic,” 1(b), 19–20.

acid is that the acid "is more thoroughly deprived of its Phlogiston."[42] The importance of phlogiston in Cavendish's reasoning in chemistry is evident in his earliest research.

We look next at Cavendish's other surviving early chemical research, probably carried out about the same time.[43] The subject was tartar, a hard, thick crust deposited on the sides of wine casks, red or white depending on the color of the wine. Upon purifying, filtering, and crystallizing by evaporation or cold, it forms small, white crystals, "cream of tartar" (potassium hydrogen tartrate), a known acid at the time.[44] Cavendish's interest seems to have been in determining the amounts of alkali in cream of tartar and in soluble tartar (normal potassium tartrate); in the course of his experiments, he isolated tartaric acid. There is a similarity between this problem and the previous one: like arsenic, cream of tartar has a complex nature, a possible reason Cavendish was drawn to them. The stimulus was probably a publication in 1764 by the German chemist Andreas Sigismund Marggraf, who showed that despite its reputation as an acid, tartar contains an alkali.[45] A pupil of Neumann's who was renowned for his precision, Marggraf has been called the "beginner of chemical analysis."[46] An admirer of Marggraf, Hadley said in his chemical lectures that he was "most uncommonly Eminent whether we consider his ingenuity in Contriving, his practical Skill in conducting his Experiments, or his Sagacity and judgment in the Conclusions he draws from them."[47] Cavendish began his chemical researches in contact with one of the best.

In his experiments on tartar, Cavendish made use of equivalent weights. The word "equivalent" was original with him, but the concept went back to the turn of the eighteenth century, to the Dutch physician and natural philosopher Wilhelm Homberg, who introduced equivalent weights as a measure of the quantity and strength of various acids required to neutralize a given quantity of salt of tartar, an alkali. Cavendish determined the quantity of alkali needed to saturate cream of tartar and the equivalent weights of other alkalis, marble and pearl ash (potassium carbonate). Thorpe found Cavendish's work on tartar to be "remarkably accurate."[48]

Both arsenical acid and tartaric acid became known to chemists through publications in the 1770s by the Swedish chemist Carl Wilhelm Scheele, who was celebrated for his discoveries of acids (Figs. 14.9–14.10).[49] If Cavendish had published his experiments on tartar, he would have come before the scientific world as a chemist skilled in chemical synthesis and analysis. Instead he came before it as a pneumatic chemist. Because of his surviving early chemical manuscripts, we can see him move from the one to the other.

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[42] Cavendish made the acid or, in effect, the same thing, the neutral arsenical salt, three ways: distilling arsenic with nitre, dissolving arsenic in concentrated spirit of nitre, and heating arsenic with fixed alkali. All three ways had the same rationale: the effect of exposing a metal (for that is how he regarded arsenic) to an acid or to heat and open air was to deprive it of its phlogiston. "Arsenic," 1(b), 16.

[43] Cavendish performed two sets of experiments on tartar, neither carrying a date, described on unnumbered sheets: "old experiments on tartar," 10 ff., and "new experiments on tartar," 24 ff., plus 6 more sheets. Cavendish Mss II, 2(a) and 2(b), respectively.

[44] Macquer (1771, 1:771–772).

[45] Thorpe, in Cavendish, *Sci. Pap.* 2:301. Cavendish "discovered the true nature of cream of tartar … and its relation to soluble tartar": J.R. Partington (1957, 104).

[46] Thomson (1830–1831, 1:271).

[47] Coleby (1952a, 295).

[48] Thorpe, in Cavendish, *Sci. Pap.* 2:304.

[49] Carl Wilhelm Scheele (1786). Partington (1961–62, 1964, 2:729). Thomson (1830–1831, 2:63). Thorpe surmises that Cavendish's later experiments might have followed Scheele's paper on tartaric acid in 1769, though they could have been earlier, a possible reason he did not publish his own. Cavendish, *Sci. Pap.* 2:302.

Factitious Air

Air was studied scientifically in the seventeenth century by Boyle, J.B. van Helmont, and John Mayow among others, but the branch of chemistry known as pneumatic chemistry did not begin with them. Although some experiments at the time suggested that there were different kinds of air, the early chemists held to the ancient belief of air as an element, and until that belief was seriously questioned, there was little incentive to study the chemical properties of air. Boyle's law relating the pressure and volume of an air was a physical law, which because of its universality reinforced the idea of a single elementary air. The early investigators were also hampered by their inability to collect air in a pure state, a problem which was solved by Stephen Hales early in the next century. From a variety of substances, by means of heat, fermentation, and putrefaction, he freed "fixed air,"or air fixed in liquids and solids, collecting it over water using what he called a "pneumatic trough." When he experimented on air, he measured its volume without however recognizing that airs differ from one another by their solubility in water and by their sources. He studied air quantitatively while ignoring its qualitative features, which he regarded as inessential, because like everyone else at the time he believed in a single air. For this reason the foundation of pneumatic chemistry is usually attributed to Joseph Black, who thirty years later recognized chemically distinct airs.[50] After Black the next major contributors to pneumatic chemistry were the Irish physician David Macbride and Cavendish.

We begin where we left off, with Cavendish's early experiments on tartar. In his *Treatise on [...] Air*, Tiberius Cavallo said that fixed air can be obtained from many substances, giving as examples cream of tartar and salt of tartar, which contain a great quantity of it. As evidence he referred to Cavendish's finding that crystals of salt of tartar contain 423/1000 of their weight of fixed air, and to Priestley's production of 170 ounces by volume of elastic fluid by heating an ounce of cream of tartar, about two thirds of which was fixed air.[51] The release of air from tartar was known to be powerful, capable of bursting into slivers the vessels used in distilling tartar. Cavendish observed "effervescence" in his experiments on tartar. Likewise, in his experiments on arsenic, he observed "effervescence," "air," "vapors," and "fumes." Cavendish did not yet collect airborne substances to be studied in their own right, but in retrospect we see that he was partway to pneumatic chemistry. Direct evidence that his work in pneumatic chemistry connected with his work on arsenic is a theoretical discussion he wrote for his paper on arsenic and rewrote for his paper on factitious air, "On the Solution of Metals in Acids: Digression to Paper on Inflammable Air."[52]

The connection is also evident in his first chemical work to be laid before the Royal Society, in 1764, two years before his paper on factitious air. William Heberden's brother Thomas acquired an alkali from the lip of a volcano, a place where brimstone (sulfur) might be expected but not a salt like the one he found, fossil alkali or natron (a mineral hydrous sodium carbonate). From experiments "made and communicated to me by the Hon. Henry Cavendish," William Heberden set out propositions about ways of making fossil alkali. He said that this alkali differs from the vegetable alkali (potash) by crystallizing upon the addi-

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[50] Aaron J. Ihde (1964, 30–38).

[51] Tiberius Cavallo (1781, 594–596, 606–608).

[52] The title of the paper is not Cavendish's, and in the end he did not publish it. It generalized the conclusion he had arrived at in the published part of his paper on factitious air, which is that acids deprive metals of their phlogiston, which flies off with the acid. His earliest chemical experiments on arsenic have substantial overlap with his study of factitious air through their common concern with phlogiston, metals, acids, and aerial substances.

tion of fixed air (carbon dioxide), and here he cited Black's experiments on magnesia alba (magnesium carbonate), the second to do so, it would seem, just after Macbride. In quotation marks, Heberden stated Cavendish's conclusion, a comparison between fossil and vegetable alkali, finding that the latter has a stronger affinity to the mineral acids than the fossil alkali. It is conceivable that in his chemical examination of a mineral for Heberden, Cavendish's thoughts were directed to pneumatic chemistry. Another possible connection is with his study of tartar: one of his experiments for Heberden included a compound of tartar.[53] To this point in his life, when undertaking something new, Cavendish had always made the first move with his father; this time, coming into print, it was with his father's close friend, another eminent member of the Royal Society, Heberden.

We can see why Joseph Black was important to Cavendish (Fig. 14.5). In 1756 he published an enlarged version of his medical thesis at the University of Edinburgh on magnesia alba. He selected his subject, magnesia alba, to learn if he could acquire a lime water from it that was more effective than the lime water then in medical use. When he found that magnesia did not form a lime water, he abandoned his original project to focus instead on the interesting chemistry of the substance. Twenty-seven years old and an expert experimenter, Black had an advantage Cavendish did not, a great teacher, William Cullen. If Cavendish's father was in some ways an equivalent, there is no evidence that he was particularly drawn to chemistry. Cullen regarded chemistry as a branch of natural philosophy with laws as fixed as those of mechanics, and Black's work in chemistry agrees with this. Like Cavendish, Black was an admirer of Macquer, recommending his text to his students, and of Marggraf, whose essays he said he would rather have written than anything else in the library of chemistry. *Experiments upon Magnesia Alba* was Black's major publication, on which his chemical reputation was based.[54]

Black and Cavendish were similar in a number of ways. Both were methodical, unaffected, cautious in their reasoning, exacting in their research, and alert to careless error. Cavendish was rich, and Black was well-to-do. Both led outwardly uneventful lives. Both made chemistry and heat major fields of research, and in both fields they began with the same subjects, factitious air and specific and latent heats. Both were reluctant to publish, Black even more so than Cavendish. They both shirked correspondence. Otherwise, in their dealings with people, they were not alike. Cavendish was difficult to engage in conversation, and uninterested in any subject that was not scientific. Black was affable, always ready to enter into conversation, serious or trivial. For the whole of his career, Black was a professor, who lectured on his discoveries. If Cavendish had been a professor, his researches, like Black's, would have been spread by his students, and he would have had greater influence on the course of science in the eighteenth century. So far as we know, Black and Cavendish never met.[55]

Black's originality began with his observation that when subjected to fire, magnesia alba loses a substantial proportion of its weight and that the lost portion is mainly a kind of air, or gas (carbon dioxide); he further observed that the loss of weight is recovered when the calcined magnesia alba, a caustic substance he called magnesia usta (magnesium oxide), is recombined with the same air. He showed that this same air, "fixed air" (Hales's term), is found in other alkalis such as chalk (calcium carbonate); when caustic quicklime, which is

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[53] William Heberden (1765). This paper was read at the Royal Society on 7 Feb. 1764.

[54] William Ramsay (1918, 4–5, 14–15). Henry Guerlac (1957, 433–434).

[55] Ramsay (1918, 1–2, 114–115, 133).

produced by calcining chalk with heat, is combined with fixed air (not directly but through a series of steps involving slaked lime, potash, and caustic potash), the chalk is recovered. Black performed an experiment that showed that the air contained in calcareous earths such as chalk is chemically distinct from common air, a novel claim. Beyond that, he had little to say about the properties of the new air, but he recognized in it a widening field for research. He said that the air would probably be the "subject of my further inquiry," but he did not get to it, leaving the field to Cavendish and others. Black's study is significant for proving by means of careful weighing that an elastic fluid is fixed in exact proportions in magnesia alba and related substances. More than anyone before him, Black used the chemical balance to advantage, and in this respect too Cavendish was to follow in his footsteps.[56]

Cavendish's first scientific publication under his own name appeared in 1766 in the *Philosophical Transactions*, an exacting investigation of an experimental field, pneumatic chemistry. Coming ten years after Black's publication on magnesia alba, Cavendish's paper was the next major study of elastic fluids fixed in substances. Called the "first true disciple" of Black's, Cavendish recognized what was important in Black's work and carried it further, introducing novel methods for distinguishing airs and determining their properties. His paper of 1766 "marked the beginning of the systematic study of gases."[57]

For the kind of study it was, Cavendish's paper was unusual, as a glance at the journal shows. His paper was preceded by one by John Michell on determining the degree of longitude at the equator and by a paper on an uncommonly large hernia and followed by an account of the Polish cochineal and four more papers about animals. Cavendish's second paper, in 1767, appeared in similar mixed company: an account of men "eight feet tall, most considerably more" observed near the Straits of Magellan in the country of Patagonia, an account of a locked jaw and a paralysis cured by electricity, and an account of a meteor and another about a swarm of gnats seen at Oxford. In the context, Cavendish's reports of laboratory precision were perhaps the most remarkable.

Instead of the term "factitious" air, Cavendish could have used "fixed," since the usual meaning of "fixed air" then was any sort of air contained in bodies, but he wanted to retain the specific meaning for "fixed air" that Black had used for the air he studied. To avoid confusion Cavendish borrowed Boyle's expression "factitious air," by which he meant "any kind of air which is contained in other bodies in an elastic state, and is produced from thence by art."[58] The names Boyle and Black are revealing. For his work on arsenic and tartar Cavendish's sources were foreign chemists, while in his paper on factitious air and the related paper the next year on fixed air in mineral water, they were British: in addition to Boyle and Black, they were Cotes, Hales, Macbride, and Brownrigg.[59] In the new field, British chemists took the lead.

The paper was three papers published as one, as the title says, "Three Papers, Containing Experiments on Factitious Air." The first paper was received by the Royal Society on 12 May and read on 29 May 1766, on the eve of the long summer recess, and the second and third papers were read on two successive meetings after the recess, on 6 and 13 November.

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[56] Henry Guerlac (1970, 2:173–183).

[57] Guerlac (1957, 454–456).

[58] Cavendish (1766, 77). Black gave a fuller description of "factitious air." "Chemists have often observed, in their distillations, that part of the body has vanished from their senses, notwithstanding the utmost care to retain it; and they have always found, upon further inquiry, that subtle part to be air, which having been imprisoned in the body, under a solid form, was set free and rendered fluid and elastic by the fire." Joseph Black (1898, 16).

[59] Cavendish (1766, 83, 95–96; 1767, 105).

Cavendish drafted a fourth paper but withheld it. The papers, the three published ones and the unpublished fourth, formed a series, their experiments relating to each other by subject, method, apparatus, and theory. Each addressed a certain kind of factitious air produced by certain kinds of processes: inflammable air from metals and acids; fixed air from alkalis by solution in acids and by calcination; mixed airs from organic substances by fermentation and putrefaction; and other mixed airs from organic substances released by distillation. Within the text, the four divisions are called "parts" rather than "papers"; adopting that terminology, we refer to the publication as one paper with four parts.

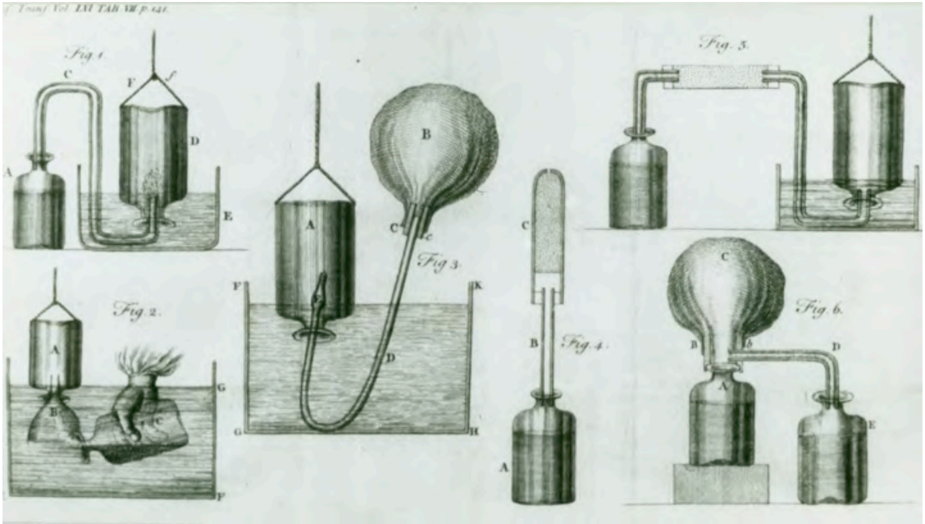

Figure 8.3: Factitious Air Apparatus. The numbered figures are from Cavendish's first publication, for which he received the Royal Society's Copley Medal. Figure 1 shows his technique for filling a bottle D with air. The bottle, containing water, is inverted in the vessel of water E; the air to be captured is generated by dissolving metals by acids and by other means in bottle A. The measure of quantity of air is the weight of the water it displaces in D. Figure 2 shows how air is transferred from one bottle to another. Figure 3 shows how air is withdrawn from a bottle by means of a bladder. The speckled substance in Figures 4 and 5 is dry pearl ash, through which air is passed to free it from water and acid. Cavendish (1766).

Cavendish's techniques for collecting and transferring inflammable and other airs are seen in his drawings (Fig. 8.3). In both spirit of salt (hydrochloric acid) and dilute oil of vitriol (sulfuric acid), he dissolved each of three metals, zinc, iron, and tin, and investigated the air that was released. He found that it was insoluble in water, allowing him to collect it in vessels inverted over water, adapting Hales's pneumatic trough. He assumed that the air came from the metal not the acid, a teaching of the phlogiston theory. The volume of air released depended on the metal, and the air in each case was permanently elastic. In the presence of common air, the new air exploded when lit, a property he investigated further, comparing the loudness of the explosions when the air was mixed with common air in different proportions. He determined the density of the air two ways: one was to weigh a bladder filled with the air and again with it empty, noting the increase of weight (in the case of an air that is lighter than common air); the second way was to note the loss of weight of the combined acid and metal when the discharged air was allowed to escape. He compared the density of several samples of the air obtained using different metals and acids with the density of water and the density of common air, concluding from a mean of his experiments that the air was "8760 times lighter than water, or eleven times lighter than common air,"which given his method is surprisingly close to our value 14.4. When the air was kept in bottles inverted over water, it was capable of holding "near 1/9 its weight of moisture," making the specific gravity of the moist air "7840 times less than that of water."[60] These figures and others served to specify the physical properties of a substance to which Cavendish gave the name "inflammable air," which again was not original. When Cavendish dissolved metals in concentrated instead of dilute oil of vitriol with the aid of heat, he obtained a non-inflammable air, which he regarded as a compound of the acid and phlogiston, the acid depriving the phlogiston of its inflammability, incidently contradicting Stahl.[61] On the day the first part of Cavendish's paper was read, the secretary of the Royal Society wrote in the Journal Book that "it is impossible to do Justice to the Experiments under the title 'On Inflammable Air' without reciting them wholly."[62] We agree with the secretary.

Part II of Cavendish's paper is about "fixed air," the factitious air released by alkalis when dissolved in acids or calcined, our carbon dioxide. As he had inflammable air, he examined fixed air for elasticity, density, solubility in water and in other liquids, and combustibility. Otherwise than being permanently elastic, fixed air had properties distinct from those of inflammable air and common air: it was 1½ times heavier than ordinary air, which being heavier than inflammable air was easier to work with; it did not support fire; it was soluble in water, because of which Cavendish collected it over mercury or caught it directly. Its solubility in water varied, suggesting to him that fixed air obtained from marble "consists of substances of different natures." He determined the quantity of fixed air in several alkaline substances, expressing the results in terms of marble. His use of marble as a standard is shone by the following typical statement: a parcel of volatile sal ammoniac "contained more fixed air, in proportion to the quantity of acid that it can saturate, than marble does, in the proportion of… 217 to 100."[63]

Cavendish's point of departure in Part III was a study of fermented and putrefied substances by Macbride in 1764. Finding that "fixed air" was given off, Macbride concluded that this air plays an essential role as the cement of living bodies. He took his understanding of air from Hales, and in citing Black, he made Black's apparatus and work better known. This was his main contribution to pneumatic chemistry, his interest in the subject being primarily medical and physiological.[64] Cavendish wanted to know if fermentation and putrefaction yielded any factitious air other than what Macbride found, Black's fixed air. He discovered that the air produced by fermenting brown sugar and apple juice with yeast was the same as that produced from marble by solution in acids, "fixed air." The air he ob-

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[60] Ibid., 84–86.

[61] Stahl thought that a compound of phlogiston and an acid was inflammable. Thomson (1830–1831, 2:340).

[62] 29 May 1766, JB, Royal Society 25:876.

[63] Cavendish (1766, 89, 91, 93).

[64] E.L. Scott (1970, 46). Macbride's *Experimental Essays* were published in 1764. Guerlac (1957, 454).

tained from putrefying gravy broth and raw meat he found to be a mixture of fixed air and inflammable air, neither pure.[65]

In Part IV, Cavendish again treated vegetable and animal substances, this time distilling wood, tartar, and hartshorn, obtaining a mixture of non-flammable and inflammable airs. He found that the new inflammable air differed from the inflammable air produced by dissolving metals in acids, his test being the loudness of explosions when the air was mixed with ordinary air and lit. He completed Part IV after writing his second published paper, on a mineral water, since he referred to it there; if he had published it, it would not have appeared with "Three Papers," but later. He said that he intended to follow up Part IV with another publication. His laboratory notes indicate that he returned to this subject later but with no more conclusiveness.[66]

For his experiments on factitious air, Cavendish was awarded the Copley Medal of the Royal Society. Two others received the Copley Medal that year with him, Brownrigg for his analysis of mineral water and Edward Delaval for his study of the colors of metal films. Delaval showed that thin metal deposits on glass differed in color in the order of their density, a study which could be called chemical optics.[67] The year 1766 was the year of the chemists.

In Cavendish's study of factitious air, we see characteristics that will reappear in his later work. One is caution, shown by his wording. The inflammable air produced by putrefaction was "nearly of the same kind" as the inflammable air from metals but "not exactly the same."[68] An intended addendum to Part I is tentatively expressed, "I have not indeed made sufficient experiments to speak quite positively as to this point."[69] Another characteristic is patience; Cavendish inverted a flask of fixed air over mercury "upwards of a year."[70] Another is a mix of quantitative and qualitative methods, weighing air being an example of the former, judging the loudness of explosions an example of the latter. A related characteristic is his focus on physical properties: in addition to loudness, these were elasticity, solubility, and density. Another characteristic is thoroughness: in generating airs, he made use of a range of metals, acids, alkalies, and organic substances. Another is his use of equivalent weights: he measured the volumes of inflammable air from one ounce of each of three metals, from which the equivalent weights of the three metals can be found by assuming a constant volume of the air.[71] Other characteristics have to do with accuracy. He introduced a standard, marble, which he used to express the amount of fixed air in an alkali. He repeated his experiments and took the mean of the results. He estimated accuracies quantitatively: in determining how much fixed air water absorbs, his accuracy was "about three or four 1000th parts of the whole bulk of air introduced."[72] He claimed no greater accuracy for his conclusions than was justified by his experiments: he gave the specific gravities of inflammable and fixed airs to three places, the maximum accuracy for measurements of that

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[65] Cavendish (1766, 98–100).

[66] Henry Cavendish, "Experiments on Air. Part IV," *Sci. Pap.* 2:307–315.

[67] Edward Delaval (1765).

[68] Cavendish (1766, 100).

[69] Cavendish, "On the Solution of Metals in Acids," 305.

[70] Cavendish (1766, 88).

[71] Berry (1960, 51).

[72] Cavendish (1766, 89).

sort.[73] A final characteristic is his use of theory as a guide in his experiments, which brings us to phlogiston.

We look at Cavendish's view of phlogiston at the time of his early work in chemistry. In his paper of 1766, he wrote that when certain metals and acids react, the phlogiston of the metals flies off "without having its nature changed by the acid, and forms inflammable air."[74] Whichever metal he tried, iron, zinc, or tin, and whichever acid, dilute sulfuric or muiatic, he obtained the same air. Thomas Thomson understood Cavendish to have concluded from this that inflammable air from a metal is pure phlogiston.[75] Vernon Harcourt, a later chemist who studied Cavendish's work historically, concluded that Cavendish identified phlogiston with inflammable air "as early as 1766, or very soon after." Cavendish found that there is more than one species of inflammable air, but since the one he obtained from zinc and iron had a constant specific gravity and was constant in its combining properties, "*his Phlogiston* therefore *was* hydrogen and nothing else."[76] The identification of phlogiston in its elastic state with inflammable air is consistent with the experiments he reported in his paper of 1766.

A counter argument can be made. First, there was Cavendish's cautious wording: in 1766 he wrote that phlogiston "forms," not "is," inflammable air. Second, chemists who later identified phlogiston with inflammable air did not credit Cavendish with the idea. In 1782, Richard Kirwan having explained the origin of inflammable air much as Cavendish did went on to prove its "identity and homogeneity with phlogiston," though he also associated phlogiston with Black's fluid of heat, which Cavendish rejected.[77] In 1783, guided by experiments of his own, Joseph Priestley identified phlogiston with inflammable air.[78] What exactly Cavendish thought about the relationship of phlogiston and inflammable air at the time of his first paper we may never know for certain, and Cavendish himself may have believed that his experiments were not decisive on this point. What seems clear is that he was not in serious doubt about the reality of "phlogiston" and its importance in chemistry, as he would later be. In a footnote in Part IV he cited John Hadley, who explained the increase in weight of a metal upon calcination (oxidation) by the absorption of fixed air (carbon dioxide), forestalling a potential and eventually serious difficulty for phlogiston.[79]

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[73] The notion of significant figures had not taken hold everywhere. The chemist William Nicholson said that the best chemical balances were accurate to five or six places, according to claims made for them. In weighing an air, the error was thirty times as great in proportion to the whole as it was in weighing other substances. This means that if a balance was accurate to five places in common weighing, it was accurate to only three places in the case of an air, and because of the complications of temperature and pressure, the accuracy was probably less than three places. Lavoisier nonetheless gave the specific gravities of airs to five places, on which he made calculations to six or eight places, thousands of times their real accuracy in, what James Short (above) called a "pretense" of accuracy. Nicholson's comments in his translation of the notes by French chemists to the French edition of Richard Kirwan (1789, vii–ix).

[74] Cavendish (1766, 79).

[75] Thomson (1830–1831, 2:340).

[76] W. Vernon Harcourt (1839, 28).

[77] Richard Kirwan (1782, 195–197).

[78] Joseph Priestley (1783, 400).

[79] In the footnote, Cavendish says that Hadley distilled the salt sal ammoniac with red lead, or lead oxide, and also with bare metal, and that the different results show that metals contain no fixed air, or carbon dioxide, and that metallic calces, or oxides, contain a great deal. He says that the reason that minium, another name for lead oxide, weighs more than the bare metal lead is that lead absorbs fixed air on being converted into minium. In the manuscript of Hadley's lectures, we find what Cavendish refers to here: Hadley says that 100 pounds of lead give 110 pounds of minium, and that the increased weight is due to the fixed air united to the minium. The reference to Hadley

Following the work of Black, in his first published paper Cavendish helped to discredit the ancient idea of a single, a universal air. He showed that inflammable air and fixed air differ from one another and from common air, and that one of them, inflammable air is a single, uniform substance. He failed to recognize that like inflammable air, fixed air is a single substance, but the incompleteness of his analysis of this and other kinds of air only reveal the difficulty of the field at this early stage. His contribution to pneumatic chemistry was to have made the first attempt "to collect the different kinds of air, and endeavor to ascertain their nature."[80] By introducing methods for isolating and characterizing different kinds of air, he provided a "model to future experimenters," opening new avenues for research. The Scottish chemist Thomas Thomson, who was inspired by Black to take up the study of chemistry, wrote that Cavendish "first began the true investigation of gases," extending the bounds of pneumatic chemistry, with the caution and precision of a Newton."[81]

Cavendish's contribution to pneumatic chemistry can be contrasted to Priestley's. He did not discover new airs, which in any case was not his objective. An example makes the point. In the course of an experiment, he dissolved copper in muriatic acid (HCl) assisted by heat, producing an air that was soluble and not inflammable air, a new kind of air, but he did not examine it further. When Priestley read about this "remarkable kind of air" in Cavendish's paper, he "was exceedingly desirous of making myself acquainted with it." He collected the air over mercury and performed experiments on it, discovering a new air, "muriatic acid gas."[82] The air that Cavendish studied most thoroughly, and which he is most closely identified with, inflammable air, he did not discover; it had been known from Boyle's time, though it was confused with other airs we can identify now.

In the following year, 1767, Cavendish published an analysis of water obtained from a location near Soho Square, Rathbone-Place.[83] Having a practical use, mineral water was a familiar object of chemical study, though Cavendish's interest would seem to have been purely scientific. The chemist William Lewis wrote in 1759 that the analysis of mineral waters was held back by a great many experiments "more ostentatious than useful" and "for the most part fallacious," very different waters giving similar appearances because of faulty methods. He laid out a "simple and obvious method" of going about the analysis: first distill the mineral water, then separately analyze the distilled water and the residuum, which consists of soluble salts and insoluble earths, and lastly separate the salts by crystallization or directly by adding chemicals.[84] Cavendish's first two experiments followed these steps exactly, but the other experiments were about fixed air, calling for methods appropriate to this elastic substance.

The occasion for his study would seem to have been a paper in the *Philosophical Transactions* in 1765 by William Brownrigg, whom we mention earlier in the book where we dis-

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shows that Cavendish and Hadley were aware that the increase in weight on the calcination (oxidation) of metals was a problem and that phlogiston, as they understood it, could not solve it: they thought (incorrectly) that fixed air (carbon dioxide) was the explanation. Hadley's statement is based on Macquer's book on the elements of chemistry, though Macquer does not give an explanation for the increase in weight, commenting only on the "numerous ingenious but not altogether satisfying explanations." Hadley's explanation takes into account the experiments on airs by Stephen Hales and Joseph Black. Page 208 of the manuscript lectures, quoted in Coleby (1952a, 299).

[80] A.L. Donovan (1975, 219). J.R. Partington (1961–62, 3:316).

[81] Thomson (1830–1831, 2:1, 343).

[82] Ibid. 2:341. Joseph Priestley (1772b, 234–235).

[83] Henry Cavendish (1767).

[84] William Lewis, in Neumann (1759, 252–253).

cuss Charles Cavendish's executorship of the Lowther estate in Cumberland. Brownrigg has a place in the early history of pneumatic chemistry, which if not of equal importance to that of Black, Macbride, and Cavendish merits our attention all the same. His distinction is to have been the first to undertake a systematic study of dangerous air in coal mines. A native of Cumberland, he studied medicine in Leiden while Boerhaave was teaching, obtaining a doctorate there, and upon his return he set up practice in Whitehaven, in a coal-mining region. He married the daughter of John Spedding, steward to the estate of Sir James Lowther, whose personal physician he became. A few years earlier, in 1737, an explosion in one of Lowther's coal mines killed nearly two dozen men, and Brownrigg treated the injured, the background to his interest in two related questions, how to prevent explosions in mines, and how to treat miners who were poisoned by the fumes. In 1733 and 1736, he developed ways of transferring and collecting coal "damps" and provided Lowther with bladders filled with it to submit to the Royal Society.[85] In 1741 and 1742 Brownrigg presented a series of papers to the Society on explosive "fulminating damp" and on suffocating "choak-damp," on the basis of which he was elected to the Royal Society. With the backing of Lowther's colliery steward Carlise Spedding, in 1743 he proposed setting up a laboratory near one of the pits for him to carry out experiments on explosive and poisonous airs. Lowther agreed to pay half the cost of it. After a visit to a spa in Europe, Brownrigg prepared a paper on the air released from the water he found there, which he identified with the choke damp he had been studying, a "particular kind of air, or permanently elastic fluid" distinct from common air. He speculated correctly that the repulsive particles released from various kinds of dense bodies vary from one another, often composing "elastic fluids, which differ as much from each other, as those bodies differ from which they are produced…. So that two elastic fluids, although they both possess a repulsive quality, may yet in their other qualities differ as much as inelastic fluids [vapours] are found to differ." He had a clear notion of chemically distinct airs, the insight of pneumatic chemistry. His paper on the spa water, an extension of a paper read to the Royal Society in 1741, was published in the *Philosophical Transactions* in 1765 and awarded the Copley Medal the following year.[86] Cavendish would have been interested in Brownrigg's paper about air in mines and in mineral water, which was what his paper in 1767 was mainly about. Further evidence of his interest is a paper on damps written by Brownrigg for Lowther found among Cavendish's manuscripts.[87]

Produced by a spring, Rathbone-Place water until a few years before had been raised by an engine for public distribution in the neighborhood. Now a pump remained, from which Cavendish drew his sample, which he described as "foul to the eye," forming a "scurf" over time. To see if what Brownrigg found in the spa water was true of Rathbone-Place water, Cavendish evaporated a sample of it and analyzed the airs given off. Separating off the fixed air, he mixed the remaining air with inflammable air and lit it. From the loudness of the explosion, he determined that the water contained a quantity of ordinary air as well as a quantity of fixed air. He arrived at the answer to the question he began with: the reason for the suspension of calcareous earth in the water was "its being united to more than its

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[85] This was in 1733. "Sir James Lowther, 4th Baronet." Anon.,"William Brownrigg" (https://en.wikipedia.org/wiki/William_Brownrigg). Thomas Young (1816–1824, 436).

[86] William Brownrigg (1765, 218–219, 238); on 336–343 is an extract from a paper read to the Royal Society in 1741, from which the new paper was written. J.V. Beckett (1977a, 255–258). J. Russell-Wood (1950, 436–438).

[87] "Some Observations upon the Several Damps in the Coal Mines near Whitehaven by Dr Willm Brownrig Phisitian of that Town Communicated by Him to Sr James Lowther Bart," Cavendish Scientific Manuscripts, Devon. Coll., Chatsworth, Misc. Hereafter Cavendish Mss.

natural proportion of fixed air." When the fixed air was driven off, the earth was immediately precipitated.[88] Cavendish's examination of solubilities (of certain bicarbonates) can be seen as a continuation of his study of fixed airs. His analysis of Rathbone-Place water listed the impurities by weight in one pint of the water: fixed air, unneutralized earth (magnesium and calcareous earth), volatile alkali, selenite, and a mixture of sea salt and Epsom salt, the total solid contents coming to $17\frac{1}{2}$ grains. Cavendish concluded his study by examining three other London waters, including water from a pump near his father's house on Great Marlborough Street.

Cavendish's analysis of a mineral water was the first that could claim "tolerable accuracy," Thomson said.[89] Writing about the analysis of waters a few years later, the Swedish chemist Torbern Bergman said that it was "one of the most difficult problems in chemistry" because there were so many impurities in the water and the quantities were so small.[90] It was a problem to show Cavendish's skills as a chemist once again.

Instruments and Meteorology

By Cavendish's time, the craft of instrument making was highly advanced. Aided by improvements in materials and the graduation of scales, instrument makers kept up with (and stimulated) the demand for better instruments.[91] Living in a city with a flourishing trade in instruments, Cavendish could conveniently inspect, buy, and commission the thermometers, telescopes, and other tools he needed for his research. At some stage, he employed an instrument maker of his own. His interest and skill were recognized by the Royal Society, which regarded him as its resident authority on matters having to do with instruments of all kinds.

Because he was wealthy, Cavendish could buy any instrument he wanted, and because his scientific interests were wide-ranging, he owned a large number of them. In 1816, six years after his death, his collection was put up for auction. At the time, Cavendish was too recent for his instruments to be collected as memorabilia, and his name was not mentioned in the auction catalog, only a "Gentleman Deceased." The makers of the instruments not their owners were important to buyers: an air pump by Nairne and Blunt, a thermometer by John Bird, and a theodolite by Jesse Ramsden. Because the instruments used by Cavendish in the 1780s were still in use at the time of the sale, the unnamed buyers would have been persons with a scientific object. By the time of the auction, the collection had been well picked over, leaving behind a miscellany, telescopes, hygrometers, and thermometers (forty-four of them). The catalog lists ninety-one numbered items, some of which are multiple; all told, it lists 150 instruments together with bottles, retorts, and maps. At the time of Cavendish's death, his instruments were valued at £544; at the auction sale, they brought £159, a measure of the depletion of his collection by then.[92]

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[88] Cavendish (1767, 105, 107).

[89] Thomson (1830–1831, 2:344). Berry writes, "Truly indeed was Cavendish the founder of water analysis." (1960, 57).

[90] Torbern Bergman (1784, 109).

[91] Maurice Daumas (1963, 421–424).

[92] "Extracts from Valuations of Furniture," *A Catalogue of Sundry Very Curious and Valuable Mathematical, Philosophical, and Optical Instruments ... Of a Gentleman Deceased ... On Saturday the Fifteenth of June 1816, at Twelve O'clock*, Devon. Coll.

Accurate measurements in Cavendish's main experimental fields, electricity, chemistry, and heat, and in his main observational field, meteorology, began to become important around the time he began to do research, the 1760s and 70s. Researchers did not yet depend on great accuracy in their measurements, but physical theory, quantifiable concepts, and standards of work all pointed in that direction.[93] Colleagues considered Cavendish to be accurate in his work, by which they meant that he took care to come as close to the truth as was possible given the means available to him. They understood that what constituted accuracy and precision varied over time.

All instruments are imperfect in their infancy, J.A. Deluc said, and though they never achieve perfection, they approach ever nearer to it; the ordinary watch becomes Harrison's precise timekeeper, and the ordinary balance becomes the precise scales of the chemist.[94] The gradual approach to perfection was the instrument maker Jesse Ramsden's guide to practice: sensible that the "theory" of astronomy was held back not by the nature of its instruments but by their imperfection, he was "always inclined to improve rather than invent," except when he was convinced that the imperfection of an instrument lay in the principle of its construction.[95] Cavendish implicitly agreed with Ramsden, for he too was an improver of instruments, not an inventor.

To see how Cavendish worked with instruments, we consider those he used in studying the weather. His colleague Richard Kirwan traced the origins of the science to the invention of the thermometer and barometer, attributing its slow development to the imperfections of the instruments and also to the interruptions of the historical record of the weather. He intended his book as a step in the direction of a "theory of the winds," which he regarded as the object of meteorology, the first step of which was to connect the diverse phenomena of the weather by taking measurements of the weather at all latitudes and longitudes in both hemispheres. The single most important measurement of the weather is the temperature, which causes the winds, which in turn affects the temperature, determining the "state of the atmosphere." The science of the weather differed from most other sciences in that it did not enable people to "alter the spontaneous course of nature, except in a very few cases," such as in the promotion of vegetation and the drainage of morasses. In this respect, it was like astronomy, and like astronomy, which predicts the motions of the planets, a perfected meteorology would "foresee those changes [in the weather] we could not prevent."[96] We have no way of knowing if Cavendish's understanding of meteorology differed in any important way from Kirwan's, but we know that he regarded the science in its current state as incapable of prediction, unlike astronomy. His brother Frederick told him that he read in the paper that Herschel predicted a wet end-of-summer. Henry, who had read the paper too, told his brother that Herschel could have said no such thing since he had "too much sense to make predictions of the weather."[97] Henry knew his astronomical colleague Herschel, who earlier complained that the "papers have ascribed to me a foreknowledge of the weather [...] which I am not so happy as to be in possession of."[98]

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[93] Daumas (1963, 418, 428–430).

[94] Jean André Deluc (1773, 430–432).

[95] Jesse Ramsden (1779, 419).

[96] Richard Kirwan (1787, v–vi).

[97] Frederick Cavendish to Henry Cavendish 10 Sep. 1809; Henry Cavendish to Frederick Cavendish, n.d., draft; in Russell McCormmach (2014, 260).

[98] William Herschel to Lord Salisbury, late Jan. 1789, Royal Astronomical Society, Mss Herschel, W 1/1, 170–171.

Like many other serious students of the weather before and after him, Cavendish designed a better wind measurer. Having commissioned the firm of Nairne and Blunt to build it, he requested the employee who made the instrument to be present when he came to pick it up. Cavendish "insisted upon his taking the whole apparatus to pieces, and then, by means of a file and a magnifying glass, he tested the pinions to see that they were properly hardened and polished, and of the right shape, according to his written directions."[99] We suppose that during the inspection of the pinions, the instrument maker felt some anxiety, but since the account ends here, we also suppose that the outcome was favorable to all parties. At Nairne and Blunt's, Cavendish was both a demanding customer and a frequent one, whose behavior would have been familiar and more than tolerated, his patronage of the firm serving as an advertisement for it. Edward Nairne was Cavendish's all-purpose instrument maker of choice, and also an experimental collaborator of his and fellow of the Royal Society. Thomas Blunt began as an apprentice to Nairne and then became a partner.[100]

A specific reason why Cavendish commissioned Nairne and Blunt to build a wind measurer may have been that they had recently built a portable wind gauge for use at sea for James Lind, physician to George III. This instrument was the best of its kind, which was the kind of nearly all early wind gauges. They were, in effect, pressure gauges, used by seamen who were interested in that property of the wind, its pressure.[101] The inspiration of Cavendish's earliest experiments may have come from Alexander Brice, who measured the velocity of wind by observing the motion of the shadows of clouds, his answer to the irregularities in the velocity of wind as determined by light objects such as feathers carried along in the breeze.[102] Cavendish thought that Brice's experiments published in the *Philosophical Transactions* in 1766 were "ingenious" but incomplete, since he failed to measure the wind on the ground in an open place to discover if there is a difference in wind velocity at the surface of the Earth and high above it, and he also failed to observe the angular velocity of the clouds at the same time as he observed their shadows, which would have determined their perpendicular altitude. "The most convenient way I know of measuring the velocity of the wind," Cavendish wrote to an unnamed correspondent, "is by a kind of horizontal windmill with rack work like that used for measuring wheels to count the number of revolutions it makes…. it will be easy finding by experiment the actual number of revolutions which it makes while the wind moves over a given space."[103] Cavendish's wind measurer was a horizontal windmill, built nearly on the scale of the familiar vertical windmill with the revolving arm measuring eighteen feet. This was the kind of instrument Cavendish commissioned Nairne and Blunt to build, described as "a train of wheels worked by a vaned fly."[104] It was of a different kind of wind measurer than the seamens' pressure gauges, one suited for meteorology in the tradition of the vane-mill (re)invented by Robert Hooke in the previous century.[105] Because Cavendish's method was to count the number of revolutions

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[99] The account of Cavendish originated with the instrument maker John Newman, of Regent Street, in Wilson (1851, 179).

[100] On Edward Nairne and Thomas Blunt: E.G.R. Taylor (1966, 62, 214, 256).

[101] A. Wolf (1961, 1:320–323).

[102] Wolf (1961, 1:324).

[103] Henry Cavendish to "your Lordship," undated, Cavendish Mss, Msc.

[104] Wilson (1851, 179).

[105] William E. Knowles Middleton (1969, 203). Before Robert Hooke, the Italian architect Leon Battista Alberti invented a mechanical wind measurer, consisting of a disc oriented perpendicularly to the wind mounted on an arm free to rotate. Hooke's device was similar.

corresponding to winds of different strengths, the accuracy of the pinions he insisted on inspecting at Nairne and Blunt's was key to the accuracy of the instrument across a wide range of wind velocities. Among his manuscripts are trials of the "Measurer of Wind" with dates scattered through them, in 1768–69, and twenty years later, in 1788.[106] He described the capability of the wind measurer: "By the help of such an instrument one might easily find the velocity of the wind at any time & if one had a mind to keep a register of its velocity almost as easily as one can that of the thermometer."[107] Ideally, a complete weather journal would record the velocity of the wind in addition to its direction, which was then routinely observed by the weather vane. Complex and cumbersome wind measurers were invented and reinvented throughout the century, without leading to a standard practice. By the procedures recommended by Cavendish for recording the weather at the Royal Society, the strength of the wind was denoted numerically, but only by rank: 0, 1, 2, and 3 stood for "no wind," "gentle," "brisk," and "violent or stormy."[108] To determine the strength, Cavendish advised observing how smoke was blown or listening to how the wind sounded,[109] a qualitative estimate. Like other patient observers of the weather, Cavendish probably desired greater exactness and settled for less.

There had long been instruments for tracking the weather—weather vane, rain catch, and even a crude indicator of humidity—but these did not make the study of the weather scientific. By Cavendish's time, it was understood that a science of the weather required measuring instruments capable of reasonable accuracy. Besides the barometer, the most important of these was the thermometer,[110] which was the subject of Cavendish's first assignment by the Royal Society, in 1766.

The rudimentary state of thermometry at the beginning of the eighteenth century is suggested by Newton's experiments with a linseed-oil thermometer and a scale fixed by two points, the heat of the air standing above water when it begins to freeze, and the heat of blood, from which Newton extrapolated freely to high temperatures.[111] Nearly forty years later, Robert Smith, who translated Newton's directions for making thermometers, observed that none of the thermometers he had seen had been tested for comparability,[112] still largely the state of affairs when Cavendish studied thermometers thirty years after Smith. There was a variety of scales in use and a wide variation in their adjustment.[113]

The precision of a thermometer—the fractions of a degree to which it could be read—had little meaning in practice owing largely to an uncertainty in the upper fixed point. Cavendish (probably with other fellows) tried a number of thermometers built by leading instrument makers, Bird, Ramsden, Nairne, and George Adams, finding that they differed in their readings of the boiling point of water by two or three degrees. Astronomical precision in meteorology was not regarded as important or obtainable, but a disparity of that magnitude in the boiling point of water was unacceptable. Cavendish recognized that to ensure the consistency and compatibility of readings with instruments used by different observers, it

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[106] Henry Cavendish, "No. 1. Measurer of Wind," and "Trial of Windgauge," Cavendish Mss, Misc.

[107] Cavendish to "your Lordship."

[108] Henry Cavendish (1776b).

[109] 9 Dec. 1773, Minutes of Council, Royal Society 6:202.

[110] Richard Kirwan (1787, iii).

[111] William E. Knowles Middleton (1966, 57–58).

[112] Robert Smith, "The Editor's Preface," in Roger Cotes (1747).

[113] Middleton (1966, 65, 75, 115). Britain and Scandinavia used the Fahrentheit scale, while on the Continent, the Réaumur, Delisle , and Swedish scales were used. Kirwan (1787, vi).

was necessary for all of the mercury in the thermometer to be heated equally. He carried out experiments to determine if the upper fixed point of a thermometer scale is affected by the rapidity of boiling of the water and by the immersion of the thermometer either in the boiling water or in the steam above the water. His experiments showed that the rapidity of boiling was not a factor and that immersing the thermometer in steam was more exact and convenient than immersing it in boiling water. In fixing the boiling point, the entire bulb and column were to be exposed only to the steam or else the bulb of the mercury column was to be just barely submerged, since at any appreciable depth it would be compressed, giving a reading that was too high.[114]

The Royal Society called upon Cavendish's skill with meteorological instruments again in 1773, this time to draw up a plan for taking daily meteorological readings and keeping a journal or register of the weather.[115] Weather journals began to appear with some frequency in the *Philosophical Transactions*, coming to outnumber isolated weather reports by the late eighteenth century. They were a means to the end, as the weather-journal advocate William Borlase put it, of making "more perfect Theories of Wind and Weather in our Climate" or else of showing the "uncertainty and vanity of all such attempts."[116] What Charles Hutton wrote in his scientific dictionary at the end of the eighteenth century could have been said at any time during the century:

> There does not seem in all philosophy any thing of more immediate concernment to us, than the state of the weather.... To establish a proper theory of the weather, it would be necessary to have registers carefully kept in divers parts of the globe, for a long series of years; from whence we might be enabled to determine the direction, breadth, and bounds of the winds, and of the weather they bring with them.... We might thus in time learn to foretell many great emergencies; as, extraordinary heats, rains, frosts, draughts, dearths, and even plagues, and other epidemical diseases.[117]

At once a challenge to science and a vital issue to humanity, the weather was the kind of problem the Royal Society regarded as its reason for being, meteorology embodying its early belief in the advancement of science and human welfare through natural histories. The means in the late eighteenth century was weather registers like the Royal Society's.

To keep the register, Cavendish directed the clerk of the Society to read the barometer and indoor and outdoor thermometers the first thing in the morning and again at midday and in the evening, and every morning to measure how much rain had fallen, every afternoon to estimate the wind, and one fortnight a year to consult the Earth magnetic variation and dipping needles four times a day. (Because the magnetism of the Earth draws the needle not only north but also down, there are two kinds of instruments, the variation compass and the dipping needle.) The clerk was also directed to calculate an involved series of means of readings. He was to set down the mean morning and midday heats for each month, the mean

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[114] Henry Cavendish (1921a, 2:351–353); Cavendish (1776b, 115). William E. Knowles Middleton (1964, 132). Middleton dates the increase in accuracy of calibration from about 1770, the time we are considering.

[115] The Council ordered the clerk of the Society to make daily observations of the weather "with the instruments to be procured for that purpose, & proper accommodations under the inspection of the Hon. Henry Cavendish." 22 Nov. 1773, Minutes of Council, Royal Society 6:197.

[116] J. Oliver (1969, 291).

[117] Charles Hutton (1795–1796, 2:677).

heat for each year, and the mean height of the barometer and the mean heat of the thermometer placed near it for each month and each year. Following Cavendish's recommendation, the register was printed at the end of the last part of the *Philosophical Transactions* for each year, beginning with the weather in 1776; the annual readings were set out in nine columns, including one for the date. So that members did not have to wait until the end of the year to learn what the weather had been, the clerk was ordered to post the previous week's record in the public meeting room of the Society.[118]

The Royal Society's "Meteorological Journal," as Cavendish called it, was a conventional journal in the features of the weather it reported: temperatures, pressures, and the like. It did not contain a chemical column for the composition of atmospheric air, and in a few years Cavendish would show that there was no need for such a column, for the composition was unchanging. Nor did it contain electrical columns, though there was some interest in this. Recently the atmosphere had taken on a new complexity and interest as an electrical medium, and prosaic events such as fog and falling weather and spectacular phenomena such as lightning, thunder, auroras, meteors, earthquakes were observed with that in mind. William Henly, inventor of an electrometer Cavendish used, urged readers of the *Philosophical Transactions* to keep an "electrical journal" of the weather, as he did: "Let a large book be provided, and ruled in the manner of a bill-book, used by tradesmen …." The entries in the columns would be the same as in the standard weather journals except for a new measurement, the divergence of the balls of an electrometer, and a new observation, the type of electricity. Henly recommended another new standard measurement, the temperature of the upper air in all kinds of weather, for which he thought Charles Cavendish's self-registering minimum thermometer carried as high as possible by kites would serve.[119]

Even without the complications of electrical and upper-air measurements, the keeping of the Royal Society's weather register was demanding, requiring the clerk to make multiple observations at different times of the day. Less confining would have been fully automatic clock-driven instruments, which were already an old idea. Christopher Wren in the previous century had proposed a "weather clock," and Robert Hooke had developed the idea into a futuristic meteorograph using punches on rolled paper.[120] Cavendish had ideas of this sort, though in connection with a thermometer only: he considered an elaborate mechanical contrivance for recording the temperature every ten minutes on a rotating barrel, making a carefully ruled drawing to scale, probably for his instrument maker.[121] He owned a self-registering meteorological instrument, a dial-type thermometer, not original with him, in which a bulb containing alcohol was connected to a U-tube containing mercury. A heavy pointer registered the temperature at the time, and two lighter pointers moved by the heavy pointer registered the maximum and minimum temperatures (Fig. 8.4).[122]

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[118] "The following scheme drawn up by the Hon. Henry Cavendish for the regulating the manner of making daily meteorological observations by the Clerk of the Royal Society …," 9 Dec. 1773, Minutes of Council, Royal Society, 6:200–204. "Meteorological Journal Kept at the House of the Royal Society, by Order of the President and Council," *PT* 67 (1777): 357–384.

[119] William Henly (1774, 426–427).

[120] Middleton (1969, 254–255).

[121] Henry Cavendish, "Clock for Keeping Register of Thermometer," Cavendish Mss IV, 1.

[122] This instrument was calibrated at Chatsworth in 1779, more or less dating it. Charles Cavendish could have designed it, but at that late date it was more likely Henry Cavendish, if it was not an instrument maker. Through Humphry Davy this instrument eventually passed to the Royal Institution, where it is kept in its collection of historical instruments. Middleton (1966, 138–139). Cavendish, *Sci. Pap.* 2:395–97. Among Cavendish's manuscripts

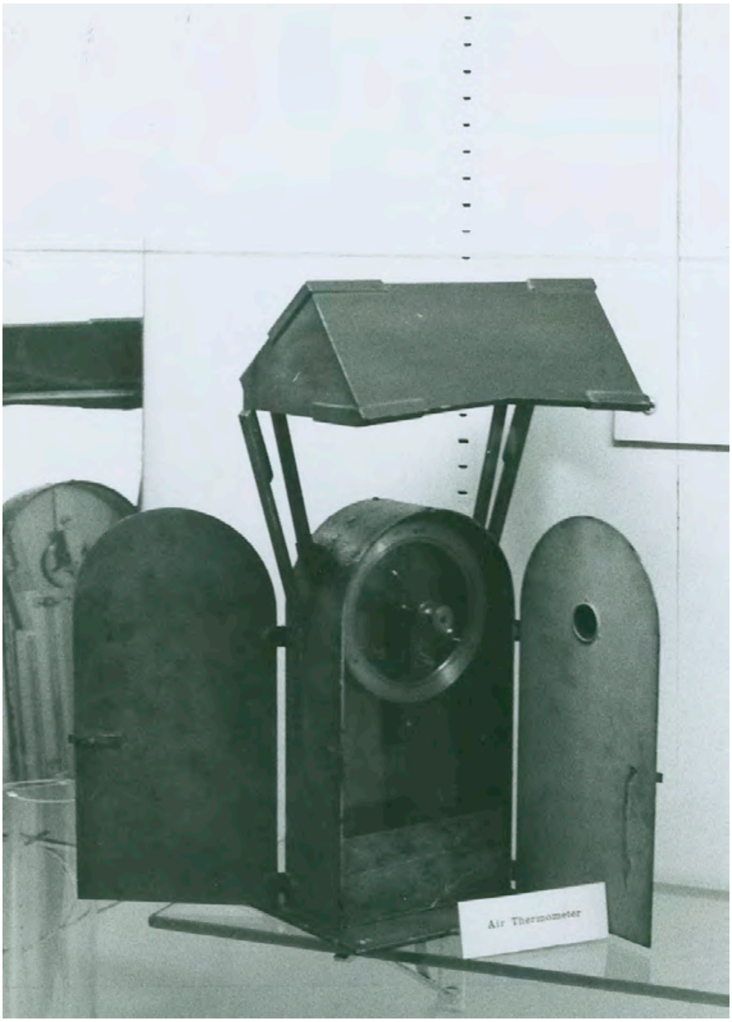

Figure 8.4: Register Thermometer. Photograph by the authors. Cavendish's original instrument is in the Royal Institution, a gift of Humphry Davy's. Alcohol contained in a large tube expands with heat, causing mercury in the U-end of the tube to move. Through a cord attached to an ivory slip on the surface of the mercury, a hand moves across a circular scale graduated in degrees of heat. This hand in turn moves light friction hands, which remain at the maximum and minimum heats for any one setting of the instrument. A description of the instrument together with an engraving of it is in George Wilson (1851, 477–478).

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is "Thermometer for Greatest Heat by Inverting the End of Tube into a Movable Cyl. Of Spt. & Water," Cavendish Mss III(a), 14(c).

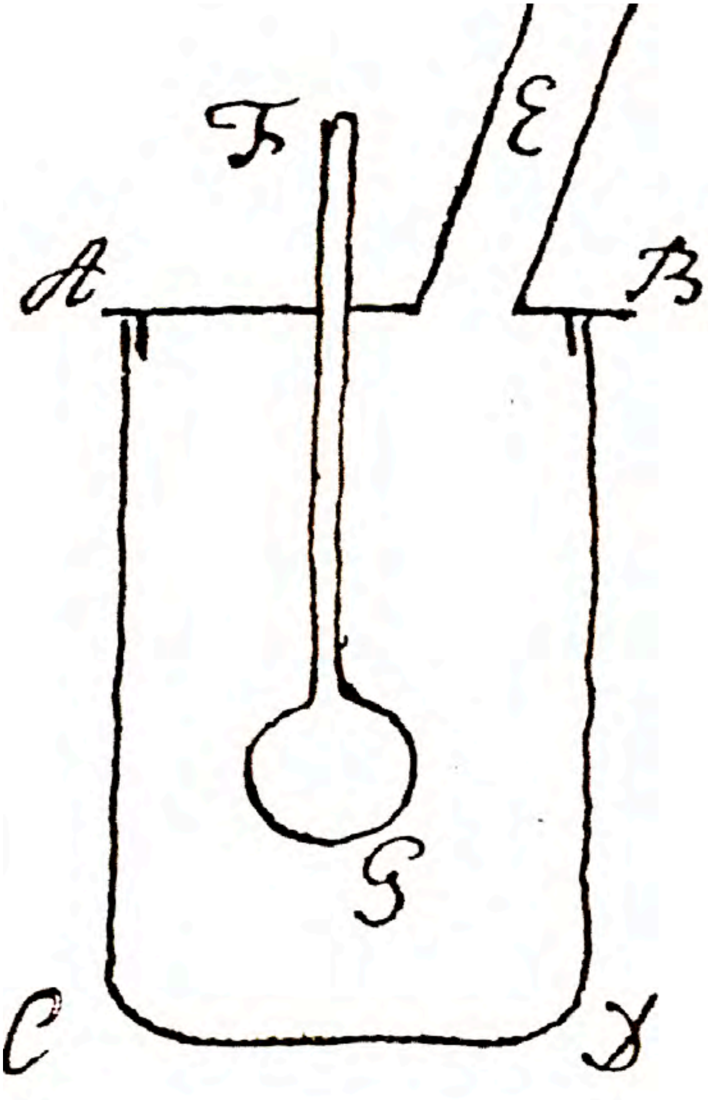

Figure 8.5: Apparatus for Adjusting the Boiling Point. The committee of the Royal Society, which Cavendish chaired, conducted experiments to determine the regularity of the boiling point. ABCD is the pot, AB the cover, E the chimney to carry off steam, FG the thermometer fitted tightly to the cover. The stem of the thermometer as well as the ball are immersed in steam, not water, in accord with Cavendish's recommendation. The committee recommended this apparatus, including an almost identical drawing, in its published paper. "The Report of the Committee Appointed by the Royal Society to Consider of the Best Method of Adjusting the Fixed Points of Thermometers; and of the Precautions Necessary to Be Used in Making Experiments with Those Instruments," *PT* 67 (1777): 816–857, opposite 856. The drawing by Cavendish is in Cavendish Mss III(a), 2. Reproduced by permission of the Trustees of the Chatsworth Settlement.

In 1776 Cavendish together with Aubert, Maskelyne, and Nairne was appointed a committee to "examine into the state of the Society's instruments."[123] Meanwhile a larger committee of seven was formed with Cavendish as chairman to examine the "best method of adjusting the fixed points of thermometers" and the precautions to be taken in "making experiments with those instruments." The other members of the committee were Maskelyne and Aubert, who as astronomers necessarily concerned themselves with temperature and also constantly with instruments; Samuel Horsley, a mathematician, astronomer, and avid observer and analyst of the weather; William Heberden, who kept a meteorological journal; the Swiss meteorologist J.A. Deluc, the most important member other than Cavendish, who had published an influential work calling for the perfection thermometers; and the secretary of the Society Joseph Planta. It was recognized that two fixed points on a thermometer were better than one, with melting ice universally used for the lower fixed point.[124] The recommendation by the committee on the upper fixed point was drawn from Cavendish's earlier report. Because it was known that the boiling point varies with atmospheric pressure, the committee specified a standard pressure to be used when adjusting the fixed point, 29.8 English inches of mercury, giving a formula to be used when the adjustment was made at a different pressure. The committee's paper, which at least in part was written by Cavendish, as we know from his manuscripts, was published in the *Philosophical Transactions* in 1777.[125] (Fig. 8.5). What Cavendish said about the adjustment of the upper fixed point on the scale of a thermometer applies to his overall effort in meteorology: "It is very much to be wished, therefore, that some means were used to establish an uniform method of proceeding; and there are none which seem more proper, or more likely to be effectual, than that the Royal Society should take it into consideration, and recommend that method of proceeding which shall appear to them to be most expedient."[126] Apart from its implicit justification of a national scientific society, Cavendish's wish supported Kirwan's belief that no other science required "such a conspiracy of nations" as meteorology,[127] demanding a uniformity of practice of observers around the world. The method of adjusting the upper fixed point recommended by the committee was made standard on the authority of the Royal Society, and it has been used ever since.[128]

Cavendish published a full account of the meteorological instruments of the Royal Society in the *Philosophical Transactions* in 1776, beginning with the thermometer, the instrument he had examined for the Society ten years before. He again explained the need to immerse the mercury in the stem as well as in the bulb of the thermometer in the steam of boiling water when setting its upper fixed point. He described the proper method for reading the barometer, making corrections for the capillary depression of mercury in the tube based upon his father's observations, though it seems that Cavendish made the calculations for the table he included. To determine if the variation compass was affected by any iron work in the Society's house, Cavendish removed the instrument to the large garden "belonging to a house on Great Marlborough Street," no doubt his father's house, distant from any iron work. He compared the compass readings in the two locations, finding that in the Society's

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[123] 14 Nov. 1776, Minutes of Council, Royal Society 6:303.

[124] Middleton (1966, 116–117, 127). Douglas W. Freshfield and H.F. Montagnier (1920, 176–177).

[125] Signed by Cavendish (listed first), Heberden, Aubert, Deluc, Maskelyne, Horsley, and Planta (1777).

[126] Cavendish (1776b, 115).

[127] Kirwan (1787, iv).

[128] Middleton (1966, 128).

house the needle was drawn aside $15\frac{1}{2}$ minutes toward the northwest by the iron work in the vicinity. He told how to determine the "error of the instrument" by inverting the magnetic needle of the compass. He discussed an "error" of the dipping needle, which he regarded as an "unavoidable imperfection": the ends of the axis of the needle of this instrument rolled on horizontal planes, the error arising from the ends of the axis not being truly cylindrical. In this case, Cavendish was satisfied that the Society's dipping needle was "as least as exact, if not more so, than any which has been yet made." As he had with the variation compass, Cavendish removed the dipping needle to the garden on Great Marlborough Street to determine the true dip, finding a difference of 7 minutes, showing that the dipping needle in the Society's house was not much affected by nearby iron work. "Accuracy" in the recording of the weather, a first consideration in making meteorology more scientific, was improved by raising the funnel collecting rain above the roof of the Society's house where there seemed "no danger of any rain dashing into it," and by sheltering the hygrometer from the rain and locating it "where the Sun scarce ever shines on It," leaving it open to the wind. Acuracy was also improved by taking the mean of observations, by applying corrections such as Deluc's corrections of the barometer by the thermometer, and by modifying instruments; for example, by preventing the vibration of the needle of the variation compass from disturbing the observation of the needle.[129]

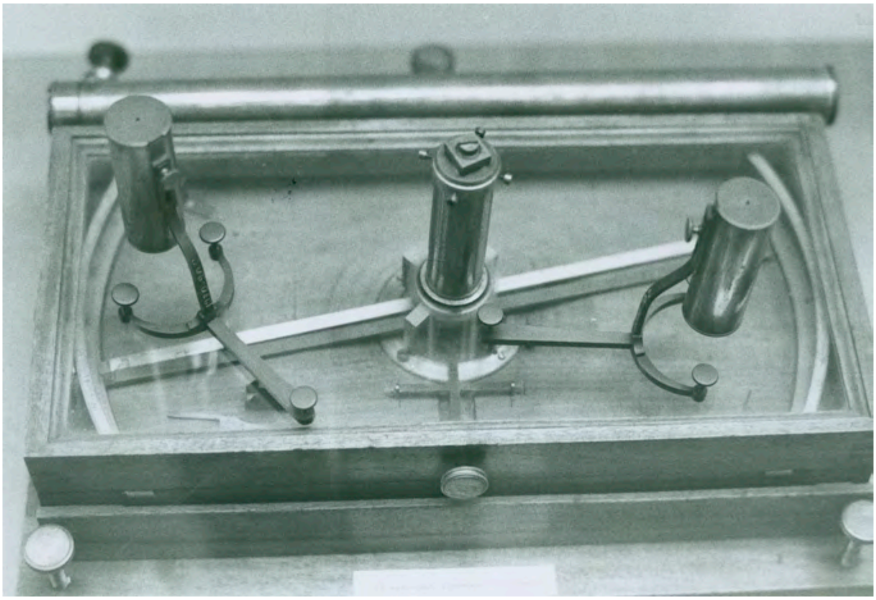

Figure 8.6: Variation Needle. Earth magnetic instrument owned by Henry Cavendish. Photographs by the authors. By permission of the Science Museum, London/Science & Society Picture Library.

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[129] Cavendish (1776b, 117, 124–125).

Figure 8.7: Dipping Needle. Earth magnetic instrument owned by Henry Cavendish. By permission of the Science Museum, London/Science & Society Picture Library.

We return to Cavendish's garden and magnetic instruments. Like the weather, the Earth's magnetism varies complexly from place to place and from time to time, periodically and secularly. Cavendish observed the Earth's magnetic variation and dip at regular intervals and calculated their mean yearly values. Before his study of the Royal Society's meteorological instruments, in the early 1770s he and his father alternated in taking readings with a variation compass in the "garden." (Fig. 8.6). Mixed in with Cavendish's readings are others taken by Heberden at Heberden's house and also, it would seem, in Cavendish's garden.[130] Upon moving from his father's house, Cavendish kept a record of variation of the magnetic compass at his next house at Hampstead from 1782 and later at his house on Clapham Common until 1809, the year before he died. This record consists of more or less daily readings through the summer months,[131] beginning before eight in the morning and ending about 11 at night. He did not place much weight on his readings; when he was asked about the mean variation of his observatory at Clapham Common, he provided it for the past summer but not for past years, because, he said, many other persons there had observed the variation longer than he had.[132] His interest centered on the instruments, experimenting with different suspensions, shapes, and sizes of magnetic needles, trying his father's, Sisson's, and Nairne's needles and his own variant. (Fig. 8.7). He drew up directions for using a dipping needle on several voyages.[133]

We have chosen meteorology as a source of examples to show Cavendish's way with instruments. Whoever examines his meteorological manuscripts must be struck by the tenacity with which he compared his instruments among themselves and with those belonging to the Royal Society and others belonging to colleagues. Take hygrometers, the instruments for measuring the moisture of air, a variety of which were invented from the 1780s with their respective champions. One of the inventors Deluc criticized Saussure's hair hygrometer, and Saussure responded, the two disputing with with such spirit that Blagden spoke of "open war."[134] Deluc had the better temper, but Saussure had the better hygrometer, his being the only one used for serious meteorology by 1820.[135] Their claims aside, all inventors agreed with what Deluc called the "essential point" about hygrometers, that they should be contrived so that all "observers might understand each other, when mentioning degrees of humidity."[136] John Smeaton, another inventor, agreed that the goal was to make hygrometers that, like the best thermometers, were "capable of speaking the same language."[137] To that end Cavendish made trials with Smeaton's hygrometer, which was used by the Royal Society, and with other hygrometers labeled variously "Nairne's," "Harrison's," "Coventry's," "common," "old," "new," "4-stringed," and "ivory." The type of instrument he studied was the hygroscopic hygrometer, which either weighed the water by the increase in weight of dry salt after moist air was passed over it or measured the change in dimensions of a moistened substance such as the contraction of strings; Cavendish generally preferred weighing to measuring as the more exact method, but in this instance he preferred measuring in contrast to our preference today, weighing. He roasted, salted, wetted, and stretched moisture-absorbing strings, and he mixed vapors from acids and alkalis with the air to see

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[130] Cavendish, "Horizontal Needle." On page 3, alongside Cavendish's readings taken in his garden, there are readings by Heberden, who must have been there too. Cavendish's manuscripts also contain readings of the variation compass taken at Heberden's house. Cavendish Mss IX, 19, 21, 23.

[131] Henry Cavendish, "Observations of Magnetic Declination," Cavendish Mss IX, 1. The earliest observations in this manuscript of 256 numbered pages were made at Hampstead; those from page 30 on were made on Clapham Common.

[132] Henry Cavendish to J. Churchman, n.d. [after 12 July 1793], draft; in Jungnickel and McCormmach (1999, 694).

[133] Cavendish's manuscripts contain his instructions to an instrument maker. "Dipping Needle"; "Trials of Dipping Needle"; "On the Different Construction of Dipping Needles," Cavendish Mss IX, 7, 11, and 40. He drew up directions for the use of the dipping needle for three voyages, by Richard Pickergill, James Cook and William Bayley, and Alexander Dalrymple. Ibid., 41–43.

[134] Middleton (1964, 100). On Saussure and Deluc's disagreements: Charles Blagden to Henry Cavendish, 23 Sep. 1787; in Jungnickel and McCormmach (1999, 641).

[135] Middleton (1969, 103, 106).

[136] Deluc (1773, 405).

[137] John Smeaton (1771, 199).

if they made a difference. At times he took readings daily, morning and evening, as often as every twenty minutes, in warm rooms and cold rooms, often together with thermometer readings.[138] For ten years he compared hygrometers. If this activity seems obsessive, it was an essential scientific activity, for the reliability of the instrument and the method of its use were an inseparable part of the scientific argument. It could be said, and Cavendish would have agreed, that an unexamined instrument was not worth using.

In Cavendish's day it was common for researchers to build some of their apparatus but they usually bought or commissioned their instruments. Researchers occasionally invented instruments and instrument makers like Nairne made scentific experiments, but instrument making was a business, and science for someone like Cavendish was a full-time activity. Nearly all of Cavendish's instruments were made in London by contemporary, highly skilled artisans. An exacting experimenter, Cavendish lived in the right place at the right time.

Cavendish's examination of Nairne and Blunt's wind measurer for accuracy was an implicit form of tribute. His colleague George Shuckburgh made it explicit, remarking on the "singular success with which this age and nation has introduced a mathematical precision, hitherto unheard of, into the construction of philosophical instruments."[139] In his living quarters at Greenwich Observatory, the astronomer royal Maskelyne exhibited in addition to a bust of Newton, maker of reflecting telescopes as well as explicator of the system of the world, prints of the builder of the great eight-foot mural quadrant for Greenwich, John Bird, and of the inventor of the achromatic telescope used at Greenwich, John Dolland.[140] In the advancement of science in Cavendish's time, instrument makers were as important as their users.

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[138] Henry Cavendish, "Hygrometers," Cavendish Mss IV, 5. This manuscript consists of 77 numbered pages of laboratory notes and an index.

[139] George Shuckburgh (1779, 362).

[140] 29 July 1785, "Visitations of Greenwich Observatory, 1763 to 1815," Royal Society, Ms. 600, XIV.d, f. 36.