Inventing Temperature: Measurement and Scientific Progress

Hasok Chang

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Inventing Temperature

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To my parents

Acknowledgments

As this is my first book, I need to thank not only those who helped me directly with it but also those who helped me become the scholar and person who could write such a book.

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Contents

Note on Translation

Where there are existing English translations of non-English texts, I have relied on them in quotations except as indicated. In other cases, translations are my own.

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c. 1690	Eschinardi, Renaldini, etc.: first use of the boiling and melting points as fixed points of thermometry
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Inventing Temperature

Introduction

This book aspires to be a showcase of what I call "complementary science," which contributes to scientific knowledge through historical and philosophical investigations. Complementary science asks scientific questions that are excluded from current specialist science. It begins by re-examining the obvious, by asking why we accept the basic truths of science that have become educated common sense. Because many things are protected from questioning and criticism in specialist science, its demonstrated effectiveness is also unavoidably accompanied by a degree of dogmatism and a narrowness of focus that can actually result in a loss of knowledge. History and philosophy of science in its "complementary" mode can ameliorate this situation, as I hope the following chapters will illustrate in concrete detail.

Today even the most severe critics of science actually take a lot of scientific knowledge for granted. Many results of science that we readily believe are in fact quite extraordinary claims. Take a moment to reflect on how unbelievable the following propositions would have appeared to a keen and intelligent observer of nature from 500 years ago. The earth is very old, well over 4 billion years of age; it exists in a near-vacuum and revolves around the sun, which is about 150 million kilometers away; in the sun a great deal of energy is produced by nuclear fusion, the same kind of process as the explosion of a hydrogen bomb; all material objects are made up of invisible molecules and atoms, which are in turn made up of elementary particles, all far too small ever to be seen or felt directly; in each cell of a living creature there is a hypercomplex molecule called DNA, which largely determines the shape and functioning of the organism; and so on. Most members of today's educated public subscribing to the "Western" civilization would assent to most of these propositions without hesitation, teach them confidently to their children, and become indignant when some ignorant people question these truths. However, if they were asked to say why they believe these items of scientific common sense, most would be unable to produce any convincing arguments. It may even be that the more basic and firm the belief is, the more stumped we tend to feel in trying to justify it. Such a correlation would indicate that unquestioning belief has served as a substitute for genuine understanding.

Nowhere is this situation more striking than in our scientific knowledge of heat, which is why it is an appropriate subject matter of this study. Instead of revisiting debates about the metaphysical nature of heat, which are very well known to historians of science, I will investigate some basic difficulties in an area that is usually considered much less problematic, and at the same time fundamental to all empirical studies of heat. That area of study is *thermometry*, the measurement of temperature. How do we know that our thermometers tell us the temperature correctly, especially when they disagree with each other? How can we test whether the fluid in our thermometer expands regularly with increasing temperature, without a circular reliance on the temperature readings provided by the thermometer itself? How did people without thermometers learn that water boiled or ice melted always at the same temperature, so that those phenomena could be used as "fixed points" for calibrating thermometers? In the extremes of hot and cold where all known thermometers broke down materially, how were new standards of temperature established and verified? And were there any reliable theories to support the thermometric practices, and if so, how was it possible to test those theories empirically, in the absence of thermometry that was already well established?

These questions form the topics of the first four chapters of this book, where they will be addressed in full detail, both historically and philosophically. I concentrate on developments in the eighteenth and nineteenth centuries, when scientists established the forms of thermometry familiar today in everyday life, basic experimental science, and standard technological applications. Therefore I will be discussing quite simple instruments throughout, but simple epistemic questions about these simple instruments quickly lead us to some extremely complex issues. I will show how a whole host of eminent past scientists grappled with these issues and critically examine the solutions they produced.

I aim to show that many simple items of knowledge that we take for granted are in fact spectacular achievements, obtained only after a great deal of innovative thinking, painstaking experiments, bold conjectures, and serious controversies, which may in fact never have been resolved quite satisfactorily. I will point out deep philosophical questions and serious technical challenges lurking behind very elementary results. I will bring back to life the loving labors of the great minds who created and debated these results. I will attempt to communicate my humble appreciation for these achievements, while sweeping away the blind faith in them that is merely a result of schoolroom and media indoctrination.

It is neither desirable nor any longer effective to try bullying people into accepting the authority of science. Instead, all members of the educated public can be invited to participate in science, in order to experience the true nature and value of scientific inquiry. This does not mean listening to professional scientists tell condescending stories about how they have discovered wonderful things, which you should believe for reasons that are too difficult for you to understand in real depth and detail. Doing science ought to mean asking your own questions, making your own investigations, and drawing your own conclusions for your own reasons. Of course it will not be feasible to advance the "cutting edge" or "frontier" of modern science without first acquiring years of specialist training. However, the cutting edge is not all there is to science, nor is it necessarily the most valuable part of science. Questions that have been answered are still worth asking again, so you can understand for yourself how to arrive at the standard answers, and possibly discover new answers or recover forgotten answers that are valuable.

In a way, I am calling for a revival of an old style of science, the kind of "natural philosophy" that was practiced by the European "gentlemen" of the eighteenth and nineteenth centuries with such seriousness and delight. But the situation in our time is indeed different. On the encouraging side, today a much larger number of women and men can afford to engage in activities that are not strictly necessary for their immediate survival. On the other hand, science has become so much more advanced, professionalized, and specialized in the last two centuries that it is no longer very plausible for the amateurs to interact with the professionals on an equal footing and contribute in an immediate sense to the advancement of specialist knowledge.

In this modern circumstance, science for the non-specialist and by the non-specialist should be historical and philosophical. It is best practiced as "complementary science" (or the complementary mode of history and philosophy of science), as I explain in detail in chapter 6. The studies contained in the first four chapters are presented as illustrations. They are offered as exemplars that may be followed in pursuing other studies in complementary science. I hope that they will convince you that complementary science can improve our knowledge of nature. Most of the scientific material presented there is historical, so I am not claiming to have produced much that is strictly new. However, I believe that the rehabilitation of discarded or forgotten knowledge does constitute a form of knowledge creation. Knowing the historical circumstances will also set us free to agree or disagree with the best judgments reached by the past masters, which form the basis of our modern consensus.

Each of the first four chapters takes an item of scientific knowledge regarding temperature that is taken for granted now. Closer study, however, reveals a deep puzzle that makes it appear that it would actually be quite impossible to obtain and secure the item of knowledge that seemed so straightforward at first glance. A historical look reveals an actual scientific controversy that took place, whose vicissitudes are followed in some detail. The conclusion of each episode takes the form of a judgment regarding the cogency of the answers proposed and debated by the past scientists, a judgment reached by my own independent reflections—sometimes in agreement with the verdict of modern science, sometimes not quite.

Each of those chapters consists of two parts. The narrative part states the philosophical puzzle and gives a problem-centered narrative about the historical attempts to solve that puzzle. The analysis part contains various in-depth analyses of certain scientific, historical, and philosophical aspects of the story that would have distracted the flow of the main narrative given in the first part. The analysis part of each chapter will tend to contain more philosophical analyses and arguments than the narrative, but I must stress that the division is not meant to be a separation of history and philosophy. It is not the case that philosophical ideas and arguments cannot be embodied in a narrative, and it is also not the case that history should always be presented in a narrative form.

The last parts of the book are more abstract and methodological. Chapter 5 presents in a more systematic and explicit manner a set of abstract epistemological ideas that were embedded in the concrete studies in the first four chapters. In that discussion I identify measurement as a locus where the problems of foundationalism are revealed with stark clarity. The alternative I propose is a brand of coherentism buttressed by the method of "epistemic iteration." In epistemic iteration we start by adopting an existing system of knowledge, with some respect for it but without any firm assurance that it is correct; on the basis of that initially affirmed system we launch inquiries that result in the refinement and even correction of the original system. It is this self-correcting progress that justifies (retrospectively) successful courses of development in science, not any assurance by reference to some indubitable foundation. Finally, in chapter 6, I close with a manifesto that articulates in explicit methodological terms what it is that I am trying to achieve with the kind of studies that are included in this book. The notion of complementary science, which I have sketched only very briefly for now, will be developed more fully and systematically there.

As this book incorporates diverse elements, it could be read selectively. The main themes can be gathered by reading the narrative parts of the first four chapters; in that case, various sections in the analysis parts of those chapters can be sampled according to your particular interests. If you have little patience for historical details, it may work to read just the analysis parts of chapters 1 to 4 (skipping the obviously historical sections), then chapter 5. If you are simply too busy and also prefer to take philosophy in the more abstract vein, then chapter 5 could be read by itself; however, the arguments there will be much less vivid and convincing unless you have seen at least some of the details in earlier chapters. Chapter 6 is intended mainly for professional scholars and advanced students in the history and philosophy of science. However, for anyone particularly excited, puzzled, or disturbed by the work contained in the first five chapters, it will be helpful to read chapter 6 to get my own explanation of what I am trying to do. In general, the chapters could be read independently of each other and in any order. However, they are arranged in roughly chronological order and both the historical and the philosophical discussions contained in them do accumulate in a real sense, so if you have the time and intention to read all of the chapters, you would do well to read them in the order presented.

As indicated by its inclusion in the Oxford Studies in the Philosophy of Science, this book is intended to be a work of philosophy. However, the studies presented here are works of philosophy, science, and history simultaneously. I am aware that they may cross some boundaries and offend the sensibilities of particular academic disciplines. And if I go into explanations of various elementary points well known to specialists, that is not a sign of condescension or ignorance, but only an allowance for the variety of intended readership. I fear that professional philosophy today is at risk of becoming an ailing academic discipline shunned by large numbers of students and seemingly out of touch with other human concerns. It should not be that way, and this book humbly offers one model of how philosophy might engage more productively with endeavors that are perceived to be more practically significant, such as empirical scientific research. I hope that this book will serve as a reminder that interesting and useful philosophical insights can emerge from a critical study of concrete scientific practices.

The intended audience closest to my own professional heart is that small band of scholars and students who are still trying to practice and promote history-and-philosophy of science as an integrated discipline. More broadly, discussions of epistemology and scientific methodology included in this book will interest philosophers of science, and perhaps philosophers in general. Discussions of physics and chemistry in the eighteenth and nineteenth centuries will be of interest to historians of science. Much of the historical material in the first four chapters is not to be found in the secondary literature and is intended as an original contribution to the history of science. I also hope that the stories of how we came to believe what we believe, or how we discovered what we know, will interest many practicing scientists, science students, and non-professional lovers of science. But, in the end, professional labels are not so relevant to my main aspirations. If you can glimpse through my words any of the fascination that has forced me to write them, then this book is for you.

1

Keeping the Fixed Points Fixed

Narrative: What to Do When Water Refuses to Boil at the Boiling Point

> The *excess* of the heat of water *above the boiling point* is influenced by a great variety of circumstances.
>
> Henry Cavendish, "Theory of Boiling," c. 1780

The scientific study of heat started with the invention of the thermometer. That is a well-worn cliché, but it contains enough truth to serve as the starting point of our inquiry. And the construction of the thermometer had to start with the establishment of "fixed points." Today we tend to be oblivious to the great challenges that the early scientists faced in establishing the familiar fixed points of thermometry, such as the boiling and freezing points of water. This chapter is an attempt to become reacquainted with those old challenges, which are no less real for being forgotten. The narrative of the chapter gives a historical account of the surprising difficulties encountered and overcome in establishing one particular fixed point, the boiling point of water. The analysis in the second half of the chapter touches on broader philosophical and historical issues and provides in-depth discussions that would have interrupted the flow of the narrative.

Blood, Butter, and Deep Cellars: The Necessity and Scarcity of Fixed Points

Galileo and his contemporaries were already using thermometers around 1600. By the late seventeenth century, thermometers were very fashionable but still notoriously unstandardized. Witness the complaint made about the existing thermometers in 1693 by Edmond Halley (1656–1742), astronomer of the comet fame and secretary of the Royal Society of London at the time:

> I cannot learn that any of them...were ever made or adjusted, so as it might be concluded, what the Degrees or Divisions...did mean; neither were they ever otherwise graduated, but by Standards kept by each particular Workman, without any agreement or reference to one another. (Halley 1693, 655)

Most fundamentally, there were no standard "fixed points," namely phenomena that could be used as thermometric benchmarks because they were known to take place always at the same temperature. Without credible fixed points it was impossible to create any meaningful temperature scale, and without shared fixed points used by all makers of thermometers there was little hope of making a standardized scale.

Halley himself recommended using the boiling point of alcohol ("spirit of wine") as a fixed point, having seen how the alcohol in his thermometer always came up to the same level when it started to boil. But he was also quick to add a cautionary note: "Only it must be observed, that the Spirit of Wine used to this purpose be highly rectified or dephlegmed, for otherwise the differing Goodness of the Spirit will occasion it to boil sooner or later, and thereby pervert the designed Exactness" (1693, 654). As for the lower fixed point, he repudiated Robert Hooke's and Robert Boyle's practice of using the freezing points of water and aniseed oil, either of which he thought was "not so justly determinable, but with a considerable latitude." In general Halley thought that "the just beginning of the Scales of Heat and Cold should not be from such a Point as freezes any thing," but instead recommended using the temperature of deep places underground, such as "the Grottoes under the Observatory at Paris," which a "certain Experiment of the curious Mr. Mariotte" had shown to be constant in all seasons (656).[1]

Halley's contribution clearly revealed a basic problem that was to plague thermometry for a long time to come: in order to ensure the stability and usefulness of thermometers, we must be quite certain that the presumed fixed points are actually fixed sharply, instead of having "a considerable latitude." There are two parts to this problem, one epistemic and the other material. The epistemic problem is to know how to judge whether a proposed fixed point is actually fixed: how can that judgment be made in the absence of an already-trusted thermometer? This problem will not feature prominently on the surface of the narrative about the history of fixed points to be given now; however, in the analysis part of this chapter, it will be discussed as a matter of priority (see especially "The Validation of Standards" section). Assuming that we know how to judge fixedness, we can face the material problem of finding or creating some actual points that are fixed.

Throughout the seventeenth century and the early parts of the eighteenth century, there was a profusion of proposed fixed points, with no clear consensus as to which ones were the best. Table 1.1 gives a summary of some of the fixed

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[1] He did not name Hooke and Boyle explicitly. See Birch [1756–57] 1968, 1:364–365, for Hooke's suggestion to the Royal Society in 1663 to use the freezing point of water; see Barnett 1956, 290, for Boyle's use of aniseed oil.

TABLE 1.1. Summary of fixed points used by various scientists

Person	Year	Fixed points ("and" indicates a two-point system)	Source of information
Sanctorius	c. 1600	candle flame *and* snow	Bolton 1900, 22
Accademia del Cimento	c. 1640?	most severe winter cold *and* greatest summer heat	Boyer 1942, 176
Otto Von Guericke	c. 1660?	first night frost	Barnett 1956, 294
Robert Hooke	1663	freezing distilled water	Bolton 1900, 44–45; Birch [1756] 1968, 1:364–365
Robert Boyle	1665?	congealing oil of aniseed *or* freezing distilled water	Bolton 1900, 43
Christiaan Huygens	1665	boiling water *or* freezing water	Bolton 1900, 46; Barnett 1956, 293
Honoré Fabri	1669	snow *and* highest summer heat	Barnett 1956, 295
Francesco Eschinardi	1680	melting ice *and* boiling water	Middleton 1966, 55
Joachim Dalencé	1688	freezing water *and* melting butter *or* ice *and* deep cellars	Bolton 1900, 51
Edmond Halley	1693	deep caves *and* boiling spirit	Halley 1693, 655–656
Carlo Renaldini	1694	melting ice *and* boiling water	Middleton 1966, 55
Isaac Newton	1701	melting snow *and* blood heat	Newton [1701] 1935, 125, 127
Guillaume Amontons	1702	boiling water	Bolton 1900, 61
Ole Rømer	1702	ice/salt mixture *and* boiling water	Boyer 1942, 176
Philippe de la Hire	1708	freezing water *and* Paris Observatory cellars	Middleton 1966, 56
Daniel Gabriel Fahrenheit	c. 1720	ice/water/salt mixture *and* ice/water mixture *and* healthy body temperature	Bolton 1900, 70
John Fowler	c. 1727	freezing water *and* water hottest to be endured by a hand held still	Bolton 1900, 79–80
R. A. F. de Réaumur	c. 1730	freezing water	Bolton 1900, 82
Joseph-Nicolas De l'Isle	1733	boiling water	Middleton 1966, 87–89
Anders Celsius	by 1741	melting ice *and* boiling water	Beckman 1998
J. B. Micheli du Crest	1741	Paris Observatory cellars *and* boiling water	Du Crest 1741, 8
Encyclopaedia Britannica	1771	freezing water *and* congealing wax	*Encyclopaedia Britannica*, 1st ed., 3:487

points used by the most respectable scientists up to the late eighteenth century. One of the most amusing to our modern eyes is a temperature scale proposed by Joachim Dalencé (1640–1707?), which used the melting point of butter as its upper fixed point. But even that was an improvement over previous proposals like the "greatest summer heat" used in the thermometers of the Accademia del Cimento, a group of experimental philosophers in Florence led by Grand Duke Ferdinand II and his brother Leopold Medici. Even the great Isaac Newton (1642–1727) seems to have made an unwise choice in using what was often called "blood heat," namely human body temperature, as a fixed point in his 1701 scale of temperatures.[2]

By the middle of the eighteenth century, a consensus was emerging about using the boiling and freezing of water as the preferred fixed points of thermometry, thanks to the work of the Swedish astronomer Anders Celsius (1701–1744), among others.[3] However, the consensus was neither complete nor unproblematic. In 1772 Jean-André De Luc (1727–1817), whose work I shall be examining in great detail shortly, published these words of caution:

> Today people believe that they are in secure possession of these [fixed] points, and pay little attention to the uncertainties that even the most famous men had regarding this matter, nor to the kind of anarchy that resulted from such uncertainties, from which we still have not emerged at all. (De Luc 1772, 1:331, §427[4])

To appreciate the "anarchy" that De Luc was talking about, it may be sufficient to witness the following recommendation for the upper fixed point given as late as 1771, in the first edition of the *Encyclopaedia Britannica*: "water just hot enough to let wax, that swims upon it, begin to coagulate" (3:487).[5] Or there is the more exotic case of Charles Piazzi Smith (1819–1900), astronomer royal for Scotland, who proposed as the upper fixed point the mean temperature of the King's Chamber at the center of the Great Pyramid of Giza.[6]

The Vexatious Variations of the Boiling Point

In 1776 the Royal Society of London appointed an illustrious seven-member committee to make definite recommendations about the fixed points of thermometers.[7] The chair of this committee was Henry Cavendish (1731–1810), the reclusive

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[2] See Newton [1701] 1935, 125, 127. Further discussion can be found in Bolton 1900, 58, and Middleton 1966, 57. Blood heat may actually not have been such a poor choice in relative terms, as I will discuss further in "The Validation of Standards" in the analysis part of this chapter. Middleton, rashly in my view, berates Newton's work in thermometry as "scarcely worthy of him." According to modern estimates, the temperatures of healthy human bodies vary by about 1 degree centigrade.

[3] On Celsius's contributions, see Beckman 1998. According to the consensus emerging in the late eighteenth century, both of these points were used together to define a scale. However, it should be noted that it is equally cogent to use only one fixed point, as emphasized in Boyer 1942. In the one-point method, temperature is measured by noting the volume of the thermometric fluid in relation to its volume at the one fixed point.

[4] In citing from this work, I will give both the paragraph number and the page number from the two-volume edition (quarto) that I am using, since there was also a four-volume edition (octavo) with different pagination.

[5] Newton ([1701] 1935, 125) had assigned the temperature of 20 and 2/11 degrees on his scale to this point. It was not till the 3d edition of 1797 that *Britannica* caught on to the dominant trend and noted: "The fixed points which are now universally chosen… are the boiling and freezing water points." See "Thermometer," *Encyclopaedia Britannica*, 3d ed., 18:492–500, on pp. 494–495.

[6] The information about Piazzi Smith is from the display in the Royal Scottish Museum, Edinburgh.

[7] This committee was appointed at the meeting of 12 December 1776 and consisted of Aubert, Cavendish, Heberden, Horsley, De Luc, Maskelyne, and Smeaton. See the Journal Book of the Royal Society, vol. 28 (1774–1777), 533–534, in the archives of the Royal Society of London.

aristocrat and devoted scientist who was once described as "the wisest of the rich and the richest of the wise."[8] The Royal Society committee did take it for granted that the two water points should be used, but addressed the widespread doubts that existed about their true fixity, particularly regarding the boiling point. The committee's published report started by noting that the existing thermometers, even those made by the "best artists," differed among themselves in their specifications of the boiling point. The differences easily amounted to 2–3 degrees Fahrenheit. Two causes of variation were clearly identified and successfully dealt with.[9] First, the boiling temperature was by then widely known to vary with the atmospheric pressure,[10] and the committee specified a standard pressure of 29.8 English inches (roughly 757 mm) of mercury, under which the boiling point should be taken. Drawing on De Luc's previous work, the committee also gave a formula for adjusting the boiling point according to pressure, in case it was not convenient to wait for the atmosphere to assume the standard pressure. The second major cause of variation was that the mercury in the stem of the thermometer was not necessarily at the same temperature as the mercury in the thermometer bulb. This was also dealt with in a straightforward manner, by means of a setup in which the entire mercury column was submerged in boiling water (or in steam coming off the boiling water). Thus, the Royal Society committee identified two main problems and solved both of them satisfactorily.

However, the committee's report also mentioned other, much less tractable questions. One such question is represented emblematically in a thermometric scale from the 1750s that is preserved in the Science Museum in London. That scale (shown in fig. 1.1), by George Adams the Elder (?–1773), has two boiling points: at 204° Fahrenheit "water begins to boyle," and at 212°F "water boyles vehemently." In other words, Adams recognized a temperature interval as wide as 8°F in which various stages of boiling took place. This was not an aberrant quirk of an incompetent craftsman. Adams was one of Britain's premier instrument-makers, the official "Mathematical Instrument Maker" to George III, starting from 1756 while the latter was the Prince of Wales.[11] Cavendish himself had addressed the question of whether there was a temperature difference between "fast" and "slow" boiling ([1766] 1921, 351). The notion that there are different temperatures associated with different "degree of boiling" can be traced back to Newton ([1701] 1935, 125), who recorded that water began to boil at 33° of his scale and boiled vehemently at 34° to 34.5°, indicating a range of about 5–8°F. Similar observations were made by

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[8] This description was by Jean-Baptiste Biot, quoted in Jungnickel and McCormmach 1999, 1. Cavendish was a grandson of William Cavendish, the Second Duke of Devonshire, and Rachel Russell; his mother was Anne de Grey, daughter of Henry de Grey, Duke of Kent. See Jungnickel and McCormmach 1999, 736–737, for the Cavendish and Grey family trees.

[9] For further details, see Cavendish et al. 1777, esp. 816–818, 853–855.

[10] Robert Boyle had already noted this in the seventeenth century, and Daniel Gabriel Fahrenheit knew the quantitative relations well enough to make a barometer that inferred the atmospheric pressure from the boiling point of water. See Barnett 1956, 298.

[11] The description of Adams's scale is from Chaldecott 1955, 7 (no. 20). For information about his status and work, see Morton and Wess 1993, 470, and passim.