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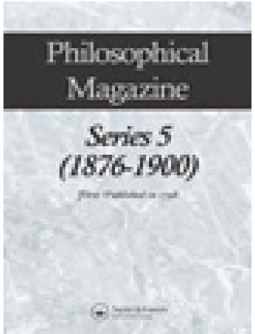

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VI. Notes on thermometry

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VI. *Notes on Thermometry.* *By* F. D. Brown, *B.Sc.*, *Demonstrator of Chemistry at the University Museum, Oxford**.

[Plate II.]

SOME years ago, when I determined to try and find out something about the attractive forces which the atoms and molecules seem to possess, by studying the effects of heat upon chemical substances and upon mixtures of such substances, I was led to the conviction that, if the work which I proposed to do was to be of any permanent use, I should be obliged to take many and minute precautions regarding the measurement of temperatures—a measurement which, owing to the peculiarities of mercurial and other thermometers, is so liable to error. In order to learn how best to use my thermometers, and how to refer their readings to a satisfactory standard, I made a considerable number of experiments. At the time when these experiments were made I imagined that the subject of thermometry, although presenting many difficulties to my mind, had been thoroughly worked out by others, and therefore that a printed record of my observations would be generally deemed to be of little utility. The recent publication of a paper by Dr. E. J. Mills (Edin. Roy. Soc. Trans. 1880), of one by Professors T. E. Thorpe and Rücker (Phil. Mag. [5] xii. p. 1), and more especially of a report by M. Pernet (*Mém. et Travaux du Bur. inter. des poids et mes.* i. 1881, pp. 1–52), has led me to change my opinion, and to think that there still remain many points connected with thermometers about which not only I, but others also, would be glad to have more certain information. Acting upon this belief, I have put together in the following pages some of the results of my experiments.

The Mercurial Thermometer as a Standard.

I was soon convinced that any attempt to express temperatures in degrees of an ideal absolute thermometer, or even to refer them correctly to the readings of an air-thermometer, would involve a most extensive and wearisome investigation, which would postpone indefinitely the work I wished to do. To avoid this substitution of the means for the end, I decided to construct a mercurial thermometer and to use it as a standard, keeping it until such time as the progress of our knowledge should render its comparison with the air-thermometer a matter of less difficulty.

As a mercurial thermometer is very liable to be broken, I first wanted to know whether this instrument fulfilled the primary condition of a true standard, of being capable of

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* Communicated by the Physical Society.

reproduction when lost or destroyed. With this end in view, I made two thermometers at different times, and wholly independently one of the other, and compared their readings. To those who may wish at any time to construct a mercurial thermometer without the elaborate appliances ordinarily employed, but in which absolute confidence may be placed, the following details may be of interest:—

A capillary tube of medium bore, about 800 millimetres long, free from all flaws, and having as uniform a section as possible, is provided with a millimetre-scale of 600 divisions. The etching of this scale is a matter of great consequence: it very frequently happens that the divisions on glass tubes are not of exactly equal length, but that, owing to some defect in the dividing-engine or some movement of the tube while undergoing the process of division, some of the divisions are so much longer or shorter than the rest as seriously to interfere with the subsequent process of calibration. Even when all the lines are equidistant, they are often so thick, and present so irregular an outline when viewed through a telescope, that it is impossible to fix upon any particular point as that represented by the dividing-line. The tubes I employed were selected and divided with special care by Mr. Casella, the lines being perfectly straight, less than 0·4 millim. in thickness, and in all cases equidistant.

As a glass tube, however carefully selected, is never of uniform bore, it is necessary to ascertain the relative capacities of the several divisions of the tube, or, in other words, to "calibrate" it. As is well known, this is easily done by placing a thread of mercury in successive positions along the tube and observing its length, the mean capacity of the divisions occupied by the thread being, of course, inversely proportional to that length. In this way, and by adopting the plan of correcting the position of the thread suggested by Dr. Mills in the paper above referred to, which plan he had been kind enough previously to communicate to me privately, a table is readily constructed showing the volume of the tube from the line marked 0 to any line marked n, and also the value of the succeeding division. The only difficulty connected with this process is the accurate measurement of the length of the thread of mercury in its several positions. It is true that this may easily be done with a dividing-engine or some similar instrument, such as a cathetometer provided with a micrometer eyepiece and placed horizontally. As, however, reliable instruments of this class are exceedingly costly, I designed a small piece of apparatus for the purpose, which has proved so convenient and useful that I venture to describe it here.

A mahogany board, B B (Pl. II. fig. 1), about 18 inches long and 4 inches wide, is provided with a groove, G G, of the shape shown in the section (fig. 1 *a*) ; a piece of gun-metal, about 5 inches long and $\frac{1}{4}$ inch thick, slides in this groove with some little difficulty—the friction, which is produced by the spring *f f*, being necessary to retain the plate rigidly in any given position. The plate, D, is provided with a slot, *e e*, and a millimetre-scale, S S, the dividing lines of which must, like those of the tube to be calibrated, be very fine and truly equidistant. The piece of gun-metal, E, which is provided with a vernier, carries the reading-microscope, M, and can be moved along S S by means of the rack and pinion *p* ; the movement is rendered smooth and free from lateral displacement by the spring *c*, which causes the ends of E to remain always in contact with the straight edge of the slot. The thermometer-tube is fixed with suitable screws under the path of the microscope, so that the length of a thread of mercury can be easily measured by placing the microscope so that its cross wire coincides first with one end of the thread and then with the other, and noting on the scale the distance between the two positions.

The millimetres of the brass scale and those of the tube, if marked off by different makers, will often differ a little in length ; hence it is generally more satisfactory to obtain from the glass scale the number of whole divisions occupied by the thread, and to measure the terminal fractions only by the microscope.

Since the line on the outside of the tube is nearer the eye than the thread of mercury inside the tube, it is clear that when the microscope is adjusted to view the end of the thread, and is then moved along until the cross wire coincides with the nearest line, this last will be out of focus, and either the whole microscope must be raised up or the distance between the object-glass and eyepiece altered. Now, unless the instrument be constructed with great solidity, and much care be taken to fit accurately all the moving parts, this adjustment will probably alter the position of the optical axis, and so render the measurements inaccurate. To avoid this difficulty, I added a half-lens, L, fitted in the ordinary way on a brass tube sliding on the end of the microscope. This lens of course brings the focus of half the field nearer the object-glass ; so that, by properly adjusting it, the divisions are seen through the half-lens at the same time that the mercury is observed through the unprotected part of the object-glass. In this way all disturbance of the microscope is avoided throughout the calibration, which is thus carried out with much greater comfort and accuracy.

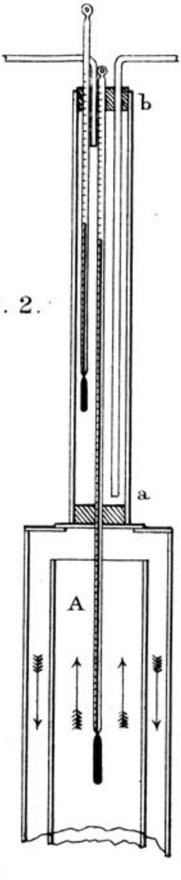

Fig. 2.

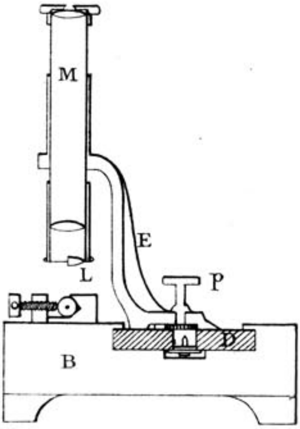

Fig. 1a.

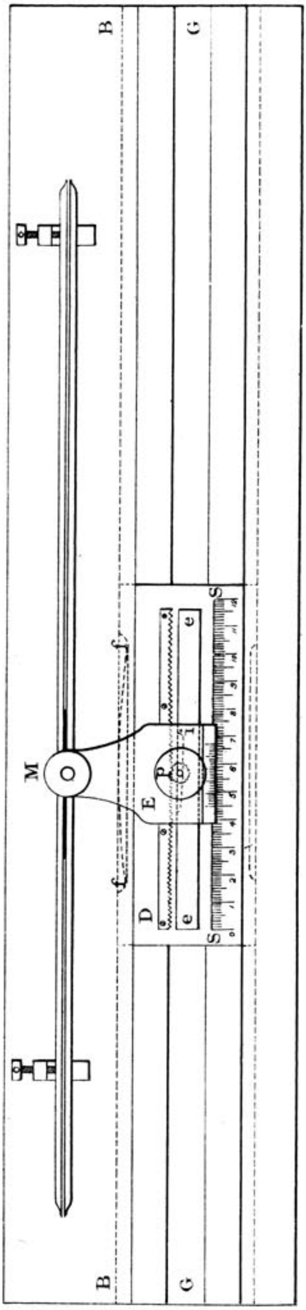

Fig. 1.

Mintern, Bros. lith.

Two tubes were calibrated with this apparatus, and tables of their volumes from the first division compiled; they were then furnished with bulbs, filled with mercury, and sealed up in such a manner that they formed thermometers capable of indicating temperatures between 0° and 150° C. The fixed points of the two thermometers having been determined with the precautions indicated below, tables showing the temperatures corresponding to the readings of the scale were made in the usual manner; the two instruments were then compared together, either in a large tank of water which was kept well stirred, or in the steam-apparatus which I described to the Physical Society at the time when these experiments were made. Before a series of readings were taken, both thermometers were heated for at least half an hour in steam, while their zero-points were observed after the series was completed. The numbers given in the following table show that the two thermometers gave practically identical readings. It would seem, therefore, that the mercurial thermometer, when carefully made and systematically heated, does really possess that valuable property of a standard, of being capable of exact reproduction.

Reading of AS, corrected for index-error.	Reading of BS, corrected for index-error.	Corresponding value of AS, in degrees.	Corresponding value of BS, in degrees.	Difference.
58·55	70·64	14·30	14·29	−·01
134·33	150·11	33·69	33·71	+·02
179·69	197·20	45·29	45·30	+·01
321·97	345·96	81·88	81·88	·00
23·42	33·74	5·28	5·28	·00
26·33	36·83	6·03	6·04	+·01
30·07	40·63	6·99	6·97	−·02
33·76	44·57	7·94	7·93	−·01
43·32	54·70	10·40	10·40	·00
47·98	59·60	11·59	11·59	·00
69·42	82·13	17·09	17·09	·00
91·61	105·52	22·78	22·79	+·01

Determination of the Zero-point.

In most books on physics it is stated that, in order to obtain the zero-point of a thermometer, the instrument should be placed in a vessel filled with broken ice and provided with holes at the bottom, through which the water formed by the melting of the ice may escape. In order to learn whether this method is the best possible, the following experiments were made:—A number of tin pots, about 7 inches high and 4 inches in diameter, were obtained, and holes made in the bottoms of two or three of them. A large block of ice was broken up into small fragments, which were well mixed up, so as to render the whole perfectly uniform in character. One of the tin pots, which we will call A, was filled with some of this ice, which had been washed in a funnel with ordinary water; A was then filled up with water, so as to form a mixture in which the ice largely predominated. A second tin, B, was filled with some more of the ice, which had been washed with ordinary water in the same way; B, however, had holes at the bottom, and the water formed by the fusion of the ice thus drained away. A third tin, C, contained some of the same ice, which had been washed in a funnel with distilled water, and then mixed with distilled water in the same way as in A the ice was mixed with ordinary water. In a fourth tin, D, which was provided with holes, some ice was placed which had been washed with distilled water. Finally a quantity of distilled water was artificially frozen, the ice broken up into small pieces, washed, and mixed with distilled water in a fifth tin, E. A thermometer with a long narrow bulb, and with a stem divided into millimetres, was carefully inserted into each tin in succession, and readings taken with a cathetometer. About 17 millim. of the scale were equivalent to one degree Centigrade. In A the readings soon became constant at 1°·00; in B the readings varied considerably for about half an hour, but finally became constant at 1°·12; in C the thermometer became rapidly constant at 1°·16; in D the readings became constant after a short time at 1°·06; in E the readings did not vary after the first four or five minutes, remaining at 0°·64.

At the end of these observations, which occupied nearly two hours, the thermometer was replaced in A, where the mercury rapidly assumed the same position as before, viz. 1°·00. Seeing that, with the exception of E, the greatest difference in the readings does not amount to 0°·01 C., we may fairly draw the following conclusions:—First, that a constant temperature is more rapidly and certainly obtained with a mixture of ice and water than with ice alone; secondly, that the temperature thus obtained is really that of melting ice; thirdly, that it is preferable to wash and mix the ice with distilled water, ordinary water tending to lower the temperature, though to an insignificant extent.

With the view of seeing whether different varieties of ice gave the same results, two specimens of block ice and one of the rough thin ice collected in winter near London were obtained, while two cylinders of distilled-water ice were artificially produced. These were all broken up separately into small pieces, washed with distilled water, and then mixed with the same in five tins, A, B, C, D, E. The thermometer placed in these tins marked 1°·30, 1°·34, 1°·26, 1°·30, and 1°·27 respectively (these numbers are not comparable with the former, as the experiments were made a month or so later, when the zero of the thermometer had altered its position). These experiments showed that distilled-water ice gave the same results as ordinary ice, and that the melting-point of different specimens of ice, when mixed with distilled water, was the same within 0°·005 C. The exceptionally low reading obtained with the tin E in the first series of experiments was probably due to the fact that the ice, having been made by means of a freezing-mixture, was not at its maximum temperature.

In subsequent determinations of the zero of thermometers I have always used ordinary block ice, washed and mixed with sufficient distilled water just to fill up the spaces between the pieces, and have not allowed the water to drain away. These results are in accord with those obtained by M. Pernet.

Zero-movements, and Substitution of the Determination of the Steam-point for that of the Zero-point.

In considering the well-worn question of the zero-movements of thermometers, it is important to distinguish between its practical and theoretical aspects. To make a study of zero-movements from an abstract point of view, to find out equations expressing these movements under different circumstances and with different thermometers, to learn that when a certain thermometer has been subjected to a certain series of temperatures at certain intervals of time its indications on next changing its temperature will be affected with a certain index-error, may possibly be of some utility, but it does not aid us much in the endeavour to free the readings of thermometers from the errors with which they are surrounded. When once we have acquired the information that a thermometer subjected only to those changes of temperature which are due to the weather exhibits a gradual rise of zero, that the rise thus taking place in a given time diminishes as the age of the thermometer increases, but differs for different thermometers, when we also know that a thermometer subjected to a high temperature after a considerable period of rest exhibits a decrease in its zero-reading, dependent on the thermometer itself and also on its previous history,—we know all, or nearly all, that we can put to practical use.

Thus, for example, the thermometer attached to my standard barometer was verified at Kew Observatory when it was first supplied to me, some four or five years ago. Since then I have from time to time observed its reading in melting ice, and have modified accordingly the correction to be applied to it. Now, no observations of other thermometers—no curves or equations representing their zero-movements—could be of any assistance to me in this matter. I knew that the zero would probably rise, and that the amount of the rise would not be the same in my case as in that of others, and that, therefore, I must obtain the index-error experimentally. I also knew that if I boiled the thermometer I should cause irregular changes in the position of the zero; and as there was no necessity for the operation, I avoided boiling it. But if by mischance it had fallen in boiling water, no equations representing the zero-movements of other thermometers would have told me exactly what had happened to mine ; I should simply have been obliged to observe its index-error more frequently than before the accident happened.

The question which seems to me to be of the greatest importance with regard to zero-movements is, how we can best reduce the trouble which they cause us. In the case of all meteorological and clinical thermometers, where the changes of temperature are small, as in the above case, it is evident that all we can or need do is to protect the instrument from unnecessary changes of temperature. When, on the contrary, our observations extend over wide ranges of temperature, the difficulties increase considerably. Suppose, for example, that I want to use a thermometer to indicate accurately a series of temperatures between 70° and 90°. It is obvious that if I observe the index-error beforehand, and apply the correction thus obtained to my readings, I shall not be doing right; for the very heating of the thermometer to 70°–90° will have altered the index-error. But if, on the other hand, I first heat the thermometer to 100°, then ascertain its index-error, then make my experiments with it, and finally observe its reading in ice a second time, I shall be tolerably certain, if the index-error is the same at the end as at the beginning of the experiment, that no variation has occurred during the observations.

In most laboratories, however, the frequent determination of the zero-point of a thermometer involves a considerable expenditure of labour : ice has to be purchased, broken up into small pieces, washed, and placed in a suitable vessel. All this requires no little time, and has, moreover, to be repeated at every determination, since the broken ice melts away in the interval. On the other hand, the apparatus for the observation of the steam-point is always in readiness; if, therefore, no greater error arises when the index-error is determined before and after the experiments by means of the steam-point, a great saving of time will be effected, without any corresponding loss of accuracy.

When the temperatures to which the thermometer is to be exposed are greater than 100°, the instrument should be heated for some time to the highest probable temperature before the steam-point is observed for the first time. In this way the lowering of the zero which takes place when a thermometer is heated from 100° to some higher temperature, to which it has not been exposed for some time previously, is effected first of all, and does not take place during the experiments, as it otherwise would.

The only objection which can be raised to this method is that, when some at least of the temperatures to be measured are below 100°, it is possible that the steam-point, which is lowered by the first heating in steam, rises again during the experiments (that is, when the thermometer is at a lower temperature), and then, by the second heating in steam, is again brought to the same position as at first. In this way the observations in steam, although concordant, would not give the true index-correction to be applied to the readings. That the error which thus arises is of no importance is, I think, rendered probable by the following considerations:—The gradual rise of the zero of a thermometer receives its most natural explanation when it is supposed that the glass bulb, after having been heated and somewhat quickly cooled, is in a state of strain which causes it to have a larger capacity than it would have if no such strain existed. As time goes on, and more especially as the thermometer is subjected to small fluctuations of temperature, the particles of the glass gradually yield to the forces which are acting upon them, and take up new and more suitable positions. These molecular movements result in a gradual diminution of the capacity of the bulb, and consequently in a rise of the zero. Now it is evident that, if a certain state of strain is set up when a thermometer is cooled from 100° to 0°, when it is cooled from 100° to some intermediate temperature t the strain set up will be less considerable; there will therefore be a greater tendency for the zero to rise when the thermometer is placed in melting ice than when it is subjected to the temperature t. Consequently, if it be found that, when a thermometer after being heated in steam is placed in ice, no change of the zero takes place for three or four hours afterwards, we may legitimately conclude that, if the thermometer were maintained for the same time at the temperature t, no movement of the zero would occur. I have frequently kept recently-heated thermometers in melting ice for several hours, renewing the ice when necessary; and I have always observed, with all of my instruments, that no change took place for the first three hours, and that during the next two or three hours the rise was extremely small. It follows, therefore, that if in any series of observations lasting more than three hours the thermometer be heated in steam at the end of every third hour, there will be no uncertainty as to the position of the zero; that if the experiments be carried on continuously for six hours, a slight rise of the zero may occur during the last part of the time, but that this rise will not amount to more than one or two hundredths of a degree.

Correction for the Exposed Portion of the Thread.

When a thermometer is only partially immersed in the medium of which the temperature is to be observed, the readings become subject to an error which arises from the fact that a part of the thread of mercury, together with the corresponding portion of the stem, are at a temperature different from that of the bulb and immersed portion of the stem. The correction, C, usually applied in this case is given by the formula

$$C = m(T - t)N, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

where T = the reading of the thermometer,
t = the temperature of the exposed portion,
N = the number of exposed divisions of the stem which are filled with mercury,
m = the apparent expansion of mercury in glass.

This formula is founded on the assumption that the error in the reading has no other cause than the comparatively unexpanded condition of a portion of the thread and stem.

The apparent expansion of mercury in glass, as obtained from Regnault's experiments, is about ·0001545; but it differs, of course, for different specimens of glass. When this number is employed in the above formula, the values of C obtained are generally believed to be too large; indeed a little reflection will convince us that this must be the case whenever the temperature of the exposed portion is merely measured by placing another thermometer with its bulb halfway up it. This second thermometer evidently measures the temperature of the ascending stream of warm air around the stem; if the stem of the chief thermometer were subjected to the heating influence of this stream, and to no other, its temperature would be rightly given by the subsidiary thermometer; but the thermal conduction along the thread of mercury and along the glass stem must necessarily raise the lower part of the exposed stem to a temperature higher than that indicated by the subsidiary thermometer. The value of $(T-t)$ therefore is too great, and consequently also that of C.

In order to meet this difficulty, Dr. Mills, instead of endeavouring to give to $(T-t)$ its proper value, has made a large number of experiments with different thermometers with a view to assign a more satisfactory value to m, and has thus been led to draw the following conclusions:—The value ·0001545 of the coefficient m is invariably too great. This coefficient varies with the thermometer employed, and also with the number of divisions of the thread exposed; so that, instead of assigning one definite value to m for each thermometer, we must give it a value

$$m=a+\beta N,$$

where a and β must be determined for each thermometer.

Professors Thorpe and Rücker, on the other hand, while admitting that the value $m=\cdot0001545$ may be generally too large, maintain that it is sufficient to replace it by some other single number, and that the employment of the varying coefficients $a+\beta N$ is unnecessary; they support this opinion by showing that in Dr. Mills's own experiments the alterations in the value of C, caused by the introduction of the term βN, do not amount to more than one or two hundredths of a degree, and are therefore insignificant. Dr. Mills, replying to this, states that the change in the correction C brought about by the term βN often amounts to so many hundredths of a degree that it cannot be neglected.

Now it is clear that by merely placing a second thermometer halfway up the exposed thread, only the roughest idea is obtained of the real temperature of the thread. Suppose, for example, that $T=100°$, and that t is taken at 15°, being subject to an error of 5°: the value of $(T-t)$, which is 85, will be subject to an error of 5°, or about 6 per cent. What, therefore, can be the use of attempting to determine the coefficient β, of which the value would appear ordinarily to be about 0·0000002, when so great a source of error is left unprovided for?

In all experiments in which I have had occasion to use mercurial thermometers, I have endeavoured to avoid any correction for the exposed thread, by making the apparatus and thermometers employed of such relative dimensions that the whole thread and bulb, except the topmost division, are at the same temperature. When this is impossible, and when the experiments require such extreme accuracy, it seems to me that the first thing to be done is to surround the exposed portion of the thread with a current of running water, and so, while preserving it from the uncertain effects of conduction, radiation, &c., to render possible the observation of its exact temperature. The value of $(T-t)$ being thus correctly measured, that of m is found to be constant for all values of N, and to differ but little from 0·001545. It varies, however, with different thermometers.

The following experiments show most distinctly the truth of this statement:—

One of the standard thermometers mentioned in the first section of this communication was partially surrounded by a glass tube, *a b* (fig. 2), about an inch in diameter; this tube was closed at the bottom with a piece of good cork, about 8 millim. thick, through which the stem of the thermometer passed. The upper end of the tube *a b* was fitted with a cork, in which were four holes—one for the stem of the chief thermometer, a second for a thermometer to indicate the temperature of the water contained in the tube, while through the two others passed the tubes by means of which the current of water was maintained. The thermometer thus furnished was fixed vertically in the ordinary apparatus, A, for determining the 100°-point of thermometers. The open end of A was closed with a thin disk of brass, with a small central hole, through which the thermometer passed. One degree was equal to about four divisions of the millimetre-scale of the thermometer, the readings of which were observed with a cathetometer, and the fractions of a division measured with that instrument. It was found that the readings of the thermometer under these conditions were correct to ·02 of a millimetre, or ·005 of a degree. The numbers given below are the means of three readings, which, however, were nearly always identical. The thermometer in the water was graduated to fifths of a degree, and had been compared with the standard.

The chief thermometer was first heated in the steam for an hour, with two or three inches of the thread above the cork; it was then pushed down until the quicksilver was only just visible above the cork, and the reading noted; it was then pulled up again, and readings taken in various positions, as given in the following table; finally the thermometer was again pushed down as far as possible, and the reading taken, when it was found to be the same as before, showing that no change in the 100°-point had supervened during the experiment. Of several series of observations made in this manner, the one contained in the following table will suffice, since they all led to precisely the same result.

Reading of Standard when wholly immersed = 393·42.
Barometric pressure, corrected and reduced, = 760·1.
Corresponding temperature of steam = 100°·00.

Number of divisions surrounded by cold water and occupied by mercury.	Temperature of water.	Reading of Standard, T.	Value of C = 393·42 − T.	Value of m, $= \frac{C}{(T-t)N}$.
317	13°·0	389·01	4·41	·0001599
277·5	12·3	389·54	3·88	·0001594
221	12·0	390·30	3·12	·0001604
173	12·1	390·94	2·48	·0001631
130	12·1	391·58	1·84	·0001610
79	12·1	392·30	1·12	·0001612

An inspection of the above table is sufficient to convince us that the value of m is constant, and equal to the apparent expansion of mercury in the glass of which the thermometer was made; the numbers would probably have agreed even more closely, were it not that it is impossible to arrange the apparatus so that the cold portion of the thermometer-stem follows directly upon the hot portion. There must always be an interval occupied by the cork, the temperature of which is uncertain. It should be remarked that there is no indication whatever of the value of m increasing when that of N increases.

Precisely the same results were obtained with the second standard thermometer, as is shown by the following table:—

Reading of Standard BS, when wholly immersed, = 419·21.
Barometric pressure, corrected and reduced, = 760·5.
Corresponding temperature of steam = 100°·02.

Number of divisions surrounded by cold water and occupied by mercury.	Temperature of water.	Reading of Standard BS, = T.	Value of C, = 419·21 − T.	Value of m, $= \frac{C}{(T-t)N}$.
302	12°·0	415·13	4·08	·0001535
237	11·9	415·96	3·25	·0001556
174	12·0	416·84	2·37	·0001548
127	12·0	417·47	1·74	·0001557

Here, again, the value of m varies only within the limits of the error of observation, and shows no tendency to increase when N increases. It may be noted that with both the above thermometers the mean value of m is *greater* than ·0001545, the value usually assigned to it, but that it differs from that number by so little that the error committed by substituting the one for the other in the calculation of the correction C will rarely amount to more than 0°·02 C.

The above experiments were made at 100°, because this is the only temperature which can be maintained absolutely constant for an hour without the use of a quantity of complicated apparatus; and it is evident that the slightest variation in the temperature would entirely spoil the series of observations. At higher temperatures the sources of error which beset the readings of thermometers increase so rapidly that the exact value of the coefficient m becomes of less and less importance as the temperature rises, notwithstanding the fact that the correction C increases in amount. Since there is no reason whatever to suppose that any different results would be obtained at such higher temperatures, I thought it unnecessary to make any further experiments, more especially as those given above yielded precisely those numbers which the ordinary laws of expansion predicted.

There is another point connected with thermometry, to which I devoted attention some years ago. It has been suggested that when a thermometer is placed in a vapour at maximum tension, as in the ordinary chemical process of distillation, it does not truly indicate the temperature of the vapour. This suggestion owes its origin to the fact that drops are seen to accumulate and drop off the end of the thermometer. It has been supposed that this condensation of the vapour on a surface which should be as hot itself, is due to the molecular attraction of the glass for the vapour. If this be the case, the heat evolved by the vapour during liquefaction on the thermometer-bulb would raise the temperature of the latter. The thermometer would thus indicate a higher temperature than that of the mass of the vapour. The experiments which I made upon this subject, like those instituted by others, were inconclusive. I possess, however, an apparatus which seems to me eminently suited to answer the question satisfactorily. It is at present being employed for other purposes; but I trust that, when it is at liberty, I shall be able to put it to this not unimportant use.