XXV. *On the Determination of the Constants of the Cup Anemometer by Experiments with a Whirling Machine*—Part II.

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(55.)* In the preceding Part (Philosophical Transactions for 1878, p. 777) I gave the results obtained by anemometers attached to a whirling machine, which substitutes motion through the air for real wind. If the air were quiescent this method would be nearly unexceptionable; but the whirling gives the air a vorticose motion for which it is impossible to make an exact allowance, and therefore some uncertainty affects these results. In the conclusion of that paper I expressed an opinion that greater certainty might be obtained by comparing two anemometers, similar and equal in every respect except friction; and stated that I would endeavour to carry this into effect. I propose now to give an account of my attempt to do so.

(56.) The instruments used, and their arrangement, are described in paragraph (51). The situation in which they are placed would be a good one but for the dome of the west equatorial, which in some points of the wind may interfere with its full action on one or the other of the instruments.

The diameter is 13′·6; the height of its summit above the platform is 15′·75; that of the arms of the instruments being 16′. The horizontal distance of its centre from the Kew instrument (K)=21′·5, bearing from it S.S.E., 2° S. The distance from the experimental one (E) is 23′, and its bearing S.W. b. S.

The distance between K and E=22′. Of course when the wind is S.S.E., K will be less acted on than E, and *vice versâ*, but probably the difference will be much less than that caused by fluctuations of the wind itself. When the wind is E. or W. the eddies caused by the windward anemometers may perhaps reach the leeward, but not, I think, to any great extent.

(57.) The chronograph record of each experiment was at first entered in groups during which v, the velocity of K, was nearly uniform; and A, the number of turns made by each instrument, was an integer. The length of the chronograph helix gives the time; it is measured in eighths of an inch (as the Observatory possesses a scale of eightieths) and when divided by the length of a second on the same scale, we have the number of seconds. As the chronograph in its present situation is exposed to considerable variations of temperature, its rate is not as regular as it was at Rathmines, but the second-space was determined each day of observations. The average in

* For facility of reference the numeration of the paragraphs and tables is in sequence to that of Part I.

winter is 1·665; and the times so deduced are certain to less than $0^{s}\cdot1$. Latterly the time was noted by a watch.

(58.) It was soon found that the method proposed in paragraph (52) is not available, for the wind is never uniform long enough to make two successive experiments fairly comparable. It was therefore necessary to use that of paragraph (53). Assuming such values of α, x, and y, the constants of equation III. as will give V very nearly equal to V′ (the accented letters belong to E), we may correct them so that the mean V′—V may vanish. This assumes first, that however the wind may vary in the course of an observation from one instrument to another, yet if the time be sufficient it comes to each of them with an equal amount; its deficiency at one part of the time being made up by its excess at another; and secondly, that the V computed for a mean value of v will be its own mean value.

(59.) As to the first of these assumptions, I have come to the conclusion that if an observation lasts for nine or ten minutes, the average action of the wind on the two instruments will be nearly equal, though during portions of the time it may vary very much. This may be illustrated by the following table, which contains a set of v and v' taken with the normal frictions at K and E, which are 13·5 and 23·2; these were taken September 17, 1878, under unfavourable circumstances, for the wind was S.W. The v and v' ought to be nearly equal, for the difference of the friction will only diminish v by 0·24.

TABLE XX.

No.	Time.	v.	v'.	$v-v'$.	No.	Time.	v.	v'.	$v-v'$.	No.	Time.	v.	v'.	$v-v'$.
	s.					s.					s.			
1	15·1	6·522	4·660	1·869	22	5·2	6·441	6·441	0·000	43	18·4	5·349	6·876	−1·527
2	28·0	5·254	5·254	0·000	23	10·3	5·475	5·475	0·000	44	29·4	7·175	8·400	−1·225
3	25·5	7·156	6·620	0·536	24	9·2	6·132	4·599	1·533	45	50·3	7·540	6·143	1·397
4	11·5	6·084	3·650	2·334	25	17·5	8·012	6·409	1·603	46	32·4	6·077	5·208	0·869
5	11·3	7·638	6·236	1·402	26	22·0	5·702	4·474	1·228	47	9·2	4·589	5·842	−1·253
6	21·1	5·313	5·313	0·000	27	9·6	5·882	5·882	0·000	48	16·4	5·989	5·135	0·854
7	13·5	4·174	2·086	2·088	28	32·4	7·791	7·353	0·438	49	11·8	7·142	5·959	1·183
8	8·5	6·624	6·624	0·000	29	10·8	6·482	6·482	0·000	50	19·3	5·082	4·841	0·241
9	12·1	4·642	3·481	1·161	30	12·1	4·595	5·795	−1·200	51	45·3	6·512	6·512	0·000
10	28·7	2·934	4·995	0·939	31	15·2	3·697	4·622	−0·925	52	39·2	5·378	3·222	2·148
11	10·9	6·458	5·162	1·296	32	35·4	4·637	3·174	1·462	53	14·0	3·193	3·017	0·176
12	13·2	4·256	5·311	−1·055	33	10·8	5·163	2·581	2·582	54	13·9	5·040	4·032	1·008
13	5·8	7·314	7·314	0·000	34	22·5	3·739	1·246	2·493	55	20·4	4·116	3·430	0·686
14	10·6	9·247	9·247	0·000	35	14·6	3·852	0·764	3·088	56	19·7	4·281	2·140	2·141
15	25·1	8·959	6·298	2·661	36	29·4	4·298	1·910	2·388	57	15·5	4·525	1·802	2·723
16	7·3	3·817	1·915	1·902	37	8·4	5·032	3·355	1·677	58	14·8	3·794	2·846	0·948
17	11·2	5·006	3·754	1·252	38	15·0	4·693	2·816	1·877	59	31·9	5·599	4·900	0·699
18	13·7	6·127	6·127	0·000	39	13·3	4·229	4·229	0·000	60	24·5	4·560	3·434	1·126
19	12·4	6·815	4·544	2·271	40	18·2	3·885	3·885	0·000	61	18·0	4·691	5·473	−0·782
20	6·5	8·589	8·589	0·000	41	45·5	7·247	8·153	−0·806	62	21·2	6·626	8·540	−1·914
21	17·1	6·554	5·735	0·819	42	11·5	7·358	6·512	0·846					

Total time $=646^{s}\cdot7$; mean $v=5\cdot816$; mean $v'=5\cdot218$; mean $v-v'=0\cdot598$.

These show plainly both the variation of wind at one anemometer and the difference at the two. In No. 14, $v=9\cdot247$; in No. 10, it is 2·934. These represent V=26·264, and 8·551. If we look to the column $v-v'$, at No. 35 we find +3·088, at No. 62

$-1{\cdot}914$; fourteen are $=0$, and nine are negative. But if we divide them into four consecutive and nearly isochronal groups the discordance is much less.

s.			
Time $=161{\cdot}4$	$v=6{\cdot}219$	$v'=5{\cdot}203$	$v-v'=1{\cdot}016$
$159{\cdot}8$	$5{\cdot}508$	$4{\cdot}375$	$1{\cdot}133$
$160{\cdot}6$	$6{\cdot}241$	$5{\cdot}926$	$0{\cdot}315$
$164{\cdot}9$	$5{\cdot}369$	$5{\cdot}067$	$0{\cdot}302$

The extreme range here is $0{\cdot}831$ instead of $5{\cdot}002$, grouping them in pairs

$T=321{\cdot}2$	$v=5{\cdot}864$	$v'=4{\cdot}791$	$v-v'=1{\cdot}073$
$325{\cdot}5$	$5{\cdot}799$	$5{\cdot}489$	$0{\cdot}310$

There can be little doubt that the total means are nearly correct, and these values of $v-v'$ differ from the mean one by $+0{\cdot}475$ and $-0{\cdot}289$. In general, $v-v'$ will be less than this; and if it be observed by inspecting the chronograph while an observation is proceeding that the ratio of A to A′ varies notably, a longer time should be taken.

(60.) As to the second point it is easily shown that no great errors can arise from assuming that V is truly given by the mean v. The mean V of a series is, taking the time into account,

$$=\frac{\mathrm{SVT}}{\mathrm{ST}}=\frac{x\mathrm{S}v\mathrm{T}}{\mathrm{ST}}+\frac{\mathrm{S}v\mathrm{T}\times\sqrt{z+\frac{\phi}{v^2}}}{\mathrm{ST}}$$

Now the first of these $=x\times$ mean v. In instruments like K where ϕ is small, if we develop the radical in powers of ϕ, the second term becomes

$$\mathrm{S}v\mathrm{T}\times\left(\sqrt{z}+\frac{9\phi}{2v^2\sqrt{z}}\right)$$

and as the ϕ term may be neglected the mean of radical becomes $\sqrt{z}\times$ mean velocity. When ϕ is large the simplest course is (calling the radical R) to compute $\frac{\mathrm{S}v\mathrm{T}\times\mathrm{R}}{\mathrm{ST}}$, or what comes to the same thing $\frac{\mathrm{S}v\mathrm{CR}}{\mathrm{SC}}$, C being the time-space, and compare this with the R computed with the mean v. Taking at random No. X. of Table XXI. whose $\phi=343{\cdot}28$, we have for the separate groups whose A and A′ are nearly uniform

No.	C.	v'.
I.	31·8	6·081
II.	95·2	7·411
III.	88·3	7·350
IV.	49·35	4·886
V.	32·5	7·364
VI.	18·68	6·042
	315·83	6·835

$$\frac{\text{sum } v\mathrm{CR}}{\text{sum C}}=20{\cdot}633.$$

R for mean $v'=20{\cdot}689$.

Requiring the correction $=0{\cdot}056$.

Here the v''s do not range very widely, and I take a more aberrant set observed September 16, 1878, $\phi'=173{\cdot}80$.

No.	C.	v'.
I.	15·38	3·768
II.	107·86	9·537
III.	99·83	4·644
IV.	55·11	3·155
V.	48·47	1·196
VI.	60·66	4·060
VII.	63·16	1·606
VIII.	36·39	1·5925
IX.	54·87	7·393
X.	64·98	5·351
XI.	15·05	0·9625
XII.	16·02	6·331
XIII.	23·09	6·275
XIV.	43·58	4·987
	704·45	4·854

$$\frac{\text{sum } v'\text{RC}}{\text{sum C}}=14{\cdot}984.$$

R for mean $v=14{\cdot}728$.

Correction $=+0{\cdot}156$.

Even here the error is not of a nature to interfere with the determination of the constants, though in such work terms like V. and XI. had better be omitted. If it were thought necessary the exact computation is not difficult.

(61.) At first the additional friction was applied by the brake-levers, and was measured by the process described in the note to paragraph (19); but it was soon found to be irregular on account of the rusting of the cast-iron disc on which the rubbers pressed. This could not be prevented in the present location of the instrument.

The rust wore off in the course of an experiment and filled the pores of the cloth on the rubbers. Yet more, it became evident that the constants which in the whirling experiments had given V—W pretty fairly, fail totally here: for instance, with the set last given they give $V=14{\cdot}605$, $V'=20{\cdot}066$, the difference being far too great to be caused by any error of the friction.

(62.) I intended to remove the uncertainty caused by the rust by substituting for the iron disc one of bell metal of the same diameter; it is, however, some 20 oz. heavier, and the normal friction of E is now 30·4 grains, and its moment $=27542$. But while it was being prepared it occurred to me that instead of measuring the brake friction first and assuming its permanence during a series of observations it would be better to record and measure it during the entire time of each observation. PRONY'S brake afforded a ready means of effecting this, and was thus applied: a ring of iron an inch deep and $\frac{1}{16}$ inch thick is divided into two semicircles held together by screws tapped in the lugs; when these are removed it can be got on the axle, lowered to the disc, and is made to clamp it with any required pressure by replacing and tightening the screws. The ring has an arm which carries an arc of the same depth concentric with the disc and of 8″ radius. It is obvious that when the anemometer is turning, a cord attached to this arc will be pulled by a force $=$the moment of the ring friction at 8″. This pull is measured by a spring balance which I made with one of the clock springs described in paragraph (26).

(63.) It consists of an iron spindle 6″ long, turning on a small toe below, and above in a brass collar carried by a transverse piece of wood supported on two uprights. It has a projecting piece to which the inner end of the spring is attached by a screw secured by a check nut; the outer end is fixed to one of the uprights. On the top of the spindle is screwed a disc of mahogany 13″ diameter and 0″·5 thick, on the edge of which is turned a groove in which the thread that connects it with the brake arc is wound. On this disc is fitted one of the papers used with my original anemometer, which has its circumference graduated to half degrees, and is covered with circles 0″·05 apart, every tenth one stronger than the rest. By pulling the cord the spindle is turned and the spring tended, the number of its revolutions is shown by a tell-tale fixed to one of the uprights, and the degrees by a pointer.

(64.) It is thus used: tighten the clamp screws so that when the arm is held fast the anemometer shall turn without coming to a stop; pass the cord of the balance through a hole in the remote end of the arc, and tighten it till the increased tension keeps the arm nearly in the same position, then secure it to a pin provided for the purpose. In this state of things it is evident that the tension corresponding to θ, the angle through which the balance has been turned, is the moment of the friction at 8″, from which the moment at the cups is known, to which must be added the normal friction. The brake-ring weighs 14 oz., which would increase the friction a few grains, but this was obviated by hanging an equivalent weight on the relieving apparatus mentioned in paragraph (51). The ring was at first lined with cloth, but as it slightly abraded the bell-metal, I removed it and used the iron surface lubricated with lard.

(65.) The relation between the tension of the spring and θ was thus obtained: the balance being clamped to a table its cord was passed over a pulley; nineteen weights in regular succession from 2 oz. to 36 oz. were hung to it; and to eliminate the effect of friction the disc was turned a few degrees in advance and in rear of the positions of rest, when they were attained, the mean of the θ's was taken as that due to the tension. From ten to thirteen sets were taken for each weight. I had expected that the tension would be very nearly as θ, but with this spring such is not the case; at the beginning $1^\circ = 13$ grains, at 4 rev. it $= 20$, and the change is not uniform; so I formed from the series an interpolation table with θ argument, from which T is easily computed by a formula analogous to that given by STIRLING for equal intervals.

(66.) In carrying out the work I was met by an unexpected difficulty: friction applied in this way is not constant; and I found that in strong breezes (when the wind is always fitful) the arm oscillated more than 90°, the utmost range which the opening of the iron box (paragraph 2) permitted unless the friction was reduced. These oscillations made it necessary to have a record of the tension, which was provided by clockwork moving a pencil from the centre to the circumference of the graduated paper at the rate of 0″·5 per minute. This traces an irregular sector from which the mean θ is easily obtained. But, besides this, it is necessary to reduce the oscillations below 90°, so that they all may be recorded. This was effected by connecting with

the arc an auxiliar balance, so that its action would begin only at the minor limit of the arm's motion, and increase, so as to prevent it from reaching the major limit. It consists of an iron tube 0″·75 diameter, containing 12″·5 deep of mercury. In this is immersed a rod of iron 0″·3 diameter, reaching to the bottom, and with a cross piece at top resting on the tube; from this cross piece descend two wires carrying weights just sufficient to balance the flotation of the mercury. To the top of the rod is attached a cord, passing over a pulley to the arc. It is easily shown that if the rod be raised an inch the cord will be pulled with a force-weight of a cylinder of mercury 0″·3 diameter and 1″·191 high. (For this also I formed an interpolation table, but in it δ^2 is nearly insensible.)

To use it the spring balance is tended till it keeps the arm near the middle of the opening of the box; the arm is then pressed back to touch the box, the cord is looped on the pin already mentioned and shortened till the cross just rests on the tube, and the θ read which gives the zero of the auxiliar. Deducting this from the mean θ, we have θ', the measure of the auxiliar tension.*

The largest oscillation which I have observed under this arrangement was 54°; the wind was moderate, V being only 16^{m}. This is equivalent to a change of tension $=764$ grains at the cups, nearly 0·4 of the entire tension there. I cannot account for the great irregularities of this friction, but I believe similar facts have been observed on a large scale in applying PRONY'S brake to machinery. The extent and frequency of the oscillations do not seem to follow any regular law of V or v, though they evidently are related to them.

(67.) The process described in paragraph (58) gives for each observation an equation containing three unknown quantities, a, x, y, and two unknown variables, V and V′, or $V \pm w$, w being the difference of wind at the two instruments. It is shown by Table XX. that w may be considerable for a few seconds, but when the time is a few minutes it is probably confined within the limits ± 3. Even when (as in the whirling experiments) we know V approximately, and have not V′ to consider, the coefficients are so related that it is impossible to get accurate values of the constants by the usual methods of elimination, and here the difficulty is still greater. I have therefore thought it best to assume probable values for a and y, and determine x so that the mean $V - V'$ may vanish. In the first approximation to this, supposing U the true value of the wind at K, we have $U = V + edx = V \pm w + e'dx$; e being $= \dfrac{V}{\sqrt{z + \frac{\phi}{v^2}}}$.

Hence $S(V' - V) \pm Sw = \Delta x \times S(e - e')$.

* A far better mode of retarding the motion of E would be to have on its shaft a sheave connected with an apparatus like that described in paragraph (6), so that the instrument in revolving might draw up the driving weight. The moment of this at the cups would be constant and accurately known, and the observer would only have to continually unwind the cord. Unluckily, this did not occur to me till the series of Table XXI. was nearly completed; and I was unwilling to repeat the measures, for, owing to deficiency of wind, that series had occupied several months.

If the number of observations be sufficient, $S(\pm w)=0$, and we have $\Delta x=\frac{S(V'-V)}{S(e-e')}$. This will give an x nearer the truth (not exact unless $\frac{\Delta x^2}{n}\times\frac{d^2V}{dx^2}$ be insensible), and a second computation will in general be sufficient. When the constants give the mean $V-V'=0$, the V so obtained must be very nearly $=U$, as shall be shown presently.

(68.) First as to α: in the case of $v=0$, we have the measure of it given in paragraph (27). These must be reduced by some hypothesis as to the action of friction. In the first part of my paper I assumed that the momentum of the cups carried them past the point of equilibrium, induced to this by the small value of α given by min. squares (paragraph 39), $=99$. This gives $\alpha=\frac{T-f}{(V-W)^2}$. My preliminary work with the two instruments showed that this was too small, and I recurred to the more natural supposition that the cups stop when the wind's force $=T+f$. This gives $\alpha=15\cdot315$ at Bar. 30° and Therm. 32°.* For 4″ cups it is 3·357, very nearly in the proportion of the areas. I know no means of determining whether this constant varies with v; the individual measures seem to show that it does not change with V. The lateral pressure on the upper bearing of the shaft causes a resistance as V^2, and will diminish α; but the probable value of its coefficient is $\alpha\times 0\cdot00051$, which may safely be neglected. The change of α, if it exist, cannot have much influence on V; for $\frac{dV}{d\alpha}=\frac{\phi}{2\alpha\sqrt{z+\frac{\phi}{v^2}}}$. Taking I. and X. of Table XXI., where ϕ is a minimum and maximum, we have $dV=d\alpha\times 0\cdot0573$ and $d\alpha\times 0\cdot1603$.

(69.) As to y, if there be no resistance as v^2, except what appears in the resultant, the equation of motion is $V^2+v^2-2Vvx-\frac{f}{\alpha}=0$, from which we see that y, the coefficient of v^2, $=1$. This is its major limit; if we diminish y by Δy, the equality of V and V′ may still be preserved by diminishing x. But the value of V is a little decreased: so the $\Delta V=\sqrt{V^2-\Delta y\times vv'}-V$, or, in the case of K, $-\frac{\Delta y\times vv'}{2V}$. Such diminution can only be affected by an expenditure of power in driving air before the cups, or throwing it outwards; and I tried to find a limit by making them revolve in quiescent air. For this purpose I mounted four forms of E on a vertical spindle driven by HUYGHEN's maintaining apparatus, and noted the time and moment at the cups. The resistance was always more than twice the action of direct wind on the convex sides, and I think its excess may be taken as the extreme possible value of y in the

* I tried to measure it by the spring balance, but the oscillations were too extensive to permit any continuous observations. By noting the time, and counting the turns of K while the oscillations of E_2 were clear of the sides of the box, I got two values of α, 15·165 and 19·562; but the possible difference of wind must be remembered.

negative direction. It would give for $y\,\frac{a-r}{a}$; but I think this action must be small in a current of wind moving with nearly three times the velocity of the cups. It is found to increase as the diameter of the cups and the length of the arms diminish; for E it gives $y=+0{\cdot}0546$, but for E_4 (to be soon described) $-3{\cdot}3406$. This supposition would give smaller values of V; for No. II. of Table XXI., where V with $y=1$ is 35·255, the difference is 1·871, and the true value is certainly between these. I will use $y=1$ as certainly known.

(70.) For x, as K and E_1 are similar and equal, it must be the same in both, and the means of obtaining it are explained in paragraph (67). Here I need only show how its first approximation is got: Supposing $w=0$, we have $2Vvx=\frac{\phi'-\phi}{v-v'}+(v+v')$; but as in K ϕ is small, and may here be neglected, we have $V=v(x\pm\sqrt{x^2-1})$, and the sum of the equation becomes

$$2x(\sqrt{x^2-1}+x)\times Sv=S\frac{\phi'-\phi}{v-v'}+S(v+v') \text{ (VII.)}$$

from which x is easily found. When E is not similar to K the process is simpler: the reading of K gives V, and we have $2xVv'=V^2+yv'^2-\phi'$, whence

$$2x\times Sv=SV+yS\frac{v'^2}{V}-S\frac{\phi'}{V} \text{ (VIII.)}$$

Both these formulæ are defective from omitting w, but are near enough to begin with.

(71.) The following table gives the results of the comparison of K and E_1, which is equal and similar to K. The second column gives the wind's direction; the third log. air's density; the fourth the time in seconds; the two next A and A′, the number of turns made by K and E_1; the seventh log. of $\frac{f'}{a\times D}$ or ϕ'; the two next the velocities of the centres of K and E; the two next the *computed* velocities of the wind; and the twelfth V′−V. V and V′ were computed by the formulæ $V=v(x+\sqrt{z})+\frac{\phi}{2v\sqrt{z}}$; $V'=v'\left(x+\sqrt{z+\frac{\phi'}{v^2}}\right)$ when $z=x^2-1$.

TABLE XXI.

No.	Dir.	L. Dens.	Time.	A.	A′.	Log. ϕ'.	v.	v'.	V.	V′.	V′−V.
			s.								
I.	S.W.	9·97776	176·1	74	51	1·42156	3·601	2·482	10·291	10·030	−0·261
II.	S.W. b. S.	9·97760	391·8	569	511	1·56164	12·444	11·176	35·255	32·471	−2·784
III.	S.W.	9·99124	353·9	226	130	1·88336	5·393	3·102	15·327	14·380	−0·947
IV.	E. b. N.	9·99320	359·9	268·3	170	2·07307	6·389	4·047	18·137	18·294	+0·157
V.	S.W. b. S.	9·97684	334·4	322·8	194	2·25147	8·271	4·971	23·453	22·606	−0·847
VI.	S.E. b. E.	9·98104	278·0	215·9	134	1·97602	6·659	4·133	18·874	17·512	−1·302
VII.	,,	0·98132	330·9	260·7	161	2·21161	6·749	4·168	19·334	20·397	+1·063
VIII.	,,	,,	210·1	189·5	114	2·27685	7·733	4·652	21·932	22·315	+0·383
IX.	S.E.	9·97757	237·0	306·8	192	2·50054	11·092	6·941	31·428	30·812	−0·616
X.	,,	,,	176·1	234·3	153	2·53450	10·446	6·835	29·664	31·249	+1·585
XI.	,,	,,	197·3	213·6	152	2·34007	9·252	6·599	26·228	27·405	+1·177
XII.	,,	,,	154·4	180	122	2·39493	10·102	6·772	28·631	28·762	+0·131
XIII.	,,	,,	131·1	157·3	104	2·41366	10·285	6·800	29·149	28·994	−0·155
XIV.	N.	9·98883	455·3	369·7	211	2·16403	6·957	3·970	19·741	19·363	−0·378
XV.	S.E. b. S.	9·97683	278·6	231·1	160	2·01434	7·108	6·920	20·169	19·441	−0·728
XVI.	,,	,,	259·9	204·4	134	2·10555	6·738	4·417	19·116	19·570	+0·454
XVII.	S. b. E.	9·96418	646·9	639	512	2·13691	8·462	6·780	23·994	25·202	+1·208
XVIII.	S.	9·96469	414·3	209	126	2·01367	4·321	2·605	12·313	14·807	+2·494
XIX.	S.W.	9·96609	684·9	708·6	489	2·26543	8·867	6·119	25·141	25·293	+0·152
XX.	,,	9·96872	404·6	310·3	187	2·10790	6·571	3·960	18·656	18·643	−0·013
XXI.	,,	9·95783	713·5	477·5	251·2	1·90599	5·734	3·003	16·290	14·523	−1·767

These were computed with $x=1·5920$ and $z=1·534$; S $(V'-V)=-0·994$, which being divided by S $(e-e)=164·56$, we have $dx=-0·0060$; $x=1·5860$; $z=1·515$; $x+\sqrt{z}=2·826$; log. 0·45111; limit of $\frac{V}{v}=2·826$.

For K, $V=v\times 2·831+\frac{0·355}{v}$.

It is evident from the values of V′−V that the constants do not change with v or v'; but that their differences are casual owing to the difference of wind at the two instruments. They differ when the v's are nearly equal: For instance, I. and VIII. differ by 2·995; VII. and XIV. by 1·441, and IX. and X. by 2·181; and that such differences of wind may exist for some time is shown by Table XX. where during the first 321 seconds $V'-V=-2·888$, and during the following 325^s it is $-0·784$.*

The minus values predominate during S.W. winds as might be expected from paragraph (56).

This x and z are larger than those given in paragraph (40), namely, $x=1·2282$, and $z=1·340$, which give for the limit 2·286.

This difference is due partly to my having then used an α only two-thirds of what I believe to be its real value, partly to the uncertainty of the frictions employed and of W, and partly to the defect of the method of minimum squares in such a case.

* As these instruments are generally constructed to register $V=3v'$, their readings should be corrected by subtracting 0·056 of the recorded V.

Reducing the first 21 of Table X. by formula XIII., and with my present values of α and y I get $x=1{\cdot}3744$, $z=0{\cdot}889$, and the limit $=2{\cdot}317$.

The W's used in computing these constants were certainly inaccurate. I measured them in the plane of the centre of the anemometer, but as the disturbance of the air will be as $V \mp v \times \sin\theta$, W must be less in the upper semicircle than in the lower, while it acts with less mechanical advantage in lessening $\boldsymbol{v}$. It must also be kept in mind that any measure of W is an average one, and that it may have very different values in parts of the air vortex.

(72.) In E_2, the cross remaining the same the 9″ cups were set at 12″ from the axis; it is my No. III. In it the constant for v' is half that for K's v, and the normal friction is double $=60{\cdot}8$. With the approximate $x=1{\cdot}7481$ and $z=2{\cdot}056$, the results are given in the following Table, in which the densities are omitted as involved in ϕ'.

TABLE XXII.

No.	Dir.	Time.	A.	A′.	Log. ϕ'.	v.	v'.	V.	V′.	V − V′.
		s.								
I.	N.E.	474	108	165	0·67241	0·976	1·491	3·151	5·590	−2·439
II.	N.E.	605·6	313	540·5	0·62808	4·434	3·828	12·637	12·539	+0·098
III.	S.E.	541·7	281·5	505·5	0·64167	4·453	3·998	12·693	13·093	−0·400
IV.	S.E. b. S.	550·9	570	1011·0	0·63633	8·864	7·861	25·135	25·206	−0·071
V.	S.W. b. S.	503·6	203·0	342·0	0·63394	3·453	2·909	9·795	9·853	−0·058
VI.	S.E.	446·2	261·6	453	0·63185	5·0235	4·350	14·298	14·179	+0·119
VII.	S.	601·3	611	1134	0·68300	8·705	8·078	24·686	25·891	−1·205
VIII.	S.	547·1	440·3	752	0·68300	6·895	5·888	19·562	18·971	+0·591
IX.	S.W.	607·8	453·5	782	0·63397	6·348	5·551	18·020	17·779	+0·241
X.	W.	552·1	380	647	0·63127	5·896	5·019	16·754	16·083	+0·671
XI.	S.W.	599·4	133	240	0·63644	1·914	1·727	5·617	6·254	−0·637
XII.	S.W.	475·8	340	576·5	0·63295	6·121	5·190	17·389	16·797	+0·592
XIII.	S.W.	480·8	330	582	0·63966	5·345	5·185	15·201	16·624	−1·423
XIV.	W. b. N.	572·1	192	331·5	0·63283	3·212	2·773	9·212	8·938	+0·274
XV.	N.W.	663·5	314	535·5	0·62821	4·045	3·4495	11·584	11·381	+0·203
XVI.	N.W.	515·2	290	497	0·62623	4·3765	3·674	12·474	12·068	+0·406
XVII.	N.W.	600	248·2	412·5	0·62710	3·544	2·945	10·139	9·845	+0·294
XVIII.	N.W.	660	178	313·8	0·62130	2·311	2·037	6·704	7·127	−0·423
XIX.	S.W.	946·6	488·7	752	0·63008	4·4205	3·401	12·598	11·243	+1·355
XX.	S.	720	354	599	0·63364	4·212	3·564	12·014	11·453	+0·561

$S(V-V')=-1{\cdot}251$ which divided by $Se'=145{\cdot}185$, we have $dx=-0{\cdot}0086$ $x=1{\cdot}7395$, $z=2{\cdot}026$, and the limit $=3{\cdot}163$. These are larger than those of E_1. The results obtained in paragraphs (38) and (39) would make it less, but in the present work it is the rule that diminishing the length of the arms increases x.

(73.) In E_3 the 9″ inch cups were fixed 8″ distance from the axis: too near for good work, but I wished to see the effect on x. With its approximate values $x=2{\cdot}1359$ and $z=3{\cdot}562$, I computed Table XXIII.

TABLE XXIII.

No.	Dir.	Time.	A.	A′.	Log. ϕ'.	v.	v'.	V.	V′.	V−V′.
		s.								
I.	S.	660	348·5	677·5	0·81086	4·524	2·931	12·891	12·349	+0·542
II.	W. b. N.	475·4	131	237	0·80236	2·359	1·423	6·836	6·721	+0·115
III.	W. b. N.	600	238	433	0·80390	3·399	2·061	9·731	9·037	+0·694
IV.	W.	660	192	360	0·80174	2·742	1·714	7·900	7·761	+0·139
V.	S.E.	540	458	940	0·81188	7·267	4·972	20·623	20·424	+0·199
VI.	S. b. E.	646·9	494·5	1011·3	0·81938	6·631	4·972	19·088	20·343	−1·255
VII.	S.W.	334·8	227·3	471	0·80834	5·883	4·063	16·718	16·757	−0·039
VIII.	S. b. W.	600	300·5	599	0·80480	4·291	2·851	12·233	12·032	+0·201
IX.	S.S.W.	600	260	465·5	0·82025	3·713	2·216	10·616	9·629	+0·987
X.	S.S.W.	720	540	1047	0·81604	6·426	4·153	18·251	17·115	+1·136
XI.	S.W.	600	286	593·5	0·81637	4·084	2·825	11·682	11·927	−0·245
XII.	S.W.	600	285	609	0·81150	4·070	2·898	11·596	12·223	−0·627
XIII.	S.W.	420	316	638	0·81497	6·446	4·336	18·307	17·623	+0·684
XIV.	S.	600	466·5	910	0·81140	6·662	4·331	18·915	17·817	+1·098
XV.	W.	720	291	574	0·80694	3·462	2·277	9·910	9·839	+0·071
XVI.	N.W.	600	235·5	445	0·80694	3·362	2·118	9·631	9·255	+0·376
XVII.	S.W. b. W.	511	463·3	959·5	0·79931	7·767	5·363	22·033	21·883	+0·150
XVIII.	S.W. b. W.	509·1	395	883·7	0·79712	6·960	4·957	20·029	20·271	−0·242
XIX.	S.W.	600	350·3	727	0·80133	5·002	3·460	14·235	14·389	−0·154
XX.	S. b. W.	491·9	312·5	751	0·80780	5·443	4·361	15·477	17·937	−2·460

The sum of $V-V'=+1·370$, $Se'=119·53$. Hence $\Delta x=+0·0114$, $x=2·1473$, $z=3·611$ and the limit $=4·047$. The residue is a little too large; but I did not think it necessary to pursue the approximation farther. The great increase of x is remarkable, and I think shows that when the cups are so near the axis of rotation they disturb the regular action of the wind. Even with the 12″ arms this effect is sensible.

(74.) E_4. I now fixed the 4″ cups on the cross at $10''\frac{2}{3}$ from the axis. This arrangement is *similar* to the 9″ cups at 24″, and I thought that the same x might serve for both, but it was far otherwise.

The measures in paragraph (27) give for 4″ cups $\alpha=3·357$ at the normal pressure and temperature. For the first ten observations $f'=35·19$; but as these cups are 35·5 oz. lighter than the 9″ ones, I thought there was too little pressure on the toe, and changed the relieving weight from the 11·5 lbs. to 9 lbs. This made the friction $=68·89$. I computed with $x=2·57$ and $z=5·405$ Table XXIV.

TABLE XXIV.

No.	Dir.	Time.	A.	A′.	Log. φ′.	v.	v′.	V.	V′.	V−V′.
		s.								
I.	S.W.	600	291·5	343	1·04878	4·156	2·177	11·853	11·719	+0·134
II.	S.W.	420	202·5	211	1·04675	4·131	1·946	11·785	10·516	+1·219
III.	S.W.	225·03*	138·6	147	1·05066	5·526	2·488	15·710	13·169	+2·541
IV.	S.W.	588·7	433	554·5	1·05384	6·302	3·587	17·919	19·696	−1·777
V.	W. b. N.	540	156	139·5	1·04918	2·475	0·990	7·159	6·676	+0·483
VI.	S.W.	600	313·5	414·5	1·04366	4·477	2·631	12·750	13·362	−0·612
VII.	S.W.	600	263	306·3	1·04293	3·756	1·941	10·730	10·674	+0·056
VIII.	S. b. W.	780	391·7	449	1·04205	4·303	2·192	12·266	11·796	+0·470
IX.	S. b. W.	600	371·5	449	1·04491	5·305	2·850	15·096	15·314	−0·218
X.	S. S. W.	660	402	499	1·04389	5·219	2·879	14·844	14·969	−0·125
XI.	S. b. E.	840	430·3	471	1·33626	4·389	2·185	12·509	12·575	−0·066
XII.	W.S.W.	532·3	261·1	290·4	1·33929	4·145	2·078	11·824	12·112	−0·288
XIII.	S.W. b. S.	600	420·3	535·5	1·33648	6·002	2·085	17·052	18·201	−1·149
XIV.	S.W. b. W.	600	419·5	503	1·33768	5·949	3·193	17·019	17·117	−0·098
XV.	S.W.	608·1	362·3	408·5	1·33538	5·105	2·558	14·524	14·211	+0·313
XVI.	N.	540	320·5	407	1·32198	5·085	2·870	14·470	15·480	−1·011
XVII.	N.	600	363·5	430	1·32039	5·191	2·729	14·763	14·933	−0·170
XVIII.	N.	600	382·5	493	1·32025	5·462	3·129	15·527	16·741	−1·214
XIX.	W.	666	493·5	512·5	1·33003	6·407	2·957	18·193	15·991	+2·202
XX.	W. b. N.	600	461·6	550·5	1·33073	6·592	3·494	18·714	18·456	+0·258

The sum of V−V′$=+0·948$; $Sc'=83·121$, so $\Delta x=+0·0114$, $x=2·5814$, $z=5·664$, and the limit $=4·961$. This great excess of x above that of K is very remarkable, and shows not only that anemometers must be equal as well as similar to have the same constants, but also that x depends on the diameter of the cups as well as the length of the arms; for here it is greater than in E_3, though the arms are longer.

(75.) E_5. The 4″ cups were fixed as far out on the cross as possible, the distance from the axis being 26″·75; 2·75 greater than that of K, and I expected its x would be less. At first it was mounted on the axle of E, but it moved so much slower than K that I thought its friction $=27·42$ was too much for the small cups. I therefore mounted it on the spindle already mentioned with friction $=4·72$, and with *its* results (VI., XXIII.) computed Table XXV.

TABLE XXV.

No.	Dir.	Time.	A.	A′.	Log. φ′.	v.	v′.	V.	V′.	V−V′.
		s.								
I.	S.	600	547	378	0·94639	7·812	6·016	22·163	21·194	+0·969
II.	S.W. b. S.	600	444·5	292·3	0·94689	6·347	4·652	18·026	16·617	+1·409
III.	W. b. N.	600	302	172·5	0·92562	4·313	2·825	12·293	10·599	+1·694
IV.	N.W.	300	261·8	183	0·92552	7·477	5·825	21·207	20·527	+0·680
V.	S.W.	600	547	387	0·94124	7·811	6·160	22·159	21·674	+0·485
VI.	S.W.	660	646	484	9·97483	8·386	7·003	23·783	24·291	−0·508
VII.	S.W.	660	723·5	561	0·16863	9·3925	8·117	26·627	28·134	−1·507
VIII.	S.W.	600	679	513	0·16820	9·696	8·300	27·485	28·300	−0·815
IX.	S.W.	600	309·8	209·5	0·17272	4·424	3·334	12·609	11·616	+0·993
X.	W.	1200	922	660·5	0·16968	6·583	5·256	18·689	18·269	+0·420
XI.	W.	480	250	162·25	0·16849	4·452	3·288	12·686	11·512	+1·174
XII.	S.W.	600	521	384	0·16846	7·440	6·112	21·110	21·219	−0·109
XIII.	S.W.	600	458	320·5	0·16785	6·540	5·101	18·570	17·736	+0·834
XIV.	S.W. b. S.	600	473	320	0·16788	6·754	5·093	19·174	17·710	+1·464
XV.	S.	600	432	314·5	0·16727	6·056	5·007	17·205	17·412	−0·207
XVI.	S.	600	555	437·5	0·16864	7·925	6·963	22·481	24·450	−1·969
XVII.	S.E.	600	388	302	0·15962	5·542	4·817	15·749	16·720	−0·971
XVIII.	S.E.	600	357·3	268	0·16041	5·103	4·266	14·514	14·870	−0·356
XIX.	. .	1200	908	725	0·16274	6·483	5·770	18·408	20·037	−1·629
XX.	S.W.	669	659	495·5	0·17775	8·555	7·169	21·261	22·902	−1·641
XXI.	W. b. S.	600	417	291·5	0·17795	5·956	4·639	16·920	16·152	+0·768
XXII.	S.W.	600	459·5	322	0·17390	6·561	5·125	18·632	17·833	+0·799
XXIII.	S.W.	600	362·5	264	0·17404	5·176	4·202	14·726	14·645	+0·081

* Time short because it was at the end of the chronograph sheet.

Omitting the first five $S(V-V')=-3\cdot179$, $Se'=269\cdot80$. Hence $\Delta x=-0\cdot0117$ and $x=1\cdot8624$ and $z=2\cdot468$ and the limit $=3\cdot436$. This result surprised me, for the friction was so small that no irregularity of it could have any sensible influence, nor does it seem probable that the pressures on the surfaces of the two sets of cups are in any other ratio than that of the surfaces. The x is actually larger than that of E_2. The five first V''s were computed with the final x. They give V′ rather too small, but in three of them the wind was S.W.

(76.) E_6. I now placed my old anemometer, cups 12″, arms 23″·17, on the axle of E. The α of these cups (if as their area)$=27\cdot227$* and their $f=29\cdot0$. With the second approximation, $x=1\cdot5897$, $z=1\cdot527$, I recomputed the V and V′ of Table XXVI.

TABLE XXVI.

No.	Dir.	Time.	A.	A′.	Log. ϕ'.	v.	v'.	V.	V′.	V − V′.
		s.								
I.	N.W.	600	313	339	0·05547	4·470	4·674	12·737	13·415	−0·678
II.	N.	600	332·5	386·5	0·04264	4·747	5·328	13·519	15·133	−1·614
III.	N.	1200	695	784	0·05223	4·962	5·404	14·122	15·351	−1·229
IV.	N. b. E.	600	240	286	0·04524	3·427	3·943	9·809	11·247	−1·438
V.	N.E.	600	382	378	0·04701	5·455	5·211	15·509	14·806	+0·703
VI.	W. b. N.	600	292	300	0·05411	4·170	4·136	11·896	11·795	+0·101
VII.	S.W.	600	398	368	0·05435	5·683	5·074	16·379	14·404	+1·975
VIII.	S.E.	600	751	783	0·05273	10·725	10·796	30·392	30·464	−0·072
IX.	S.	600	726	751·7	0·05918	8·640	8·636	24·499	24·484	+0·015
X.	S.	660	620	615·8	0·05918	8·048	7·778	22·830	22·014	+0·816
XI.	S.	660	659	679·6	0·05918	8·555	8·517	24·261	24·120	+0·141
XII.	S.	660	502	516·7	0·05918	6·516	6·476	18·506	18·365	+0·141
XIII.	N.E.	600	286	282	0·04772	4·277	3·887	11·652	11·591	+0·061
XIV.	E.	600	340	329	0·04556	4·855	4·535	15·497	12·900	+2·597
XV.	E.	600	358	348·5	0·04833	5·112	4·935	14·454	14·037	+0·417
XVI.	E.	1200	793	860	0·04435	5·662	5·928	16·093	16·890	−0·797
XVII.	N.E. b. E.	600	279·7	294·5	0·04666	3·994	4·060	11·399	11·578	−0·179
XVIII.	N.E. b. E.	600	278·5	277·8	0·04797	3·977	3·830	11·351	10·934	+0·417

It will be observed here, as in Table XX., that v' is sometimes greater, sometimes less than v; the near equality of the constants of the 12″ cups to those of K makes the irregularities of the wind manifest.† The S (V−V′) giving III. and XVI. (double weight) $=-0\cdot649$, $Se'=282\cdot65$, therefore $dx=-0\cdot0023$, $x=1\cdot5874$, $z=1\cdot520$, and the limit 2·8202. This x is a little less than that of K; which shows that the influence of the diameter of the cups is felt even here, overpowering the effect of the shorter ones.

(77.) I shall conclude with a few remarks on the preceding results.

The process by which the x of K is determined seems liable to but two objections:

* In my original paper "On the Cup Anemometer" (Trans. R.I.A., vol. xxii., p. 170) I have mentioned some trials to measure α. As the V's given there were doubtful, I have recomputed them with these constants and the friction of that instrument $=48\cdot61$. The six give for α (at normal pressure and temperature) 27·898, agreeing fairly with that given in the text.

† I may mention here, as further proof of the unsteadiness of the wind, that on one occasion I reversed two cups of this anemometer so that all the convexes were opposed to the wind; I expected they would remain at rest, but they were in continual oscillation through many degrees, so that in the limited area 5′ × 1′ there must have been differences of V able to overcome a friction of 53 grains.

the assumption that $y=1$, and the possibility that the wind errors are not eliminated. As to the first, I have shown in paragraph (69) that it cannot be far astray; even were the extreme diminution of it which is mentioned there to occur, the error for $V=100$ would only be 6·1; but such is not likely to be the case, and $y=1$ may be accepted as a major limit and one not far from the truth. As to the second it is certain that in a sufficient number of observations the + and − errors must balance each other; but it may be a question whether the XXI. of Table XXI. were enough. Still, it is evident, from inspection of the column $V-V'$ that there cannot be any large outstanding residue. I have pointed out the defects of the situation. Could I have had my wish I would have established the instruments on spars 20′ high, erected on a level ground away from any influence of houses or trees, and used a better mode of applying friction to E_1. I also regret that no strong gale occurred during these experiments to verify the formula for a very large v. Under favourable circumstances, I think this mode of determining x the best that is known. I have stated reasons for distrusting the results obtained by the whirling machine, and as yet no unexceptional mode of carrying an anemometer through the air in a right line has been devised. Even could we get a locomotive which could travel without disturbing the air through which it passes (and perhaps the new electric motors might effect this), and a line of rails certainly screened from wind, there would still remain the doubt whether the pressure is the same when a body is moved through a quiescent fluid or a current impinges on the body.

(78.) The results obtained with other anemometers show that x is a function both of R, the length of the arm, and r, the radius of the cups. I subjoin its values.

$R=23·17$	$r=6$	$x=1·5880$
24	4·5	1·5919
12	4·5	1·7463
8	4·5	2·1488
26·75	2	1·8587
10·67	2	2·5798

If we take Nos. 2, 3, and 4 in which the cups are equal, the dependence of x on R is manifest. In No. 3 it is $\frac{1}{10}$ larger than No. 2, and in No. 4 $\frac{1}{3}$. This is partly due to the air's escape before the convexes being less easy as the circle described by them is less. Such a fact is strikingly shown by the whirling experiments (paragraph 69) which I made in search of a minor limit for y, in which I found the resistance =30·61, and 79 for the three respectively. This was in quiescent air; but a similar though much smaller effect must occur in the actual working of an anemometer. Its influence can be obtained only by experiments, such as the present.*

* I thought to test this by removing two opposite cups in E_3. As in this case there is only one cup in each semicircle at a time, the probability of their mutual disturbance was small. A set of ten gave the

(79.) It is more difficult to account for the similar dependence of x on the size of the cups; *à priori*, there seems no reason why small cups should be more resisted than large ones, but such is evidently the case. Unluckily I did not place the 12″ and 4″ cups at the same distances as the 9″, so that the effect of the cups on x is mixed with that of R. I tried to eliminate the latter by interpolating for the values that the 9″x would have at the R's of Nos. 1, 5, and 6, but this could not be done very exactly from the three values. However, this gives x for the 12″ 0·005 less than for the 9″; for the 4″, in No. 5, 0·2894 greater, and in No. 6, 0·7517. The only way in which I can conceive the possibility of such an occurrence is the existence of powers of r and R in the factors, which express the mean effects of wind on the concave and convex surfaces of the cups. In equation III. I suppose the mean v to be that of the centre of the cups, and that the mean impulse and resistance act at these points. But this is not necessarily the case. The effect of the resultant on an element of the cup is (1) as the square of that resultant; (2) as the perpendicular pressure on the element; (3) as the resultant of that pressure perpendicular to the plane of the cup's mouth; (4) as the distance from the axis at which the projection of that resultant meets the arm; and (5) as the magnitude of the element. Of these five factors the first contains v and v^2. Now v as the element $=v\times\frac{(4)}{\mathrm{R}}$ which contains R and r; r^2 also enters the fifth, so that the differential may contain R^3 and r^5. As to the second we are ignorant of its formula, and it is pretty certain that it will depend on powers of the sine and cosine of incidence and (at least for the concave) on the curvature. If we knew its exact form we could integrate the differential which they form and get the impulse and resistance for a given θ, and multiplying this by $d\theta$, and again integrating from 0 to π we should find their mean values. Of the terms in this last integration those which have $\sin^2\theta$ as a factor disappear; πr^2 (the surface) will be a factor of the others, among which *may* be the three first powers of $\frac{r}{\mathrm{R}}$; and these may produce the change of x.

(80.) In paragraph (41) I inferred from the work with the whirling machine that with 9″ cups the x is the same for 24″ and 12″ arms; but what precedes shows that this is not the fact, and that each type of anemometer has a special x. I would therefore suggest to meteorologists and opticians the propriety of confining themselves to two types: one for fixed instruments, the other for portable ones. For the first the Kew type should, I think, be adopted; if the determination of its constants, given in paragraph (70), be not thought sufficiently exact, there would be little difficulty in making more observations like those described there, and under more

$x=2{\cdot}0709$ less than in No. 4, but so large as to make it evident that there must be some other cause of the increased value of x.

favourable circumstances ; but I think they would make very little change in my number. For the portable instrument, the only one of which I have experience is CASELLA'S 3″ cups and 6″ arms, and I found it very convenient : its x should be determined as above. Some such arrangement seems necessary to ensure a uniformity of velocity measures.

ERRATUM.

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Page 1057, at line 20, $\mathrm{S}v\mathrm{T}\times\left(\sqrt{z}+\frac{\vartheta\phi}{2v^2\sqrt{z}}\right)$ is put for $\mathrm{S}v\mathrm{T}\times\left(\sqrt{z}+\frac{\phi}{2v^2\sqrt{z}}\right)$