



LXXVII. Inquiries into the laws of dilatation of solids, liquids, and elastic fluids, and on the exact measurement of temperatures

Messr. Dulong & Messr. Petit

To cite this article: Messr. Dulong & Messr. Petit (1816) LXXVII. Inquiries into the laws of dilatation of solids, liquids, and elastic fluids, and on the exact measurement of temperatures , Philosophical Magazine Series 1, 48:223, 373-380, DOI: [10.1080/14786441608637689](https://doi.org/10.1080/14786441608637689)

To link to this article: <http://dx.doi.org/10.1080/14786441608637689>



Published online: 27 Jul 2009.



Submit your article to this journal [↗](#)



Article views: 2



View related articles [↗](#)

LXXVII. *Inquiries into the Laws of Dilatation of Solids, Liquids, and elastic Fluids, and on the exact Measurement of Temperatures.* By Messrs. DULONG and PETIT. Read to the Institute 29th of May 1816*.

WHEN we endeavour to scrutinize most of the questions relative to the theory of heat, our progress is soon arrested by a difficulty which reappears at every step under different forms. What are we to understand by the word temperature? and what is the relation which exists between the indications of the thermometer and the quantities of heat added or subtracted, in order to produce determinate variations in the temperature? Here are two questions which must be resolved ere we can find, for example, the true laws of cooling: and it was, in fact, in order to solve them, that we embarked in the inquiries, a part of which we now submit to the judgement of the Class.

The construction of all the instruments destined to measure temperatures rests on the property possessed by bodies of changing their volume by the action of heat; but these instruments, in order to be rigorously exact, ought to satisfy two conditions equally indispensable. The first is, to be capable of comparison with each other, *i. e.* to agree always in their indications: the second is, to be comparable with themselves, *i. e.* to have a course which shall be in a known ratio with the variations of temperature.

The first of these conditions may be now obtained with great exactitude. We know well all the causes which can influence the justice of the indications of these instruments, and by employing the proper precautions we may attain a precision sufficient for all observations.

It is much more difficult to satisfy the second condition, which consists in establishing a graduation in which the equal parts of the scale answer to equal variations of temperature. In fact, in order to fulfil it completely, it is not sufficient to determine the relation of dilatations undergone by the thermometrical substance which we employ, to the quantities of heat which we communicate to it: we must also ascertain that the capacity of this substance for heat does not change, or at least keep an account of this change if it takes place. The extreme difficulty of determining with precision how the specific heat of a body varies, particularly at high temperatures, may be regarded as one of the greatest obstacles to the direct solution of this question.

Experimentalists, however, have made some attempts to attain it. DeLuc, who was the first, and is perhaps the only one

* *Annales de Chimie et de Physique*, July 1816, tome ii. p. 240.

who took up the subject, supposed that the capacity of water does not vary between 0 and 100°; so that, by mixing two equal masses of this fluid at different temperatures, he took for that of the mixture the mean of the temperatures of the separated masses.

Mr. Dalton thinks that it is not equal masses which must be mixed, but rather equal volumes; because he supposes that the capacity of the same mass increases with the temperature in proportion to the volume which it acquires; or, in other words, he supposes that the capacity of bodies referred to their volume will remain constant. It is very easy to prove that the specific heat of the gases, and even of the liquids, undergoes variations in the ratio indicated by Mr. Dalton; but we do not find in the work of this eminent philosopher any experiment in support of the law which he seeks to establish.

We see that the researches hitherto set on foot on the true gradations of a thermometer are reduced to very little, and we may even add that no person has undertaken direct experiments to ascertain them beyond 100°.

After having compared with each other the different experimental methods which may be employed in order to attain the solution of this important question, we have thought that the determination of the quantities of heat, particularly at high temperatures, was not susceptible of sufficient precision. We thought it preferable to compare, in the first place, with the mercurial thermometer the progress of the dilatation of bodies the most homogeneous, and of such a nature that the causes which visibly affect the uniformity of the dilatation should have no influence over them. These bodies ought evidently to be taken from among the gases, or from among the solids endowed with a great infusibility. The experiments of M. Gay Lussac having taught us that all the gases are dilated exactly in the same way when they are placed in the same circumstances, it is natural to conclude that the dilatability of one and the same gas ought to be constant, and that consequently, at increments equal in volume or in elastic force, they ought to answer to equal increments of temperature.

It was by setting out from this principle that M. Gay Lussac ascertained that the march of the mercurial thermometer was regular between the freezing and boiling points.

Experimentalists are pretty generally agreed in regarding the dilatation of solids which are very difficult to melt as being uniform. The experiments of Messrs. Laplace and Lavoisier on the dilatation of most of the metals between 0 and 100° come in aid of this opinion.

Now, if bodies so different as the metals and the gases followed

lowed the same course in their dilatations, it will become extremely probable that these dilatations will indicate the true temperatures: this is what might besides be verified by the corresponding quantities of heat. By following this course, we shall have the advantage of bringing into the determination of the true thermometrical scale, all the precision which we may now give in the measurement of dilatations; and these measurements themselves, independent of the important consequences which we might deduce from them relative to the theory of heat, will present data which will be useful under several circumstances. Such are the considerations and the motives which determined us to commence our labours by the comparison of the dilatations of the gases and the solids with the mercurial thermometer in high temperatures.

Comparison of the Dilatation of the Gases and of the Progress of the mercurial Thermometer.

The apparatus which we used was composed of a rectangular tub of red copper, seven decimetres long, one decimetre broad, and a decimetre deep. This tub has on one of its lateral faces two openings, one of which serves to introduce, in a horizontal situation, a mercurial thermometer, and the other holds the open extremity of a tube which is placed horizontally at the same height with the thermometer. This tube is perfectly dry, and contains air which is dry also.

The tub rests on a furnace constructed in such a manner as to heat all parts equally. It is filled with a fixed oil, which may, as we know, support a temperature of more than 300 degrees without boiling. The tube which contains the air is terminated on the side of the aperture by a short tube of a very small diameter, which partly issues from the tub. The quantity of air contained in the exterior portion of this tube, and which does not participate in the heating of the rest, is not worth noticing. We ascertained that it never exceeded a two-thousandth part of the total mass; and besides, we had the precaution to heat it during every experiment, in order to reduce the error which might result from it.

The tub is covered by a lid pierced with several holes: some are traversed by thermometers, which serve to indicate if the different parts of the liquid mass are at the same temperature; the others have stalks or sticks terminated by vertical plates of copper, which we can turn; we thereby produce in the liquid a brisk agitation, the object of which is to establish the uniformity of temperature.

The following is the course pursued by us in every experiment.

ment. We heated, in the first place, the tub to a temperature little inferior to that which we wished to obtain, and we afterwards closed all the apertures of the furnace. The equilibrium of heat tending then to be established throughout the whole heated mass, the temperature of the oil still rose some degrees, and soon attained its maximum, where it became some time stationary, and consequently easy to measure with precision. It was then indicated by the horizontal thermometer, which was sunk sufficiently in the oil, in order that the whole column of mercury should be inserted in it: at the same instant we closed by means of the blowpipe the fine point of the external part of the air-tube, and we noted the barometrical height. This being done, we withdrew the tube and carried it into a separate chamber, the temperature of which was nearly invariable: we placed it vertically, and in such a way that the point entered a mercurial bath perfectly dry. We broke this point in the mercury, and this liquid ascended until the equilibrium was established with the external pressure: we left it in this situation a sufficient time to give it precisely the temperature of the room, which we knew with great precision by means of a very sensible thermometer suspended beside the tube. When the equilibrium of temperature was perfectly established, we measured, by means of a vertical scale fitted with an index, the height of the column raised in the tube. We observed at the same time the barometrical height, and the difference of these heights made us acquainted with the elasticity of the cold air. We then withdrew the tube, by taking all necessary precaution to retain the mercury of which the column that had been raised was composed. The tube and the mercury which it contained were weighed: we afterwards weighed this same tube successively empty and entirely full of mercury; by subtracting from the result of this last weight those of the first two, we had the weight of two equal volumes of mercury, the one with the volume of hot air and the other with the volume of cold air; and from these weights we inferred the volumes themselves, which we afterwards brought to what they would have been under the same pressure, since we knew the elasticity of the cold air which had been measured as we have indicated, and that of the warm air which was equal to the pressure of the external air observed at the instant of closing the tube.

In order to judge more easily of the degree of confidence which the results deserve to which we had been led, it will not be unavailing to give some details relative to the precautions which we took in every experiment. One of the greatest obstacles which we meet with in this description of inquiries is owing to the difficulty of establishing a perfect uniformity of temperature in a
great

great liquid mass 200 or 300 degrees warmer than the surrounding air. We attain this rigorously when the temperature at which we operate is, for example, that of the ebullition of the liquid which we employ: then this temperature is necessarily fixed; but in every other case the progress more or less rapid of the heating or cooling of the different points of the mass, opposes the necessary uniformity from taking place. We are of opinion, however, that the arrangement of our apparatus remedies in a great measure this kind of inconvenience, and this is owing, on the one hand, to the copper tub being sunk in the furnace, and composing with it a considerable mass, which is cooled slowly, particularly when it is near its maximum of temperature; and in the second place, the liquid being continually agitated, the heat ought to spread through it more uniformly. In short, to remove all doubts, we inserted into this tub two thermometers situated horizontally at the same height; we had raised the temperature in the same way as in our ordinary experiments, and we ascertained that by shaking the liquid the thermometers never differed but by a few tenths of a degree.

Besides, even supposing that the different particles of the liquid layer which surround the air-tube were not exactly at the same temperature, the error will not be so great as at first believed; for, in consequence of the arrangement of the apparatus, the bulb of the thermometer nearly answers the middle of the length of the tube, and consequently this instrument ought in all cases to indicate a temperature not far from the mean of the different parts of the tube. We ought to recollect also, from what has been said, the necessity which there is, in order to know the true indications of the thermometer, to sink it sufficiently in the liquid, that the whole column of mercury should enter it. This precaution, which appears to be unnecessary in low temperatures, ought not to be omitted in high temperatures; for then the column of mercury contained in the tube may undergo an increment of very sensible length. Thus we have remarked that, at the temperature of 300° for instance, there was frequently more than 15° of difference between the indications of one and the same thermometer, whether the whole of the mercury was in the liquid, or the bulb only entered it. We might indeed, according to our knowledge of the dilatation of the mercury, estimate the error which we commit by plunging only part of the thermometer into it; but the correction to make in this case, bearing on considerable numbers, will occasion serious errors, because we never know exactly the mean temperature of the mercury contained in the tube. It appeared to us to be preferable in all cases to place the thermometers horizontally. In order to give a more precise idea of the various operations of which each of our experiments is composed,

378 *Inquiries into the Laws of Dilatation of Solids, Liquids,*

we shall subjoin a complete series, with all the indications necessary for calculating them.

This series does not comprise any observations for temperatures approximating 100° , although we have repeated several times the experiment of the dilatation of the air in boiling water. We did not propose by any means to verify thereby a determination on which no doubts can be entertained; but the coincidence of our result with that of M. Gay Lussac has been for us the best proof of the rigorous exactitude of the process which we made use of.

Temperature of cold air.	Temperature of warm air.	Elasticity of cold air.	Elasticity of warm air.	VOLUME in centimetre cubes,		Temperature deduced from the dilatation of the air.
				of cold air.	of warm air.	
17°,06	156°,85	0,6186 ^{mt}	0,7653 ^{mt}	63,526 ^c	76,438 ^c	155°,7
16°,74	197°,53	0,5771 ^{mt}	0,7561 ^{mt}	34,8573 ^c	43,287 ^c	194°,64
18°,25	249°,43	0,55695 ^{mt}	0,7594 ^{mt}	53,225 ^c	63,862 ^c	243°,25
18°,24	318°,11	0,52525 ^{mt}	0,7603 ^{mt}	66,1728 ^c	92,2875 ^c	309°,7

We have made several similar series of experiments and nearly at the same temperatures. By a very simple interpolation and by taking mediums, we have formed the following table, which indicates the correspondence of degrees marked by the mercurial thermometer, and those which are deduced from the dilatation of the air,

Temperatures indicated by the mercurial thermometer.	Temperatures deduced from the dilatation of the air.	Differences.
100°	100°	0°
150°	148° 70	1° 30
200°	197° 05	2° 95
250°	244° 17	5° 83
300°	291° 77	8° 23

Although the experiments which we have given present a remarkable coincidence, we thought it right to attempt the attainment of the same results by a different route.

In these new experiments we made use of an air-tube of a much greater capacity than in the first, and we placed it in the same way; only the very narrow tube which was soldered to it was curved at its issuing from the tub, and prolonged vertically for the length of almost five decimetres: it was heated by taking
all

and elastic Fluids, and on the Measurement of Temperatures. 379

all the precautions which we have mentioned; and when we had attained the stationary temperature and had noted the barometrical height, we conveyed under the inferior extremity of the vertical tube a capsule full of very dry mercury: the tube was allowed to cool until the oil had resumed nearly the temperature of the air. During the continuance of this cooling, the mercury ascends into the vertical tube, and does not stop until the air contained in the tube is completely cooled. The elastic force of this air is then equal to the external pressure of the atmosphere diminished by the height of the column raised up: that of the warm air was equal to the barometrical height observed at the instant when the temperature was stationary. We might therefore conclude, by means of the law of Mariotte, what would have been the dilatation of the air if it could have been dilated. In order to render this process completely exact, we ought to have kept an account of the capillary depression which the mercury undergoes in the straight tube into which it had risen. This depression had been measured beforehand, and care had been taken to make choice of a tube of such a calibre as not to admit of its varying sensibly.

In the second place, the volume of air did not remain exactly constant; the portion comprised in the vertical tube re-entered in part into the great tube, in proportion as the column of mercury rose, and this portion of air did not sensibly change its temperature. It became necessary to calculate the influence of these two causes, and make our observations undergo the correction flowing from it. This correction depended on the relation of the capacity of the large tube with that of the small; the calculation which gives it is besides too simple to render it necessary to indicate it. We shall now give the results of one of the series of experiments made by this new process: all of them agree with the observations made by the first means, and they have entered into the determination of the mean measurements formerly related.

	Millimetres.	Capillary Correction.
Length of the great tube	.. 0 62	millimetres 4.5.
Length of the small tube	.. 0 57	

Elasticities of the warm air corrected from the capillary depression.	Temperatures corresponding observed on the mercurial thermometer.	Temperatures calculated ac- cording to the variation of the elastic force of the air.
0, 48,60	91°,417	91°,417
0,579,45	163°,21	160°,27
0,706,85	263°,8	254°,14
0,765,75	309°,88	297°,54

The results which we have made known inform us that the dilatation of the mercury in the thermometer follows a more rapid course than that of the air; so that, if we regard the latter as necessary to serve for the exact measurement to the temperatures, we ought to conclude that the indications of the mercurial thermometer are too high in the temperatures superior to that of boiling water, and the numbers which we have given might serve to make these indications undergo the proper corrections. Those numbers increase besides in a sufficiently regular manner, in order that we may without sensible error determine the correction relative to the temperatures intermediate to those which are comprehended in the table.

This conclusion destroys a doubt which was raised with respect to the law of dilatation of the gases. This law had not been announced in the same way by Messrs. Gay Lussac and Dalton, whose experiments on the subject now before us appeared at the same period. The experiments of M. Gay Lussac proved that the dilatation of the gases referred to the mercurial thermometer is for each degree a constant fraction of the volume at a determinate temperature. Mr. Dalton, on the contrary, supposed that the increase of the volume is for each equal variation of temperature, a constant portion of the volume at the foregoing temperature. In truth, Mr. Dalton does not appear to have made direct experiments on this head: the only arguments which he advances in favour of his hypothesis are reduced to the extreme simplicity under which laws in appearance very complex will then be presented, such as the law of the cooling of bodies, and that of the variation of the elastic force of steam.

We ascertained, however, that the first of those laws will by no means assume a character so simple as he pretends, even admitting his hypothesis to be correct.

To conclude: It is not by considerations of this kind that we can establish laws which observation alone ought to furnish. The experiments which we have made at high temperatures completely destroy the hypothesis of the English experimentalist: for, so far from these experiments pointing out any thing perfectly positive as to the measurement of temperatures, it is at least very probable that the march of the mercurial thermometer ought to be more rapid than that of the temperatures, since in all the other liquids the dilatability increases in proportion as they are heated; whereas in the hypothesis which we are attacking, we shall find, on the contrary, that the dilatability of the mercury decreases rapidly in proportion as it is heated; a result completely opposite to the very principles on which Mr. Dalton had founded his theory of the measurement of temperatures.

[To be continued.]