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> "Nec aranearum sane textus ideo melior quia ex se fila gignunt, nec noster vilior quia ex alienis libamus ut apes." Just. Lips. *Polit.* lib. i. cap. 1. Not.

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paths: now in the former case if the two beams travel D between separating and meeting, and meet again at an angle 2α, then the linear separation of corresponding points may be taken as $2\alpha D$, which amounts to the same as our $2\alpha p$, and hence the conditions are such as favour the degree of sensitiveness expected by Michelson and Morley; while in their own experiment, as each of the separated beams is reflected fifteen times in its own independent path there is opportunity for a considerable lateral shift of the one beam relative to the other when they meet, although both are adjusted as nearly to parallelism as is necessary, that is to say that c is independent of $2\alpha p$, and the sensitiveness of the system of fringes is unknown, but in all probability small compared to that expected. In their celebrated repetition of Fizeau's great experiment on the effect of running water on the æther Michelson and Morley got their well-known positive result, but in this case the divided beams were sent in opposite directions round the same path, so that the optical arrangement had the sensitiveness expected. It is the use of multiple reflexion along different paths in the experiment on the relative motion of earth and æther that introduces the possibility of comparatively large lateral shift.

If the argument in this paper is correct it ought to be possible by careful adjustment for the requisite smallness of c, or for getting the absolute central band into the field of view, to give the Michelson and Morley apparatus the sensitiveness desired for measurement of the relative motion of earth and æther; and in any case an experimental examination of the effect of lateral shift seems desirable.

Melbourne, Sept. 1897.

III. *The Transmission of Radiant Heat by Gases at Varying Pressures.* *By* CHARLES F. BRUSH[*].

[Plates I. to X.]

BEFORE describing my own investigations on the transmission of heat by gases, I shall refer briefly to the classical work of a somewhat similar nature by MM. Dulong and Petit early in the present century. Their memoir entitled "Researches on the Measure of Temperatures, and on the Laws of the Communication of Heat," gained the prize voted by the Academy of Sciences in 1818. A translation of this important paper may be found in the 'Annals of Philosophy,' for February, March, April, and May, 1819.

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[*] Communicated by the Author, having been read before the American Association for the Advancement of Science, August 10, 1897.

In their researches on the "Communication of Heat," Dulong and Petit used as the cooling body a very large thermometer bulb filled with mercury; and as the recipient of the heat, a large copper bulb or "balloon" about three decimetres in diameter, in the centre of which the thermometer bulb was placed. The copper balloon was coated with lamp-black on the inside, and kept at any desired constant temperature by means of a water-bath or melting ice. The thermometer tube was of such length as to bring the zero of the scale outside the balloon; and the thermometer was adapted to be removed, heated, and quickly replaced, air-tight. The balloon was connected with an air-pump capable of rapidly exhausting it down to about two millimetres pressure; and also with a gas-holder from which it could be quickly filled with the gas whose cooling properties were to be determined. The rate or "velocity" of cooling of the thermometer bulb was deduced from observations of the falling temperature at equal intervals of time.

With this apparatus Dulong and Petit made many carefully conducted experiments at differences of temperature between the thermometer and balloon ranging as high as 300 degrees; and with several different gases besides air, ranging in pressure from atmospheric to two millimetres. From the results of these experiments they deduced several laws of cooling which they held to be general in their application. They sharply divided the cooling into two parts; that due to convection—the actual contact of the surrounding cooler gas renewed by its own currents, and that due purely to radiation—the same as would occur in an "absolute vacuum." They derived a constant value for the latter, and values for the former varying with different gases and different pressures. They generally used the thermometer bulb naked, with its natural vitreous surface, but sometimes they silvered it. While this radical change in the character of surface greatly changed the loss of heat due to radiation, it apparently had no effect on that due to convection. They also tried thermometers with different-sized bulbs; and again, various shaped vessels, filled with various liquids surrounding a small thermometer bulb, were tried as the cooling body. None of these changes affected the general laws of cooling which they had deduced. But they did not try a smaller copper balloon. Had they tried a very small one they would have found some of their general deductions untenable.

MM. Dulong and Petit devoted a lengthy chapter to the discussion of the cooling of bodies in vacuo. But they fell into the grave error of deducing the behaviour of the last few millimetres of gas from that of the rest. In this way they arrived at the following "Sixth Law:"—

"The cooling power of a fluid diminishes in a geometrical progression when its tension itself diminishes in a geometrical progression. If the ratio of this second progression is 2, the ratio of the first is 1·366 for air; 1·301 for hydrogen; 1·431 for carbonic acid; and 1·415 for olefiant gas."

My own observations show that this law can be approximately true only in the case of a large balloon; and at pressures from a few millimetres upward. There is no suggestion of it when a small balloon is used; and at small pressures it does not obtain with either large or small balloons.

I find that in a small balloon the cooling effect of the last millimetre of air is nearly ten times as great as that of all the rest, up to atmospheric pressure, combined.

It was through misplaced confidence in their Sixth Law that Dulong and Petit were led to place a value on the rate or velocity of cooling in vacuo, something like a hundred per cent. too high, as I shall show later; and as they derived the cooling values of gases by deducting the cooling effect of a vacuum from the total cooling observed, all their values for gases are much too low. These large errors vitiate much of their otherwise excellent work, and render the numerical values of the ratios given in the Second and Third Laws extremely doubtful.

Other experimentalists also have studied the transfer of heat by air and other gases at various pressures. Kundt and Warburg (Pogg. *Ann.* 1874–5) and Winkelmann (Pogg. *Ann.* 1875–6) observed that the rate of heat transmission remained substantially constant through a long range of diminishing pressure; and then decreased with further exhaustion. But as they made no measurements of pressure below one millimetre (1316 millionths of atmospheric pressure), their results have no quantitative value for low pressures.

Crookes, in his paper "On Heat Conduction in Highly Rarefied Air (Proc. Roy. Soc. 1880), described a similar experiment in which he carried the pressure measurements as low as 2M. (two millionths). From the fall in the rate of heat loss which occurred between the pressures of 760 millimetres and 1 millimetre, and 5 M. and 2 M., he concludes: "We may legitimately infer that each additional diminution of a millionth would produce a still greater retardation of cooling, so that in such high vacua as exist in planetary space the loss of heat—which in that case would only take place by radiation—would be exceedingly slow."

In this conclusion Sir Wm. Crookes was, I think, wrong. I find that the curve representing the rate of cooling does not break down materially at pressures as low as a twentieth of a millionth.

My own investigations on "The Transmission of Radiant Heat by Gases at Varying Pressures" form a part of a general study of the properties of high vacua, in which I have long been engaged, and which is yet far from being completed.

In the course of my work it became necessary to know how much of the heat communicated by a good radiating body at ordinary temperatures, to a neighbouring body at a slightly lower temperature, through an intervening gas, is transmitted by the so-called æther, and how much by the gas; and whether any of that transmitted by the gas is communicated otherwise than by the process of convection. Also why, and to what extent, do the gases differ from each other in their heat transmitting capacities.

In the drawings herewith, Plate I. is a diagram of the apparatus used in my experiments. A is the thermometer whose cooling was observed. It has a very open scale divided into two-tenths degrees C. The zero-point is placed a long distance (about 170 millimetres) above the bulb, for obvious reasons. The bulb is cylindrical, about 20 millim. long and about 7 millim. in diameter, and is coated with lampblack applied with a very thin alcoholic solution of shellac. After several hours' baking at 100 degrees in a good vacuum, this bulb gave constant radiation results. The thermometer is suspended by a platinum wire, with its bulb in the centre of the large pear-shaped glass bulb B, about 112 millim. in diameter. The stem of the thermometer hangs freely in the long neck of the large bulb. I shall hereafter call the glass bulb B, the "large radiation bulb," or simply the "large bulb," to distinguish it from a smaller one used later. The bulb B is surrounded by a copper tank C, lagged with woollen cloth, and filled with crushed ice and distilled water. A wire netting C′ serves to keep some of the ice always below the lowest point of B. The tank C is movable on vertical guides, whereby it may quickly be raised to, or lowered from, the position shown, thus exposing the bulb B alternately to the ice-bath and the atmosphere of the laboratory. The bulb B communicates freely with the large barometer-tube D, which is used for measuring all but very small pressures. E is a standard boiled barometer, dipping into the mercury-cistern F, common to both barometers. G is a McLeod gauge giving very accurate measurements of small pressures, and H is a drying-bulb containing phosphorus pentoxide. The glass stopcock I serves to admit other gases than air, and is of course made absolutely tight when closed. The mercury valve K prevents any leakage backward from the pump when the latter is stopped during observations. Exhaustion is effected by an automatic Sprengel pump having five fall tubes. L is a fine cathetometer placed in front of the whole apparatus, and by rotation on its vertical axis is adapted to read the McLeod gauge, both barometers, and the thermometer. It has a vertically divided scale with vernier and microscope, for reading the barometers, and a micrometer for reading the gauge. A watch N is mounted close beside the thermometer on a sliding frame, so as to be easily kept in the field of view of the cathetometer telescope when the latter is used to observe the falling temperature.

Before using this apparatus I always exhausted to a good vacuum, and heated the bulb B by means of a water-bath, and all other vacuous parts by means of an air-bath, to 100 degrees for several hours. This was found necessary in the first instance with air, in order to divest the inner glass surfaces of that portion of their coating of adherent gas most easily given off in a vacuum. This gas was pumped out; and not being principally air, was not largely re-absorbed when air was admitted. Without this precaution I was unable to obtain constant results at very low pressures. When other gases were tried successively, the preliminary heating prevented gas from one operation attaching itself to the glass and remaining to contaminate the succeeding gas at very low pressures.

I next introduced the proper gas up to atmospheric pressure, and made a preliminary cooling of the thermometer by raising the ice-tank C. This preliminary cooling was found to have a slight effect on the readings next following, and was done to make the first set of readings on any day entirely comparable with the others. I then lowered the ice-tank, and when the temperature had risen to 18 degrees, stirred the ice and water thoroughly, raised the tank again, and observed the thermometer through the telescope—noting by the watch N the instant when the falling mercury passed each degree of the scale. Then, with the ice-tank still up, I noted the pressure by measuring with the cathetometer the difference in height of the barometer columns in D and E. The barometer D showed that the gas in the radiation bulb cooled nearly to zero with very great rapidity when the ice-tank was raised. I always measured pressures with the radiation bulb cold. It was usual to repeat the whole operation to confirm results before reducing the pressure by the pump.

Observations were thus made at pressures varying from atmospheric down to the best vacuum obtainable. In some instances many series of observations were made at varying pressures all within the last millionth. The gauge could be relied upon to measure these small pressures with very great accuracy. But it was difficult to maintain them long at any exactly constant value on account of the continual, though slight, evolution of gas from the glass of the apparatus.

As I desired only comparative results, no correction was made for the probable slight inequalities in the callibration of the thermometer; nor for heat conducted to or from the bulb by the stem; nor for the change of zero-point due to changing external pressure. The mercury fell exactly to zero at atmospheric pressure, and about one-fiftieth of a degree lower at no pressure. The pressure error due to differences of capillary depression in the two barometers was ascertained at high exhaustions, and found nearly constant. It was always corrected. The different gases used were carefully prepared and dried, and were introduced quite free from any admixture with air.

My observations have extended over a long period, and are far too voluminous to be recorded here in detail. But I have embodied their most salient features in a series of curves which render them readily apparent to the eye. In these curves the abscissæ represent the pressure, and the ordinates represent the rate of heat transmission through the gas, from the thermometer bulb to the ice-cold envelope. The rate of transmission at any particular pressure is expressed by the reciprocal of the number of seconds required for the temperature to fall through a given number of degrees. For convenience of scale all the reciprocals are multiplied by 500.

Pl. II. shows the curve for air. The heavy line represents the rate of cooling from 15 degrees to 10 degrees. It is in three sections, A, B, and C. Section A embraces the whole range of pressure from nothing to atmospheric; section B embraces the range of pressure from nothing to ·01 of atmospheric; and section C embraces the range of pressure from nothing to ·0001 of atmospheric, *i. e.* 100 M. (one hundred millionths). Atmospheric pressure is taken at 760 millim. Thus it will be seen that section B is the last hundredth of A, magnified a hundred times; and section C is the last hundredth of B, magnified a hundred times. The cross-section paper on which these curves are drawn is 50 centimetres wide; hence if the curve were completed on the scale of B, it would be 50 metres long; and if completed on the scale of C, it would be five kilometres long. This magnification of the abscissæ without change of the ordinates enables us to study every part of the curve with ease. The small circles represent the points in the curve established by observation. These points are shown exactly as found, without any attempt to smooth out rough places in the curve. The same is true of the curves of other gases. The heavy dotted line parallel with the base represents that portion of the total heat transmission due to the æther; while all above it represents that due to the air.

Starting at the left-hand end of section A, representing the rate of heat transmission at atmospheric pressure, we observe that the curve drops regularly at a rate faster than the diminution of pressure, during 95 per cent. of the whole range of pressure from atmospheric to zero. Beyond this point the rate of heat transmission remains substantially constant, as shown by section B and the latter part of A, down to a pressure of about ·0003—a range of nearly ninety-nine and a half per cent. of that remaining. Here the curve suddenly begins to drop again, and falls steadily, as shown by section C and the latter part of B, until it meets the æther line at the zero of pressure.

Under the curve A I have drawn curves with finer lines representing the rate of heat transmission at smaller differences of temperature between the thermometer and ice-bath. As before stated, A represents the cooling from 15 degrees to 10 degrees. On the same scale *a* represents the cooling from 9 degrees to 6 degrees; *a a* from 6 degrees to 4 degrees; and *a a a* from 3 degrees to 2 degrees. Now, Newton's law of cooling requires that the rate shall vary directly with the difference of temperature between the cooling body and the surrounding medium. While this law is known to be incorrect for large differences of temperature, it is generally accepted for very small differences. If it were correct under the conditions of the present experiment, then the ratios of the times required for the temperature to fall through the several ranges above indicated, would all equal unity, and the curves A, *a*, *a a*, *a a a* would coalesce. But they are very far from doing this. It will be observed that all of these curves preserve their relative values very closely indeed, until they approach the point of pressure where the curve A reverses itself; then they begin to bunch themselves very much closer together, especially the lower ones, and shortly reach a greatly reduced, as well as varied ratio of values which they retain substantially unchanged to the end, as shown in connexion with section C. To avoid confusion of lines I have omitted the secondary curves corresponding with section B.

Pl. III. shows the curves for carbon monoxide. This gas was chosen for comparison with air, because its absorptive power for radiant heat is ninety times greater, while its specific heat is almost exactly the same. The principal curve, representing the rate of heat transmission from 15 degrees to 10 degrees, differs very little from that of air. It shows a slightly better rate than air at very small pressures; not quite so good a rate as air at intermediate pressures; and the same rate at atmospheric pressure. But the curves *a*, *a a*, *a a a*, representing equivalent amounts of cooling at smaller temperature differences, are materially unlike those of air. At high pressures they have about the same ratio values as with air; but the ratio diminishes much less at intermediate and low pressures; that is to say, the curves remain further apart. It is equally noticeable that the curves *a a*, *a a a* retain their full relative ratio values at low pressures, while with air they nearly coalesce.

Pl. IV. shows the curve for ethylene. It was thought that this gas might transmit heat more rapidly than air, because of its much higher specific heat. But it does not do so. Its curve has the same form as those of air and carbon monoxide. It transmits heat nearly as well as air at atmospheric pressure, but not nearly so well at intermediate pressures. At a very few millionths, however, it conducts a trifle better than air. The curves *a*, *a a*, and *a a a* have the same characteristics, and about the same ratios, as those of carbon monoxide.

Hydrogen was next tried, on account of its very low coefficient of viscosity, as well as its very high specific heat. Pl. V. illustrates the hydrogen curve on the same scale as the others. While in general form it resembles the air curve, all the ordinates are immensely increased. It is noticeable that the intermediate section B of the curve lies much nearer A than C, quite different from its relative position in the curves of the other gases. This section of the curve shows that hydrogen retains about two-thirds of its initial heat-transmitting power at a pressure nearly two hundred times smaller than does air. The curves A, *a*, *a a*, and *a a a* have something like the same ratios as they have in the cases of carbon monoxide and ethylene. In general, it may be said of hydrogen in the large radiation bulb, that it transmits heat nearly four times as fast as air at atmospheric pressure; more than twice as fast at a very few millionths, and more than seven times as fast through a long range of intermediate pressures.

As evidence of the accuracy of the observations on which the curves thus far described are based, it is gratifying to note that the vacuum, or æther line, locates itself exactly the same in all.

In making the above-described observations I looked for some change in the phenomena when the exhaustion reached the point at which the mean free path of the gas molecules equalled the distance between the thermometer bulb and the cold walls of the enclosing globe. This should have been at a pressure of about two millionths. No such change was observable, however, in any case. Partly in pursuance of the same idea, I resolved to repeat some of my experiments, using a very much smaller radiation bulb. This I expected would also reduce that portion of the total cooling effect due to convection currents. I accordingly employed the bulb or tube P (Pl. I.) in my further experiments. This is made from a thin glass tube slightly less than 20 millimetres internal diameter, and in it hangs the same thermometer A which was used before. In transferring the thermometer, great care was taken to avoid any disturbance of the coating of lampblack on its bulb. At *b* is a contraction of the tube P to prevent the thermometer bulb swinging against the inside of the tube. The contraction *b* is, however, much larger than the thermometer stem, so that normally the latter does not touch it. The thermometer bulb hangs exactly in the centre of P, near its bottom, and is separated from it by a space of a trifle more than six millimetres—almost exactly a quarter of an inch; instead of two inches, as in the case of the "large bulb." The tube or bulb P, I shall hereafter designate the "small radiation bulb," or simply "small bulb," to distinguish it from the large one.

Pl. VI. shows the curve for hydrogen, with the small bulb. It differs radically in size and form from that obtained with the large bulb. Section A, instead of drooping rapidly with decreasing pressure, maintains almost its full value throughout. Section B starts with nearly double its old value, but breaks down much earlier. Section C starts with a little higher value, but is much straighter, and consequently has a lower value throughout most of its length. The curves *a*, *a a*, *a a a* are very peculiar. They start at atmospheric pressure with much smaller total, and very different relative ratios than in Pl. V., and are successively absorbed into A. They reappear later, however, as shown in section C, but with smaller ratios than in Pl. V.

Pl. VII. gives the curve for air, with the small bulb. It differs from that with the large bulb quite as much as did the hydrogen curve. Section A droops slightly, and then regains almost its full atmospheric value at one per cent. pressure. Section B has the same form as with the large bulb (Pl. II.), but more than double its value; and section C also has a much higher value throughout. The curves *a*, *a a*, *a a a* have small ratio values at the beginning, and are absorbed into section A the same as with hydrogen. But *a a* and *a a a* coalesce when they reappear, and coincide to the end; while the ratio between *a* and *a a* remains constant at a very small value.

Pl. VIII. is the curve for carbon dioxide, with the small bulb. It closely resembles the air-curve in form, but has a very much smaller value throughout. While the curves *a a* and *a a a* are soon united, and remain so to the end, *a* and *a a* never disappear as they did in the cases of hydrogen and air.

With the small bulb, as with the large, no change in the character of the phenomena was observable when the exhaustion had reached the point at which the mean free path of the molecules equalled the space through which the heat was conducted. This point was reached in the small bulb at a pressure of about fourteen millionths.

It seems reasonable to assume that the radical difference between sections A of the curves obtained with the large and small bulbs respectively was due to an almost complete suppression of convection-currents in the latter case. In the absence of convection-currents, that part of the heat transmitted by the gas was probably carried by a process analogous to conduction in solids. The shortness of conductor in the case of the small bulb may account for the greatly increased rate of conduction. But why the conductivity of a gas remains nearly constant through a very wide range of pressure is not clear. Sir Wm. Crookes's explanation of this phenomenon seems to me very unsatisfactory.

It will be noticed that the "æther-line" is about four per cent. lower with the small bulb than with the large one. This may be due to the greatly decreased amount of surface presented by the small bulb for absorption of the radiant heat.

The enormous heat-conducting capacity of gases at very small pressures is strikingly shown in all the curves. But hydrogen is preeminent in this respect. Thus, in the large bulb, hydrogen at a pressure of only twenty-six millionths of an atmosphere transmits heat as rapidly as the æther! At seventy-six millionths it equals air at atmospheric pressure; that is to say, it does the work of nearly two hundred thousand times its weight of air!

It is remarkable that at pressures up to a few millionths all of the curves are nearly straight lines. This is especially noticeable in the small-bulb curves, showing that at these small pressures the heat-transmitting power of a gas varies directly with its amount. Hence it seems reasonably certain that if the very small fraction of a millionth of the gas examined, which remained at the end of each experiment, could have been entirely removed, the heat-transmitting power of the vacuum would not have been materially diminished. It was customary at the end of the experiments with each gas to close the gauge permanently when the pressure had fallen to a tenth of a millionth or so, and with the capacity of the whole apparatus thus reduced run the pump continuously from one to two hours. Several sets of observations were always made during this extreme exhaustion, and while the change in the rate of cooling of the thermometer was generally appreciable it was always very small indeed. In my earlier experiments I took the greatest care to ensure the absence of mercury-vapour in the final vacuum. But the presence or absence of mercury-vapour made no difference distinguishable from the errors of observation.

Of course the best vacuum producible by a Sprengel-pump still contains many thousands of millions of gas-molecules per cubic centimetre. This may be regarded as a prodigiously large or exceedingly small quantity of gas, according to our point of view. While it has no apparent effect on the general heat-transmitting capacity of the vacuum, it does seem to interfere with or modify some function of the æther. This is the only explanation of certain phenomena that I can offer. I refer to the different behaviour of the vacua with different residual gases, and in different-sized bulbs, in the matter of adherence to, or departure from, Newton's simple law of cooling. The curves *a*, *a a*, *a a a* illustrate these differences in the several cases at the extreme end of section C of the principal curves. These differences are too large to be attributed to errors of observation. This is one of several reasons which lead me to suspect that at higher pressures all the gases examined interfere materially with and retard the transmission of heat by the æther. In other words, I suspect that the dotted æther line of my curve sheets should not be drawn parallel with the base, and have a constant value at all gaseous pressures as shown, but should have a decreasing value as the gas pressure rises from zero. On this interesting phase of my subject I hope to have more to say at a future date.

Before closing I will call attention to two more curve sheets. Pl. IX. is an air-curve plotted from figures given in Dulong and Petit's paper. It is drawn to such a scale that the rate of heat-conduction at atmospheric pressure is the same as in my own experiment with air in the large bulb, and illustrated in Pl. II. The first five stations in the curve are the ones from which they deduced their "Sixth Law" of cooling. The rest of the curve is drawn in accordance with that law, and the vacuum line represents exactly the value they assigned to the cooling power of an absolute vacuum. Comparison with Pl. II. shows how much they erred in their deductions.

Pl. X. embodies the results obtained with a mixture of three volumes of hydrogen and five volumes of carbon dioxide in the small bulb. A study of this curve in connexion with Pl. VI. shows that the carbon dioxide interfered very greatly with the performance of the hydrogen. Before any exhaustion was made, the hydrogen *alone* would have done more than three times the work of both gases. It was not until the pressure had fallen to about one hundred millionths that both gases combined did as well as the hydrogen would have done alone. Below this pressure both gases contributed to the result.

This interference of mixed gases is a very interesting phenomenon, and seems to warrant the careful investigation which it is my intention to give it.

IV. *The Stresses and Deflection of Braced Girders.*
By W. H. MACAULAY[*].

A STRUCTURE consisting of a number of bars hinged together (or pin-jointed) at their ends is called a frame. The points at which hinges occur are called joints. A frame is said to be *stiff* if the number and arrangement of bars are such that the frame cannot be distorted without stretching one or more of them. If this is not the case it is said to be loose. A frame is said to be *just stiff*, if it is stiff and the removal of any one bar could make it loose. If a frame is stiff, but not just stiff, it is possible, by removing a set of one or more bars, to make it just stiff without depriving it of any joints; such bars are called *redundant bars*. A frame may have more than one set of redundant bars, that is to say it may be possible to reduce it to being just stiff, without loss of joints, in more than one way. The same terminology may be applied to a frame in one plane; it is then assumed that no distortion is admissible except in that plane, and that any forces applied to the frame are in its plane.

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[*] Communicated by the Author.