

V. *Report on Hygrometric Methods; First Part, including the Saturation Method and the Chemical Method, and Dew-point Instruments.*

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[PLATE 5.]

IN August, 1879, at the request of the Meteorological Council, I undertook an experimental comparison of the various methods of determining the hygrometric state of the air, with the following instruction, "The chemical method to be employed, and with it to be compared the dry-and-wet-bulb hygrometer, REGNAULT'S, DINES'S, ALLUARD'S, and the hair hygrometer." Since that time I have devoted to the subject the time that was at my disposal, and I now beg leave to lay before the Council a statement of the experiments I have made and the results I have arrived at with respect to the chemical method and the dew-point instruments.

The arrangement of the experiments was left to my discretion: I had, therefore, first to practise myself in the use of the different methods in order to arrange in some sort of order the means by which the various inquiries should be undertaken; and, further, it was necessary for me to know and to carefully consider the very numerous contributions made by other observers to the discussion of hygrometric methods, so that I might be able to distinguish between those points which had been satisfactorily and permanently settled and those upon which further experimental investigation might throw additional light. The distinction proved to be not a very easy one to draw, and I shall therefore append to this report a summary of the work done in the subject since the time of DANIELL. I made a summary of this kind for my own use at the outset, but since then many important memoirs have been published, chiefly on the Continent, which bear particularly upon the question of the trustworthiness of observations with the wet and dry bulb. I have taken account of those memoirs in the summary which I now offer. It will, I believe, be found to justify the following general conclusions:—

1. There is no hygrometric method of which it has been *proved* that an observer following out definite written instructions with due care and skill can obtain measurements of vapour pressure which are accurate to within 1 per cent. The accuracy claimed by REGNAULT for the chemical method is only "about a fiftieth," and to

this method authors nearly always refer, as being the ultimate standard of reference for the other methods. There is no evidence to show that, speaking generally, the various absolute hygrometers, such as those of SCHWACKHÖFER, EDELMANN, NEESEN, DINES, and others, can be relied upon to that degree of accuracy, whatever may be done by one particular observer after laborious trial and testing with one particular instrument.

2. It is possible by the use of suitable desiccating tubes to absorb the moisture from air passed through them, and thereby to determine the weight of water contained in the air *which actually passes over* the desiccating substances. The degree of accuracy attainable in this measurement is limited only by the uncertainty of the weight of the drying tubes. As the drying tubes are somewhat bulky and their weights are liable to alterations in consequence of variations in the state of their external surface, this uncertainty is quite appreciable. No data are given with regard to it by REGNAULT or other observers who have employed the method. From a number of experiments of my own I may, however, conclude that it is not safe to assign an accuracy to the weighings greater than that of 1 milligramme, but that with due precautions this limit need not be exceeded. This may, of course, be the same, whatever the total amount of moisture absorbed may be, and the fractional error will therefore depend upon that total amount of moisture. If 1 gramme be taken up, the limit of error will be one part in one thousand, so that an accuracy of 1 per cent. is well within the reach of an observer.

3. Air may be saturated by vapour arising from water in a vessel with glass sides so that the vapour pressure reaches a value agreeing to within about 2 per cent. with the vapour pressure *in vacuo* at the same temperature. There are no observations to show whether a still closer approximation to the vacuum saturation-pressure would be obtained by using air drawn from an enclosure surrounded by wet muslin. REGNAULT's experiments bearing upon this question do not give a decisive answer, in consequence of complications arising from the uncertainty as to the density of saturated vapour, and the effect of the glass.

4. The dew-point instruments, in the hands of skilled observers, give readings of the so-called dew-point which are sensitive to within $0^{\circ}.1$ C., but the reading may depend on the skill of the observer, and there is no evidence to show within what limits of accuracy the temperature so observed may be regarded as the true saturation temperature of the air. Suggestions have been made as to causes of error, but no measurements of the effects of those causes have been made which would enable an observer to specify the degree of accuracy of the inferences from his observations.

5. With reference to the wet-and-dry-bulb method, two points are clear:— (i.) Different observers use different tables* for the reduction of their observations, and in certain cases these different reductions lead to very serious differences of results; (ii.) The ordinary method of exposure when the two thermometers are

* See note B., p. 146.

freely exposed without any provision for a definite circulation of the air cannot be expected to give results which are accurate to within 2 per cent.; in fact, no satisfactory formula of reduction can be found.

The errors are especially serious when the air is nearly or quite saturated and the wet bulb coated with ice.

The most accurate results with this method are obtained when an artificial circulation of air is maintained, and in that case the highest accuracy claimed for the method allows an error of ± 2 per cent. with temperatures above zero, and twice that amount with lower temperatures, the comparisons being made with ALLUARD'S form of REGNAULT'S dew-point instrument.

6. The case of the hair hygrometer is very perplexing; opinions are very conflicting, and it seems to be a question upon which meteorologists take sides. An immense amount of experimental work has been done, and it is possible that an observer might make very useful observations with the instrument, but whether the inferences from his observations would be regarded with any degree of confidence by others seems to be still an open question.

With the exception of REGNAULT'S researches on the chemical method (p. 121), the experimental work that has hitherto been published has consisted in the simultaneous observation of the vapour pressure in free air by two different hygrometric methods. This plan is open to uncertainties arising from the two following causes:—

(i.) The air is taken from two different positions for the two instruments; in other words, the observations are made upon two different specimens of air, whose hygrometric states are *assumed* to be identical, not upon the same air. Perhaps the assumption is sufficiently justified when the air has immediate access to the reading parts of each instrument, but I have pointed out in the summary of results certain reasons for considering this uncertainty to be serious when the access is not direct, as in the case of the comparison of a dew-point instrument with a ventilator-psychrometer, in which case the air may suffer alteration in passing over the vanes of the ventilator. And, indeed, in any case, in an experiment designed to compare the efficiency of different methods, the uncertainty arising from this cause is a disadvantage, for the liability to error in the instruments themselves is quite sufficient to make the comparisons difficult; and, moreover, if we could rely upon the identity of the results of two instruments, a similar arrangement might be employed to determine whether there is any local variation in the hygrometric state of the air, an independent question not without interest.

(ii.) The observations obtained with different instruments differ in character. The chemical method gives the mean value of the vapour pressure during the period of the experiment, and does not indicate any small variations from time to time. The wet bulb takes a certain finite, though it may be short, time to reach its equilibrium state, and therefore does not give the vapour pressure at any particular instant. The dew-point instrument, on the other hand, may be taken as giving the vapour pressure

at the instant of the formation of the dew deposit. These differences may, perhaps, best be illustrated by considering what would be their effects upon the ideal curve which would represent the continuous variation of the hygrometric state of the air, if we suppose the air to be subject to a series of rapid alternations of moisture and dryness; the dew-point instrument would give a series of points on the true curve; the observations with the wet and dry bulb would give a series of points, not on the true curve, but a somewhat modified curve, the slopes being more gradual; and, provided the alternations were sufficiently rapid, the curve of the wet bulb would generally be smoother than the true curve. A series of observations with the chemical method would give a series of points on a third curve quite different from the other two, and one from which all effect of rapid alternations would have disappeared, and only the more permanent changes would be shown. Hence, two instruments of different kinds can only be compared by taking the mean of a number of consecutive observations, and assuming that the effect of all rapid alternations disappears; and this is probably the case under all ordinary circumstances. This must, however, be borne in mind in considering the comparisons, because the rapid alternation is possibly of not infrequent occurrence. When using a dew-point instrument it is sometimes observed that at the same temperature the dew forms and then disappears again. This phenomenon has been differently interpreted by different observers, some considering that it shows the extreme sensitiveness of the instrument to small atmospheric changes, while others have attributed it to the variation of the observed dew-point with variation of wind velocity (see p. 142).

It is, therefore, evident that it would be of some advantage to eliminate if possible these two causes of uncertainty. I have endeavoured to do so in the experiments I am now communicating, in the following manner: The instrument to be tested was enclosed in a glass vessel, B, which was connected on the one side with an apparatus, which I will call a saturator, A, designed to saturate air at a given known temperature, and on the other side with drying tubes, C. An aspirator drew air through the whole arrangement. The hygrometric state of the air is given (1) by the temperature of the saturator, A; (2) by the instrument; (3) by the drying tubes, C. I first ascertained (§ 1 to § 10) that when B was cut out and A and C put in immediate connexion, the two methods—namely, the saturation method and the chemical method—gave concordant results. Then B was introduced, but the dew-point instrument was not worked, and the results of A and C were again compared and found to be equally concordant. This showed that the state of the air was not altered by the mere presence of B; and, finally, observations were taken with the dew-point instrument while the air was being drawn through, and the results of A and C were again compared. It was found that they were still concordant, and since the state of the air was known before it passed over the instrument, and was proved to be the same after passing the instrument, we are quite safe in assuming the hygrometric state of the air while it was passing the instrument to be that given either by the saturator A *or* by the

drying tubes C. Hence observations made with the instrument may quite fairly be taken to be observations upon air whose hygrometric state is really accurately known; the degree of accuracy will be clear from the particulars of the experiments. The experiments accordingly group themselves in the following manner:—

I. Experiments to ascertain the limits of accuracy of agreement between the method of saturation and the chemical method.

These experiments are practically a repetition of REGNAULT'S work with my arrangement of the apparatus and absorbent substances. This repetition is necessary (1) as a preliminary, in order to make sure that the apparatus is in satisfactory working order, and as a test of the drying substances used (Table II.); (2) because REGNAULT'S observations were undertaken with the view of determining the density of vapour in saturated air, and nearly always the air was practically saturated when it reached the drying tubes. In order to complete our knowledge, we have to extend the observations to cases in which the temperature of the air when it reaches the tubes is considerably above the temperature of the saturator. In this case one would expect *a priori* that the agreement would be very nearly the same, but I know of no published experiments to directly test the point.

II. Experiments to determine whether the interposition of the vessel C interferes with the concordance of the results obtained. The only apparent reasons for such interference are condensation upon the connecting tube or the vessel C, or leakage from the dew-point instrument. The second part of I. may, therefore, be taken with these. The results of I. and II. are shown in Tables II. to V.

III. Comparisons of the results of the dew-point instrument with those of the saturator and chemical method when all three are taken together. (Table VIII.)

IV. Observations with the dew-point instrument compared with the results of the saturator only. A few such observations were made for the purpose of testing special points under circumstances that made the double testing of the air inconvenient or undesirable. (Exp. 73 to 83.)

I may now proceed to give the details of the experiments.

THE METHOD OF SATURATION AND THE CHEMICAL METHOD. TESTS OF THE EFFICIENCY OF THE APPARATUS AND OF THE DESICCATING SUBSTANCES.

§ 1. The method of saturation which is here referred to simply means passing air through some form of apparatus by which it is saturated, the temperature of saturation being read by a thermometer placed in the saturator. This is, of course, not a method of measuring the pressure of moisture in a given specimen of air, but simply a means of obtaining air the pressure of vapour in which is known from its temperature. The saturators used will be described later.

§ 2. The chemical method consists, as is well known, in causing a known volume, v , of air to pass through weighed tubes capable of abstracting *the whole* of the moisture

from the air and determining the weight of aqueous vapour thus absorbed by the tubes. Then, if f be the amount of moisture per cubic metre in the air which enters the tubes; Δ , the density of dry air at 0° C. and 760 mm. pressure; d , the specific gravity of aqueous vapour referred to dry air at the same temperature and pressure; t , the temperature of the entering air; α , its coefficient of expansion per degree Centigrade; e , the pressure of the aqueous vapour, then

$$e = \frac{760 (1 + \alpha t)}{\Delta d} f. \quad \dots \dots \dots (1)$$

In order to determine the volume, v , of air which enters the tubes, a known volume of water is allowed to flow out of an aspirator, its place being taken by the air which passes through the drying tubes. Let V be the volume of water which is allowed to run out of the aspirator, this volume will then be occupied by *saturated* air, the dry part of which has passed through the drying tubes; if T be its temperature when in the aspirator, E the corresponding pressure of aqueous vapour, the pressure due to the dry air will be $B - E$, where B is the height of the barometer at the time. Its pressure before entering the tubes was $B - e$, and its temperature was t ; its volume, therefore, was—

$$v = V \cdot \frac{B - E}{B - e} \cdot \frac{1 + \alpha t}{1 + \alpha T}.$$

Hence, if w be the number of grammes of moisture absorbed,

$$f = \frac{w}{V} \cdot \frac{B - e}{B - E} \cdot \frac{1 + \alpha T}{1 + \alpha t},$$

and we get for determining e the following equation:—

$$e = \frac{760 (1 + \alpha t)}{\Delta d} \cdot \frac{w}{V} \cdot \frac{B - e}{B - E} \cdot \frac{1 + \alpha T}{1 + \alpha t}, \quad \dots \dots \dots (2)$$

or

$$\begin{aligned} \frac{e}{B - e} &= \frac{760}{B - E} \cdot \frac{1 + \alpha T}{V} \cdot \frac{w}{\Delta d} \\ &= \frac{760 (1 + \alpha T)}{\Delta d} \cdot \frac{w}{(B - E) V} \end{aligned}$$

$$\frac{e}{B - e} = X \frac{w}{(B - E) V};$$

therefore

$$e = X \frac{w}{V} \left(1 + \frac{E - e}{B} \right), \quad \dots \dots \dots (3)$$

approximately, neglecting squares not likely to be larger than $\frac{1}{6000}$;

or

$$e = X \frac{w}{V} \left(1 + \frac{E - Xw/V}{B} \right) \text{ approximately.} \quad \dots \dots \dots (4)$$

The application of this formula involves the assumption of a value of d , the specific gravity of steam. The difficulties connected with this are discussed in Note A (p. 121). The value $\cdot 622$ has been assumed throughout my experiments. For Xw/V inside the bracket the value of e derived from the temperature of the saturator may be substituted without appreciable error.

To facilitate computation I have tabulated the values of X , *i.e.*, $760(1 + \alpha T)/\Delta d$, for each degree Centigrade as below :—

TABLE for Reduction of Aspirator Observations.

Table of the value of $760(1 + \alpha T)/d\Delta$ for every degree of temperature between -10° C. and $+30^{\circ}$ C.

$$d = 0\cdot622.$$

$$\Delta = 1293 \text{ grammes per cubic metre.}$$

$$\alpha = \cdot00366.$$

Temperature T.	$\frac{760(1 + \alpha T)}{d\Delta}$	Log.	Temperature T.	$\frac{760(1 + \alpha T)}{d\Delta}$	Log.
°			°		
-10	·9104	1·9592313	11	·9830	1·9925666
- 9	·9139	·9608781	12	·9865	·9949919
- 8	·9173	·9625187	13	·9899	·9956119
- 7	·9208	·9641531	14	·9934	·9971266
- 6	·9242	·9657813	15	·9969	·9986360
- 5	·9277	·9674035	16	1·0003	0·0001402
- 4	·9311	·9690196	17	1·0038	·0016392
- 3	·9346	·9706298	18	1·0073	·0031330
- 2	·9381	·9722340	19	1·0107	·0046258
- 1	·9415	·9738525	20	1·0142	·0061054
0	·9449	·9754247	21	1·0176	·0075839
+ 1	·9484	·9770114	22	1·0211	·0090575
+ 2	·9519	·9785921	23	1·0245	·0105260
+ 3	·9554	·9801673	24	1·0280	·0119898
+ 4	·9588	·9817453	25	1·0314	·0134484
+ 5	·9623	·9833004	26	1·0349	·0149023
+ 6	·9657	·9848586	27	1·0383	·0163117
+ 7	·9692	·9864112	28	1·0418	·0177550
+ 8	·9726	·9879583	29	1·0542	·0191955
+ 9	·9761	·9894998	30	1·0487	·0206303
+10	·9796	·9910359			

The fraction $(E - e)/B$ is always small, and B has therefore always been taken equal to 760. The practical error thus introduced is only of the order $\cdot00002$.

§ 3. Before describing the apparatus first employed, it will be well to call attention to the temperature measurement. For this purpose I have been able to avail myself of eleven thermometers; of these, two, by NEGRETTI, are the property of the Meteorological Office, graduated to half degrees FAHRENHEIT; two of my own, by HICKS, are graduated to fifths Centigrade; two, by HICKS, graduated to fifths FAHRENHEIT, one, by HICKS, graduated to fifths Centigrade, one, by CETTI, graduated to fifths Centigrade, and three, by GEISSLER, graduated to tenths Centigrade, are the property of

the Cavendish Laboratory. All these thermometers were, through the kindness of Mr. WHIPPLE, specially compared for me at Kew for every degree Centigrade in January, 1882. Their freezing points have been determined from time to time, and the tables of corrections revised. The temperature readings may be regarded as accurate to within $0\cdot1$ C.

§ 4. The apparatus used for the first series of experiments (Table II.), of which fig. 1 (Plate 5) shows the general arrangement, consisted of three distinct parts :—

(i.) The aspirator (C) for causing the passage of a known volume of air over the desiccating substances in the drying tubes.

(ii.) The weighed drying tubes (B, B) for determining by their increase of weight the quantity of moisture in a known volume.

(iii.) A saturator (A) for supplying saturated air at the temperature of the room.

Between the drying tubes and the aspirator was placed an additional drying tube, or a bottle filled with chloride of calcium, to prevent moisture reaching the weighed tubes from the aspirator.

(i.) *The aspirator* was of the ordinary form, a copper cylinder with conical ends; a tap was fixed at the bottom, and through a cork at the top passed three glass tubes: the first, for the delivery of the air from the drying tubes, passed nearly to the bottom of the aspirator to ensure a uniform flow of air; the second, connected with a similar one entering at the bottom, served as a gauge; the third passed to the lower surface of the cork, and was used to fill the aspirator by being connected with an aspirating pump. The aspirator was also provided with a thermometer, passing through the side of the upper cone.

The volume of each aspirator was determined by completely filling it with water as for an experiment, and running the water out gradually into a litre flask of known weight when empty, and weighing the flask each time when full, the flask being carefully dried by means of a hot cloth between successive fillings.

Two aspirators were employed marked A and B, and the quantity of water in each was determined twice with the following results :—

Aspirator A, 1st observation	16365·9	grammes,	temperature	18° C.
2nd	,,	16371·2	,,	,,
Aspirator B, 1st	,,	16384·0	,,	,,
2nd	,,	16383·8	,,	,,

We have therefore, allowing for the density of the water used and the temperature,

Volume of A at 0° 16384 cc.

 ,, B at 0° 16400 cc.

The volumes at any temperature t will then be

Aspirator A, 16384 ($1 + \cdot 000052 t$) cubic centimetres.

 ,, B, 16400 ($1 + \cdot 000052 t$) ,,

One of these two gives the volume of water run out in any experiment. The water would be replaced partly by air which had come through the drying tubes and partly by the vapour of the water formed in the aspirator. *We may assume that the air in the aspirator at the end of the experiment is saturated with moisture.*

(ii.) *The drying tubes.*—A specially constructed form was used. Instead of being closed with corks perforated with glass tubes, glass connexions of wider bore were used, which were thickened and ground into the U-tubes. These latter were of the ordinary size, about 6 inches long and $\frac{1}{2}$ -inch internal diameter. The long tubular stoppers were bent over, in the case of sulphuric acid and pumice tubes through two right angles, and in the case of phosphoric acid tubes (shown in fig. 2, Plate 5), through one right angle. The wide ends of these tubes could then pass over narrower tubes coming vertically through the bottoms of small mercury cups, and thus forming the connexions between the drying tubes and the other parts of the apparatus. The connexions were thus made by means of mercury joints. These joints were tested, and found to be quite tight for differences of external and internal pressure many times greater than those occurring in the experiments. The arrangement is very convenient, as the tubes can be simply lifted from their places and as easily replaced; they require careful brushing to remove the adhering mercury, and the ends are closed for weighing with small india-rubber stoppers. The liability to error in consequence of moisture on the surface of the mercury is probably not so great as that to which the tubes would be exposed by using india-rubber connexions. The drying tubes were filled either with phosphoric anhydride, or with rather coarse fragments of pumice saturated with the strongest sulphuric acid (sp. gr. 1.84). Experiments will be detailed below to show that either of these substances is perfectly efficient for the purpose of withdrawing all the moisture from the air passed over it. A number of experiments have shown me that drying tubes filled with recently fused chloride of calcium, although in many ways convenient, are not capable of extracting all the moisture from air.*

Correction of the weight of the drying tubes for weighing in air.—The main part of

* The experiments were of two kinds:—

(1.) Two chloride of calcium tubes were arranged in front of two sulphuric acid tubes and an aspirator-ful of saturated air passed through all four. The gain of weight in each of the four for three observations is given below:—

1st tube. (CaCl ₂)	2nd tube. (CaCl ₂)	3rd tube. (H ₂ SO ₄)	4th tube. (H ₂ SO ₄)
gm. ·2025	gm. ·0010	gm. ·0208	gm.
·2220	·0000	·0245	·0013
·2226	·0000	·0190	·0000

(2.) The two aspirators were used to determine by two independent simultaneous observations the

the weight of the tubes is the weight of the glass, and the specific gravity of the pumice and of the strong sulphuric acid does not differ much from that of glass; we may, therefore, calculate the correction for weighing in air on the assumption that the specific gravity of the whole tube is that of flint glass, which we may take to be 3.5. This would make the correction to weight *in vacuo* for a tube of 150 grammes equal to 30 milligrammes. The effect of a barometric variation of 1 cm. upon such a tube would therefore be to alter its apparent weight by .39 mgm., and a variation of 1° C. of temperature would produce an alteration of .11 mgm. in the apparent weight. The changes in barometric pressure and temperature between two successive weighings may therefore be such as to cause the apparent weight of the tube to alter by a considerable fraction of a milligramme. Now the amount of moisture is determined by the difference of weight of the tube at the two weighings, and accordingly any error in the weighing, due to neglecting to correct for weighing in air, will be of the same absolute magnitude, and of very much greater relative importance, in the weight of moisture absorbed by the tube. The variations of pressure and temperature, however, between the successive weighings of the tubes during the observations were not sufficient to produce any appreciable effect upon the results.

(iii.) *The saturator.*—This part of the apparatus was similar in principle to that used by REGNAULT. Two long bell-jars stood in a shallow dish of distilled water; the one jar was filled with well-washed sponge, and the second contained a wire frame covered with muslin, which, with it, stood in the water at the bottom of the dish. The air supplied to the aspirator was drawn by means of a glass tube passing through the cork in the top of the jar from the middle of the muslin cage, and close to the opening of the tube was the bulb of the thermometer, which also passed through the cork; the place of the air thus removed was supplied by air passing from the outside through the sponge vessel, and delivered into the second vessel outside the muslin cage. During an observation, which lasted generally about two hours, the thermometer was read every quarter of an hour by means of a telescope placed at some distance, in order to avoid any error of parallax, and the mean of the readings taken as the temperature of the saturated air. The muslin cage served to

amount of moisture in the air of the room, first by pumice tubes, and secondly by chloride of calcium tubes. The amounts are given below:—

By pumice tubes.	By chloride of calcium tubes.	Difference.
gm. .1550	gm. .1435	gm. .0115
.1498	.1352	.0146

Since the above was written I have found that previous observers have expressed the same opinion about calcium chloride. (See ANDREWS, 'Phil. Mag.,' vol. 4, 1852, p. 330, and MÜLLER-ERZBACH, 'Deutsch. Chem. Gesell. Ber.,' vol. 14, p. 1093.)

protect the thermometer from external radiation, as well as to ensure the complete saturation of the air.

§ 5. A series of observations was first made, with the view of testing the drying tubes and other parts of the apparatus. There were two points to be determined: first, whether the desiccating substances used could be regarded as completely drying the air passed over them;* and secondly, whether one drying tube was sufficient for the purpose, or two or more were necessary. For this purpose four drying tubes were mounted, two being filled with phosphoric anhydride in the ordinary form of white powder; the other two were filled with pumice moistened with commercial sulphuric acid (sp. gr. 1.84). The pumice was broken into coarse fragments, and before being used was saturated with sulphuric acid, and heated to redness.

The saturated air was then sent through all four tubes, and usually the gain in weight of each tube determined. From the *gain in weight of the first tube alone*, the pressure of vapour in the saturator was calculated by the formula—

$$e = \frac{760(1 + at)}{\Delta d} \cdot \frac{w}{V} \cdot \frac{B - e}{B - E} \cdot \frac{1 + \alpha T}{1 + at},$$

and this was compared with the pressure as given by REGNAULT'S table for the temperature indicated by the thermometer in the saturator, corrected by the table of Kew corrections, and a determination of the freezing-point during the course of the series of observations. The subjoined tables give the results obtained. Table I. is a specimen of the observations as they were taken, and Table II. gives the collected results.

TABLE I. Experiment 10. Aspirator A.

Time.	Tempera- ture of saturator.	Tempera- ture of aspirator.	Barometer.	Weight of pumice tube, I., II.	Weight of pumice tube, III., IV.	Weight of phosphoric tube, III., IV.
3.55	19.5	°	..	176.4613	177.5045	146.9780
4.10	19.5			
4.30	19.5					
4.44	19.45					
5.00	19.45					
5.30	19.45					
5.50	19.40	19.4	29.750	176.7315	177.5061	146.9798
Means and differences.	19.462702	.0016	.0018
Corrections.	-.12	-.3				

Tension of vapour calculated from Tube I., II., 16.66 mm.

* See note at end (p. 148).

TABLE II.

Number of experiment.	Date.	Aspirator.	Gain in weight, tube 1, in grammes.	Gain in weight, tube 2.	Gain in weight, tube 3.	Gain in weight, tube 4.	Mean corrected temperature of saturator (t).	Corrected temperature of aspirator (T).	Tension in mm. calculated from tube 1.	Tension in mm. calculated from temperature t of saturator.	Difference.	Duration of experiment.
			Phosphoric acid.		Sulphuric acid.		°	°				h. m.
1	July 4	B	·2915	-·0050	+·0048	+·0028	21·05	22·7	18·23	18·55	-·32	1 20
2	" 4	A	·2928	+·0008	+·0117	+·0018	20·93	22·2	18·29	18·41	-·12	2 15
3	" 6	"	·2916	+·0023	+·0080	+·0040	21·05	21·8	18·17	18·55	-·38	2 23
4	" 7	"	·2944	+·0005	+·0011	-·0008	21·06	21·7	18·34	18·56	-·22	2 3
5	" 7	"	·3074	+·0023	+·0033	+·0017	21·59	21·5	19·12	19·18	-·06	2 7
6	" 9	"	·2931	+·0007	+·0022	+·0006	20·91	21·45	18·25	18·39	-·14	2 45
7	" 9	"	·3020	-·0001	-·0002	-·0009	21·31	21·0	18·77	18·85	-·08	1 15
			Sulphuric acid.		Phosphoric acid.							
8	" 10	"	·2870	+·0027	+·0019	..	20·64	21·2	17·85	18·09	-·24	3 2
9	" 11	B	·2785	-·0005	+·0018	..	20·00	21·4	17·35	17·39	-·04	2 0
10	" 13	"	·2702	+·0016	+·0018	..	19·34	19·1	16·66	16·70	-·04	1 55
11	" 14	"	·2557	+·0013	+·0010	..	18·55	18·7	15·74	15·80	-·06	2 0
12	" 12	"	·2682	+·0023	+·0013	..	19·08	?	16·53	16·53	-·00	2 0

In the first seven experiments the two phosphoric acid tubes were placed first, and in the last five the sulphuric acid tubes were in that position. The columns giving the increase of weight in the different tubes show that these nearly always gained a small amount, but that amount is very irregular, and is about the same whether the phosphoric acid tubes or the sulphuric acid are placed first, and the calculated tension is in nearly every case within 1 per cent. of the tabulated tension. This being about the same error as that which occurs in REGNAULT'S observations, we may take it that the first tube was sufficient to completely dry the air passed through it, and that the increase of weight in the other tubes was due to some other cause.

§ 6. The different connexions were made partly by glass and partly by india-rubber tubing, and this suggested itself as a possible source of the observed differences. A number of observations were therefore taken with a view of determining how far this might be the case.

I. A glass tube about five feet long was mounted as a connexion between two mercury cups; and air, first dried by passing through a phosphoric acid tube, was passed through a long glass tube and then through a sulphuric acid tube, and the weight of the latter determined before and after the passage of the air. There was accordingly nothing but the glass tube between the two drying tubes. The results were as follows for four experiments:—

Date.	Increase in weight of the sulphuric acid tube, in grammes.
July 18	+·0029
„ 19	+·0002
„ 20	—·0004
„ 21	—·0002

It appears, therefore, that after the tube had been once dried no further increase of weight of the sulphuric acid tube occurred, and hence that, with due precaution, glass tubes may be used as connexions without any fear of error.

II. The glass tube was replaced by an india-rubber tube six feet long, and similar observations taken, fifteen experiments being made. The sulphuric acid tube always gained in weight, although every precaution was taken to keep the india-rubber tube dry between the experiments. The least amount of moisture was obtained when a second observation was taken, immediately after the completion of a first. The increase of weight of the sulphuric acid tube generally amounted to about 15 milligrammes. An india-rubber tube cannot, therefore, be used with any security for connecting two drying tubes.

§ 7. We may accordingly conclude that the increase in weight of the drying tubes after the first, in the table of results given, was due for the most part to moisture derived from the india-rubber connexions. For the last three observations in the table, p. 84, these connexions were made as short as possible, so that the amount of india-rubber surface exposed to the dry air might be small. With the apparatus in that form the drying tubes gained very much less in weight than before, and we may give the results obtained from these three observations as instances of the accuracy which may be expected by this method.

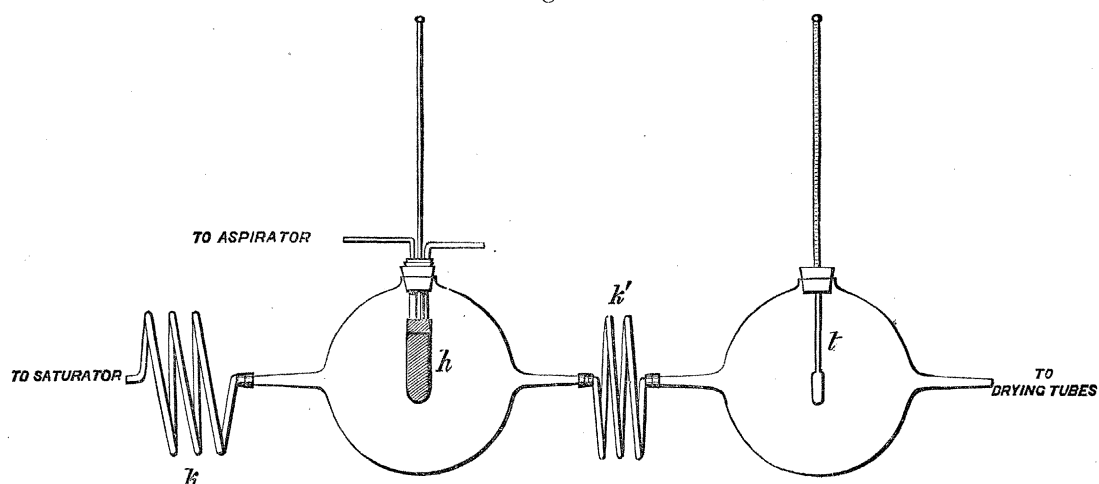
THE METHOD OF SATURATION AND THE CHEMICAL METHOD.—EXPERIMENTS WITH UNSATURATED AIR.

§ 8. With the experience gained by the experiments just described, I proceeded to arrange the apparatus by which the result of the chemical method could be compared with that of the saturation method on unsaturated air, and by which moreover dew-point observations could also be taken during the progress of an experiment such as those described. At first a REGNAULT dew-point instrument alone was introduced.

It was fixed by means of its glass tube into the cork of a three-necked globe in a manner which will be sufficiently indicated by a glance at the figure (fig. 3); on each side of the globe, between its long horizontal neck and the rest of the apparatus, was attached a coil of copper tube to ensure that the temperature of the internal air was the same as that of the external, except in the vessel containing the hygrometer

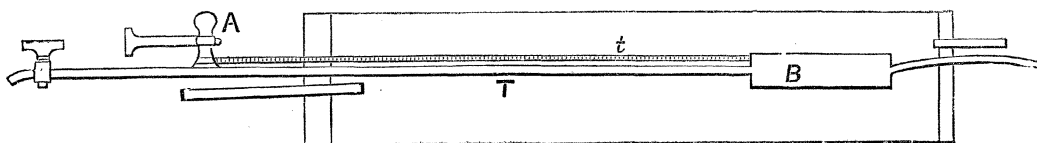
itself; attached to the second copper coil was a second globe with three necks, which contained a thermometer inserted in exactly the same manner as the REGNAULT hygrometer, and likewise shown in the figure. This sequence, consisting of (1) a copper coil k (fig. 3), (2) a globe containing the hygrometer, h , (3) a copper coil, k' , (4) a globe containing a thermometer, t , was simply interposed between the saturator and the aspirator, so that the air passed through the whole series.

Fig. 3.



In order to fit the REGNAULT thimble tightly into the globe it was cemented by means of gelatine to a glass tube, over which it would just slip. This was found not to be quite tight when there was a considerable difference of pressure between the inside and outside, and the leakage might have interfered with some of the observations. The joint was therefore strengthened by lapping the line of junction of the silver and glass with pure india-rubber strip pulled very thin and having each successive layer painted over with benzene. After a sufficient thickness was lapped on, the whole was painted with a solution of india-rubber in benzene and varnished, and no further trouble was caused by any leakage at the joint except in the five experiments noted in Table VI.

Fig. 4.



Subsequently a DINES hygrometer enclosed in a cylindrical glass tube, fig. 4, was added to the apparatus immediately following the globe containing the REGNAULT. The metal box, connecting tube, and thermometer of the DINES were detached from the wooden frame in which they are usually mounted. The delivery end of the tube

was easily slipped through a piece of thick india-rubber sheet forming a stopper to the 3-inch glass cylinder, and the other end, having the tube and thermometer very close together, was enclosed by half slitting an india-rubber bung previously carefully drilled, the slit passing along the common diameter of the two drilled holes; the half slit bung was then slipped on the tube and thermometer, carefully pressed together with shellac varnish, and the bung coated with varnish and pressed home. It is perhaps surprising that there was no leak, but repeated tests showed that the DINES was in this way fixed air-tight in the cylinder. At first a leakage took place where the blackened glass of the instrument was fixed into the metal box. This was closed by painting round the edge with a solution of india-rubber in benzene, and after a good thick layer had been formed it was varnished with shellac varnish.

These dew-point instruments were sometimes excluded, the REGNAULT by withdrawing the hygrometer and filling up the neck of its globe with a bung, the DINES by removing the cylinder altogether.

All the connexions between the different parts of the apparatus were made by glass and very short pieces of india-rubber tubing carefully wired and varnished.

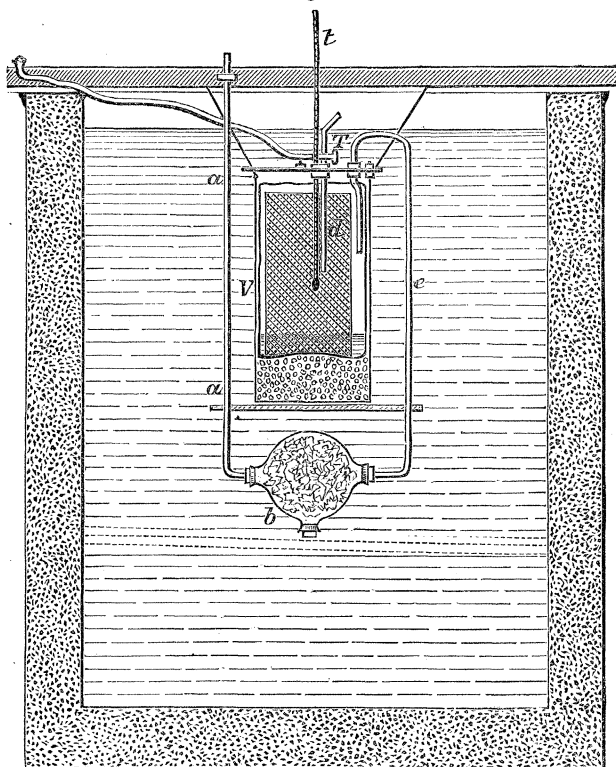
The rest of the apparatus was similar to that already described, but was modified in the following details:—

i. *The saturator.*—This was required to saturate air at a temperature below that of the room. For this purpose a large galvanized iron water tank jacketed with 3 inches of sawdust in a second galvanized iron tank was introduced to form a bath in which the saturator could be completely immersed. The saturator proper will be understood by reference to fig. 5. The glass tube *a* was open to the external air, and led to a spherical glass vessel, *b*, filled with moist sponge; from this vessel a bent glass tube, *c*, led through a cork to the interior of the copper cylindrical vessel, *V*, through a metal lid bolted to it, and made air tight by an india-rubber washer round the rim. The vessel *V* was loaded with shot so that the whole apparatus just sank in water, and above the shot was a thin glass cylinder containing a layer of water at the bottom, in which stood a wire frame covered with well-washed muslin. The tube *C* opened into the vessel *V* outside the muslin frame, but inside the glass. The air was drawn out of the interior of the vessel through the tube *d*, close to which was the bulb of a thermometer; the tube *d* was the extension of a glass three-way tap *T*, which was fixed just outside the copper vessel; by it I could send air through the rest of the apparatus without its passing through the saturator, and thus dry the connecting tubes, &c., when necessary. This part of the apparatus was immersed in water, in the final experiments so that the tap was covered (the projecting nozzle being extended by an india-rubber tube), in the tank already spoken of, and the temperature of the water therein was reduced by stirring ice in it to any required extent.

ii. *The drying tubes.*—Two sulphuric acid or two phosphoric acid ones were used for the observations. These remained as before, except that the connexions (see fig. 1) between them were made by glass tubes and shortened as much as possible.

The ends of the connecting tubes coming through the mercury cups were moreover covered with little glass caps between the observations to protect them from damp during that time.

Fig. 5.



One-tenth natural size.

With these precautions it was assumed that any deposit received in the second tube ought to be added to that in the first tube, and this departure from the practice adopted in the first series of experiments is further justified by the fact that the new aspirator, which will be described presently, more than doubled each of the old ones in volume, so that the tubes had to take up twice the quantity of moisture (for the same temperature) and in the same time as that required for the smaller amount in the previous series. The variable amounts of moisture received by the second tube showed that this part could not be neglected (see § 15).

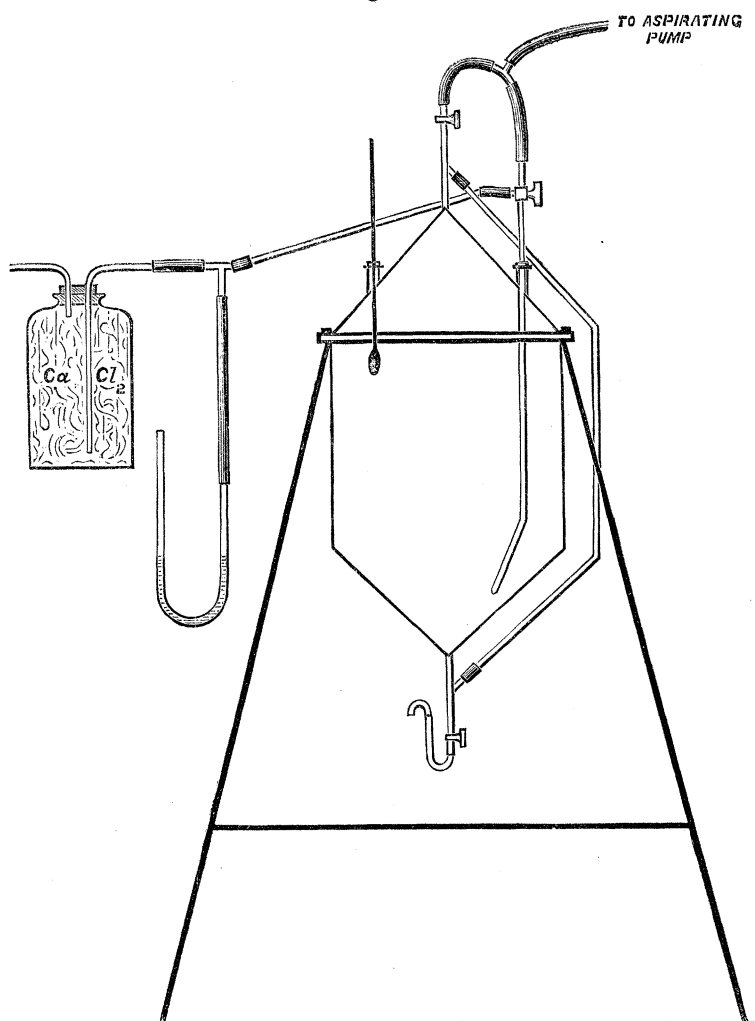
iii. *The aspirator.*—The pattern of this was similar to those already described, but its volume was more than twice as great, and provision was made for increased accuracy in reading; it is accordingly figured in fig. 6. Between it and the rest of the apparatus there was, moreover, a water-pressure gauge attached, which was found very useful, first, as a test of the tightness of the joints of the apparatus; and, secondly, to see that communication was quite free between all the parts. The larger aspirator was used, as it was intended to supply in some cases comparatively dry air, and a larger volume was required to avoid the effect of errors of weighing.

Two determinations of the capacity of the new aspirator were made on the same plan as for the old ones (see p. 80), with the following results :—

1st observation, 35987·44 grammes of water at 12°·7 C.

2nd „ 35989·28 „ „ 12°·2 „

Fig. 6.



One-tenth natural size.

These, corrected for temperature expansion of water and copper, give volumes :—

(1.) 36049·33 c.c. at 15° C.

(2.) 36049·73 „ „

Mean . . . 36049·53 c.c. at 15° C.

The volume of the aspirator is therefore at t°

$$= 36049\cdot5 \{ 1 + \cdot0000525 (t - 15) \}.$$

§ 9. We have first to discuss the comparison of the results given by the saturator with those of the chemical method for air which is not saturated.

The apparatus was mounted on November 7 with the REGNAULT dew-point instrument in position, and the first four observations were wasted by the washer between the copper cylinder and its lid being cut through and letting the water in. It was put in order again on November 17, and gave no trouble afterwards. An observation was taken with the temperature of the water as it stood in the tank, with the following result:—

No. 5.	November 17.	Vapour pressure, by saturator	10·63 mm.
		„ „ chemical method	10·65 „
		Difference	+·02 mm.

Temperature of air, 12°·98 C.

Ice was then added, and when it was melted a second observation was taken, as follows:—

No. 6.	November 19.	Vapour pressure, by saturator	8·54 mm.
		„ „ chemical method	8·59 „
		Difference	+·05 mm.

Temperature of air, 14°·74 C.

These results seemed to give quite satisfactory agreement between the two indications of the states of the air. Passing over No. 7 for the present, the next five gave results which were increasingly divergent, viz. :—

TABLE III.

No.	Date.	Temperature of saturator.	Temperature of air.	Pressure by saturator.	Pressure by chemical method.	Difference.
8	Feb. 9	11·76	14·91	10·27	10·42	+·15
9	„ 9	11·92	18·08	10·38	10·75	+·37
10	„ 11	13·14	15·83	11·24	11·84	+·60
11	„ 12	13·07	14·37	11·18	12·20	+1·02
12	„ 14	13·43	..	11·45	13·30	+1·85

§ 10. I was unable at first to assign any cause for this increasing difference, and thought that in No. 9 it might be due to the air being supersaturated in passing into the saturator, so in the next experiment, No. 10, I passed the air through a tube of ice before it entered the vertical tube α of the saturator (fig. 5). I tried, moreover, whether it was due to the action of the carbonic acid of the air, and interposed a

potash tube in No. 11, but neither of these trials showed that the cause was discovered. The next observation, however, furnished an explanation. The temperature of the saturator was above that of the air, and the chemical method gave a result $\cdot 53$ mm. below that of the saturator. On inspecting the connecting tubes I found a very fine deposit of dew upon them; and, as the apparatus was left in connexion with the saturator during the night, I have no doubt that during the cold part of the night the vapour was condensed in the connecting tubes, and that these were not dry when the observations marked 8 to 12 were made, although I took the precaution of running dry air through them, generally for a quarter of an hour, before each experiment. Thenceforward the connecting tube was left in communication, by means of the three-way tap, with a calcium chloride bottle over night, and the tubes were dried more carefully before the observations. The next set of four observations, however, showed that the amount of moisture in the drying tubes was still too great, the pressure differences being $\cdot 11$, $\cdot 20$, $\cdot 20$, $\cdot 40$ mm. respectively. I then thought that the dew-point instrument might be producing some effect by leakage or otherwise, and accordingly removed it. The temperature of the bath was reduced by ice to $9^{\circ}\cdot 96$ C., and the observation showed a difference of $\cdot 07$ mm. only. More ice was added, and the temperature reduced to $7^{\circ}\cdot 39$ C.: the difference at that observation was $0\cdot 09$ mm.; with further reductions of temperature, however, to $4^{\circ}\cdot 74$ C., $4^{\circ}\cdot 82$ C., and $3^{\circ}\cdot 06$ C., the drying tubes got *too little* moisture, the differences being $\cdot 47$ mm., $\cdot 68$ mm., and $\cdot 81$ mm. respectively.

Then the temperature was allowed to rise gradually, and the difference changed sign, having the values $\cdot 19$ mm. and $\cdot 18$ mm. at $6^{\circ}\cdot 55$ C. and $10^{\circ}\cdot 42$ C. respectively. It appeared that the amount of moisture obtained by the chemical method was practically identical with that given by the saturation temperature, when the saturator was not cooled below 8° C.; below that, when the temperature was falling, the chemical method gave too small a result; but, as the temperature rose again, too large a result was obtained. The next series of observations, Nos. 31 to 38 (Table IV.), shows this order most clearly. Starting with the temperature of the saturator at $16^{\circ}\cdot 78$ C., and gradually cooling it down to $2^{\circ}\cdot 94$ C., then allowing it to rise to $6^{\circ}\cdot 47$ C., I got the following results:—

TABLE IV.

No.	Temperature of saturator.	Temperature of air.	Vapour pressure by saturator.	Vapour pressure by chemical method.	Difference.
					mm.
31	16°78	20	14·20	14·29	+ ·09
32	16·33	18	13·80	13·88	+ ·08
33	16·33	18	13·80	13·82	+ ·02
34	11·96	17	10·41	10·47	+ ·06
35	7·65	17·5	7·78	7·81	+ ·03
36	4·81	16	6·42	5·64	- ·78
37	2·94	18	5·63	4·93	- ·70
38	6·47	18·2	7·20	7·50	+ ·30

It then occurred to me that the effect was probably due to differences of temperature in different parts of the bath.

I had not paid much attention to the uniformity of temperature, as the air was drawn from a point in the interior of the copper cylinder quite close to the bulb of the thermometer, and I considered that the air so withdrawn would be saturated at that temperature, but I had neglected to take sufficient notice of the fact that it was saturated air I was dealing with, and that the effect of the tube which led away the air ought to be considered. It seemed likely that if the water in the bath was not kept thoroughly well stirred, the water, cooled by the melting of the ice, would sink rapidly until the temperature got near 4°, and that then a layer of cold water would be formed on the top of the bath, and cool the top of the cylinder and the tube through which the air came; if this was colder than the saturated air, a part of the moisture would be deposited in the tube; the layer of moisture thus formed would, on the other hand, be at a higher temperature than the rest when the bath gradually got warmer by contact with the air at the top, and the air for the experiment would be slightly warmed and receive an addition to its moisture in passing over this band of moisture. At the end of the series of observations there was no moisture visible on the parts of the tube above the cylinder, but on drawing the tube out of its cork a little way a collar of moisture was distinctly visible where the tube had been cooled by conduction through the cork, and thus my suspicion was confirmed. In consequence of this discovery, considerable attention was devoted to the stirring of the bath, to keep it at a uniform temperature. During an observation I could not hope that the tube would be kept free from deposit, and the only thing to do was to prevent its having any effect on the temperature of saturation. The next six observations show the result of this precaution; the temperature began at 6°·44 C., was reduced to 3°·10 C., and then allowed to rise to 7°·93 C., and therefore included that part of the scale in which the differences were previously most conspicuous.

TABLE V.

No.	Temperature of saturator.	Temperature of air.	Vapour pressure from saturator.	Vapour pressure by chemical method.	Difference.
39	6.44	18.8	7.18	7.25	+ .07
40	6.00	19.3	6.97	7.01	+ .04
41	3.10	19.9	5.70	5.63	- .07
42	4.76	19.9	6.40	6.43	+ .03
43	7.44	21.4	7.69	7.71	+ .02
44	7.93	21.1	7.95	8.09	+ .14

These six observations gave results agreeing very closely, the mean difference between the tabular saturation pressure and the pressure as deduced from the chemical method being .06 mm. only. It will be observed that the same degree of accuracy of agreement is given by the observations in Table IV., which are free from the difficulty of the deposit on the leading tube, the mean of the differences for the first five observations in that table being .056 mm.

As the temperature readings could not be relied upon to an accuracy much beyond 0.1 C., it seemed unnecessary to endeavour to press the comparison of the saturator and the chemical method further.

§ 11. I concluded that I then knew enough about the methods to be justified in expecting the results of the chemical method to agree with the tabular result obtained from the temperature of the saturator within 0.1 mm., and in regarding the air under those circumstances as being in a known state, and in using it in the investigation of the action of the dew-point instruments. It was, however, necessary to determine whether or not the presence of these instruments affected the air by leakage or otherwise: so that for the succeeding observations I always determined the pressure by the chemical method. First, the REGNAULT was replaced: the result appeared to be disastrous, as the first two observations gave differences between the saturator and chemical method results of 1.06 and - .37 respectively, but an investigation disclosed a deposit of moisture in the tube above the tap. The rest of the observations taken are all given in the following table, and notes are appended in cases where some disturbing cause was acting.

TABLE VI.

No.	Date.	Hygrometer included.	Temperature, saturator.	Temperature, air.	Vapour pressure, saturator.	Vapour pressure, chemical method.	Difference.	Remarks.
47	July 19	REGNAULT	16.85	19.14	14.26	14.28	mm. + .02	
48	" 19	"	16.89	19.3	14.30	14.27	- .03	
49	" 21	"	2.94	19.6	5.63	5.90	+ .27	
50	" 22	"	2.98	20	5.65	5.75	+ .10	
51	" 22	"	3.68	20	5.93	6.01	+ .08	
52	" 23	"	7.09	20.5	7.51	7.53	+ .02	

TABLE VI.—(continued).

No.	Date.	Hygrometer included.	Temperature, saturator.	Temperature, air.	Vapour pressure, saturator.	Vapour pressure, chemical method.	Difference.	Remarks.
53	July 24	REGNAULT	3°22	20°	5·75	5·73	mm. — ·02	Saturator not completely covered by water.
54	" 24	"	3·89	20	6·02	6·00	— ·02	
55	" 25	"	7·08	19	7·47	7·57	+ ·10	
56	" 26	"	8·73	17	8·39	8·52	+ ·13	
57	" 28	"	11·74	18·18	10·26	10·29	+ ·03	
58	" 29	"	13·09	19	11·21	11·48	+ ·27	
59	" 31	"	15·25	21·5	12·88	13·35	+ ·47	Water not stirred. The drying tubes were left in connexion with the apparatus for some time, and moisture was gained by diffusion.
60	Aug. 1	"	16·49	24	13·94	14·00	+ ·06	Hygrometer showed increasing signs of leaking at the joint; was accordingly taken out and replaced.
61	" 1	"	16·85	24	14·26	14·72	+ ·46	
62	" 2	"	17·62	24	15·00	15·45	+ ·45	
63	" 2	"	18·12	24	15·45	15·84	+ ·39	
64	" 4	"	18·82	22	16·14	16·50	+ ·36	
65	" 5	"	18·45	21	15·86	16·08	+ ·22	
66	" 10	REGNAULT and DINES.	21·60	27	19·16	19·42	+ ·26	The globe containing the REGNAULT had been washed and not thoroughly dried.
67	" 11	"	22·02	25	19·65	19·72	+ ·07	DINES read during the observation. Its tube became covered with moisture that was not taken up subsequently, and therefore lost to the drying tubes. The method of reading was therefore altered (see § 22).
68	" 12	"	22·06	23	19·70	17·04	— 1·96	
69	" 12	"	22·09	24	19·74	19·16	— ·58	
70	" 16	"	20·27	23·5	17·66	16·90	— ·76	
71	" 19	"	20·63	23	18·05	16·56	— 1·49	
72	" 21	"	20·09	23	17·46	17·73	+ ·27	
73	" 22	"	20·03	24·5	17·39	17·34	— ·05	
74	" 27	"	15·75	18	13·30	13·47	+ ·17	84 lbs. of ice were melted to cool the bath, and it is probable that the leading tube remained too cold during the observation.
75	" 28	"	15·72	18	13·27	13·23	— ·04	
76	" 29	"	6·38	18·5	7·15	6·87	— ·28	
77	" 30	"	7·41	17	7·68	7·76	+ ·08	
78	" 30	"	7·96	18	7·97	7·95	— ·02	
79	Sept. 1	"	11·71	18	10·24	10·33	+ ·09	
80	" 1	"	12·90	18·5	11·06	11·14	+ ·08	
81	" 3	"	14·50	18·5	12·27	12·31	+ ·04	
82	" 5	"	1·09	17	4·94	4·91	— ·03	

It will be seen that there are definite reasons for rejecting the experiments numbered 56, 59, 61-65, 68-72, and of the rest there are five only for which the differences reach 0.1 mm., and the mean difference for the whole number (without regard to sign) of those not rejected amounts to .077, or again less than .1 mm.

§ 12. Rejecting from Table VI. then the numbers indicated, and arranging the observations in the order of humidity of the air, we get the following re-arrangement of the Tables V. and VI. :—

TABLE VII.

Number of experiment.	Temperature of air.	Percentage humidity.	Difference between result of chemical method and saturation pressure.	
			Positive.	Negative.
	°		mm.	mm.
50	20	32	+ .10	
49	19.6	33	.27	
41	19.9	33	..	— .07
53	20	33	..	.02
51	20	34	.08	..
82	17	34	..	.03
54	20	35	..	.02
42	19.9	37	.03	
43	21.4	41	.02	
40	19.3	42	.04	
52	20.5	42	.02	
44	21.4	43	.14	
39	18.8	45	.07	
76	18.5	45	..	.28
55	19	46	.10	
78	18	52	..	.02
77	17	53	.08	
60	24	63	.06	
57	18.5	64	.03	
79	18	67	.09	
80	18.5	75	.08	
73	24.5	76	..	.05
81	18.5	78	.04	
67	25	82	.07	
48	19.5	86	..	.03
74	18	87	.17	
75	18	87	..	.04
47	19.14	87	.02	
28	15	88	.01	
27	14.8	91	..	.05
5	13	96	..	.07
13	12.7	100	..	.18

The last four observations are not among those quoted in Tables V. and VI. ; they were taken before the behaviour of the saturator and its difficulties were fully understood. They are, however, observations for a very high degree of humidity, and in such cases the peculiar difficulties of the apparatus do not come in ; they may therefore, I think, be fairly included in the table of final results.

§ 13. I have now accounted for the whole number of 82 experiments made with the modified apparatus (including those in which dew-point readings were taken) for comparing the saturator and the chemical method at different degrees of humidity, with the exception of three, Nos. 7, 25, and 26. No. 25 gave a difference of -0.07 when the humidity was 70, and simply confirms the general result; No. 7 gave a difference of $+1.73$ when the humidity was 87; and No. 26 a difference of -0.57 when the humidity was 86; these show a wide divergence, for which I am quite unable to account. There is nothing in my note-book which places those observations on a different footing from the rest; it is true that they were made at a time when I had not fully appreciated the necessity of keeping the bath at a uniform temperature, but the difficulties did not occur with other observations at corresponding humidities. I can only attribute the large differences to clerical errors in entering the weights.

§ 14. The reductions of the observations have been made by formula 3 of p. 78, with the aid of the table of p. 79. The value there adopted for the specific gravity of steam referred to air at the same temperature and pressure is $.622$. The table of vapour pressures which I used at first was REGNAULT'S; those given in the results are derived from the re-calculation in LANDOLT and BÖRNSTEIN'S table. The effect of this change is to diminish the saturation pressure by $.02$ mm. or $.03$ mm. in each case, and has consequently given to the Table VII. a preponderance of $+$ differences which did not show itself when REGNAULT'S numbers were taken. As the table now stands, 20 observations give a positive difference and 12 a negative one. If the $.03$ mm. be restored 14 observations will have a positive difference, 15 a negative one, while 3 will give zero difference. Almost precisely the same result would be attained by using the value $.624$ for the specific gravity of steam instead of $.622$. The one observation on saturated air with the negative difference $.18$ mm. in the second series of experiments bears out the results of the previous experiments with the original apparatus (§ 5).

The net result of the whole investigation upon the chemical method would seem to show:—

(1.) That if the specific gravity of saturated aqueous vapour referred to air at the same temperature and pressure be assumed to be $.624$, the calculation of the pressure from the weight of moisture absorbed gives a value agreeing to within 0.1 mm. (the mean difference being $.07$ mm.) with that derived from the temperature of saturation, provided that the air is superheated after passing through the saturator.

(2.) That there is no reason for assigning the differences in the observations to other than experimental errors.

(3.) That for saturated air, the result of the chemical method is slightly less than the tabulated saturation pressure. This is shown in the first series of experiments, and agrees with REGNAULT'S result. But I think that the later experiments and the observations with the dew-point instrument which follow clearly show that when saturated air is passed along glass tubes at the same or even a slightly higher

temperature a deposit of moisture is formed on the glass. The observations of REGNAULT and HERWIG are in favour of this view; and, if that be so, it is clearly impossible to conduct saturated air along glass tubes to the drying tubes; some of the moisture will be deposited on the way. How much moisture will be required to saturate the glass is, I believe, unknown; but if the conducting tubes are maintained at a temperature a degree or more above that of the air when it is being saturated the deposit will not take place.* Thus, in the observations upon air saturated at a temperature below that of the glass, the air could only travel a very short distance before it came in contact with glass at a temperature higher than its own, and the amount of moisture necessary to saturate this short length of glass would be supplied by the saturated air passed through the apparatus as a preliminary part of the experiment. So that this suggestion seems to me to explain satisfactorily what is otherwise a great difficulty, namely, that the saturation pressure agrees with the tabulated pressure only if the air is heated after being saturated.

To assume that the result of the chemical method should agree exactly with that given by the temperature of the saturator is to assume not only DALTON'S law to be true, but also that the expansion of vapour with rise of temperature from the saturation point takes place, according to the law of GAY-LUSSAC, with a coefficient of expansion the same as that of air. The specific gravity of steam which must be substituted, therefore, in order to obtain identical results by the two methods is not the actual specific gravity of steam in the unsaturated air experimented on, but what would be the specific gravity if the vapour obeyed GAY-LUSSAC'S law in the manner indicated, or (since the specific gravity is referred to air at the same temperature and pressure) the specific gravity of the saturated vapour. Thus the observations may be employed (as REGNAULT employed similar observations) to determine the specific gravity of saturated steam at the temperature of the saturator. The observations do not admit of sufficient accuracy to trace the variation of the specific gravity with temperature, but the mean specific gravity for the 32 experiments of Table VII. for temperatures of steam between 1° C. and 21° C., if referred to air at the same temperature and pressure, is .6245.

CLAUSIUS has calculated the specific gravity of saturated steam by thermodynamic reasoning from other known constants of steam ('Mechanical Theory of Heat,' p. 153). Assuming the value at 0° C. to be .622, he gives the value at 50° C., .631. From his results, therefore, we obtain .6240 as the mean value between 1° C. and 21° C., a result agreeing very closely with that obtained from my observations.

If, however, the specific gravity of saturated steam be greater than that of unsaturated steam, the vapour cannot obey GAY-LUSSAC'S law in the manner stated, and in consequence the pressure of the vapour of unsaturated steam, calculated by the chemical method, with the true specific gravity of unsaturated steam (.622) ought to be somewhat greater than that given in the table of pressures of saturated vapour; and the results of Table VII. may accordingly be held to prove that the true pressure of

* See note at end of paper (p. 149).

vapour in unsaturated air is greater than the tabulated pressure for the temperature of the saturator by $\frac{2}{622}$ of the saturation pressure, that is, by an amount varying from .02 mm. to .06 mm. for vapour pressures between 4.94 mm. and 19.65 mm. In other words, to obtain the true pressure the proper value to substitute for d in the formula of reduction of the chemical method is .622, at any rate when the fraction of saturation of the air under experiment is less than .8, but if the air is saturated the value .624 must be substituted.

NOTES. (November 11, 1887.)—(1) In order further to verify the condensation of moisture upon a surface when the air surrounding it is not fully saturated, I have tried to detect the actual difference of weight produced by the deposit. For this purpose I have suspended a glass globe from one arm of a balance, and have counterpoised it, surrounding the globe by a cylinder with a lid formed of two closely-fitting pieces of glass, with a small hole left for the suspension wire. Inside the cylinder were placed alternately a vessel of water and a vessel of sulphuric acid. After making all allowances for the density of the air displaced, the experiments show an excess of weight in the moist air corresponding to the condensation of about 1×10^{-5} gramme per square centimetre. More accurate experiments are, however, in progress.

(2) In this discussion DALTON'S law has been assumed to be strictly accurate. If the differences observed by REGNAULT (p. 120) should prove on further investigation to be real differences, the specific gravity of the saturated vapour must be increased to about .645 for saturated air, and .643 for non-saturated air.

§ 15. In conclusion, it may be well to give a specimen of the observations from which the tables are compiled, and some notes of the practical conduct of the work. I take one experiment at random from my note-book : No. 68, August 12.

		grammes.		grammes.	Barometer.	Temp.	Balance Case.
Initial weight of tubes V., VI.	.	142.8090	III., IV.	147.9447	30°00		24°70
Final " "	.	143.3856	" "	147.9638	30°00		25°00
Gain5766		.0191			
		Total gain5957 gramme.					

	Time.	Saturator temp.	REGNAULT	DINES.	Air temp.		
	12.00	22.39	—	—	23.80	Final temp. of aspirator 24°00 $e \text{ (tubes)} = \frac{1.0280 \times .5957 \times 1.00323}{36066.5}$ $= 19.78$ $e \text{ (sat.)} = 19.70$	
	12.15	22.38	—	—	23.80		
	12.30	22.38	—	—	23.78		
	12.45	22.37	—	—	23.73		
	1.00	22.37	21.15	21.15	23.80		
	1.15	22.38	—	—	23.80		
	1.30	22.37	—	—	23.82		
	1.45	22.37	—	—	—		
	1.53	22.37	—	—	24.00		
Duration 1hr. 53min. Temp. correction . .	Means ..	22.37 .31	21.15 .27	21.15 —	— .11		Diff.08
		22.06	20.88	—	—		

The balance was a short-beam OERTLING, and the weights a box of OERTLING'S, compared with the Laboratory Standard. The weighings were carried to 0·1 mgm. The drying tubes after an experiment were transferred to a desiccator to cool, and then weighed. The barometer and temperature of the balance case were read at each weighing, and recorded, so that a correction for weighing in air might be introduced if necessary. (See § 4.) For Experiments 1–27 the tubes were filled with sulphuric acid and pumice, and those for 28–82 with phosphoric acid. The alterations in the state of surface of the tube are sufficient to prevent the accuracy of weighing to 0·1 mgm. being entirely trustworthy, but the error is less than 1 mgm. (See p. 74.) This limit gives to the accuracy of the weighing operations in the chemical method a limit of ·6 per cent. for the smallest amount of moisture, viz., ·1737 gramme, and of ·16 per cent. for the largest amount, viz., ·6880 gramme.

The duration of the experiments was generally about two hours, varying, in fact, from 1 hr. 10 min. to 2 hr. 55 min. During that time 36 litres of air passed through the tubes. The amount received by the second tube was very variable. In 29 experiments out of the 82 it was less than 1 mgm.; in one experiment, however, it reached 99 mgm. The amount was always added to that received in the first tube, for reasons previously stated (§ 6–8).

Before an experiment was commenced dried air was drawn through the whole of the apparatus, except the saturator and aspirator, by the use of an aspirating water-pump for from a quarter to half an hour, to clear the tubes of deposited moisture; for this purpose the third aperture of the three-way tap was employed. After this the plug of the tap was turned, and the air passed through the saturator for a quarter of an hour or more to fill the vessels with air in the state required for the observation. The pump was then cut off, the aspirator connected, and the drying tubes put in their places; the entry tube to the saturator was then closed, and the tightness of the apparatus tested by reading the pressure gauge and observing the cessation of drip from the aspirator. At the close of an observation the moist air was swept out of the globe, &c., by dried air (using the water-pump), unless it was intended to take a second observation immediately; when left over night, the globes, &c., were connected with a calcium chloride bottle.

The temperature readings were by the following thermometers, compared at Kew, as described in § 3:—

Saturator	Exp. 1–14	GEISSLER, C.L.C. 21	graduated to 0°·1 C.
„	„ 15–82	HICKS 79,916	„ 0°·2 C.
Air		HICKS 87,400	„ 0°·2 C.
REGNAULT		HICKS 79,915	„ 0°·2 C.
DINES.—Its own thermometer, by HICKS (not compared).			

Aspirator.—An ordinary chemical thermometer compared in the Laboratory, graduated to 0°·5 C.

Other small points of detail, employed to secure as close an accuracy as possible, may be passed over.

DEW-POINT HYGROMETERS.

1. REGNAULT'S HYGROMETER.

§ 16. I pass on now to consider the observations made with REGNAULT'S hygrometer upon air in a known state of humidity. The method by which the thimble was exposed to the air to be investigated has already been described (§ 8).

I need now only give particulars of the apparatus adopted for cooling the ether and for reading. The thermometer had a very short cylindrical bulb, and was well covered by the ether in the thimble. The stem of the thermometer was passed through a cork fitting into the glass tube which held the thimble, and through the cork passed also two very narrow copper tubes, one to the bottom of the ether, the other just through the cork. The end of the latter was connected to a MAGNUS aspirator, which was provided with a tap, so that the rate of passage of the air was under very easy regulation. As the thimble was enclosed in a glass globe, it could be viewed quite closely without any fear of the presence of the observer altering the dew-point.

§ 17. So far as I can gather from published accounts, REGNAULT'S instrument has always been regarded as a standard (see Note A, p. 130); that is to say, a dew-point determination has been held to give a final verdict as to the pressure of vapour in the atmosphere. REGNAULT'S directions for the use of the instrument are, first of all, to cool the ether so as to obtain a deposit, and note the temperature; then let the temperature rise until the deposit is gone; then cool slowly by tenths of a degree until two-tenths are determined, for the lower of which there is a deposit and for the higher no deposit; the mean gives the dew-point, from which the pressure of vapour in the air is at once obtained by reference to the table of vapour pressures. No calculation is necessary on account of the temperature of the air, because a change of temperature of the air would not cause any appreciable change of pressure, either of dry air or vapour, but only a change of volume. With regard to this method of observation, I may remark that it is not always possible to confine "deposit" and "no deposit" temperatures within the limits of a tenth of a degree. It is comparatively easy to do so when the circulation of air in the neighbourhood of the polished surface is brisk, and I have found from some experiments on dew-point instruments in the strong current produced by a rotary fan that the air current simply improved the facility of reading the instrument, and did not seem to alter the final reading; but this opinion is, I believe, not shared by all experimenters, and I do not press it now, as my observations assumed to some extent the permanence of the condition of the air operated on.* In the experiments I am now dealing with the circulation was slow. The thimble of the hygrometer was contained in a globe of 5 cm. radius, holding therefore about half a litre; through this 36 litres of air were passed on an

* On this point, see below, pp. 111 and 142.

average in two hours, giving a minimum motion of 3 cm. per minute—assuming, that is, that the motion was uniform over the whole central section of the globe. The motion at the thimble in the middle of the globe was probably considerably greater than this, but at any rate could not be called rapid. Under such circumstances the temperature of the thimble and ether does not rise uniformly, so that one had to keep the ether mixed by an occasional bubble of air, and the practical temperature limits of deposit and no deposit, though sometimes two-tenths, were sometimes four-tenths of a degree centigrade.

§ 18. Let us call the temperature of saturation of the air the theoretical dew-point. Then, in order that the observed dew-point may coincide with the theoretical, the following assumptions seem to be required:—(1.) That as soon as the temperature of the thimble is below the theoretical dew-point a deposit of moisture is formed. (2.) That the observer can see it. (3.) That there is no deposit when the temperature is above the theoretical dew-point. These three assumptions may be regarded as independent, though their consideration must to some extent overlap. I will take No. 2 first. The possibility of seeing a deposit depends upon circumstances. In the first place the thimble must be highly polished. In one observation, No. 15, when the thimble was tarnished, the saturator being at $16^{\circ}96$ C., and the chemical method corresponding to a temperature of $17^{\circ}2$ C., two readings of temperature of earliest *visible* deposit were 15° C. and $16^{\circ}3$ C. respectively, either of them much lower than the theoretical dew-point, and indicating uncertainty in the readings, which did not occur when the thimble was well polished. Secondly, the illumination is of importance. On this point no general direction seems useful; my observations were made indoors, in a room with two windows. I found that I could see the deposit most easily sitting with my back to the windows, and having a reflector or a lamp placed almost behind the thimble, so that the light grazed the side of it, a uniform dark background being immediately behind. Small specks on the polished surface seem to facilitate the reading, the first indication of a deposit being a slight fringe round the specks. Thirdly, different observers do not take the same readings, although practice may do away with the differences.

I was assisted in the observations by one of the students at the Cavendish Laboratory, who had had no previous experience in dew-point readings; at first our readings differed considerably, mine being the higher. The following cases show this:—

Saturator.	Dew-point.	Observer.
$17^{\circ}4$	$16^{\circ}92$	S. H.
..	$17^{\circ}17$	W. N. S.
$13^{\circ}96$	$13^{\circ}53$	S. H.
..	$13^{\circ}68$	W. N. S.

The differences were still larger in the earlier observations, beginning indeed with a whole degree, but with continued practice they diminished, and after a time became no longer perceptible. As a rule, however, the observation of the dew-point fell to my share. It was necessary, in order to prevent unconscious bias towards a known result, that the observer who read the dew-point temperature should not know what the saturator thermometer was indicating, and in the division of duty I took the dew-point readings; but, as I have said, in the later observations our readings agreed, and if we exchanged the duties there was no recognisable difference.

§ 19. With the utmost anxiety to be quite fair and to observe only what was really to be seen, it was sometimes difficult to form a satisfactory opinion as to whether a deposit was on the thimble or not. Here, for instance, are some notes made on dew-point readings during Experiment 6 (air temperature 15° C.) :—

Time.	Saturator.	Thermometer readings.	Remarks.
12.40	$9^{\circ}55$	$8^{\circ}7$	Very faint deposit.
1.0	$9^{\circ}50$	$8^{\circ}6$	" "
..	..	$9^{\circ}7$	Deposit gone.
1.15	$9^{\circ}35$	$8^{\circ}9$	Deposit round specks.
1.25	..	$9^{\circ}0$	Fine deposit specks.
1.30	$9^{\circ}50$	$10^{\circ}9$	Deposit gone.
..	..	$10^{\circ}0$? Deposit.
2.0	$9^{\circ}50$	$9^{\circ}45$	Dew } very faint.
2.15	$9^{\circ}50$	$9^{\circ}70$	Dew }
3.0	$9^{\circ}50$	$7^{\circ}3$	Heavy dew.
Thermometer correction . }	.50	.27-.24	

In this case the observation was exceptionally difficult in consequence of the slower rate of aspiration. According to the assumption, I ought to have seen the deposit when the temperature reading was below $9^{\circ}24$, and not otherwise. At other times observations seemed to be easier; the following is extracted from my note-book for the next experiment :—

Saturator temperature $12^{\circ}6$ – $12^{\circ}75$. Mean $12^{\circ}68$. Correction $0^{\circ}42$.

Mean theoretical dew-point $12^{\circ}26$.

Dew-point observations.	
Dew on.	Dew off.
$12^{\circ}2$	$12^{\circ}4$
$12^{\circ}2$	$12^{\circ}5$
$12^{\circ}1$	$12^{\circ}5$
$12^{\circ}4$	$12^{\circ}6$
$12^{\circ}4$	$12^{\circ}8$
$12^{\circ}5$	$12^{\circ}5$

Mean dew-point	12 ^o ·42
Correction	·22
Observed dew-point	12·20
Difference from theoretical	·06

The impression that is left on the mind by observations with the instrument is a sense of insecurity and want of confidence in the observations, that is sometimes followed by surprise at the concordance of the reading with the theoretical dew-point, and the closeness with which the indication of the dew-point follows any change in the temperature of the saturator, as, for instance, in the example just quoted, where the saturator readings rose from 12^o·18 C. to 12^o·33 C., the dew-point readings followed from 12^o·08 C. to 12^o·28 C.

§ 20. The other two assumptions referred to in § 18 may be tested by the accuracy of agreement of the final dew-point readings deduced from the observations with the theoretical dew-point given by the saturator, and the chemical method. The results of the comparison for a number of observations are given in the following table; the numbers of the experiments are the same as those of Table VI.

TABLE VIII.

Number of experiment.	Temperature of air.	Percentage humidity.	Theoretical dew-point.	Observed dew-point by REGNAULT.	Difference.
50	20	32	2·98	2·90	—·08
49	19·6	33	2·94	2·91	—·03
53	20	33	3·22	2·94	—·28
51	20	34	3·68	3·63	—·05
82	17	34	1·17	1·08	—·09
54	20	35	3·89	3·76	—·13
76	18·5	45	6·14	6·10	—·04
55	19	46	7·08	6·85	—·23
78	18	52	7·96	7·88	—·08
77	17	53	7·41	7·33	—·08
60	24	63	16·49	16·35	—·14
57	18·5	64	11·74	11·71	—·03
79	18	67	11·73	11·71	—·02
80	18·5	75	12·90	12·91	+·01
73	24·5	76	20·03	20·19	+·16
81	18·5	78	14·50	14·62	+·12
67	25	82	22·02	22·19	+·17
48	19·5	86	16·89	16·92	+·03
74	18	87	15·75	15·72	—·03
75	18	87	15·72	15·72	·00
47	19·14	87	16·85	16·80	—·05
5	13	96	12·38	? 12·38	? ·00

The theoretical dew-point is taken from the temperature of the saturator, but the observations included in the table are only those for which the result of the chemical method agreed very closely with that of the saturator.

It will be seen that the differences are most frequently negative. They are generally very small, so that it would appear that the assumptions specified on p. 101 are generally justified by the experiments. There are two cases in which the dew-point is considerably below the saturator temperature, namely, Nos. 53 and 55. I quote the observations for these two experiments:—

No. 53.

Time.	Saturator.	Dew-point.
	°	°
12.34	3·57	3·10
12.49	3·40	3·05
1.10	3·38	3·05
1.40	3·39	3·10
1.55	3·40	3·05
2.10	3·43	3·30
2.25	3·47	3·35
2.40	3·50	3·30

If in this case we take the last three readings only, we get—

Mean temperature of saturator	3°·47
Correction	·23
	———— 3°·24
Mean dew-point	3°·32
Correction	·22
	———— 3°·10
	————
Difference	·14,

so that a nearer approximation would be obtained. It appears that the last three observations correspond to no physical change in the air, but simply refer to some alteration in what was taken as indicating visible dew. This example well illustrates the uncertainty that rests with dew-point determinations.

No. 55.

Time.	Saturator.	Dew-point.
	°	°
3.5	7.56	7.15
3.20	7.59	7.05
3.35	7.21	6.9 7.4
		7.15
3.50	7.22	6.8 7.2
		7.0
4.10	7.27	6.8 7.2
		7.0
4.25	7.31	6.95 7.3
		7.12
4.40	7.36	7.1 7.2
		7.15
4.55	7.40	7.30
Mean	7.40	7.11
Correction32	.26
	7.08	6.85

In this case no reason can be readily assigned for the difference. It must be attributed to mere errors of observation, but the rapid and unusual variation of the temperature of the saturator may account for part of the difficulty of obtaining concordant dew-points.

The other noteworthy cases are those in which the dew-point was higher than the saturator temperature, Nos. 80, 73, 81, 67, and 48. In the first four of these experiments I was paying special attention to the very faintest indications of the presence of dew. I had come to the conclusion, from observations with DINES'S hygrometer conducted in the manner detailed below, that it was possible to get a dew deposit upon a glass surface at a temperature above the theoretical dew-point, and I was endeavouring to ascertain if the same occurred with the REGNAULT instrument. It would appear that to a certain extent that is the case. I give my observations and notes for Experiment 81 :—

No. 81.

Time.	Saturator.	Dew point.	Remarks.
5.30	14.79	14.80	Deposit Deposit gone
5.45	14.80	14.70	
6.0	14.80	14.72	Deposit Deposit gone
6.15	14.80	14.80	
6.27	A dew-point reading taken, cooling very slowly. Dew seen at 15°.0. Dew-point 15°.02. Observation repeated: dew at 14°.9
6.30	14.80	14.9	Dew at 14°.9, very definite indication
6.45	14.81	14.85	Dew at 14°.85, very definite indication
Thermometer correction	0.30	0.18	

The dew-point is put down as the temperature of appearance of dew in the last two readings, as the deposit was extremely transient. It would appear from this that dew may, with very great care, be seen at a temperature from 0°.16 C. to 0°.32 C. above the dew-point. The other observations are merely ordinary dew-point determinations, and may be affected by too high a reading being set down for the temperature of disappearance; but I had no doubt in the case quoted above. I find also the following note in my note-book for Experiment 82, when the saturator read 1°.18 C. (corrected): "There is a change in appearance of the silver at 2°.1 (uncorrected), and the dew deposit is very slight even when the temperature is considerably below the dew-point. The dew-point as 'naturally' determined is given in the table. The plan of reading deposit both on and off is quite useless, as the dew often disappears when the thermometer shows a temperature below that at which dew was previously deposited." If the change in appearance at 2°.1 indicated a deposit, allowing for thermometer corrections, the deposit would have been seen at 1°.88, when the theoretical dew-point was 1°.18, in other words at a temperature of 0°.7 above the saturation temperature.

§ 21. It would appear, therefore, that the third assumption of p. 101, viz., that there is no deposit unless the temperature of the thimble is below the theoretical dew-point, is not strictly true if the very first indications of a dew deposit are taken into account.

With the exceptions thus mentioned, the theoretical and observed dew-points are in very close agreement. It may be said that at first an observer would probably obtain too low a reading with REGNAULT'S instrument, and that with a fair amount of practice his observed dew-points would probably agree on the average with the temperature of saturation, although there might be exceptional observations with somewhat widely diverging readings, depending possibly upon the illumination of the

thimble. Finally, if very closely observed, the dew-point would be some tenths of a degree too high, in consequence of a slight deposit formed above the true dew-point.

DINES'S HYGROMETER.

§ 22. The method of introducing the instrument into the air to be investigated has been already described, § 8. The presence of the instrument caused at first considerable interference with the comparison of the saturator and chemical method. In order to cool the glass surface upon which the deposit is formed, water, cooled by dissolving ice in it, is made to pass along a metal tube (fig. 4), which runs close to the stem of the thermometer *t*, and ends in the metallic box B, into which the thermometer dips. The small cistern which holds the cooled water is in the ordinary course screwed on to the leading tube at A. I found, however, that the neighbourhood of this cold reservoir caused a deposit of moisture in the tubes of the apparatus near it, and in consequence spoiled Experiment 71, and the previous Experiments 68, 69, and 70 had been rendered useless by the deposit formed in the long metal tube, which must, of course, be cooled below the dew-point and cause condensation before the glass surface can be cooled. So that the observations of dew-point with DINES'S hygrometer reduced from these experiments are not of much value. In order to avoid these defects the flow of water in the DINES was reversed, so that it had only to pass through a very short length of tube before reaching the box, and the cistern was separated from the rest of the apparatus by a considerable length of india-rubber tubing. Further, the observations were only taken with DINES'S instrument at the close of an experiment, after the drying tubes had been detached from the aspirator. The apparatus was then connected with an aspirating air pump, and air drawn through the saturator and the vessel containing the hygrometer.

§ 23. The observation of a dew deposit upon the surface of the blackened glass is a very easy matter, and much more satisfactory than with the REGNAULT thimble. The arrangement that I have found to give the easiest observation is to attach a piece of black paper to the middle of the window of the room, and look at the glass surface from such a direction that it reflects only the black paper when there is no dew on the surface. Under these circumstances a deposit shows itself with surprising facility. The observations taken with DINES'S instrument were as follows:—

After Experiment 73.	Saturator reading	20°·05
	DINES, 68° F. =	20°·00
	—	0°·05 diff.

„ 77. Saturator reading, 7°·49.
DINES first showed dew at 10°·6, but no more at 8°·3; on a second trial dew at 7°·8.

After Experiment 78. Saturator reading, $8^{\circ}00$.

DINES :—Water run through slowly, dew appeared at the first edge (nearest to the entering water) at $9^{\circ}6$; water stopped :—temperature gradually sank to $9^{\circ}00$, and dew appeared at the other end, the first deposit vanishing. The second deposit was visible only in the best light and vanished without any measurable alteration of temperature. Observation repeated :—Dew found at first edge at $9^{\circ}72$, but no dew formed on other parts, although the temperature was reduced to $9^{\circ}06$. The temperature was run down to $7^{\circ}78$ before anything like a wide deposit of dew was formed. An estimate of $8^{\circ}3$ was made as the dew-point, and confirmed by repetition of the observation.

„ 79. Saturator reading, $11^{\circ}61$.

DINES :—Dew at further end at $15^{\circ}61$. Thermometer went down without any more cold water to $13^{\circ}6$, and the dew extended partly over the glass. Dew at both ends at $13^{\circ}06$; vanished from the one end without alteration of temperature; disappeared altogether, although the thermometer fell to $12^{\circ}67$.

„ 80. Saturator reading, $13^{\circ}83$.

DINES :—Observation taken without air being drawn through, *i.e.*, without circulation; a deposit was formed at the near end only at $9^{\circ}28$.

„ 81. Saturator reading, $14^{\circ}51$.

DINES :—Temperature reduced to $14^{\circ}00$ without drawing any air through; on starting the air, dew was deposited at first on the edge nearest to the air inlet, and it gradually extended over part of the glass surface without alteration of temperature.

„ 82. Saturator reading, $0^{\circ}80$.

DINES dew-point, $1^{\circ}22$.

With ice only, the temperature of DINES could not be reduced below $1^{\circ}22$, at which temperature the dew was just visible. Calcium chloride and sodium chloride were then added to the ice, and the following observations made. The faintest visible deposit was at $1^{\circ}2$. Cooling down to $0^{\circ}56$ increased the deposit, but not very greatly. This was repeated many times, and in one experiment the thermometer was taken down to 0° ,

but the deposit was not then very large, though quite distinct.

On September 4, the day after Experiment 81, observations were made with DINES'S instrument, but no observation with the chemical method was made.

The object of the experiment was to watch for the deposits of the dew when the temperature of DINES was maintained as nearly as possible at constant temperatures.

The following are the observations :—

Experiment 83.

Time.	Saturator.	Air temperature.	DINES.
b. m.	°	°	
3 28	15·09	16·04	
3 30	15·01	..	Thermometer run slowly down: dew first showed at the near end at 15°·00, and gradually extended all over the further side while the thermometer fell—ultimately to 12°·2; the deposit continued to increase, but only on the same portion of the surface.
			Dry air passed through and deposit removed.
3 45	15·00	..	Dry air stopped and damp air sent through. DINES reading 12°·89. Deposit formed on a portion only of the face.
			Deposit removed by dry air.
3 52	14·98	..	DINES, 13°·89; damp air turned on and gave a deposit after some time.
3 55	DINES, 14°·44; damp air put on; dew visible in 1m. 45s. Dry air put on; dew gone in 11s.
4 0	15·00	..	DINES, 15°·00. First sign of dew visible at 1m. 11s. after starting air. DINES was then standing at 15°·22; it rose to 15°·44, the dew still remaining.
4 6	15·00	..	DINES, 15°·56; damp air sent through, a very faint deposit of dew appeared, which did not disappear until the thermometer had reached 15°·72.
4 20	15·00	..	DINES cooled again slowly. First indication of dew at 15°·7, very faint; water stopped, and thermometer fell to 15°·4, and dew slightly increased. Still on at 15°·6, rather fainter at 16°·1.
4 35	15·01	..	The deposit did not completely disappear until the thermometer reached 16°·94.
4 46	The water was then run through still more slowly. A deposit of dew was visible when the thermometer reading was 16°·2, and the water was then kept just flowing. The thermometer reading never went below 16°·1, but the dew deposit was quite distinct.
5 7	15·02	..	Damp air turned on, and last observation repeated. Started at 16°·7; dew began to form almost immediately, and the flow was stopped, the thermometer fell to 15°·8, showing increase of dew without further supply.
			To verify that there was a real deposit, and not an incidental reflexion, the tap was turned and dry air sent through, the spot on the glass being carefully watched. The cloud distinctly cleared off.

§ 24. It will be seen from these experiments (Nos. 78 and 79) that the temperature of the water in the interior of the metal box, of which the glass surface forms the lid, is not necessarily uniform. If the flow is very slow a narrow current is established through the box and a deposit forms along it, the other part being left bare (Experiment 81). The facility of reading is much improved by a circulation of the air

(Experiment 80). Certain of the experiments show that a dew deposit is formed at a temperature above the theoretical dew-point. Thus in Experiment 82 the deposit is obtained at $1^{\circ}22$, the theoretical dew-point being $0^{\circ}80$; and in Experiment 83 a deposit was obtained at $16^{\circ}22$, the theoretical dew-point being $15^{\circ}02$. These readings were obtained many times successively, so that they cannot be assigned to want of uniformity in the temperature. They show that a visible deposit is obtainable upon a glass surface when the air is not saturated, and the difference is larger than in the case of the REGNAULT hygrometer, amounting to a whole degree in Experiment 83.

It, therefore, seems that the result obtained from observations with DINES's hygrometer is likely to give a very easy determination of the dew-point, that is within small limits of error; but, that if it is observed with the closest attention, the result will be considerably too high, in consequence of the premature formation of a dew deposit, and it may be erroneous in consequence of the variations in temperature of the different parts of the box containing the thermometer. This latter effect may be almost entirely avoided by using for forming the deposit water which is itself of a uniform temperature and very slightly below the dew-point, so that a large quantity may be required to flow for a small change of temperature, or by the method suggested by Professor CHRYSTAL, and in use by some of the Scottish observers, as by Mr. H. N. DICKSON (see 'Edinburgh, Roy. Soc. Proc.,' vol. 13, pp. 199 and 951). But the instrument can hardly be regarded as a standard one.

The deposit upon the glass surface above the dew-point is, I think, well established; it was suspected by REGNAULT (see p. 121), and appears to be less obvious in the case of silver than of glass. The difference may, however, be due to the greater difficulty of observing it.

3.—ALLUARD'S INSTRUMENT.

§ 25. A description of this modification of REGNAULT's instrument is given in Note A (p. 141). The difference between the two lies entirely in the facility of reading. ALLUARD encloses his thermometer in a brass box with gilt and polished sides, and surrounds one side with a brass plate so cut that the side of the box is surrounded by a surface of brass in the same plane with it. It is easier and less uncertain in reading than REGNAULT's instrument. The best way of illuminating it is similar to that suggested for DINES's hygrometer, namely, to set up a small patch of black paper in the window and arrange the reflecting surface of the hygrometer so that the eye sees the reflection of the black paper. A very faint deposit of dew is at once visible. I have not been able to arrange the instrument so that I could observe its behaviour in air of a known state. The instrument is too bulky for me to do that without apparatus on a much larger scale than mine. I cannot, however, think that such a course is necessary. The observations cannot differ from those with REGNAULT's instrument, except in regard to the facility of seeing deposits. For that reason it is

more suitable for use as a standard instrument, as the feeling of uncertainty, that is very strong while working with REGNAULT'S, is much less with ALLUARD'S.*

4.—BOGEN'S INSTRUMENT.

§ 26. I need spend but few words over this instrument. It is a dew-point instrument in which the cooling is produced by solution of ammonium nitrate contained in a silvered glass vessel. The water is squeezed into the glass vessel by means of a flexible india-rubber ball, and the temperature of deposition is read in the usual way. It affords a fairly good lecture experiment, but continuous observations are quite impracticable, as the apparatus has to be washed out as soon as one observation is taken, and the delay and trouble are too great. There is, besides, no possibility of graduating the fall of temperature. I have made a few observations with it, but it is not worth while to recall them.

* On turning back to some results that I obtained in 1883, I find considerable differences in the readings of different dew-point instruments. I was at that time comparing all the different instruments that I had, viz., ALLUARD, REGNAULT, DINES, hair-hygrometer, and a large number of wet and dry bulbs, whose indications were reduced by two sets of tables, viz., JELINEK'S and GLAISHER'S. They were honest attempts to obtain a comparison of the indications of different instruments, but the results were such that they showed that a very systematic investigation was required before one could hope to obtain concordant observations. I quote a table of results here. I made the assumption that by driving the air of the room over all the instruments by means of a rotary fan I should obtain all the instruments exposed to air in the same state. I fear that could not have been correct. I used the chemical method as a check, but I have since that time seen means of improvement, and I only offer the table as a specimen of the difficulties of a hygrometric observer.

TABLE of Vapour Pressure in Room, as deduced from Observations with various Instruments, 1883.

Date.	Chemical method.	ALLUARD.	REGNAULT.	DINES.	Hair.	Wet and dry by GLAISHER.	Wet and dry by JELINEK.
	mm.	mm.	mm.	mm.	mm.	mm.	mm.
April 10 . . .	4·81	4·91	7·34	5·81	..	6·24	5·48
„ 11 . . .	4·87	5·49	5·85	..	5·25	7·39 (?)	7·04 (?)
„ 12 . . .	5·4	5·69	5·53	6·9	5·35	6·98	6·49
„ 13 . . .	5·74	..	5·66	7·54	5·44	6·93	6·38
May 2 . . .	5·1	5·10	5·40	5·82	5·14	6·57	5·96
„ 3 . . .	6·5	5·53	6·01	6·24	5·68	6·45	5·81
„ 5 . . .	6·1	5·89	6·23	6·45	5·31	6·40	5·85
„ 8 . . .	7·65	7·57	7·88	8·35	7·27	8·23	8·14

I had previously attempted to compare all the dew-point instruments together in the manner described in my preliminary report of May 10, 1881, but after a few observations I came to the conclusion that any such attempt was worthless unless a circulation of air was provided, observations of different hygrometers in a closed vessel not giving comparable results.

NOTE A.

SUMMARY OF THE RESULTS OF WORK ON HYGROMETRIC INSTRUMENTS AND METHODS
SINCE THE TIME OF PROFESSOR J. F. DANIELL.

The subject of Hygrometry was considered in an essay by DANIELL, a third edition of which was published in 1845, after the author's death; it discusses the method of hygrometric observation founded on the deposition of dew upon an artificially cooled surface, and describes the method of using and the advantages of the dew-point hygrometer invented by DANIELL* in 1820. The following paragraphs may be quoted as bearing on the subject which I have now in hand:—

“It was also an important object to ascertain whether any hygrometric property of the glass, or difference between it and the metal in attraction of moisture, would have any appreciable effect upon the condensing power.

“Long experience has, however, convinced me that the metallic hygrometer possesses no real superiority over the glass one. The visibility of the deposition in the latter is rendered perfect by making the condensing ball of black glass, and viewing it by reflected light in the manner of a mirror; and I never could perceive any difference in the sensibility of the two instruments.”

The essay also discusses tables of pressure of aqueous vapour, &c., which have been entirely modified by subsequent and more accurate experimental data. Hygrometers founded upon the variation of organic substances are considered, and evidence to show the superiority of the dew-point methods is adduced. The chief reasons against organic hygrometers, as DE SAUSSURE'S and DE LUC'S, are quoted from DE SAUSSURE'S essay. Setting aside the difficulty of preparation of the hairs, it would appear that DE SAUSSURE did not expect two hair hygrometers to give identical readings, even when similarly prepared and mounted, and the following instance of a case in which identical readings would not be obtained was taken by DANIELL from DE SAUSSURE. If two otherwise identical hygrometers are both exposed for a long time to very dry air, say at 40° of the hair hygrometer scale, and then one of them is exposed to a humidity of 30° while the other is in an atmosphere of 50°, when the two are replaced in the same atmosphere of 40° humidity, neither will return to the 40° indication; one will read about 37° or 38°, and the other 42° or 43°.

DANIELL had used DE LUC'S hygrometer, and considered it as unsatisfactory as DE SAUSSURE'S.

He discusses also the wet-and-dry-bulb method (which was introduced by Dr. HUTTON and modified by Sir JOHN LESLIE) in the form given to it by Dr. MASON. We need not trouble about previous modifications, as MASON'S form is now practically adopted.

* ‘Quarterly Journal of Science,’ 1820.

At this time APJOHN'S formula $f'' = f' - d/88 \times p/30$ for calculating the pressure of vapour from the wet-and-dry-bulb indications was known. DANIELL calls attention to the following disadvantages attending their use:—

(1.) The smallness of the scale of the instrument, so that the probable errors of observation bear a very high proportion to the required result, particularly about the freezing point.

(2.) The liability of the indication to be affected by causes other than the humidity of the air and its temperature. The suggestion of PELTIER, that it may be affected by the electrical state of the atmosphere, is referred to as a possible explanation of the cases of the temperature of the wet bulb being found above that of the dry “which occur in all long series of observations.” (See pp. 129 and 138.)

(3.) The uncertainty in the results computed from the observations; instances are given to show that the results deduced by APJOHN'S formula are different from those obtained from the Greenwich table of factors, derived from experimental comparison of the wet-and-dry-bulb readings with direct observations of the dew-point.

There is another discussion, in English, of the various questions relating to hygrometric measurements, occupying 22 pages of the seventh edition of the ‘Encyclopædia Britannica,’ and published in 1842, from which we extract the following information about *Balance Hygrometers*:—

“Hygrometers have frequently been formed by suspending from one arm of a balance some substance which strongly attracts moisture from the atmosphere, and nicely counterpoising it by a weight on the other arm. The changes in the humidity of the air are then meant to be indicated by the changes in the position of the beam, arising from the gain or loss of weight in the suspended body. A great variety of substances have been used for this purpose, such as sponge, caustic potash, the deliquescent salts, sulphuric acid, &c. These, like the former instruments, are all too late in their indications, though some of them might scarcely be liable to lose their sensibility were it not that they soon become useless from the accumulation of dust, soot, &c., especially if in or near a large city.”

DE SAUSSURE'S hair and DE LUC'S whalebone hygrometers are carefully described, and a comparison of their scales is given, showing very considerable divergence, and indicating very peculiar behaviour of the hair hygrometer, as though it had a maximum near the saturation point. I give half the table of comparison to make the divergence clear. This is DE LUC'S comparison, and was repudiated by DE SAUSSURE, because DE LUC saturated the hygrometer by actual contact with water.

DE LUC.	DE SAUSSURE.
°	°
55	88.8
60	91.6
65	93.8
70	95.6
75	97.2
80	98
85	100
90	100
95	99.3
100	98.3

We may perhaps disregard WILSON'S hygrometer, which is a mercury thermometer, with a rat's bladder bulb. A good deal of the article is taken up with a discussion of some of the thermodynamic properties of air, not much to the point.

We have next a discussion of dew-point methods, and a description of DANIELL'S hygrometer, and an exposition of its defects, as well as those of the various suggestions made as improvements on DANIELL'S, of which there are many.

A table of results of comparisons of DANIELL'S and various instruments of this kind is given, and the mean errors (28 experiments) from the results obtained by LEROY'S method (cooled water in an open vessel) are given as follows:—

ADIE'S	— 0.1
DANIELL'S	+ 2.9
Spherical bulb	— 4.78
Long bulb	— 6.6

The author of the article then turns his attention to the wet-and-dry-bulb method, which he says has been sadly neglected for 30 years. The following extract will show the application of a formula of reduction to very wide variations in the conditions of observation; the result is very fairly satisfactory:—

“The results of experiments determining the dew-point for a considerable number of indications of the wet and dry thermometers, and under various pressures, though principally at pretty high temperatures, are given in a Calcutta journal, ‘Gleanings in Science,’ Nos. 2 and 3, 1829, and in the ‘Edinburgh Phil. Journ.’ for October, 1883, from which we have obtained the following table. The sixth column is derived from the formula—

$$f_v = \frac{(f_t + .66372)(t - t')}{175.438 f_t} = f_{v'}$$

where t is the Fahrenheit temperature of the air, t' that of the moist bulb dew-point; and f_t , f_v , $f_{v'}$ are the forces of aqueous vapour in a state these temperatures respectively.

PHILOSOPHICAL TRANSACTIONS, VOL. 179 (1888), A.

On page 114, line 5 from bottom, *for* "1883," *read* "1833."

Barometer.	Temperature of the air.	Temperature of moist bulb.	Difference or depression.	Observed dew-point.	Computed dew-point.	Difference.	Remarks.
29.75	67.2	52.0	15.2	35.7	35.7	0	Dr. ANDERSON'S experiments
30.025	56.4	49.5	6.9	39.5	40.7	+1.2	
29.35	65.0	51.5	13.5	35.45	36.6	+1.1	Observations made in India, at the level of the sea, by means of LESLIE'S and DANIELL'S hygrometers
29.787	82.0	76.8	5.2	74.0	75.0	+1.0	
29.83	81.0	72.1	8.9	68.0	68.6	+0.6	
29.78	81.5	70.9	10.6	66.5	66.5	0	
29.8	74.75	67.3	7.45	63.0	63.6	+0.6	
28.739	91.5	69.2	21.3	60.5	60.1	-0.4	
28.739	91.5	70.7	20.8	62.5	62.7	+0.2	
28.807	87.5	71.48	16.02	64.0	65.4	+1.4	
24.342	70.25	60.0	10.25	54.0	53.6	-0.4	
22.945	61.75	54.37	7.38	48.0	47.98	-0.02	
22.921	63.0	53.46	9.54	46.0	45.3	-0.7	Ditto on hills in the south of India
22.917	61.75	46.09	15.66	26.5	25.0	-1.5	
22.909	57.75	47.75	10.0	36.0	35.7	-0.3	

“The dew-points in the sixth column do not differ very materially from observation; but the temperatures from which they were computed had first to be corrected for the barometric pressure being different from 30 inches. The precise rule for estimating such a correction is as yet unknown; but it appears that, for the same temperature of the moist bulb, the difference between it and the dry thermometer, when the pressure amounts to 30 inches, is to their difference under any other pressure, B , nearly in the inverse ratio of 57 to $27 + B$. On this supposition, $t - t'$, the observed depression in the fourth column, before being used in the formula, has been multiplied by $(27 + B)/57$, and the difference between the product and $t - t'$ has likewise been applied, with its sign changed, as a correction to t , the temperature of the air.”

Passing by a discussion of the theory of the instrument, which may require modification in the light of subsequent progress in the theory of heat, and a graphic method of reducing observations proposed by Mr. MEIKLE, the other points of interest in the article are a brief account of DE LA RIVE'S suggestion (since developed by Mr. WILDMAN WHITEHOUSE) to apply the rise of temperature produced in a thermometer moistened with sulphuric acid, to determine the hygrometric state of the air.

“While the sulphuric-acid hygrometer displays considerable ingenuity, the other instrument, the wet-bulb, is on several accounts so decidedly preferable that the invention of M. DE LA RIVE is not likely ever to come into general use. Water can be more readily obtained everywhere, and is much more safe and portable than sulphuric acid. Besides, owing to sulphuric acid freezing at an uncertain or variable temperature, depending on its strength, such an instrument would be apt to give doubtful results at low temperatures. For, whatever be the strength of the acid at first, it will continue to decrease in an uncertain manner on the bulb by gradually absorbing moisture. However, the heat derived from the condensation of the vapour

will sometimes be sufficient to keep the acid in a liquid state at a temperature which would freeze it in a close vessel; and whenever it happens that sulphuric acid remains liquid on the bulb of one thermometer, while water is frozen on that of another, a comparison of the two instruments might throw some light on the influence of frost on the temperature of the latter. We presume, therefore, that the most important use likely to be derived from this hygrometer of M. DE LA RIVE would be to assist in perfecting the theory of the moist-bulb hygrometer; and possibly some other absorbent substances might answer even better for this purpose than sulphuric acid does."

The British Association Report of 1832 includes a report on meteorology by Professor J. D. FORBES, the chief points of which are recapitulated in the Encyclopædia article already cited; it is, however, mentioned that DE SAUSSURE'S "views of hygrometry were, in some respects, so very imperfect that he was not aware of the fact that the coolness produced by the evaporation of water from porous bodies was independent of the rate at which the moisture was carried off by currents of air—a want of knowledge which gave him much trouble."* There is in addition a passing reference to the labours of GAY-LUSSAC and MELLONI upon the scale of DE SAUSSURE'S hygrometer. These, I think, it is needless to discuss, as the subject has since been taken up by REGNAULT.

A supplementary report of the same authority appears in the British Association Report of 1840, in which the advantages of the wet-and-dry-bulb method, as compared with the dew-point method, are pointed out. A good deal of attention had been paid to the former method about that time, and from the results of the work FORBES considered that "we may now consider the moist bulb problem as practically solved." The solution appears to be as follows:—

Theoretical considerations lead to the assumption of a formula:

$$e'' = e' - m(t - t') \frac{b - e'}{B}.$$

Where e'' represents the actual pressure of vapour in the air.

e' the saturation pressure at the temperature t' .

t and t' the readings of the dry and wet bulbs respectively.

b the height of the barometer.

B a standard barometric height.

m is a constant to be determined, either by calculation or by auxiliary hygrometric determinations.

"This formula, employed by AUGUST and BOHNENBERGER, coincides essentially with that of IVORY ('Phil. Mag.', vol. 60, 1822, p. 81.), who first gave a proper theory of

* 'Brit. Assoc. Report,' 1832, p. 239.

the moistened-bulb hygrometer. His value for m is not far from the truth, being $\frac{1}{40}$ for Cent. degrees or $\frac{1}{72}$ for FAHRENHEIT.”*

APJOHN tested the value of m in a variety of circumstances by his own experiments and by those of others. Assuming DE LA ROCHE and BÉRARD’S value of the specific heat of air, he finds m for English inches and for FAHRENHEIT’S degree to be $\frac{1}{87} = \cdot 01149$; *a posteriori* he has determined it—

- (1.) From experiments on the dew-point $\cdot 01151$
- (2.) „ „ refrigeration in dry air $\cdot 01150$
- (3.) „ „ „ air once saturated, then warmed $\cdot 01140$

BOHNENBERGER’S value of the constant m reduced to inches and FAHRENHEIT degrees is $m = \cdot 0114$, which is practically identical with APJOHN’S values.

KÄMTZ† employs a formula which differs only slightly from that given above, and from observations on the Faulhorn obtains a value $\cdot 0118$ for m when reduced to the same units.

KUPFFER‡ gives the value $m = \cdot 01135$.

PRINSEP§ has furnished us with a large number of valuable test experiments in a warm climate. They are already referred to in the table on p. 115. The report continues; “When we find that Mr. PRINSEP once more coincides with Dr. APJOHN’S numbers, only hesitating whether to prefer $\frac{1}{84}$ to $\frac{1}{87}$ for the value of m , we are prepared to admit that this problem is, *practically* speaking, *completely* resolved, and this being the case, it is scarcely worth while to disentangle the various imperfect steps by which so happy a consummation has been attained, and the hygrometer rendered as commodious and as accurate as the common thermometer.”

These citations and references will perhaps be sufficient to show what were accepted in England as the well-established facts in hygrometry before REGNAULT introduced a degree of accuracy into thermal experimental measurement that necessitated a revision of the experimental work done in all branches of science connected therewith. It would, however, appear that there was some valuable work done on the Continent, which, if known in England, had not found its way into the summaries of the subject that I have been able to consult.

There is a very good account of the state of hygrometrical science in the second volume of the ‘Corso Elementare di Fisica Sperimentale,’ by GIUSEPPE BELLI, published in 1831. The various rough methods (including DE LA RIVE’S sulphuric acid arrangement) are described, and their weak points are thoughtfully laid bare. DANIELL’S hygrometer is examined, and the reasons which may make its indications

* ‘Brit. Assoc. Report,’ 1840, p. 98 (note).
 † ‘Poggendorff’s Annalen,’ vol. 30, 1836, p. 43.
 ‡ ‘Bulletin de l’Académie des Sciences de St. Pétersbourg,’ vol. 6, No. 22.
 § ‘Journal of the Asiatic Society of Bengal,’ 1836.

untrustworthy are set forth. DÖBEREINER'S modification* is cited as one of many given in GEHLER'S dictionary. The author then goes on to describe a method of using a dew-depositing arrangement which consists in reading the *temperature of the boundary* of a dew deposit upon a surface whose temperature varies continuously, instead of reading the temperature of a surface assumed to be at a uniform temperature at the *instant* at which a deposit occurs.

It is unnecessary to give a lengthy description; it will be sufficient to say that the lower end of a vertical column of mercury in an iron cylindrical tube is maintained by ice or by a freezing mixture at a very low temperature; after some time the temperature of the column becomes steady, gradually increasing in the vertical direction. Dew is deposited up to a certain height, and the temperature at that height is read by a thermometer with a small bulb sunk in the mercury to the required level. The height of the thermometer bulb can be adjusted, and its position defined by two index points which are on the same level as the bulb. The precautions necessary for accuracy are described in due course. This instrument was used by BELLI as a standard hygrometer.

After this discussion he passes on to his evaporation hygrometer, and the wet-and-dry-bulb. His experiments showed (1) a depression of the wet-bulb reading of about $0^{\circ} \cdot 7$ R. as corresponding to a relative motion of the air of three metres per second; (2) in still air wet-bulb thermometers with large bulbs take a longer time to arrive at their final temperature than those with small bulbs, and the final temperature is higher in the former than in the latter case. I was interested to notice that he included in his comparison observations a brass ball 80 mm. in diameter, containing the bulb of a thermometer, as I had made a similar experiment with the same object before I knew of BELLI'S work. I quote his results. He concludes from them that, provided there is a sufficiently good air current, the reading is independent of the size of the bulb.

TABLE I.—Air still.

	Temperature of Room.	A. 8 mm. diam.	B. Cylindrical. 44 mm. and 7 mm.	C. 16 mm. diam.	D. Brass ball.
h. m.	°	°	°	°	°
3 45	10·9 R.	7·75	8·0	8·20	9·2
3 55	10·9	7·75	8·0	8·15	8·9
4 5	10·75	7·75	8·0	8·1	8·6
4 15	10·7	7·75	8·0	8·05	8·6
4 25	10·67	7·75	8·0	8·05	8·6

* 'Gilbert's Annalen,' vol. 70, 1822, p. 136.

TABLE II.—Air agitated by a Cardboard Fan.

Time.	Temperature of Room.	A.	B.	C.	D.
Air still	10·67 R.	7·75	8·0	8·05	8·6
After 4 m. agitation	10·85	7·5	7·5	7·7	7·9
„ 6 m. more . . .	10·9	7·48	7·5	7·5	7·65
„ some more . . .	11·0	7·48	7·6	7·6	7·65
Freshly bathed, and the air then agitated for 6 m.					
h. m.					
4 55	11·05	7·6	7·7	7·7	8·2
5 0	11·1	7·5	7·7	..	7·9
5 5	..	7·55	7·65	7·7	7·7
5 10	11·1	7·6	7·7	7·7	7·8
Left standing without agitation of air for 10 m.					
..	10·9	8·0	8·1	8·1	8·5

Some experiments to determine the effect of radiation upon a wet bulb are then detailed. A wet-bulb thermometer was observed alternately in the sun's rays and in shadow, in two positions in the same room a short distance apart. The air surrounding the bulb was kept in motion for each of the positions; the bulb in the sun gave a mean temperature of $20^{\circ} \cdot 66$ C., and in the shade $20^{\circ} \cdot 14$ C., the temperature of the air being $25^{\circ} \cdot 25$ C. From this observation a calculation is made that the effect of radiation upon a wet bulb under ordinary circumstances would not exceed $0^{\circ} \cdot 2$ C.

BELLI therefore considered that a satisfactory observation with a wet bulb that could be used as a basis of calculation could be obtained, provided that the bulb is small, not greater than 7 mm. in diameter, and that the air in its neighbourhood is kept in a state of brisk agitation.

These conditions being satisfied, sets of tables might be formed, each table corresponding to a particular pressure, and showing, for that pressure, the pressure of aqueous vapour for a given temperature of the air, and a given difference between the wet and dry bulb.

BELLI then proceeds to calculate a formula for the psychrometer, which comes out for the pressure 760 mm. :—

$$e = E' - \cdot 592(T - T').$$

This agrees remarkably well with AUGUST'S formula, $e = E' - 0\cdot 0007832(T - T')P$, for the same value of the air pressure.*

Some comparisons of results obtained by this formula are given. They are not very consistent as they stand, but the tables used in their reduction have been considerably

* See below, p. 124.

modified since that time, so that the observations require to be re-calculated before any opinion can be formed about them.

An apparently complete enumeration of hygrometrical methods is given in the article "Hygrometer" in GEHLER'S 'Wörterbuch' (1829), already alluded to. Upwards of 50 different instruments and modifications are described. A section is devoted to the theory of the different methods, which it is needless to reproduce. The author prefers the psychrometer to DANIELL'S hygrometer, and is of opinion that it is impossible to construct a truly scientific instrument founded on the alteration of dimensions of animal or vegetable substances.

I pass on now to the consideration of the work bearing upon hygrometric measurements by REGNAULT. That which was directly intended for the elucidation of the subject is contained in two papers published in the 'Annales de Chimie,' and entitled "Études sur l'hygrométrie."*

The first part of the first memoir is devoted to the discussion of the fundamental data of all hygrometric calculation, viz. :—

- (1.) The saturation pressure of aqueous vapour in the air at different atmospheric temperatures.
- (2.) The specific gravity of aqueous vapour in saturated air referred to dry air, at the same temperature and pressure.
- (3.) The specific gravity of aqueous vapour referred to air, at the same temperature and pressure, when the fraction of saturation is less than unity.

The discussion of the first question amounts to an investigation of the truth of DALTON'S hypothesis with regard to saturated air, namely, that the saturation pressure of water vapour in air is the same as it would be at the same temperature *in vacuo*; so that in any hygrometric calculation the saturation pressure of vapour may be taken from the table of pressures of water vapour *in vacuo* for different temperatures.

For the experiments we are now referring to, REGNAULT used a slightly modified arrangement of his apparatus for determining the vacuum pressures, and obtained the saturation pressure in air and nitrogen gas. The pressure in air was observed for 34 temperatures lying between the limits 0° C. and 38° C. The results obtained show a pressure in air less than that given by the table for vacuum, by amounts varying between .10 mm. and .74 mm., the mean of the differences between the two tables of observations being .44 mm.

The results obtained with nitrogen gas are very similar, the mean differences for the first of two sets of observations being .56 mm. The differences, though very irregular in amount, are considerable, and are always in the same direction, and might, therefore, be held to show that DALTON'S law is only approximately true. REGNAULT, however, suggests that they may be due to some constant error which he could not discover, and later, in the same memoir, and in a subsequent paper on the pressures of ether and other vapours ('Mémoires de l'Institut,' vol. 26), he adduces reasons in

* 'Annales de Chimie,' vol. 15, 1845, p. 129, and vol. 37, 1853, p. 257.

favour of that suggestion ; attributing, in fact, the diminished pressure of vapour to the molecular action of the glass side of the vessel upon the saturated vapour contained in the air, producing a condensation upon the glass, the slowness of diffusion preventing the pressure reaching its maximum value by consequent evaporation.*

The second and third questions may likewise be disposed of theoretically, or, perhaps I should say, hypothetically, provided aqueous vapour may be regarded as behaving like a perfect gas when its pressure and temperature are made to vary.

In that case the specific gravity of steam referred to hydrogen may be determined from its molecular weight, and the specific gravity referred to air may then be calculated from the known specific gravity of hydrogen ; this theoretical value would of course be *constant for all temperatures and pressures*, and equal to 0·622. Several series of experiments to ascertain if such were the case are detailed in the paper we are now considering. The first two series of experiments were made on water vapour *in vacuo*, and showed that the number quoted, '622, was, within the limits of error of experiment, the true value of the specific gravity, provided the fraction of saturation of the vapour experimented on did not exceed 0·8, but the specific gravity is sensibly greater when the state of saturation is more nearly approached. The third series of experiments was made by applying the chemical hygrometric method to air artificially saturated with moisture at a known temperature. A volume of this air was made to pass through two drying tubes of sulphuric acid and pumice,† by means of an aspirator whose capacity was accurately determined ; the gain in weight of the drying tubes gave the quantity of water vapour contained in unit volume, and this could also be calculated from the known pressure of saturation at the temperature of the saturating vessel, assuming the theoretical value '622 for the specific gravity of the vapour. A comparison of the results obtained serves to show whether this assumption of '622 as the value of the specific gravity is justifiable or not.

The series comprises 68 experiments on air saturated at temperatures varying between 0° C. and 27° C. The following table shows the mean percentage difference between the observed and calculated values of the mass of moisture per unit of volume at the different temperatures :—

* This method of accounting for the discrepancies between hypothesis and experiment in reference to DALTON'S law has been since confirmed by an experiment of HERWIG (POGGENDORFF'S 'Annalen,' vol. 137, 1869, p. 592) upon compression of vapours. He found that the pressure of the vapour could be increased beyond the point at which a deposit was first formed on the sides of the vessel, and that the vacuum saturation pressure was the *increased* pressure, and not the pressure at which the deposit is first formed. (See also p. 96.)

† A number of preliminary experiments were made to verify that the sulphuric acid was efficient as an absorbent.

Temperature of saturated air.	Number of experiments.	Percentage difference.
0·0	9	·44
14·0	9	·77
7·0	7	·86
20·5	21	·45
24·5	22	·90

The differences thus tabulated lie all in the same direction, the observed weight of moisture being in each case too small. They may be accounted for in four different ways :—(1) the pressure of aqueous vapour in the saturated air may be slightly less than that given by the table of pressures *in vacuo*; (2) the density of vapour in saturated air may be less by about 1 per cent. than the theoretical density; (3) the temperature readings of the saturated space may be slightly inaccurate, errors in the temperature readings of from 0°·06 to 0°·15 C. being sufficient to account for the differences; (4) the differences may be due to the incidental errors of the experiments. The irregularity in the numbers for different temperatures tends rather to show that either of the first two suggestions is insufficient completely to account for the observations, and there is evidence that suggestion (2) is directly contradicted by experiment.* The amount of thermometric error required in each case to account for the differences is very small, and REGNAULT gives no details as to the method of correcting the thermometers he employed in the experiments, but his known familiarity with thermometers of every kind and their errors makes it highly improbable that he could have overlooked these in the case before us. The precautions adopted in order to secure the observations against the possible sources of error that are suggested by the arrangement of the apparatus are not definitely stated.

A table of results of some experiments to determine by the same method the specific gravity of saturated steam at temperatures above that of the air of the room is also given, but their author does not regard the result as satisfactory.†

The second part of the memoir considers the various methods employed to determine the fraction of saturation of the air. The chemical method is passed over, as being fully discussed in the previous part of the memoir.

With regard to the behaviour of DE SAUSSURE'S hair hygrometer, REGNAULT set

* For another suggestion as to the cause of the difference based upon my own observations, see § 14 (*ante*).

† REGNAULT sums up this part of his work in the following words :—“ Quoi qu'il en soit, on voit qu'en prenant pour base ma table des forces élastiques de la vapeur aqueuse dans le vide, et admettant que la densité de la vapeur est constamment égale à 0·622, celle de l'air dans les mêmes circonstances étant 1, le poids de la vapeur d'eau calculé ne peut différer de la quantité réelle que d'une fraction très petite, un centième environ.”

himself to determine whether the indications of hair hygrometers are strictly comparable under the following different conditions :—

1. When they are constructed of the same kind of hair, prepared at the same operation,
2. When they are constructed of different kinds of hair, but prepared at the same operation.
3. When they are constructed of different kinds of hair, and prepared at different operations by different processes,

A considerable number of hygrometers were compared with each other, their "fixed points" being determined by means of saturated air, and air dried by sulphuric acid. The results are summed up by REGNAULT as follows :—

"Two hygrometers, mounted with similar hair, may be non-comparable solely on account of their not being stretched by equal weights."

"Hygrometers constructed with hair of the same kind, prepared at the same operation, do not give identical results, but they do not differ to such an extent that, for the majority of observations, they may not be regarded as comparable." The maximum difference between different instruments was about three hygrometric degrees.

"Hygrometers constructed with hairs of different kinds and prepared in different ways may show very great differences in their indications, even when they agree at the fixed points."

It is consequently impracticable to construct a single table for the calculation of results which is applicable to all instruments, and it is desirable that observers should have at their disposal a simple process which enables them to make for themselves a table for their own instrument, and by which they can verify the graduation of the instrument as often as they wish.

With this object REGNAULT proposes to enclose the hygrometer in a cylindrical vessel at the bottom of which can be placed successive layers of mixtures of sulphuric acid and water of different known strengths; he gives formulæ and tables for the pressure of aqueous vapour due to such solutions at different temperatures. The process of forming a scale for the hygrometer is described. REGNAULT proposes to abandon the attempt to determine the point of the scale corresponding to absolute dryness, and indeed to use instead the fractional humidity of about .20, as given by a suitable solution of sulphuric acid. The effect of changes of temperature upon hair hygrometers was not determined, in consequence of the breaking of the apparatus intended for that purpose. (See p. 139.)

REGNAULT gives as his final opinion about hair hygrometers, in his second memoir : "Il est à désirer que les observateurs renoncent définitivement à un appareil sur le bon état duquel ils ne peuvent jamais compter."*

* 'Annales de Chimie,' vol. 37, 1853, p. 258. (For later information about the hair hygrometer, see p. 139.)

After laying down the following objections to DANIELL'S hygrometer, viz. :—

1. The temperature indicated by the thermometer may be different from that of the surface upon which the deposit is formed.
2. The observer is too close to the instrument.
3. The evaporation of the ether on the bulb may alter the temperature and hygrometric state of the air.
4. The ether generally contains some water, which evaporates and changes the hygrometric state of the neighbouring air still more.
5. In dry weather it is generally difficult, and sometimes impossible, to get a deposit of dew at all.

REGNAULT describes his own instrument, proposing to produce the evaporation of the ether in the capsule by causing air to bubble through it by means of an aspirator, which enables one to adjust the temperature with very great nicety. No comparisons of results with those of the chemical method are given.

The memoir then describes a large number of experiments upon the psychrometer, the object of which was to determine to what extent AUGUST'S formula—

$$f = f' - \frac{0.429(t - t')}{610 - t'} h$$

is applicable to psychrometer observations.

It is first made clear by direct experiment that when perfectly dry air is made to pass over a dry bulb and wet bulb successively the temperature of the latter depends upon the rate at which the air passes; from this it is inferred that a similar result will be found with air more or less moist, and this is verified by direct experiment. Several sets of experiments, however, including a series upon two wet bulbs exposed in an open court 2 metres from the wall and 7 metres above the ground, one of them being supplied with a current of air of known velocity by means of a ventilating fan, and the other left to itself, lead to the following conclusions :—Psychrometer observations in moving air are practically independent of the size of the bulb. The agitation of the air certainly influences the formula for the psychrometer, but when the instrument is exposed to free air the same formula can be adopted, so long as the velocity of the wind does not exceed 5 or 6 metres per second.

REGNAULT suggests the advisability of determining only the form of a reduction formula theoretically, and then assuming constants, determining their values by actual comparison with some other method.

His psychrometer observations were all reduced to absolute measure by comparison with the results of the chemical method, and in consequence of the comparison the constant of AUGUST'S formula was altered, so that it became—

$$f = f'' - \frac{0.480 (t - t')}{610 - t'} h.$$

This represents very satisfactorily the results obtained for fractions of saturation exceeding .40.

For values of t' below 0° C. the number 610 must be replaced by 689.

Observations by M. IZARN in the Pyrenees are quoted to show the applicability of the formula at high-level stations.

The second memoir ('Annales de Chimie,' vol. 37, 1853) is mainly devoted to the discussion of the formula for reduction of psychrometer observations.

AUGUST'S theoretical formula, recast and calculated with the new values of the constants which REGNAULT was then able to supply (regarding the latent heat of vaporisation of water at temperatures likely to occur in meteorological observations as being constant and equal to 600), becomes—

$$x = f'' - 0.000635 (t - t') H;$$

but, instead of using this formula for calculating his results, REGNAULT assumes the form

$$x = f'' - A (t - t') H,$$

and determines the value of the constant A for psychrometers exposed in such situations as are likely to give rise to various conditions, by obtaining simultaneous readings of the humidity by means of the chemical method.

The mean results of his various series of experiments are given in the following table:—

Number of the series.	Number of experiments in the series.	Situation of the psychrometer.	Barometric range during the series, in mm.	Range of dry-bulb temperature during the series.	Range of fractional humidity.	Most suitable value of A.	Extreme percentage error.		Mean percentage error.
							Positive.	Negative.	
1	10	Closed room, 190 cubic metres.	755 to 760	21·4 to 23·4	·480 to ·588	0·00128	+2·3	-3·3	1·7
2	8	Closed room, 1,000 cubic metres.	735 ,, 765	8·06 ,, 15·25	·589 ,, ·758	0·00100	4·3	1·6	1·5
3	7	Same room as No. 2, but with two opposite windows open	751 ,, 756	13·01 ,, 17·49	·452 ,, ·672	0·00077	1·1	0·9	·4
4	41	The large open court of the Collège de France, 1,000 square metres in area [Protected from the sun; 7 metres above ground, and 2 metres from the the wall.]	748 ,, 764	7·16 ,, 29·78	·197 ,, ·745	0·00074	6·1	6·6	2·7
5	15	The long court of the Collège de France, planted with trees.	748 ,, 772	0·85 ,, 9·84	·624 ,, ·984	0·00100	10·5	2·0	2·8
6	9	Same locality, but exposed to full sunshine.	759 ,, 762	29·06 ,, 30·46	·228 ,, ·328	0·00090	10·7	10·0	
7	16	Same locality . .	739 ,, 774	-7·63 ,, -0·13	·611 ,, ·879	0·00075*	11·8	2·8	
8†	16	The large court of the Taverne inn, Eaux Bonnes, Pyrenees.	694 ,, 703	10·43 ,, 26·15	·350 ,, ·780	0·00090	2·3	2·6	
	18	Pyrenees. . . . [Sometimes exposed to sun.]	644 ,, 649	9·52 ,, 23·67	·417 ,, ·980	0·00090	9·5	8·6	

The conclusion which REGNAULT draws from these series of observations is that the psychrometer may be satisfactorily employed, and give the fractions of saturation to within one-fortieth, provided that it be regarded as an empirical instrument. The instrument should be protected from the direct action of the wind, and its indications should be interpreted by means of a formula similar in form to the one given, the constant A being determined for each locality where observations are to be taken, and, if necessary, a different value should be taken for different parts of the scale.

A series of experiments to compare the indications of the psychrometer in a closed room with the results of the chemical or BRUNNER'S hygrometric method was made by VOGEL ('Abhandlungen der Münchener Akademie,' vol. 8, 1860, p. 295). The wet

* For degrees of saturation above ·7 the number quoted is apparently too small for ice-covered bulbs. The number A = 0·0013 would have been more suitable for such observations.

† Experiments of M. IZARN.

bulb was read as it stood at rest, and also after it had been swinging pendulum fashion at the end of a string. The mean of differences observed between the stationary and vibrating readings was $0^{\circ}\cdot59$. It is difficult to follow the results of the 75 experiments included in the table, in consequence of the unusual units employed. The results are, however, more widely divergent than REGNAULT'S. REGNAULT'S constant A is calculated for each observation, and its value varies between $\cdot00217$ and $\cdot00094$.

The errors from the true percentage humidity, as given by the chemical method, lie between $+2$ and $+13$ for the swinging instrument, their mean value being $7\cdot4$.

The subjoined table for computing the dew-point from psychrometer observations is extracted from a letter from Lieutenant NOBLE, of Toronto, published in the 'Proceedings of the Royal Society,' vol. 7, 1855, p. 528. It was obtained by means of simultaneous observations with REGNAULT'S hygrometer. The factor, f , of the second column, corresponding to the Greenwich factor,* is that by which the difference of temperatures of the wet and dry bulb must be multiplied in order to give the difference between the air temperature and the dew-point, the formula being $T = t - f(t - t')$.

TABLE of Factors by NOBLE and CAMPBELL.

Temperature of air (t).	Factor (f).	Number of observations.	Probable error of a single datum.
$^{\circ}$ F.			
48 to 51	2·31	21	·30
46 ,, 47	2·38	13	·26
42 ,, 45	2·53	41	·40
40 ,, 41	2·63	17	·41
38 ,, 39	2·83	25	·48
34 ,, 37	3·02	64	·43
32 ,, 33	3·33	25	·63
30 ,, 31	3·81	22	·61
28 ,, 29	4·40	27	·66
24 ,, 27	5·46	43	·82
22 ,, 23	6·06	15	1·20
20 ,, 21	6·93	6	1·40
18 ,, 19	7·13	21	1·44
16 ,, 17	7·60	20	1·76
14 ,, 15	8·97	17	1·72
12 ,, 13	10·30	20	2·53
10 ,, 11	11·50	11	2·19
8 ,, 9	13·06	8	4·63
6 ,, 7	15·20	7	3·66
0 ,, 5	16·23	14	1·87
- 1 ,, - 4	19·37	10	4·11
- 5 ,, - 10	21·64	6	4·65
- 11 ,, - 16	37·83	6	10·96

This method of reducing results is quite different from those already mentioned, and is therefore only comparable with them by taking some actual observations and

* See Note B., p. 147.

applying each method. Nothing is said in the letter as to the manner of exposure of the wet bulb, except, parenthetically, that the thermometers were protected from the full force of the wind. It would appear, therefore, that the table should correspond to REGNAULT'S value of $A = \cdot 00074$.

NOBLE points out that the factors for temperatures below the freezing point do not coincide with those deduced from the Greenwich observations. Without assigning any cause for the difference, he mentions two circumstances which may bear upon the question :—

1. If the air be a little above and has been below 32° F. there will frequently be found a small button of ice at the foot of the wet-bulb thermometer which is not easily perceived, and which will keep it at 32° F. when the temperature of evaporation is really above that point.

2. The water may be cooled below 32° F. without freezing.

The hygrometric methods have also been discussed by KÄMTZ, in a paper published in 'Kämtz, Repertorium,' vol. 2, pp. 341 to 361. I have (December 20, 1887) only lately seen the original paper; the following notes are taken from an abstract given in the 'Fortschritte der Physik' for 1861.

Comparing REGNAULT'S hygrometer in the original form with one consisting of glass test-tubes silvered inside, and with DANIELL'S, and testing the results by the chemical method, KÄMTZ prefers the test-tube form of REGNAULT, and considers that the difference of radiation and the difference in the surface may account for different temperatures at which dew is deposited.

Concerning the psychrometer, we have a discussion of a large number of observations by this method, including the Greenwich observations and some of REGNAULT'S, as well as his own.

Taking the general theoretical psychrometric formula—

$$e = \frac{1 + \alpha \tau}{1 + \beta \tau} e_1 - \frac{\alpha + \tau}{1 + \beta \tau} h$$

(where e is the required pressure of vapour, e_1 the pressure at the temperature of the wet-bulb, τ the psychrometric difference, h the barometric height, and α and β are constants), instead of adopting REGNAULT'S approximate formula—

$$e = e_1 - \frac{0.429 \tau}{610 - t_1} h,$$

KÄMTZ expands the general form in powers of τ : thus—

$$e = e_1 + \alpha \tau e_1 + \beta \tau + \gamma \tau^2 + \delta \frac{\tau d}{745},$$

where $d = 745 - h$.

The various series of observations were then separately employed to determine the constants of this formula, with the following results :—

A. REGNAULT'S observations (with a large psychrometer)—

$$e = e_1 - 0.6698 \tau + 0.015253 \tau e_1 - 0.00522 \tau^2 + 0.58299 \tau \frac{745 - h}{745}.$$

B. REGNAULT'S observations (small psychrometer)—

$$e = e_1 - 1.24442 \tau + 0.05881 \tau e_1 + 0.009006 \tau^2 + 0.58299 \tau \frac{745 - h}{745}.$$

C. Greenwich observations—

$$e = e_1 - 0.27842 \tau - 0.02309 \tau e_1 + 0.002437 \tau^2 + 0.58299 \tau \frac{745 - h}{745}.$$

D. KÄMTZ'S observations. Psychrometer and DANIELL'S hygrometer comparisons—

$$e = e_1 - 0.64569 \tau - 0.003861 \tau e_1 + 0.012397 \tau^2 + 0.73932 \tau \frac{745 - h}{745}.$$

E. KÄMTZ. Chemical method and psychrometer—

$$e = e_1 - 0.50051 \tau - 0.021641 \tau e_1 + 0.015548 \tau^2 + 0.58299 \tau \frac{745 - h}{745}.$$

F. Condensation experiments—

$$e = e_1 - 0.67894 \tau - 0.0045928 \tau e_1 + 0.010690 \tau^2 + 0.58299 \tau \frac{745 - h}{745}.$$

G. All the observations together—

$$e = e_1 - 0.57515 (t - t_1) - 0.005989 (t - t_1) e_1 \\ + 0.002664 (t - t_1)^2 + 0.58899 (t - t_1) \frac{745 - h}{745}.$$

Apart from the concordance of the barometric correction, there is nothing in these formulæ that seems to indicate any advantage in introducing the additional constants, nor does the reduction of observations, specimens of which are given, by means of them, make them any more reassuring; the differences in the pressure calculated by the different formulæ amount frequently to 20 per cent. of the whole, and KÄMTZ assigns one-sixth as the extreme error probable in using the general formulæ, G. He points out that the size of the bulb has some influence on the reading, and considers that comparable results are only to be obtained by making comparisons between the separate instruments and the condensation or the chemical method.

To another paper by KÄMTZ on the psychrometer below the freezing point, in KÄMTZ'S 'Repertorium,' vol. 3, I have not been able to obtain access,* nor yet to

* Since the above was written I have seen the paper. The chief points of it are referred to in the extracts quoted from PERNTER'S memoir (p. 137). KÄMTZ proposes to add 0°·5 C. to the observed psychrometric difference for ice-covered bulbs.

“Observations Anormales des Psychromètres” (‘Bulletin de la Société Vaudoise,’ vol. 9, p. 234) nor the “Rapport de la Commission Hygrométrique” (‘Actes de la Soc. Helvétique des Sciences Naturelles,’ 1866).

CHISTONI, at the request of the Italian Meteorological Office, undertook a comparison of the ordinary psychrometer and a ventilation psychrometer with a standard hygrometer. The ventilation psychrometer is an instrument provided with two centrifugal fans on the same axle driven by clockwork. Each fan leads to a separate vertical tube, and the bulbs of the two thermometers are placed in the mouths of these tubes, so that air passes over them at a rate which is sensibly constant, and the same at each observation. The experiments were carried out at Ostiano. REGNAULT’S hygrometer was chosen as the standard instrument with which to compare the psychrometers, for the following reasons:—(1) The chemical method was too cumbersome for use in the country; (2) BELLI’S standard hygrometer might be supposed to affect the air in its neighbourhood by its large cold mass. The air was drawn through REGNAULT’S hygrometer by means of a very ingenious aspirator formed by allowing sand to fall from a cloth funnel down a long vertical tube, provided simply with a side opening connected with the hygrometer.

The observations were made in a school-room with three windows, with two psychrometers, each used in turn as a ventilated and non-ventilated instrument. They were reduced by the ‘Tavole ad uso della Meteorologia.’*

A summary of the results is given in the following table:—

Instrument.	Size of cylindrical bulb.	No. of observations.	Range of barometer.	Range of dry bulb.	Range of humidity.	Extreme error from REGNAULT, per cent.	Absolute mean error per cent.
No. 1. Ventilated .	6 mm. × 25 mm.	46	750–766	12° to 30°	30 to 70	(Cir.) 10	(Cir.) 2·6
No. 2. Ventilated .	6 „ × 69 „	51	„	„	„	„ 7	„ 2·8
No. 1. Unventilated	6 „ × 25 „	51	„	„	„	„ 15	„ 6·6
No. 2. Unventilated	6 „ × 69 „	40	„	„	„	„ 14	„ 5·8

The constant A of REGNAULT’S formula was calculated for each of the ventilator observations; its value varies between 0·00072 and 0·00098 for No. 1, and between 0·00069 and 0·00110 for No. 2. These results are strong evidence in favour of the ventilation method.

The wet bulbs were covered with muslin and moistened before each observation, so that the results are not necessarily applicable to cases in which the wet bulb is kept continually moist by means of a wick or other capillary arrangement.

CHISTONI extended his observations to temperatures of the dry bulb below 15° C. in the winter months of 1877-8 at Pavia. The summary is as follows:—

* See below, p. 148.

Instrument.	Size of bulb (cylindrical).	No. of observations.	Range of barometer.	Range of dry bulb.	Range of humidity.	Extreme error from REGNAULT (per cent.).	Absolute mean error per cent.
No. 1. Ventilated .	6 mm. × 25 mm.	69	739-765	- 1.2 ^o to 19.6	44 to 92	(Cir.) 15	(Cir.) 3.3
No. 2. Ventilated .	6 " × 69 "	69	"	- 1.0 " 18.9	"	" 13	" 2.6
No. 1. Unventilated	6 " × 25 "	70	"	- 3.7 " 19.4	"	" 22	" 5.0
No. 2. Unventilated	6 " × 69 "	70	"	- 3.6 " 19.4	"	" 18	" 3.7

The values of the constant A of REGNAULT'S formula lay between 0.00062 and 0.00233 for No. 1, and between 0.00060 and 0.00199 for No. 2.

CHISTONI has continued his work by making a series of comparisons between the ventilated psychrometer and REGNAULT'S hygrometer readings for temperatures below 15° C. The observations published in the 'Annali della Meteorologia' for 1879 comprise 191 comparisons for temperatures mostly below 15° C., when the wet bulb stood above 0° C.; 32 comparisons when the psychrometer was in the so-called critical state, *i.e.*, when the dry bulb stood above 0° C. and the wet bulb below that temperature, and 32 comparisons when the dry and wet bulbs were both below zero. The observations were made at Collio, where the mean barometric pressure for the six months over which the observations lasted was 680 mm.

The following table is taken from CHISTONI'S paper :—

Temperature interval.	Maximum actual positive error from REGNAULT.	Maximum actual negative error from REGNAULT.	Mean absolute error.
$t < 0 \quad t' < 0$	19	29	9.8
$t > 0 \quad t' < 0$	20	51	13.1
$t > 0 \quad t' > 0$	21	29	6.4

The errors given are the actual errors in the computed humidity. I have not calculated the percentage errors; a rough idea of them may be got by taking the humidity at 50, when the percentage errors would require each number given in the columns of the table to be multiplied by 2.

These errors are very serious; they are only briefly discussed in the paper; the generalisation is made, namely, that the errors are positive and small when the humidity is great, and they are negative and larger when the humidity is small. The psychrometer observations were reduced, as before, by means of HAEGHENS'S 'Tavole ad uso della Meteorologia.'

CHISTONI has also published* a paper discussing the various psychrometrical

* 'Memorie e Notizie della Meteorologia Italiana,' 1878.

formulae and their application. He first explains that the assumptions made by AUGUST and others in order to obtain a theoretical formula cannot practically be justified, and he then deduces the formula originally given by BELLI* and supplies it with the more accurate values of the constants obtained since BELLI'S time, and reduces it by omitting negligible quantities to the form :—

$$f = f' - \frac{(0.237 B - 1.79)(t - t')}{377.2 - 0.490 t'}$$

Following REGNAULT'S suggestion that the theoretical formulae should be regarded only as general forms, he has the three following :—

REGNAULT'S form, $f = f' - A (t - t') B$ (1.)

BELLI'S form, $f = f' - \frac{(m B + n)(t - t')}{1 + p t'}$ (2.)

AUGUST'S form, $f = f' - \frac{m(t - t') B}{n - t'}$ (3.)

to these he adds as a suggestion :—

$$f = f' + m(t - t') B + n(t - t')^2 B^2$$
 . (4)

where A, m, n, p are constants to be determined by experiment.

He then applies these four formulae to the computation of the pressure of aqueous vapour from psychrometric observations, determining the constants by the method of least squares, using the results given by REGNAULT'S hygrometer as standards. He then tabulates the errors ; if we denote by $\Delta f_1 \Delta f_2 \dots$ the errors from REGNAULT of the formulae (1) (2) . . . we get the following values for the mean absolute error, *i.e.*, the mean of the errors without regard to sign :—

$$\begin{aligned} \Delta f_1 &= 0.33 \text{ mm.} \\ \Delta f_2 &= 0.32 \text{ ,,} \\ \Delta f_3 &= 0.29 \text{ ,,} \\ \Delta f_4 &= 0.25 \text{ ,,} \end{aligned}$$

He concludes that his own suggestion most nearly represents the actual case, but at the same time he remarks that the psychrometric method is, so far as he can see, not capable of giving the pressure of aqueous vapour to within 0.1 mm.

Certain practical points with reference to psychrometer observations are discussed in a paper by CANTONI.* By a series of observations upon the temperature of the air as recorded by thermometers exposed freely to the sun, or protected by various screens or metallic tubes, he shows that the best results are obtained without screens, provided that a current of air is made to pass over the bulbs, and a similar advantage results

* See PERNTNER'S paper, quoted below.

in the case of a thermometer which has its bulb covered with a thin layer of any material. From this he is led to test the effect of ventilation upon the psychrometer, and the observations given show a considerable depression of the temperature of the wet bulb in consequence of the current of air produced by the double-fan arrangement by the Tecnomasio of Milan. BELLI originally suggested the reading of wet-bulb thermometers contained in a metal tube through which air was driven, and the suggestion was practically carried out in an instrument designed by BUZZETTI, but the new arrangement of the Tecnomasio gave a much stronger current than the original apparatus of BUZZETTI, and from CANTONI'S observations it would appear that the temperature of the wet bulb might be depressed often more than a whole degree by the stronger current of air.

A further contribution to the discussion of psychrometric observations has been made by ANGOT,* whose report is printed in full in the 'Annales du Bureau Central Météorologique,' 1880. Starting from REGNAULT'S general form—

$$x = f' - A (t - t') h,$$

and writing it

$$A = \frac{f' - x}{h (t - t')},$$

he determines the value A for each of 3670 comparisons between psychrometer and dew-point observations made at Paris (height 40 metres), at the observatory of the Puy de Dôme (height 390 metres), or at the station on the summit of the Puy de Dôme (height 1470 metres). Of these, 282 observations gave the temperature of the wet bulb below 0° C. The range of the wet bulb temperatures was from -20°·5 to +23°·6 C., and the difference of the wet and dry bulb readings reached 16° C. The dew-point instrument was ALLUARD'S modification of REGNAULT'S arrangement.†

Having thus obtained a series of values for A, ANGOT found the mean of the values for temperatures of the wet bulb lying between 0° C. and 1° C., between 1° C. and 2° C., and so on for each station.

The means in every case showed that the value of A depended on the value of $t - t'$. The following is cited as an example:—

Puy de Dôme (Plain). $t - t'$	A. $t' = 6^{\circ}42.$
0·35	0·001022
1·43	0·000948
2·41	0·000821
3·40	0·000818
4·40	0·000792
6·77	0·000705

* 'Journal de Physique,' vol. 1, 1882, p. 119.

† See below, p. 141.

He therefore assumed that A was a function of $t - t'$, and, writing—

$$A = a + b(t - t'),$$

determined the values of a and b for each series by CAUCHY'S method, giving to each equation a weight equal to the number of observations from which the values A and $t - t'$ were obtained.

The 36 values of b thus obtained presented no regular variation, whether arranged with reference to $t - t'$, t' , or h .

He therefore supposed b to be constant and found it equal to -0.000028 .

The values of a increased when t' , and consequently f' , increased, and when h diminished and could be represented by the equation—

$$a = 0.000776 + 0.0159 \frac{f'}{h}.$$

The psychrometric formula therefore became—

$$x = f' [1 - 0.0159(t - t')] - 0.000776 h (t - t') [1 - 0.0361(t - t')]$$

for temperatures above 0° C., and

$$x = t' [1 - 0.0159(t - t')] - 0.000682 h (t - t') [1 - 0.0411(t - t')]$$

for temperatures below 0° C.

From these formulæ tables have been constructed; these are not given in the abstract in the 'Journal de Physique,' but a diagram which serves the purpose is appended at the end of the paper.

The results with these tables seem to be very satisfactory, as compared with those given by HÆGGHENS'S tables, the mean percentage error of 90 observations at Paris being $+1.1$ by the latter and only -0.5 by the former; while for 91 observations of CHISTONI'S the mean error by the old tables is 2.5 , and by ANGOT'S only $+0.5$; and further, applying the new tables to REGNAULT'S observations below the freezing point, the mean error is only $+0.7$, as compared with $+3.5$ obtained when REGNAULT'S formula is employed. On the whole ANGOT states that the error of the computation from a single observation above zero is about ± 2 units in the relative humidity, and the absolute mean error of a series of some 20 to 30 observations 0.5 ; for temperatures below zero it would probably be necessary to double these figures, but at any rate even for very hot and dry regions the tables never give negative values for the humidity, which is a possibility with the old tables. No particulars are given as to the mounting and moistening of the thermometers.

An account of an interesting series of observations with the psychrometer in very dry atmosphere is given by H. F. BLANFORD in the 'Journal of the Asiatic Society of Bengal,' vol. 45, 1876. The observations were taken in various stations in India,

and the dew-point was computed from them by APJOHN'S formula,* by GLAISHER'S factors, and by AUGUST'S theoretical formula† supplied with REGNAULT'S values of the constants occurring therein and adapted to English units. The thermometers were either a pair with small pea bulbs or a pair of CASSELLA'S Kew pattern, with small spherical bulbs. Particulars are given with reference to the verification of their indications.

The wet bulbs were exposed either (1) in a thermometer shed, *i.e.*, "in a frame with one or two cross bars (generally protected by wire netting at back and front) under a thatched shed open on all sides to the wind," or (2) by the "sling" method. For the Upper India series the bulbs were covered with a single thickness of old thin calico. A water bottle was placed two inches to the side of the bulb with the water level a quarter-inch below it, and communication was made by a well-washed lamp-wick of some dozen threads of coarse yarn.

The dew-point instrument was CASSELLA'S form of REGNAULT.

Assuming the direct dew-point determinations to be correct, the following are the errors shown by the several computations :—

Place.	Series.	No. of observations.	Mean air temp.	Mean wet bulb.	Dew-point below air.	Barometer.	Errors.			Conditions.
							AUG.	APJ.	GLAISHER.	
Secunderabad .	1	6	92.4	67.9	44.7	28.07	+2.5	+6.1	+5.4	In thermometer shed
" .	2	1	93.4	70.1	41.0		+3.3	+5.9	+3.4	
" .	3	6	94.8	70.6	44.0		+4.9	+8.0	+5.6	
" .	4	12	92.7	69.6	41.9	28.4	+3.9	+7.0	+4.6	" "
Bellary	5	7	94.0	69.7	40.7		-0.4	+2.8	+0.9	
"	6	2	94.7	68.9	37.0		? -7.0	? -3.3	? -4.7	
"	7	8	94.7	68.4	43.7	28.39	-2.0	+1.9	+0.9	" "
"	8	9	95.8	66.6	48.3		-4.6	+0.6	+1.6	
Coimbatore . .	9	8	96.8	68.9	48.1		+0.5	+4.6	+3.8	
"	10	1	96.4	68.5	49.6	29.35	+1.9	+6.1	+5.5	" "
"	11	10	95.3	69.9	42.9		+0.3	+3.8	+1.9	
Trichinopoly .	12	6	89.6	77.2	18.5		+0.7	+1.7	-1.6	
Madras	13	5	97.0	74.1	31.1	29.72	-0.3	+2.3	-0.5	In verandah. Sling thermometer
Calcutta . . .	14	9	84.2	79.4	13.0	29.60	-0.4	-0.1	-1.6	In sitting-room. Sling thermometer
"	15	12	83.8	79.7	6.3	29.55	+0.5	+0.8	-0.5	
Allahabad . .	16	1	77.4	61.2	29.3	29.54	+0.2	+3.0	+2.9	In verandah.
"	17	5	87.3	65.2	41.4	29.45	+1.8	+5.4	+5.5	
Agra	18	7	86.1	60.8	45.1	29.35	-6.1	-0.2	+3.7	" "
Lahore	19	8	82.9	62.3	42.9	29.08	+4.6	+8.2	+8.7	
"	20	8	73.0	55.6	35.4	29.18	-2.9	+1.4	+3.2	In thermometer shed
"	21	7	74.9	56.9	37.6	29.18	+0.8	+4.6	+6.7	
Mean	+0.46	+3.70	+3.00	

$$* x = t - \frac{t' - t}{88} \cdot \frac{h}{30}$$

$$† x = f - \frac{0.480 \times \frac{5}{9} (t - t')}{610 - \frac{5}{9} (t' - 32)} h = f - \frac{0.480 (t - t')}{1130 - t'} h \cdot (t' > 32^\circ)$$

The results show that AUGUST's formula gives the dew-point from psychrometer observations with considerable accuracy, even when the dew-point is very far below the temperature of the air. The further conclusion to be drawn from the observations is already made clear, namely, the necessity for the exposure of the wet-bulb to a sufficient current of air. The readings are too high by all the computations when the exposure was in the verandah or room.

BLANFORD does not regard his dew-point observations as unexceptionable, and calls attention, among other practical points in connexion with this observation, to the necessity for a highly burnished surface for the thimble and freedom from microscopic scratches, as these make it very difficult to seize the moment of definition when the humidity is very small and amount of deposit consequently light.

The attempts at deducing theoretically a formula for the computation of psychrometric observations, to which allusion has hitherto been made, have been based upon the assumption that there was a continuous replacement of a layer of air, no matter how thin, surrounding the wet bulb. This layer of air was supposed to be reduced to the temperature of the wet bulb, and completely saturated with moisture. The effect of the varying rapidity of motion of the air was not taken into account, and the effect of radiation was regarded as insensible. An equation is obtained by AUGUST, APJOHN, and others, between the amount of heat supplied to the surrounding air and the amount lost by the evaporating liquid. BELLI, on the other hand, equates the mass of air reduced, per unit time, from the temperature, t , of the dry to the temperature, t' , of the wet bulb, to the mass of air saturated by evaporation per unit time. This slight divergence between the methods of obtaining the equation leads to no difference in the results, because the theory evidently represents ultimately the same physical state of things. The matter is, however, treated from a different point of view in MAXWELL's article on "Diffusion" in the 'Encycl. Britann.' (9th edit.). In the discussion of the physical problem, as there treated, he considers a "steady" distribution of moisture and temperature round the wet bulb, supposed to be maintained in a perfectly still atmosphere of indefinite extent, the distribution of heat and moisture being brought about by conduction, radiation, and diffusion. MAXWELL calls this the conduction and diffusion theory in calm air, in contradistinction to the convection theory referred to above. The mathematical solution is identical in form with that of the corresponding electrical problem to find the distribution of potential, due to a charged body in a field, and consequently, with the correction for radiation, leads to the following equation:—

$$p_0 = p_1 - \frac{PS}{L\sigma} \left\{ \frac{K}{D} + \frac{AR}{4\pi C\rho SD} \right\} (\theta_0 - \theta_1),$$

where

p_0 is the vapour pressure in the air undisturbed by the presence of the wet thermometer bulb.

p_1 is the vapour pressure at the surface of the bulb.

- P the whole pressure of the air.
 S is the specific heat of the air.
 L is the latent heat of the vapour at the temperature θ .
 σ is the specific gravity of the aqueous vapour referred to air.
 K is the coefficient of thermal conductivity of the air.
 D the coefficient of the diffusion of the vapour.
 R is the coefficient of radiation per unit of surface of the bulb.
 A is the area of the bulb.
 C is identical with the electrostatic capacity of the bulb, and is therefore equal to the radius for a spherical bulb.
 ρ is the mass per unit of volume of air.
 θ_0 the temperature of the air undisturbed by the wet bulb, *i.e.*, the dry-bulb temperature.
 θ_1 is the wet-bulb temperature.

This equation, which has also been obtained by STEFAN, for a spherical bulb, differs from the convection formula, which is $p_0 = p_1 - PS(\theta_0 - \theta_1)/L\sigma$, only by the factor in the last term. The factor K/D is said, by MAXWELL, to be less than unity, and probably about .77. STEFAN, in the 'Zeitschrift der Oesterreich. Gesell. für Meteorologie,' vol. 16, 1881, assigns the value unity to K/D . He, moreover, deduces the special case of the formula for a rotational ellipsoidal bulb.

It appears from the formula that for spherical bulbs the effect of radiation is proportional to the diameter of the bulb. Further, if we take account of convection currents, their effect would practically be to increase R and D, keeping their ratio constant, so that the relative importance of the radiation term would be diminished by the effect of a current of air.

The application of this formula is further considered by PERNTNER,* who points out that the usual assumption that the effect of radiation is negligible leads to serious error for bulbs of considerable magnitude. Substituting in the formula, he finds that for still air the radiation effect is equal to the conduction and diffusion effect if the radius of the bulb (supposed spherical) is equal to 0.57 cm. With a convective current the effect of radiation would diminish, so that for a current of given velocity the formula might be written—

$$p_0 = p_1 - AP \left(1 + \frac{1}{\alpha} \right) (t - t')$$

where A is a constant and probably equal to 0.000630, α is constant for a given velocity of the air, and for a given pressure. Its value, as deduced from a consideration of observations by BLANFORD, ANGOT, CHISTONI, and the author of the paper, is 3.0 P/760. Moreover, PERNTNER considers a certain further correction in the following

* 'Sitzungsberichte der Wiener Akademie,' vol. 87, 1883.

paragraphs, which I have translated directly from his memoir :—“ A further influence, of which no account was taken in the formula, lies in the muslin covering of the wet thermometer. In establishing the formula no regard was had to any such effect. KÄMTZ has, however, called attention to the frequent case, which he had occasion to observe, that the moist thermometer stood sometimes as high, sometimes higher, than the dry, when a comparison with the hygrometer did not show a saturation of the air with water vapour. He had investigated the cause of this phenomenon, and had found it in the muslin covering of the wet thermometer. According to his experiments at low temperatures, the wet thermometer stood $0^{\circ}\cdot46$ C. too high.

“ That this should be an effect of radiation, as is no doubt the case with dry muslin, would require as a consequence that the radiation effect should increase with increase of the difference $(t - t')$. The fact is, however, that this effect in proportionate comparisons diminishes (*mehr verschwindet*) as $t - t'$ becomes greater, at any rate in moving air. This behaviour will require further searching investigation. I will, however, at present mention that I am of opinion that this phenomenon is to be ascribed to an ‘inertia’ of the wet thermometer when near the saturation point of air, so that in very moist air the evaporation does not take place rapidly enough in relation to the in-rushing air. This is confirmed also by experiment. If account is to be taken of this ‘inertia’ of the psychrometer in the formula, a correction can be so applied to the psychrometer difference $(t - t')$, that it is a maximum when $t - t' = 0$, and is chosen inversely proportional to the difference $(t - t')$. If v be the maximum value, this correction would be $= v/(t - t' + 1)$; if we, with KÄMTZ, take $v = 0^{\circ}\cdot5$ C., this correction will only be of appreciable value up to differences of 9° , practically only up to those of 6° .”

I have translated this directly, as the point is an important one, and this is the first explanation of it that I have seen attempted, but I cannot say that I fully understand the method of introducing PERNTER'S correction. The quantity v is said to be the maximum value of the correction, which I should take to be the temperature excess of the wet bulb over the dry when the air is *actually* saturated, but this would evidently not be the case when $t - t' = 0$. The complete formula, as thus corrected, stands thus—

$$p_0 = p_1 - P \times 0\cdot000630 \left\{ 1 + \frac{760}{3\cdot0 P} \right\} \left\{ t - t' + \frac{0\cdot5}{t - t' + 1} \right\}.$$

There is evidently something wrong with this, as the condition for saturation, viz, $p_0 = p_1$, leads to the imaginary difference—

$$t - t' = -\cdot5 \{ 1 \pm \sqrt{-1} \}.$$

No comparisons of dew-points computed by the formula with actual observations are given, although the observations of BLANFORD, CHISTONI, and others are discussed in the course of the paper, and the values of the constants deduced from them are given.

PERNTER has also made some comparisons between the results of observations with WILD's ventilated-psychrometer and REGNAULT's hygrometer on the Obir (2048 metres high); these observations are 30 in number, the dry-bulb temperature lying between $6^{\circ}2$ C. and $14^{\circ}4$ C., the temperature difference between $0^{\circ}6$ and $3^{\circ}6$, and the barometer between 591 mm. and 600 mm., so that they all refer to states of great humidity. The values of REGNAULT's constant A are computed for each observation; they vary between $\cdot0008432$ and $\cdot0014946$.

In spite of the recommendation of REGNAULT and others, the hair hygrometer has not been entirely abandoned. In 1852 KREIL used a hair hygrometer as a hygrometer and found a difference of ± 4 per cent. between its indications and those of a psychrometer, but he laid some part of the errors at the door of the psychrometer. In 1858 PICHOT suggested the graduation of a hair hygrometer by using always the same air, and producing changes in the humidity by changing the pressure or temperature. An improved form of hair hygrometer was introduced by HERMANN and PFISTER* in 1870, and during the same year R. WOLF compared the indications of two such instruments with those of a psychrometer; the results are in favour of the hair hygrometer, especially below the freezing point, inasmuch as the two hair hygrometers are nearer together than either and the psychrometer, but no comparison with a standard method was made, so that the evidence of superiority is not quite complete. In 1878 Dr. KARL KOPPE of Zurich published a small book† in which the whole question of hygrometric measurement is briefly discussed and the advantages of a hair hygrometer with provision for adjustment are more elaborately described. The adjustment is carried out by means of a screw turned by a watch key, and in order to get a fixed point the hygrometer is enclosed in a metal case provided with a glass front and back. The front can be removed for observations, but for adjusting it is put in its place and the interior of the case is saturated by a wet cloth. KOPPE regards hair hygrometers as quite sufficiently comparable, and gives the following particulars as to the expansion of hair by heat alone. DE SAUSSURE found an expansion for 1° R. of 19 millionths of the length of the hair, and KOPPE, by observing the readings in completely saturated air at various temperatures, found no change of length indicated for changes of temperature up to 20° C.

REGNAULT has taken up the subject of hygrometric methods again in CARL'S 'Repertorium,' vol. 8, 1872, p. 1.‡ He, however, only recapitulates his former conclusions, adding some practical directions for applying the chemical method continuously in meteorological observatories so as to obtain the mean quantity of moisture for every three hours. VON BAUMHAUER,§ referring to REGNAULT'S paper,

* 'Carl, Repertorium,' vol. 6, p. 117.

† 'Die Messung des Feuchtigkeitsgehaltes der Luft.' Zurich, 1878.

‡ Translation of part of a paper entitled "Instructions pouvant servir à l'établissement des observatoires météorologiques" in the Geneva 'Archives des Sciences,' vol. 40, 1871.

§ 'Poggendorff's Annalen,' vol. 148, 1873, p. 448. (Translation of a paper in the 'Archives Néerlandaises,' vol. 6, 1881.)

agrees with that author in preferring the chemical method, and recalls the attention of meteorologists to an apparatus, described by him 17 years previously, intended to give continuous observations by the chemical method. It is a sort of hydrometer containing pumice and sulphuric acid, and is floated in an oil bath, and there is an arrangement for passing the air through without interfering with the free flotation.

A similar apparatus is described by SNELLEN.* I am inclined to look upon all apparatus requiring anything like a long leading tube with very grave suspicion. A sudden change in the temperature such as sometimes covers all walls and furniture with a deposit of moisture would supply a layer of water to the leading tube that would nearly saturate the air for a considerable time and render the instrument worse than useless at a time when its results are wanted to be most accurate. VON BAUMHAUER suggests leading air by means of a lead tube from a captive balloon to his instrument in an observing room. It is almost painful to try and think what an observation with such an arrangement would really mean.

A short paper by LEFEBVRE† gives the results of experiments to compare REGNAULT'S hygrometer with the chemical method. One series of experiments compares the hygrometric state as given by the chemical hygrometer with that deduced from the dew-point reading of REGNAULT'S instrument, taking the dew-point to be:—

- (1.) The temperature of the first appearance of dew.
- (2.) The temperature of disappearance.
- (3.) The mean of the two preceding.

The collective results may be tabulated as follows, calling differences from the results of the chemical hygrometer errors:—

Number of observations, 15.

Range of temperature, 17°·3 C. to 24°·0 C.

Extreme error of "first appearance" observations	{	- 2·00	in percentage humidity.
		+ ·60	" "
" " "disappearance" "	{	- 1·83	" "
		+ 1·18	" "
" " "mean" "	{	- 1·92	" "
		+ ·75	" "
Mean error of "first appearance" observations	.	·47	" "
" " "disappearance" "	.	·74	" "
" " "mean" "	.	·55	" "

A second set of experiments gives a comparison of two chemical hygrometers with each other and with REGNAULT'S. I give a table of the differences in the fractional

* 'Archives Néerlandaises,' vol. 9, p. 477.

† 'Annales de Chimie,' vol. 25, 1849, p. 110.

humidity between the two chemical hygrometers, and between their mean and REGNAULT'S, the fractional humidity in each case being not far from $\cdot 50$:—

No. of Experiment.	Difference between the fractional humidity as given by two chemical hygrometers.	Difference between REGNAULT and the mean of chemical hygrometers.
1	+ $\cdot 0007$	+ $\cdot 0060$
2	- $\cdot 0058$	+ $\cdot 0051$
3	+ $\cdot 0031$	+ $\cdot 0157$
4	+ $\cdot 0016$	+ $\cdot 0002$
5	+ $\cdot 0055$	+ $\cdot 0047$
6
7	+ $\cdot 0016$	+ $\cdot 0018$
8	+ $\cdot 0004$	+ $\cdot 0062$
9
10	+ $\cdot 0004$	+ $\cdot 0062$
11	+ $\cdot 0000$	+ $\cdot 0049$
12	- $\cdot 0031$	- $\cdot 0087$

These results are very satisfactorily concordant. The aspirator for the chemical method contained 4.278 litres, and the tubes were filled with sulphuric acid and pumice, the air was taken from out of doors, and the REGNAULT, placed at the window, was observed at the beginning, middle, and end of each experiment, and the mean of the three dew-point readings taken.

Observations were also taken with DANIELL'S instrument, but they were so discordant that they are not recorded.

For many years REGNAULT'S form of condensation hygrometer was regarded as a satisfactory standard instrument. In 1877 ALLUARD introduced a somewhat modified form, which was used by ANGOT in the researches already discussed (p. 133). The modification consisted in replacing the silver thimble by a brass tube of square section provided with various metal tubes to allow of the passage of air through the ether contained in the tube. Instead of the glass upon which REGNAULT'S thimble was mounted, there are two windows in opposite sides of the square tube, near to the top. These enable the bubbling of the air through the ether to be watched. The sides of the tube are gilt and highly polished, and one of them is framed by a broad band of brass, gilt and polished in like manner. This surrounding band is very near to, but does not touch, the brass tube. The dew is therefore deposited on a flat gilt surface, and the identification of a deposit is rendered easier by the proximity of an unaltered surface with which to compare the cooled one.

A dew-point instrument was also introduced in 1871, by Mr. DINES, in which the dew deposit was caused by cooled water, and took place on blackened glass; this was modified by its inventor in 1879,* so that it could be used with either water or ether.

* See Symons's "History of Hygrometers," 'Meteorol. Soc. Quart. Journ.,' vol. 7, p. 161.

These instruments, however, may be regarded as being intended to facilitate the reading of the dew-point rather than as questioning the results given by REGNAULT'S instrument. The case is different with a new condensing hygrometer, introduced by CROVA.* This consists of a tube of thin brass, the interior of which is nickel-plated and carefully polished; one end is closed by a plate of ground glass, and the other by a lens. The air to be experimented on is drawn through this tube by means of two tubulures entering it at right angles, and the tube is cooled by surrounding it with a brass box, partly filled by carbon disulphide, through which air is made to pass. The temperature is read by means of a thermometer dipping into the liquid. The dew deposit is seen by means of the lens, and the arrangement is such that the mean temperature of appearance and disappearance can be read to less than $0^{\circ}\cdot 1$ C. This arrangement was adopted by CROVA, because he was dissatisfied with the behaviour of REGNAULT'S instrument when the dew-point was very low, and when there was a considerable wind.

Having arranged the instrument, he made a series of comparisons of the results of REGNAULT'S instrument, observed in the open air or window, with those of his own, the air being in that case conducted through a fine lead tube to the instrument inside. The thermometer employed had been previously corrected.

The following are some of the results:—

SERIES I.—Wind north-east, fairly strong. Barometer 759.

Hour.	Temperature of air.	Dew-point.	
		REGNAULT.	CROVA.
h. m.	$^{\circ}$	$^{\circ}$	$^{\circ}$
9 40	21·0	4·6	4·7
9 50	21·0	5·3	5·9
10 0	21·0	5·9	7·4
10 30	21·5	6·5	7·5
11 0	22·0	6·5	7·0
1 30	22·5	6·7	8·3

SERIES II.—Wind north-west, gradually increasing. Barometer 758. Psychrometer humidity 0·54 to 0·58.

Hour.	Temperature of air.	Dew-point.	
		REGNAULT.	CROVA.
h. m.	$^{\circ}$	$^{\circ}$	$^{\circ}$
8 15	19·4	9·5	9·5
8 30	19·9	9·7	10·5
8 45	19·7	8·3	9·8
9 0	19·6	8·1	9·5

* 'Journal de Physique,' vol. 2, 1883, p. 166.

SERIES V.—North-west to north, variable. Barometer 751. Psychrometer humidity 0·405.

Hour.	Temperature of air.	Dew-point.	
		REGNAULT.	CROVA.
h. m. 2 30	16°75	Succes- sively. {	2·45
			3·05
			1·90
			1·25
			2·05
3 0	..	1·35	5·65
			4·05 Constant

These differences in the dew-point readings by the two methods are very serious, as, if this be the real state of affairs, the work done with REGNAULT'S hygrometer as a standard instrument is so far invalidated.

It will be noticed that the methods of finding the pressure of aqueous vapour in air hitherto described are all indirect: that is to say, when the instrument has done its work, a calculation has to be gone through in order to obtain the value of the vapour pressure. The dew-point method approaches, perhaps, the nearest to a direct method, but it must be remembered that what is actually observed, or rather what is supposed to be observed, is the temperature at which a deposit takes place on a certain cooled surface. It has always been assumed that this temperature corresponds to the temperature of saturation of the air, and that in consequence we may take the corresponding pressure of vapour from REGNAULT'S table. The experiments of REGNAULT referred to above, page 120, do not completely justify this assumption, although the error is but small, but those of CROVA just described throw doubt, for different reasons, upon this fundamental point.

The calculation of the pressure of vapour from the weight of water contained in a given volume of air—the chemical method—depends in like manner upon assumptions for which there is at present no absolute experimental verification. Such experiments as there are show discrepancies between observation and calculation which are beyond the limit of experimental errors. And yet a direct experimental determination seems in principle extremely easy. Assuming only that the pressure of dry air in moist air is independent of the pressure of the vapour, which must be assumed if we are to assign any meaning at all to the “pressure of aqueous vapour in the atmosphere,” it follows that, if we abstract the moisture without altering the volume, and measure the resulting pressure of the dry air, the pressure of the vapour is simply the difference between the initial and final pressure of the air. Moreover, the dry air is known to obey the law of BOYLE with extreme exactitude for small variations of pressure and volume, and hence the measure of the diminution of volume of air produced by the

removal of its moisture without altering its pressure furnishes an equally accurate means of finding the original pressure of the vapour. Only one stage less direct is the method of finding the increase of pressure produced by saturating a specimen of air whose volume is kept constant and assuming the saturation pressure from REGNAULT'S tables. Calling such determinations "absolute," an instrument for making them is called an absolute hygrometer, and of these very many have been suggested by various experimenters in recent years. Before describing them, I will just mention that the simplicity of the method is entirely illusory. The differences of pressure to be measured are small, never practically exceeding 30 mm. in our climate, and being generally about 10 mm. Taking the latter amount, and supposing that an accuracy of 1 per cent. is required, the pressure difference must be measured to 0.1 mm., which is less than $\frac{1}{5000}$ of the ordinary barometric pressure. Consequently the volume or the pressure must be kept constant during the measurement to the same degree of accuracy; and, since a change of temperature of 1° C. alters the volume or pressure by about .00366 of its amount, the temperature of the apparatus must be kept constant, or its change compensated for, to within 0.02, which is a very difficult matter. Further, any taps or connexions must hold sufficiently tight to guarantee that the vessel will not lose or gain air to the extent of $\frac{1}{5000}$ of its whole volume during the course of an experiment. These are a few of the difficulties.

The best known of these new instruments is that of SCHWACKHÖFER, described and figured in JELINEK'S 'Zeitschrift' for 1879. In its modified form it is an apparatus for finding the diminution of volume of air in consequence of the absorption of the vapour by sulphuric acid. The air is first enclosed in a burette, with a graduated stem communicating with a mercury cistern; the burette is then, by a suitable arrangement of taps, brought into communication with a second vessel, open at the bottom and shaped like an elongated bell, containing a number of vertical glass tubes, and filled up with sulphuric acid. The mercury is then forced by means of a plunger dipping into its cistern into the burette, and the air passes over into the second vessel, displacing the sulphuric acid, leaving, of course, a large surface of acid exposed, which dries the air after four or five transfers backwards and forwards. The sulphuric acid is then driven back to its original level, which is carefully marked; the pressure of the dried air is adjusted to be the same as before by means of an oil gauge, and the diminution of volume is read off on the graduated stem of the burette. The temperature is read by a thermometer sealed into the burette, and kept uniform by a jacket of glycerine round the burette.

I have omitted a large number of small precautions which are quite necessary in taking an observation. The apparatus is very delicate, and is therefore available only for laboratory use.

PERNTER worked with an instrument of this kind. He says: "Bei fünfzig Versuche waren resultatlos, bis es mir gelang die Hähne zu dichten, das Quecksilber luftfrei und rein zu halten und die ganze Röhre vor aller Verunreinigung zu schützen.

Endlich, nachdem ich drei Wochen daran mich abgemüht, schienen die Versuchsergebnisse einigermassen entsprechend. Die . . . fünfzig . . . Versuche lieferten alle eine zu kleine absolute Feuchtigkeit und zwar im Mittel um 2.5 mm. circa zu klein."

A similar apparatus was used by SWORYKIN, who seems to have had more success with it.

An apparatus somewhat similar to SCHWACKHÖFER's was designed and made by Mr. DINES. It is described in the 'Meteorological Magazine,' September, 1883, and is said to work satisfactorily.

The next instrument of the kind is EDELMANN's, described in WIEDEMANN's 'Annalen,' vol. 6, 1879, p. 455. It is of very much simpler construction. A glass cylinder is closed at each end with india-rubber corks perforated so as to take a tube containing a thermometer and connected with a mercury pressure-gauge at one end, and at the other a second tube communicating with the top and bottom respectively of a small vessel of sulphuric acid by means of glass cone joints. The mercury gauge is also attached to a cone joint; there are stop cocks between the cone joints and the cylinder. The cylinder is provided with a metal jacket. By the double connexion the sulphuric acid is allowed to run into the cylinder without altering the volume of the air. Between each operation the cylinder has to be cleaned and dried, and washing out with alcohol and ether is recommended. It is said to give good results, though none are quoted in the paper, and if, as suggested, the vessel is finally dried by passing through it air from a "Wasser-trommel-gebläse," good results ought certainly to be received with unusual gratitude. The description of the instrument, and its use, reads rather like an instrument maker's solution of the problem "to make an absolute hygrometer." VAN HASSELT* has described an instrument somewhat similar to EDELMANN's, but he uses phosphoric anhydride instead of sulphuric acid. The anhydride is enclosed in a thin glass bulb, which is broken by shaking when the moisture is to be absorbed; the difference of pressure is observed by means of an oil manometer, which consists of two glass tubes connected by a flexible india-rubber one. This arrangement allows the volume to be reduced to its original value before reading off the pressure.

RÜDORFF† uses a Woulff's bottle, with three necks, in an ingenious manner. Into the middle neck is fixed a burette, which supplies the sulphuric acid; this is allowed to pass into the vessel until the pressure reaches its original value, as indicated by a manometer of sulphuric acid, sp. gr. 1.300. Thus the volume of sulphuric acid run in gives at once the diminution of volume of the air due to the absorption of the moisture.

This plan was somewhat improved by NEESEN,‡ who has two Woulff bottles, one

* 'Beiblätter,' 1879, p. 697

† 'Beiblätter,' 1880, p. 349

‡ 'WIEDEMANN, Annalen,' vol. 11 1880, p. 526.

connected with each limb of the manometer. By this means the temperature correction is very ingeniously compensated, and, moreover, a second determination can be made with the same apparatus by using the other bottle.

MATERN* also uses the compensation by a second vessel, but the vessels are glass cylinders, provided with lids ground flat, so that they can simply be laid on. The necessary connexions, &c., are sealed into the lids. The method he suggests is that of finding the increase of pressure, due to the saturation of the air, by allowing water to drop into one of the vessels on to blotting paper. He also gives† a simplified form of the instrument for more rapid use.

I have not been able to find any series of observations showing the comparative results obtained with these different instruments: they have been tried by their inventors, but means of comparison are not at hand, except for SCHWACKHÖFER'S instrument, as already mentioned.

NOTE B.

HYGROMETRICAL TABLES.

It may be useful to add a statement with reference to the tables at present in use by meteorological observers in different countries for the reduction of wet-and-dry-bulb observations. Mr. R. H. SCOTT has very kindly supplied me with the following list of original hygrometrical tables:—

HAEGHENS ('Annuaire de la Société Météorologique,' 1849), followed by RENOUE, France, and DENZA, Italy.

WILD ('Repertorium für Meteorologie,' vol. 1, St. Petersburg, 1870), followed by JELINEK, Vienna, 1876.

BLANFORD.—'Tables for the Reduction of Meteorological Observations in India.' Calcutta, 1876.

GLAISHER.—'Hygrometrical Tables,' 7th edition. London, 1885.

GUYOT.—"Psychrometrical Tables," 'Smithsonian Miscellaneous Collections.' Washington, 1887.

Of these HAEGHENS'S and WILD'S tables are millimetre-centigrade tables founded upon REGNAULT'S table of pressures and the psychrometric formulæ of the same authority, viz.:—

$$e' = e - \frac{0.480(t - t')}{610 - t'} b, \quad t' > 0^\circ \text{C.}$$

$$e' = e - \frac{0.480(t - t')}{689 - t'}, \quad t' < 0^\circ \text{C.}$$

* 'WIEDEMANN, Annalen,' vol. 9, p. 147.

† 'WIEDEMANN, Annalen,' vol. 10, p. 149.

BLANFORD'S tables are based upon the same observations and formulæ, but they are adapted for English inches and FAHRENHEIT degrees (see p. 135). The following information respecting them is extracted from the introduction to the tables (p. 4):—

“TABLE III.—This table gives the tension of saturated aqueous vapour, in decimals of an inch of mercury at the temperature 32° , in latitude 22° , at the level of the sea. It has been reduced from the original table for the latitude of Dublin, computed (from REGNAULT'S observations) by the Rev. ROBERT DIXON, by correcting his values for the difference of gravity, viz., multiplying them by the constant factor 1.00286184.

“The psychrometric tables which follow are all based on this table, and the computation has been chiefly made by the aid of the arithmometer.

“AUGUST'S formula (modified by REGNAULT), which has been used in computing the Tables IV. to XI., is as follows:—

“For temperatures of the wet bulb below 32° :

$$x = f' - \frac{.480(t-t')}{1240.2-t'} h,$$

and for temperatures of wet bulb above 32° :

$$x = f' - \frac{.480(t-t')}{1130-t'} h,$$

wherein t' and t are the temperatures of the dry and wet bulb thermometers respectively, in FAHRENHEIT degrees, f' the tension of vapour at temperature t' , h the reading of the barometer in inches, and x the tension of the vapour present in the air at the time of the observation.”

GLAISHER'S tables are based upon REGNAULT'S table of pressures, and the table of Greenwich factors, which the following extract from the introduction to GLAISHER'S tables (p. 4) sufficiently describes. These are adapted for English inches and FAHRENHEIT'S scale of temperature:—

“*Determination of the Dew-point from observations of the Dry and Wet Bulb Thermometers.*”

“TABLE I.—Factors by which it is necessary to multiply the excess of the reading of the dry thermometer over that of the wet, to give the excess of the temperature of the air above that of the dew-point, for every degree of air temperature, from 10° to 100° .

“The numbers in this table have been found from the combination of many thousand simultaneous observations of the dry and wet bulb thermometers and of DANIELL'S hygrometer, taken at the Royal Observatory, Greenwich, from the year 1841 to 1854, and from observations taken at high temperatures in India, and others at low and medium temperatures at Toronto. The results at the same temperatures

were found to be alike at these different places; and therefore the factors may be considered as of general application.

“By means of the numbers in this table the temperatures of the dew-point in the general tables have been calculated; and these were for many years checked by direct observations with DANIELL’S hygrometer made by me at the Royal Observatory, Greenwich.”

GUYOT’S tables are a very complete set, comprising:—

- i. Extended tables based upon REGNAULT’S formulæ already given, adapted for millimetres and Centigrade degrees.
- ii. The same adapted for inches and FAHRENHEIT degrees. (These are given “for ordinary use.”)
- iii. GLAISHER’S table of Greenwich factors and an old set of GLAISHER’S psychrometrical tables, based upon them and a Greenwich table of pressures. (These are given for the purpose of “comparing results.”)

Besides these there are ANGOT’S tables referred to on p. 134, but I am not aware that they are in general use in any country.

The information may be summarised in the following table:—

Country.	Tables.	Formula of reduction.	Units.
England	GLAISHER	Greenwich factors . .	Inches, FAHRENHEIT
India	BLANFORD	REGNAULT’S formulæ .	” ”
America	GUYOT	” ”	Both
France	HÆGGHENS	” ”	Millimetres, Centigrade
Italy	”	” ”	” ”
Germany	WILD	” ”	” ”
Austria	JELINEK	” ”	” ”
Russia	WILD	” ”	” ”

NOTE, APRIL 23, 1888 (see p. 83).

I have assumed REGNAULT’S investigation of the behaviour of sulphuric acid tubes (‘Annales de Chimie,’ vol. 15, 1845) to be conclusive evidence that *his* acid was efficient for its purpose. His experiments were of two kinds. First, four sulphuric acid tubes were mounted “in series.” The first two were at the ordinary temperature, the third at 0° C., and the fourth at — 30° C. The first tube gained 1.235 gramme in weight when moist air was sent through the four; the weights of the other three tubes were unaffected. Secondly, air was dried by passing it through three sulphuric acid tubes, each one metre long, the third being in a freezing mixture. The dried air was then led through a weighed vessel containing moist sponge, and thence through two weighed sulphuric acid tubes. The first of these two

took up 0.767 gramme of moisture, being precisely the same amount as that lost by the sponge vessel. My attention has lately been called to the papers by E. W. MORLEY ('Amer. Journ. Sci.,' vol. 30, 1884, p. 140; vol. 34, 1887, p. 199), in which the question of residual moisture in air passed over sulphuric acid or phosphoric anhydride is more rigorously treated. It appears that the residual moisture would not appreciably affect the weighings in the observations here recorded, for sulphuric acid only leaves unabsorbed "not far from a fourth of a milligramme of moisture in 100 litres of a gas," and "the moisture left unabsorbed by phosphorus pentoxide, if capable of determination, may be very roughly stated as possibly a fourth of a milligramme in 10,000 litres."

NOTE, APRIL 23, 1888 (see p. 97).

The researches of WARBURG and IHMORI ('Wiedemann, Annalen,' vol. 27, 1886, p. 481) afford some information about the formation of a deposit. On a specimen of fresh lead glass at about 16° C. exposed to the vapour of water *in vacuo* a deposit of 18×10^{-8} grammes per square centimetre was observed when the vapour pressure inside the vessel corresponded to the temperature 4°.71 C.; the deposit increased to 194×10^{-8} grammes per square centimetre when the vapour pressure was increased so as to correspond to a temperature 0°.87 C. below that of the glass surface. No results are given for lead glass which enable one to form an opinion as to what happens when the difference of temperature is made very small, but for Thuringian glass a sudden increase of 50 per cent. in the deposit is shown for a diminution of the temperature-difference from 0°.27 C. to 0°.17 C. With lead glass, of which even a fresh surface showed comparatively very feeble absorption of vapour, no absorption at all was detected after treatment with boiling water. The apparatus referred to in the text was, excepting the three-way tap, constructed of lead glass that had been frequently washed with cold water.

Fig. 1.

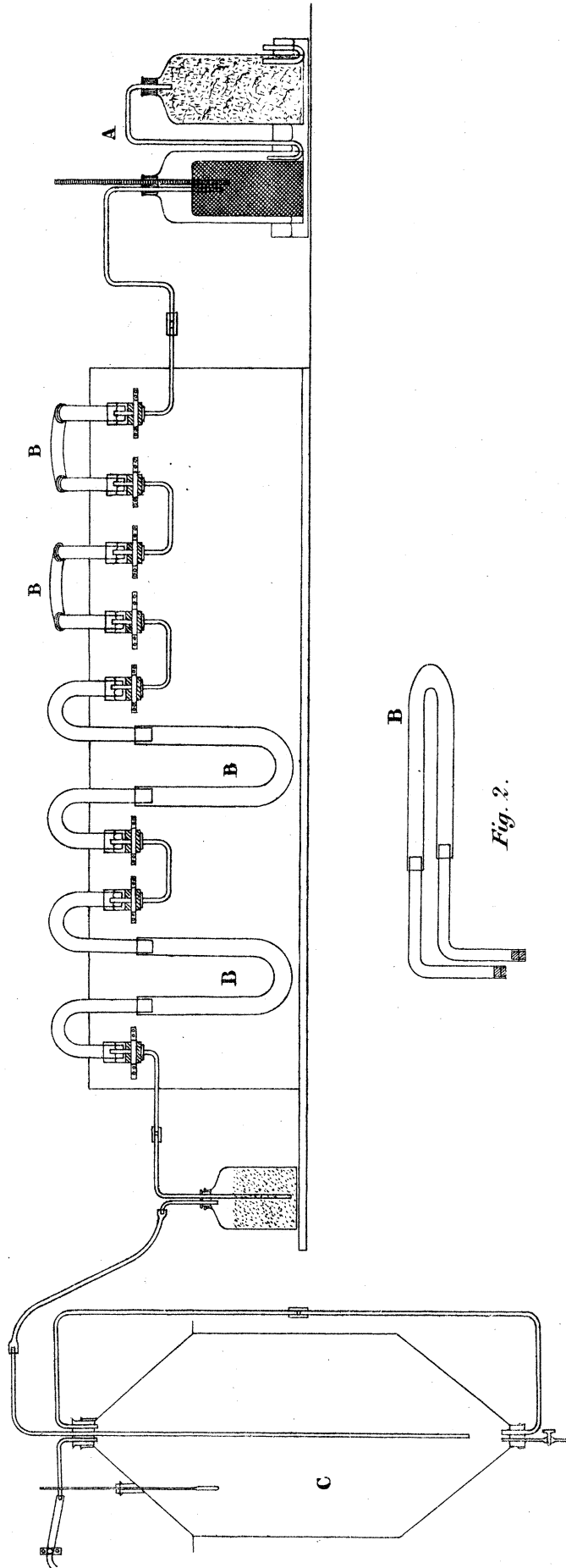
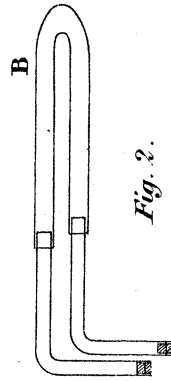


Fig. 2.



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