History of ‘Temperature’: Maturation of a Measurement Concept

John P. McCaskey

Accounts of how the concept of temperature has evolved typically cast the story as ancillary to the history of the thermometer or the history of the concept of heat. But then, because the history of temperature is not treated as a subject in its own right, modern associations inadvertently get read back into the historical record. This essay attempts to lay down an authoritative record not of what people in the past thought about what we call ‘temperature’ but of what they thought about what they called ‘temperature’ (or one of its cognates), from medieval times to today. It is found that invention of the thermometer had little impact on the concept of temperature. Much more significant were Fahrenheit’s invention of a reliable instrument and William Thomson’s effort to make a degree of temperature a unit of measure. Overlapping definitions of temperature then emerged in the late nineteenth century, and twentieth-century scientific developments forced physicists to reconsider temperature’s conceptual boundaries. It turns out that the concept of temperature has evolved through stages that correspond to four increasingly sophisticated types of measurement. Its maturity sheds light on the philosophy of conceptual change.

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1. Introduction

The concept of temperature has long attracted the attention of historians, philosophers, and sociologists of science.[1] Unfortunately, studies of its evolution have often been marred by the mistake of reading current conceptual associations back into the historical record. We see an old instrument that we would call a thermometer and presume its users thought about temperature the way we do. We might—to use a few simple examples—take for granted that thermometers measure heat, that to be cold is to have a low temperature and to be hot is to have a high temperature, that cold is the absence of heat, and so on, and we read such presumptions back into the history even when they would have been foreign to how people of the past conceptualized their world.

[1] For some examples, see Ernst Mach, *Die Prinzipien der Wärmelehre* (Leipzig: Barth, 1896) and the English translations of excerpts on temperature published in *The Open Court* in 1902 and 1903, translated by Thomas J. McCormack; Paul Feyerabend, ‘Explanation, Reduction and Empiricism’, in H. Feigl and G. Maxwell (ed.), *Scientific Explanation, Space, and Time* (Minneapolis: University of Minneapolis Press, 1962), pp. 28–97, and the subsequent literature on incommensurability, much of it influenced by Thomas Kuhn, *The Structure of Scientific Revolutions* (Chicago: University of Chicago Press, 1962 and 1970); Hilary Putnam, ‘The Nature of Mental States’, *Mind, Language, and Reality* (Cambridge University Press, 1975); Hasok Chang, *Inventing Temperature: Measurement and Scientific Progress* (Oxford University Press, 2004); and Travis Norsen, ‘Scientific Cumulativity and Conceptual Change: The Case of “Temperature,”’ preprint, submitted Oct. 14, 2010, http://philsci-archive.pitt.edu/id/eprint/8332, and the commentaries thereon in a forthcoming volume, *Concepts, Induction, and the Growth of Scientific Knowledge*, ed. Corinne Bloch-Mullins and Theodore Arabatzis, with relevant contributions by Hasok Chang, James G. Lennox, John P. McCaskey, John D. Norton, and Gregory Salmieri.

Also, the history of temperature is often treated as secondary to the history of thermal physics or the history of thermometers. But 'temperature' has not always meant degree of heat and the numbers on a thermometer (when there were any) were not always called temperatures. To understand the history of 'temperature', we must not read back into that history our own presumptions about the relationships between temperature, thermometers, and heat. When the understanding of heat changed, for example, the understanding of temperature might have changed a lot, a little, or barely at all. We need to check the historical record to see. Maybe it was the relationship between the concepts, and not the concept of temperature, that changed more.

'Temperature' is a concept of measurement, and such concepts are of different types.[2] The measurement values might be merely *nominal*, as when we assign bodies into one group we name 'hot' or another we name 'cold'. Within each group we could add an *ordinal* ranking, using names, such as 'warm', 'hot', and 'very hot', or using numerals, such 1 to 10. But even when numerals indicate an order, we might not be able to meaningfully use them as numbers in calculations. Measurements that are *cardinal* and not just ordinal can be used in arithmetical operations but maybe not all such operations. An *interval* scale, such as today's Fahrenheit temperature scale, allows sums and differences—but not ratios—to be meaningfully calculated. The *interval* between 10°F and 15°F is the same as the interval between 210°F and 215°F, but

[2] The taxonomy proposed in S. S. Stevens, 'On the Theory of Scales of Measurement', *Science*, vol. 103 (June 7, 1946), pp. 677–680, has been influential and widely adopted though not without challenges. For an up-to-date survey, see Eran Tal, 'Measurement in Science', *The Stanford Encyclopedia of Philosophy* (Fall 2017 Edition), ed. Edward N. Zalta, https://plato.stanford.edu/archives/fall2017/entries/measurement-science/. One challenge has been whether nominal and ordinal gradings deserve to be called measurements at all. Whether they should or not, they certainly played a role in the history of temperature.

water at 15°F is not 50% hotter than water at 10°F. The Kelvin temperature scale is a *ratio* scale and does allow such percentage comparisons. It also allows an absolute zero, where a measurement of 0 indicates an absence of the measured property. The history of ‘temperature’ is a case study in how increasingly advanced measurement categories can also be chronological stages. This maturation has philosophical implications for what it means to say a concept changes.

2. Prior studies, sources, and method

A history of the concept of temperature for as long as the word has been around has heretofore not been written. This is strange. For there have been many studies, commentaries, and histories that would have benefitted from a reliable history of temperature. Those studies had to proceed, to their detriment, with what was available—partial accounts, rough outlines, and many unchecked assumptions.[3]

[3] Prior studies on temperature, often with important information and crucial insights, but generally either limited in scope or focused less on the history of temperature than on the history of thermometers, of thermometry, of heat, or of thermodynamics include the following. Henry Carrington Bolton, *Evolution of the Thermometer, 1592-1743* (1900); Kirstine Meyer, *Die Entwickelung des Temperaturbegriffs im Laufe der Zeiten* (Braunschweig: S. Vieweg und Sohn, 1913); F. Sherwood Taylor, ‘The Origin of the Thermometer’, *Annals of Science*, vol. 5 (1942), pp. 129–156; C. B. Boyer, ‘History of the Measurement of Heat,’ *Scientific Monthly*, vol. 57 (1943), pp. 442–452, 546–554; Martin Barnett, ‘The Development of Thermometry and the Temperature Concept,’ *Osiris* 12 (1956), p. 269–341; Duane Roller, ‘The Early Development of the Concepts of Temperature and Heat: The Rise and Decline of the Caloric Theory,’ *Harvard Case Histories in Experimental Science*, vol. 1 (Harvard University Press, 1957); W. E. Knowles Middleton, *A History of the Thermometer and Its Use in Meteorology* (Baltimore: The Johns Hopkins Press, 1966); T. J. Quinn, ‘The Meaning of Temperature and the Development of Thermometry’, *Temperature*, 2nd ed. (Elsevier, 1990), pp. 1–23; Hasok Chang, *Inventing Temperature: Measurement and Scientific Progress* (Oxford University Press, 2004); Arianna Borelli, ‘The

For a while—from the time of Ernst Mach, to that of Paul Feyerabend and Thomas Kuhn, and then to that of many commentators thereon—temperature was treated as a case study in the incommensurability of concepts across paradigm shifts. Philosophers puzzled over how a community that thought of heat as what a thermometer measures could actually communicate with another that thought of heat as caloric, or as kinetic energy, or as the mathematical relationship of energy and entropy. Were different scientists really thinking and talking about the 'same thing'? Is 'scientific progress' actually progress, or just a series of incommensurable idea-bundles, adopted by each generation for its own personal, professional, and even political purposes?

But although the word was central, very few of these discussions were in fact about temperature. Researchers generally took for granted current presumptions about how heat and temperature are related, noted historical changes in theories of heat, and then projected backwards how ideas of temperature presumably changed, without really checking that that is what happened. Maybe those studies told us something about the evolving science of heat, but they needed much more grounding in the evolving relationship between 'heat' and 'temperature'

Weatherglass and its Observers in the Early Seventeenth Century', *Philosophies of Technology: Francis Bacon and His Contemporaries*, ed. Claus Zittel et al. (Brill, 2008), pp. 67–130; Matteo Valleriani, 'Pneumatics, the Thermoscope and the New Atomistic Conception of Heat', *Galileo Engineer* (Springer, 2010); Arianna Borrelli, 'Die Reproduktion des Temperaturbegriffs', *Epistemologie und Differenz: Zur Reproduktion in den Wissenschaften*, ed. Ute Frietsch and Bettina Bock von Wulfingen (Bielefeld: Transcript, 2010), pp. 59–82; David Sherry, 'Thermoscopes, Thermometers, and the Foundations of Measurement', *Studies in History and Philosophy of Science, Part A*, vol. 42 (Dec 2011), pp. 509–524; and William F. Wright, 'Early Evolution of the Thermometer and Application to Clinical Medicine,' *Journal of Thermal Biology*, vol. 56 (2016), pp. 18–30.

if they were to say anything reliable about the second. And they needed that if they were to successfully use temperature to study the philosophy of measurement, the incommensurability of linguistic constructs, or any debate between instrumentalism and realism.

A significant contribution was the landmark study in 2004 by Hasok Chang, *Inventing Temperature*. It provided crucial historical grounding to important philosophical issues. The book’s title notwithstanding, however, its subject is not really the history of the concept of temperature. Its ‘area of study’ is not temperature, but, the author says, ‘*thermometry*, the measurement of temperature.’[4] There is a chapter on how researchers sought two fixed points of reference (such as the freezing and boiling points of water), one on how they dealt with inconsistent readings between those two fixed points, one on extremes of hot and cold far beyond those fixed points, and one on William Thomson’s attempt to define a thermometric scale that would be independent of the idiosyncrasies of thermometric fluids. The chapters cover overlapping, not sequential, time periods. The work offers no single chronological narrative. Each chapter grounds an important discussion on a corresponding topic in the philosophy of thermometry.[5]

The project here is different. It is intended to be a conceptual etymology of ‘temperature’ for as long as the word or its direct cognates have been around. It traces how the concept changed as

[4] Chang, *Inventing Temperature*, p. 4.

[5] The focus on thermometry instead of temperature carried over into studies spawned by Chang’s book, including those at the conference ‘The Making of Measurement’, held at the University of Cambridge, July 23–24, 2015 and the published as a special issue of *Studies in History and Philosophy of Science, Part A*, vols. 65–66 (October–December, 2017).

it was passed from generation to generation and provides a straight narrative, across a longer time period than Chang covered and with a different focus.

This style of intellectual history makes a few important assumptions. First, it adopts a narrow understanding of *concept*. A *concept* here is the cognitive content that corresponds to a word[6]. As such, a concept is not a synonym for the broader *idea* or *thought*. To say someone in the past had a *concept* for something is to say the person had a word for it—not that the person had an interconnected set of ideas that *we* would concretize with a word. Also, conceptual etymology presumes that concepts are, in two senses, organic. First, they are individual products of individual organic beings. You have your concept of something; I have mine; others have theirs. The concepts are similar enough—in what they refer to, their relationships with other concepts, etc.—that we can say they are concepts *of* some one thing. But my concept is not yours and yours is not mine. Our concepts are the same but also different. You and I might disagree over whether eclairs and fritters should be classed as donuts, but we can still carry on a substantive conversation about donuts. Concepts are also organic in the sense that they change a little generation to generation. Concepts shared by our generation might not differ as much as ours differ from our parents’ or our children’s. So to trace the etymology of a concept is to trace how the meaning of a single word—or a small group of words that people at the time treated as synonyms or translations—has changed. Our task here is to find what people in the past thought when they used the English word *temperature*—or Latin *temperatura* if people at the time thought that was a direct translation—not to trace how people thought about what *we* have in mind when *we* say ‘temperature’. Our goal here is to avoid putting our words into their mouths.

[6] Or a multi-word lexeme, such as *fire engine*, but I will not repeat the exception.

We want to know how they thought, not how they would have thought had they organized their thoughts the way we do.

Conceptual etymology is not the only way to study the history of an idea, but it provides a baseline, a way to ground the understanding of how people in the past thought about their world. And it helps us avoid the easy mistake of reading our own conceptual associations back into the historical record. Let me offer a few examples of the risk. In his invaluable analysis of early thermometers, F. Sherwood Taylor said that in 1578 Johann Hasler 'sets out two scales of temperature' and that 'in his day the idea of a temperature-scale was no novelty.' In the 1990 edition of the important reference work, *Temperature*, by T. J. Quinn, the author repeats the claim. Hasler 'set up a temperature scale in which there were Galen's four degrees of heat and four degrees of cold with a zero in the middle.'[7] Understood with sufficient context, the claims are accurate and useful. But they can also be misleading. Hasler did not describe what *he* set out as a 'temperature scale'—or anything like that in Latin, French, German, or Italian. We should not read back into his innovation what *we* mean when *we* say 'temperature' or 'scale'. As another example, in *Inventing Temperature*, Chang says that something happened 'before people could say with any confidence what it was that thermometers measured'.[8] But in fact there was never such a time. People back then had no hesitation saying that thermometers measured what *they* called temperature, even if their understanding of temperature would not meet *our* standard for what temperature 'really is'. Early in his book, Chang says he will distinguish thermometers

[7] Quinn, *Temperature*, p. 16.

[8] Another statement that will mislead is Chang's use of the term 'thermoscope'. In chapter 1, he says he will use it for instruments in which the numbers

from thermoscopes. But this is a distinction never held by the writers Chang surveys, and unsurprisingly then he has a hard time maintaining the distinction in his separate narratives.[9]

Another example of how easy it is to read current conceptions into the past comes from translations of an important historical document of 1612.[10] Taylor translated the key passage, ‘I must inform you of a marvellous method, by which, with the aid of a glass instrument, I am wont to measure the cold or hot temperature (*temperaturam frigidam et calidam*) of the air . . . . I can measure with the compass the degrees (*gradus*) and ultimate stations (*ultimas mansiones*) of heat and cold (*caliditatis et frigiditatis*)’.[11] But this translation makes the report sound too close to our way of thinking. In the Latin, the phrase is ‘cold *and* hot temperature’, not ‘cold *or* hot

[9] How the words were in fact used will be described below. Middleton said he would introduce the distinction that ‘a thermometer is simply a thermoscope provided with a scale.’ Chang says he ‘will follow Middleton’ (p. 41) and, in the glossary, says that a thermoscope ‘indicates the relative changes or comparisons of temperatures, without giving numbers’ (p. 258). But Chang also says he gives the term a ‘non-standard’ meaning (p. 251) and that a thermoscope does indeed have a scale, but the scale is ordinal not cardinal. Neither distinction is well maintained in the book. If the second were, ‘thermoscope,’ not ‘thermometer’, would have been the term for the whole period before Thomson’s introduction of an absolute scale. The confusing treatment in Chang is also noticed by David Sherry, ‘Thermoscopes, thermometers, and the foundations of measurement,’ *Studies in History and Philosophy of Science*, vol. 42 (2011), p. 511. Middleton’s distinction has unfortunately been too widely and too uncritically adopted. See M. Valleriani, ‘Pneumatics, the Thermoscope, and the New Atomistic Conception of Heat’, *Galileo Engineer* (Dordrecht: Springer, 2010) for a recent example.

[10] Sanctorius, *Commentaria in artem medicinalem Galeni* (Venice: Somaschus), pt. III, cap. 85, particula X, p. 612 in the 1632 edition.

[11] W. E. Knowles Middleton’s translation (p. 9) was ‘I wish to tell you about a marvellous way in which I am accustomed to measure, with a certain glass instrument, the cold and hot temperature of the air . . . . we can measure with the compass the degrees and ultimate limits of heat and cold’. Middleton gets ‘cold *and* hot temperature’ correct but then seems stumped by ‘*ultimas mansions*’.

temperature'; as we will see, the phrase means 'mixture of hot and cold'; using 'or' is misleading. In the next sentence, using 'degrees' for *gradus* suggests mathematical granularity out of place here. (More on this below). 'Degrees' is also too static. *Gradus* meant a discrete step along some path, as on stairs, a ladder, or a journey, and indeed users relied on early thermometers to show changes more than absolute proportions in the mixtures of hot and cold that they were measuring. Fevers, for example, were measured by how fast the water level dropped, not its absolute level. This sheds light on that otherwise odd *ultimas mansiones*, a phrase one would use for the major stops on a journey. The description was meant to convey an image of water constantly rising and falling and only temporarily resting. 'The stages and then stopping points' would be a good translation. While we think of thermometers as giving stable readings as precise as our visual acuity permits, thermometers then were conceived as in perpetual motion or always alive. We must be careful not to read our way of thinking into translations that might then be mistakenly used as evidence for how modern past thinking was.

There are other good and important ways to study the history of temperature, but to ensure we do not introduce anachronisms when we use newer conceptual frameworks, we should have an accurate conceptual etymology at hand. The study here attempts to provide one. It relies heavily on primary sources, familiar and not, from the sixteenth century to the twentieth, in Latin, Italian, French, German, and English. (Translations are my own unless otherwise indicated.)[12]

[12] Helpful recent roadmaps through the science of the primary sources include works by Peter Weinberger, J. J. Mares, and Wayne Saslow (in addition to Chang and Brush).

Though this essay mainly just aspires to provide an accurate historical account for the benefit of those who want to draw their own conclusions or assess the conclusions others have proffered, I will nonetheless append a short philosophical reflection of my own on a remarkable finding, namely the way that the concept of temperature not only evolved but matured, and—if its development is typical—on the implications of that maturation for the nature and formulation of scientific laws.

3. At first, 'temperature' meant mixture, including of hot and cold

Our concept of temperature goes back largely to Galenic medicine, which was based on Aristotelian physics. Several aspects about the ancient background are important for understanding the history of the concept of temperature.

For Aristotle and others in antiquity, hot and cold were qualities, not quantities. In his *Categories* and *Metaphysics*, hot and cold appear in the category that has sweet and bitter, hard and soft, good and bad, and viscous and brittle, not in the category that has time, lines, and shapes.[13] Aristotle allows that some qualities can be quantified in a secondary sense, but what he offers is not particularly helpful. He says an action can be quantified by the time it takes, which seems plain enough. But he says white can be quantified by the size of what is white. He also says, without explanation, that the quality of aesthetically refined (*mousikon*) can be quantified. For the qualities that he says can be quantified, the quantification is of only one part of any contrary pair. It is white or hot that would be quantified, not the location on a continuum between white and black or hot and cold. Quantities, he says, have no contraries. How in general to quantify qualities attracted the attention of al-Kindi (c. 801–873), Avicenna (980–1037),

[13] *Categories* chapter 8 and *Metaphysics* book 5(Δ), chapters 13 and 14, 1020a7–b25.

Arnaldus de Villanova (c. 1240–1311), Nicole Oresme (c. 1320s–1382), and the Oxford Calculators of Merton College in the mid-fourteenth century, but progress was limited.[14] Paths forward were just not obvious. Once a second wall in a room gets painted white, for example, should we, following Aristotle’s apparent suggestion, say the room is twice as white as when only one wall was painted? And is there really such a thing as more and less hard, or just, or healthy? Or are the qualities unvarying and things just possess that one quality in differing amounts?[15] The same for hot and cold, wet and dry. Is a kiln hotter than a candle because heat has intensities, because heat does not have intensities but there is more of it in the kiln, or simply because the kiln is larger? For heat, questions like these remained puzzling even long after invention of the thermometer (as we will see).

That hot and cold are primary and independent qualities, even independent forces, was conventional thinking into the Renaissance and beyond.[16] Bernardino Telesio (1509–1588) was particularly influential. He replaced Aristotle’s matter and form with passive matter and two

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[14] For entry to the literature, see Joel Kaye, *A History of Balance: 1250–1375* (Cambridge University Press, 2014), especially bibliographical note 107 on p. 213; Edith Sylla, ‘Medieval Quantification of Qualities: The “Merton School”’, *Archive for History of Exact Sciences*, vol 8 (1971), pp. 7–39; and Arianna Borrelli, ‘Die Reproduktion des Temperaturbegriffs’, p. 32.

[15] “For if justice could be more or less justice, certain problems might thereon arise, as is also the case with all qualities which we may call dispositions. And some go so far as to say that these cannot admit of degrees. Health and justice themselves, they contend, are not subject to such variations, but people in varying degrees are possessed of health, justice and so on.” Aristotle, *Categories* ch. 8, 10b30–35.

[16] For the topic in Scholasticism, see Robert Pasnau, ‘Scholastic Qualities, Primary and Secondary’, *Primary and Secondary Qualities: The Historical and Ongoing Debate* (Oxford University Press, 2020).

active and competing forces—hot and cold.[17] Following him, Francis Bacon (1561–1626) said, ‘Heat and Cold are Natures two hands, whereby she chiefly worketh’.[18] Robert Boyle (1627–1691) agreed. Galileo (1564–1642) did not and thought cold was merely the absence of heat, but this was a minority view all through the seventeenth century. In the eighteenth, claims either way were frequently hedged. In 1778, the *Encyclopaedia Britannica* accepted that there was simply no agreement on whether hot and cold were independent properties. Cold’s independence seemed confirmed when, around 1790, Marc-Auguste Pictet found that he could make cold travel in a beam at high speed, just as he could make hot do. Only in the early 1800s, some two hundred years after invention of the thermometer, did a consensus form that cold is merely the absence of heat.[19]

Another important aspect of early thinking about hot and cold involves scales, grades, and degrees—or lack thereof. There was no conceptual difficulty in referring to comparisons such as hotter, colder, wetter, drier, stronger, weaker, and so on. To refer to how hot something is, no concept of temperature was needed. The noun forms of the adjective—*heat* or *hotness* in

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[17] Bernardino Telesio, *De rerum natura iuxta propria principia libri IX* (Rome: Antonium Bladum, 1586). The final edition of the work included the chapter ‘Calor frigusque uni, eidemque subjecto non erat indendum’ (The heat and cold in something are not one and the same), bk. 3, ch. 31. A nice summary of Telesio’s thinking on this, along with his influencers and influences, and associated literature appear in Michaela Boenke, ‘Bernardino Telesio’, *The Stanford Encyclopedia of Philosophy*, ed. Edward N. Zalta (Winter 2018 Edition).

[18] Bacon, *Sylva Sylvarum*, century 1, before no. 69.

[19] For the late seventeenth to the early nineteenth centuries, including the development of the concept of temperature more broadly, see Hasok Chang, ‘Rumford and the Reflection of Radiant Cold: Historical Reflections and Metaphysical Reflexes’, *Physics in Perspective*, vol. 4 (2002), pp. 127–169, and *Inventing Temperature*, pp. 164–168. Chang’s discussion of Pictet is on pp. 164–167, on Rumford p. 167.

English, *calor* or *caliditas* in Latin, *thermotēs* in Greek, etc.—or just comparative forms of the adjective were perfectly functional.

We must be careful, then, with the word ‘degree’. The classical Latin word was *gradus* and it meant a step one takes while walking. (The verb was *gradi*, to walk.) *Gradus* was a concept for something discrete, not continuous. The word got used for steps on a staircase or a ladder and then also for rank or order. In late Latin and then French, the stepping up and down, and thus the ranking of higher and lower, could be emphasized by adding the prefix *de-*. This produced *de-gre* in early French and then ‘degree’ in English. But it was a long time before this carried with it the connotation of continuous divisibility. Once that happened, there came a need for the old idea for a discrete step up or down in rank; so starting around 1800, we see writers reviving the old Latin word and making an English word ‘grade’. When we see an English writer before about 1800—or this essay when recounting such writers—use the word ‘degree’, we must think of discrete steps, not the continuously divisible measurements familiar in mathematics and science today. We should think instead of crimes in the first, second, or third degrees, or first-, second-, and third-degree burns. We should not read our meaning back into the word as it was used. And we should not create confusion by inserting the word where it is not needed, as in translating Aristotle. If his Greek is literally just ‘more and less hot and cold’, we should not translate that as ‘greater and lesser degrees of hot and cold’— as has too often been done. Even if the translator is careful, when we hear ‘hotter’ and ‘colder’, we should not automatically think ‘degrees’. It is an easy mistake to make.

Finally, we should not presume that it took any scientific revolution or the invention of thermometers for people to distinguish how hot something really is from how hot it feels. The difference had long been understood. Many Greek philosophers engaged with the question of perception's reliability, but it did not take a specialist to appreciate the issue. Kiln operators, of even prehistoric antiquity, used test-pieces and the colours of heated clay to objectively check how hot their kilns were.[20] Blacksmiths and metallurgists used the melting points of known materials and the colours of heated ingots. Alchemists knew that melting and boiling points were more reliable than unaided sense experience. Even everyday cooks knew to watch for water to boil, butter to melt, or oil to sizzle.[21]

It was not a need for precision or objectivity that led to the concept of temperature, nor was it the belief that cold is the absence of heat or heat the absence of cold. It was also not the invention of the thermometer. The concept 'temperature' arose because people wanted to think about and refer to combinations of qualities, especially combinations of contrary qualities.

Ancient Greek had a noun, *krasis*, which meant a mixing or compounding, such as a diluting of wine with water. (A *kratēr* was the bowl in which the water and wine were mixed.) *Krasis* became central in the work of the physician Galen (129–c. 210 AD). In the *Technē Iatrikē* (*Art of Medicine*) and the *Peri Kraseōn* (*On Mixtures*), Galen explains that health comes from a good mixing—a good *krasis*, a *eukrasia*—of qualities, including of the complementary qualities hot and cold. Both works entered Latin medical literature in the twelfth century, and both in two

[20] Heated clay gives off colours ranging from a deep red at 500°C to white at 1400°C. Joseph V. Noble, 'The Technique of Attic Vase-Painting', *American Journal of Archaeology*, vol. 64 (Oct., 1960), pp. 307–318. 'Text-box 62: Heat and colour, pyrometric cones and test-pieces', *Ceramics in Archaeology: From Prehistoric to Medieval Times* (p. 373).

[21] In 1880, William Thomson gave several everyday examples of such indicators and called them 'discontinuous intrinsic thermoscopes'. *Encyclopaedia Britannica*, 9th ed. (1880), s.v. 'heat'.

translations, one directly from the Greek, one by way of Arabic.[22] The anonymous translator of the direct translation just transliterated the original Greek and used *crasis* as a Latin word. The other translator, who we know was Gerard of Cremona, translated what he found in the Arabic and came up with *complexio*. It was not an ideal translation, but it caught on with commentators and coloured their understanding accordingly. The *Peri Kraseōn* became known as *De Complexionibus*.

Occasionally the anonymous translator, working directly from the Greek, tried to use a native Latin word instead of the transliteration. Latin had a verb, *temperare*, that meant to mix, especially to mix opposites, especially to mix opposites in a good proportion. This translator used the present participle, duly made into a substantive noun, and came up with Latin *temperantia* for Greek *krasis*—a better translation, in fact, than *complexio*. In the sixteenth century, there were several new Latin translations of Galen’s works and noun forms of *temperare*—*temperies*, *temperantia*, *temperamentum*, and *temperatura*—became the normal rendering of *krasis*. Galen’s *Peri Kraseōn* became known now as *De Temperamentis*. Although the nouns were often used interchangeably, there could be subtle differences. When *temperatura* and *temperamentum* were distinguished, both meant a tempering or moderating by mixing, but with *temperatura* something was tempered with its contrary, such as hot with cold or dry with wet, and with *temperamentum*, the components need not be contraries, as tempering wine with water. But these distinctions were not strict. One could readily hear that the temperature outside was hot and dry. Of course, the words were also used outside of medicine and for mixtures of

[22] Per-Gunnar Ottosson, *Scholastic Medicine and Philosophy: A Study of Commentaries on Galen’s* Tegni *(ca. 1300–1450)*, (Bibliopolis, 1984), ch. 1.

other than hot and cold, wet and dry. Ethicists used *temperatura* and *temperamentum* for a moderate and virtuous balance of extremes.[23] Metallurgists used *temperatura* to refer to a mix of metals.[24] Erasmus used *temperatura* for a mixing of authority and obedience.[25] Geographers used *temperies* and *temperatura* for the combination that constituted the climate in a region.[26] The word *temperatura* was not normally used for extreme heat. Unmoderated heat is a heat *not* tempered by cold; it is a *non-mixture*, a *dis-temperatura*.[27] The Latin words entered English directly as *temper, temperance, temperament*, *temperature*, and *distemperature* and entered French, Italian, and German in comparable forms.

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[23] For example, 'Temperatura: . . . , good disposition, temperatnes,' Thomas Thomas, *Dictionarium linguae Latinae et Anglicanae* (1587). Sometimes also in phrases 'temperamentum ad pondus' (mixed to an equilibrium) or Martial's 'sequi temperamentum in re aliqua' (to follow the proper mix in anything).

[24] For example, in the *Natural History* (1st century AD), bk. 34, Pliny writes, 'Sequens temperature statuaria est eademque tabularis hoc modo' (The proper mixture for statues and tablets is as follows). In *De Natura Fossilium* (1546), bk. 8, George Agricola writes, 'Nam incensa Corintho aurum, argentum, aes in unum confluxerant, tribusque aeris Corinthii generibus fortuna dedit temperamentum' (For in a fire at Corinth gold, silver and copper were melted into one; good luck produced the blend for three kinds of Corinthian copper), and 'varie permiscentes metalla cum metallis: κράματα Graeci vocant, Latini temperaturas' (The mixings of metal with metals Greek-writers call *krámata*, Latin-writers *temperaturas*). In *De Re Metallica* (1556), Agricola writes, 'Temperatura primum adiecto plumbo coquatur in catino cinereo' (The alloy, with the lead in it, is first heated in a cupel).

[25] For example, 'autoritatis et obsequii temperatura' (temperature of auctorytie and of obsequy or ser-uyce), Desiderius Erasmus, 'Liturgia Virginis Lauretanae' (1525) in *Opera Omnia* (Basil: 1540–1541). The English translation was published as a pamphlet by Richard Wyer (London: 1533).

[26] For example, Pliny says the 'caeli temperies' (temperature of the climate) affects blossoming of a particular rose in Spain. *Natural History*, bk. 21.

[27] English examples are in *Oxford English Dictionary*, s.v. 'distemperature'.

Since at least the Hippocratic writings in the fifth century BC, there was the idea that health consisted of a balance of elements or qualities. Most prominent in Galenic medicine was the balances of hot and cold and wet and dry. Galenic physicians graded a mixture, a *temperatura*, on a nine-point scale. There was a balanced mix, considered most healthy and proper (the *eukrasia* in Greek, *bona temperatura* in Latin), and then four grades (*gradus*) hotter and four grades colder; the same for wet and dry. The scales were used to measure the mix of hot and cold or wet and dry in medicines, foods, plants, air, water, and of course the human body. The balance point, however, was not universal and absolute but specific to species, sex, organ, age, season, even latitude.[28] Healthy dogs are drier; healthy men are wetter. A healthy lion is hotter than a healthy man. The healthy mix for a child two years old might be different than that of one five years old. And in any one species there is a healthy mixture specific to the blood, brain, liver, bones, and other components. The four grades of hot and cold, wet and dry were then relative to the particular balance point, the *neutrum*. Medicines too were graded. The fourth degree was fatal, the third degree potentially sickening, the second degree effective, and the first degree effective only if used for a long time. Hemlock was cold in the fourth degree, fennel hot in the third degree and dry in the first, psyllium cold in the second degree but neutral in dryness and moistness.[29] The physician's challenge was to match a countervailing remedy with the patient's imbalance so as to restore a *bona temperatura*. A medicine that targeted the liver and was two grades to the cold might be prescribed for someone whose liver was two grades too hot. Of

[28] Joel Kaye, *A History of Balance* (Cambridge University Press, 2014), chapters 3 and 4; for the examples, pp. 169, 146, 176,

[29] William Turner, *The Names of Herbs in Greek, Latin, English, Dutch, and French* (1548).

course determining what qualified as one, two, three, or four grades in an herb and what qualified as such in a man's liver challenged physicians endlessly.

In summary, in medieval times, a *temperatura* was a mixture. In natural philosophy, medicine, and discussion of the weather, the normal use of 'temperature', or its cognates in Latin, Italian, and French, was for the particular mix of wet and dry or hot and cold. Especially by Galenic physicians, mixtures stronger in one quality than another could be graded with 'degrees', but a degree was not a unit of measure the same from one context to another. Galenic temperatures were how hot and cold (or wet and dry) something is but they were relative, qualitative, and elusive through and through.

4. Thermometer itself had little effect on the concept of temperature

At the end of the sixteenth century, it had long been known that if you invert a long-necked flask partly filled with water and put the mouth in water, the water in the tube will rise as the enclosed air is cooled and fall as the enclosed air is heated. When the air warms, it expands and the waterline drops; when the air cools, it compresses and the waterline rises. In the ancient world, Heron of Alexandria knew about this and his work was translated into Latin in 1547 and again in 1589. In the latter year Giambattista della Porta wrote about this phenomenon in *Magia Naturalis*. And in the 1590s, Galileo discussed using it to measure changes in the relative mix of hot and cold, that is, changes in the *temperatura* with regard to hot and cold.[30] One of Galileo's colleagues, a Galenically trained but innovative experimenter on the medical faculty at Padua,

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[30] For the scattered evidence for invention(s) of the thermometer, see Taylor, 'The Origin of the Thermometer'; Middleton, *A History of the Thermometer and Its Use in Meteorology,* ch. 1; and now, with previously unnoticed evidence regarding Galileo, Matteo Valleriani, 'Pneumatics, the Thermoscope and the New Atomistic Conception of Heat'.

Sanctorio Sanctorius, became enthusiastic about the possibilities. In his commentaries on Galen, published in 1612, he said he has such a glass flask 'by which is measured . . . all the degrees (*gradus*) of hotness or coldness' at any time of day, in all regions, in all places, and in all parts of the body.[31] His device probably had no scale (his first published drawing of one, thirteen years later, did not) and his reference to the *gradus* of hotness or coldness is a reference to the four broad grades of heat and four of cold that were standard in Galenic medicine. Nonetheless, he said he taught his medical student how to use the device and they responded to the novelty with no little astonishment. Also in 1612, Galileo's industrious friend Giovanfrancesco Sagredo told Galileo he was making ten of these instruments an hour at a cost of four lire each.[32] None of these men gave the instrument a name, and when they used the word *temperatura*, it was to say the device can be used to measure the relative mix—the *temperatura*—of hot and cold.

The instrument came to have two names, one in Italy and one in northern Europe. It turns out that the device, as described, with a pocket of air over a column of water, does not just respond to hot and cold. It also responds—though no one at the time knew this—to changing atmospheric pressure. A version that looks more like a teapot than an inverted flask had been used in northern Europe for long enough that people knew that, if the unit is kept in the constant warmth of a home, a quick drop in the water level indicates a coming storm. That is, people used the simple instrument as we now use a barometer, and the device came to be called a weather

[31] 'quo metimur . . . omnes gradus caliditatis, vel frigiditatis'. Sanctorius, *Commentaria in artem medicinalem Galeni* (Venice: Somaschus), pt. III, cap. 85, particula X, p. 612 in the 1632 edition. Middleton's translation, 'measure the degrees of heat and cold', is potentially misleading.

[32] Sagredo says the devices he was making were Sanctorius's instrument but also, in a later letter, that the original was Galileo's invention. We do not know how Galileo's, Sagredo's, and Sanctorius's differed.

glass in English, a *Wetterglas* in German, a *weerglas* in Dutch, a *vitrum calendarium* in Latin, etc. When northerners saw the Italian flask-like version, they recognized the similarities and called that a weather glass too. In his 1620 *Novum Organum*, Francis Bacon, in London, described how to make one.[33]

Things went differently in Italy. Around the time Francis Bacon was writing his description, another one of Galileo’s admirers, a Jesuit from Bologna named Giuseppe Biancani, was writing, in Latin, what would become an influential cosmological treatise, *Sphaera Mundi*. It was published in 1620 and again in 1630, 1635, and 1653. Twice in the treatise, Biancani describes the inverted-flask instrument and says it was invented by the physician Sanctorius. Unlike his predecessors, though, Biancani felt the need to give it a name. He says, ‘It would not be inappropriate to call this flask a *Thermoscopium*’, and ‘I might well call this a *Thermoscopium*’.[34] We do not know who changed the name, but a few years later, the author of *Récréations Mathématiques*, making no claim to originality, called the Italian instrument a *thermomètre*; the charming book became popular throughout Europe.[35] Over time *thermometer*

[33] Francis Bacon, *Novum Organum*, in *Instauratio Magna* (London: 1620), bk. 2, aphorism 13.

[34] ‘Hinc patet ampullam hanc Thermoscopium non inepte appellari posse’ and ‘quod ego Thermoscopium libenter appellarem’. Giuseppe Biancani, *Sphaera mundi seu cosmographia demonstratiua* (Bologna: Girolamo Tamburini, 1620), pp. 127 and 111.

[35] ‘Thermomètre ou instrument pour mésurer les degrez de chalour ou de froidure qui sont en l'air (The thermometer, an instrument to measure the degrees of heat and cold in the air)’. *Récréations Mathématiques* (1624), p. 75, plate p. 69. The work has long been attributed to the Parisian Jesuit Jean Leurechon but on little evidence. See Albrecht Heeffer, ‘Récréations Mathématiques: A Study of Its Authorship, Sources and Influence,’ *Gibecière*, vol. 1 (Pont-à-Mousson: Jean Appier Hanzelet, 2010), available at https://www.researchgate.net/publication/266334553.

became the standard term across Europe, and *weather glass* was relegated to the old teapot-shaped device (still seen hanging on walls today, often now called a Dutch weatherglass).[36]

It is interesting that Biancani chose *thermo-* (heat) rather than *tempero-* (mixture) as the root of his new word. Apparently, he conceived of the device as one for observing or measuring heat, even though to say it was for measuring *temperatura* was to say it was for measuring the mix of hot *and* cold. This was to jump far ahead of the established science. For though Galileo—and apparently his followers—thought cold was just a lack of heat, there was good evidence to the contrary for another two hundred years.

The first instruments did not have scales. (Dutch weatherglasses today still do not.) Once they did, the methods of assigning numbers to degrees of heat and cold varied greatly.[37] There was the Galenic nine-point scale, an eight-point scale, the Florentine 90-0-90 scale, a merely qualitative seven-point scale used by Otto von Guericke that ran from *magnus calor* (greatest hot) at the bottom to *magnus frigus* (greatest cold) at the top, and many more. Also, in early air thermometers, a lower water level corresponded to more heat, not more cold. Even after liquid thermometers, which worked the other way around, became the norm, higher numbers

[36] Middleton, *History of the Thermometer* (1966) proposed to distinguish a thermoscope, a device without a scale, from a thermometer, a device with a scale. This distinction was not sustained in the seventeenth century. In the 1660s, Robert Boyle used 'thermometer', 'thermoscope', and 'weather-glass' interchangeably, as in Robert Boyle, *New Thermometrical Experiments and Thoughts*. In the same years, Robert Hooke used 'thermoscope' for devices with a scale. In the late nineteenth century, William Thomson used 'thermoscope' for anything that indicates temperature, including thermometers with scales but also safety devices that trip when a certain temperature is reached.

[37] The history of thermometric scales has been traced in Bolton, *Evolution of the Thermometer*; Taylor, 'The Origin of the Thermometer'; Middleton, *A History of the Thermometer*; and Chang, *Inventing Temperature*.

sometimes indicated greater cold. Celsius's original scale, for example, had the freezing point of water at 100 and the boiling point at 0.[38] This made sense. If a heat-meter (a thermo-meter) is measuring *temperatura*, that is, the degree of tempering, a higher number should indicate greater lessening of heat. In Galenic medicine, a neutral *temperatura* was an equal mix of hot and cold, and some scales put 0 at the middle of the thermometer; increasing numbers above (or below) indicated more heat, on the other side more cold. Even though thermometers were always devices for measuring intensity of hot and cold, it was a long time before it become natural to speak of cold as having a low temperature and heat a high temperature.

5. After Fahrenheit, temperature became degree of heat

Not all numerical grades represent objective, absolute, and precise measurements. We can meaningfully rate our pain on a scale of 1 to 10. But a 4 on my scale may not be the same as a 4 on yours. And the scale could just as easily have been −5 to +5. And it would be meaningless to distinguish pains to a third decimal place. The limitations are not all due to subjectivity. We can objectively categorize farm produce as being grade 1, 2, 3, or 4, crimes as first, second, or third degree, or minerals on the Mohs hardness scale from 1 to 10. But that does not mean we can perform meaningful algebraic operations using the numbers. We could assign a numerical rating to the softness of bath towels, the difficulty of college curricula, or the intensity of storms, and still—for many and varied reasons—not have measurements qualitatively like weight or length. In the sixteenth and seventeenth centuries, it was taken for granted that heat and cold were objective properties in the world. But it was not clear that the properties could, even theoretically, be the subjects of precise mathematical operations. It took more than marking a

[38] Chang, *Inventing Temperature*, p. 160.

scale on a thermoscope for people to think of temperature as a precisely measurable degree of a determinant physical property.

Throughout the seventeenth century, there were attempts to find stable and absolute points of reference upon which to build an objective scale for thermometers. Sanctorius suggested candle flame and snow, Robert Hooke and Robert Boyle the freezing of distilled water, Christian Huygens freezing water or boiling water, Isaac Newton blood heat and melting snow. All these proposals, of course, highlighted the chicken-and-egg problem of early thermometric scales—stable points of reference were needed to develop objective scales but there was no objective scale with which to test that stability. Several scholars have taken on the task of documenting exactly how natural philosophers overcame the early challenges of thermometric scales.[39] Their project may be a history of what *we* call temperature, but it is not a history of the concept ‘temperature’.

For scales were not the main problem with early thermometers. In 1665 Robert Boyle lamented that there were no standards for measuring cold and heat, as there were for weight, distance, and time; there was no certain way to know degrees of coldness or heat ‘determinately’ and no way to communicate ‘knowledge to a remote Correspondent’.[40] But the concern was not that researchers could not agree on a scale. Boyle continued, ‘[A thermometer] is so mutable a thing, ev’n in the same place, and oft-times in the same day, if not the same hour, that it seems

[39] See especially Chang, *Inventing Temperature*.

[40] Robert Boyle, *New Experiments Touching Cold* (1665) in Michael Hunter and Edward B. Davis (eds.), *The Works of Robert Boyle*, vol. 4 (Pickering & Chatto, 1999), p. 240. The passage Bolton quotes on p. 41 is actually a paraphrase from Peter Shaw, *The Philosophical Works of the Honourable Robert Boyle, Esq; Abridged, Methodized, and Disposed* . . . , 2nd ed., vol 3 (London, 1738), p. 579.

little else than a Moral impossibility, to settle such an universal & procurable Standard of Cold, as we have of several other things’.[41] The problem was that the readings on thermometers were so erratic. Thermometers gave no good evidence that heat and cold were physical properties that could ever be measured precisely.

Back in Galileo’s day, *temperatura*, *temperamentis,* and *temperies* (or temperature, temperament, and temper, or whatever the spellings were in different languages) were, for the most part, used interchangeably. By 1700 though, in Latin and English, *temperies* and temper had become the more common term, *temperatura* and temperature less common. This shift in preference for synonyms was the only major change in the concept across that time. From the Middle Ages until at least a hundred years after Galileo’s colleagues were making thermometers, temper—or temperature—was a mixture, especially a mixture of opposites, opposites that might need to be named if the context did not make them clear. In a work on measuring humidity, Boyle wrote, ‘the temperature of the Air is neither considerably moist, nor considerably dry’.[42] Where we would say just ‘temperature’, natural philosophers of the seventeenth century would say something like ‘the temper as to heat and cold’.[43] At the time, one did not ask for the

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[41] *Ibid*.

[42] Robert Boyle, *A Brief Account of the Utilities of Hygroscopes* (1673) in Michael Hunter and Edward B. Davis (eds.), *The Works of Robert Boyle*, vol. 7 (Pickering & Chatto, 1999), p. 433.

[43] Two of countless examples: ‘Thermometers [show] . . . the differing temper of Heat and Cold’, in Edmund Halley, ‘An account of several Experiments made to examine the Nature of the Expansion and Contraction of Fluids by Heat and Cold, in order to ascertain the Divisions of the Thermometer’, *Philosophical Transactions*, vol. 18 (1694), p. 655. ‘This Liquor perpetually varies its Temperature as to Cold and Heat’, in Robert Boyle, *A Free Enquiry into the Vulgarly Received Notion of Nature* (1686) in Michael Hunter and Edward B. Davis (eds.), *The Works of Robert Boyle*, vol. 10 (Pickering & Chatto, 1999), p. 505.

temperature of the air and expect to be given a number. One might say the temper or temperature of the air—that is, the mix of qualities—was hotter or colder than on the day before but not that the temperature was higher or lower.

'Temperatures', that is, did not rise and fall. What rose and fell were just the heights of mercury, water, or alcohol in thermometers. The numbers on a thermometer were not 'temperatures'. One might say that the height of the liquid was 50 degrees but not that the temperature was 50 degrees. In 1665, using an alcohol thermometer, Robert Boyle recorded the intensity of heat and cold in inches. 'In the Seal'd Weather glass . . . the tincted Spirit rested at 8⅝ inches . . . it descended at length a little beneath 7⅝ inches'.[44] The same year, Robert Hooke constructed a thermometer that became something of a standard reference around the Royal Society.[45] On Sunday, March 10, 1672, he made his first weather report: '☿ [mercury] fell from 170 to 185'.[46] In 1734 a report in France said, 'The winter was very moderate; the greatest cold has caused the liquid of the ordinary thermometer to descend to 23½ degrees'.[47] In 1747: 'The

[44] Robert Boyle, 'A New Frigorifick Experiment Shewing, How a Considerable Degree of Cold May Be Suddenly Produced Without the Help of Snow, Ice, Haile, Wind, or Niter, and That at Any Time of the Year', *Philosophical Transactions* (July 18, 1666), in Michael Hunter and Edward B. Davis (eds), *The Works of Robert Boyle*, vol. 5 (Pickering & Chatto, 1999), p. 520.

[45] Louise Diehl Patterson, 'Thermometers of the Royal Society, 1663-1768', *American Journal of Physics*, vol. 19 (1951), pp. 523-535. Louise Diehl Patterson, 'The Royal Society's Standard Thermometer, 1663-1709', *Isis*, vol. 44 (Jun., 1953), pp. 51-64. Middleton (1966), pp. 58–62.

[46] Felicity Henderson, 'Unpublished Material from the Memorandum Book of Robert Hooke, Guildhall Library MS 1758', Notes and Records of the Royal Society of London, vol. 61 (2007), pp. 129–175.

[47] 'L'hiver a été très-modéré, le plus grand froid n's fait descendre la liqueue du Thermometre ordinaire qu'a 23 degrés ½'. *Mémoires de l'Académie royale des sciences* (1734).

liquid of the thermometer dropped to 5 degrees below freezing’.[48] In 1777, the heading of a column of numbers could still be, not ‘temperature’, but ‘State of the Thermometer / Degrees’.[49] Even a century after scales were added to thermometers it was not normal to think of the readings as degrees of temperature, that is, not to think of the numbers as temperatures.

In 1702, the astronomer Ole Rømer (1644–1710) broke his leg and was confined to his house in Copenhagen. While recovering, he worked out mathematical and practical requirements for a reliable thermometer. Most importantly, he developed a way, using a drop of mercury, to test the uniformity of the bore in a glass tube and to account for any variation.[50] For reference points, he used the freezing and the boiling points of water. A few years later, in 1708, Rømer showed his techniques to Daniel Gabriel Fahrenheit (1686–1736), a young man from Danzig who was trained in business and interested in natural science. Fahrenheit settled in The Hague and in 1717 was selling mercury thermometers based on the techniques he had learned from Rømer and on others he developed himself.

Fahrenheit’s thermometers were revolutionary. They behaved consistently and agreed with one other. They provided evidence that heat and cold were determinant, continuously variable, physical properties that existed along a single spectrum and were subject to precise measurement. In 1714, Fahrenheit showed a pair of his thermometers to Christian Wolff, then

[48] ‘la liqueur du thermomètre est descendue à 5 degrés au dessous de la congélation’. *Mémoires de l’Académie royale des sciences* (1747), p. 573.

[49] ‘Etat du Thermomètre / Degrés’, Louis Cotte, *Traité De Météorologie* (Imprimerie Royale, 1774), p. 228. A nearby table (p. 230) did have ‘température’ as a heading but entries were not numbers and were not about hot and cold; they were ‘froid & humide’, ‘variable, sec & froid’, etc.

[50] His calculations and his procedure for how best to construct a thermometer appear in his working papers, *Adversaria*, ed. Kirstine Meyer (Copenhagen: B. Lunos bogtrykkeri, 1910), pp. 202–213.

professor of mathematics at the University of Halle. Wolff thought it was so extraordinary that Fahrenheit’s thermometers agreed with one another that he published a notice of the fact in the learned *Acta Eruditorum*.[51] For the first time thermometers could be used as real scientific instruments.[52] And people who grew up around Fahrenheit thermometers (or competitors they spawned) conceptualized temperature differently than had their predecessors.

6. In a generation, the post-Fahrenheit concept became the norm

Recall that in English and Latin ‘temperature’ and *temperatura* had largely fallen out of favour. When thinking about intensities of hot and cold, one thought of the ‘temper as to heat and cold’, that is, of the mix of heat and cold. But natural philosophers born after, say, 1725—those such as William Roy, Jean-André De Luc, Joseph Black, Joseph Priestley, James Watt, Antoine Lavoisier, Jacques Charles, and Benjamin Thompson (Count Rumford)—thought differently. For one, they increasingly thought of cold as simply a low intensity of heat. Second, they felt a need to give the scale on the thermometer a name. There had to be a way to think not just about heat but about its measurement, not just about heat but about the degree of heat. ‘Temperature’, the old synonym for ‘temper’, got revived, now used when one wanted to emphasize the condition (Latin suffix *-ure*) of the ‘temper’ (as with *press-ure*, the condition of the pressing). In France in 1772, Jean-Andre De Luc (1727–1817) suggested the meaning was

[51] Christian Wolff, ‘Relatio de Novo Barometrorum & Thermometrorum Concordantium Genere’, *Acta Eruditorum* (1714), p. 380–1. See Bolton, pp. 65–66, and Chang (2004), p. 77.

[52] In the 1830s, *The Popular Encyclopedia* (Glasgow: Blackie & Son, 1835-[41]), s.v. ‘Thermometer’, offered a similar assessment of the history: ‘We now come to mention the greatest improvement made upon the thermometer since the period of its invention.’ The work of Rømer and Fahrenheit is summarized. ‘From this period the thermometer became of scientific utility.’ *The Popular Encyclopedia* (Glasgow: Blackie & Son, 1835-[41]), s.v. ‘Thermometer’.

new when he included it in this glossary entry (in French): ‘Temperature: This word is taken for degree of heat’.[53]

It may have been a fellow Frenchman, Abbé Jean-Antoine Nollet, who introduced the new meaning to England. In 1730, Fahrenheit thermometers were known throughout Europe and becoming popular in England, the Low Countries, and Germany. In Paris, the high-born savant René-Antoine Ferchault de Réaumur (1683–1757) proposed a would-be improvement, an alcohol thermometer by which water froze at 0 degrees instead of 32 degrees and boiled at 80 degrees instead of 212 degrees.[54] Abbé Nollet was Réaumur’s instrument-maker. Réaumur himself did not grow up with the new concept of temperature. When he wrote in 1740, he used ‘the different degrees of cold and heat’;[55] he did not say that something *had* a temperature of so-and-so many degrees. But his younger craftsman did. In an article of 1756, by then a member of the Royal Society of London, Nollet was using the new nomenclature liberally—’the temperature of 12 degrees’, ‘the temperature which was 9 degrees’, ‘that temperature was 45 degrees on Fahrenheit’s thermometer’.[56] In 1762, *The London Chronicle* summarized that article

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[53] ‘TEMPERATURE. Ce mot pris pour *Degré de chaleur*’. Jean-Andre De Luc, *Recherches sur les modifications de l’atmosphère*, vol. 2 (Geneva, 1772), p. 483.

[54] For Réaumur’s thermometer, see Middleton (1966), pp. 79–80, and now especially Jean-Francois Gauvin, ‘The Instrument That Never Was: Inventing, Manufacturing, and Branding Réaumur’s Thermometer During the Enlightenment’, *Annals of Science*, vol. 69 (Oct., 2012), pp. 515–549.

[55] ‘les différents degrés de froid et de chaud’. René-Antoine Ferchault de Réaumur, ‘Observations du Thermometre Faites en MDCCXL a Paris, & dans d’autres endroits, soit du Royaume, soit des Pays étrangers’, *Mémoires de l'Académie royale des sciences* (1740), second part, pp. 539–566.

[56] ‘la température de 12 degrés’, ‘la température qui étoit de neuf degrés’, and ‘celle température étoit de 45 degrés au thermomètre de Fahrenheit’. Abbé Jean-Antoine Nollet, ‘Recherches sur les moyens de

and incorporated the new way of thinking: 'the same temperature of 9 degrees', 'the same temperature, that is, three degrees and a half above freezing', etc.[57] This way to conceptualize temperature seems totally natural to us, but it simply did not exist before the mid-1700s.

The new conception and reliable thermometers enabled a new science. In 1760, Joseph Black was studying and lecturing on chemistry at the University of Glasgow. He stated what would later be called the zeroth law of thermodynamics. 'We must therefore adopt, as one of the most general laws of heat, that "all bodies communicating freely with each other, and exposed to no inequality of external action, acquire the same temperature, as indicated by a thermometer"'.[58] Of the principle, he said, 'we owe the discovery entirely to the thermometer'.[59] Black also made this crucial distinction: 'Heat may be considered, either in respect of its quantity, or of its intensity'.[60] If to one bucket of water, we add another of the same temperature, the quantity of heat doubles but the intensity—the temperature—does not. Black said much recent research into heat has suffered from 'confounding' the two concepts. He used the distinction to explain two phenomena he and others had discovered. The first is that different materials warm up differently

suppléer à l'usage de la glace dans les temps & dans les lieux où elle manque', *Mémoires de l'Académie royale des sciences* (1756), second part, pp. 85, 90, and 90 respectively.

[57] 'A Method for supplying the Want of Ice-houses for cooling Liquors. By the Abbe Nollet'. *The London Chronicle*, May 29–June 1, 1762, p. 515.

[58] Joseph Black, *Lectures on the Elements of Chemistry*, ed. John Robison, vol. 1 (Edinburgh, 1803), p. 77.

[59] *Ibid*., p. 76.

[60] Alexander Law, *Notes of Black's Lectures*, vol. 1, p. 5, cited in Charles Coulston Gillispie, *Dictionary of Scientific Biography*: Volumes 1-2 (1981), p. 178. Compare Black, *Lectures on the Elements of Chemistry*, p. 78.

when exposed to the same heat.[61] If equal amounts of water, at 100 degrees and 150 degrees, are combined, the result is at 125 degrees. But if the materials are water at 100 degrees and mercury at 150 degrees, the result will be at only 120 degrees, not 125 degrees. The heat that raised the temperature of water 20 degrees reduced the temperature of the quicksilver by 30 degrees; the heat was the same but the changes in temperature were different. Also, mercury and water set side by side and exposed to a fire do not warm at the same rate. Each kind of material, Black concluded, has a *specific capacity* for acquiring heat—at first called *specific heat capacity*, later *specific heat*. Black also observed that ice can absorb heat—it can melt—without changing temperature. Similarly, water that is boiling stays at the same temperature, even though it is continually heated. Black called the heat that goes into a material when it changes state but not its temperature *latent heat*.[62] Black's principles of equilibrium, specific heat, and latent heat—all based on the new *conception* of temperature—were the beginning of a true science of temperature.

The foundation was completed by Jean-Andre De Luc. De Luc was a businessman and civic leader in Geneva, an admirer of Francis Bacon, and a man (living in Switzerland, as he was) interested in using the barometer to measure elevation of mountains. How warm or cool the air was affected his calculation, and he ended up writing the most comprehensive study of thermometers of the late eighteenth century, long anticipated and finally published in 1772, in the two-volume *Recherches sur les modifications de l'atmosphère*. The same year, his business fell victim to a French embargo and he fled Geneva. He entered the educated community of

[61] Black, *Lectures on the Elements of Chemistry*, p. 79–81.

[62] See Robison's account in 'Preface', Black, *Lectures on the Elements of Chemistry*, pp. xxxiv–xxxix.

London as a minor celebrity. He was immediately made a member of the Royal Society and given the prestigious appointment of ‘reader’ to the queen.[63] He brought his concept of temperature with him, and he did two things to give it scientific footing. First, though he did not invent, he embraced and advanced, the method of mixtures to ensure that the scale on thermometers was linear. Mixing equal amounts of water of, say, 40 degrees and 60 degrees should, De Luc insisted, produce water of 50 degrees, a mix of 72 degrees and 74 degrees should produce 73 degrees, and so on. Second, he used temperature in a precise mathematical formula, his formula for elevation. This was a milestone. Temperature was now conceived to be an objective property whose measurement could be used in precise scientific calculations.

The new conception became firmly established in the 1770s, as those who grew up around Fahrenheit thermometers replaced those who had not. We can watch the change in the *Philosophical Transactions of the Royal Society of London*. Until the late 1760s, natural philosophers publishing there thought of the temper (or temperature, that is, mixture) as to heat and cold. They did not think of cold as a low temperature or hot as a high one. Thermometers had scales, but the number alongside the liquid’s was not considered a temperature. These natural philosophers did not think of a temperature of so many degrees. Then, in 1768, the surveyors Charles Mason (born 1728) and Jeremiah Dixon (born 1733), in a report of their work in North America, used ‘the temperature of 62° of Fahrenheit’s thermometer’.[64] In the *Philosophical Transactions* of 1771, there was a report that the ‘temperature of the snow on

[63] Chang, *Inventing Temperature*, pp. 14.

[64] Charles Mason and Jeremiah Dixon, ‘Observations for determining the length of a degree of latitude in the provinces of Maryland and Pennsylvania, in North America’, *Philosophical Transactions*, vol. 58 (1768), p. 313.

Sunday morning . . . was near to 30 deg’.[65] In the *Philosophical Transactions* of 1774, ‘temperature’, in its new usage, appeared about 200 times, in two articles about De Luc’s formula for calculating elevation, one by Nevil Maskelyne, Astronomer Royal (born 1732), and one by Samuel Horsley, Secretary of the Royal Society (born 1733).[66]

We can use 1777 to mark the year that the new conception of temperature overtook the old. That year, the *Philosophical Transactions* included a forty-one-page report from a committee’s investigation into the best fixed points for a thermometric scale.[67] It was written mostly by men of the new generation but with a transitional vocabulary. ‘Temper’ appears just twice—in ‘temper of the air in the room’, never in the stock phrase ‘temper as to heat and cold’. The writers were comfortable with ‘degrees of heat’, ‘degrees on the thermometer’, and ‘the number of degrees’, but the word ‘temperature’ appears not even once. Yet outside that article, in the same issue, the word ‘temperature’ appears about 225 times, virtually all in the new way. Included is the first example I have found in which the numbers in a column are labelled

[65] Alexander Wilson, ‘An account of the remarkable cold observed at Glasgow, in the month of January, 1768’, *Philosophical Transactions*, vol. 61 (1771), p. 328.

[66] Nevil Maskelyne, ‘M. De Luc’s rule for measuring heights by the barometer, reduced to the English measure of length, and adapted to Fahrenheit’s thermometer, and other scales of heat, and reduced to a more convenient expression’, *Philosophical Transactions*, vol. 64 (1774), pp. 158–170; Samuel Horsley, ‘M. de Luc’s rules, for the measurement of heights by the barometer, compared with theory, and reduced to English measures of length, and adapted to Fahrenheit’s scale of the thermometer: with tables and precepts, for expediting the practical application of them’, *Philosophical Transactions*, vol. 64 (1774), pp. 214–301.

[67] Henry Cavendish et al., ‘The report of the Committee appointed by the Royal Society to consider of the best method of adjusting the fixed points of thermometers; and of the precautions necessary to be used in making experiments with those instruments’, *Philosophical Transactions*, vol. 67 (1777), pp. 816–857.

‘Temperature’ instead of, say, ‘Height’, ‘Inches’, or ‘Degrees’.[68] The issue also had one of the earlier uses of the symbol ° for thermometric degrees.[69] The complete transition continued for a few more decades, as a new generation of natural philosophers replaced the old.[70] In the third edition of the *Encyclopaedia Britannica* (1797), some articles, especially the long one on weather, used the new conception freely but the article on the thermometer did not. By the *Supplement* of 1824, the old conception had disappeared. The 1781 edition of Brisson’s *Dictionnaire raisonné de physique* had no entry for *température*; the edition of 1800 said temperature is the name given to degree of heat.[71] *Le Dictionnaire de l'Académie française* had long defined temperature as the mix of hot and cold or wet and dry. In 1835, it added, ‘It is also

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[68] George Augustus William Shuckburgh, ‘Observations made in Savoy, in order to ascertain the height of mountains by means of the barometer; being an examination of Mr. De Luc’s Rules, delivered in his *Recherches sur les Modifications de l’atmosphere*’, *Philosophical Transactions*, vol. 67 (1777), p. 518.

[69] In 1740, Reaumur spelled out *degrés* or abbreviated *deg* or *degr*. In 1768, Mason and Dixon, trained in surveying, a profession that was already using ° for angular degrees, used the symbol for thermometric degrees as well. De Luc spelled out *dégrés* in 1772. Use of ° became common in the 1780s. I have not noticed any attempt to use a sexagesimal system for temperature.

[70] In America, the transition can be witnessed in issues of the *Pennsylvania Gazette*. In the 1750s and ’60s, reports would say, for example, ‘the liquor in the thermometer rose thirty degrees’ (Oct. 11, 1753) but not that the temperature did. Similarly for a report on unusually cold weather (Jan. 8, 1767). But in the 1780s, one reads of ‘temperature’ being reduced (Jun. 29, 1785) or of sixty or seventy degrees being a ‘proper temperature’ for adding rennet when making cheese (Jun. 15, 1785). Benjamin Franklin (born 1706), in ‘Physical and Meteorological Observations, Conjectures, and Suppositions,’ *Philosophical Transactions of the Royal Society*, vol. 55 (1765), p. 187, used ‘temperature’ the old way; Thomas Jefferson (born 1743), in *Notes on the State of Virginia*, used it the new way.

[71] ‘Température: Nom que l’on donne au degré de chaleur’. Mathurin-Jacques Brisson, *Dictionnaire raisonné de physique* (Paris).

said of the degree of heat’ and cited Brisson’s *Traité élémentaire ou Principes de physique* (1789–1803) as an early use.[72] In 1804 Count Rumford conducted experiments that firmly established that cold is just an absence of heat.[73]

Come 1817, when De Luc died, one hundred years after Fahrenheit began selling the world’s first reliable thermometers—and not much before, if at all—‘temperature’ was universally taken to mean degree of heat.

7. Thomson sought to replace the ordinal scale with a cardinal one

Fahrenheit-quality thermometers and De Luc’s formula for elevation grounded a new conception of temperature, but they could also provide a false sense of security. It might be easy to think otherwise, but degrees marked on the thermometer were ordinal rankings and not necessarily cardinal values. They were not units of measure. Water at 80° was hotter than water at 79°, but maybe not by the same amount as water at 40° was hotter than water at 39°. It had not been established that the thermal expansion of mercury, or any other material, was exactly and uniformly linear—and it was not clear how that could be established. There was no way to measure intensity of heat without using the scale on a thermometer, and the accuracy of that scale could not be verified without an independent way to measure intensity of heat. The conundrum was not resolved until the 1850s.

Though mercury thermometers had, for practical reasons, become the norm, there was renewed interest in air thermometers in the years around 1800. It was thought they might be

[72] ‘Il se dit aussi Du degré de chaleur.’ *Le Dictionnaire de l'Académie française*, 6th ed. (Paris, 1835)

[73] Hasok Chang, ‘Rumford and the Reflection of Radiant Cold: Historical Reflections and Metaphysical Reflexes’, *Physics in Perspective*, vol. 4 (2002), pp. 127–169.

more linear and more accurate. In the late 1700s, there had been efforts by De Luc (1772), William Roy (1777), and Horace-Bénédict de Saussure (1783) to measure how much air expands with increasing temperature, but their results were inconsistent.[74] Joseph Priestley tried to compare expansion of different gases, but his methods too were inadequate to the task.[75] In the late 1780s, Jacques Charles, natural philosopher and innovator in hydrogen ballooning, found that oxygen, nitrogen, hydrogen, carbonic acid (carbon dioxide), and atmospheric air all expanded the same amount from 0° to 100° on the centigrade scale, but he could not accurately measure how much that was. He never published his results, but he did show his apparatus and results to Gay-Lussac, who published his own findings on the subject in 1802, the same year John Dalton did also. Dalton and Gay-Lussac independently concluded that all gases expand the same amount from one temperature to another, higher, temperature. As Gay-Lussac summarized, 'I therefore conclude that gases in general, provided they are all placed in the same conditions,

[74] De Luc, *Recherches sur les modifications de l'atmosphère* (Geneva: 1772), part 4, chapter 3. William Roy, 'Experiments and observations made in Britain, in order to obtain a rule for measuring heights with the barometer', *Philosophical Transactions*, vol 67 (1777), p. 704. Horace-Bénédict de Saussure, *Essais sur l'Hygrométrie* (Neuchatel: 1783), p. 108.

[75] Reviews of the efforts appear in Gay-Lussac, 'Enquiries concerning the Dilatation of the Gases and Vapors', *A Journal of Natural Philosophy, Chemistry, and the Arts*, vol. 3 (London: 1801), pp. 212–213; and John Dalton, 'Essay IV: On the Expansion of Elastic Fluids by Heat', *Memoirs and Proceedings of the Manchester Literary and Philosophical Society*, vol. 5 (1802), p. 595–596.

expand equally with equal degrees of heat’.[76] Dalton reported, ‘I see no sufficient reason why we may not conclude, that *all elastic fluids under the same pressure expand equally by heat’*.[77]

Unfortunately, when taken out of context these statements could be taken to mean that every advance from one degree mark on a thermometer to the next corresponds to the same increase in heat intensity. Neither man intended this interpretation. Gay-Lussac wrote, ‘The thermometer, as it is at present constructed, cannot be applied to point out the exact proportions of heat, because we are not yet acquainted with the relation between its degrees and the quantities of heat. It is indeed generally thought that equal divisions of its scale represent equal tensions . . . ; but this opinion is not founded on any well decided fact’,[78] and Dalton, ‘Since the publication of my experiments on the expansion of elastic fluids by heat, and those of Gay Lussac, immediately succeeding them . . . it has been imagined by some that gases expand equally; but this is not

[76] Gay-Lussac, ‘Enquiries concerning the Dilatation of the Gases and Vapors’, p. 208, translation of ‘Je conclus que tous les gaz, en général, se dilatent également par les mêmes degrés de chaleur; pourvu qu’on les mette tous dans les mêmes conditions’, *Annales de Chimie*, vol. 43 (Sept., 1802), p. 172.

[77] Dalton, ‘Essay IV’, p. 600. Dalton’s emphasis.

[78] ‘Le thermomètre, tel qu'il est aujourd’hui construit, ne peut servir à indiquer des rapports exacts de chaleur, parce que l'on ne sait pas encore quel rapport il y a entre les degrés du thermomètre et les quantités de chaleur qu'ils peuvent indiquer. On croit, il est vrai, généralement, que des divisions égales de son échelle représentent des tensions égales de calorique; mais cette opinion n'est fondée sur aucun fait bien positif.’ Joseph Louis Gay-Lussac, ‘Enquiries concerning the Dilatation of the Gases and Vapors’, *Annales de Chimie*, vol. 43 (Sept., 1802), pp. 138–139. A translation, used here, was published in English the same year, ‘Enquiries concerning the Dilatation of the Gases and Vapors’, *A Journal of Natural Philosophy, Chemistry, and the Arts*, vol. 3 (1802), pp. 207–216, 257–267. An adjacent article in that issue of the journal was William Henry, ‘A Review of Experiments, Which Have Been Supposed to Disprove the Materiality of Heat’.

corroborated by experience'.[79] The warnings were not always heeded, not even by Gay-Lussac's own friend, Louis-Jacques Thenard in his influential textbook, *Traité de chimie élémentaire, théorique et pratique*, which included, 'All gases . . . expand equally, and their expansion is uniform and equal for each degree. . . . The discovery of this law must be attributed to Dalton and Gay-Lussac'.[80] The temptation to think this way was strong, but in fact it still had not been shown that degrees on a thermometer were anything more than ordinal rankings. They were not units of measure.

In 1848, the talented twenty-four-year-old William Thomson,[81] already professor of natural philosophy at the University of Glasgow and later in life to become Lord Kelvin, wrote, 'The principle to be followed in constructing a thermometric scale might at first seem to be obvious, . . . a perfect thermometer would indicate equal additions of heat, as corresponding to equal

[79] John Dalton, *New System of Chemical Philosophy*, pt. 1 (Manchester: R. Bickerstaff, 1808), p. 9.

[80] Louis-Jacques Thenard, *Traité de chemie élémentaire, théoretique et pratique*, vol. 1 (Paris: Crochard, 1813), p. 37. Translation from Chang, *Inventing Temperature*, p. 69.

[81] Here the most relevant papers by Thomson, or Thomson and Joule, are the following, gathered in—and cited here with the page numbers in—Sir William Thomson, *Mathematical and Physical Papers*, vol. I (Cambridge University Press, 1882), hereafter *MPP*-I. 'On an Absolute Thermometric Scale Founded on Carnot's Theory of the Motive Power of Heat, and Calculated from Regnault's Observations', *Cambridge Philosophical Society Proceedings* (1848), art. 38 in *MPP*-I, pp. 100–106. 'An Account of Carnot's Theory of the Motive Power of Heat; With Numerical Results Deduced from Regnault's Experiments on Steam', *Transactions of the Royal Society of Edinburgh* (1849), art. 41 in *MPP*-I, pp. 113–154. 'On the Dynamical Theory of Heat, with Numerical Results from Mr. Joule's Equivalent of a Thermal Unit, and M. Regnault's Observations on Steam', *Transactions of the Royal Society of Edinburgh* (1851), art. 48 in *MPP*-I, pp. 174–332. 'Thermo-Electric Currents', Transactions of the Royal Society of Edinburgh, vol. 21 (1854), added as pt. 6 of art. 48 in *MPP*-I, pp. 232–291.

elevations of temperature'.[82] But, he lamented, a scale built on this principle was currently 'impossible'; any existing scale was merely 'an arbitrary series of numbered points of reference'.[83]

Thomson thought he espied a way to fix this problem. A few years earlier, he spent four and a half months working in the Parisian laboratory of Victor Regnault, an outstanding experimentalist commissioned by the French government—in its effort to catch up to the English in the use of steam power—to gather as much data as possible on the nature of steam. Thomson got introduced to the elite of Paris's scientific community and gained valuable experience in the art of precise scientific experimentation. While working there, he came across an intriguing paper from 1834 written by a steam-engine designer named Emile Clapeyron.[84] In it, Clapeyron attempted to bring some attention and mathematical rigor to ideas he found in a little-noticed pamphlet written in 1824 by a young engineer named Sadi Carnot, *Réflexions sur la puissance motrice du feu (Reflections on the Motive Power of Fire)*.[85] Carnot's father, the mathematician and politician Lazare Carnot, had done some work on determining the maximum efficiency of

[82] Thomson, 'On the Absolute Thermometric Scale' (1848), *MPP*-I, p. 100.

[83] Thomson, 'On the Absolute Thermometric Scale' (1848), *MPP*-I, p. 102.

[84] Emile Clapeyron, 'Mémoire sur la puissance mortice de la chaleur,' *Journal de l'École Polytechnique*, vol. 14, p. 13–190. Richard Taylor, trans., 'Memoir on the Motive Power of Heat', *Scientific Memoirs, Selected from the Transactions of Foreign Academies of Science and Learned Societies and from Foreign Journals* vol. 1, pp. 347–376.

[85] Sadi Carnot, *Réflexions sur la puissance motrice du feu et sur les machines propres à développer cette puissance* [Reflections on the motive power of fire and on machines capable of developing that power] (Paris: Bachelier, 1824). R. H. Thurston, trans., *Reflections on the Motive Power of Heat,* second, revised, edition (New York: John Wiley & Sons and London: Chapman & Hall, 1897). Robert Fox, trans., *Reflections on the Motive Power of Heat* (Manchester: Manchester University Press, 1986).

waterwheels. Water enters an overshot waterwheel at the top. The same amount of water exits at the bottom. In dropping from higher to lower, the water does work. It produces a mechanical effect.

The younger Carnot thought of steam engines the same way. By changing the pressure and volume of some material (in this case steam) in a repeating four-stage cycle, the engines do work by moving heat—*calorique* in Carnot's French—from a place of higher temperature to a place of lower temperature. In his ideal model for such a machine, he determined that the mechanical effect is proportional to the difference between the two temperatures. Importantly, this effect does not depend on whether the engine drives its piston with steam, air, or anything else. And just as importantly, the mechanical effect can be calculated based on volume and pressure data obtained elsewhere. One does not need to—or even could in fact—construct a Carnot engine; the model is just a mathematical tool.

Thomson was intrigued. Thermometers too depend on expansion and contraction, but with them the rates depend on the materials. So mercury thermometers do not agree with air thermometers, which do not agree with alcohol thermometers, etc. None provide an absolute thermometric scale. In Carnot's idealized engines, Thomson thought he saw a way to relate temperature and motive force in a strict mathematical way that did not depend on the properties of particular materials; as long as he could find the necessary data for one thermometric fluid—any one—he could create an objective thermometric scale in which each degree would be an objective unit of measure.

It turns out Regnault had the relevant data (or something close enough[86]), for steam, for all whole degrees from 0° to 230° on the centigrade scale of a high-quality air thermometer, and Thomson could use that. In 1848, he published in the *Cambridge Philosophical Society Proceedings* his landmark call to action, ‘On an Absolute Thermometric Scale Founded on Carnot’s Theory of the Motive Power of Heat, and Calculated from Regnault’s Observations’.[87] Thomson wrote, ‘The characteristic property of the scale which I now propose is, that all degrees have the same value; that is, that a unit of heat descending from a body *A* at the temperature $T°$ of this scale, to a body *B* at the temperature $(T-1)°$, would give out the same mechanical effect, whatever be the number *T*. This may justly be termed an absolute scale, since its characteristic is quite independent of the physical properties of any specific substance.’[88] The details were published the following year in ‘An Account of Carnot’s Theory of the Motive Power of Heat; With Numerical Results Deduced from Regnault’s Experiments on Steam’.[89]

In Thomson’s scale, the mechanical effect that could be produced in a Carnot engine by a 1° temperature difference, what Thomson labelled μ, would be the same for all degrees.[90] But in Regnault’s measured results for steam, μ was not constant. So Thomson made an adjustment. He

[86] The only problem was that Regnault had latent heat by weight and Thomson needed it by volume. Thomson expected Regnault would gather that data eventually. In the interim Thomson would derive one from the other by assuming steam obeyed the gas laws of Boyle and Dalton. He figured he could refine his results later.

[87] Thomson, ‘On the Absolute Thermometric Scale’ (1848), *MPP*-I, pp. 100–106.

[88] Thomson, ‘On the Absolute Thermometric Scale’ (1848), *MPP*-I, p. 104.

[89] Thomson, ‘An Account of Carnot’s Theory’ (1849), *MPP*-I, pp. 113–154.

[90] For a history of this μ, see William H. Cropper, ‘Carnot’s Function: Origins of the Thermodynamic Concept of Temperature’, *American Journal of Physics*, vol. 55 (1987), pp. 120–129; the detailed studies cited there; and Chang, *Inventing Temperature*, pp. 182–186.

worked out μ for each degree from 0° to 230° on the centigrade scale, then worked backwards to make a new scale in which μ is constant.[91] Each degree of temperature on the new scale would be theoretically capable of producing the same amount of mechanical effect.

But the new scale had a significant practical problem. Of course there had been complaints about the old scales—there was no way to know if every degree really represented an equal change in the intensity of heat; there was no objective way to choose between the different readings on mercury, alcohol, and gas thermometers; the only way to confirm the accuracy of one thermometer was by reference to another one; the degrees on a thermometer were just an ordered set of marks, not units of measurement. But for all the complaining, the old scales were working fairly well. De Luc's approach was producing accurate measurements of elevation. Regnault's results were consistent enough to produce important insights into the behaviour of steam. Other researchers were using temperature readings to successfully characterize gases and kinds of gases. The old scales, Fahrenheit or centigrade, may not have been as precise, accurate, or objective as researchers wanted, but there was something good about them. To be believable and useful, a new scale would need to be close to, or at least easily relatable to, the old ones. And Thomson's was not. When scaled to agree with a high-quality air thermometer at 0° and 100° centigrade, the air thermometer read 50° when Thomson said it should read 53.3°. Around 200°,

[91] Values for the new scale are the mechanical effects given in Table II in 'Account of Carnot's Theory' (1849), p. 140, linearly scaled so that the temperatures at 1° and 100° are the same on both old and new scales. Chang, *Inventing Temperature*, p. 191, presents the results of this calculation, mapping degrees on an air thermometer centigrade scale to degrees on Thomson's proposed scale.

the two differed by 20°. The discrepancies were, as Thomson later wrote, ‘inconveniently wide’.[92]

The new scale had another shortcoming. In an important sense, it was no more absolute than the conventional scales—it was not what we today call a ratio scale. In a ratio scale, not only do equal *intervals* indicate equal *differences* in what is being measured—the difference in heat intensity between $T\,°$ and $T\,° + 5$ is the same for all values of *T*—but the *ratios* of numbers also indicate *ratios* of what is being measured. On Fahrenheit and centigrade scales, 15° does not indicate a 50% higher heat intensity than does 10°. But on a ratio scale it would. A ratio scale has a true zero point, a point at which what is being measured is absent, and Thomson’s first scale did not have one. This was a shortcoming, but a true zero point was just not at first one of Thomson’s goals.

Thomson’s goals and approach would change under influence of other researchers. In using the idealized model that Carnot developed for heat engines, Thomson was approaching indirectly what others had started addressing head-on, namely, the possibility that heat and mechanical work can be converted into each other at some consistent, determinate ratio.[93] He was also grappling with the nature of heat. In 1783, Lavoisier proposed that there exists ‘a subtle fluid, the accumulation of which is the cause of heat and the absence of which is the cause of coldness’. He called this fluid ‘*igneous fluid, the matter of heat and fire*’.[94] In 1787, his colleague Louis-

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[92] Thomson, ‘Thermo-Electric Currents’, *MPP*-I, p. 233.

[93] Thomas S. Kuhn examined the work of a dozen men working on the topic. ‘Energy Conservation as an Example of Simultaneous Discovery’, *Critical Problems in the History of Science,* ed. Marshall Clagett (University of Wisconsin Press, Madison, WI, 1959), pp. 321–356.

[94] Antoine Lavoisier, ‘Réflexions sur le phlogistique,’ in Jean-Baptiste Dumas, ed., *Œuvres de Lavoisier*, vol. 2 (Paris: Imprimerie Impériale, 1862), p. 641. Nicholas W. Best, trans., ‘Lavoisier’s

Bernard Guyton de Morveauthe called the fluid *calorique*. The word appeared the next year in English as 'caloric'. Oddly, commitments to the fluid's actual materiality varied. One could use the word merely to emphasize the fluid nature of heat: Heat *flows* from hot objects to cold ones and anything that flows is, after all, a *fluid*.[95] In fact, even with his waterwheel model, this is probably all that Carnot meant when he referred to caloric. Some of his undated personal notes revealed that he thought the material nature of caloric was a disproven hypothesis.[96] Still, for a generation or two after Lavoisier's proposal, the idea that heat was a material substance, or caused by one, was accepted widely.

In 1798, Count Rumford argued otherwise, based on his experience with cannon-boring. For two reasons, he said, heat could not be a substance. The first involved the fact that the measured heat capacity of the metallic shavings did not differ from that of the metal stock. The second was that the amount of heat that Thompson could generate by friction 'appeared evidently to be *inexhaustible*'.[97] He argued that heat was a form of motion—not a substance—and reported one

"Reflections on Phlogiston" II: On the Nature of Heat', *Foundations of Chemistry*, vol. 18 (2016). Emphasis in original.

[95] Decades later, in 1871, James Clerk Maxwell wrote, 'Heat, therefore, may pass out of one body into another just as water may be poured from one vessel into another, and it may be retained in a body for any time, just as water may be kept in a vessel. We have a right therefore to speak of heat as a *measurable quantity*, and to treat it mathematically . . . . [The] word Caloric was introduced to signify heat as a measurable quantity. So long as the word denoted nothing more than this, it might be usefully employed.' *Theory of Heat*, p. 7.

[96] Printed as Appendix A in Thurston, trans., *Reflections on the Motive Power of Heat* (1897), p. 219.

[97] Benjamin [Thompson] Count of Rumford, 'An Inquiry concerning the Source of the Heat which is excited by Friction', *Philosophical Transactions of the Royal Society*, vol. 88 (1798), p. 99. Emphasis in original. Advocates of the caloric theory saw nothing insurmountable in the second reason, just as nowadays the ability to generate apparently limitless amounts of static electricity by rubbing materials

example of how much heat could be generated with a certain amount of work. He also proposed, incidentally, a mechanical equivalent to heat: In one experiment, a force of 1,034 ft-lbs raised one pound of water 1° Fahrenheit.[98] He did not claim the number was a universal standard.

But in 1831, Pierre Louis Dulong, French chemist, claimed that, for all gases, there was one standard ratio. ‘Equal volumes of all the elastic fluids, taken at the same temperature, and under the same pressure, being compressed or dilated suddenly to the same fraction of their volume, disengage or absorb the same absolute quantity of heat.’[99] He did not propose a numeric value for the quantity.

In 1842, the physician Julius Robert Mayer published a short article proposing that we should think of force (*kraft* in Mayer’s German) broadly, as we do of matter, and that just as matter cannot be created or destroyed but can be converted from one kind to another in

together weighs little against the theory of electrons. Thompson’s first reason was more important. For engagements with his conclusions at the time, see, for example, William Henry, ‘A Review of Experiments, Which Have Been Supposed to Disprove the Materiality of Heat’, cited above; Claude-Louis Berthollet, ‘Note VI’, *Essai de Statique Chimique*, vol. 2 (Paris: 1803), pp. 247–250; Charles Haldat, ‘Inquiries Concerning the Heat Produced by Friction’, *Journal de Physique*, vol. 65, p. 213, reprinted in Nicholson’s *Journal of Natural* Philosophy, vol. 26 (1810); and ‘Caloric’, *The Cyclopaedia; or, Universal Dictionary of Arts, Sciences, and Literature*, ed. Abraham Rees (London: 1819).

[98] James P. Joule, ‘On the Mechanical Equivalent of Heat’, *Philosophical Transactions of the Royal Society of London*, vol. 140 (1850), p. 62.

[99] ‘Que des volumes égaux de tous les fluides élastiques pris à une même température et sous une même pression, étant comprimés ou dilatés subitement d’une même fraction de leur volume, dégagent ou absorbent la même *quantité absolue de chaleur*.’ Pierre Louis Dulong, ‘Recherches sur la chaleur spécifique des fluides élastiques’, *Mémoires de l’Académie des Sciences*, vol. 10 (1831) p. 188. The translation is given by Joule in ‘Mechanical Equivalent of Heat’, p. 62. The emphasis is in both Dulong and Joule.

determinate measurable ways, so too with forces.[100] (In time, what Mayer had in mind came to be called 'energy'.) We rub two plates together, they get warm. We shake water and its temperature rises. 'In . . . numberless cases . . . the expenditure of motion is accompanied by the appearance of heat.'[101] Mayer's article was qualitative and conceptual, but in the last paragraph, he made a clever, insightful, and essentially correct proposal for the ratio at which heat converts to mechanical work. Specific heat capacity is how much heat must be added to something to raise its temperature 1°. It was known that when measuring this for gas in a container, it matters whether the container is allowed to expand or not. If the container is allowed to expand, more heat will be needed to get the 1° increase. Mayer proposed that this is because some of the heat gets converted into the work of expanding the container. Using data he had for the two specific heat capacities of air, Mayer calculated that 'the warming of a given weight of water from 0° to 1° centigrade corresponds to the fall of an equal weight from the height of about 365 metres'.[102] Unfortunately, the data he used was inaccurate; but more detrimental was that he assumed all the extra heat was going into the work of expanding the container and none of it into the gas itself.

In 1843, unaware of Mayer's proposal, James Prescott Joule, a 25-year-old brewer's son in Manchester, England, began direct measurements of the 'mechanical equivalent of heat'. He

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[100] Julius Robert Mayer, 'Bemerkungen über die Kräfte de unbelebten Natur', *Annalen de Chemie und Pharmacie*, vol. 42 (1842), pp. 233–40. Translated into English by G. C. Foster as 'Remarks on the Forces of Inorganic Nature', *Philosophical Magazine*, vol. 24 (1862), pp. 371–377. Reprinted in Stephen G. Brush, ed., *The Kinetic Theory Of Gases: An Anthology Of Classic Papers With Historical Commentary* (World Scientific, 2003), pp. 71–77.

[101] Foster, trans., 'Remarks on the Forces', p. 374; Brush, *Kinetic Theory*, p. 74.

[102] Foster, trans., 'Remarks on the Forces', p. 377; Brush, *Kinetic Theory*, p. 77. Foster (in 1862) added this footnote: 'When the corrected specific heat of air is introduced into the calculation this number is increased, and agrees then with the experimental determinations of Mr. Joule'.

rigged a falling weight to drive a set of paddles in a vat of water. Stirring the water increased its temperature. Joule measured how far the weight would fall for each 1° increase in water temperature. His first result was that a weight of 896 pounds falling one foot would raise the temperature of one pound of water by 1° Fahrenheit. Subsequent results, with increasingly sophisticated methods, were 1001, 1040, 910, 1026, 587, 742, and 840 foot-pounds.[103] Despite the erratic results, Joule was convinced there is in nature a single determinate ratio at which mechanical work can be converted to heat. By May 1845, he was confident that the correct number was about 800. On Thursday June 24, 1847, in a presentation to the Oxford meeting of the British Association, he claimed the mechanical equivalent of heat was 775.4 foot-pounds per pound of water per degree Fahrenheit. Illustrious members of his audience included John F. W. Herschel, William Whewell, Charles Wheatstone, and the mathematician Baden Powell.[104] But

[103] Donald S. L. Cardwell, *James Joule: A Biography* (Manchester University Press, 1989), p. 57.

[104] There are conflicting reports about what numbers Joule reported at this meeting, and the uncertainty may be telling us something about the situation. Joule, young and not an academic, had had a hard time getting respect for his proposal—based on simple experiments with a brewer's vat—that there is one universal conversion factor. He now had here an audience of Britain's scientific elite at its most prestigious event. It would have been important that he speak with conviction. Cardwell, *Joule: A Biography*, p. 82, reports that the meeting was running late and Joule was forced to summarize. We do not have a record of exactly what he said.

‘On the Mechanical Equivalent of Heat, as determined by the Heat evolved by the Friction of Fluids, . . . Read before the Mathematical and Physical Section of the British Association at Oxford, June 1847,’ *Philosophical Magazine*, vol. 31, p. 173, and the abstract published in the Association's own *Proceedings*, report 781.5 for water and 782.1 for sperm oil. But the report printed in *The London Literary Gazette and Journal of Belles Lettres, Arts, Sciences, etc*., Saturday, June 26, 1847, p. 459, written by someone at the meeting, says Joule gave 775.4 for water and 775.9 for sperm oil. That same issue of the *Philosophical Magazine*, p. 114, printed a letter, ‘On the Theoretical Velocity of Sound,’ from Joule, written three weeks after the meeting, in which he wrote, ‘The equivalent of a degree of heat the audience member who would turn out to be the most important for spreading Joule’s proposal was William Thompson, who would celebrate his twenty-third birthday two days later. The two young men became close collaborators.

Joule encouraged Thomson to continue his development of a new temperature scale, but to abandon the caloric doctrine on which it was based, in particular the part that said heat is conserved in a Carnot cycle, like water falling in a watermill. Carnot did not think of motion as heat converted but as the by-product of heat passing from a high-temperature location to a low-temperature one. ‘I dare say’, Joule wrote in a letter to Thomson, ‘they [your ideas] will lose none of their interest or value even if Carnot’s theory be ultimately found incorrect.’[105] But it was not just that Joule thought Carnot’s theory of heat conservation was unnecessary; he thought it was plainly untrue. He described a scenario in which, if heat were not being converted into power, it was being created out of nothing. Thomson admitted problems and in fact replied with his own hypothetical challenge. In October, 1848, he was not ready to surrender the theory of heat conservation, but he conceded that ‘I see no way as yet of explaining this difficulty; but

per lb. of water, determined by careful experiments brought before the British Association at Oxford, is 775 lb. through a foot.’ That would be consistent with the reporter’s account. Then, oddly, Joule’s *Scientific Papers*, vol. 1, p. 282, reprints that letter, but adds as follows: ‘by careful experiments *made since those* brought before the British Association’ (emphasis added).

I wonder if just before the meeting Joule realized a problem with his numbers, if this as much as delays in the agenda explains why he merely summarized, and if a lack of resolve partly explains the lukewarm reception he received.

[105] Letter from Joule to Thomson, October 6, 1848. Quoted in Chang, *Inventing Temperature*, pp. 182–183.

there must be an answer’.[106] A few months later, in the 1849 article ‘Carnot’s Theory of the Motive Power of Heat’, he conceded, ‘A perfect theory of heat imperatively demands an answer to this question, yet no answer can be given in the present state of science’.[107]

8. In the 1850s, a degree became a unit of measure

Part of the answer would lie in abandoning the theory that heat is a material substance, and adopting the kinetic—or as it was called ‘dynamical’—theory of heat that Humphry Davy had developed and that Joule wanted Thomson to accept, namely, that heat is ‘a motion excited among the particles of bodies’.[108] By early 1851, Thomson was convinced and had begun reworking his proposal for a new temperature scale. In March, he published ‘On the Dynamical Theory of Heat, with Numerical Results Deduced from Mr. Joule’s Equivalent of a Thermal Unit, and M. Regnault’s Observations on Steam’,[109] in which he explored what part of his earlier work could be salvaged and what parts needed to be abandoned. Thomson concluded that the incremental values for μ that he took from Regnault could stand as they were, but that they did not accumulate in the simple way he had earlier thought. The total mechanical effect, M, should not be a simple summation or integral of those μ values; it should be a function with that integral as an exponent of the mathematical constant e.[110] Unfortunately for Thomson, this formula

[106] Letter from Thomson to Joule, October 27, 1848. Quoted in Donald S. L. Cardwell, *Springs of Scientific Creativity: Essays on Founders of Modern Science*, ed. Rutherford Aris, Howard Ted Davis, and Roger H. Stuewer (U of Minnesota Press, 1983), p. 59.

[107] Thomson, ‘An Account of Carnot’s Theory’ (1849), *MPP*-I, p. 119.

[108] Thomson, ‘On the Dynamical Theory of Heat’ (1851), *MPP*-I, p. 174.

[109] Thomson, ‘On the Dynamical Theory of Heat’ (1851), *MPP*-I, p. 174–315.

[110] For the first equation, see Thomson, ‘Account of Carnot’s Theory’ (1849), *MPP*-I, p. 134; for the second, Thomson, ‘On the Dynamical Theory of Heat’ (1851), *MPP*-I, p. 190.

produced a temperature scale that was even further from the conventional one than was his first.[111]

Back in December, 1848, in the letter encouraging Thomson to abandon Carnot's presumption of heat conservation, Joule also suggested that Thomson deal differently with μ, the marginal contribution to mechanical effect for each degree of temperature. Thomson wanted μ to be the same for each degree of temperature, found it otherwise in Regnault's experimental results, constructed a new temperature scale that met this goal, and then found that scale too different from established ones to inspire confidence and attract adherents. Joule, on the other hand, thought that it is not the marginal mechanical effect μ but the total mechanical equivalent of heat that is, in nature, constant. The incremental μ times temperature, Joule proposed, should be a constant equal to that mechanical equivalent of heat that Joule had been trying to measure; μ values, he thought, should not be a constant but should be proportional to the reciprocal of temperature. But the μ values Thomson had calculated were not. Joule wrote in his letter, 'I strongly suspect that the experimental data on which the table is calculated are not quite correct'.[112]

Thomson was not immediately convinced, but he did come around step by step. Even though his rejection of material caloric and his conversion to the dynamical theory of heat had not yet gotten him a better temperature scale, it did move him away from his commitment to a constant value for μ. He realized that his new absolute scale did not really require that temperature be a linear summation; he just needed a continuous monotonic function that

[111] Thomson, 'On the Dynamical Theory of Heat' (1851), *MPP*-I, p. 198. The scale can be gotten from Table 38, 'Table of the Motive Power of Heat', column IV.

[112] Cardwell, *James Joule*, p. 99.

produced a linear result.[113] He even realized that had he simply taken the logarithm of his initial *M* value, duly translated and scaled, his scale would have been significantly closer to conventional thermometers, within 0.6° from 0° to 100° centigrade.[114] Thomson had become open to a non-constant value for μ and began calling μ not Carnot's coefficient but Carnot's function.

It is important here to keep a few things separate that we nowadays easily conflate. As mentioned earlier, what Thomson meant by 'absolute' was that whatever quantity a degree represented would be in some mathematical sense the same for each degree. It was not, at first, a goal that an increase in temperature of some percentage would represent an increase in heat intensity of that same percentage. Nowadays, we have a hard time thinking equivalence of degrees was ever in doubt and easily fall into thinking that Thomson's innovation—Lord Kelvin's innovation—was just to offset the centigrade scale by about 273° to give it an absolute zero point and convert it into a true ratio scale, where ratios of temperature values correspond to ratios of what is being measured. But the proposal for a 273° offset was not Thomson's innovation—or really an innovation at all. That idea had been around since at least 1700, when

[113] 'Carnot's function . . . , or any arbitrary function of Carnot's function, may be defined as temperature'. Joule and Thomson, 'On the Thermal Effects of Fluids in Motion', part 2, section 4 (1854), *MPP*-I, p. 393.

[114] See the footnote that Thomson added in 1881 to a reprint of the 1848 paper. *MPP*-I, p. 106. The math works because Joule was correct about the μ values; in an ideal gas they fall off reciprocally. Thomson was summing those values, and the integral of $1/t$ is $log_e(t)$. The reason his result was not closer than 0.6° is that steam, on which Thomson's μ values were based, is not an ideal gas. I disagree with Chang, *Inventing Temperature*, (p. 183) that the footnote is retrospective bravado. Thomson is referring in the note to three attempts at a scale, not just two. The note helpfully explains how the three were in fact related.

Guillaume Amontons (1663–1738) was promoting it. Amontons and others believed that since air expanded with increasing temperature, as air's temperature dropped there would be a point at which its volume and pressure would go to zero. In the late 1840s, it was thought air's coefficient of expansion was right around 0.00366 per degree centigrade. If this was constant through the temperature range, pressure would be zero when temperature was about −273°C. A scale in which that point was set to 0° would be in some sense an absolute temperature scale. But such a feature was not Thomson's concern, even though it was a key part of Joule's proposal.

Joule was proposing that Carnot's function, $\mu(T)$, would simply be 'equal to the mechanical equivalent of the thermal unit divided by the temperature by the air thermometer from its zero of expansion',[115] that is, $\mu = J/T$, where J is the mechanical equivalent of heat and T is a temperature with 0° set to Amontons' theoretical point of zero pressure.

Thomson had opened up to the idea that μ could vary by temperature. And he was coming to accept that it might not be good to rely on the data for saturated steam; he was coming to accept that he may have read too much into Carnot's claim that the substance on which thermodynamic calculations were based did not matter. And in 1850, Rudolph Clausius clearly established that steam could not be any sort of ideal or perfect gas.[116] But Thomson was still not willing to accept Joule's contention that the value of Carnot's function was simply a scaled reciprocal of temperature. If and only if that were true, Thomson insisted, then Mayer's hypothesis about

[115] Thomson, 'On the Dynamical Theory of Heat' (1851), *MPP*-I, note on p. 233.

[116] Keith Hutchison, 'Mayer's Hypothesis: A Study of the Early Years of Thermodynamics', *Centaurus*, vol. 20 (1976), pp. 288 and 290.

specific heat capacities had to be true.[117] And Thomson could not accept that hypothesis without experimental proof.

Thomson got Joule to collaborate with him on a test of Mayer’s hypothesis. The test, the so-called porous-plug experiment, involved forcing a gas through a throttling orifice. If a gas were ideal, temperatures on both sides of the orifice would be the same; the greater the temperature difference, the less ideal the gas.[118] So not only could Thomson and Joule test Mayer’s hypothesis, they could measure how much a particular gas deviated from the ideal. Their conclusion was that for an ideal gas Mayer’s hypothesis would indeed be true, that for hydrogen it is almost exactly true, for air a little less so, for carbonic acid (carbon dioxide) even less, and for steam much less. They had no ideal gas, but based on how a real gas behaved, they could calculate how an ideal one would.

So Thomson could now, finally, in 1854, accept Joule’s proposal from 1848 but he took it one step further. He not only accepted that μ should measure out as $1/T$, but he decided it would have that value *by definition*. On the scale Thomson now proposed, temperature would be, by definition, the reciprocal of the marginal mechanical effect (μ) per degree per unit of heat at that temperature, that is, $\mu(T) = 1/T$. And since Thomson had also worked out the relationship between μ, pressure, and volume, T would be proportional to pressure times volume of an ideal gas.

Thomson’s move here was profound but subtle. After six years of research into Joule’s proposal, Thomson concluded, ‘We may now accept this suggestion with great advantage, since

[117] The hypothesis, in fact, had several interchangeable corollaries. Hutchison, ‘Mayer’s Hypothesis’, p. 279 lists seven.

[118] Thomson, ‘On the Dynamical Theory of Heat’ (1851), *MPP*-I, p. 220.

we have found that Carnot’s function varies *very nearly* in the inverse ratio of what has been called “temperature from the zero of the air-thermometer”’.[119] Thomson did not go on to say that readings on conventional thermometers are close enough to the reciprocal of Carnot’s function that we need merely select one centigrade thermometer as the reference, add 273.7° to its readings, set aside any differences between the results and the Carnot function, and have our desired absolute scale. Instead, he said, we should construct a temperature scale from the mathematics of a non-existent idealized Carnot engine and the behaviour of a non-existent idealized gas. Any differences between the calculated temperature and the readings of a thermometer will be attributed to shortcomings in the thermometer. Thomson set out to construct a temperature scale based on a property he thought would be the same in all materials. He ended using a property that could be measured in none.

Thomson and Joule calculated that the relationship between temperature, pressure, and volume for an actual gas is the following.

$$V = \frac{CT}{P} - \frac{1}{3}AJK\left(\frac{273.7}{T}\right)^2$$

T is temperature on the new absolute scale; *V* is volume; *P* is pressure; and *C*, *A*, *J*, and *K* are constants and measurable gas-specific parameters.[120] This let Thomson and Joule relate the new absolute scale to readings on actual gas thermometers. Temperature, pressure, and volume follow the ideal gas laws, with a gas-specific adjustment due to deviation by actual gases from those laws.

[119] Joule and Thomson, ‘On the Thermal Effects of Fluids in Motion’, part 4 (1862), *MPP*-I, p. 393. Emphasis added. Cited by Chang, *Inventing Temperature*, p. 185.

[120] Joule and Thomson, ‘On the Thermal Effects of Fluids in Motion’, part 4 (1862), *MPP*-I, p. 430. Cited by Chang, *Inventing Temperature*, p. 195.

In 1854, Thomson and Joule published a comparison of the new absolute temperature and a high-quality centigrade air thermometer, calibrated so that the one, offset by 273.7° agreed with the other at the freezing and the boiling points of water.[121] At 313.7° on the absolute scale—that is, $40° + 273.7°$—the two scales agreed to within 0.04°; at 673.7° on the absolute scale, the two agreed to within 0.41°. What should now be done with those tiny differences? At first it would have been said that 673.7° on the absolute scale equals $300° - 0.41° = 299.59°$ on the centigrade scale.

But once theoreticians and practitioners fully embraced Thomson’s proposal, they made two changes. They renamed the scale from centigrade to Celsius. And they defined it *with reference to* Thomson’s absolute scale—the ‘kelvin’ scale. Now 200° on the kelvin scale is, on the Celsius scale, *by definition of the Celsius scale*, 200° plus that offset to absolute zero. So now, on either scale, a difference between the value calculated from pressures and volumes, and the reading of the thermometer, is attributed to inaccuracy in the thermometer.

The unit of measure for temperature became a calculation based on pressure, volume, and the work done by an ideal Carnot engine. By measuring the intensity of heat by the absolute kelvin scale, the mechanical equivalent of heat became a constant, by the very definition of the scales.

9. Overlapping definitions emerged

In the decades after Thomson’s proposal for an absolute scale, there emerged three distinct ways to think of temperature. They were similar and often mathematically interchangeable. But they prevailed in different communities and were used in different situations. Their referents

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[121] Where we now use 273.15°, by definition, they used 273.7°, based on their measurements.

were often identical but the cognitive integrations varied. In a few cases, scientists were forced to decide what one conception or one definition is more important or more fundamental. Some of those discussions remain unresolved. Some lie even today at the cutting edge of scientific research.

The first way to define temperature was by using the formula for an ideal gas, $T = PV/nRT$. R and n are known constants and pressure (P) and volume (V) can be readily measured for any gas. Results from porous-plug experiments (or others increasingly sensitive) can provide the adjustments needed to compensate for the fact that an existing gas is not ideal.

The second definition involves entropy. Starting with Clapeyron, physicists and mathematicians—using the methods of differential and integral calculus, and even extending them—became quite adept at generalizing what was at first a simple model for a four-stage steam engine. They developed what were, within specific domains, mathematically equivalent treatments of temperature and its scale. Thomson's relating of μ, a coefficient relating work and temperature, to the pressure and volume of an ideal gas was just one such development. Rudolph Clausius's invention of the concept of entropy (as we will see) produced another. So too then will be development of statistical molecular mechanics by James Clerk Maxwell (1831–1879) and Ludwig Boltzmann (1844–1906).

Thomson and Joule were contributing to a new scientific conception of energy and to a principle of the conversation of energy. The part of that development most relevant in the new field of thermodynamics was that in a heat engine, every little bit of heat energy that goes into a heat engine (dQ) either stays there as a small increase in the internal energy of the engine's components (dU) or is converted into a small amount of external mechanical work (dW). No energy is lost or created. That is,

$$dQ = dU + dW.$$

Clausius called this the ‘first main principle’[122] of the mechanical theory.

Once there was an absolute temperature scale, it was realized that in a simple Carnot engine, with its two isothermal phases, the amount of heat (Q) that enters (as a positive number) divided by its absolute temperature (T) is equal to the amount of heat that leaves (as a negative number) divided by its absolute temperature. The two quotients add to zero. That is,

$$\frac{Q_{\text{in}}}{T_{\text{in}}} + \frac{Q_{\text{out}}}{T_{\text{out}}} = 0.$$

This was readily generalized to reversible heat engines with not just two, but i, isothermal phases, the sum of all adding to zero,

$$\sum \frac{Q_i}{T_i} = 0,$$

and then to reversible heat engines without distinct phases, where pressure and temperature vary continuously,[123] that is, where changes in Q are indefinitely small and the fraction is integrated instead of summed,

$$\int \frac{dQ}{T} = 0.$$

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[122] *Hauptsatz* in German. ‘Law’ would eventually prevail, but ‘main principle’ was a good rendering at the time. Browne used it in his translation of 1879. Preston, *The Theory of Heat*, (1894, 1904, and 1919) used ‘fundamental principle’.

[123] In *The Mechanical Theory of Heat* (1875), p. 90, Clausius claimed to have been the first to publish this equation, in *Poggendorfs Annalen*, vol. 93 (1854), p 500.

The fraction here, dQ/T, has important properties and came to appear in many thermodynamic equations.[124] It is the perfect differential of a quantity that Rudolph Clausius labelled S and named ‘entropy’.[125] Thus,

$$\frac{dQ}{T} = dS,$$

or

$$dQ = T\,dS.$$

Clausius said this equation expresses the ‘second main principle’ of the mechanical theory of heat. He combined it with the equation for the first main principle and got

$$dU = T\,dS - dW.$$

In a heat engine, the incremental work done (dW) is equal to pressure (P) times any small change in volume (dV), and so

$$dU = T\,dS - P\,dV.$$

That is—soon generalized to any thermodynamic system, not just heat engines—a little change in entropy times the temperature at which the change occurs, less the work done by a change of

[124] It is, for example, what remains constant in the other two phases of a four-phase Carnot cycle. Two are isothermal, the other two isentropic.

[125] The fraction attracted the attention of Rudolph Clausius in Germany but also William Rankine in Scotland. While Clausius used the symbol S and the name ‘entropy’, Rankine used φ and labelled the fraction as simply a thermodynamic function. Well into the twentieth century, those writing in English normally used Clausius’s ‘entropy’ and Rankine’s φ. Of course, what – if any – physical macroscopic physical property corresponds to entropy has troubled physicists and countless students since. In 1910, Hugh L. Callendar, ‘The Caloric Theory of Heat and Carnot’s Principle’, *Proceedings of the Physical Society of London*, vol. 23 (1910), pp. 153–189, proposed that entropy is nothing but the old caloric. The suggestion has received insufficient consideration.

volume at some pressure, equals the change in the system’s internal energy. Clausius had this tidy synthesis—the ‘thermodynamic equation’ or the ‘thermodynamic identity’—worked out by the time he published *Die Mechanische Wärmetheorie* in 1876. The work was published in English, as *The Mechanical Theory of Heat*, in 1879,[126] and this one equation has been a unifying principle for thermodynamics ever since—like Newton’s $f = ma$ for mechanics or Einstein’s $e = mc^2$ for relativity or $V = IR$ for electrical engineering.[127] It would have an important effect on the conception of temperature.

As with comparable scientific equations, this one specifies a relationship between parameters. It does not specify a direction of causality. If a sufficient number of the variables are determined, all of the others are.[128] This means, for example, that when no work is being done (that is, when dV and therefore $P\,dV$ are zero), temperature is a ratio of incremental energy and incremental entropy,

$$T = \left(\frac{dU}{dS}\right)_{V\text{ constant}}$$

or equivalently

[126] Rudolph Clausius, *Die Mechanische Wärmetheorie* (Braunschweig: Vieweg, 1876). Walter R. Brown, trans., *The Mechanical Theory of Heat* (London: Macmillan and Co., 1879).

[127] The identity was anticipated in Rankine, ‘On the Geometrical Representation of the Expansive Action of Heat, and the Theory of Thermodynamic Engines’, *Transactions of the Royal Society*, vol. 144 (1854), pp. 115–175, but it was Clausius’s approach that was more influential.

[128] The importance of this is noted by Clausius, ‘Formation of the Two Fundamental Equations’, *The Mechanical Theory of Heat* (1879), chapter 5; Tait, ‘Elements of Thermodynamics’, *Heat* (1884, 1892, 1895, 1904), chapter 21; and Preston, ‘Thermodynamic Formulae’, *The Theory of Heat* (1894, 1904, 1919), section 5. Like Thomson before them, Tait and Preston use Rankine’s φ instead of Clausius’s *S*.

$$\frac{1}{T} = \left(\frac{dS}{dU}\right)_{V\,\text{constant}}.$$

In practice, it was normal to determine entropy by measuring energy and temperature, rather than temperature by entropy and heat, but the relationship was soon noticed as theoretically important.[129] The textbook *Heat* (1884, 1892, 1895, 1904), by Thomson's collaborator P. G. Tait, said, 'If the substance be kept at constant volume, the gain of energy, per unit increase of entropy, is measured by the absolute temperature'.[130] Thomas Preston's popular textbook, *The Theory of Heat* (1894, 1904, 1919) had 'The absolute temperature measures the increase of internal energy per unit change of entropy at constant volume'.[131]

A third definition of temperature developed alongside the entropy-based one and was based on the theory that heat is molecular motion. In 1854, Thomson abandoned the caloric theory of heat and adopted the dynamical theory. The move got him to his absolute temperature scale, one that made the ideal gas law—pressure times volume is proportional to temperature, $PV \propto T$—true by definition. It turns out, however, that what mattered more to Thomson's progress was not the details of molecular motion but simply that he abandoned conservation of heat and accepted conservation of energy. In fact, when Thomson tried to use particle dynamics to calculate his μ, his results were worse not better. But once he had developed the absolute scale and degrees

[129] In 1880, Thomson had a way to use entropy in an equation for temperature, but the formula worked by reducing entropy to a constant that could then be eliminated from the computation, so it never had to be measured. Thomson, *Heat* (1880), §48. The article was also published the same year as the entry for 'Heat' in the ninth edition of the *Encyclopaedia Britannica*.

[130] P. G. Tait, *Heat* (London: Macmillan and Co.), §385 in all editions.

[131] Thomas Preston, *The Theory of Heat* (London: Macmillan and Co., 1894), chap. 8, art. 307, p. 642.

could become units for measuring temperature, molecular dynamics started to play a role in mathematical thermodynamics.

Back in 1738, Daniel Bernoulli calculated what the pressure of a gas would be, in terms of velocity of the particles, if pressure is just the impact of small particles hitting the wall of the container. The formula is not difficult and was revived or redeveloped several times in the coming decades; interest in it waxed and waned with interest in a kinetic theory of heat. But little was, or could be, done with the formula. That changed once there was an objective scale for temperature. It had been easy enough to show, using Newtonian laws of force, that kinetic energy—mass of an hypothesized particle times the average velocity of many of them—would be proportional to pressure times volume of a gas. Now, with an absolute temperature scale in hand, this could be combined with the definition of temperature by which temperature is proportional to pressure times volume of a gas. The conclusion is that kinetic energy of a gas is proportional to absolute temperature.[132] It was readily accepted that if the unseen particles really exist, if they all have the same mass, and if they obey laws of Newtonian mechanics, then temperature measures their mean kinetic energy—or the energy measures the temperature. In his 1894 textbook, Preston wrote, ‘The temperature, then, must be measured in some way by $\overline{V}^2$, the mean square of the velocities of the molecules, or by their mean kinetic energy’.[133] The conclusion, however, was limited. It applied only to gases and there was a problem with the constant of proportionality.

[132] For example, James Clerk Maxwell, *Theory of Heat* (London: Longmans, Green, and Co., 1871), chapter 22, pp. 281–312.

[133] Preston, *Theory of Heat* (1894), chap. 1, art. 55, p. 70.

And that problem was part of an even larger one, the so-called specific heat anomaly.[134] Recall that specific heat is how much heat must be added to something to raise its temperature 1° and that specific heat differs depending on whether the container is allowed to expand or not. If heat is molecular motion and temperature measures the average kinetic energy with which molecules move from place to place, it should be easy to calculate the ratio between the two measured specific heat values. Clausius brought up the topic in 1857, but did not expect it to be a problem. In 1860, Maxwell realized it was. The measured specific heat ratio for a certain large class of gases was 1.408; the theory said it should be 1.634. '[This] result of the dynamical theory, being at variance with experiment, overturns the whole hypothesis'.[135] Maxwell soon tried another approach, and the results were even worse. In 1871, Ludwig Boltzmann derived a theoretical value of 1.33—closer but still far from the observed 1.408. In 1875, surveying 'evidence of the molecular constitution of bodies', Maxwell published a promising possible solution, but it conflicted with observations of spectral lines. The next year, Maxwell's colleague Henry William published *A Treatise on the Kinetic Theory of Gases* and had to agree that although the theory predicted correct values for many properties, it was inconsistent with observed specific heat and light spectra.[136] In 1875, two researchers measured the specific heat

[134] Much of the following on the specific heat anomaly is drawn from Henk W. de Regt, 'Philosophy and the Kinetic Theory of Gases', *The British Journal for the Philosophy of Science*, vol. 47 (1996), pp. 31–62.

[135] James Clerk Maxwell, 'On the Results of Bernoulli's Theory of Gases as Applied to their Internal Friction, their Diffusion, and their Conductivity for Heat', in 'Notes and Abstracts', *Report of the Thirtieth Meeting of the British Association for the Advancement of Science* (1861), pp. 15–16.

[136] Henry William Watson, *Treatise on the Kinetic Theory of Gases* (Oxford: Clarendon Press, 1876), pp. 23–25.

ratio of mercury vapor as 1.66, and this provided an important new data point. In 1876, Boltzmann offered yet another possible solution to the specific heat anomaly, but in a review of Watson’s book, Maxwell argued that Boltzmann’s latest, too, was inadequate. Twenty years later, Boltzmann reported that at least in Germany, ‘it has been concluded that the assumption that heat is motion of the smallest particles of matter will eventually be proved false and discarded’.[137] Those few who conceived of temperature as a measure of average kinetic energy of molecules were on the defensive.

These defenders pressed on. In *The Dynamical Theory of Gases* of 1904, J. H. Jeans said he would simply *define* a temperature scale in which temperature equals the average translational kinetic energy of molecules in a gas.[138] Covering similar material the same year, J. H. Poynting and J. J. Thomson said, ‘the questions here discussed or rather indicated are still open’.[139] Textbooks on heat continued to be primarily about macroscopic thermodynamics as developed by Thomson, Joule, and Clausius. Kinetic theory got, if anything, a supplementary hypothetical chapter at the end of the book, as it had in Maxwell’s own *Theory of Heat*. But fortunes turned in the early twentieth century after Albert Einstein published a paper on specific heat in 1906, which he said would show how the latest thinking on radiation ‘leads to a modification of the molecular-kinetic theory of heat by which some difficulties obstructing the implementation of

[137] Ludwig Boltzmann, *Lectures on Gas Theory*, trans. Stephen G. Brush (University of California, 1964 [1896–98]), p. 23.

[138] J. H. Jeans, *The Dynamical Theory of Gases* (1904), ch. 6, art. 124, p. 108, under heading ‘Temperature: Definition’.

[139] J. H. Poynting and J. J. Thomson, *A Textbook of Physics: Heat*, 3rd ed. (London: 1904), p. 140.

that theory can be eliminated'.[140] One textbook that got on board was Preston's *Theory of Heat*, now amended posthumously by Joseph Rogerson Cotter. The third edition, of 1919, was the first to include a chapter on kinetic theory, and it fully adopted the new way to think about temperature: 'We will now show that if the temperature of a gas be *defined* as *a quantity proportional to the mean kinetic energy of translation*, then the gas of the kinetic theory will obey the ordinary laws of gases'.[141]

Just as temperature's identification with average translational kinetic energy was gaining adherents, its association with entropy was also getting stronger. Recall that, at first, entropy was conceived as a ratio determined by the independent variables energy and temperature. But as the theory got fleshed out, entropy took its place in an equation that mutually determined several variables. In theory, temperature could be defined in terms of entropy as easily as the other way around. But at first it was not; it was simply not clear what physical property corresponded to a quantity that was at first a purely mathematical construct.

But in 1877, Boltzmann used probability to explore the fact that the speeds and thus the momenta of molecules vary.[142] He determined that the probability that some combination of

[140] *The Collected Papers of Albert Einstein*, vol. 2, *The Swiss Years: Writings, 1900–1909* (English translation supplement), trans. Anna Beck (Princeton University Press), pp. 214–224.

[141] Thomas Preston and J. Rogerson Cotter, *The Theory of Heat*, 3rd ed. (London: Macmillan and Co., 1894), chap. 9, art. 374, p. 803. Emphasis on 'defined' added; the rest is in the original.

[142] Ludwig Boltzmann, 'Über die beziehung dem zweiten Haubtsatze der mechanischen Wärmetheorie und der Wahrscheinlichkeitsrechnung respektive den Sätzen über das Wärmegleichgewicht,' *Wiener Berichte*, vol. 76 (1877), pp. 373–435. Kim Sharp and Franz Matschinsky, trans. 'Translation of Ludwig Boltzmann's Paper "On the Relationship between the Second Fundamental Theorem of the Mechanical Theory of Heat and Probability Calculations Regarding the Conditions for Thermal Equilibrium"', *Entropy*, vol. 17 (2015), pp. 1971–2009.

positions and velocities would produce a macroscopic property such as temperature was equal to a number raised to the power of the entropy. Also, since probability here could be calculated by simply counting the number of ways the macroscopic property could be produced, entropy is equal to the logarithm of the number of microstates that produce some macrostate. Boltzmann did not formulate this discovery as we do now, $S = k \ln W$, or use the discovery to much change his thinking about temperature. He did not, for example, use it in any substantial way in his *Lectures on Gas Theory*, published twenty years later. His colleagues, too, gave it little attention. Max Planck, for example, did not use it in his *Treatise on Thermodynamics* in 1897.

But, in 1900, Planck tried using Boltzmann's discovery about entropy to solve a problem in blackbody radiation that he was struggling with. And it worked. In a move that ended up being the start of quantum mechanics, Planck used Boltzmann's discovery about entropy to compute a distribution for thermal blackbody radiation that completely agreed with experimental results for low, medium, and high frequencies. In 1906, Einstein's proposal for light quanta gave Planck's empirical prediction needed theoretical support. Planck later reported that very few physicists had been giving entropy much attention and his own choice to do so gave him his breakthrough. As quantum theory developed over the next twenty years, it was realized how the entropy equation—now retroactively attributed to Boltzmann—applied to quantum microstates and not just classical ones. Interest in entropy greatly increased, and the old thermodynamic definition of temperature in terms of energy and entropy took a place alongside the recently revived definition in terms of the translational kinetic energy of molecules.

In 1939, Sydney Chapman and Thomas George Cowling, in their influential treatise *The Mathematical Theory of Non-Uniform Gases*, summarized the situation that had emerged:[143] There were multiple systems of reckoning temperature in common use among physicists. There was the ideal gas thermometer (or, in practice, actual gas thermometers with a correction factor applied). There was the theoretical temperature of thermodynamics, that is, $T = dU/dS$. And there was temperature as the mean translational kinetic energy of molecules. In general, the three agreed with each other, but there were cases (and came to be more) where only one was applicable, or where results differed. Following are three such.

10. In the twentieth century, boundaries of the concept got challenged

In classical thermodynamics, entropy can be calculated only under conditions of thermal equilibrium. But average translational kinetic energy can be calculated whenever there are enough molecules for an average to be meaningful. Chapman and Cowling said, 'The kinetic-theory definition of temperature is applicable whether or not the gas is in a uniform or steady state, and therefore it provides a concept of temperature more general than that of thermodynamics'.[144] In their day, this theoretical difference had limited relevance, since no one was actually averaging known velocities of individual molecules. But that changed in the last quarter of the twentieth century with advances in computational molecular simulations. How to

[143] Sydney Chapman and Thomas George Cowling, *The Mathematical Theory of Non-Uniform Gases* (Cambridge University Press, 1939), ch. 2, art. 2.41, p. 37.

[144] *Ibid*.

calculate kinetic temperature where entropy-based temperature is undefined is now a vigorous part of the science of non-equilibrium molecular dynamics (NEMD).[145]

Back in 1902, Josiah Willard Gibbs, in *Elementary Principles in Statistical Mechanics*, explored the relationship between temperature and dU/dS. He agreed that the two had a certain correspondence, but he would not allow that the two were analogous, because dU/dS could in theory be negative and absolute temperature could not.[146] Fifty years later, that presumption was under attack on two fronts. In an article about hydrodynamics in 1949, L. Onsager considered a case of point vortices where dU/dS would turn negative and, Onsager said, the result would be (with his quotation marks around the term) ‘negative “temperatures”’.[147] Onsager’s extension of the concept was contentious and by 1980 there was a literature on how to explain the oddity. In 1991, there was a proposal that ‘the well-known Onsager paradox of negative temperatures . . . could be resolved by adopting a proper formula for entropy’.[148] The debate continues.

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[145] J. Casas-Vázquez and D. Jou, ‘Nonequilibrium temperature versus local-equilibrium temperature’, *Physical Review E*, vol. 49 (Feb. 1994); and J. Casas-Vázquez and D. Jou, ‘Temperature in non-equilibrium states: a review of open problems and current proposals’, *Reports on Progress in Physics*, vol. 66 (2003) provide entry into the literature. The first article reports, ‘Out of equilibrium, . . . definition (1) [that is, $1/T = dS/dU$] is not directly operative and relation (2) [that is, $(3/2)kT = \langle(1/2)mv^2\rangle$] is used as a definition of temperature . . . in molecular-dynamics simulations’ (p. 1042); little was said about these simulations. The later progress report studies them in some depth and also explores cases where both kinetic and thermodynamic temperatures can be calculated but differ slightly.

[146] Josiah Willard Gibbs, *Elementary Principles in Statistical Mechanics* (New York: Charles Scribner’s Sons, 1902), pp. 169–176.

[147] L. Onsager, ‘Statistical Hydrodynamics,’ *Il Nuovo Cimento*, vol. 6 (1949), p. 281.

[148] V. Berdichevsky, I. Kunin, and F. Hussain, ‘Negative temperature of vortex motion,’ *Physical Review A*, vol. 43 (Feb 15, 1991), p. 2050.

At Harvard University in 1950, Norman Ramsay, Robert Pound, and Edward Purcell discovered puzzling behaviour in the nuclear magnetic resonance of a crystal of lithium fluoride.[149] They concluded that, although normally entropy increases with increasing energy, in this case it was decreasing, that is, dS/dU (and so also dU/dS) was turning negative—and so it was appropriate to say that the absolute temperature was negative. Ramsay reported that it took some work to convince his colleague at Oxford Sir Francis Simon that this was the right way to speak. Again, this would extend the concept beyond that of kinetically defined temperature. Later, Charles Townes said a talk on negative temperature that he heard Ramsay give at Columbia University stimulated his invention of the maser, and negative temperature has become the conventional way to describe the state of certain molecular spin systems. The idea, as new but uncontroversial, made it into the fifth edition, 1968, of Zemansky's influential textbook.[150] Objections do still exist but are not frequent.[151]

Anything hot glows with the thermal radiation that Planck was researching. Like white light, thermal radiation is not of just one colour. It is a combination of many colours. The light has a spectrum, a spectrum specific to the temperature of the body glowing. The concept is familiar nowadays to those shopping for light bulbs. A light bulb with a so-called 'colour temperature' of

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[149] See Ramsay's reflections (including the stories about Simon and Townes) in the prefatory abstract to 'Paper 6.1: "Thermodynamics and Statistical Mechanics at Negative Absolute Temperatures," N. F. Ramsey, *Phys. Rev.* 103, 20–28 (1956)', *Spectroscopy With Coherent Radiation: Selected Papers of Norman F. Ramsey (With Commentary)* (World Scientific, 1998), p. 389.

[150] Mark W. Zemansky, *Heat and Thermodynamics*, 5th ed. (McGraw-Hill, 1968), pp. 487–492.

[151] E.g., Henning Struchtrup, 'Work Storage in States of Apparent Negative Thermodynamic Temperature,' *Physical Review Letters*, vol. 120 (2018); and Quanmin Guo, 'Negative Absolute Temperatures,' preprint, submitted Oct. 4, 2019, https://arxiv.org/ftp/arxiv/papers/1910/1910.01915.pdf.

5900K emits a spectrum of light like that emitted by a body heated to 5900 degrees on the Kelvin scale—which is about the surface temperature of our sun. Early in the life of our universe, when its matter had a temperature of about 3000K, there was an emission of thermal radiation that decoupled from its source. The electromagnetic radiation is still with us, thirteen and a half billion years later. Because of a phenomena called red-shift (an example of the Doppler effect), the spectrum of that radiation is now the same as that of a blackbody heated to 2.7K. That is, we say the cosmic microwave background radiation has a colour temperature of 2.7K, just as we say a light bulb has a colour temperature of 2700K. Now we can meaningfully discuss the energy and the entropy of thermal radiation, but not its kinetic energy. So many physics consider it meaningful to use the entropy-based definition of temperature to assign a temperature to any electromagnetic field that has the spectrum of a blackbody at that temperature—that is, to consider ‘colour temperature’ as a kind of temperature. Others insist that it is the blackbody that emitted or emits the radiation, rather than the radiation itself, that has a temperature. By this thinking, the entropy-based temperature and the kinetic temperature are the same. By the other thinking, entropy-based temperature is simply a broader concept than can apply even when kinetic temperature does not.

11. Summary of the history of ‘temperature’

Let me summarize.

In 1871, James Clerk Maxwell, who had just lived through and personally contributed to one of history’s most consequential studies of heat and temperature, wrote ‘The temperature of a body, therefore, is a quantity which indicates how hot or cold the body is’.[152] Someone hearing

[152] Maxwell, *Theory of Heat*, p. 2.

this a few hundred years earlier would have followed the sentence easily enough but also found it a little odd.

For starters, the claim would have sounded narrow, for back then ‘temperature’ (or cognates in Latin, French, and Italian) was not limited to hot and cold. It could refer to any mixture of opposites—hot and cold, yes, but also wet and dry, even brave and cowardly. A speaker in Shakespeare’s day would qualify the term and say, for example, ‘temperature (or temper) as to hot and cold’ or ‘temperature as to hard and soft’. Also though temperature could indeed be how hot or cold something is, more precisely, it was how hot *and* cold something is. For hot was one thing, cold another, and most things in the world had in them some mixture of the two. The cold tempered the hot; the hot tempered the cold. Opposites temper one another. But the idea that temperature was a property of things in the world and not just an artifact of sense experience was familiar, well established at least as far back as Aristotle. Most unfamiliar in Maxwell’s statement would have been the claim that temperature is a quantity.

First steps in that direction were taken by Galenic physicians. By the late 1500s, it was recognized that you can use an inverted and partly filled flask of water or wine to indicate how hot or how cold the air is. By about 1620s, such flasks—then called thermoscopes or thermometers—were used to grade mixtures into nine categories: an even mix of hot and cold, then four grades hotter and four grade colder. Makers of such devices then attempted finer gradations, but with little consistency. There were many scales, some in which larger numbers indicated hotter mixtures, some in which larger numbers indicated colder mixtures. To establish points of comparison, natural philosophers including Robert Boyle, Christian Huygens, and Isaac Newton proposed assigning certain values to, say, the mix of hot and cold at which water, wine, or butter froze, or boiled, or the mix of hot and cold in a healthy human body. The large problem with thermometers, however, was not a lack of consensus about reference points. It was the inconsistent behaviour of the devices. No two units produced the same results. Even one unit produced different results day to day, even hour to hour. It was just not clear that temperature—or more commonly at the time, ‘temper’—was by nature a quantifiable property of things.

The problem turned out to be a manufacturing one, and it was solved by Daniel Gabriel Fahrenheit in around 1714. The fact that he could produce two thermometers that produced identical results was itself worthy of a report in one of Europe’s leading academic journals. It now appeared that how hot or cold something is is in fact an objective and measurable property in nature and that, since the two really are physically on a continuum, we can think of cold as an absence of heat. The term ‘temperature’—*temper*, the ‘mix’, plus *-ure*, the ‘state or condition of’—got revived and used when one wanted to indicate the degree of heat. Consequently, people who grew up around Fahrenheit thermometers thought of temperature in a new way. To them, temperature was no longer a mixture of hot and cold; it was a degree of heat and a number could be assigned to that degree. The numbers on a thermometer became temperatures, and people started speaking of temperatures rising and falling. In the late eighteenth century, having a high temperature became a synonym for hot; having a low temperature a synonym for cold.

A degree of temperature, however, was not a unit of measure. Something with a temperature of 45° was hotter than something with a temperature of 44°, but that difference was not necessarily the same as the difference between 46° and 45°. Not all degrees represented the same quantity. The numbers were members of an ordered set but did not indicate quantities on an absolute scale. In 1848, William Thomson (known to us now as Lord Kelvin) proposed to construct an absolute scale for temperature based on the latest research into the ratio at which mechanical work can be converted to heat, and vice versa. It took him six years, but with help from James Prescott Joule, he succeeded. Temperature took its place with pressure, weight, and length as an objective, measurable, physical quantity. Only after that would Maxwell’s statement—‘The temperature of a body is a quantity which indicates how hot or cold the body is’—seem natural and obvious.

Thomson’s proposal worked its way into three definitions. The first defines temperature by its relationship to pressure, volume, and number of particles in an ideal gas, that is, by the relationship $PV = nRT$. The second defines temperature by its role in a larger thermodynamics identity that involves entropy and internal energy and reduces in the relevant condition to $T = dU/dS$. The third defines temperature as translational kinetic energy of molecules, that is, T as proportional to the average of $(\frac{1}{2})mv^2$. The definitions are usually mathematically interchangeable. In the second half of the twentieth century, however, physicists were pressed to rethink the boundaries of the concept. Most (though not all) physicists have, for example, decided it is good to extend the concept of temperature to cases where dU/dS indicates a negative absolute temperature, even though that makes no sense under the kinetic definition; or that we can rightly use the kinetic definition in cases where change in entropy (dS) is undefined; or that we can meaningfully speak of electromagnetic waves having a temperature, even though the waves contain no particles and so have no kinetic energy.

That is, the concept of temperature matured through the categories of increasing sophistication summarized in this essay’s introduction—nominal, ordinal, interval, and ratio. At first, there was hot and cold, and descriptive terms like warm, hot, and very hot. When hot and cold were mixed together, each tempered the other. In early thermometers—devices for indicating the amount of tempering—numbers (when there were any) were hardly more than names for warm, hot, very hot, and extremely hot, cool, cold, very cold, and extremely cold.

Early scales increased the resolution but the result was still just an ordinal ranking; a higher number came to indicat heat of higher intensity, but there was no assurance that a degree on one end of the scale represented the same amount of heat as a degree on the other end of the scale. Centigrade and Fahrenheit scales then sought to be interval scales; in which, for example, five degrees would represent the same quantity everywhere on the scale. The goal was not really met, however, until these scales could be defined relative to the absolute ratio scale that William Thomson (Lord Kelvin) introduced. On his scale, not only can one perform operations of addition and subtraction—not only is any degree equal to any other—but the ratio of two temperatures corresponds to the ratio of physical properties. Something at 200° is twice as hot as something at 100°, and a temperature of 0° means there is no heat. Maturation of the temperature concept was an often slow, sometimes fast, progression from one way to measure how hot and/or cold something is to another.

12. Philosophical reflections

Though, as mentioned, the above account is intended primarily for those who need a reliable history of temperature from which to draw their own philosophical conclusions, let me nevertheless offer a few observations of my own.[153]

The concept of temperature is a concept of relative magnitude, what Aristotle called a concept of 'the more and less' (*to mallon kai ētton*). The instrument of measuring this magnitude (thermometer), the process for doing so (thermometry), and what the magnitude is a magnitude

[153] In recent decades, discussions about temperature as a case study in incommensurability and conceptual change—cited in a note at the beginning of this essay—have died down some. But for background, revival and essays relevant here, see Corinne Bloch-Mullins and Theodore Arabatzis, eds, *Concepts, Induction, and the Growth of Scientific Knowledge*.

of (heat) are all different. The separate concepts have their own developments, but the developments also influence others. How people thought about thermometers influenced how they thought about temperature; how they thought about temperature influenced how they thought about heat; and so on. That is why—even though temperature is not the measurement itself but what is being measured—the concept of temperature evolved in parallel with the maturation of thermometric scales, nominal to ordinal to interval to ratio. Because of this parallel, we can say the concept of temperature matured. But it did so in another and more important way as well. Temperature serves as a case study in how a concept of magnitude can becomes scientific in the sense that equations in which it enters will be both true as the result of extensive inductive inquiry and true by definition.

Concepts are individual cognitive products. You have your concept of temperature (as of anything else); I have mine. Dictionaries may document similarities between the concepts that people in a community have at a point in time, but the concepts do not exist outside anyone’s mind, inside the dictionary or anywhere else. The fact that enough members of one generation think similarly about something—and think about it differently than the previous generation did—makes histories such as this one about temperature possible. But the history should not obscure the fact that we are merely documenting what individual human beings had in their minds.

Concepts are, therefore, organic—in two senses of the term. They are the cognitive products of organic beings, but also the concepts shared in a community change a little from one generation to the next. In several ways, the later concepts can be more mature. The boundaries of inclusion may be better delimited—the community might agree more on what would and would not qualify as instances. The integrations might be broader, deeper, stronger, better characterized. This all happened with the concept of temperature, from the sixteenth century into the nineteenth. The concept of temperature that Black and De Luc shared was identifiably more advanced than the one Galileo and Boyle and Newton had in mind. Black distinguished temperature of heat and quantity of heat, something Galileo did not. De Luc and Nollet thought of temperature as a measurable property of nature, a magnitude that could be used in a mathematical equation. Natural philosophers of their grandparents’ generation thought of it as a mix of heat and cold not subject to objective quantification. Temperature was considered a relative and qualitative mix of hot and cold and later an objective and quantifiable property of things.

A remarkable thing then happened in the 1850s. Researchers from De Luc in the 1770s to Regnault and Joule in the 1840s had been seeking correlations between temperature and other magnitudes in nature. None of the correlations were found to be exceptionless. Then in 1853, Thomson announced that, after six years of research that he had conducted with Joule, he saw the value in creating a new temperature scale in which specified correlations would always be exact—*by the very definition of that scale*. The temperature of an ideal gas would be related to its pressure and volume by a specified equation. If results in some experiment did not agree, then the gas was not ideal, the thermometer was inaccurate, or the pressure or volume had been mis-measured. And scientists accepted Thomson’s (Lord Kelvin’s) proposal. A concept had been studied enough, stressed enough, reconsidered enough—it had become *mature* enough—that a scientific law could be formulated that was true *by definition* of that concept.

Algebra uses concepts of extreme maturity. We long ago decided that 2 plus 3 would equal 5 by the very boundaries we set on the concepts 2, 3, 5, plus, and equals. If we find a situation that does not fulfil that relationship, then one or more of the particulars are not instances of those concepts.

It is unlikely we will come across situations that tempt us to redraw the boundaries on our concepts of basic mathematical operations, but that did happen with the concept of temperature. Three seemingly interchangeable definitions came into use—one based on ideal gases, one based on entropy, and one based on molecular motion. Then we discovered cases where the three were not interchangeable. And we were forced to decide. A few physicists found that they could explain unusual molecular properties if they accepted as universal the definition of temperature based on entropy and treated the one based on molecular motion as a special case; for the work they were doing, the choice had great practical benefit. Other physicists found benefit in doing the opposite, taking the definition based on molecular motion as the more general and one based on entropy as a special case. Nowadays most physicists treat the general case as the one where all definitions agree and just draw on one or the other as needed in the special cases where one definition would apply and the other would not. Work continues on ways to integrate subcategories of temperature and find a definition that encompasses all cases.

The concept of temperature has reached a maturity that allows us to say large classes of relationships are true as a result of extensive inductive inquiry *and* true by definition. This sort of concurrence deserves more study.

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