Reinterpretation of the Genesis of Newton's "Law of Cooling"

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Communicated by A. R. HALL

Students of elementary physics usually encounter and often run an experiment based on NEWTON's "Law of Cooling". For example, the following statement is found in a standard contemporary physics text[1]: "The rate at which a hot body cools to the temperature of its surroundings is given by an empirical formula first discovered by Sir ISAAC NEWTON. The law states the rate at which heat is lost by a body to its surroundings is proportional to the difference in temperature between them." Now, most assuredly, NEWTON did not regard his generalization about the rate at which bodies lose heat as an empirical law, but there is little or nothing in the literature of the history or logic of science to suggest how the law was established, what its logical status was, or what factors surrounded its "discovery". Moreover, the dates proposed for the conception of this generalization are almost certainly wrong. Consider what MORE, drawing on BREWSTER and BIOT, has to say on the subject:

> In May 1701, NEWTON read the only paper on Chemistry which was published except for his short articles on acids. Under the title of *Scala graduum caloris*, he described a thermometer which he had invented probably some years previously, at least his note-books show that he was experimenting with thermometers in 1693. It was during these experiments that he discovered his law of cooling bodies; the second discovery was his observation of the constancy of fusion and boiling temperatures; and his third, was the graduation of thermometers between these constant temperatures, thus making them comparable[2].

The classification of this paper as chemical, strange as it may seem, is perfectly correct for as BOAS has stated, "it is essentially a chemical paper, not only because of its interesting use of the melting points of mixtures of metals, but also because the related problems of heat and fire were considered to be part of chemistry, rather than physics, in the eighteenth century"[3]. Yet their

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[1] WHITE, HARVEY E.: Modern College Physics, Third Edition, p. 288. Princeton 1956.

[2] MORE, L. T.: Isaac Newton, p. 488. New York 1934. NEWTON's paper "Scala graduum caloris" ("A scale of the Degrees of Heat") appeared in "Philosophical Transactions" No. 270, 1701 and is reprinted in COHEN, I. BERNARD: Isaac Newton's Papers and Letters on Natural Philosophy, Cambridge, Mass. 1958, pp. 259—264 (Latin) and pp. 265—269 (English translation). For BREWSTER and BIOT, see Footnotes 6 and 7.

[3] In: COHEN, *op. cit.* p. 243.

readiness to classify the subject matter in this way may have blinded scholars to the quite clear *suggestion* of the law of cooling found in the *Principia*. The *Principia* is a book on mathematical physics, and one would not expect or know where to find passages dealing with heat or chemistry. Yet in the section on comets in Book III a number of arguments about the physical nature of the heads and tails of comets are based upon thermal considerations[4]. The neglect of this material on heat grows out of the more general neglect of the material on comets. Perhaps the last attention given to NEWTON's views on heat and the cooling law as revealed in his comments on comets was that of MARTINE (1748) who pointed out a number of conceptual errors[5]. Apparently nobody since that time has written on these early speculations on heat and tied them to the problem of understanding NEWTON's conceptual development on heat, in general, or the cooling law in particular. Only the particular problem on the development of the cooling law will be treated here.

MORE indicated that the cooling law, which is found in the anonymous paper of 1701, was "discovered" during the course of experiments NEWTON performed in 1693. This interpretation, which is *partially* correct, is based on a polemic BREWSTER[6] wrote against BIOT[7]. In his biography of NEWTON, BIOT noted that the 1701 paper contained three important discoveries:

> l'une est la manière de rendre les thermomètres comparables, en determinant les termes extrêmes de leur graduation d'apres des phénomènes de températures constantes; la seconde est la determination de la loi du refroidissement des corps solides à des températures peu élevées; enfin, la troisième est l'observation de la constance des températures dans les phénomènes de fusion et d'ébullition, constance qui est devenue l'un des fondements de la théorie de la chaleur.

This list of NEWTON's achievements was repeated by BOLTON[8] and, as we have seen, MORE. BIOT argued that these discoveries must have been made before 1693 because NEWTON was supposed to have suffered a period of temporary insanity in that year and was not competent to perform original work in anything but theology after that time. BREWSTER was inflamed by this suggestion on two counts. Not only did it deprecate theology but it also detracted from the "flawless character" of NEWTON. It is not profitable, at least in this context, to pursue the anti-religious bias of BIOT nor the tendency of nineteenth-century scholars like BREWSTER to romanticize the life and work of the great scientists. Put very briefly, BREWSTER searched NEWTON's notebooks for evidence by which he could date these particular achievements and thereby refute BIOT. Finding an incomplete notebook entry, he stopped without delving more deeply into the problem. This entry, which is printed by BREWSTER, is clearly related to the

[4] CAJORI edition of the Principia, Berkeley 1934, pp. 521—522, 528. See also section on the heat of the planets, p. 417. A comprehensive study of NEWTON on comets is now in progress.

[5] MARTINE, GEORGE: Essays Medical and Philosophical. London 1740 (Essays V and VI). Later critics of the law of cooling apparently have not recognized, or taken into account, the significance of the remarks in the Principia.

[6] BREWSTER, DAVID: Memoirs of the Life, Writings, and Discoveries of Sir Isaac Newton, Vol. 2, pp. 362—367, 132—134. London 1854.

[7] Biographie Universelle, Ancienne et Moderne, T. 31, pp. 190, 167—169. Paris 1822.

[8] BOLTON, HENRY C.: The Evolution of the Thermometer, p. 54. Easton, Pa. 1900.

1701 paper and contains the date March 10, 1692/93 [9]. BREWSTER triumphantly concluded: "... from this manuscript it is obvious that NEWTON was engaged in his experiments on the scale of heat at the very time he was [supposed by BIOT to be] incapable of such an effort; and as he had not yet then completed the inquiry it follows that the discoveries which it [the 1701 paper] contains were made at a later date"[10]. BREWSTER's finding may refute BIOT's assertion that NEWTON was incapable of original scientific work after 1693, but it does not warrant the assertion that the three discoveries were made between 1693 and 1701. In fact, the notes dating from 1692/93 are quite puzzling, especially when viewed in the context of NEWTON's achievements in heat and thermometry before that time.

The laboratory notes can be divided into two parts. The first part consists of a list of ten phenomena for which the temperature is to be found. The list consists of such entries as: water begins to freeze, water boils vehemently, tin begins to melt, molten lead sets, *etc*. However, only two of the blanks are filled in. These are, "... y^e heat of my body (to w^{ch} I equal y^t of a bird hatching her eggs) stands at y^e degree of $17\frac{3}{4}$, March 10, 1692/93", and "when water is as hot as y^e hand can endure to stay long in at y^e degree 26"[11]. The second part consists of slightly over a page of rough laboratory notes on the procedures NEWTON used to determine the expansibilities of air, linseed oil, and alcohol. He concludes that the rarefaction of air to water (slip for oil) in equal heats is, in round numbers, 10 to 1, the rarefaction of oil to alcohol is about 15 to 1, and therefore the rarefaction of air to alcohol is as 150 to 1. These conclusions were repeated in the 1701 paper which, however, also included much more detail on the parts of expansion at various fixed points such as are listed in the first part of the MS. One slight difference is noticed in that the passage in the notebook, "the space w^{ch} lintseed oyle took up wth such a heat as I could give to a little bolthead wth my body was to y^e space w^{ch} it took up in such a degree of coldness as made water begin to freeze, as 41 to 40"[12] is replaced in the 1701 paper by "first it was found by the thermometer with linseed oil, that if, when it was placed in melted snow, the oil possessed the space of 10000 parts; then the same oil rarefied with the heat ... of a human body possessed the space of 10256 parts ...; therefore the rarefied oil was to the same expanded by the heat of the human body, as 40 to 39"[13]. The difference between these ratios is only about 6 in 10,000 but NEWTON saw fit to change them.

The ten phenomena listed in the laboratory notes are found in the 1701 temperature scale along with numerous others. In the paper, of course, they are assigned numerical values on a scale with fixed points at the ice point and at body heat to which are assigned the values 0 and 12 respectively. Thus while there is a clear connection between the notes and the published paper, it is evident that a great deal of careful experimental work was done between 1693

[9] The original manuscript notebook is Cambridge University Library MS. Add. 3975, described in "Catalogue of the Portsmouth Collection" (Cambridge, 1887), 21. The thermometrical notes are on f. 45.

[10] BREWSTER, p. 367.

[11] MS. Add. 3975, f. 45.

[12] MS. Add. 3975, f. 46.

[13] COHEN, pp. 267—268.

and 1701. This of course is all that BREWSTER wanted to show; however, a number of questions deserving of consideration arise in studying the notes. Why are temperatures provided for only two phenomena on the list and then with numerical values different from those given in the 1701 paper? Does this indicate a period of exhaustion (one does not have to assume NEWTON had a mental breakdown) during which NEWTON could not carry on the work? Why were the determinations not inserted in the notes at a later date? Did NEWTON conceive of a different procedure and decide to start anew, or did he merely misplace the notes? At the time these initial experiments were planned did NEWTON intend to devise a new thermometer or new thermometric technique, or did he rather intend to establish a new scale of temperature based on existing measuring techniques? What was the status of the cooling law at various stages in these experiments? These are among the interesting questions that can be asked, ones whose answers would throw fresh light on NEWTON's development as an experimental philosopher.

Consider first the numbers in the notebook assigned to body heat and water as hot as the hand can stand, $17\frac{3}{4}$ and 26 respectively. On the 1701 scale, as we have seen, body heat was arbitrarily assigned the value of 12, which is about $\frac{2}{3}$ the earlier value. In the measurement of the heat of water an ambiguity is found. On the 1701 scale two cases are distinguished; "Almost the greatest heat of a bath, which a man can bear by moving his hand in it for some time; also that of blood newly drawn" and "Greatest degree of heat of a bath which a man can bear for some time without stirring his hand in it"[14]. These are assigned values of $14\frac{3}{11}$ and 17, respectively. The problem is, to which of these values does the earlier value correspond? We shall see that the value of 26 on the earlier scale almost certainly corresponds to 17 on the 1701 scale, and that the numbers read on the later scale are accordingly two-thirds of the corresponding numbers on the earlier scale. On the assumption that a linear relationship exists between the two scales, the zero point of the scale used in 1693 can be calculated. It turns out that it falls at about 7.2 on the 1701 scale (about 71° F) if we assume that the hot water at 26 (1693) corresponds to the hot water at $14\frac{3}{11}$ (1701). However, if we make the alternate assumption that 26 (1693) corresponds to 17 (1701), the zeros of the two scales coincide. While a zero point equivalent to 71° F may seem strange, it is not obviously wrong because, at this period, thermometers were often constructed having the zero point fixed at maximum summer heat or "the heat of a cave". For example, an instrument built by JOHN PATRICK for JOHN LOCKE in 1701 had its zero fixed at a point equivalent to 89° F. It is not likely, however, that NEWTON had an instrument of this type, because the zeros fail to coincide, and, more important, the numbers on LOCKE's scale increased with lower temperatures. However, the most conclusive argument depends on internal evidence in the notebook. The procedure that NEWTON used to determine the expansibility of air involved holding a bolthead (flask) under water for 6 to 8 minutes. In an effort to get the greatest possible expansion, using this technique, water as hot as can be endured must have been used, and its temperature, undoubtedly, must have been measured with whatever thermometer was close at hand. Very significantly we can presume that the bolthead

[14] COHEN, p. 265.

must have been held motionless in order to avoid the loss of air from it by agitation. Hence, as it is likely that the value reported in 1693 corresponds to the value listed in the 1701 paper for the greatest temperature of still water that the hand can endure, it is evident that the scale in use in 1693 had the same zero-point as that proposed in 1701 and that the size of the degree on the earlier scale was only about $\frac{2}{3}$ that of the later degree.

This relation between the two scales is further supported by information found in a memorandum made by ARCHIBALD PITCAIRNE during a visit to NEWTON at Cambridge on 3 March 1691/92; "... an incubated egg, or anything else warmed only by the heat of the skin, grows so hot that it raises the oil contained in a thermometer to ten degrees; the heat of blood raises it to twenty degrees, that of milk to less. But the summer heat of the sun or of the air at that season raises it to $7\frac{1}{2}$, and boiling water to 52."[15] The temperature of an incubated egg *etc.* is not listed in the 1701 paper but the others are found in it, and the values for corresponding phenomena differ by the same factor of $\frac{2}{3}$ previously discovered. This agreement indicates that the values in the Pitcairne memorandum and the laboratory notebook are based on the same scale and that the latter was properly interpreted. This is illustrated in Table 1. Small differences between the various scale values are unavoidable not only because of

Table 1. *A comparison of temperature scale values drawn from different sources*

Phenomena	1701 scale (°N)	1693 scale (°M)	Pitcairne scale (1692)*	Proposed conversion of M° to N°	
				°M = 1.5° N**	°M = 3.6° N − 26
Melting snow	*0*			*0*	−26
Heat of summer air	4, *5*, 6		*7.5*	6, *7.5*, 9	−11.6, −8 −4.4
Incubated egg			10		
Body heat	*12*	*17¾*		*18*	17¾
Greatest heat hand can stand in stirred water and heat of blood	*14 3/11*	26?	*20* (heat of blood)	*21 9/21*	26
Greatest heat hand can stand in still water	*17*	*26?*		*25.5*	35
Water begins to boil	33		(water boils)	49.5	94
Water boils vehemently	*34*		*52*	*51*	96.6
Water by boiling scarcely acquires any greater degree	34.5			51.75	98.4

* Undoubtedly the same scale used in 1693.
** The most likely conversion formula.

[15] TURNBULL, H. W.: The Correspondence of Isaac Newton: 1688—1694, III, p. 209. Cambridge 1961.

inaccuracies in the thermometers, but also because the equation developed to relate the 1701 scale with that in use earlier assumes perfect linearity of the apparent expansion throughout the entire range. We now know that this assumption is incorrect and that if the indications of two thermometers agree at two or more points they are most likely to diverge at other points by a small amount.

On looking further into the matter it becomes evident that the early thermometers used by NEWTON were calibrated against the instrument constructed by HOOKE at the direction of the Royal Society and known as the "Royal Society's Standard Thermoscope". The history of this instrument has been thoroughly described by PATTERSON[16]. It served as a standard instrument for some 40 years (c. 1665–1709). Thus it continued to be used for a number of years after NEWTON had abandoned its scale in favor of his own. What is curious is that although a large number of meteorological observations based on HOOKE'S scale were made by BOYLE, HOOKE, LOCKE, and others, no one had the interest or the foresight to ascertain the conversion factor relating HOOKE'S to the newer scales so that their data could be compared with new data. It is strange that efforts of the most important scientific society of the day undertaken to provide a common standard of temperature measurement should have dropped so completely from scientific consciousness.

PATTERSON has delved into the problem of finding the conversion factor between HOOKE'S standard scale and a modern scale. Her article should be consulted for the details of the arguments which are sketched here. Considering both HOOKE'S description of the thermometric substance used and his general method of calibration (a method using the proportional expansion of colored alcohol from the freezing point of water) PATTERSON concludes that the *prima facie* conversion equation should be: $°C = 1.1$ (or $1.2) \times °H$. But this yields values that are too low compared with expected air temperatures. Fortunately HOOKE used his scale on the marine barometer that he devised, consisting essentially of an alcohol-thermometer mounted beside an air-thermometer. As it was commonly known that the air-thermometer had the defect of responding to changing pressure as well as changing temperature, the inventive genius of HOOKE devised a method by which the "error" shown by the air-thermometer could be converted to a deviation of existing air pressure from some standard pressure. HALLEY provided a numerical example of the method involved, and PATTERSON has reworked his data using the modern gas law and has been able to calculate what the temperature should have been and thus what the conversion factor between the HOOKE scale and the centigrade is: namely $°C = 2.38$ (or $2.39) \times °H$. This she interprets as a doubling of the unit degree some time between 1664, when the method was conceived, and 1672, when the first extant data recorded in what must be assumed to be "degrees Hooke" are found in HOOKE'S *Diary*. This is corroborated by several other findings. An experiment to determine the density of air was performed by PAPIN in 1684. The Royal Society suspected the accuracy of this experiment and ordered its repetition; from the data a conversion factor of 2.1 can be calculated. HAUKSBEE performed a similar

[16] PATTERSON, LOUISE D.: "The Royal Society's Standard Thermometer, 1665—1709". Isis **44**, 51—64 (1953).

experiment in 1708, which also yields a factor of about 2.1 (2.08). In 1709 HAUKSBEE performed an experiment to determine the density of water, the data from which yield a factor of 2.56. None of these values is incompatible with the value obtained from HALLEY's example relating to the marine barometer, and PATTERSON concludes that as an approximation we can convert from HOOKE's scale to the centigrade scale using the equation: $°C = 2.4 \times °H$.

In order to determine whether or not NEWTON was using HOOKE's scale, the conversion factors can be calculated for the two points measured by NEWTON to which we can attribute modern values with high plausibility. Since $37° \text{ C} = 2 \times 17\tfrac{3}{4}° \text{ N}$ (body temperature) and $100° \text{ C} = 2 \times 52° \text{ N}$ (boiling water), approximately, we might propose a conversion factor of 2:1 which agrees with one of PATTERSON's calculated equivalents, though it fails to fit the ratio of $2 \cdot 4 : 1$ that she finally accepted. At any rate, the agreement is close enough to suggest that NEWTON used HOOKE's scale before establishing his own. Because of the known technical difficulties with zero points, uniform bores, *etc.* in seventeenth century instruments, and known theoretical difficulties in comparing instruments having different non-uniform expansions and different sizes of bulb, bore, *etc.*, we cannot expect anything but rough agreement between the measurements made with different thermometers using HOOKE's scale. Furthermore, it is reasonable to suppose that NEWTON would have used a scale widely employed within the Royal Society on his early instruments. Thus, we can be virtually certain that NEWTON's data was based on HOOKE's scale and, in fact, provide an independent check on PATTERSON's findings.

The question of why NEWTON decided to change the "size" of the degree is tied to the question of whether or not oil expands uniformly, and this in turn is related to the logical status of the cooling law. These questions will be considered later. Let us first consider the kinds of thermometers that NEWTON used in the 1680's and 90's and the problems that probably arose in using them.

The notebook in which NEWTON considered the degrees of heat makes it plain that he was actively engaged in chemical experiments in every year from 1680 to 1686, though he may perhaps have not resumed them again until 1690. It also makes manifest his interest in fusibility and melting-points, while the well-known reminiscences of his amanuensis, HUMPHREY NEWTON, speak of the ceaseless laboratory fire and an ample stock of chemical materials and apparatus[17]. All this leads one to guess that NEWTON was equipped with common alcohol-filled thermometers and that he would have had good reason to be dissatisfied with their inability to measure the high temperatures he was working with. It is highly probable that it was in the course of such chemical experiments that NEWTON was stimulated to devise an instrument having a more extended range than the common alcohol thermometers of his day. It is likely that NEWTON had already devised an oil-in-glass thermometer by the time the *Principia* was published, because it contains a good measurement of the boiling point of water. This could not have been obtained using an ordinary alcohol-in-glass thermometer. It is quite possible that the oil-in-glass instrument was designed to extend the measurable range of temperatures upwards, but even these in-

[17] BOAS, MARIE & A. RUPERT HALL: "Newton's Chemical Experiments". Archives Internationales d'Histoire des Sciences, II, 1958, 120, 147, etc. BREWSTER, 362.

struments ultimately prove to be unsatisfactory for use in chemical experimentation.

The thermoscope or thermometer during the seventeenth century suffered much the same fate as the microscope. In many respects they both remained curiosities; in the case of the former, because the scientists of the day had no theory to which thermometric indications were applicable and therefore hardly knew what to do with them. This stands in sharp contrast with the relatively widespread and effective use of the telescope. Thermometers were used almost exclusively for meteorological or medical purposes. This can be seen in the work of HOOKE and others within the Royal Society. NEWTON was the first, or among the first, to try to overcome the limitations of thermometry which restricted it to meteorology or medicine and to develop thermometers capable of investigating a wide range of thermal phenomena in the laboratory. This is not to indicate a lack of problems in the construction or use of meteorological thermometers; far from it. It is rather to insist that in NEWTON's day there was hardly a thermometer which could be called a laboratory or an industrial device. To provide such a device for his chemical studies, NEWTON not only had to concern himself with such problems as standardization, portability, durability, sluggishness, and changing air pressure in the case of the air thermometer, as were common in meteorological thermometers; he also had to deal with the problem of extending their range upward. If it had been as easy to obtain low temperatures as high ones and if low temperatures had been of chemical interest, NEWTON might have concerned himself with extending the range of thermometers downward. As it was, he was concerned with the measurement of high temperatures, and he set out to develop in careful experimental detail an entirely new concept in thermometry, the measurement of temperature from noting the time of cooling of a heated iron slab.

DAVID GREGORY visited NEWTON at Cambridge in May 1694 and in the course of several days wrote several memoranda. Two entries are relevant to the "law of cooling":

> 32. The author thinks that glowing iron is of the same heat as coals in a kitchen fire. 33. He has judged of the degree of heat from the time taken to cool, and from a thermometer filled with oil which better resists intense heat[18].

There is nothing revealed here which is not found in the published paper except for the phrase "... oil which better resists intense heat". The implied contrast must be between oil and alcohol, and the phrase means, at the least, that oil is useful as a thermometric substance to a higher degree of heat than is alcohol. Whether or not the phrase also implies that problems also occur with oil at high temperatures is not clear. BLACK[19] lists the boiling point of "oleum lini" as 600° F which, assuming uniform expansion, is theoretically equivalent to about 100° on the 1701 scale. Its flash point also should be near this temperature. Moreover, a glass thermometer in a coal fire would soften and change shape. Thus it would seem clear that NEWTON could not have used an oil-in-glass thermometer to measure directly the temperatures, such as that of a coal fire, found

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18 TURNBULL, III, p. 317.

19 BLACK, JOSEPH: Lectures in the Elements of Chemistry edit. by JOHN ROBISON. Edinburgh 1803.

at the top of the 1701 scale. Yet there is a curious note by GREGORY related to the statements quoted above:

The author's method of establishing the different degrees of heat is of this kind: a thermometer with linseed- or olive-oil in it, of rather thick glass, and in fact open except that it is stopped with cotton to prevent the oil from flowing out, he plunges into the same fire with iron, and he likewise plunges it into boiling water, and into dry earth heated by the Sun. But in registering his experiments he describes the degree of heat in relation to the melting of the metals, lead, tin, silver, bismuth, resin, wax, etc.[20].

This is an incredible statement and is not in accord with NEWTON'S own statements about his technique of measuring the temperatures near the top of his scale. Further, it does not seem possible that NEWTON could have obtained any reading that was at all repeatable by sticking an oil-in-glass thermometer directly into a coal fire hot enough to make iron glow. We can but suppose that GREGORY misunderstood NEWTON'S account of his procedure, or that in some experiments — preliminary ones perhaps — the oil-thermometer was "plunged into the fire with the iron" but allowed to attain much less than a glowing red heat.

In the 1701 paper NEWTON makes no mention of the use of an oil thermometer at temperatures above the fusion point of tin (232° C) which he places at 72°. His investigation of the higher temperatures he describes thus:

Having discovered these things; in order to investigate the rest, there was heated a pretty thick piece of iron red-hot, which was taken out of the fire with a pair of pincers, which was also red-hot, and laid in a cold place, where the wind blew continually upon it, and putting on it particles of several metals, and other fusible bodies, the time of its cooling was marked, till all the particles were hardened, and the heat of the iron was equal to the heat of the human body; then supposing that the excess of the degrees of the heat of the iron, and the particles above the heat of the atmosphere, found by the thermometer, were in geometrical progression when the times are in an arithmetical progression, the several degrees of heat were discovered ...[21].

The principle that he used in this method is what is now called "NEWTON'S Law of Cooling". NEWTON did not state it in the mathematical form of his fluxions, but, using modern notation, we can see how the conclusion about the degrees of heat retained standing in geometrical progression for equal time intervals follows from the principle that "the heat which the iron loses in a certain time, is as the whole heat of the iron".

$$\frac{dh}{dt} = -ch,$$

$$\int_{h_0}^{h_n} \frac{dh}{h} = \int_{t=0}^{t=n} -c\,dt,$$

$$\ln h_n - \ln h_0 = -cn$$

evaluated at $n=1$,

$$-c = \ln \frac{n_1}{n_0} \doteq \ln r,$$

$$\ln h_n = \ln h_0 + n \ln r,$$

$$h_n = h_0 r^n.$$

[20] TURNBULL, III, p. 322.

[21] COHEN, p. 268.

NEWTON was not altogether consistent, because at one point he spoke of the "whole heat of the iron"[22], while at another he spoke of "the excess of the degrees of heat ... above the heat of the atmosphere"[23]. In fact, the "whole heat of the iron" is never taken into account, and it is only the excess of temperature above the environment that is important. But this is a minor point.

Let us now attempt a reconstruction of NEWTON's procedure, from which results closely agreeing with his are obtainable. In calibrating his "slab" thermometer, NEWTON worked, as it were, backwards. He defined 12 degrees of heat as the interval between the freezing point of water and "the external heat of the body in its natural state", in effect defining one degree as $\frac{1}{12}$ of this interval. On the assumption that oil-in-glass expands uniformly he graduated a thermometer to a degree high enough to permit the convenient determination of the proportionality constant for the cooling of his "pretty thick piece of iron". In order to calibrate his iron slab it was heated to the degree of redness obtainable from the "degree of heat of live coals in a small kitchen fire, made up of bituminous pit coals, and that burn without using bellows"[24]. Then taking the red-hot slab from the coals he placed various metals on it; these, of course, immediately melted. Now suppose that he observed that the molten lead hardened after the slab had been cooling for slightly more than 15 minutes, that a 50—50 mixture of tin and bismuth set after about 30 minutes, and that the slab finally reached body temperature after about 60 minutes. The problem is to find the original temperature of the slab and its cooling constant for some particular environmental temperature. (The temperature of the environment does not effect the value of the original temperature, or any other temperature of the slab but only the rate at which the temperature changes.) With this information the temperatures of other "high temperature" phenomena can be calculated, which, once established, can serve as secondary standards by which to estimate temperatures in general. Body heat was fixed at 12°, and using the oil thermometer, the temperature at which the 50—50 tin and bismuth mixture sets was found to be 48°. With these predetermined points and a record of the time at which the particular phenomena occurred their corresponding temperatures can be calculated. The reconstructed method and results are presented in Table 2.

There is no necessary conflict between this procedure which we have reconstructed from the published account and that noted by GREGORY in 1694. One might even speculate that NEWTON was using an admittedly crude method in 1694 before he "discovered" the cooling law which enabled him to calculate temperatures exactly. This is essentially MORE's view, one which is, however, only partially correct. The true situation probably is that at some time between 1694 and 1701 NEWTON performed a series of careful measurements on time of cooling, *etc.*, and, using the cooling law, constructed the scale. The law was neither discovered nor conceived during this period, although it was brought to full articulation and development and very possibly underwent a change of logical status. From being a conjecture, it became a fundamental principle.

[22] COHEN, p. 267.
[23] COHEN, p. 268.
[24] COHEN, p. 267.

Table 2. *Examples of calculations based on* NEWTON's *law of cooling*

Time	Phenomena	Degrees of heat in geometrical progression	Logarithms of degrees	Degrees	Comments
1) 60 min	body heat	$h_0\, r^6$	$\ln h_0 + 6 \ln r$	12° N	Known from linseed oil thermometer
2) 30 min	setting of 50—50 tin-bismuth mixture	$h_0\, r^3$	$\ln h_0 + 3 \ln r$	48° N	
3) 15 min	setting of lead	$h_0\, r^{1.5}$	$\ln h_0 + 1.5 \ln r$	$X = 96°$ N	to be determined
4) start	red-hot iron	$h_0\, r^0$	$\ln h_0$	$h_0 = 192°$ N	

From 1) and 2) we get:

$$\ln h_0 + 6 \ln r = \ln 12$$
$$\ln h_0 + 3 \ln r = \ln 48$$

Solving for $\ln h_0$ we get:

$$\ln h_0 = 2 \ln 48 - \ln 12$$
$$\ln h_0 = 5.2575$$
$$h_0 = 192° \text{ N}$$

Solving for $\ln r$ we get:

$$-3 \ln r = \ln 48 - \ln 12$$
$$-\ln r = 0.4621$$
$$(r = 0.63)$$

From 3) we get:

$$\ln h_0 + 1.5 \ln r = \ln X$$
$$\ln X = 4.5643$$
$$X = 96° \text{ N}$$

To see this, consider the difference between a statement in the *Principia* (1687) and one in the paper of 1701. In the latter we find that red-hot iron has a temperature (value) almost exactly six times that of boiling water. Iron barely glowing under different conditions of illumination has temperature (values) of from about 3.5 to 5 times that of boiling water. However, in the *Principia* we find that the "... heat of red-hot iron (if my conjecture is right) is about three or four times that of boiling water"[25]. If NEWTON had been able to determine the temperature of red-hot iron even roughly by sticking an oil-in-glass thermometer directly into a fire with the iron he would hardly have written "si recte conjector" in his qualifying phrase. It is possible that NEWTON made a lucky guess about the temperature of red-hot iron, but it is more likely that he had conceived of the method sketched in the 1701 paper and had made some rough determinations with it. Later, after 1693 or 1694, he perfected the technique for accurate determinations. Presumably before that time he possessed no ordinary thermometer calibrated accurately to a sufficiently high temperature to permit the measurement of the cooling slab over a substantial range of known temperatures. It is also possible that it was not until he had completed a number of experiments that he became convinced of the truth of the principle of the degradation of heat underlying his method.

The significance of the experimental work after 1692/93 is that it provided NEWTON with the data necessary to construct a thermometer graduated accurately to near the melting point of tin or lead. This interpretation makes the most sense out of the form of the notebook entry of March 1692/93. A series of phenomena occurring at temperatures up to the melting-point of lead were listed in the notebook, the intention being to determine their temperatures with an oil filled thermometer. Possibly early in the effort NEWTON decided that a scale change would be advantageous so that body temperature could be used as a

[25] CAJORI edition, p. 521.

second fixed point having an integral number value. It is also possible that the work was laid aside briefly. Whatever diverted NEWTON from his original plans, it appears that when he picked up the work again he started a new set of notes which have since been lost. It seems clear that as the work was carried to completion it was guided by the "law of cooling", and it was not the case that the law grew out of the experimental findings.

We have seen that red hot iron was found to have a temperature of 192°. On the basis of 1° N = 3° C this is equivalent to a temperature of about 600° C, while the correct value would be about 800° C. Similarly, the degree at which heated bodies barely glow in the dark was set at 114°. This is theoretically equivalent to about 350° C, while the correct value lies between 450 and 475° C. Lead was found to solidify at 96° which yields a theoretical value of about 290° C against a correct value of 327° C. The error in NEWTON'S method, as is well known, becomes progressively greater with increasing temperatures because radiative heat losses increase with the fourth power of the (absolute) temperature and the convective heat losses which do follow NEWTON'S law, at least approximately, become relatively smaller at high temperatures. Of course, NEWTON could not have known this and had no reason to expect a more complicated law than he adopted. Let us now examine some grounds on which the cooling law could have been justified.

It has been shown that NEWTON used his linseed oil thermometer to establish the known degrees of heat from which the slab thermometer was calibrated. The procedure was then turned around, as it were, and the results obtained from the iron slab thermometer used to establish the validity of the uniform graduation of the oil thermometer. NEWTON writes: "the several degrees of heat thus found [by the slab thermometer and the cooling law] had the same ratio among themselves with those found by the [linseed oil] thermometer: and therefore the rarefactions of the oil were properly assumed proportional to its degree of heat"[26]. From this statement we can see that he intends, ultimately, to define a scale of heat by means of his cooling law. He seems to be saying something to the following effect: it is possible that I have wrongly assumed that the expansion of linseed oil is uniform in equal degrees of heat. Thus, for example, the value assigned to the melting of the 50—50 tin-bismuth mixture by the oil thermometer (48°) will be too high or too low depending on whether the coefficient of expansion of linseed oil decreases or increases with higher degrees of heat. The same would be true in other degrees of heat up to the limit of the oil thermometer (about 72°). However, I find that assuming the true temperature of the melting tin-bismuth mixture to be 48°, or what is the same thing, by assuming the expansion of the oil to be uniform, I can determine the proportionality constant of my "law" of cooling. Then using this information, I find exact enough agreement between temperatures calculated by the law and those measured by the oil thermometer, in the overlapping range where both techniques apply. If the expansion of the oil were not uniform, you would either find discord between the two methods or you would have to conclude that the degrees found by the cooling law vary in exactly the same way as the variation in the expansibility

[26] COHEN, p. 268.

of the oil. The former does not happen, and the latter is counter-intuitive. In some circumstances, one might expect the length of the marking of a true degree on the stem of a thermometer to vary, but one should not expect the size of a degree calculated from a law of nature to admit of variation, because this is counter to the very idea of a degree of heat. This argument can be made clearer by an analogy. Consider how the straightness of two boards picked at random out of a pile can be checked. If one is placed on top of the other and it is found that they touch at all points where they overlap, it is more reasonable to assume that they are both straight rather than they both have exactly the same curvature.

There seem to be two basic ways in which NEWTON could, if challenged, have justified his law of cooling and the propriety of its definition of temperature. He might have claimed that it is too improbable to suppose that the cooling of an iron slab would vary in exactly the same way as the contraction of linseed oil. Thus it is probable that the expansion or contraction of linseed oil is uniform and that the law of cooling properly describes the phenomena. Put in this way, the law of cooling is acceptable until discrepancies show up between the two methods (when another substance is substituted for the linseed oil, for example) or until experimentation becomes sufficiently accurate and sophisticated to demonstrate that the calculations based on the cooling of the slab diverge from the indications of the ordinary thermometer. However, should a disagreement between the two methods become apparent, only further study could decide which principle should be scrapped: the uniformity of the expansion of the thermometric substance or the law of cooling. This is essentially what happened when the law of cooling was critized in the middle of the eighteenth century. Discrepancies in cooling rates were noticed, increasing with higher temperature, when temperatures measured by Fahrenheit thermometers were compared with those calculated from the cooling law. Logically speaking, the decision in favor of the Fahrenheit scale was premature. With growing awareness of the phenomena of specific heat, to say nothing of problems involving the expansion of glass, the decision between temperature as defined by the Fahrenheit thermometer and temperature defined by Newton's law of cooling was not a simple one to make. The *a priori* nature of the cooling law apparently prejudiced the eighteenth-century empiricists against it, and DALTON was one of the few who recognized that the law could be taken as a defining principle rather than as a straightforwardly falsifiable generalization. If a body were heated to 1000° above the environment, he wrote, according to NEWTON's law

> the times in cooling from 1000° to 100, from 100 to 10, and from 10 to 1, ought all to be the same. This, though nearly, is not accurately true if we adopt the common [Fahrenheit] scale, as is well known; the times in the lower intervals of temperature are found longer than in the upper; but the new [Dalton] scale proposed, by shortening the [length of the markings of the] lower degrees and lengthening the higher, is found perfectly according to this [Newton's] remarkable law of heat[27].

The second line of defense of Newton's law might consist in appealing to the principle of the simplicity of nature. We cannot push this line too far, nor the one

[27] DALTON, JOHN: A New System of Chemical Philosophy, Part I, p. 12. Manchester 1808.

sketched above for that matter, because there is no explicit evidence in NEWTON's notes, article, or queries which help us to understand the logical status of the law. Nevertheless, one can imagine that his thoughts might have run something as follows. Heat is motion of some sort, and whenever there is more of it in a body than in its environment, some of it will be transferred to the environment. The greater the difference in the degree or intensity of heat, the more rapidly it will be transferred. In this regard consider the statement in Query 18; "and do not hot Bodies communicate their Heat to contiguous cold ones, by the Vibrations of this Medium [the Aether] propagated from them into the cold ones"[28]? However, it is not obvious that the rate of transfer should be uniformly proportional to the temperature difference. For example, the rate might depend on some higher power of the temperature or even some more complicated function which would yield a "constant" that differs in different temperatures ranges. (Both of these moves were made at one time or another in endeavors to patch up the law.) However, from both a methodological and a metaphysical point of view, unless there are independent experimental or intra-theoretical reasons to accept a more complicated relationship, the simplest first-order equation should be accepted until such time as either experimental evidence or intra-theoretical considerations seem to require a change. The logarithmic notion that underlies the cooling law may not be the simplest relationship available from mathematics, but it was a common one that found (and in its exponential form still finds) widespread use in physical problems. For example, Proposition II, Therorem II, Book II of the *Principia* deals with the degradation of velocity in a resisting medium and is suggestive of the later proposition or law dealing with the degradation of heat:

> If a body is resisted in the ratio of its velocity, and moves, by its inertia only, through an homogeneous medium, and the times be taken as equal, the velocities in the beginning of each of the times are as the velocities[29].

A parapharase by COTES, although written much later, is helpful in seeing the physical analogy that could underlie the two notions. "Bodies in going through a fluid communicate their motion to the ambient fluid little by little, and by that communication lose their own motion, and by losing it are retarded. Therefore the retardation is proportional to the motion communicated." If it is assumed that "the motion of bodies are resisted in the ratio of the velocity", the geometrical law follows[30]. This talk about bodies in motion, *etc.* can easily be changed to talk about thermal motion being communicated to the environment, with the result that cooling follows the geometrical progression actually adopted.

One does not have to conclude that NEWTON did, in fact, follow the analogical reasoning given above. Perhaps no analogy at all was entertained. The point here is that geometrical progressions are relatively simple and were widely used by NEWTON and his contemporaries. When NEWTON was confronted with the problem of measuring high temperatures, he hit upon a conjectural method that apparently had, from the very beginning, a commonplace mathematical

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[28] NEWTON, ISAAC: Opticks (Dover reprint edition), p. 349.
[29] CAJORI edition, p. 236.
[30] CAJORI edition, p. XXXI.

basis. If the analysis given above is correct, the conjecture which was conceived before 1687 was elevated to a basic principle in the 1690's as the result of careful experimentation and measurement. Newton's Law of Cooling has a far richer conceptual basis than is usually taken to be the case[31].

[31] The writer first treated this subject in Professor A. R. HALL's seminar. He wishes to acknowledge the helpful criticism of Professor HALL while developing the paper in its present form.

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(Received January 21, 1963)