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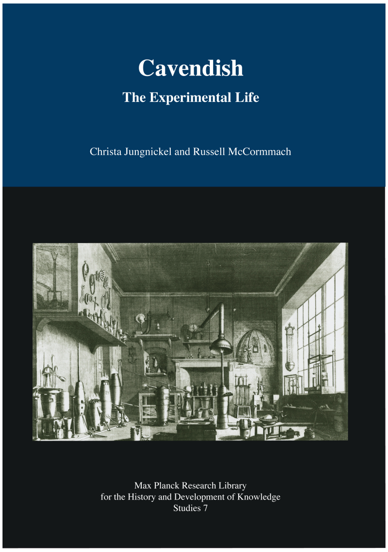
Mercury



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Chapter 15
Mercury

After chemistry and electricity, heat was the third major experimental field in the eighteenth century. Benjamin Thompson, a leading investigator in the field, compared heat with gravity as a principal mover in nature: "The effects produced in the world by the agency of Heat are probably just as extensive, and quite as important, as those which are owing to the tendency of the particles of matter towards each other," and "its operations are, in all cases, determined by laws equally immutable."[1] Heat, Joseph Black told his students, "is certainly the chief material principle of activity in nature," and if it were removed, "a total stop would be put to all the operations of nature."[2] His student William Cleghorn said that without heat, "Nature would sink into chaos." Of fields of investigation, he said, "nothing will seem more deserving of the attention of philosophers" than heat.[3] Heat awaited its Newton, who would lay down its laws and erect a system to stand beside the theory of gravitation and the system of the Sun and planets. As he did in electricity, Cavendish set out on this quest.

Specific and Latent Heats

Heat was a difficult field. The chemist and physician Adair Crawford, a pioneer in the measurement of specific and latent heats, explained the difficulty of performing repeatable experiments in heat: "A change in the temperature of the air in the room, a variation in the time that is employed in mixing together the substances which are to have their comparative heats determined, a difference in the shape of the vessel, or in the degree of agitation that is given to the mixture, will often produce a considerable diversity in the result of the same experiment."[4] In his experiments on heats, Cavendish made corrections, took the mean of repeated trials, and followed up every source of error. With his precautions, and with the help of good thermometers, he achieved, in Crawford's words, a "very near approximation to the truth." Wilson said that Cavendish's experiments on heat showed "all the precision and accuracy" we have come to associate with him.[5]

At about the same time that Cavendish carried out his first dated chemical experiments and began preparing for his electrical researches, he undertook a series of experiments on specific and latent heats, which he recorded in an untitled, indexed packet of 117 numbered octavo sheets. Because the first and earliest date, 5 February 1765, occurs near the end of the record, we assume that the experiments began in 1764.[6] Their sequence follows more or less

[1]Benjamin Thompson (1798); in (1870–1875, 1:491).
[2]Joseph Black (1803, 1:11–12).
[3]Douglas McKie and Niels H. de V. Heathcote (1958, 13–15).
[4]Adair Crawford (1779), advertisement.
[5]George Wilson (1851, 447).
[6]Cavendish Mss III(a), 9:89. On pp. 92 and 94, there are two more dates, both in 1776; the experiments involve freezing mixtures.

a progression of questions and answers. Cavendish sometimes reordered experiments, but usually he cross-referenced them, and in any case the interruption of chronology is minor and obvious. The bundle of sheets conveys the feel of experimental research leading to important, sometimes unanticipated results. This work was comparable in thoroughness to his experiments on air and on electricity. Because heat enters into the phenomena of most branches of experimental science, we need to know how Cavendish treated it to understand how he approached natural philosophy.

The sheets are not the original slips containing measurements recorded in the laboratory but an intermediate record, from which Cavendish wrote a paper, fifty quarto pages in length, "Experiments on Heat" (not Cavendish's title). Wilson said that if Cavendish had cared to publish this paper, it "might at once have been printed."[7] The paper is not that close to publication,[8] but Wilson was right that if Cavendish had wanted to publish it, he had a draft of much of it and most of the material for the rest of it. As it stands, the paper was written for an unidentified specific reader in mind, whom we know only as "you."

When Cavendish came forward as a researcher in the 1760s, the experimental field of heat had begun to be developed as a quantitative science. Central to this development was the distinction between thermometer readings and quantities of heat, on which the quantitative concepts of specific and latent heats depended. Although the immediate stimulus for Cavendish's heat experiments is unknown, a reasonable speculation can be made about it.

Apart from Cavendish's own work, the important researches on heat were not made in London. He mentioned only one name in his experimental notes, which comes at the very end of the packet, "Martin,"[9] clearly a reference to the Scottish physician George Martine, who in 1740 published an account of rates of heating and cooling.[10] In his paper "Experiments on Heat," Cavendish mentioned three names in connection with latent heat. One was the French physical scientist Jean Jacques Marain, who observed the generation of heat in the freezing of water.[11] The other two were the Scottish chemists Cullen and Black, whose work was current.

Cullen, the older of the two, was professor of medicine and lecturer in chemistry at the University of Glasgow, in whose laboratory Black worked for a time. When Cullen moved to the University of Edinburgh Black succeeded him in Glasgow, and ten years later Black again succeeded him in Edinburgh as professor of medicine and chemistry, a position he held for over thirty years.[12] Prompted by the simple observation by a student that a thermometer cools when it is removed from a solution, and suspecting that evaporation is the cause, Cullen made a series of experiments to find out. He evaporated some thirteen acidic and alkaline liquids, listing them in order of their power to produce cold and obtaining cold of "so great a degree" that he suspected no one had observed it before. He thought that the whole subject should be "further examined by experiment."[13]

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[7]Wilson (1851, 446).

[8]This paper is published: Henry Cavendish (1921c). The manuscript of the paper consists of 41 numbered pages followed by 9 unnumbered pages. The numbered pages are complete, but the remaining ones are sketchy.

[9]Cavendish Mss III(a), 9:114.

[10]George Martine (1740).

[11]Cavendish (1921d). His source was probably J.J. d'Ortous de Mairan (1749).

[12]In Glasgow Black was professor of anatomy but soon exchanged duties with the professor of medicine. In Edinburgh Cullen took over the chemistry chair in 1766, freeing the chair of medicine and chemistry, which Black took over. In Scottish universities, there was a good deal of shuffling of chairs. Ramsay (1918, 31, 47).

[13]Cullen's paper was first published in 1755 in *Edinburgh Philosophical and Literary Essays* and was republished together with Black's essay: *Experiments upon Magnesia Alba, Quick-lime, and Other Alkaline Substances; by*

Stimulated by Cullen's experiments and by an observation of Daniel Gabriel Fahrenheit's on super-cooled water, reported in Herman Boerhaave's *Elementa Chemisticae*, perhaps as early as the winter of 1757–58 Black lectured on the heat accompanying changes of state of substances. To convey the concept, he gave a homely and effective example: if snow and ice were to melt immediately at the melting temperature, the commonly held view, then every spring the world would suddenly be overwhelmed by floods, which "would tear up and sweep away every thing, and that so suddenly, that mankind should have great difficulty to escape from their ravages." The reason why this did not happen is that it takes time for ice and snow to absorb the heat that originally is lost in the change of state of water to ice and snow; the heat that is latent in the water does not register on the thermometer. In 1761 Black measured the heat of fusion of ice, reporting on it to the local scientific club in Glasgow the next year.[14] In 1764, Black together with his student William Irvine measured the latent heat of steam by condensing water vapor in a worm tube immersed in a cold water bath. He extended the investigation to substances other than water: at his request, Irvine measured the latent heats of metals such as tin and soft substances such as spermaceti and beeswax. The term "latent heat" is Black's, standing for the heat absorbed or generated in a change of state.[15]

In 1760 Black arrived at his second important discovery, specific heats. He was guided to it again by an experiment of Fahrenheit's reported in Boerhaave's text on chemistry and also by an experiment in Martine's essay, both experiments pointing to different heating effects of water and mercury. Black recognized that different kinds of matter communicate heat differently, having different heat "capacities," another name for which is "specific heats."[16] Specific heat is the heat required to raise the temperature of a given weight of a specific substance one degree; Black used water as the standard substance. Latent and specific heats were new, permanent, and characteristic properties of substances.

Black published nothing of his work on heat, but student notes of his lectures were in circulation by 1767, and an anonymous account of his lectures was published in 1770; in addition his students Adair Crawford and William Cleghorn published his views on heat.[17] Irvine too published nothing of his work on heat. His papers were collected and published after his death, but by then his work was well-known. By the late 1770s, a serious investigator of heat would have known about Black's and Irvine's work in some detail.[18]

Black's work can be seen as the beginning of the quantitative study of heat. He agreed with Boerhaave that the thermometer measures heat, but what it measures is the intensity of heat, not the quantity; and he agreed with Boerhaave that heat seeks equilibrium, though in equilibrium the intensity, or temperature, is the same, not the quantity of heat, a confusion Boerhaave made. Black was able to discover specific and latent heats because of his sound method of measuring heat.[19]

Joseph Black. To Which Is Annexed, An Essay on the Cold Produced by Evaporating Fluids, and of Other Means of Producing Cold; by William Cullen (1898, 132).

[14]McKie and Heathcote (1935, 16, 35). Henry Guerlac (1970, 177). A.L. Donovan (1975, 238–240).

[15]Donovan (1975, 240–246).

[16]Guerlac (1970, 178–179).

[17]Robert E. Schofield (1970, 186).

[18]The Swedish physicist Johan Carl Wilcke discovered latent heat independently of Black and later, in 1772. Unlike Black he published his work on latent and specific heat, discussed in McKie and Heathcote (1935, 54–121).

[19]Schofield (1970, 188–189). By his method, Black measured "heat exchanges in terms of a temperature change of so many 'degrees of heat on Fahrenheit's scale' for an equal mass of water." McKie and Heathcote (1935, 122).

If not from the beginning, by the time he wrote up his heat experiments as a paper, Cavendish knew about Cullen's work on evaporation.[20] He could have come across it in a publication from Edinburgh in 1756, or in conversation with Scottish guests at meetings of the Royal Society and its dining club. He may also have heard about Cullen's experiments from his colleague John Hadley, who repeated one of them in the presence of Benjamin Franklin in 1762.[21] Cavendish knew something about Black's work too. He was "informed" that Black had made observations on distilling water in a worm tube, and not knowing how the experiment came out, he repeated it. In addition to whatever information he acquired informally about Black's or his students' work on heat, he undoubtedly read the same book as Black, Boerhaave's text on chemistry, which was recommended reading at Cambridge when he was a student.[22] He would have read there about Fahrenheit's experiments on hardening and melting, which showed that a change of state of a substance involves a heat that does not register on the thermometer, and about Fahrenheit's demonstration that mercury and water have different heat capacities.[23] He also may have known about Brook Taylor, who in the *Philosophical Transactions* in 1721 published a study of thermometers, in which he mixed given quantities of hot and cold water and measured the resulting temperature.[24] With a similar intention, Cavendish began his researches with experiments to insure that the mercury thermometer is an accurate, uniform measurer of temperature.[25]

Cavendish's experiments on heat were contemporary with Black's or slightly later. Because Black did not describe his method of measuring specific heats, we do not know how close his was to Cavendish's. The equipment Cavendish used for his experiments consisted of thermometers, lamps for heating substances and mixtures, containers made of glass or tin, scales, and a time-keeper. He took three readings three minutes apart to determine the rate of cooling, and he did a separate experiment to determine the heating effect of the container. His method was that of mixtures. He first experimented with the simplest mixture, hot and cold water, which had been studied before him by Fahrenheit, Taylor, and Black.

The paper containing the results, "Experiments on Heat," is reproduced nearly in entirety in Cavendish's *Scientific Papers*, including the first page, which in the manuscript is largely crossed out by Cavendish. On the bottom of that page, separated by a line from the text, perhaps indicating a footnote, is a detail that is not reproduced. The detail, which explains what is otherwise hard to understand by a verbal description, gives the only equation I have found in Cavendish's scientific papers. It expresses the basic law behind the method of mixtures, as applied to the simplest mixture, hot and cold water:

$$m(H + C) = hH + cC, \tag{15.1}$$

[20]Cavendish wrote: "Dr Cullen has sufficiently proved that most if not all fluids generate cold by the first species of evaporation." By "first species," Cavendish meant evaporation produced by heating a liquid without boiling it, which he attributed to absorption by the air. Cavendish (1921c, 344).

[21]Benjamin Franklin to Ebenezer Kinnersley, 20 Feb. 1762, in Benjamin Franklin (1941, 360).

[22]Boerhaave's *A New Method of Chemistry* is listed in Christopher Wordsworth (1968, 79).

[23]Guerlac (1970, 177–178). Fahrenheit was an instrument maker, a friend of Boerhaave's, and a fellow of the Royal Society, who published papers on meteorological instruments in the *Philosophical Transactions*.

[24]Brook Taylor was a mathematician and fellow of the Royal Society, whose experiments were reported in the *Philosophical Transactions* for 1721; they are described in A. Wolf (1961, 1:189–190). Wilson (1851, 447).

[25]Cavendish (1921c, 327). Wilson (1851, 447). Black began his experiments the same way, by examining the thermometer. Guerlac (1970, 177–178).

where h is the temperature of the hot water, c is the temperture of the cold water, H is the weight of the hot water, C is the weight of the cold water, and m is the temperature of the mixture. He does not work with the equation but by rearranging the terms he writes $(m - c) : (h - c) :: H : (H + C)$, which is a proportion, the mathematical relation he always works with. Cavendish does not call it a law; instead, he says that "it seems natural to suppose," crossing that out, "it seems natural to imagine," and crossing that out, "it seems reasonable to imagine" that the equation correctly describes what happens; the object of his experiments is to find if the proportion "really" is correct. In words, his "experiments were made with an intent to see whether the excess of the heats of the hot water and the mixture above the cold water really bore that proportion [the sum of the weights of the hot and cold water to the weight of the hot water] to each other or not." (His verbal description is the inverse of the proportion written in symbols above.) He expects the proportion to be confirmed (because it is "reasonable," elsewhere because it is a "theory") "if the expansion of the mercury in the therm. is proportional to the increase of heat."[26] It is the instrument that he is investigating here.

He next experimented with a mixture of hot mercury and cold water, finding that the heating effect of mercury is equivalent to 31.35 times its weight of water, the standard substance. He then reversed the temperatures, mixing cold mercury with hot water, obtaining a water equivalent for mercury close to the first value. His measured heats of mixtures and the theoretically computed heats agreed to within a half degree, a realistic accuracy for experiments of this kind.[27] He continued his experiments with an improved apparatus consisting of a funnel tightly joined to a pan, with stirrers and thermometers inserted in both the funnel and the pan.

He carried out experiments on a variety of liquids and solids, taken in part from his shelves of chemical reagents: besides water and mercury, they were spirits of wine, oil of vitriol, solution of pearl ashes, sand, iron filings, shot, pounded glass, marble, charcoal, brimstone, coal, and spermaceti. He also estimated the specific heat of air using a different method, blowing cold air through a worm tube surrounded by hot water.[28] His results were contrary to what was expected, as he explained in "Experiments on Heat": "One would naturally imagine that if cold [mercury] or any other substance is added to hot water the heat of the mixture would be the same as if an equal quantity of water of the same degree of heat had been added; or, in other words, that all bodies heat and cool each other when mixed together equally in proportion to their weights"; his experiment on mercury and water showed "that this is very far from being the case."[29] From this statement, Wilson said, it was plain that Cavendish did not know about the experiments by Black and his pupils;[30] we are inclined to agree that he knew nothing specific about them at this time. His own experiments were original, and judging from the way he described and analyzed them, he clearly believed that his findings were new. "The true explanation of these phenomena seems to be that it requires a greater quantity of heat to raise the heat of some bodies a given number of degrees

[26]Cavendish, "Experiments on Heat," Mss III(a). On p. 1 in the manuscript. In the equation and proportion, the parentheses are added.
[27]Cavendish Mss III(a), 9:48–56.
[28]Cavendish (1921c, 341–343). He used a worm tube again in his experiments on latent heat, finding the heat generated by condensing water vapor, and mentioning Black. Ibid., 346–347.
[29]Ibid., 332.
[30]Wilson (1851, 447).

by the thermometer than it does to raise other bodies by the same number of degrees."[31] With this statement, Cavendish had a theory of specific heats, which explained the unexpected outcome of mixing diverse substances.

He paused at this juncture in the flow of his experiments to carry out an extended investigation of one substance, spermaceti, and with it he changed subjects: "Concerning heat & cold produced by hardening & melting of spermaceti" is the heading of his first experiments on latent heats.[32] In the first experiment of this group, he poured melted spermaceti into cold water, hardening it. He calculated that the observed heat communicated to the water would have raised an amount of water equal in weight to the spermaceti by 93.32°. From experiments on the specific heat of spermaceti, he further calculated that if no heat had been generated in hardening, the spermaceti would have communicated 26° of heat to that same quantity of water. The difference of the two numbers gave him the contribution of heat from the change of state: the "heat gen. by hardening of sperm. is sufficient to communicate 67⅓ ° of heat to an equal weight of water." In the converse arrangement, mixing cold spermaceti with hot water, he found a value for the latent heat close to the first: the cold produced by melting spermaceti "is sufficient to cool a quantity of water equal to it in weight about 70 degrees."[33]

The place that spermaceti had in his researches is evident: it was one of the substances he used to establish a general law or rule of nature. What is unclear is the place it had in his understanding of heat. When he began his experiments, he would have known about the cold produced by evaporation and by melting ice, but we have no way of knowing if he had the idea of a general law of latent heats. It is conceivable that his experiments with spermaceti suggested the idea, in which case there is an element of discovery in his experiments. It is at least equally likely that he already had the idea and that he began with spermaceti for reasons of convenience: it melted at a modest temperature, it had physical qualities he was interested in, and it was a substance at hand. In favor of the second explanation are the substances that he chose to experiment with, which were the same ones that Irvine experimented with: in addition to spermaceti, they were beeswax, another soft substance, and tin and other metals. Against the explanation is Cavendish's failure to mention any experiments on these substances done by Irvine or anyone else.

Cavendish's experiments on latent heats established inductively a second law valid for all bodies, which he stated at the beginning of Part 2 of "Experiments on Heat": "As far as I can perceive it seems a constant rule in nature that all bodies in changing from a solid state to a fluid state or from a non elastic state to the state of an elastic fluid generate cold, & by the contrary change they generate heat." As in the case of specific heats, Cavendish had an explanation of latent heats: "The reason of this phenomenon seems to be that it requires a greater quantity of heat to make bodies shew the same heat by the thermometer when in a fluid than in a solid state, and when in and elastic state that in a non-elastic state."[34] With his rules of nature and physical explanations, Cavendish had a theory of both specific and latent heats.

Cavendish's explanation of the change state of a body might be mistaken for Irvine's. Irvine thought that latent heat depends on specific heat, and to go from one to the other, he

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[31]Cavendish (1921c, 340).
[32]Cavendish Mss III(a), 9:22, 27.
[33]Ibid., 32.
[34]Cavendish (1921c, 343).

introduced a third heat, the "total" heat of a body. He theorized that the specific heat, or "heat capacity," of a body measures the total heat in a body, the body acting as a container holding the heat. For example, because water has a larger measured heat capacity than ice, it takes more heat to fill its container than it does the same quantity of ice with its smaller container, when both are at the same (freezing or melting) temperature; that is, it takes more heat to maintain water at that temperature than it does ice, the additional heat being the measured latent heat.[35] Cavendish rejected Irvine's theory, but his wording in "Experiments on Heat" is compatible with it.[36]

He followed his statement of the law of latent heats with a discussion of experiments that supported it, beginning with experiments on boiling. Cullen had "sufficiently proved" that fluids generate cold when they evaporate at a temperature below the boiling point and are absorbed in the air. Cavendish treated the other "species of evaporation," boiling, which is independent of the air, finding that 982 degrees of cold are generated in the conversion of water to steam. His discussion of the generation of cold in the change from an inelastic to an elastic state ends with a brief "sketch of the other experiments," one of which was an attempt to find if cold is generated by dissolving alkaline substances in acids, releasing fixed air. This experiment was original; he gave no details, and his method could not have yielded accurate results, but the principle was sound.[37] He next discussed the cold generated in the change from a solid to a liquid state, beginning with the cold generated by melting snow in solutions of sea salt, pearl ashes, spirits of wine, and aqua fortis. He followed this with a discussion of the cold generated by melting spermaceti and beeswax and then by melting "simple metals," lead, bismuth, and tin, and "mixtures" of these metals. The latter, "alloys," differed from the simple metals in that they changed state over a range of temperatures rather than at a fixed temperature, analogous to spermaceti and beeswax. He briefly discussed the inverse change of state of these substances, from liquid to solid, generating heat.[38] Cavendish's long series of experiments on specific and latent heats ended here.

Wilson and others have suggested that Cavendish did not publish his experiments on heat because he did not want to enter into rivalry with Black.[39] That may be, but he published on factitious air even though Black said that he intended to do more work on the subject. The two cases differ in a way that may be relevant: Black published his original experiments on fixed air, whereas he published nothing on heat. Not fear of rivalry but eventual knowledge of Black's work is the more likely reason Cavendish did not publish his experiments on heat; after writing his paper he probably learned more about Black's lectures and realized that his own work was not new. Black's lectures were, in effect, a slow but sure publication, and

[35]Thomas L. Hankins (1985, 76).

[36]Nowhere does Cavendish mention Irvine by name, but his manuscripts contain two short memoranda directed to principal points of Irvine's theory. One states a proposition that any heat that appears in bodies depends entirely on their heat capacities and changes in them. The proposition is expressed in the language of the material theory of heat, its only appearance in any of Cavendish's papers: heat is said to be "absorbed" by bodies and "united" to them. The second memorandum is an experimental "compleate proof" that the absolute heat in bodies is not proportional to their specific heat, as Irvine's theory requires. There is a second copy in Cavendish's hand of the second memorandum in Blagden's papers, suggesting Blagden as a possible reason he wrote out the proof. Henry Cavendish, "That All the Heat Which Appears in Bodies …"; "A Complete Proof that the Quantity of Heat …" Cavendish Mss Misc. Blagden Collection, Misc. Notes, Royal Society, 93.

[37]"On heat produced by sat. alkalies with acids," Cavendish Mss III(a), 9:82–83. Wilson (1851, 347). Berry (1960, 144).

[38]Henry Cavendish (1921d). This is a section of "Experiments on Heat."

[39]Wilson (1851, 446). McKie and Heatcote agree with Wilson (1935, 52).

a number of researchers in Britain worked with concepts of heat that Black communicated through his lectures. In addition, important work on specific and latent heats was carried out abroad, in particular by Wilcke in the 1770s. When Cavendish published on the freezing point of mercury in 1783, he invoked the rule of latent heat in a discussion of the freezing of water, giving neither an argument nor a citation for it but simply remarking that it was a "circumstance now pretty well known to philosophers." The "circumstance," he explained, was "that all, or almost all, bodies by changing from a fluid to a solid state, or from the state of an elastic to that of an unelastic fluid, generate heat; and that cold is produced by the contrary process," wording taken from his paper "Experiments on Heat" based on his experiments from the 1760s.[40]

Another question is Cavendish's satisfaction with his experiments and their explanation. Part I of "Experiments on Heat," which deals with specific heats, is complete and apparently ready to be rewritten in fair copy. The experiments in the incomplete Part II, which deals with latent heats, move beyond heats involved in a change of state of bodies to heats involved in mixing interacting fluids and in chemically releasing fixed air, for which he did not have a "general rule in nature." The same happened with his experiments on electricity: he completed Part II of his work insofar as it was about experiments explained by the theory of Part I, but he had gone on to make experiments on electrical conduction, which his theory had not addressed. As with his experiments on electricity, he did not bring his experiments on heat to a natural conclusion, but on the contrary, he expanded them.

Finally, we need to consider the theoretical side of his experiments. His paper on heat ends with "Thoughts Concerning the Above Mentiond Phenomena," which reads: "There are several of the above mentiond experiments which at first seemd to me very difficult to reconcile with Newtons theory of heat, but on further consideration they seem by no means to be so. But to understand this you must read the following proposition."[41] The proposition is not given. Cavendish held two theories of heat, a mechanical theory which he called "Newton's theory" and the theory of specific and latent heats which he worked out at the time of his experimental researches. It is clear from the report of one of the experiments that he had both theories in mind. The change in temperature generated by a mixture of water and spirits of wine was caused either by the "commotion made by the particles of one uniting with those of the other" or by the "mixture of spts & water requiring a greater quantity of heat to make it raise the thermom to a given degree than the 2 liquours separately do."[42] The first explanation referred to the mechanical theory of heat, the second to the theory of specific and latent heats applied to mixtures of substances that have an affinity for one another. Cavendish almost certainly did not have a theoretical explanation for all of his heat experiments at the time. He did in the 1780s, but by then there was no point in publishing the experiments.

Cavendish developed a special theory for a specific change of state, evaporation and boiling of water, which he carefully drafted but did not publish.[43] The theory was an explanation of his recommendation on setting the upper fixed point of the thermometer, which he wrote out to show to Deluc, a member of his committee. Deluc returned the paper with a letter in French, thanking Cavendish "for the pains you have taken to introduce me to your

[40]Henry Cavendish (1783b); in *Sci. Pap.* 2:145–160, on 150.
[41]Cavendish (1921c, 351). "Thoughts" is an unnumbered page in the manuscript.
[42]Cavendish Mss III(a), 9:39–40.
[43]Henry Cavendish (1921h).

theory which makes you favor the vapor of boiling water to the boiling water itself for fixing the upper point of the thermometer." He had read the paper "three times with much care," without finding in it "any reasons to abandon my own theory," which explained all of Cavendish's experiments. He did not argue the merits of their theories, he said, because he would then have to introduce material not just about boiling and the thermometer but also about the barometer and the hygrometer, and also because any discussion of matters "not susceptible to geometric demonstration" but only to probability can be endless. He limited his comments on Cavendish's paper to the constancy or variability of the temperature of the vapor of boiling water. He closed the letter saying that he would come to Cavendish's house the next day, no doubt to witness experiments and perhaps to participate in them.[44]

A preparation for Cavendish's theory of boiling was his paper on specific and latent heats; a section from "Experiments on Heat" reappears verbatim in "Theory of Boiling." We assume that by this time he had abandoned his intention to publish "Experiments on Heat," treating it as a resource. His theory of boiling brings together several major strands of his development as a natural philosopher. It pays us to look at it.

Around the time of Cavendish's theory, Cavallo wrote in his treatise on air that the explanation of evaporation was still unsettled. His own opinion was that evaporation is the absorption of water by air, facilitated by heat, but to the question of how air assists in evaporation, the "present knowledge of philosophy [...] does not afford a satisfactory answer." A good many hypotheses had been proposed, for example, capillary attraction and chemical attraction, but none was satisfactory. Cavallo thought that "a vast number of experiments is still requisite, in order not only to discover its [evaporation's] real cause, but also to ascertain its laws."[45]

Cavendish referred to evaporation and boiling as two "species" of evaporation. Earlier in the century, the first species of evaporation had been explained by a hydrostatic theory, which held that small quantities of water are expanded by heat and rise through the heavier surrounding water. Cavendish accepted the alternative explanation of Charles Le Roy's in 1755, according to which water is dissolved in air in the same way that salts are dissolved in water. This species of evaporation, Cavendish said, "is intirely owing to the action of the air."

The phenomena of the second species of boiling depend on four "principles," on which Cavendish based his theory. The first principle, which his father had demonstrated the "truth" of, is that if the water is in contact with steam or air in a closed vessel, it is immediately turned into steam once it is heated ever so little above what is required for steam. He called this heat the boiling point, which depends on the pressure on the water. The second principle is that if the water is not in contact with steam or air, it bears a considerably higher heat before it converts into steam, what Deluc called the heat of ebullition (from hissing to rolling boil). Cavendish said that Deluc had confirmed this experimentally. The third principle is that steam contained in closed vessels and not in contact with air is immediately converted back into water when it is cooled ever so slightly below the heat required to produce the steam. This was proved by Cavendish's father and also by the boiling point committee, and Cavendish performed an experiment of his own to put the "matter out of doubt." The fourth principle is that in the conversion of water to steam, a great quantity of heat is lost, and in the conversion of steam to water an equally great quantity of heat is

[44] Jean André Deluc to Henry Cavendish, 19 Feb. 1777; in Jungnickel and McCormmach (1999, 546, 549).
[45] Cavallo (1781, 505–507).

acquired. For confirmation, Cavendish referred to his own experiments on latent heat and to similar experiments by Cullen, Black, and Deluc.[46] From the principles of the theory, Cavendish explained the "chief phenomena of boiling water." When water begins to boil, the lamina of water at the bottom of the vessel heats up until small bubbles of steam are formed by ebullition. The bubbles can never be hotter than the boiling point of water, but the water itself can be and generally it is. Just how much the temperature of the water exceeds the boiling point depends on a variety of factors such as the amount of air dissolved in the water and the rate of application of heat. The theory confirms the committee's opinion that steam is a more exact method than boiling water for setting the upper fixed point of the thermometer.[47]

To explain the difference between the temperature of boiling and of ebullition, Cavendish introduced a hypothesis. In developing other theories, Cavendish began with a hypothesis, but this time he began with a set of principles, which he did not regard as hypothetical, and he placed the hypothesis at the end. The hypothesis is that particles of water repel one another over a minute distance beyond which they attract one another, and that the repulsion but not the attraction increases with heat.[48]

"Theory of Boiling" is a compendium of Cavendish's scientific practices. It shows him as a natural philosopher who makes theories and hypotheses and performs experiments. He bases his theory in part on his father's experiments, a sign of the continuing importance of his father. The importance of the Royal Society is seen in his work with a committee called to consider the accuracy of the fixed boiling point on thermometers, in itself an inducement to develop a theory of boiling. He draws on his earlier experiments on latent heat, reflecting his practice of using results of experiments he has not published. His hypothesis relates heat to the forces of particles, an idea Newton and Boscovich have discussed extensively. With his theory, he takes up an unexpected behavior, the heating of water above its boiling point, showing that it is explained by the laws of the normal course of nature, and in this respect it is similar to other topics he takes up such as the electric shock of a fish and the freezing of the mercury in thermometers. He writes the paper for an intended reader from the Royal Society, and he does not publish it.

Cold

Extremely cold temperatures were reported from the frigid North. The natural historian Johann Georg Gmelin recorded -120° in Siberia. He said that such a temperature was scarcely believable "had not experiments, made with the greatest exactness, demonstrated the reality of it."[49] Commenting on these temperatures, William Watson said that however "extraordinary" Gmelin's observations were, they were "scarce to be doubted," since they were made with "all possible exactness" and agreed with readings made by others under his direction in different parts of Siberia.[50] Pyotr Simon Pallas reported - 70° there, noting that the mercury froze to the glass stem of his thermometerand that the mercury began to melt when the

[46]Cavendish, "Theory of Boiling," 354–356.
[47]Ibid., 358–360.
[48]Ibid., 361–362.
[49]John Fothergill's extracts from Gmelin (1748, 260).
[50]John Fothergill (1748, 258–260). William Watson (1753a, 108–109).

thermometer stood at - 45°. Cavendish copied out the parts of Pallas's account of his travels in Siberia dealing with the freezing of mercury.[51]

In St. Petersburg in 1759, on a day when the temperature was -34°, a member of the Academy of Sciences J.A. Braun found that the temperature of a freezing mixture of nitric acid with snow sank below -350°. When he removed the thermometer from the mixture he saw that the mercury was immovable, and he put its freezing point roughly at a hundred degrees below zero.[52] He and his colleagues tested the solid mercury, finding that it could be hammered and drawn like any other metal. One of his colleagues Aepinus observed a change in the surface of the mercury in the thermometer, a sure indication of contraction upon freezing. Braun's experiments attracted attention in Europe; in the *Philosophical Transactions* for 1761, William Watson published an enthusiastic account of them and of this "intirely new" subject.[53] In the journal the previous year, Nicolas de Himsel said that the Petersburg experiments mostly agreed that mercury becomes solid when it drops to around -500°, but they did not "sufficiently agree as to deduce any thing certain about it."[54] In the same volume, Keane Fitzgerald observed that Himsel's own experiments on the freezing of mercury made the mercury thermometer unfit for measuring great cold.[55] There no longer could be any doubt that mercury, the substance once regarded as the essence of fluidity, could be solidified, but beyond that fact little was known for certain about the behavior of mercury at very low temperatures.

Cavendish made an extract of a paper with Braun's repetition of Fahrenheit's experiments: surprised when the mercury in the thermometer fell hundreds of degrees, and unable to arrive at a consistent freezing point of mercury, Braun said he was confident that it could not be at a "less cold than -346" degrees.[56] Braun's experiments were repeated by Thomas Hutchins, governor of Albany Fort at Hudson's Bay, using instruments and instructions sent to him by the Royal Society. In the winter of 1774–75, Hutchins froze mercury, and like Braun he found the experiments inconclusive on the freezing point. He could find no instant of freezing, and without changing its appearance, the mercury continued to fall to below -400°. He asked the Royal Society for more tubes of mercury capable of graduation to *1,000 degrees below zero.* He continued making experiments up to 1777–78, using freezing mixtures of nitrous (nitric) acid and snow, and comparing a spirit (alcohol) thermometer with a mercury thermometer. Although the temperature was cold enough to freeze mercury, the alcohol thermometer never fell lower than -46°, indicating that the freezing point of mercury was nowhere near as low as Braun supposed.[57]

The reason for Hutchins's findings was evident to two persons in Britain who had clarified to themselves the principles of latent heat, Black and Cavendish. In a letter in 1779 about Braun's and Hutchins's experiments, which was forwarded to Hutchins, Black said that frozen mercury could not record its own freezing temperature, and although he did not give his reason for this opinion, it doubtless included the contraction of mercury on freez-

[51] "Account of Freezing of ☿ from Pallas Journey into Siberia," extract in Cavendish's hand, Cavendish Mss III(a), 15. Pyotr Simon Pallas (1771–1776).
[52] Wilson (1851, 456).
[53] William Watson (1761). A.W. Badcock (1960, 100).
[54] Nicolas de Himsel (1760, 673).
[55] Keane Fitzgerald (1760, 833).
[56] This extract in Cavendish's hand is an account of experiments by several Petersburg academicians following Braun's discovery; in English translation from the French by James Parsons (1760).
[57] Thomas Hutchins (1776). Berry (1960, 146).

ing. To get around the difficulty, he proposed immersing the thermometer bulb in a mercury bath. Hutchins informed the Royal Society of Black's proposal, which he made the basis of his next series of experiments. Unknown to Black, Cavendish had already proposed the same method to the president of the Royal Society Joseph Banks. Black did not publish on this subject, as usual, but this time Cavendish did.[58] In his paper on Hutchins's experiments, Cavendish explained the method: "If a glass of water, with a thermometer in it, is exposed to the cold, the thermometer will remain perfectly stationary from the time the water begins to freeze till it is intirely congealed, and will then begin to sink again. In a like manner, if the thermometer is dipped into melted tin or lead, it will remain perfectly stationary, as I know by experience, from the time the metal begins to harden round the edges of the pot till it is all become solid, when it will again begin to descend; and there was no reason to doubt that the same thing would obtain in quicksilver."[59]

Cavendish drew up a list of experiments to be performed at Hudson's Bay on the freezing of mercury and on the change of volume of other fluids with temperature.[60] In 1781 the Royal Society sent thermometers for use in the experiments, and Cavendish sent an apparatus—a thermometer with the bulb and part of the stem enclosed in a narrow cylindrical cup for holding the mercury to be frozen—for determining the "precise degree of cold at which quicksilver freezes."[61] One day in December 1781, after taking a reading every twenty seconds for about an hour in weather colder than 20° below zero, Hutchins recorded that he "went away to warm myself," an indication of the rigors of the climate and the limits of endurance of the experimenter.[62] In the course of ten experiments on both natural and artificial cold in which he read three instruments—a mercury thermometer, an alcohol thermometer, and the apparatus—Hutchins determined the freezing point of mercury. His experiments were "very accurate," Cavendish told John Michell.[63] Hutchins said that his "excellent instructions" left him with "nothing to do but to follow them."[64] Cavendish, Blagden said, was the "real author and first mover of the whole business."[65]

Hutchins's paper appeared in the *Philosophical Transactions* for 1783, followed by a paper by Cavendish giving his "observations" on Hutchins's experiments. The experiments confirmed Cavendish's hypothesis, which was that the great sinking of mercury in thermometers in extreme cold is due to the contraction of mercury. If the earlier reports had been true, the intense cold produced by freezing mixtures would have been "really astonishing," but they were actually reports of the contraction of mercury. Submerged in freezing mixtures, Hutchins's thermometer fell to hundreds of degrees below zero, but the cold of the freezing mixture was never less than 46° below zero. The essential point was clearly

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[58]Joseph Black to Andrew Graham, 5 Oct. 1779, published by Thomas Hutchins, in "Experiments for Ascertaining the Point of Mercurial Congelation" (1783, 305–306). Black did not know that Cavendish had recommended a similar apparatus to Banks. Henry Cavendish (1783b).

[59]Cavendish (1783b, 146).

[60]There are several drafts of instructions in Cavendish's papers, most of them in Cavendish Mss III(a), 4 and 14. The first group is mainly concerned with Hutchins's experiments published in 1783, though it contains some subsequent instructions sent in 1784. The second group is concerned with the next series of experiments at Hudson's Bay Company, conducted by John McNabb, published in 1786 and 1788. In addition, there are unclassified papers on the Hudson's Bay experiments in the miscellany of Cavendish's manuscripts.

[61]Cavendish (1783b, 145, 148–149).

[62]Hutchins (1783, 317).

[63]Henry Cavendish to John Michell, 27 May 1783, draft; in Jungnickel and McCormmach (1999, 568).

[64]Hutchins (1783, 304).

[65]Charles Blagden (1783, 346).

and simply demonstrated. Because the thermometer in the container of mercury fell to -40°, where it stayed while the surrounding mercury was gradually freezing, the only possible conclusion was that mercury freezes at that temperature. Hutchins came to England and demonstrated his apparatus before Cavendish and Blagden at Cavendish's house in Hampstead.[66] Hutchins returned the thermometers to the Royal Society, where in the best practice of the time, in the presence of witnesses—in addition to Cavendish, they were Hutchins, Banks, Blagden, and Nairne, who made the apparatus—they were examined following the procedure recommended by the boiling point committee of 1777 (Fig. 15.1). Upon making corrections for the fixed point on Hutchins's thermometers, the adjusted freezing temperature of mercury was declared to be $-38\frac{2}{3}°$ or, in round numbers, -39°, in close agreement with the modern value, -38.87°. Hutchins probably did not freeze mercury solid, since the mercury in his thermometer did not fall as far as Braun's; from Braun's experiments, Cavendish concluded that upon freezing, mercury shrinks by almost 1/23 of its bulk, a figure close to modern measurements.[67]

The new understanding of mercury entered the scientific literature at once. In 1783, the year of Hutchins's and Cavendish's publications on mercury, there appeared an English translation of Bergman's treatise *Outlines of Mineralogy*, which had been published in Swedish the year before. Under the entry for mercury, Bergman wrote that it has been "erroneously ranked among the brittle metals, for at 654 degrees below zero it freezes, and then spreads under the hammer like lead. But as such an extreme degree of cold rarely happens unless artificially produced, we cease to wonder why it is always liquid or rather melted." The translator William Withering commented that recent experiments at Hudson's Bay seem to give the freezing point as 39° below zero, and he altered Bergman's "Table of Metals" accordingly: the "melting heat" of mercury now read, "-39 or -634" degrees Fahrenheit.[68] Cavendish's observations on the Hudson's Bay experiments put an end to credible reports of extravagant cold from the frozen parts of the Earth. Michell said to Cavendish that "indeed I think you are bound to find something else in it's stead, having robbed us of so excellent a measure of heat & cold, as the Quicksilver was supposed to be for so many degrees below - 39."[69] Experiments on the freezing of mercury combined several of Cavendish's interests: the work of the Royal Society, latent heats, climates of the Earth, and the workings of a principal instrument of quantitative science, the mercury thermometer.

[66]Thomas Hutchins to Charles Blagden, n.d., "Monday Morning," Blagden Letters, Royal Society, H.59.
[67]Cavendish (1783b, 157).
[68]The disparity between the two numbers for the low reading, -654 and -634, is in the text. Torbern Bergman, *Outlines of Mineralogy*, trans. W. Withering (Birmingham, 1783), 71, 83.
[69]John Michell to Henry Cavendish, 2 July 1783; in Jungnickel and McCormmach (1999, 575).

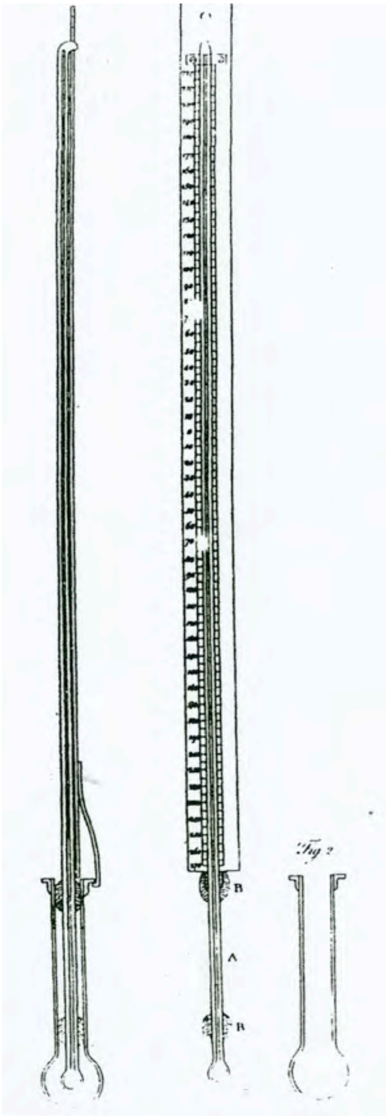

Figure 15.1: Thermometers for Extreme Cold. The stem and bulb of the middle thermometer extends below the scale. The figure on the left is a side view of the thermometer with the extended stem and bulb inserted into a cylinder holding mercury to be frozen. Thomas Hutchins (1783, *370).

Cavendish made experiments of his own too. Hutchins's first experiments were read at the Royal Society in the winter of 1775–76, and in January 1776 Cavendish performed experiments on artificial cold, using a mixture of snow and aqua fortis, recording a temperature of -25°F.[70] In February 1783, two months before Hutchins's paper on the freezing of mercury was read at the Royal Society, Cavendish froze mercury at his house in Hampstead. In an air temperature of 20°, and using a freezing solution of nitrous (nitric) acid, the mercury in his thermometer fell to -110°, part of the mercury being frozen. He then placed an alcohol thermometer in the freezing solution, obtaining a reading of about -45°.[71] In the same year he built an apparatus that cooled air by rarefying it mechanically.[72] In 1786 Blagden said that this way of producing cold was "lately much talked of, in consequence of experiments by Mr. Cavendish, Dr. Crawford, & I believe some other gentlemen."[73]

Interested in knowing the greatest cold that could be produced by a freezing mixture of snow and various chemical solutions, and in finding the cause of the cold produced by freezing mixtures, Cavendish arranged for more experiments at Hudson's Bay. The experimenter this time was John McNab, master at Henly House, a station on the Albany River 150 miles from Fort Albany. Like Hutchins, McNab earned praise from Cavendish for his "utmost attention and accuracy." Carried out in weather that reached -50°, using mainly alcohol thermometers, McNab's experiments developed "degrees of cold greatly superior to any before known" as well as insight into the "remarkable" way nitrous (nitric) and vitriolic (sulfuric) acids freeze. With a mixture of snow and dilute vitriolic acid, McNab measured a temperature of $-78\frac{1}{2}°$. Braun claimed that a thermometer filled with spirit of wine (alcohol prepared by distilling wine) sank to -148°, but McNab found that spirit of wine thermometers could not nearly approach that degree of cold. Cavendish published his account of McNab's experiments in 1786.[74] Because in that paper the freezing points corresponding to different strengths of the acids "were deduced from reasoning not sufficiently easy to strike the generality of readers with much conviction," Cavendish asked McNab to carry out more experiments "to ascertain the truth" of his earlier result. These experiments became the subject of his last paper on heat, published in 1788.[75]

We see in Cavendish's researches on cold qualities of his work we have come to expect. In connection with McNab's experiments, he published a table of specific gravities of nitric and sulfuric acids corresponding to a range of strengths at a temperature of 60°, which agree with modern theoretical values to the second decimal place. Thorpe considered the table "a striking exemplification of the care, patience and manipulative skill which he spent upon all quantitative determinations."[76]

We end this discussion with further conclusions Cavendish drew from McNab's experiments. The acids could be cooled far below their freezing points without freezing, but once they froze their temperatures rose to the freezing points. In one kind of freezing, nitric

[70] Entry for 22 Jan. 1776, Cavendish Mss III(a), 9:94–96.
[71] Blagden was with Cavendish. The following day he recorded in his diary that Cavendish had frozen mercury, and he mentioned it in his history of the freezing of mercury. 27 Feb. 1783, Charles Blagden Diary, Royal Society, 1. Blagden (1783, 359–360).
[72] Henry Cavendish (1921b).
[73] Charles Blagden to Erasmus Darwin, 14 Sep. 1786, draft, Blagden Letters, Royal Society 7:34. Charles Blagden to Mrs. Grey, 30 Jan. 1788, ibid. 7:111.
[74] Henry Cavendish (1786, 195, 197–198, 210).
[75] Henry Cavendish (1788a).
[76] Thorpe (1921, 59–60).

and sulfuric acids froze as a whole; in another kind, the watery part of the acids separated out and froze. The acids had complex freezing points, which depended on their strengths. The freezing point of dilute nitric acid was not as low as it was when the acid was made more dilute and also when the acid was not dilute; there was a definite strength of the acid at which it froze with less degree of cold than when the strength was stronger or weaker, which Cavendish called "a point of easiest freezing." Drawing upon Newton's method of interpolation, Cavendish determined that the point of easiest freezing was -2.4° Fahrenheit, and that the strength at which nitric acid froze with the least degree of cold was .418 according to his marble scale (he stated the strengths of his acids in terms of the weights of marble dissolved by a unit weight of the acids).[77] Sulfuric acid had an even more complicated pattern of freezing. Cavendish found that it had not only a strength of easiest freezing, as James Keir had recently shown in a paper communicated by Cavendish to the Royal Society, but also at greater strengths it had another point of flexure beyond which the freezing point increased again. Cavendish's biographers, the chemists Wilson and Berry, were impressed by his two papers on the freezing of acids. Wilson singled out Cavendish's implicit use of the laws of constant and reciprocal proportions in constructing a table of sulfuric acid strengths. Berry said that the accuracy of Cavendish's elaborate investigations of freezing points of acids were confirmed a century later, and that his findings "are of theoretical interest, and of fundamental importance for the recognition of the various hydrates of nitric and sulphuric acids."[78]

Cavendish's first paper on McNab's experiments cited Lorenz Crell's *Chemische Annalen* and *Neue Entdeckungen in der Chemie.* It was read to the Royal Society in February 1786, and in an earlier chapter of this book we learned that in January 1786 Cavendish (through Blagden) told Crell that he wanted to subscribe to his journal. We see that he had use for it, a possible reason for his impatience at the delays in receiving it.[79]

All told, Cavendish published three papers on heat (or cold, which belongs to the same subject), all three presenting experiments done by others, by Hutchins and McNab. He carried out experiments on freezing mixtures and on the freezing of mercury at his house, but he was content to limit his public contribution to planning, commenting on, and drawing inferences from experiments done by observers working in a cold climate, in close association with the Royal Society.

Heat

The first sentence of Blagden's contribution to the family obituary of Cavendish reads: Cavendish made himself master of "every part of Sir Isaac Newton's philosophy." Cavendish's researches in heat support this observation. He studied latent heats, but he did not use Black's word *latent*, as he explained in a footnote to his paper on the freezing point of mercury in 1783. The word "relates to an hypothesis depending on the supposition, that the heat of bodies is owing to their containing more or less of a substance called the matter of heat; and as I think Sir Isaac Newton's opinion, that heat consists in the internal motion of the particles of bodies, much the most probable, I chose to use the expression, heat is gener-

[77]Cavendish (1788a, 218).
[78]Wilson (1851, 461–465). Berry (1960, 150–154, quote on 154). Cavendish (1788a, 223).
[79]Cavendish (1786, 211).

ated."[80] In his paper "Experiments on Air" the following year, in connection with a recent paper in which Watt spoke of latent heat, Cavendish said that he avoided Watt's "form of speaking" because he thought it "more likely that there is no such thing as elementary heat" and because it could "lead to false ideas."[81] The passage on Watt in 1784 and the footnote on Black the year before are Cavendish's only published statements on the nature of heat. The manuscripts he left at his death are found to contain two references to Newton's theory of heat. One appears in a corollary to a theorem in a paper on the theory of motion, which begins, "Heat most likely is the vibrating of the particles of which bodies are composed." The other, quoted earlier, appears in his experimental paper on latent and specific heats: some of his experiments appeared to conflict with "Newton's theory of heat," but they could be reconciled by a proposition. The paper ends without the proposition, about which he may have had second thoughts. Until recently, these references, two published and two unpublished, were the only known explicit statements by Cavendish on the nature of heat. Since heat was one of his major fields of research, and since it had connections with other fields such as electricity, magnetism, pneumatic chemistry, pneumatics, and meteorology, what was missing from his scientific papers was a fully developed theory of heat comparable to his theory of electricity. He had indeed worked out such a theory, only it had been separated from his scientific manuscripts. It came to light in 1969, when a direct descendent of Cavendish's principal heir put it up for sale.

The new manuscript was inside a folded sheet labeled in Cavendish's hand "Heat." It is a theoretical paper, which he definitely wrote for publication. The first draft he referred to as the "foul copy," to which he appended a number of pages of additions and alterations, and the revised second draft is a nearly fair copy with some crossings out and certain paragraphs marked for rearrangement for the next writing, which he apparently did not carry out. He referred to the "text," to which he supplied an apparatus of footnotes, and he called the whole a "paper." The paper is a mathematical, mechanical theory of heat complete with the principle of conservation of energy and applications to the principal branches of physical science.[82]

The idea of heat as vibratory motion had received a number of formulations by Cavendish's time. To the question of what it is that moves, a variety of answers had been proposed: the ordinary particles of bodies, the air and acid sulfur in bodies, the subtle ether, and the fluid of fire. Newton's authority was invoked in support of more than one of them, but to Cavendish, Newton's theory meant the vibrations of the ordinary particles of bodies. Many examples of heat in the queries of Newton's *Opticks* agree with his answer to the question.[83]

By the time Cavendish worked out his Newtonian theory of heat, a good many arguments had been marshalled against the view of heat as motion, and we should know what he

[80]Cavendish (1783b, 150–151). The discussion of heat in this section draws on Russell McCormmach (1988). We acknowledge permission to use the material: University of Chicago Press, copyright 1988 by the History of Science Society, Inc., all rights reserved.
[81]Cavendish (1784b, 173–174).
[82]The revised draft of "Heat" consists of forty-three pages of text and notes, one page of diagrams with an accompanying page of explanation, and one page of additions and alterations. The original manuscript of both drafts of "Heat" is located in the Public Archives of Canada in Ottawa. The manuscript is published in Russell McCormmach (2004, 153–193). The page numbers of "Heat" in the following footnotes refer to the manuscript unless otherwise specified.
[83]Robert E. Schofield (1970, 13, 37, 48, 77–78, 84–85, 139, 160, 179, 183). Isaac Newton (1952, 348–349, 375–406).

was up against. One of the arguments was that cold is produced by mixing sal ammoniac and water, whereas in the mixing, particles are set in motion, which should register as heat rather than as cold. A related criticism was the apparent failure of liquids and gases to generate heat upon being agitated.[84] The specific heats of bodies were found not to be proportional to their densities, as the motion theory was understood to require. More objections to the motion theory were pointed out by the Jacksonian Professor of Natural Philosophy at Cambridge Isaac Milner in lectures he delivered in 1784–88. A basic objection, Milner said, was that vibrations of particles had not been proven to exist. Another objection was that heat was not observed to be proportional to motion. Another was that when oil and grease eliminate friction, heat seems to be eliminated too, although motion is communicated to the particles. Heat was observed to pass slowly through bodies, as a liquid might, rather than rapidly, as motion does. Heat should not spread at all, since the momentum of a system of particles is unaffected by their mutual actions and collisions. The passage of heat across the vacuum should be impossible, since there are no intervening particles to be set in vibration. The liberation of heat during the solidification of a liquid cannot be explained by motion, nor can the generation of cold during evaporation. The objections were serious, but Milner had answers, for as it happened he was a believer in the motion theory and a critic of the opposing material theories of heat. "The arguments against this [motion] Theory have of late Years been esteemed so numerous and weighty that it has almost been given up by Philosophers," but it has been given up "a little too precipitately," and Milner wished that "somebody else had endeavoured to shew the truth" of it by contrasting it with the fashionable material fluid theories of heat.[85] Cavendish set about to do that.

The difficulties of the motion theory could be seen as one general difficulty: new ideas for the theory had not kept pace with the rapid development of the experimental foundation of heat in the late eighteenth century, while the fluid theory of heat had developed together with the experiments.[86] Heat was one of a number of hypothetical fluids that had come to characterize British speculative natural philosophy from about the middle of the eighteenth century.[87] They were usually taken to be imponderable, indestructible, subtle, and closely associated with fire, and their particles were usually assumed to repel one another and to be attracted to the particles of ordinary substances. The fluid of heat had one quantitative property, the conserved quantity of heat, which was sufficient to account for the equilibrium of heat in bodies in contact and for most of the phenomena of heat.[88] The theory was readily grasped, plausible, and predictive, and like the motion theory it was considered to be Newtonian. Black's former students William Cleghorn and, if tentatively, Adair Crawford accepted it. Black himself was cautious on the subject of the nature of heat, but he said that Cleghorn's theory was the most likely to be true of any he knew.[89]

Investigators rarely needed to declare themselves for one or the other theory of heat, as they could carry out their experiments very well without doing so. A case in point is Lavoisier and Laplace's joint paper on calorimetry in 1783. Lavoisier almost certainly held the material theory of heat; what Laplace thought is uncertain, and he was later to hold the

[84] William Irvine (1805, 21–23).
[85] L.J.M. Coleby (1954, 242–252).
[86] Robert Fox (1971, 19, 22–23).
[87] J.L. Heilbron (1993a, 5–33. Schofield (1970, 157–190); P.M. Heimann (1981, 67–73). Arthur Quinn (1982, 127); McKie and Heathcote (1958). Fox (1971, 19–20, 22, 25).
[88] Schofield (1970, 185).
[89] McKie and Heathcote (1935, 28).

material theory, but in any case it was he who described the motion theory in their joint paper. Side-by-side with the motion theory, the authors presented the material theory, without deciding between the two.

Black and his followers had the common difficulty of being unable to form an idea of the internal motions of bodies that could account for the phenomena of heat, but Black's main objection to the motion theory was that none of its supporters had shown how to apply it to the entirety of the phenomena of heat,[90] a complaint which could not have been made about the material theory of heat after Cleghorn's theory in 1779. With his paper "Heat," Cavendish supplied what was missing from the side of the motion theory, Newton's theory together with comprehensive supporting evidence drawn from many parts of physical science.

Before proceeding further, we should consider a question readers might have. Because Cavendish successfully developed a theory of electricity based upon a fluid distinct from ordinary matter, it seems that a fluid of heat would have appealed to him as the starting point of a theory of heat. In the case of fluids the analogy between electricity and heat is obvious, but the analogy does not depend upon a fluid of heat, applying as well to heat as motion. In whatever way electricity and heat are conceived, their theories require two quantitative concepts, quantity and intensity: charge and potential in the first case, quantity of heat and temperature in the second. Also in both subjects, varieties of matter have defining characteristics: specific inductive capacities and conductivities in the first case, specific and latent heats (and thermal conductivities, but Cavendish did not investigate these) in the second.

We need to clarify a point in mechanics at the start. G.W. Leibniz, Newton's German contemporary and co-inventor of the calculus, made a distinction between "living force," or vis viva, and "dead force," or vis mortua. Dead force is the force that strives to generate motion; it is potential vis viva. Living force, commonly called the "force of moving bodies," is the force of a body in motion, which communicates motion in collisions. Vis viva obeys a law of conservation, its most useful property. The measure of vis viva is the product of the mass of a body and the square of its velocity, which readers may recognize as our kinetic energy only lacking the factor $\frac{1}{2}$; in practice vis viva usually appeared with $\frac{1}{2}$.

In Newtonian mechanics the measure of moving bodies is momentum, the product of the mass of a body and its velocity, not the square of its velocity. Unlike vis viva momentum is a directional quantity; like vis viva it obeys a conservation law. There was a long-standing controversy between British supporters of Newton's momentum and Continental supporters of Leibniz's vis viva over the proper measure of the force of moving bodies. Beginning in the middle of the eighteenth century, some writers on mechanics decided that both parties were right, that the dispute was over words, the two parties meaning different things by the words "force of moving bodies." A paper found among Cavendish's manuscripts shows that he agreed with them.[91]

Not long before Cavendish entered Cambridge, the Scottish mathematician Colin Maclaurin published an account of Newton's calculus, *A Treatise of Fluxions*. Recognizing that many able foreign mathematicians followed Leibniz, Maclaurin showed that Newton's and Leibniz's forms of mechanics gave the same results, though he had a preference: any solutions to mechanical problems obtained with the use of vis viva could be obtained from

[90] Schofield (1970, 186–187). Irvine (1805, 22)
[91] P.M. Heimann and J.E. McGuire (1970, 225–227).

Newton's universal principles, proven by the "most simple and uncontested experiments."[92] Cavendish knew Maclaurin's book, citing it in his plan of a treatise on mechanics. By founding Newton's theory of heat on vis viva, the invention of Newton's archrival Leibniz, Cavendish was not breaking faith with Newton but making good use of a common possession of different formulations of mechanics.

Vis viva cannot disappear without giving rise to a comparable effect, an equal quantity of potential motion. This property recommended vis viva for treating a range of mechanical problems, but it encountered difficulties in the case of collisions between bodies. From experience it was known that collisions are never perfectly elastic, implying that vis viva is lost, but it cannot really be lost. The missing vis viva was regarded as continuing on in hidden forms such as the compression of bodies or the motion of parts internal to bodies. Leibniz proposed the latter explanation, but he did not identify the hidden vis viva with heat, even though in the seventeenth century heat was commonly believed to be the internal motion of bodies. It would seem that the conceptual problems of treating heat as a quantity made this identification difficult.[93]

In an early unpublished paper, labeled by someone else "Remarks on the Theory of Motion," Cavendish discussed the usefulness of vis viva as a "way of computing the force of bodies in motion."[94] He said that vis viva was usually reserved for solving problems of machines used for "mechanical" purposes. The engineer John Smeaton wrote a paper about vis viva for fellow engineers, which he gave to Cavendish for comment.[95] For most questions arising in "philosophical inquiries," Cavendish wrote in "Remarks," the usual and most convenient way of computing the forces was Newton's momentum, but vis viva had a place. Instead of "vis viva," he spoke of the "mechanical momentum"[96] of bodies in motion; by this terminology, referring to both ways of computing the force of moving bodies as "momentum," he drew on his understanding that the use of one or the other was a practical choice, not one of fundamentals. What was fundamental is force, not the way it is measured. By assuming that forces are centrally acting, and that no force is lost by friction and inelastic collisions, Cavendish derived a general law of conservation of mechanical momentum and "additional momenta," or potential mechanical momenta. (Fig. 15.2). He extended the conservation law to encompass lost force by identifying heat with the mechanical momentum of the invisible vibrations of the particles of the bodies. He acknowledged that there were phenomena—the heats involved in fermentation, dissolution, and burning—that he did not know how to explain by his theory of heat. In "Remarks," he did not introduce the concepts of specific and latent heats, leading us to conclude that it was written before his experiments on latent and specific heats, placing it not later than the early 1760s. "Heat," which covers the same ground as "Remarks" but goes beyond, was written much later.

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[92] Colin Maclaurin (1742, 2:427, 433–434).
[93] Erwin N. Hiebert (1962, 80–93). P.M. Heimann (1977).
[94] Henry Cavendish (1921f); definition of "mechanical momentum" on 416.
[95] The paper Smeaton gave Cavendish to comment on was probably "New Fundamental Experiments upon the Collision of Bodies" (1782). J.G. Playfair (1822, 1:lxxxiii).
[96] Bernoulli first and then Smeaton called it "mechanic force." Newton treated the square of the velocity in the *Principia*, but he did not name it. W.H. Wollaston (1806, 16).

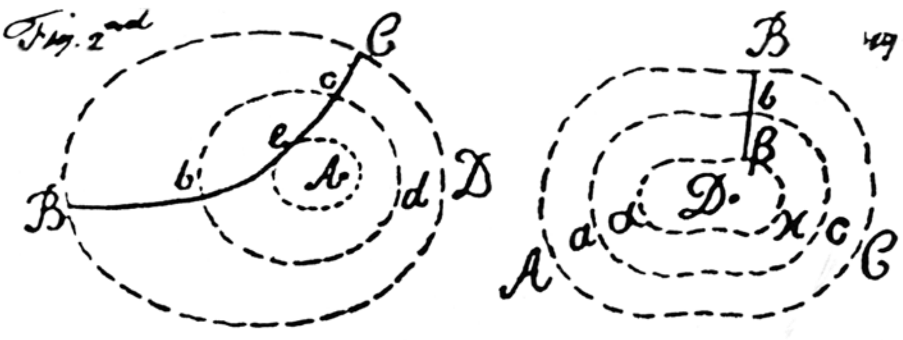

Figure 15.2: Forces. The dashed lines represent forces of attraction and repulsion of constant intensity centered on bodies or particles of matter, A and D. BC in the figure on the left and Bβ in the figure on the right are paths of a second attracting and repelling body or particle. With the aid of these diagrams and a proposition from Newton's *Principia*, Cavendish derived a general law of conservation of the sum of "real" and "additional" "mechanical momenta" (our kinetic and potential energies). It has been pointed out that Cavendish was struggling here with our concept of equipotential curves. "Remarks on the Theory of Motion," Cavendish Mss VI(b), 7: Plate 3; *Sci. Pap.* 2:430.

"Heat" carries no date. It was certainly written after 1783, for that year Cavendish rejected Black's term "latent heat" because it implied the material theory of heat, using instead expressions such as "heat is generated." In "Heat," he systematically used "latent heat," not because his opinion of the fluid theory of heat had changed, but because the expression had become standard, and he was writing to be read. In "Heat," he used another term he had avoided earlier, "vis viva" instead of "mechanical momentum," no doubt for the same reason. In the manuscript, he cited Priestley's history of optics, but that book appeared early, in 1772, and he cited the names, but not the publications, of Scheele and Horace Bénédict de Saussure for their work on radiant heat. Cavendish showed his familiarity with Scheele's only book, which appeared in English translation in 1780. His mention of Saussure no doubt referred to the second volume of his travels in the Alps, which came out in 1786.[97] The absence of citations to work done in the 1790s may be taken as indirect evidence for an upper limit for the dating of this manuscript.[98] Largely for these reasons, we place "Heat" in the late 1780s.

Cavendish begins "Heat" with a purely mechanical investigation, laying out propositions that parallel those in "Remarks," only developed more systematically and thoroughly. He defines vis viva as the mechanical effect of a body in motion, both "visible" and "in-

[97]Cavendish, "Heat," 23. Experiments on "heat rays" and light using polished metal and glass, discussed in Carl Wilhelm Scheele (1780, 72–74, 92–98). Saussure's account of experiments that he and M.A. Pictet carried out on the reflection of "obscure heat" emitted by hot, but not red hot, bodies, discussed in Saussure (1786, 354–355).
[98]For example, Pierre Prevost's experiments on heat rays and Benjamin Thompson's on the mechanical production of heat in the 1790s would have been relevant to Cavendish's argument, as perhaps would Herschel's experiments on radiant heat from 1800.

visible." The visible is the vis viva of the center of mass of a body undergoing progressive motion or of the body undergoing rotation or both; the invisible vis viva is that of the particles of the body moving among themselves; and the total vis viva of the body is the sum of both. He further divides the invisible vis viva into two parts: one is "active," the other inactive, with the potential for becoming active. His symbol s, standing for the active, is the actual vis viva of all the particles constituting the body; his symbol S stands for one half the sum of the vis viva that each particle would acquire by the attraction or repulsion of every other particle in falling from infinity to its actual position within the body. Upon assuming that the attractions and repulsions between the particles are always the same at the same separations and different at different separations, he derives the law of conservation of vis viva, active and inactive; the quantity s - S cannot change as a result of the motions of the particles among one another. Strictly speaking, the two quantities S and s individually change constantly because of the motion of the particles among one another, but because the number of particles is "inconceivably great" and because any increase in one quantity is matched by a decrease in the other, S and s do not sensibly change, unless there is an external cause.

Next Cavendish identifies the quantities occurring in the propositions about vis viva in mechanics with the quantities occurring in heat. According to his "hypothesis," "heat consists in the internal motion of the particles of which bodies are composed," which he regards as vibrations, the particles being bound close to their place by attracting and repelling forces. He identifies the "active heat" of the body with the actual vis viva, s, and the "latent heat" with the potential vis viva, - S, and consequently the "total heat" with s -S, the conserved quantity. "Sensible heat" is what Cavendish calls the heat of a body as given by a thermometer, and it is related to the active and latent heats through the constitution of the body. With these definitions, Cavendish has a technical vocabulary for the theory of heat.[99]

The first test of Cavendish's theory is its ability to account for the phenomena of heat itself. When two isolated and unequally heated bodies are brought into contact, one gives up heat and the other acquires it until the sensible heat of each is the same, the condition of equilibrium. In the exchange the total heat given up must be the same as the total heat received, but just how this heat is divided between the active and latent heats in the two bodies depends on the weights of the bodies and on "some function either of the size of their particles or of any other quality in them," for example, the frequency of vibration of the particles.[100] The distinctions, based on experimental knowledge, between sensible, total, active, and latent heats enable Cavendish to explain the phenomena of specific heat, the subject of his earliest experiments in heat.

Once he secures the vibrational theory of heat within its own field, heat, Cavendish applies it to other parts of physical science, first to optics. "There can be no doubt," he says, that light is a body consisting of extremely small particles emitted from luminous bodies with extremely high velocity. The particles of light are bound to their natural places in a body by the forces of attraction and repulsion of the particles of the body, and when the particles of the body are set in brisk vibration, the particles of light are moved into positions where they experience violent repulsion, flying off from the body as free light. When these particles

[99]Cavendish did not formally introduce a term for "specific heat"; he spoke of the "capacities for heat" of bodies. "Specific heat" first appeared in print in a publication by J.H. Magellan in 1780. Both terms are still used. Cavendish, "Heat", 11–12, 24, 41.
[100]Ibid., 14–16.

are reflected from a body, they are not reflected by a single particle or by a few particles of that body but by a great quantity of its matter, so that according to mechanical principles no perceptible vis viva is communicated to the body. The same explanation applies to the case of refracted light. But where light is absorbed, its particles are reflected back and forth within the body until their velocity is no greater than that of the particles of the body, "so that their vis viva will be equally distributed between the body & them," and the absorbing body will thereby acquire sensible heat.[101] Light falling on a body that does not have a mirror surface heats the body.

A plate of glass is heated more than a plate of polished metal when it is exposed to a fire or the Sun, but since the metal absorbs more light than the glass, according to Cavendish's theory it ought to be heated more than the glass. To resolve this apparent conflict with the theory, Cavendish refers to recent experiments by Scheele and Saussure on the newly discovered "heat rays." Cavendish assumes that heat rays, like light rays with which they comingle in various proportions, are material particles emitted by hot bodies, and although their velocity is unknown, they too must communicate vis viva. What is important is the way heat rays differ from light rays. Not only do they not excite the sensation of vision, but they are absorbed by glass and are efficiently reflected by polished metals, exactly the reverse of the behavior of light. It is the heat rays, then, and not the accompanying light rays, that warm the glass preferentially. These new invisible rays enable Cavendish to reconcile the facts with his theory of heat; if the rays did not exist, the theory would fail.[102]

Heat can be produced mechanically, for example, by friction and hammering. Because a violent force is required to produce heat, the particles of the heated body must be displaced or even torn away at its surface, and that in turn alters the latent heat of the body, giving rise to sensible heat. The same displacement and tearing away of particles are responsible for the loss of elasticity in the collision of two bodies or in the bending of a body. Cavendish's analysis of the forces of particles is more problematic here than in some other applications of the theory, but on the basic point Cavendish is "certain": if any visible vis viva is lost by the rubbing, striking, or bending of bodies, these bodies must acquire an "augmentation of total heat equivalent thereto."[103]

Electricity is the field that Cavendish had developed with the greatest thoroughness, and although he had examined the effect of heat on conduction, he had not examined the converse, the heat generated by conduction.[104] Now, he says, he is going to "argue upon the principles laid down in my paper concerning the cause of electricity" to derive a formula for the vis viva of electric fluid discharged by a Leiden jar through a wire. Because of their extreme lightness, he doubts that the particles of the electric fluid could communicate sufficient vis viva to the particles of the wire to account for the violent heat of the wire. His explanation is that the electric discharge displaces the particles of the wire, greatly diminishing its latent heat. The heat caused by electric discharge is consistent with the theory, though, Cavendish says, "it is an effect which I should not have expected."[105]

As the final application of his theory, Cavendish discusses the expansion and change of state of bodies with heat. When a body is heated, the increase in the vibrations of its

[101] Ibid., 18–20, 25–26.
[102] Ibid., 23–24.
[103] Ibid., 26–31, on 31.
[104] Cavendish (1879, 324).
[105] Cavendish, "Heat," 41.

particles alters their mutual attractions and repulsions, which in turn alter the size of the body. When the vibrations become great enough, the attractions and repulsions of the particles vary sufficiently for the body to change its form and properties entirely. This is what happens in evaporation and in melting: the increased vibrations of the particles diminish their adhesion, making bodies more fluid. By the same reasoning, Cavendish explains why chemical decomposition and combination are promoted by heat.[106]

Cavendish had a question he could not answer about the change in size of bodies. He said that bodies always expand with heat, but why this is so, and "why the size of the body is never diminished thereby, I do not pretend to explain; but there seems no reason why it may not be so."[107] He seems to have been unaware that below 4°C, water expands with cooling when he wrote this. However, a few years earlier, he was aware of it. In a proposal of experiments on the freezing of mercury for the Royal Society, he wrote: "Water takes up most room when coold almost to freezing than it does at 40° of heat & conseq. a thermometer filled with water stands higher at the former degree of heat then at the latter." He was concerned that the liquids including mercury used in the thermometers planned for the experiments might behave like water as they approach the degree at which they freeze, which would "make a puzzle in the exper.," but the remainder of the experiment would show whether or not this is the case.[108] Around this time, the singular property of water became well known. In 1797 Count Rumford spelled out the beneficial effects for all life on earth of this "miraculous" exception to the "general law of nature" that all bodies contract upon cooling.[109]

The "Conclusion" of "Heat" begins: "It has been shown therefore by as strict reasoning as can be expected in subjects not purely mathematical, that if heat consists in the vibrations of the particles of bodies, the effects will be strikingly analogous, & as far as our experiments yet go, in no case contradictory to the phenomena." That is, Cavendish shows that the hypothesis is *sufficient* to explain the phenomena, establishing one half of the argument. To establish its *necessity*, the other half, he calls implicitly on the principle of causality. "To put the matter in a stronger light," he says, it "seems certain that the action of such rays of light as are absorbed by a body must produce a motion & vibration of its particles; so that it seems certain that the particles of bodies must actually be in motion." With that, Cavendish lets rest the case for Newton's theory of heat. He has implicitly answered the main objections to it by showing that the hypothetical vibrations can account not only for the heat of friction, for example, for which the motion theory would seem to be well suited, but also for heats accompanying changes of state for which the material theory of heat is well-suited. In each application of the theory he has suggested possible motions and configurations of particles, so that unlike earlier versions of the motion theory his could not be faulted for lack of clear ideas about the mechanisms. Confirmed by its consequences, his hypothesis meets the test of a good hypothesis.

Cavendish reserves judgment on the opposing theory of heat to the end of the "Conclusion." Given the evidence for the existence of internal vibrations, he writes, there is no

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[106]Ibid., 38–39.
[107]Cavendish, "Heat," in McCormmach (2004, 101, 231).
[108]Henry Cavendish, "List of some Exper. …," Cavendish Mss III(a), 4(a).
[109]Benjamin Thompson, Count Rumford, "On the Propagation of Heat in Fluids" (1870–1875, 1:239–400, on 308–33). Tiberius Cavallo referred his readers to Rumford's discussion of this exceptional property of water (1803, 3:35–37).

reason to "have recourse to the hypothesis of a fluid, which nothing proves the existence of." He continues:

> The various hypotheses which have been formed for explaining the phenomena of heat by a fluid seem to show that none of them are very satisfactory; & though it does not seem impossible that a fluid might exist endued with such properties as to produce the effects of heat; yet any hypothesis of such kind must be of that unprecise nature, as not to admit of being reduced to strict reasoning, so as to suffer one to examine whether it will really explain the phenomena or whether it will not rather be attended with numberless inconsistencies & absurdities. So that though it might be natural for philosophers to adopt such an hypothesis when no better offerd itself; yet when a theory has been proposed by Sr I[saac] N[ewton] which, as may be shewn by strict reasoning, must produce effects strongly analogous to those observed to take place, & which seems no ways inconsistent with any, there can no longer be any reason for adhering to the former hypothesis.[110]

Cavendish does not criticize the material theory of heat for any specific failures; he criticizes it only for the kind of theory it is, prone to inconsistency, absurdity, and imprecision. To prefer it to Newton's theory is unreasonable. Three times in the "Conclusion" Cavendish uses the expression "strict reasoning." He used it before in his electrical theory: the method he proposed to follow there was first to lay down the hypothesis and then "to examine by strict mathematical reasoning, or at least, as strict reasoning as the nature of the subject will admit up, what consequences will flow from thence."[111] He uses the same method and the same wording in "Heat." Strict reasoning leads to correct conclusions.

We move on to experiments. To calculate the vis viva of light, Cavendish introduces Michell's experiment to "ascertain the momentum of light," widely regarded then as proof that light consists of streaming material particles.[112] Inside a box with a window for admitting direct sunlight, a thin sheet of copper was fastened to one end of a horizontal wire and balanced by a weight at the other end. Rays of the Sun were concentrated and directed by a concave mirror so that they struck the copper plate perpendicularly, resulting in a rotation of the wire.[113] From the observed speed of rotation and other details of the experiment, and from the assumption that the light was perfectly reflected from the copper, Cavendish calculates the vis viva of the sunlight falling each second on $1\frac{1}{2}$ square feet of surface. He translates this result into its mechanical effect: the rate of vis viva of sunlight falling on that surface exceeds the work done by two horses, that is, it exceeds two horsepower.[114] If the same quantity of light were absorbed by a fixed body instead of being reflected, the body would gain an equivalent quantity of heat and its temperature would rise. From this line of reasoning, Cavendish proposes two experiments, indicating that he intends to follow up his theory with an experiment to determine the mechanical equivalent of heat.

The first experiment is to "expose thermometers whose bulbs are coated with various dark & equally dark colord substances alternately to the ⊙ & shade & see whether they

[110] Cavendish, "Heat," 42.
[111] Henry Cavendish, *PT*: 61 (1771):584.
[112] G.N. Cantor (1983, 57).
[113] Joseph Priestley (1767, 1:387–389). Cantor (1983, 57). S.G. Brush and C.W.F. Everett (1969).
[114] Cavendish, "Heat," 22.

receive the same increase of heat in the same time." This experiment follows up what Cavendish calls "a necessary consequence of this theory." He gives no details, but experiments at the time on the heat of sunlight suggest what he had in mind. The Cambridge professor of chemistry blackened the ball of a thermometer and exposed it to direct sunlight, finding that it registered a higher temperature than when the bulb was not blackened. He expressed the wish that others would repeat the experiment with different colored paints on the thermometer to determine the ability of colors to receive and retain heat.[115] The Royal Society's Bakerian lecturer took up this suggestion, trying a variety of colors to see if the absorption of heat followed the progression of prismatic colors or some other law. He also exposed a blackened thermometer alternately to the Sun and shade, concluding that every degree of light was accompanied by a proportionate degree of heat.[116] In or around the year of Cavendish's theory, 1787, George Fordyce described an experiment to see if sunlight falling on blackened surfaces of different substances heated them equally. He was interested in a general question, whether or not the same cause of heat always produces in the same body the same quantity of heat; for example, "whether a chemical attraction taking place between equal quantities of two substances shall always produce an equal quantity of heat." He came to the question by observing reverberatory furnaces, wondering if by burning the same quantity of fuel the same quantity of heat would be produced. His experiment was indecisive, but he concluded that heat cannot be material, that it is a quality that might or might not be motion.[117] In the experiment Cavendish proposed, on the basis of his theory, the heating effect of rays from the Sun falling on equally dark surfaces presumably should be the same, since the rays would be completely absorbed, their vis viva registering as heat.

The second experiment is brought up in two places, the first in the preliminary sketch of the paper, where Cavendish speaks of a "calculation": "Calculation of vis viva of ⊙s rays & Do required to commun. given quant. heat." In the foul copy of the paper, the experiment is described: "Exper". to determine the vis viva necessary to give a given increase [of] sensible heat to a given body by alternately exposing a thermometer to the ⊙ & shading it."[118] The plan of the experiment seems to be this. Cavendish would calculate the vis viva per second of sunlight striking the surface of a blackened thermometer from the measurements in Michell's experiment with the light-mill. He would read the change in temperature directly from the thermometer exposed to the Sun's rays for a given time; this would be proportional to a quantity of heat equivalent to the vis viva of the light absorbed by the blackened thermometer. He would determine this quantity of heat from the change in temperature and the measurable heat capacity of the thermometer. The proportionality of the calculated vis viva and the measured quantity of heat would give a numerical value for the mechanical equivalent of heat. Because the light-mill was misunderstood, the value would have been wrong, but that is beside the point here. The expression "mechanical equivalent of heat" is our term, not Cavendish's; we note the anachronism, but the meaning is the same, and as we have seen, elsewhere in "Heat" he uses the word "equivalent" in this connection.

In light of Cavendish's reputation for anticipating results arrived at only much later by others, we might expect him to have calculated a value for the mechanical equivalent of heat. The value is implicit in his theory, and he apparently had it in mind in the experiment just

[115]Richard Watson (1773).
[116]Tiberius Cavallo (1780, 591–594).
[117]George Fordyce (1787).
[118]Cavendish, preliminary sketch and foul copy, "Heat," in McCormmach (2004, 153, 162).

described. Moreover he had in hand the concepts and units for expressing it: his measure of vis viva is mechanical work, the lifting of weights through a height, our foot-pound, and his measure of the quantity of heat is the same as ours, the heat required to raise the temperature of a unit weight of water 1° by the thermometer, our Brtish thermal unit. A determination of the mechanical equivalent would have made his hypothesis quantitatively complete. A parallel is the hollow-globe experiment, which by establishing the law of electric force made the hypothesis of his electrical theory quantitatively complete. This time Cavendish did not live up to our expectation, and we may ask why.

There is no record that Cavendish performed the experiment with the thermometer. He may have had reservations about Michell's experiment, which he depended on for calculating the vis viva of sunlight. When Michell performed the experiment, the concentrated rays of the Sun generated a great deal of heat, sufficient to melt the copper vane and disable the apparatus. This may have suggested to Cavendish that the heat of the air on the sunlit side of the vane, not the momentum of light, caused the arm to rotate. In any case that is the explanation of Michell's experiment, as explained by Abraham Bennett in 1792.[119] There were alternatives to Michell's experiment. In the next century James Prescott Joule gave the simplest and most persuasive demonstration of the mechanical equivalent of heat using a paddlewheel powered by descending weights.[120] Cavendish might have considered and rejected such an experiment for the reason Thomas Young gave in his text on natural philosophy: fluids cannot acquire any sensible increase in heat from internal friction, one of the standard arguments against the motion theory, mentioned above by Milner.[121] Like the heat of the emission of light, the heat of internal friction was thought to be too small to measure. Cavendish proposed an experiment on the production of heat by friction, "exper. whether friction is as much diminished by oil & grease as the heat is," but he said too little to know what he planned.

A value for the mechanical equivalent of heat would have joined a small number of useful physical constants such as the velocity of light and the acceleration of gravity, though as an equivalence it was a different kind of constant. It probably would not have had the importance to Cavendish as it does to us, a possible reason he did not pursue it further. Because he did not express physical relations as equations between terms with physical units, conversion factors and other physical constants did not come up as a matter of course as they do in modern physics; we discuss this point later in connection with the universal gravitational constant G. That he did not place particular importance on the mechanical equivalent of heat in its own right is further suggested by the full description of the experiment: he proposed to determine the vis viva needed to increase the temperature of a body a given amount "& thereby to give a guess at the velocity with which the particles of a body vibrate supposing that the total heat of a body heated to 1000° is double its heat at 0°."[122] From his statement of the problem, we deduce that the average velocity of the particles of the body in meters per second is $\sqrt{4000J}$, where J is the mechanical equivalent of heat using the Fahrenheit scale for temperature. Inserting today's value for J, and converting meters to inches, Cavendish's unit, the average velocity of the particles of the body at 1000° is about 6800 inches per second. Cavendish would have found J from Michell's experiment and his own experiment

[119] Abraham Bennett (1792, 87–88).
[120] Heintz Otto Sibum (1995, 73–74, 104–105).
[121] Young (1807, 1:655).
[122] Cavendish, "Heat"; in McCormmach (2004, 162).

exposing a blackened thermometer to the Sun, discussed above. Cavendish's estimate of the velocity of particles points to his interest in the physical reality described by Newton's theory of heat.

As to the immediate stimulus for writing the paper, Cavendish said nothing. In 1783 he received Lavoisier and Laplace's paper on calorimetry, which he read with critical interest.[123] Unlike the usual statements of the motion theory of heat, which did little more than assert the identity of heat and motion, Laplace's presentation was mechanically precise: "Heat is the vis viva resulting from the imperceptible motions of the constituent particles of a body." He pointed out that just as in the material theory of heat, in which the quantity of fluid is conserved, in the motion theory there is also a conserved quantity, vis viva. By an appeal to the law of conservation of vis viva, he explained how heat is communicated from one body to another: when two bodies of unequal temperatures are brought into contact, the vis viva of the warmer body diminishes while that of the cooler body increases until the temperatures are equalized, at which time the vis viva exchanged in each direction is identical. This is the same insight as Cavendish recorded in his early "Remarks on the Theory of Motion." In 1783 Cavendish and Laplace were both thinking about the motion theory of heat. In May of that year Cavendish's paper on the freezing point of mercury with its assertion of Newton's theory of heat was read to the Royal Society, and next month Lavoisier and Laplace's paper was read to the Royal Academy of Sciences. In Laplace's statement of the motion theory of heat, Cavendish read a reflection of his own reasoning, a possible stimulus for him later to return to the theory and improve it.

In 1785 Fordyce published an experimental paper in the *Philosophical Transactions* demonstrating a loss of weight by ice upon melting. Because the ice apparently lost weight as it gained heat, Fordyce speculated that heat might be a body possessing absolute levity, though he was inclined to believe that heat is a completely general quality like attraction, only its opposite.[124] If Fordyce's experiments were proven right, Blagden told Laplace, they would bring about an "extraordinary revolution in our ideas."[125] That was recognized by Benjamin Thompson, who in 1787 repeated Fordyce's experiments, convincing himself that heat could not be a material substance.[126] Cavendish had earlier witnessed experiments like Fordyce's, and he was kept informed on pertinent researches in Paris.[127] When Fordyce's experiment on ice was announced, Cavendish had just published his experiments on air, which included experiments disproving Warltire's contention that heat has weight. We doubt that Fordyce's paper on heat or any other theoretical or experimental paper on heat around 1787 was the occasion for Cavendish to write "Heat"; if it had been, he would have mentioned it. Nor, we believe, was the occasion any new work of his own. The central idea of "Heat", the identification of heat with vis viva, had occurred to him long before, at the time he wrote "Remarks," and he had performed the relevant experiments on specific and latent heats in the 1760s.

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[123] Antoine Laurent Lavoisier and Pierre Simon Laplace (1982, 4–6). Henry Guerlac (1976, 244–248). Charles Blagden to A.L. Lavoisier, draft, 15 Sep. 1783, Blagden Letterbook, Yale.
[124] George Fordyce (1785, 364); Coleby (1954, 245).
[125] Charles Blagden to Pierre Simon Laplace, 5 Apr. 1785, draft, Blagden Letterbook, Yale; Charles Blagden to Lorenz Crell, 28 Apr. 1785, draft, ibid.
[126] Sanborn C. Brown (1979, 219–220).
[127] John Roebuck (1776). These experiments, witnessed by Cavendish among others, showed an increase of weight in iron and silver upon cooling, a result in agreement with Fordyce's later experiment. Charles Blagden to Henry Cavendish, n.d., [1785]; in Jungnickel and McCormmach (1999, 608–609).

There are, however, several circumstances that may have affected his decision. The first is a widening interest in heat: several books published around 1780 called attention to the problem of the cause of heat, and an unusual number of papers on heat appeared in the *Philosophical Transactions* in 1787–88. The second circumstance is Cavendish's involvement in experimental heat in 1783–88. He carried out experiments of his own—on freezing mercury, freezing mixtures, and cold produced by expanding air[128]—and he devoted a good deal of attention to experiments on heat carried out by others at Hudson's Bay under his direction. These varied experiments might have been a stimulus, since they were about change of state and the thermal effect of mixing acids with water, phenomena which he did not address in his first discussion of Newton's theory of heat in "Remarks." The third circumstance is his work in chemistry. He distinguished his explanation of the production of water from Watt's by their different ideas on the nature of heat. In 1786, in a book on the latest advances in heat, light, and pneumatic chemistry, the Irish physician and chemist Bryan Higgins said that he did not need to justify his preference for the material view of heat because Cavendish, Black, and other distinguished natural philosophers "have accepted it."[129] Higgins admired Cavendish for his "precision in conducting experiments," to whom "moden Philosophy […] owed more […] than to any other man now living, except Dr. Franklin, deeming him truly worthy of […] the immortal Newton,"[130] but he was almost certainly mistaken about Cavendish's view of heat. If Cavendish read his book, he would have realized how incompletely he had informed the scientific world, a conceivable motivation for working up a paper with the intention of publishing it. The fourth circumstance is a widespread skepticism about the motion theory of heat. We mentioned numerous arguments against it. From around 1780, the fluid theory of heat came to be increasingly adopted, as notable supporters of the motion theory of heat abandoned it: Magellan in 1780, Cavallo in 1781, Macquer in 1784, and Fourcoy in 1786.[131] Other supporters of the motion theory of heat were seen to waver. An example is William Nicholson, who in his treatise on natural philosophy in 1782 wrote that the view of heat as the vibration of particles was "scarcely hypothetical," and that to postulate a fluid of heat was tantamount to multiplying causes in violation of the rules of scientific reasoning; moreover, such a fluid demanded scarcely credible, "amazing" properties. Eight years later, in his treatise on chemistry, Nicholson left undecided the nature of heat, calling it a "great question" deserving the attention of natural philosophers.[132] By the time of Cavendish's theory, the material theory of heat had acquired a large following, and by the end of the century the material theory was all but universally accepted in Britain. The arguments about heat were usually carried on among followers of the material theory themselves rather than between them and upholders of the motion theory.[133] In 1804 John Leslie, a former student of Black's, said that there were still some adherents of the motion theory of heat, but they were badly misguided. There were "insurmountable objections" to that theory; in addition to its being "vague and undefined," a "shapeless hypothesis," "merely nugatory,"

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[128] 27 Feb. 1783, Charles Blagden Diary, Royal Society. Charles Blagden to Erasmus Darwin , 14 Sept. 1786, draft, Blagden Letters, Royal Society 7:34. Henry Cavendish (1921b).
[129] Bryan Higgins (1786, 301–302).
[130] Joseph Priestley (1775, 16).
[131] Fox (1971, 23, 28).
[132] William Nicholson (1781, 1:134; 1790, 6).
[133] Fox (1971, 19–20, 23, 104–105).

it "explains nothing."[134] Cavendish responded to the trend in thinking about heat with an intended publication of a fully up-to-date version of the Newonian theory of heat.

The fifth circumstance is the state of natural philosophy at the time. The understanding of the physical world that had guided Cavendish's researches for twenty years was under attack or ignored. The elements of the new chemistry listed caloric, and pneumatic chemistry was acquiring a caloric theory, according to which particles of gases are surrounded by a repellent fiery matter. The ether and the imponderable fluids were widely thought to have provided a foundation for natural philosophy. Cavendish never mentioned the ether; as we have seen, he denied that heat is material; he believed that light has weight; and he never referred to magnetic fluids. He accepted that electricity is a fluid distinct from ordinary matter, but his electrical theory was ignored by his British colleagues and was all but unknown abroad.[135] He never referred to phlogiston as imponderable or as having negative weight or as incapable of being isolated and studied in its own right. With "Heat," Cavendish demonstrated that a principal direction of Newton's natural philosophy was capable of accommodating recent experimental advances.

The final circumstance is the abundant practical applications of heat at the time. In 1785–87, as we will see, Cavendish and Blagden made journeys to various parts of Britain, visiting industrial works wherever they went, making close observations of blast furnaces and steam engines. "Heat" contains no discussions of such applications, yet by repeated exposure to examples of the conversion of heat into mechanical work and of chemical reactions generating heat in industrial furnaces, Cavendish's thoughts would have been directed to the subject of "Heat."

If Cavendish had carried out his original intention, he would have submitted his paper to the Royal Society and a slightly abbreviated version of it would have been read at a meeting. It would have been read in its entirety by the papers committee. Very few purely theoretical papers were published in the *Philosophical Transactions*, but Cavendish's electrical theory was, and his theory of heat would have been too. It was mathematical, and there were few mathematical natural philosophers in Britain, but a few were enough. Whatever their opinion on the nature of heat, knowledgeable readers would have recognized "Heat" as a well-constructed argument directed to a worthy question, the cause of heat. They would have acknowledged that it conformed to widely held objectives in natural philosophy. It was exact, potentially quantitative, and accessible to the instruments of experimental physics; it proceeded from the laws of nature and experimental facts, and it announced a new law of nature, the conservation of energy, which applied to every physical process, establishing connections within and between the parts of natural philosophy. The hypothesis laid down a cause of heat phenomena, vis viva, the force of moving bodies, and the theory developed its consequences throughout natural philosophy; it embraced most of the important facts of heat, and it was in an acceptable meaning of the term Newtonian. With "Heat," the theory of heat looked to join a select company of theories, gravitational astronomy and mechanics. So why did Cavendish not publish it?

[134] John Leslie (1804, 140–141).

[135] Blagden, upon delivering to Cavendish a gift of René-Just Haűy's new treatise on electricity and magnetism, which contained an electrical hypothesis similar to Cavendish's, observed that the author seemed unaware of Cavendish's paper of 1771: Charles Blagden to Claude Louis Berthollet, 11 Sep. 1787, draft, Blagden Letters, Royal Society 7:69.

That question is asked about Cavendish's other work too, but in the case of "Heat," the question is unavoidable, for unlike many of his researches, he intended this one for publication from the start. Perhaps Cavendish did not want to enter into rivalry, usually the first guess. This can be ruled out, since in his lifetime no similar work was published. Experiments on the mechanical equivalent of heat began to appear only in the 1840s, and publications on the mechanical theory of heat only in the 1850s. After a brief discussion by Daniel Bernoulli in the early eighteenth century, the next publication on the kinetic theory of gases came out after Cavendish had been dead for six years, and it did not identify heat with vis viva but with momentum. Because Newton's theory of heat was out of favor, Cavendish might have wanted to avoid the criticism that was certain to follow. However, he had allies. His colleague Thomas Young said in 1807 that the "most sober reasoners of the present" subscribe to the vibration theory of heat.[136] Perhaps Cavendish had unanswered questions. Where he discussed the heat of electrical discharge and the latent heat of the wire, he noted, "This must be examined."[137] But he had questions of the same sort when he began, as we know from preliminary notes he made for his paper. Nothing suggests that he found any disagreement with experiment. The mathematical development of his theory of heat fell short of that of his electrical theory, and explanations of phenomena were largely qualitative, but he knew that at the beginning too, and in his published paper on electricity he applied his theory to phenomena only qualitatively. The theory of heat did not obviously point to a new class of phenomena in the way the theory of electricity did, but it laid the foundation for the next stage of the science of heat. With reference to Cavendish's unpublished papers, Blagden said that "it is to be supposed that he afterwards discovered some weakness or imperfection in them."[138] General as it is, it is the best explanation we are likely to get.

The Natural Philosopher

Cavendish had mastered Newton's science, but he needed more than Newton gave him to make "Newton's theory of heat," and important as Leibniz's vis viva was, that did not give it to him either. Rather Cavendish drew on these sources and on his and others' experiments on heat, and by strict reasoning he brought them together to make the theory he presented in "Heat."

The persuasiveness of "Heat" derives from its coherence, comprehensiveness, and exactness, which includes mathematical reasoning where it applies. In one after another branch of natural philosophy, Cavendish demonstrates that Newton's theory does "really explain" the phenomena of heat. "Heat" is a continuous argument for the hypothesis that heat consists of the invisible vibrations of bodies; it is a study in *understanding*.[139]

There are various ways of showing why Cavendish is seen as a natural philosopher, and "Heat" is one of them. "Philosophy," the natural philosopher and geologist James Hutton wrote, is the aim of science. Although natural philosophy cannot advance one step without experiment, unless experiment is guided by philosophy it can produce only endless collec-

[136] Young (1807, 1:656). In addition to Young, they included Humphry Davy, Benjamin Thompson, and Cavendish. Schofield (1970, 290–295).

[137] Cavendish, "Heat"; in McCormmach (2004, 190).

[138] Blagden, in the family obituary of Henry Cavendish.

[139] "Heat" disproves Yukitoshi Matsuo's assertion of Cavendish's "failure to unify a variety of heat phenomena in terms of dynamics and his subsequent abandonment of a systematic consideration of them" (1975, 93–94).

tions of facts, and that, Hutton said, is not philosophy. "The disposing of one fact, that is, the putting it into its proper place in science or the general order of one's knowledge, is doing more for natural philosophy, than a thousand experiments made without that order of connection or relation which is to inform the understanding." William Enfield, an instructor at Warrington Academy, identified natural philosophy with the ordering of scientific facts within general truths. Honor is bestowed on those who enhance the public store of experimental facts, and "one who proceeds thus far, is an experimentalist; but he alone, who, by examining the nature and absorbing the relations of facts, arrives at general truths, is a philosopher." The natural philosopher Hugh Hamilton wrote, "It is the business of natural Philosophy to reduce as many Phaenomena as may be to some general well-known Cause."[140] In "Heat," Cavendish reduces the phenomena of heat to a cause, vibrating particles together with their vis viva and the law of conservation of energy, and he works out the theory of heat within a general order of understanding, mechanics. He shows connections between phenomena belonging to all parts of natural philosophy, arriving at "general truths." Earlier we saw that in electricity, he made connections between the principal phenomena of that field. He met the criteria of a natural philosopher.

In "Heat" Cavendish tells how he thinks the physical universe is constituted. The totality of the material world and its activity arise from attractive and repulsive forces, the all-embracing general truth. Cavendish brings the perspective of the natural philosopher to bear on his discussion of the heat of friction and hammering:

> According to father Boscovich & Mr Michell matter does not consist of solid impenetrable particles as commonly supposed, but only of certain degrees of attraction & repulsion directed toward central points. They also suppose that the action of 2 of the central points on each other alternately varies from repulsion to attraction numberless times as the distance increases. There is the utmost reason to think that both of these suppositions are true; & they serve to account for many phenomena of nature which would otherwise be inexplicable. But even if it is otherwise, & if it must be admitted that there are solid impenetrable particles, still there seems sufficient reason to think that those particles do not touch each other, but are kept from ever coming in contact by their repulsive force.[141]

Matter likely is nothing other than centers of force. Cavendish thinks that Boscovich and Michell are probably right about matter, but it would change nothing in the argument if Newton, who believed in solid impenetrable particles, is right, for in either case the force of repulsion keeps particles from touching and losing vis viva. The idea of alternating attractive and repulsive forces entered explicitly in several researches of Cavendish's,[142] but only in "Heat" did he say where it came from.

[140]Hutton (1794, 36). Hugh Hamilton (1766, 36).
[141]Cavendish, "Heat," in McCormmach (2004, 187–188).
[142]He introduced it in his theory of boiling. Cavendish, "Theory of Boiling," 361. He used it to reconcile his theory of electricity with experiments. Maxwell in Cavendish (1879, 174–175). He analyzed the error of a magnetic dipping needle by assuming that the axis of the needle and the plane on which it rolls are prevented from actually touching by a repulsive force. Henry Cavendish, "On the Different Construction of Dipping Needles," Cavendish Mss IX, 40:12–14. In "Heat," he used the idea to resolve difficulties with the heat of friction in his theory of heat, and he used it to derive the general law of conservation of energy, which applies to forces and heat wherever they appear in natural philosophy.

Boscovich published his idea of forces in his *Theoria philosophiae naturalis* in 1758.[143] Michell arrived independently at a similar idea, which his friend Priestley published in his history of optics in 1772.[144] There was a British tradition paralleling Boscovich's idea,[145] which may have been more important, though Boscovich developed the concept of central points interacting through central forces in greatest detail. In Boscovich's theory of natural philosophy, at close separations central points experience infinite repulsion; at large separations they experience gravitational attraction; and in between they experience attractions and repulsions responsible for cohesion, vaporization, and a variety of other chemical and physical phenomena. He represents his universal "law of forces" by a continuous curve: above the axis the force is repulsive, below the axis it is attractive, and places where it passes between repulsion and attraction mark the limits of cohesion. When disturbed, central points vibrate around these places, and the vibrations continue indefinitely until the central points are again disturbed. The area between the curve and the axis is proportional to vis viva, the measure of the action of the force across a distance. In a general way, Boscovich's law, by accounting for combustion, dissolution, and fermentation, and by implying perpetual vibrations of particles and the conservation of vis viva, supports Cavendish's theory of heat.[146]

Bodies act on bodies across a distance. Blagden recorded in his diary that Cavendish "argued that one had no right to say that matter could not act where it was not: one knew nothing about it but from experience, & experience rather led to believe that it might."[147] The explanation of this is forces acting at a distance. The physical universe is constituted of such forces, the basic, irreducible concept of natural philosophy. The entire human experience of nature testifies to the existence and ubiquity of forces, making a case for Newton's view that the main task of natural philosophy is to determine the forces of nature. As we saw in the chapter on chemistry, the exact description of the forces responsible for the phenomena of heat was beyond the capability of natural philosophy in Cavendish's time. They act over short distances only, and no "universal synthesis of short-range forces" had been established.[148] This did not imply, however, that nothing could be learned from such forces. Newton had shown in his derivation of the sine law of refraction for individual rays of light that it is possible to determine rigorously some results of importance without knowing "what kind of Force" is acting, assuming only very general properties of the force.[149] In his derivation of the conservation law, Cavendish did exactly this, assuming only that the force with which particles attract and repel each other "is every where the same at the same distance however different at different distances."[150] When the conservation law was derived again in the next century from the same general idea of forces, it expressed the prevailing belief

[143] Cavendish would have read about Boscovich's force in his *Theoria*. Cavendish and Michell met Boscovich on his tour of England, both dining with him at the Royal Society Club on 5 June 1760, and Cavendish with him again on 26 June 1760: Minute Book of the Royal Society Club, Royal Society, 4.

[144] Priestley (1772c, 1:309–311, 392–393, 786–791).

[145] Cantor (1983, 71–72); Schofield (1970, 237–238); P.M. Heimann and J.E. McGuire (1971, 233–306).

[146] It makes no difference here that Boscovich believed in the matter of fire; Roger Joseph Boscovich (1966, 22–23, 43, 73, 76–96). Boscovich did not have a conservation law and he generally regarded vis viva as having little significance. Thomas L. Hankins (1965, 294), and on Boscovich, 291–297; on Michell and Boscovich, Schofield (1970, 36–49).

[147] 22 Nov. 1804, Charles Blagden Diary, Royal Society 4:284.

[148] Cantor (1983, 87).

[149] Newton (1952, 82).

[150] Cavendish, "Heat," in McCormmach (2004, 179).

that forces are not destroyed, only converted to other forces, inspiring investigations that would redraw the map of the physical sciences. Cavendish was onto a good idea.

In the introduction to this book, we give our reasons for preferring to speak of Cavendish as a "natural" philosopher rather than as a "Newtonian" philosopher, but that is not to deny that he was also a Newtonian of a certain persuasion. We return to this point even as we call attention to his differences with Newton and to sources other than Newton for his understanding of forces. To characterize fully Cavendish's Newtonianism would be to repeat much of what we have said about him from his education at Cambridge onwards. His researches in heat contain his most telling statement on the subject. "When a theory has been proposed by Sr I[saac] N[ewton]," Cavendish wrote, and it is in agreement with experience, there is no reason to accept an alternative theory. To no other authority did he give an endorsement like this. In "Heat" he did not weigh the evidence for the competing theories of heat, Newton's and the material, but developed only the former and rejected the latter. If there is the suggestion of a doctrinaire element in Cavendish's thinking, it remains that the theory of heat as motion would be vindicated in the next century.

Another aspect of Cavendish's Newtonianism placed him not in the vanguard of but in opposition to the next development in science. In America, Francis Hopkinson observed that when he viewed a lamp through a silk handkerchief and moved the handkerchief before his eyes, he saw dark bars which did not move. Hopkinson took his "optical Problem" to the astronomer David Rittenhouse, who performed experiments with a square of parallel hairs, observing that the lines seen through it varied in strength and color. Doubts about Rittenhouse's experiments were expressed at a meeting of the Royal Society. "Lord Cavendish," Hopkinson wrote to Thomas Jefferson, performed the experiment and "declared it was truly stated." Cavendish had a good opinion of Rittenhouse, being the first to sign the certificate recommending him for membership in the Royal Society. What Rittenhouse had constructed was a diffraction grating, which would be used to measure the wavelength of light. No doubt Cavendish had an explanation for Rittenhouse's experiment, probably agreeing with Rittenhouse's own, which was that it was an instance of the inflection of light by bodies, as described by Newton and explained by the particle theory of light. With hindsight, it would seem that Rittenhouse's experiment was a missed opportunity for Cavendish, but then other people missed it too, and there was another explanation for it.[151] Cavendish continued to hold to the particle theory of light after Thomas Young introduced the wave theory of light in 1800: in or after 1804 Cavendish calculated the gravitational bending of light passing near the surface of a body such as the limb of a star or the edge of a hair.[152]

As Young understood him, it was "Newton's opinion, that heat consists in a minute vibratory motion of the particles of bodies, and that this motion is communicated through an apparent vacuum by the undulations of an elastic medium, which is also concerned in the phenomena of light."[153] Young's understanding pointed to the physics of the ether, the origin of unified views of nature in the nineteenth century. Cavendish's understanding of Newton's opinion did not include an ether, or if it did he never mentioned it. So far as we know, he held to the view that the phenomena of nature have a uniform cause in attractive and repulsive,

[151] Francis Hopkinson to Thomas Jefferson, 14 Apr. 1787; in Boyd (1955, 288–290). Brooke Hindle (1964, 276–277). John C. Greene (1984, 158–160). 6 Nov. 1794, Certificates, Royal Society 5.
[152] *Sci. Pap.* 2:437. The calculation is undated, but an inspection of the watermark on the paper shows that it could not have been earlier than 1804.
[153] Thomas Young (1802, 149).

centrally acting forces. This view, together with mechanical theorems about the measure of the force of moving bodies, vis viva, permitted him to display a connectedness between the several major domains of phenomena constituting the broad field of natural philosophy.

Workplace

Cavendish was able to develop a comprehensive theory of heat because of his exhaustive experimental study of heat, as described earlier in this chapter. To judge from the laboratory record of his experiments on heat, and that of his experiments in other fields, he spent as much time in the laboratory as he did in his study. For a few laboratories of the time, there exist drawings. We do not have one of Cavendish's, but we have the next best, sketches he made of various apparatus, which give the reader an idea of what he would have seen if he had entered his workplace. Or what would have greeted her: John Davy recalled that a lady of rank—he thought she was the duchess of Gordon—upon visiting Cavendish at Clapham expressed surprise at seeing a long row of utensils, which turned out to be objects used in the crystallization of saline solutions.[154] For most of Cavendish's experiments, his laboratory was inside his house. Since Cavendish's laboratory had to be versatile, we include his drawings of apparatus for several fields.

[154] Wilson (1851, 178–179).

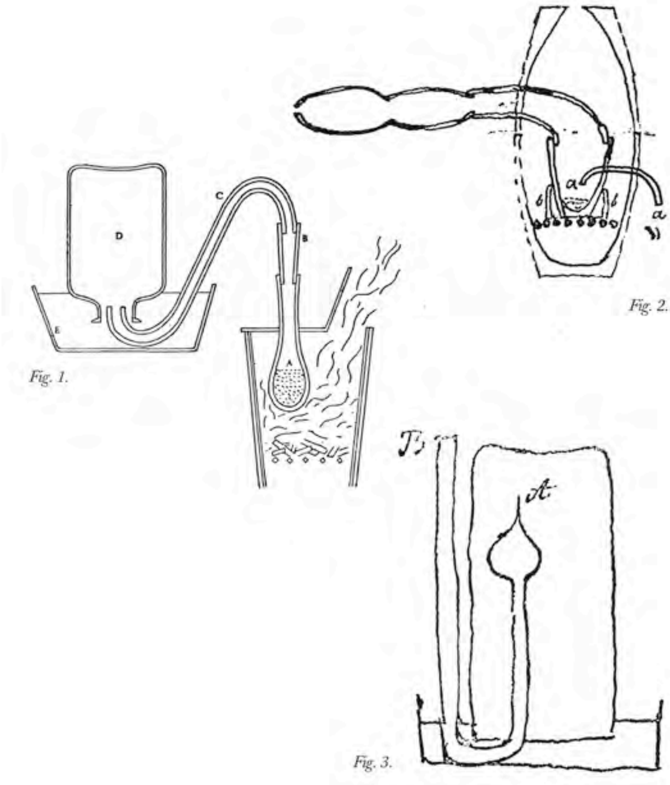

Figure 15.3: Laboratory Apparatus. Figure 1. Apparatus for distilling vegetable and animal substances. A bottle for collecting air D is filled with water and then is inverted into vessel E filled with water. *The Scientific Papers of the Honourable Henry Cavendish*, ed. E. Thorpe, 2 vols. (Cambridge: Cambridge University Press, 1921) 2:308; hereafter in the captions *Sci. Pap.* Figure 2. Apparatus for subliming arsenic in a crucible, with a set of aludels attached, placed within a reverberatory furnace. Cavendish Mss II, 1(b): 21. Figure 3. Apparatus for measuring the expansion of air with heat; the bent tube contains mercury and air. Cavendish, *Sci. Pap.* 2:374. Reproductions by permission of the Chatsworth Settlement Trustees.

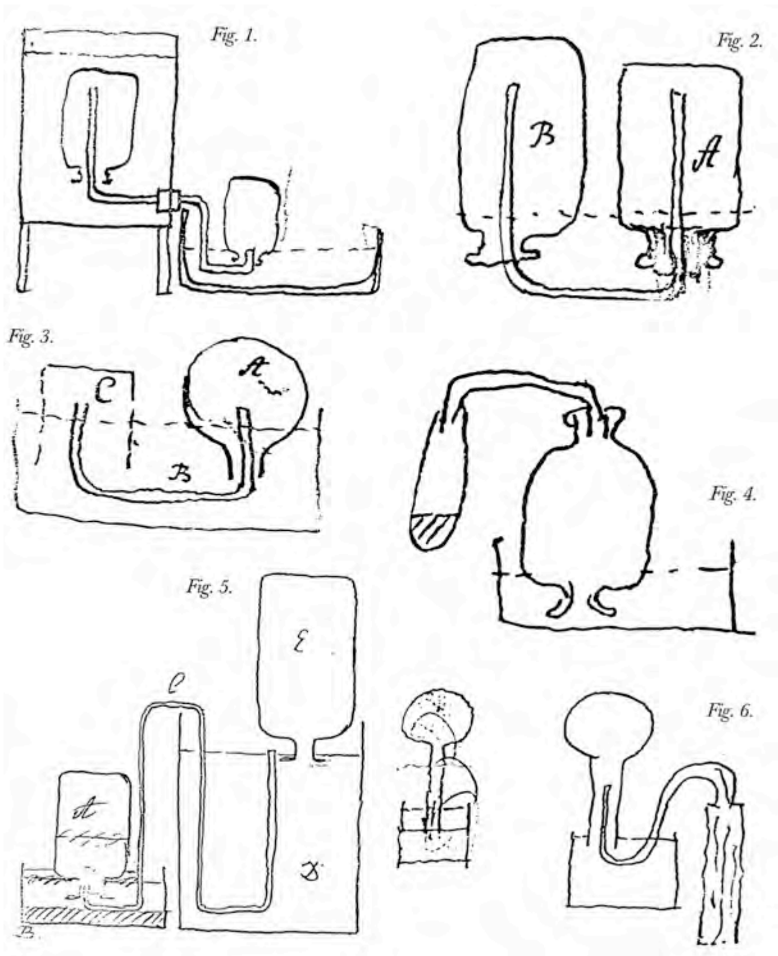

Figure 15.4: Laboratory Apparatus. Figure 1. Apparatus for an experiment to decide if heavier airs in a mixture of airs settle to the bottom. The mixture is contained in the bottle on the left, and as water is gradually let into it, different samples are caught in bottles on the right. Cavendish Mss II, 5:102. Figure 2. Apparatus for eudiometer experiments. Bottle B is filled with nitrous air, bottle with common air. Ibid. 5:42. Figure 3. Sulfur is burned in the glass globe A, and the air that is forced out by the heat is caught in jar C and examined, as part of Cavendish's eudiometer tests. Ibid. 5:61. Figure 4. Apparatus for capturing air upon boiling burnt charcoal with spirit of niter. Ibid. 5:345. Figure 5. Apparatus to determine if fixed air is produced by mixing common or dephlogisticated air in bottle A with nitrous air in bottle E. Ibid. 5:5. Figure 6. Apparatus to determine the effect on the volume of dry air by saturating it with moisture. Cavendish Mss Misc. Reproductions by permission of the Chatsworth Settlement Trustees.

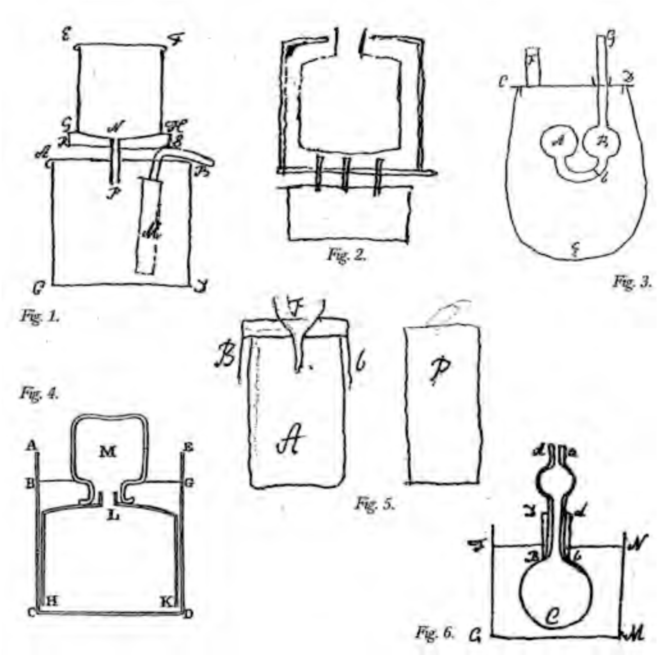

Figure 15.5: Laboratory Apparatus. Figure 1. Apparatus for experiments on the heats of mixtures. Through the cylindrical funnel on top, hot water is added to cold water in the pan below; M is a stirrer. Untitled paper on experiments on specific and latent heats, Cavendish Mss. Figure 2. Apparatus for determining the time of evaporation of boiling water. The water is contained in a tin bottle surrounded by an insulated tin frame and placed over a spirit lamp. Cavendish Mss III(a), 9:42. Figure 3. Apparatus to decide if the heat at which water becomes steam is the same as the heat of the steam. The ball A, which contains a little water and otherwise is filled with mercury to b, is exposed to steam and to the boiling water. Ibid. 1:1. Figure 4. Apparatus for collecting air discharged from pump water when it is boiled; the water is in ACDE, the air in M. Cavendish, *Sci. Pap.* 2:105. Figure 5. Apparatus to find the weight of fixed air in calcareous earth. Acid is poured through the funnel onto a sample of the earth contained in cylindrical glass A; after effervescence, the plug P is drawn in and out of the empty part of A to drive out any residual fixed air. Cavendish Mss II, 5:379. Figure 6. Apparatus to determine if the electrical charge of coated glass is the same whether hot or cold. The glass bowl C is filled with mercury as is the surrounding vessel, making it a Leiden jar, the charge of which is tested while a thermometer is dipped into the mercury at different heats. Cavendish, *Electrical Researches*, opposite p. 180. Reproductions by permission of the Chatsworth Settlement Trustees.

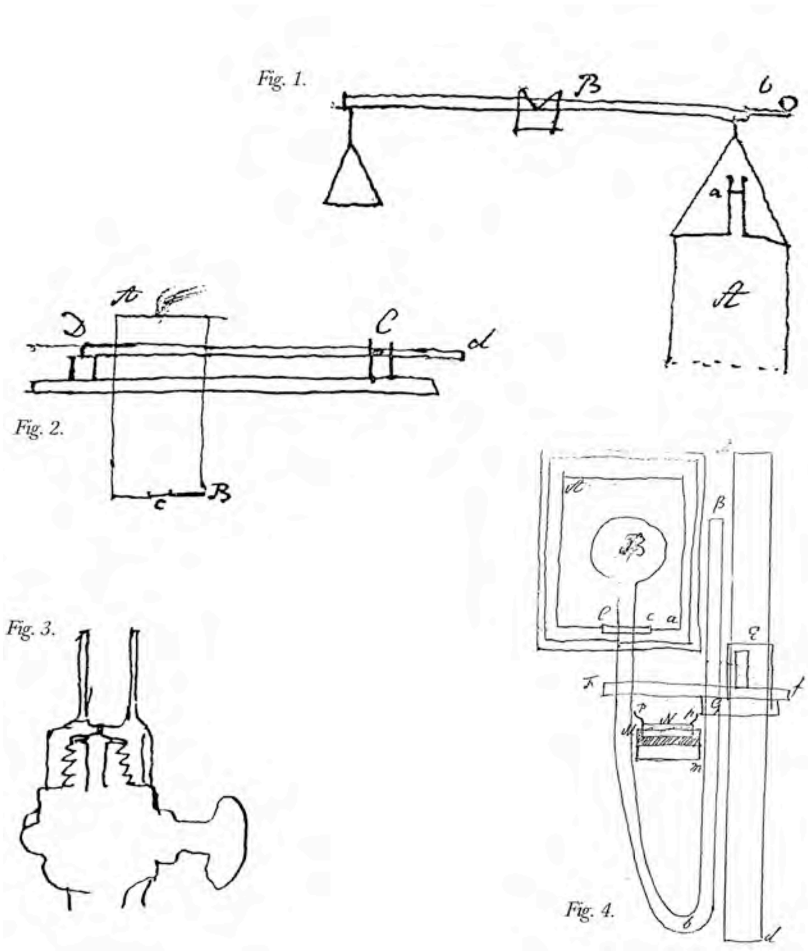

Figure 15.6: Laboratory Apparatus. Figure 1. Apparatus to test if the vis inertiae of phlogisticated air is the same in proportion to its weight as that of common air. The method requires finding the time in which a given quantity of air contained in A passes through a small hole at the top under a given pressure. Cavendish, *Sci. Pap.* 2:321. Figure 2. Apparatus for measuring the strength of the detonation of inflammable air with other airs. Air is admitted into the brass cylinder AB and electrically fired, lifting the pivoted board Dd to which it is fixed. Cavendish Mss II, 5:130. Figure 3. Apparatus for measuring the cold produced by the rarefaction of air. The brass cock is screwed over the cock of the condensing glass of an air pump. The ball of a thermometer is fitted into the cylinder of the cap, a small hole at the bottom of which allows the escaping condensed air to blow on the ball. Cavendish Mss III(a), 8:11. Figure 4. Apparatus for finding the "force of steam," or tension of aqueous vapor, at heats below 212°. A small amount of water stands above the mercury in Bb. The tin pot Aa contains heated water. Cavendish, Mss III(a), 1:40. Reproductions by permission of the Chatsworth Settlement.