—'The Old Northwest: with a View of the Thirteen Colonies as constituted by the Royal Charters,' by B. A. Hinsdale, Ph.D., constitutes No. 2 of Mac Coun's Standard Historical Series. 'The Old Northwest' is a guide to the historical facts of State, Federal, and Inter-State legislation in connection with their formation, development, and admission into the Union. —— P. Blakiston, Son, & Co. have just published a second edition of 'Medical Jurisprudence and Toxicology,' a text-book for medical and legal practitioners and students, by John J. Reese, M.D.; and 'The Physician's Visiting List for 1889,' being the thirty-eighth year of Lindsay and Blakiston's 'Physician's Visiting-List.'

LETTERS TO THE EDITOR.

Anemometer Constants.

THE last volume of the *Repertorium für Meteorologie* (Vol. XI. No. 7), just received, contains a paper by Dubinsky ('Vergleichende Verification zweier Anemometer in Hamburg, Deutsche Seewarte, und in St. Petersburg, Physical Central-Observatorium'), giving the results of comparative tests of two Robinson anemometers of very small dimensions, and using for this purpose the two whirling-machines respectively at Hamburg and St. Petersburg.

These experiments are of special interest to the writer, who was himself engaged during the past summer upon similar work for the Signal Service, and used, with the larger anemometers of the service, a very small one for studying certain parts of the problem. In this work the whirling-machine was very large, having an arm twenty-eight feet long, which in later experiments was increased to thirty-five feet. It is not intended at this time to speak further of this work, but to notice in a few remarks the method (pp. 11 *et seq.*) used by Mr. Dubinsky to ascertain a very important correction, and to compare his results with those obtained by Dohrandt (*Rep. für Met.*, Vol. IV.–Vol. VI.), who had already used the St. Peterburg machine in making a large number of experiments upon anemometers of the ordinary sizes.

The two whirling-machines, which are permanently set up in closed rooms, are nearly the same in size; that at St. Petersburg being much like a letter T in form, and adapted to be revolved about the central stem as an axis, carrying the anemometer to be tested on the outer end of one or the other of its horizontal arms, which are about eleven feet long. In the Hamburg machine one arm is quite short, and carries a counterpoise; the other is between twelve and thirteen feet long.

In using such whirlers, there is a tendency of the arm and other moving parts to set up a slow rotation in the air, as a whole, through which they revolve. This movement of the air with the arm is called by the Germans, and aptly so, the *Mitwind.* The determination of its amount is one of the most serious obstacles to overcome in experiments of this kind.

Results seem to indicate a pretty close proportionality of this *Mitwind* to the velocity of the arm; and Dohrandt concluded from his studies that in value it was about 5 per cent of the latter. Dubinsky, working with relatively very much smaller anemometers, though using the same whirling-machine, adopts 7.3 per cent as the correction for the *Mitwind.* The discrepancy in these results is really larger than it appears, when it is considered how much less the small anemometers would tend to generate *Mitwind*, as compared with those used by Dohrandt.

A brief description of the method of measuring the *Mitwind* will aid in understanding the question. For this purpose both experimenters placed close to the path of the whirled anemometer a delicate air-meter, with its axis tangent to the orbit. Its indications during the progress of an experiment give a measure of the *Mitwind*, however, being strongly acted upon by the violent disturbance of the air which immediately attends and follows just after the passage of the whirled anemometer, and which cannot be considered as a true *Mitwind.* The velocity given by the air-meter is no doubt, as Dohrandt points out, much greater than that of the true *Mitwind.*

The treatment by Dubinsky, of this observed velocity, to reduce it to the *Mitwind* velocity, is practically the same, at least in intent, as the expedient resorted to by Dohrandt (*Rep. für Met.*, Vol. IV. No. 5, p. 39), who placed on the end of the unoccupied arm of the whirler a small air-meter, which was thus carried in the path of, but diametrically opposite, the whirled anemometer. The whirling-machine is revolved, first with both anemometer and air-meter in position, and then with the air-meter alone. Owing to a decrease in the *Mitwind* attending the removal of the anemometer, the whirled air-meter registers a larger number of units in the second case than in the first; and the difference, in terms of velocity, is considered by Dohrandt as the *true value* of the difference between the *Mitwinds* in the two cases. Not questioning the correctness of this assumption, a comparison of the difference thus obtained with that derived from the indications of the stationary air-meter shows the latter to be from two to three times the former or presumed true difference. Finally, it is further assumed that the whole observed *Mitwind* and the true are in the same proportion. Or, if v_1 and v_2 are the velocities indicated by the air-meter when whirled with and without an anemometer, and x_1 and x_2 the corresponding velocities of observed *Mitwind*, we have, the velocity of the arm being the same in both cases,

$$\frac{v_2 - v_1}{x_1 - x_2} = a;$$

and the true *Mitwind* is $a \times$ observed *Mitwind.*

In applying this method, Dubinsky whirled both of the small anemometers, one on each end of the arm, and then one alone, using the stationary air-meter for observing the *Mitwind* in each case. This substitution of the small anemometer — an instrument equally influenced by equal winds in a horizontal plane, whatever their direction — for an air-meter not thus influenced, is an important modification of Dohrandt's method, and may serve to account for a part, at least, of the difference found in their results. Dubinsky has, apparently without being aware of its peculiar merits, hit upon what is believed to be a more proper method of investigating *Mitwind* than any heretofore used: that is to say, the *Mitwind* anemometer must be of the same form as the anemometer being tested, as it is evident the instrument used for measuring the *Mitwind* must be influenced thereby in the same manner, and to the same extent, as the instrument whose constants are being determined.

Throughout the tests upon the small anemometers the *Mitwind* was carefully observed by means of a stationary air-meter, and 7.3 per cent of the arm-velocity was adopted as its value at St. Petersburg, 7.6 per cent being the value found at Hamburg. A single experiment only is cited, by which the value of the factor *a* was determined, and is as follows:—

	v_0 Velocity of Arm. Kilometres per Hour.	c Contacts per Hour of Anemometer No. 74.	x Mitwind. Kilometres per Hour.
With both anemometers	62.24	60.04	4.91
With No. 74 alone	62.56	60.56	4.68

The paper further states that in the second case, had the velocity of the arm been 62.24 instead of 62.56, the recorded contacts of No. 74 would have been 60.27. Hence we have

	v_0	c	x
	62.24	60.04	4.91
	62.24	60.27	4.68
Differences		0.23	0.23

The author, apparently too hastily, jumps at the conclusion, and places

$$\frac{0.23}{0.23} = 1.00$$

as the value of the factor a, and in consequence applies all of the 7.3 per cent observed *Mitwind* as the correction for that disturbance. It is to be observed that the first 0.23 in the line of differences is in terms of *contacts per hour*, and is not a velocity. Further uncertainty arises in that the value 60.27 seems itself a little abnormal.

Referring to the equation found for this anemometer on this machine, we have,

$$v = 0.910 + 1.02729c - 0.00076c,$$

from which, when $c = 60.4$,

$$\frac{dv}{dc} = 0.935.$$

Using this co-efficient to reduce the contacts observed in the second case to those corresponding to the velocity 62.24 of the first case, we have 60.22 contacts as the number per hour. To be accurate, the observed *Mitwind* in the second case should also be reduced to the velocity of the first case. Preserving its proportionality to the arm-velocity, we find its value to be 4.67 kilometres per hour. Hence it seems we should have

	v	c	x
	62.24	60.04	4.91
	62.24	60.22	4.67
Differences		0.18	0.24

Using the co-efficient 0.935 to reduce contacts per hour to kilometres per hour, we have corresponding to 0.18 contacts per hour a velocity of 0.17 kilometres per hour. Hence finally,

$$\frac{v_2 - v_1}{x_1 - x_2} = \frac{0.17}{0.24} = 0.71 = a.$$

The true *Mitwind*, according to this value, would be 5.2 per cent, — a value practically the same as that found by Dohrandt; namely, 5 per cent. Considering that the latter value applies to much larger anemometers than the former, the still outstanding difference is probably due to the point already noted, that in the recent experiments a Robinson anemometer, and not an air-meter, was used. Further light would no doubt be thrown upon the question of the value of the *Mitwind*, if experiments were made in which the stationary air-meter is replaced by a small and very sensitive Robinson anemometer.

It is hardly probable that the *Mitwind* is strictly tangential to the path of the whirled anemometer: indeed, the writer has observed a marked tendency to a spiral motion of the air and air-meters with their axes tangent to the circular paths of the end of the whirling arm, — get, as it were, only the tangential component.

These considerations, it would seem, throw more or less doubt upon the accuracy of the *Mitwind* corrections as obtained by both experimenters, though in each case the results agree very well among themselves. Unfortunately Mr. Dubinsky does not give the numerical relations between the 'contacts' and the revolutions of the cups, by which it becomes possible to make comparisons with anemometers of different construction in this respect.

C. F. MARVIN.

Washington, D.C., Nov. 12.

A Telescope for the New Astronomy.

As we become accustomed to celestial phenomena, we find a large number of faint appearances, upon the interpretation of which our knowledge of the forces at work depends: for instance, the detection of the carbon atmosphere surrounding the sun, foreshadowed by Archimis in 1875 by the detection of the bright carbon band in the blue in the spectrum of the zodiacal light, inferred by Lockyer in 1878 from a comparison of the solar and electric arc spectra, indicated also by the observations of Schuster at Sohag and by Abney in 1881, and finally worked out line by line by the large instruments and photographic methods of Rowland; or, again, the faint bright lines detected in the spectrum of many stars, affording new ideas both as to the cause of the variability of the stars' light and the classification of stellar spectra as worked out from the study of meteorites by Lockyer. We find also that we are not dealing with constant things: change and change again are the only law. As the gravitational astronomer reaches his conclusions by following the changing positions of the heavenly bodies, so the physical astronomer must watch its ever-changing appearance. Recall to mind the discussion over the well-known comet spectrum, one astronomer averring from personal observation, deserving great respect, that the line belonged to the carbonic-oxide spectrum, while his rival assured us from equally trustworthy sources that it was nothing if not hydrocarbon. Science to-day tells us both were right, a slight change in the density of the gas being sufficient to change the spectrum from one to another. Our knowledge is therefore far from complete till we have substituted the series for the single observation.

But the human eye and the human brain are not sufficient — nay, are sometimes misleading — when complete and accurate detail are desired. Our attention is attracted by the points raised by the current theories of the day; and much is left unnoticed, or, if sought, is missed because one did not know where to look. The history of the discovery of the solar prominences, easily seen, after discovery, by the same observer, using the same telescope with which he had previously been unable to discover their existence, presents an example. Photography to-day supplies a remedy. In the hands of a master skilled both in the manipulation of the emulsion and the dye, its effect is not slight; its advantage, much the same as a balloon would give the voyager in the frozen seas, — showing at a bird's-eye glance what years of travel could not show. By it we may carry our best telescopes and our best seeing into every home and school-room; forming in his very youth the astronomer of the future, who shall work without telescope or observatory; rendering him familiar with those appearances which, not so many years ago, enchained his ancestors.

From its scientific side, of what great value has the chance delineation of the tails of comets been in the hands of Bredechin?

It is not every telescope which is fitted to this end. It must be pre-eminently a light-gatherer, which demands a large-sized object-glass, with the attendant mounting, and yet possess the ease and accuracy of motion of a sylph. It must be of great length, — thirty-five or forty feet, — and yet from end to end have no mass of metal which could produce an air-tremor. Yet such an instrument — the El Dorado of full many an astronomer — to-day grows in the workshop of an English astronomer.

Seven feet in diameter, and of great thickness, is the reflecting mirror; forty feet, its length from end to end. Yet tube it has practically none. Tons in weight, it follows the steady pressure of your little finger. Pedestal it has none, but floats upon its polar axis like a large warship; this polar axis being little else than a large boiler, so arranged, that, "should it be thrown into the sea in a given latitude, it would still point its axis to the pole."

With an instrument of less than a fifth its power, Common's well-known photograph of the nebula in Orion was taken. With one of less than a seventh of its capacity, the nebula in the Pleiades was discovered. The wonders reported from Mount Hamilton show us what we may expect it to disclose.

The instrument is secured to us through the *esprit* of a well-known astronomer. Is it not possible that among the readers of your journal may be found many who would contribute something towards its endowment? Of all our observatories, there is none that is popular. Can we not make the largest glass the world has yet seen popular?

S. O.