As a contribution to the history of typhoid fever epidemics, we warmly welcome the able report of some outbreaks of this disease in Massachusetts, made by, as well as under the direction of, Prof. W. T. Sedgwick.

The work before us was included, in the first instance, in the Twenty-fourth Annual Report of the State Board of Health of Massachusetts, but has been, and we think wisely, issued also as a separate pamphlet. No less than nine outbreaks of typhoid fever were investigated, but perhaps the most interesting and important is that which occurred in Lowell, one of the largest cities in the Merrimack Valley, and depending mainly for its water supply on the Merrimack River. Public attention was first called in December 1890 to the serious character of the epidemic of typhoid fever in the city, when it became known that in the preceding month 122 cases had been reported. and twenty-eight deaths from this disease had taken place. After a long and exhaustive investigation, the river water supply became suspected of being the vehicle of the specific infection to which the epidemic must be attributed. Prof. Sedgwick set to work, therefore, to find if there had been any special or unusual infection of the river *above* Lowell, and the discovery was made that an outbreak of typhoid fever, "such as had not been known for forty years," had occurred during the previous August, September, and October in a small village only three miles above Lowell, and situated on a small stream running into the Merrimack River. Four at least of the cases of typhoid fever were proved to have directly infected this small brook, which joins the river only two and a half miles above the intake of the Lowell Waterworks. Dr. Sedgwick states in his report that "the Merrimack River is regularly polluted above Lowell, not only by Stony Brook, but very extensively by the large cities of Nashua, Manchester, Concord, and Fitchburg, the sewers of all of which pour their raw contents directly into the Merrimack River or the Nashua. This they had been doing for months and years; and to the fact that Lowell has been willing to drink this regularly polluted water, *totally unpurified by filtration*, is chargeable the fact that typhoid fever has annually been excessive in that city. But the conditions were no worse than usual in these cities in September and October 1890. There was, however, as has been shown, an infection of a small and seemingly insignificant feeder of the Merrimack only two and a half miles above the intake of the Lowell Water Works, such as is not known to have occurred there for forty years." With respect to the chemical and bacterial examinations of the water, Dr. Sedgwick writes as follows:—

"These were made in the hope of discovering some unusual condition of the river, or of possibly detecting the Eberth bacillus itself. But, as usually happens in typhoid fever epidemics, the worst was over before the examinations began. The chemical examinations showed nothing that was not already known. The bacterial analyses revealed a noteworthy excess of *Bacillus coli communis*, confirming the chemical evidence of the presence of sewage in the city water as drawn from the river, but no Eberth bacilli were found."

We do not, however, attach perhaps the same importance as Dr. Sedgwick to the detection of the *B. coli communis* in the water, as we believe that this organism, or forms closely allied to it, may be found far more frequently present in pure peaty or other upland surface waters than is usually surmised, and that such microbial forms are not *necessarily* indicative of the access of sewage to a given water-supply.

The epidemic of typhoid fever, which apparently started above Lowell, infecting the Merrimack River, supplying that city with water, subsequently extended to Lawrence, situated on the same river, nine miles below Lowell, and also using its waters for drinking purposes. In this connection Dr. Sedgwick remarks: "Inasmuch as there is good reason to believe that this unusual epidemic was caused by the unusual infection of the river at North Chelmsford and at Lowell, it is interesting to observe that some of the infectious material was apparently able to survive the comparatively unfavourable conditions imposed by the long and slow passage through the Lawrence reservoir and the service pipes." [The "unfavourable conditions" here referred to are the processes of sedimentation to which the microbial contents of the water would be exposed under these circumstances.] "It would seem therefore that, while much of it must have perished *en route*, some of it did not; and, as the time of year was November and December, we are safe in concluding that during these months, under certain conditions, some of the infectious material of typhoid fever may be conveyed nine miles by a river, may slowly travel through a distributing reservoir, and still remain effective to a very dangerous extent if swallowed in drinking water." During the four years preceding 1891, the average mortality from typhoid fever in Lowell is stated to have been 8·44 per 10,000, whilst in 1890-91 it rose to 19·54 per 10,000. Thus the average death-rate from typhoid fever is considerably higher than we experience in London, but it sinks into comparative insignificance when we contrast it with the statistics of typhoid fever in Chicago compiled by Dr. O. M. Huff, of that city. In 1891 the deaths directly attributed to typhoid fever in Chicago amounted to no less than 16·64 per 10,000. In 1892 the condition of things was somewhat improved, although the death-rate, says Dr. Huff, still remained three times as great as in New York, five times as great as in London, and more than six times the rate of Berlin. Dr. Huff has made a minute study of the relation of typhoid fever to the water supply of Chicago, and has come to the conclusion that the contamination of the drinking water supplied to the city with sewage is the "efficient cause" of this alarming mortality. It is stated that Lake Michigan "serves both as a water-bucket and a cesspool for Chicago." An American scientific journal, in reviewing this report, reasonably suggests that every resident of Chicago ought to be advised of the fact that there is death in the water-pipe.

It is to be hoped that the attention now being bestowed in America on subjects both directly and indirectly connected with public health will lead to beneficial practical results, and that the distribution of water openly contaminated with sewage in its raw, unfiltered condition for drinking purposes, will be summarily prohibited by law in all countries before such grave consequences have again to be met as attended the distribution in Hamburg of raw, unfiltered river Elbe water for dietetic purposes.

G. C. FRANKLAND.

AN ACCOUNT OF THE CONSTRUCTION AND STANDARDISATION OF APPARATUS, RECENTLY ACQUIRED BY KEW OBSERVATORY, FOR THE MEASUREMENT OF TEMPERATURE.

THE accuracy of the measurements made at Kew Observatory may, without exaggeration, be regarded as a matter of national concern. It is right, therefore, that the scientific public should be made acquainted with the principles involved and the methods of comparison employed in any series of measurements conducted at the Observatory; more especially when a new departure is made, either in the apparatus used or in the nature of the observations.

In the absence from England of Prof. Callendar, F.R.S., the writer, at the request of the Kew Committee, undertook the responsibilities connected with the preparation and standardisation of the apparatus, recently installed at Kew, for the accurate measurement of temperatures—particularly of high temperatures.

It would be impossible, without unduly trespassing upon these pages, to give a full description of the principles on which the measurements of temperatures by platinum thermometers are founded, or of the methods of standardisation adopted. I will, however, endeavour to briefly indicate reasons for our faith in the principles involved and the accuracy of the methods employed.

I make this communication with the (unofficial) consent of the Kew Sub-Committee, to whom the oversight of this matter was delegated; at the same time it should be understood that the writer alone is responsible for the statements, or opinions, advanced in the following pages.

Sir Douglas Galton in his address at Ipswich remarked that "British students of science are compelled to resort to Berlin or Paris when they require to compare their more delicate instruments and apparatus with recognised standards." We may now hope, however, that, at all events as regards temperature measurements, his statement will ere long require modification.

I. *Brief Explanation of the Terminology and of the Principles involved in the Measurements of Temperature by Platinum Thermometers.*

A platinum temperature scale is one so constructed that a rise of one degree on that scale at any temperature would cause the electrical resistance of a platinum wire to increase by onehundredth of the difference between its resistance at 100° and 0° C.

Hence, if R be the resistance at any temperature, R_1 the resistance at 100° C., R_0 at 0° C., and *pt* the temperature on the platinum scale, then

$$pt = \frac{R - R_0}{R_1 - R_0} \times 100.$$

The investigations of Prof. Callendar[1] established the relation between *pt* and *t* (where *t* is the temperature on the air scale) over the range 0° to about 60c° C. for a particular sample of platinum wire.

This relation is given by the following equation.

$$d = t - pt = \delta\left\{\left(\frac{t}{100}\right)^2 - \frac{t}{100}\right\} \; . \; . \; . \; . \; \text{Eq. } (d)$$

the value of δ for Callendar's wire being 1·57.

If it was at all times possible to obtain platinum wires of *exactly* the same degree of purity as Callendar's, we could at once establish a standard platinum scale, which could be used for purposes of reference independently of any assumptions as to its relation to the air scale. The impossibility of securing uniformity in this respect, however, would, at first sight, appear to be an insuperable impediment to the adoption of such a proposal.

Subsequent experiments by Callendar and myself led, however, to the following conclusion.[2]

That, although the value of δ varies greatly according to the purity of the sample of platinum, the relation given by the equation (*d*) holds true, provided the percentage of impurities is small (this condition is sufficiently fulfilled by ordinary commercial samples).

This conclusion is an exceedingly important one, for (the *t* – *pt* curve in every case being a parabola) it is only necessary to determine the resistance at three different temperatures in order to ascertain the appropriate value of δ, and thus to completely standardise the thermometer.

Much experimental work had to be accomplished before we could venture to regard the above proposition as established; but I think that any impartial reader, who cares to study the original papers[3] dealing with this matter, will admit that the evidence is sufficient.

The three temperatures selected for the purposes of standardisation were the melting-point of ice, steam at a pressure of 760 m.m., and the vapour of sulphur at the same pressure.

Certain discrepancies between thermometers thus standardised and others standardised by direct comparison with the air thermometer, led to a redetermination (by means of an air thermometer) of the boiling-point of sulphur, when we found that Regnault's value (448°·34) was too high, our experiments leading to the conclusion that 444°·53 was the correct value.[4]

Subsequent investigations by different observers have confirmed the accuracy of the above conclusions, which may now be regarded as experimentally established over the range 0° to 600° C.

There is, however, a large amount of indirect evidence which indicates that formula (*d*) holds true over a far more extended range.

For example, the results obtained by Messrs. Heycock and Neville (*Chem. Soc. Trans.*, 1895) are entirely dependent on the validity of the above conclusions. They find the freezing-point of copper as 1080°·5 C., whereas Holborn and Wien, using a platinum rhodium couple standardised by direct comparison with the porcelain air thermometer, find 1082° as the value of the same constant.[1]

As illustrating the identity of the results obtained by the use of thermometers having a very different value of δ, I quote the following numbers from Table XII. of Heycock and Neville's paper:—

Pyrometer.	Value of δ.	Freezing-point of gold. °
13	1·500	1061·9
15	2·040	1061·2
18	1·574	1061·4
13A	1·553	1061·9
14	1·511	1062·0

Results of this kind prove that even if the reduction does not express the temperature accurately in the air scale, it at all events gives us a *constant* scale in which all high temperatures can be expressed, and it is further evident that this constant scale differs but little (even at these high temperatures) from the true air scale.[2]

Indications are not wanting that the same relations hold true at very low temperatures.[3]

Finally, a very careful comparison of the platinum and air thermometers over the range 0° to 100° C., and also of the platinum thermometer with the nitrogen standard of the Bureau International, establishes the validity of the methods of observation and reduction at ordinary temperatures.

As regards the constancy of platinum thermometers there should now be little uncertainty. The prevailing doubt (amongst those who have not used them) may be traced to the adverse report of a British Association Committee in 1874, on another form of the instrument, and I would refer those who may be influenced by that report to a letter by Prof. Carey Foster, F.R.S., in NATURE, August 23, 1894.

An inspection of the voluminous tables given in Heycock and Neville's paper (*supra*) will show, however, that when the thermometers are repeatedly exposed to temperatures above 900° or so, a slight permanent increase in FI (the Fundamental Interval = $R_1 - R_0$) is observable. It is probable that this change is due to a permanent thickening of the mica plates by which the wire is supported, and thus, on cooling, the wire is slightly strained. The change is small, and can always be traced by repeating the determinations of R_1 and R_0, and does not appear to appreciably affect the values of δ.

To show the order of magnitude of the change, I give the following illustration, compiled from Table VIII. of Heycock and Neville's paper.

History of Pyrometer 13.

On August 3, 1894, the fundamental interval was 100·64. During the next few months this pyrometer was used for the determination of the freezing-points of the following substances:—

Substance.	Number of determinations.
Silver	10
Aluminium	12
Potassium sulphate	5
Sodium sulphate	4
Sodium carbonate	3
Magnesium	5
Antimony	2
Tin	3
B.P. of sulphur	6

Also the pyrometer had been raised to a bright red heat in a muffle furnace some scores of times, and the exterior porcelain

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[1] *Phil. Trans. Roy. Soc.* A, 1887.

[2] A summary of these experiments is given in *Science Progress*, September 1894.

[3] Callendar, *Phil. Trans. Roy. Soc.* A, 1887; Griffiths, "Report of Electrical Standards Committee, B.A. 1890; Heycock and Neville, *Chem. Soc. Journ.* 1890; Griffiths, *Phil. Trans. Roy. Soc.* A, 1891; Callendar and Griffiths, *Phil. Trans. Roy. Soc.* A, 1891; Callendar, *Phil. Mag.*, July 1891; Griffiths and Clark, *Phil. Mag.*, December 1892; Griffiths, *Phil. Trans. Roy. Soc.* A, 1893; *ibid.*, *Proc. Roy. Soc.* vol. lv. 1894; *ibid.*, *Science Progress*, 1894; Thorpe, "Dictionary of Applied Chemistry," article "Thermometry"; Heycock and Neville, *Trans. Chem. Soc.* 1895.

[4] In the last edition of Watts's "Dictionary of Chemistry," article "Sulphur," I find that some doubts are expressed (by Mr Pattison Muir) as to the validity of this determination, owing to uncertainty as to the purity of our sample of sulphur. I subsequently investigated the boiling-point of a specially pure sample by means of one of the platinum thermometers (thermometer E) used during the original comparison of the air and platinum thermometers in sulphur vapour, and I found no evidence of any difference in the boiling-point of the two samples. We may assume, therefore, that if any impurities were present, they were not of such a nature as to influence the temperature of the vapour.

[1] The following example illustrates the importance of the alteration in the boiling-point of sulphur. In Table VI. of Heycock and Neville's paper (*supra*) are given the details of an observation on the freezing-point of Cu determined by pyrometer No. 8. They are as follows: $pt_s = 421·29$, $\delta = 1·517$, $d = 159·3$, $t = 1080·7$. If we assumed the validity of Regnault's boiling-point of sulphur (448°·34), the above value of pt_s would change the value of δ to 1·729; this would give $d = 181·6$; hence $t = 1103°·0$. In this case the discrepancy between the results of Holborn and Wien, and Heycock and Neville would be very marked—a difference of 21°·0 as against the present difference of 1°·3.

[2] Messrs. Heycock and Neville, in Table XVI. (*Chem. Soc. Trans.*, 1895, p. 195), give Violle's value for the freezing-point of gold as 1035° C., and the discrepancy between this number and that found by them (1061°·7) is considerable. A redetermination, however, by Violle in 1892 (*Comptes rendus*, 92, p. 866) raised his number to 1045°. Some recent experiments by Le Chatelier (*Comptes rendus*, August 12, 1895) lead that observer to the conclusion that Violle's later value should be further raised by about 15° (or at all events by a number "not exceeding 20°"), *i.e.* to about 1060° C., a very close approximation to the 1061°·7 found by Heycock and Neville in December 1894.

[3] Griffiths and Clark, *Phil. Mag.*, December 1892.

tube had on three occasions been removed and replaced by a new one.

At the close of these operations (December 19, 1894) the value of FI had risen to 101·003, an increase of 0·36 per cent.

It should be remembered that in each determination the substances were raised to 50° or 100° above their freezing-points before the observations were taken; for example, the freezing-point of potassium sulphate is given as 1066° C., but it is certain that when determining this point the pyrometer was previously raised to a temperature considerably exceeding 1100°. A study of the original table will show that the rate of increase in FI diminishes with use.

As this question of constancy is of vital importance, Messrs. Heycock and Neville have given me permission to state that they have used only one pyrometer during a continuous series of high-temperature determinations extending over two months of the past summer. When the account of their work is published, it will be found that although the number of the observations on the freezing-points of alloys exceeds some hundreds, the pyrometer is as efficient now as at the commencement of their work. Its FI on July 28 was 100·148, on August 20, 100·357, and on nearly all the intervening days it had been immersed in molten metal at temperatures between 900° and 1000° for five or six hours at a time. A determination of the freezing-point of copper, made at the close of the above series of experiments, gave a value practically identical with that previously published.

Apart from the slight change in FI, above illustrated, there is abundant evidence that when *completely protected from the action of furnace gases* the platinum wire undergoes no change. Space does not permit the accumulation of further evidence, but full information will be found in the papers already referred to.

II. *Description of the Apparatus.*

The facts dwelt upon in Section I. show that if the methods of platinum thermometry are adopted, the measurement of temperature becomes a question of the measurement of electrical resistance, and there are few physical quantities which can, if due precautions are taken, be measured with greater accuracy than the resistance of a conductor. The Kew apparatus, therefore, may be regarded as designed for the accurate measurement of the resistance of a platinum wire, and some of the contrivances introduced with the object of securing greater accuracy are, I believe, peculiar to this apparatus.

The designs were drawn up by Prof. Callendar and myself, after consultation with Mr. Horace Darwin, and the apparatus was constructed by the Cambridge Scientific Instrument Company, Ltd., under the personal direction of Mr. Pye.

Fig. 1 is a diagrammatic view of the connections.

The coils S_1 and S_2 are of equal resistance (about 5 ohms), Q is a set of resistance coils, A B a bridge-wire, and K a thermo-electric key. When the resistances between C_1 and C_2 and P_1 and P_2 are equal, the bridge is balanced if the resistance at Q is zero, and the contact-maker H is at the mark O near the centre of the bridge-wire. The scale of this wire is so graduated that if the reading to right or left of O be added or subtracted from r (the resistance at Q), the result gives the value of P – C where P is the resistance between P_1 and P_2, and C the resistance between C_1 and C_2.

Now $P = p + C_p$, where p is the platinum coil resistance, and C_p the resistance of the leads to that coil, including the thick platinum wires which run down the thermometer stem. An equal pair of leads run from C_1 C_2 to similar thick platinum wires in the thermometer stem, which are connected together at the lower extremities, but have no contact with the coil.

Thus $r \pm OH = p + C_p - C$; and therefore if $C_p = C$, we get $p = r \pm OH$.

The leads C_p and C are everywhere bound together except in the thermometer stem, where they are parallel and adjacent, being held in position by their mica discs, hence changes in C_p and C caused by changes in temperature do not affect the resulting value of p, and thus the readings are independent of the thermometer stem-temperature—a matter of great importance at high temperatures.[1]

A certain amount of stem immersion is, however, necessary, for the lower extremities of the leads must be heated to the bulb temperature, otherwise they would, by conduction, cool the extremities of the coil; this is an additional reason for forming the leads of platinum, which has a low thermal conductivity.

A preliminary series of experiments led to the conclusion that a certain quality of white marble had superior insulating properties to ebonite—the material generally used for the tops of resistance-boxes. This superiority was partially due to its non-hygroscopic properties; for example, I placed slabs of the best ebonite, black marble, and this white marble in an ice-safe for some time. I then removed them one by one to the warm laboratory, and tested them under similar conditions with a "pressure" of 100 volts. The insulating powers of the ebonite and black marble fell off alarmingly, while the white marble was but little affected.

Some difficulty was experienced by the makers in devising a satisfactory method of attachment between the marble and the many brass connections, &c., but this difficulty was at length overcome. The coil and bridge-wire were constructed from one sample of platinum silver. The coil of a platinum thermometer was replaced by a specimen of the wire (diameter ·008 in.) from which the coils were formed, and which had been subjected to the same process of annealing. Its temperature coefficient was then determined with great care over the range 15° to 25° C. (nitrogen scale), and was found to be ·000260 in terms of the resistance at 20° C. I proposed to keep the box when in use at a temperature near 20° C., for it is wise, when feasible, to maintain all measuring instruments at a temperature exceeding that of an ordinary room, for two reasons: (1) it is generally easy to raise the temperature of the apparatus above that of the air, whereas it is extremely difficult to keep it at a lower temperature; (2) when the temperature of the apparatus exceeds that of the room, all its surfaces are kept dry, and also more dust-free than would otherwise be the case.

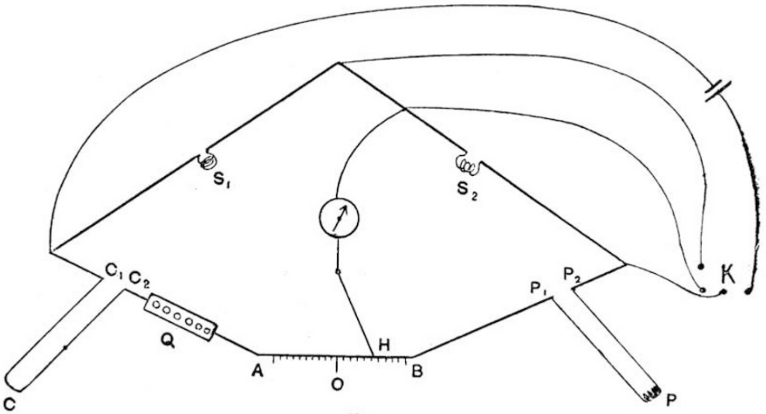

Fig. 1.

The greatest difficulty encountered in resistance measurements is (according to my experience) uncertainty as to the actual temperature of the coils. If a resistance-box is placed in a tank, it is true that five sides of it can be maintained at a constant temperature, but the top is necessarily exposed; and since all the coils are ultimately connected with the top, their temperature at times differs considerably from that of the tank.

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[1] The *absolute* equality of C and Cp is not essential; both are small as compared with p $\left(\frac{C}{p}\right.$ is always less than $\left.\frac{1}{50}\right)$, thus $Cp - C$ is a very small fraction of p, and it is only the temperature change in $Cp - C$ that affects the measurements. The total resistance of the thick copper leads from the box to the thermometer is so small, and they are subject to such comparatively slight changes of temperature, that the temperature change of their difference may almost certainly be neglected. The greater part of Cp and C is the resistance of the platinum stem leads, which are certainly exposed to considerable temperature changes. If, however, they are made of the same platinum as the coil, then any irregularity has nearly the same effect as an alteration in the original length of the coil, and does not appreciably affect the ratio $\frac{R_1}{R_0}$ or the values of pt. In all carefully constructed thermometers the value of $Cp - C$ may be regarded as zero.

Again, the thick coating of paraffin, and the solid core, which are almost universally prevalent, increase the uncertainty, for if any temperature change is taking place the lag is considerable.

These sources of error were diminished as follows: the sides and bottom of the box are the inner walls of a double copper tank, holding about eight gallons of water maintained at a temperature near 20° C. by a regulator. Over the top of the box and tank is fixed a case similar to that of an ordinary balance, the front glass of which is only raised when adjustment of plugs and contact-maker is necessary, all connecting screws being exterior to the case. The silk-covered coils (which are double in all cases to reduce the effects of current heating) are suspended from an ebonite rack within the box. They received the thinnest possible coating of paraffin and are wound loosely and openly, so as to be subjected to no strain, and also to permit free circulation of air to all parts of the wires. An open scale mercury thermometer, with a small bulb, is placed with its bulb contiguous to one of the higher resistance coils. A series of observations leads to the conclusion that, under these circumstances, the thermometer-reading sufficiently indicates the real temperature of the coils, provided of course that no rapid changes are taking place, and this conclusion is borne out by the observations of Messrs. Heycock and Neville on a box similarly protected by a balance case.

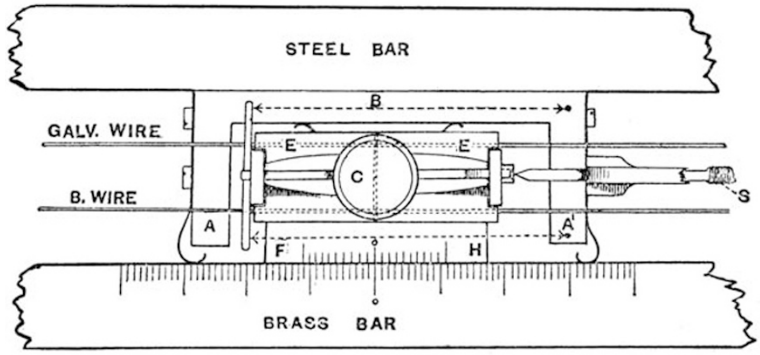

FIG. 2.

The coils were annealed in paraffin at a temperature slightly below that which would carbonise the silk covering.

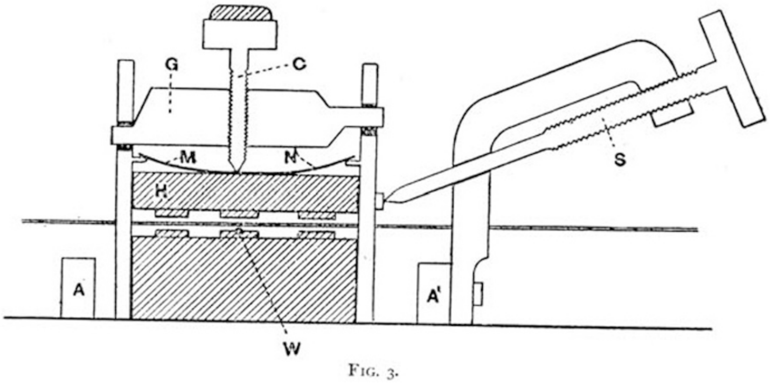

FIG. 3.

The box-unit is (approximately) $\frac{1}{100}$th of a Board of Trade ohm. The coils have the following nominal values in terms of this unit.

A	B	C	D	E	F	G	H	FI
640,	320,	160,	80,	40,	20,	10,	5,	100,

and the bridge-wire has such a section that a change of 1 c.m. in the reading indicates (approximately) a change of 1 unit in *p* (in reality 1 c.m. = ·9957 box units), and the resistance of the thermometers, with one exception, is such that a change of one box unit corresponds to 1° on the platinum scale.

The whole length of the bridge-wire is 30 c.m.; thus any resistance exceeding 40 units can be measured in three or four different ways by changing plugs and bridge-wire contact. Thus the accuracy of the various corrections can at any time be exposed to a severe test.

Great attention was given to the drawing of the bridge-wire, for, although the effect of irregularities would be eliminated by the subsequent calibration, it was desirable to make it as uniform as possible. In this matter the Scientific Instrument Company were very successful, for it was ultimately ascertained that if the wire was assumed as uniform, the greatest resulting error would not exceed 0·005 units.

Before its final attachment to the box, the wire was hung from a conductor, and had a small weight fastened to the lower end, which communicated with a cup of mercury; it was then raised to a bright red heat by means of an electric current, after which the cells were gradually switched off, so that the cooling was slow. The annealing was thus very perfect, and the wire on being released remained quite straight. The coefficient of expansion of this platinum-silver alloy lies between that of steel and brass. A narrow parallelogram was formed, whose longer sides consisted of brass and steel bars respectively—the shorter sides of ebonite. The steel and iron bars were connected at their centres to the marble box top, and the wire placed between, and parallel to, them, its ends being fixed to the ebonite cross-pieces and connected by flexible brass strips to the remainder of the bridge. By this arrangement the tension of the bridge-wire is kept constant when the temperature of the box alters, and, at the same time, the zero-point at the centre remains unchanged in position.

A second platinum-silver wire, permanently connected with the galvanometer, lies alongside the true bridge-wire. The vernier-slide carries a small cross-bar of the same wire placed beneath and at right angles to the bridge and galvanometer wires, and only just clearing them (Fig. 3). When the contact-screw is either forced, or screwed down, both wires are pressed on to the cross-piece by means of pads, and arrangements are made to prevent any pressure being exerted which could injure the bridge-wire. This method has several advantages, one of which is that only similar metals are brought in contact, and thus thermo-electric effects at this junction are avoided.

The vernier reads directly (by means of a microscope) to $\frac{1}{50}$th m.m., thus $\frac{1}{100}$th m.m. can be estimated; *i.e.* approximately 0·001 box units, or 0·00001 ohms.

Great difficulty has hitherto been experienced in constructing a fine adjustment for a bridge-wire contact. It must be of such a nature that it will permit the free movement by hand of the contact-maker to any position. Again, if, owing to an oversight the contact-maker is screwed down, and any of the ordinary means of fine adjustment are used, the bridge-wire is subjected to a scraping action which may affect its section. These difficulties have been overcome by an ingenious device designed for this apparatus by Mr. Horace Darwin.

Fig. 2 is a plan, and Fig. 3 a vertical section of the contact-maker.

A B A′ (Fig. 2) is a brass framework which slides between the steel and brass bars previously referred to. An inner block F E E′ H stands within the brass framework with a play-space at its ends of about 1 c.m. Springs at A and A′ press the brass frame against the steel bar, and springs at E, E′ press the inner block against the front brass bar. Thus the pressure of the brass frame against the steel bar is the sum of the pressures of the springs at A, A′, E and E′, whereas the pressure of the inner block on the front bar at F H is the sum of E and E′ only; if, therefore, the screw S is rotated the inner block alone is moved. As the screw S recedes the inner block is made to follow it by means of long springs indicated in the plan by the dotted lines with arrow-heads. If by inadvertence S is turned when the bridge-wire has not been released by the screw C

(Fig. 3), then the outer framework A B A′ moves instead of the inner block.

The arrangement by which the pads in the block H can be either pressed down for temporary, or screwed down for more permanent, observations is shown by the section in Fig. 3. The spring M N lifts the pads off the bridge and galvanometer wires, which therefore do not touch the cross-wire (whose section is shown at W) unless a downward pressure is exerted on the block H.

By holding the head of the screw S the whole contact-maker can be pushed to any desired position.

The vernier is shown at F H (Fig. 2).

The box contains coils of 20 and 100 ohms, which can be thrown into the battery circuit by means of a switch, and also a galvanometer shunt of about $\frac{1}{10}$ the galvanometer resistance.

With the exception of the points to which I have drawn attention, the resistance-box resembles those ordinarily in use.

The galvanometer has a resistance about 5 ohms, and is sensitive and "dead-beat." A fixed scale is placed before the mirror, and the image viewed through a microscope. Very small deflections can thus be observed, and observations can be taken in bright daylight.

This last has a coil whose resistance is 2·5 times as great as he preceding ones.

All these thermometers have been annealed at a temperature of about 1000° C., Nos. 5 and 6 being temporarily placed in porcelain tubes for that purpose.

The apparatus for the standardisation in ice, steam, and sulphur-vapour presents certain distinctive features, most of which, however, are described in *Phil. Trans. Roy. Soc.*, vol. 182 A.

Messrs. Heycock and Neville were so kind as to undertake the design and arrangement of the furnaces, &c., for the high temperature work, which are, in the main, similar to those used during this summer for the purposes of their investigations into the behaviour of alloys. As an account of their work will shortly be published, I shall not venture to anticipate it by any description. Arrangements have been made for (1) the standardisation of thermometers by observations on the freezing-point of silver when placed in a reducing atmosphere;[1] (2) the comparison of the Kew standards with other thermometers over the range 100° to 300° C. by means of a well-stirred bath of a fusible metal covered with paraffin or oil; (3) the comparison over a range 300° to 1200° C. in a bath of melted tin placed in a reducing atmosphere.

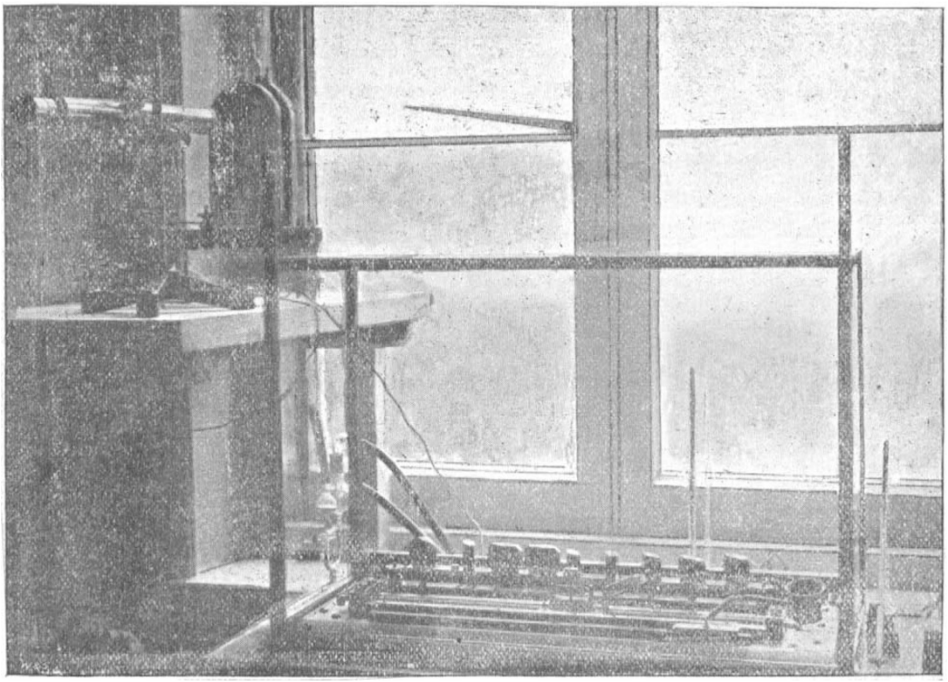

FIG. 4.

Thermo-electric effects (which at high temperatures are occasionally considerable) are eliminated by the use of a special key described in *Phil. Trans. Roy. Soc.*, vol. 184 A, p. 397.

The leads from the box to the thermometer are about five yards in length, and are each composed of 136 strands of copper wire carefully insulated. The thermometers can thus be used in any part of the room.

Six thermometers have been supplied, all of whose coils are formed from the same sample of specially pure platinum wire—diameter 0·006 inches. The length of wire in Nos. 1 to 5 is about 18 inches, the mica framework which supports the coil is from 1¼ to 1½ inches in length, and this portion is termed the bulb.

Nos. 1 and 2 are contained in porcelain tubes 1·2 c.m. in diameter, 40 c.m. long.

Nos. 3 and 4 are contained in porcelain tubes 1·5 c.m. in diameter, 36 c.m. long.

No. 5 is contained in a glass tube 1·4 c.m. in diameter, 40 c.m. long.

No. 6 is contained in a glass tube 1·7 c.m. in diameter, 40 c.m. long.

The whole of the apparatus is placed in a special building erected for this purpose by the Kew Committee, according to designs by Mr. W. N. Shaw, F.R.S., and myself, after consultation with Messrs. Heycock and Neville. The building is found to admirably fulfil its purpose, the only drawback being the defective gas supply, which will, however, shortly be remedied by the insertion of a larger main between the Observatory and the outbuildings.

Fig. 4 (a copy of a photograph by Mr. Hugo) gives the relative positions of box and galvanometer. It will be seen that the observer can manipulate the contact-maker without removing his eye from the galvanometer-microscope, a great advantage when observing small temperature changes. Immediately to the right of the resistance box, but exterior to the limits of this plate, is a large draught-chamber containing the furnaces, &c.

III. *The Standardisation of the Apparatus.*

The necessary operations were as follows:—

No. 1. Determination of the temperature coefficient of coils and bridge-wire.

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[1] See letter to NATURE, October 17, 1895.

No. 2. Calibration of bridge-wire and determination of error due to the position of the scale zero mark.

No. 3. Determination of the coil errors.

No. 4. Value of the mean box-unit in terms of the Board of Trade ohm. (This was *not* a necessity for the temperature measurements, but it appeared advisable to ascertain it.)

No. 5. Determination of R_1 R_0 and δ for each thermometer.

It is impossible to give any full description of the operations—I can but indicate the methods adopted.

No. 1 has already been described *supra.*

No. 2. A length of 4 c.m. of an unused portion of the bridge-wire was soldered across the thick posts supporting the coil marked FI; thus if balance was obtained with the plug FI in, its withdrawal would compel a movement of about 2 c.m. in the contact-maker to restore equilibrium. As the constitution of both wires was the same, and as they were included in the glass case, the length moved by the contact-maker was unaltered by changes in temperature.

C_1 C_2 (Fig. 1) were connected by a thick copper strip, while p_1 and p_2 were connected with the terminals of a resistance-box (r_1) having a slightly larger unit than the Kew box, and the extremities of r_1 were connected with two other boxes (r_2, r_3), of which r_2 could be altered by certain steps from 5 to 20,000 ohms, and r_3 by any quantity from 0·1 to 10,000 ohms; thus r_2 and r_3 may be regarded as shunts to r_1. All three boxes were maintained at a constant temperature.

This arrangement was adopted to enable a balance to be obtained when the contact-maker was at, or very near any given position; for example—suppose contact desired at reading + 15, r_2 and r_3 were made as large as possible, and r_1 adjusted until the bridge-wire reading somewhat exceeded 15; r_1 was then reduced until the reading but slightly exceeded 15. Reductions of some hundreds of ohms in r_3 would now cause but small alteration in the combined resistance of the three boxes, provided r_3 greatly exceeded r_1 and r_2 when in parallel arc. It was thus always found possible and easy to balance so that contact was nearer than 0·5 m.m. to any desired position.

The plug FI was now withdrawn, and the corresponding movement of the contact-maker noted. Repeated observations throughout the whole length of the wire were thus taken, not only by the writer, but also by Mr. G. M. Clark, who performed an independent series. Denote the resistance of the wire across FI by U, then the reciprocals of the observed lengths give in terms of U the effective resistance per c.m. of the wire at the middle point of each range. By means of the arrangement described above, the observations were so conducted that these middle points fell almost exactly on the integral numbers of the scale, and, by plotting, the exact values at the integers were obtained.

By repeating the process with the plug H the value of U in terms of H could be accurately ascertained, and a check was also obtained on the previous observations. The value per c.m. for the whole wire could thus be found in terms of H. Later in the operations, when the value of H was known in terms of the mean box unit, the b.-w. values were expressed in terms of the mean unit and integrated on each side of the zero mark.

No. 3. The effect of any inequality in the bridge arms of S_1 S_2 (Fig. 1) could not be eliminated by means of the Correction Tables, nor could such an error be easily detected by means of the box itself, as is the case in the remaining coils. Great attention was, therefore, devoted to securing equality. It is certain that they do not differ by 1 in 100,000.

The errors of coils A to H and FI (the temporary connection across FI having been removed) were now determined by a method originally adopted by Prof. Callendar. No great efforts were made to secure the identity of these coils with their nominal values, for it was certain that some small corrections would in any case be necessary, and as the magnitude of the correction in no way increases the labour of calculation, the time and energy expended in any exact adjustment would have been wasted.

The procedure was as follows:—

All plugs were inserted, and the balance obtained with the contact-maker at any convenient position by adjusting r_1, r_2, r_3, as previously described. H (5) was then removed, and the change in reading required to readjust balance observed. Let Z_1 be the consequent change in reading; r_1, r_2, r_3 were then altered until the contact-maker was brought back to about the same position as it occupied when H was in. H was then inserted, and G (10) removed. Let change = Z_2; contact was again brought to first position, D (20) removed, and G and H replaced. Let change = Z_3, &c.

When the process is completed we thus get a series of equations

$$A - (B \text{ to } H) = Z_8$$
$$B - (C \text{ to } H) = Z_7$$
$$C - (D \text{ to } H) = Z_6, \text{ \&c.}$$

By subtraction we then get

$$A - 2B = Z_8 - Z_7$$
$$B - 2C = Z_7 - Z_6, \text{ \&c.}$$

Now the values of Z_1 Z_2, &c., *in terms of H* are already known from the previous operations, hence A, B, &c., in terms of H can be found.

As the right-hand extremities of the intervals Z_1 Z_2, &c., were approximately in the same position, nearly the same portion of the bridge-wire was used throughout; thus any errors in the previous calibration would but slightly affect the results.

Knowing the values of all the coils in terms of H, it is then easy to express them all in terms of the mean coil, and hence in terms of the mean box unit, a corresponding correction being made in the integrations of the bridge-wire.

The zero error of the scale was next determined by reducing the resistances between C_1, C_2, and P_1, P_2 (Fig. 1) to zero, and replacing all plugs. The observations were checked by reversing all the connections. The displacement was found to be + 0·005 c.m.

No. 4. Finally the sum of all the coils was determined by means of a dial box, of whose comparison with the B.A. Standard full particulars are given in *Phil. Trans.* A, 1893, pp. 407–410. The result was that the mean Kew box unit at 20° C. = 0·0099993 Board of Trade ohms.

At the conclusion of the standardisation a large number of readings of the same resistance were taken by different observers with different combinations of coils and b.-w. readings. The accuracy of all the corrections was thus exposed to a crucial test, the results of which were satisfactory.

No. 5. The standardisation of the thermometers was performed after the installation of the apparatus at Kew, the previous observations having been made in my own laboratory.

I find that many misapprehensions are prevalent as to the nature of the operations, and it may therefore be of assistance to observers who standardise their own thermometers, if I give a complete example of one set of the observations as taken at Kew, together with their reductions. I select thermometer K_2, as several observers took part in its standardisation, and it therefore well illustrates the order of accuracy obtainable.

The numbers in italics show the corrections resulting from the standardisations of which an account has been given. The times are always entered, since the observations of the barometer cannot be taken simultaneously with the temperature measurements, and it is necessary, therefore, when working with steam and sulphur, to form a time-chart by which to ascertain the correct pressure at the moment of observation.

Thermometer K_2. *Determination of* R_0.

Date and observer.	Time.	Coils and correction.	B.-wire and correction.	Temp. box and correction.	R_0
Oct. 2, 1895 C.T.H.	11.47	C.D.F. = + 260 + *·035*	− 2·302 + *·004*	21·38 + *·092*	257·829
E.H.G.	11.50	,,	− 2·306 + *·004*	21·36 + *·091*	257·824
W.H.	11.57	C.D.F.H. = 265 − *·023*	− 7·260 + *·022*	21·33 + *·089*	257·828
C.T.H.	12.6	,,	− 7·263 + *·022*	21·33 + *·089*	257·825
W.H.	12.13	C.D.H. = 245 + *·014*	+ 12·784 − *·059*	21·32 + *·088*	257·827
			Mean ...	...	257·827

The separate determinations were entirely independent, and taken by three different observers; the coils were so changed that the b.-w. readings altered from − 7·263 to + 12·784, while the sum of the corrections varies from + 0·131 to + 0·043, yet the greatest departure from the mean = 0·003.

Thermometer K_2 *in Steam.*

Date and observer.	Time.	Coils and correction.	B.-wire and correction.	Temp. box and correction.	Bar. and temp. F.	R^1
Oct. 2, 1895 C.T.H.	12.37	C.D.F.FI. = 360 − *·008*	− 2·820 + *·007*	21·30 + *·121*	29·602 at 61·2	357·300
E.H.G.	12.42	,,	− 2·824 + *·007*	21·30 + *·121*	29·600 at 61·2	357·296

The barometer reading at 12.37 corrected for temperature, and for g to sea-level, lat. 45° = 750·12 m.m., and at 12.42 = 750·06 m.m. (This difference of ·06 m.m. corresponds to a decrease of 0·002 in R.)

Hence mean pressure = 750·09 m.m., and temperature of steam at this pressure = 99°·634 C.

$\therefore \dfrac{347{\cdot}298 - 257{\cdot}827}{99{\cdot}634}$ = *mean* change in R per 1° C. over this range.

Now $\dfrac{\delta pt}{\delta t} = 1 - \delta \dfrac{2t - 100}{10{,}000}$. We may assume δ for this wire as approximately 1·50.

Hence

$$\frac{\delta pt}{\delta t} = 0{\cdot}985, \quad \therefore \frac{\delta R^1}{\delta t} \text{ at } 100^\circ = {\cdot}9982 \times {\cdot}985 = {\cdot}983,$$

$$\therefore \delta R^1 \text{ for } 0{\cdot}366^\circ \text{ C.} = 0{\cdot}360.$$

Hence

$$R_1 = 357{\cdot}298 + {\cdot}360 = 357{\cdot}658.$$

Thermometer K_2 *in Sulphur-vapour.*

Date and observer.	Time.	Coils and correction.	B.-wire and correction.	Temp. box.	Mean bar. and temp. F.	R.
Oct. 2, 1895 E.H.G.	1.50	A.F.G.H. = 675 − *·207*	+ 2·789 − *·013*	21·15 + *·203*	29·610 at 61·5	677·772
E.H.G.	1.53	,,	+ 2·796 − *·013*	21·14 + *·201*		677·777
E.H.G.	1.58	A.E. = 680 − *·089*	− 2·343 + *·004*	21·13 + *·201*		677·773
W.H.	2.20	,,	− 2·322 + *·004*	21·04 + *·183*		677·776
					Mean	677·775

Here, again, changes in coils and b.-w. readings do not appreciably affect the results. It is interesting to notice that the change in box temperature between 1.58 and 2.20 p.m. almost exactly accounts for the difference of 0·021 in the b.-w. readings by the different observers. As the apparatus had but just been installed, we had not got the regulator properly under control; thus the temperature changes were greater than would usually be the case.

Barometer, corrected as before = 750·30 m.m.

Now, b. p. of sulphur

$$= 444{\cdot}53 + (p - 760) \times {\cdot}082 = 443{\cdot}73,$$

and

$$pt_s = \frac{677{\cdot}775 - 257{\cdot}827}{357{\cdot}658 - 257{\cdot}827} = 420{\cdot}66.$$

Hence, Eq. (*d*),

$$443{\cdot}73 - 420{\cdot}66 = \delta(4{\cdot}437^2 - 4{\cdot}437),$$

$$\therefore \delta = 1{\cdot}512.$$

Thermometer K_2 is now completely standardised.

Constants

$$R_1 = 357{\cdot}658 \quad \frac{R_1}{R_0} = 1{\cdot}3872$$

$$\underline{R_0 = 257{\cdot}827}$$

$$FI = 99{\cdot}831 \quad \delta = 1{\cdot}512.$$

The most simple manner of obtaining the value of t for any given value of pt is to proceed as follows. Construct, by means of Eq. (*d*), a table giving corresponding values of pt and d for regular increases in t or pt, assuming the value of δ as 1·500. (For convenience of those using these thermometers, I give such a table, as an appendix, for values of pt up to 1000.) Plot the numbers thus obtained on a large scale with pt as abscissa and d as ordinate. Having experimentally found pt in a certain case with a thermometer whose value of δ is δ^1, ascertain from the chart the corresponding value of d, then $t = pt + \dfrac{\delta^1}{1{\cdot}500} \times d$, and thus the same chart can be used for different thermometers.

The above example will suffice both to illustrate the general method and the standardisation of the Kew thermometers.

IV. *Concluding Remarks.*

I understand that the Kew Committee had two objects in view when they sanctioned the acquisition of this apparatus and undertook the task of directing a course of observations.

(1) To submit the methods and principles of platinum thermometry to an exhaustive trial, and especially to ascertain how far the apparatus would stand the test of time and use. Such a series of observations can only be undertaken by a department similar to that at Kew, where records are properly kept, and where the continuation of the experiments is not dependent on the life or inclination of individual observers.

(2) To establish some recognised system of standardisation for instruments intended for the measurement of high temperatures.

With regard to the latter object, I would venture to add a few remarks. If high-temperature mercury thermometers (such as those of Niehls, of Berlin) are sent for comparison, it must be remembered that the readings of these instruments are greatly influenced by the stem temperature, especially when the range is large, and it is impossible, under the conditions usually prevalent in high temperature measurements, to secure complete immersion of the stem. The observers at Kew will be able to state the length of the portion actually immersed, &c., and those who afterwards use such thermometers must endeavour, if they wish for accurate results, to reproduce the conditions as nearly as possible. The experience of Messrs. Heycock and Neville in their earlier work (when they used for their experiments mercury thermometers standardised by platinum ones)[1] shows that it is possible to reproduce the original conditions with sufficient accuracy.

Again, it is useless to standardise glass thermometers unless previous experience has shown that they are not subject to the zero rise usually characteristic of such instruments after exposure to high temperatures.

Another matter, to which I trust the attention of Dr. Chree will sometime be directed, is the suitability of platinum standards for the calibration of mercury thermometers *at ordinary temperatures*. The greatest value of d over the range 0° to 100° (*i.e.* near 50° C.) is with these standards less than 0°·4 C.; now an error of 1 per cent. in δ (and I do not believe that any such error is probable, or I may say possible) would mean an error of but 0°·004 in t at 50° C., and less at other temperatures. The readings are independent of changes in internal or external pressure, of position or of stem immersion, and are directly expressed in terms of the air thermometer. It was with a view to such comparisons that I designed K_6, which on account of the large value of FI would cause an error of ·003 in the readings to affect the resulting value of t by only 0°·001 C.

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[1] *Chem. Soc. Journ.*, July 1890.

In conclusion, I may be permitted to express my gratification that the efforts made by Prof. Callendar and myself to demonstrate the accuracy and convenience of the methods of platinum thermometry are, although progress has been slow, at length awakening the attention of scientific inquirers. We believe (and that belief is founded not only on our own experience, but more especially on the work of Messrs. Heycock and Neville) that it is by means of the platinum thermometer that the many difficulties attendant on thermometric measurements, either at high or low temperatures, can be most easily surmounted.

Although the acquisition and installation of the apparatus has involved a considerable expenditure of both time and money, I am confident that, under the able direction of Dr. Chree, the results will justify the action of the Committee.

APPENDIX.

The following table gives the relation between the platinum temperature scale and the air temperature scale, when the value of $\delta = 1·500$.

Platinum temperature scale.	Correction.	Air temperature scale.	Platinum temperature scale.	Correction.	Air temperature scale.
− 100	+ 2·9	− 97·1	450	+ 27·0	477·0
− 50	+ 1·1	− 48·9	500	+ 34·9	534·9
0	0·0	0	550	+ 44·0	594·0
50	− 0·4*	49·6	600	+ 54·4	654·4
100	0·0	100·0	650	+ 66·2	716·2
150	+ 1·2	151·2	700	+ 79·4	779·4
200	+ 3·1	203·1	750	+ 94·2	844·2
250	+ 6·0	256·0	800	+ 110·7	910·7
300	+ 9·8	309·8	900	+ 149·4	1049·4
350	+ 14·5	364·5	1000	+ 197·0	1197·0
400	+ 20·2	420·2			

* More accurately = − 0·375 and 49·625.

E. H. GRIFFITHS.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

OXFORD.—It is announced that the electors to the Waynflete Professorship of Mineralogy will proceed to the election of a Professor in the course of the present year. Candidates are required to send to the Registrar of the University, on or before December 7, their applications and testimonials.

The University having accepted a bequest of £900, given by the will of the late Mrs. Fielding, for the purpose of providing for the payment of a Curator of the Fielding Herbarium, it has been decreed that there shall be a Curator of the Herbarium, appointed by the Fielding Curators, and under the direct control of the Sherardian Professor of Botany. Besides the income derived from the bequest of £900, the Curators shall have the power to apply a part of the funds at their disposal to the increase of the stipend of the Curator of the Herbarium.

The following Examiners have been approved by Convocation:—For the first examination for the degree of Bachelor of Medicine, W. R. Dunstan, G. W. S. Farmer, and Dr. R. Stockman; for the second examination for the degree of Bachelor of Medicine, Dr. C. W. Mansell Moullin, Sir William Stokes, G. E. Herman, and Dr. S. H. C. Martin. In each case the appointments are for the examinations of 1896, 1897, and 1898.

CAMBRIDGE.—Mr. T. W. Bridge, Professor of Zoology in the Mason College, Birmingham, and Mr. G. H. Bryan, F.R.S., of Peterhouse, have been approved for the degree of Doctor of Science.

Mr. C. T. R. Wilson, of Sidney Sussex College, has been elected to the Clerk Maxwell Studentship in Experimental Physics.

The late Miss Jane Saul has left her collection of shells, and the cabinet containing the same, her "Conchologia Iconica," and other conchological works, to the University.

MR. J. GAD, Extraordinary Professor of Physiology in Berlin University, has been appointed Ordinary Professor of the same subject, and Director of the Physiological Institute in the German University at Prague. Dr. M. von Lenhossek, of Wurzburg, has been appointed Prosector in the Anatomical Institute at Tübingen. Other recent appointments are: Dr. Mark W. Harrington to be President of Washington State University; Mr. H. Landes to be Professor of Geology in the same University, and Dr. H. C. Myers to be Professor of Chemistry.

DR. C. M. LUXMORE has been appointed to a Research Fellowship of the Pharmaceutical Society.

MR. JAMES WILSON, Lecturer in Agriculture, University College, Aberystwyth, has been appointed to the Fordyce Lectureship in Agriculture in Aberdeen University.

FROM the *Journal* of the Society of Arts it appears that the great advances made by Swiss national industry during the last fifteen or sixteen years, both in the technical and artistic character of its products, are attributed by the *Deutsches Handels Archiv* to the beneficial influences of State and Municipal establishments for technical education. It is very remarkable how much is done in the cantons of Geneva and Neuchatel to encourage and improve local industries, especially in finer classes of goods, for the manufacture of which a considerable amount of skill and artistic knowledge is required. In these two cantons, numbering little more than 220,000 inhabitants, there are five schools for watchmakers, and in Geneva, Neuchatel, and Chaux de Fonds there are schools for instruction in the fine arts and in artistic handicrafts. Besides the institutions there are commercial schools in Geneva and Neuchatel, and the professional schools in which instruction in various industries is given to persons of both sexes. In the watchmaking school at Geneva a class for girls has recently been established, where certain operations peculiarly suitable for female labour are taught. Considerable assistance is also rendered to the watch industry by the astronomical observatories at Geneva and Neuchatel, both by testing chronometers, and by their co operation in the annual trade competitions.

SCIENTIFIC SERIALS.

Internationales Archiv für Ethnographie, Band viii. Heft iv. —This well-illustrated journal is steadily increasing in value and interest, as it is wider in its scope than it was at the commencement. Baron van Hoëvell, of Amboina, has a paper on a few notes on the kinds of the worship of gods in the south-western and south-eastern islands of the Malay Archipelago. Amongst other interesting information is a legend of the origin of two fetiches which are said to have fallen from heaven; one, which is called a sword, is probably a piece of old hoop-iron, and the other, a supposed spear-point, appears to be a piece of meteoric iron. There are also traces of a virginal conception through divine influence.—"Dogs and Primitive Folk" is the title of a comprehensive essay by Dr. B. Langkavel, in which he deals with dogs in folk custom and belief, the name as a term of reproach, ornaments derived from dogs, &c.—Dr. O. Frankfurter writes on dreams and their significance according to a Siamese dream-book.—J. D. E. Schmeltz has three communications on Papuan ethnography, of which the first, on objects from the Tugeri, is the most interesting. We are now beginning to learn something definite about these ruthless pirates that harass the western coast population of British New Guinea. A bow, tobacco-pipe, drum, and two remarkable dance ornaments are figured; the latter are slabs of wood carved to represent a flying bird (?), and several lizards or crocodiles. He also describes a wood-carving of what appears to be an echidna and some ceremonial objects. The rest of the journal is occupied with the usual notes and notices.

IN addition to articles specially interesting to Italian botanists, the *Nuovo Giornale Botanico Italiano* for July contains the following:—A study of the action of certain alkaloids on plants in darkness and in light, by Signor A. Marcacci. While quinine arrests the transformation of starch into saccharose, and of dextrose into levulose, both in the dark and in the light, strychnine does so only in the light, from which the conclusion is drawn that these changes are not simply chemical processes, but are dependent on other unknown forces. —On certain contrivances for dissemination in Angiosperms; in which more stress is laid than is generally the case on the action of water in the dispersion of seeds; as, for example, in the production of mucilage, to which the rupture of capsules is often due.—On the fruit of *Aucuba japonica*, by Signor L. Pampaloni.—On the affinities of the *Sphenophyllaceæ*, by Prof. G. Arcangeli. The author regards this group of fossil plants as having