son's cups. The personal equation, under the circumstances of the observations, was found as follows:—A gun was fired at such a distance from the Observatory as to be heard with about the same degree of distinctness as the time-gun at the Castle. This distance was found to be 1483 feet. The registrations on the chronograph were made by Mr. Kirby at the distance of 162 feet from the gun, and Mr. Mann at the Observatory. For this comparatively small distance, the time of transit calculated from the velocity deduced from the time taken to travel over the larger distance may be deemed exact. The observed time for the smaller difference of distance was found to be too great by $0^s{\cdot}09$, which correction has been applied to all the observations. It depends more on want of sensibility in picking up and recognizing faint sounds than upon mere habit of making contacts. When the observers were interchanged, the observed interval of time appeared still too large, but in this case by $0^s{\cdot}02$. It is clear that such personal equations are not eliminated by an interchange of observers, nor by return signals.

In the reduction of the equations, the coefficient of elasticity of air under a constant volume (that is to say, the ratio of the increment of pressure for an increment 1° F. of temperature to the pressure at 32° F.) was regarded as an unknown quantity as well as V, the velocity of sound at 32° F. The reduction of the equations furnished by the observations, which were 38 in number, gave

$$V = 1090{\cdot}6 \text{ feet per second,}$$
$$\alpha = 0{\cdot}0019,$$

Regnault's value of α being 0·0020.

There appeared to be but little difference between the residual errors as dependent on the motion of the air. The author grouped the residuals into two classes, according to the dampness of the air; but there appeared to be no appreciable difference in the velocity as dependent upon dampness.

IV. "On a supposed alteration in the amount of Astronomical Aberration of Light, produced by the passage of the Light through a considerable thickness of Refracting Medium." By George Biddell Airy, C.B., Astronomer Royal. Received November 17, 1871.

A discussion has taken place on the Continent, conducted partly in the 'Astronomische Nachrichten,' partly in independent pamphlets, on the change of direction which a ray of light will receive (as inferred from the Undulatory Theory of Light) when it traverses a refracting medium which has a motion of translation. The subject to which attention is particularly called is the effect that will be produced on the apparent amount of that

angular displacement of a star or planet which is caused by the Earth's motion of translation, and is known as the Aberration of Light. It has been conceived that there may be a difference in the amounts of this displacement, as seen with different telescopes, depending on the difference in the thicknesses of their object-glasses. The most important of the papers containing this discussion are:—that of Professor Klinkerfues, contained in a pamphlet published at Leipzig in 1867, August; and those of M. Hoek, one published 1867, October, in No. 1669 of the 'Astronomische Nachrichten,' and the other published in 1869 in a communication to the Netherlands Royal Academy of Sciences. Professor Klinkerfues maintained that, as a necessary result of the Undulatory Theory, the amount of Aberration would be increased, in accordance with a formula which he has given; and he supported it by the following experiment:—

In the telescope of a transit-instrument, whose focal length was about 18 inches, was inserted a column of water 8 inches in length, carried in a tube whose ends were closed with glass plates; and with this instrument he observed the transit of the Sun, and the transits of certain stars whose north-polar distances were nearly the same as that of the Sun, and which passed the meridian nearly at midnight. In these relative positions, the difference between the Apparent Right Ascension of the Sun and those of the stars is affected by double the coefficient of Aberration; and the merely astronomical circumstances are extremely favourable for the accurate testing of the theory. Professor Klinkerfues had computed that the effect of the 8-inch column of water and of a prism in the interior of the telescope would be to increase the coefficient of Aberration by eight seconds of arc. The observation appeared to show that the Aberration was really increased by 7″·1. It does not appear that this observation was repeated.

A result of physical character so important, and resting on the respectable authority of Professor Klinkerfues, merited and indeed required further examination. Having carefully considered the astronomical means which would be most accurately employed for the experiment, I decided on adopting a vertical telescope, the subject of observation being the meridional zenith distance of γ Draconis, the same star by which the existence and laws of Aberration were first established. The position of this star is at present somewhat more favourable than it was in the time of Bradley, its mean zenith-distance north at the Royal Observatory being about 100″ and still slowly diminishing. With the sanction of the Government, therefore, I planned an instrument, of which the essential part is, that the whole tube, from the lower surface of the object-glass to a plane glass closing the lower end of the tube, is filled with water, the length of the column of water being 35·3 inches. The curvatures of the surfaces of the two lenses constituting the object-glass, adapted, in conjunction with the water, to correct spherical and chromatic aberration, were investigated by myself and verified by my friend Mr. Stone (now Astronomer at the Cape Observatory). The

micrometer is constructed on a plan arranged by myself, by which the double observation in reversed positions of the instrument can be made with great ease. The reference to the vertical is given by two spirit-levels, both to be read at every single observation. The work of construction was intrusted to Mr. James Simms, who carried it out with great ability. Distilled water was supplied by H. W. Chisholm, Esq., Warden of Standards.

Had the result of the observations been confined to the determination of an astronomical constant, or the variation of its value for different telescopes, I should not have thought it worthy of communication to the Royal Society. But it is really a result of great physical importance, not only affecting the computation of the velocity of light, but also influencing the whole treatment of the Undulatory Theory of Light. In this view I have thought that an informal statement of the conclusions may be acceptable to the Society, reserving for publication in one of the annual Greenwich Volumes the details of the observations.

The instrument was mounted in a small Occasional Observatory first constructed for the transit-instrument of Mr. Struve when he was engaged in determining the longitude of Altona, and now planted on the "South Ground" of the Observatory. The seasons at which the meridional zenith-distance of γ Draconis is most affected by aberration in opposite directions are the Equinoxes.

For understanding the following Table, it is to be remarked that an apparent value of the Geographical Latitude of the Instrument is formed from every observation, by subtracting the Observed Instrumental Zenith-distance North of the Star from the Tabular Declination of the Star given in the 'Nautical Almanac.' The observed zenith-distance is affected with the True Aberration as seen in the instrument, the tabular declination is affected with the Received Aberration used in the computation of the 'Nautical Almanac,' and the apparent value of the geographical latitude is therefore affected by the difference between the True Aberration as seen in the instrument and the Received Aberration. If, therefore, under all circumstances, and especially in the comparison of days when the sign of aberration has changed, the apparent value of the geographical latitude is sensibly constant, it proves that the True Aberration is the same as the Received Aberration, or at least that one is not a multiple of the other.

The last column of the Table is given only to show to how large an extent Aberration enters into the star's Apparent Declination.

Every result for Observed Zenith-distance in the Table is the mean of observations in reversed positions of the instrument.

Day of observation.	Star's Observed Zenith-distance North.	Star's Declination from 'Nautical Almanac.'	Difference for Geographical Latitude of Instrument.	Correction for Aberration adopted in 'Nautical Almanac.'
1871.	″	° ′ ″	° ′ ″	″
Feb. 28	85·30	51 29 59·3	51 28 34·0	−18·71
March 1	85·71	59·1	33·4	18·82
3	84·19	58·9	34·7	19·02
4	82·18	58·8	36·6	19·11
16	83·63	58·0	34·4	19·73
17	84·58	58·0	33·4	19·74
21	83·87	57·9	34·0	19·73
23	82·73	57·9	35·2	19·69
24	84·18	58·0	33·8	19·66
26	84·04	58·1	34·1	19·59
27	83·48	51 29 58·2	51 28 34·7	−19·54
Mean Latitude of Instrument from Spring Observations			51 28 34·4	
Aug. 29	122·10	51 30 34·4	51 28 32·3	+18·25
Sept. 5	121·84	35·0	33·2	19·01
7	121·62	35·1	33·5	19·18
9	120·27	35·2	34·9	19·33
11	122·98	35·3	32·3	19·45
15	122·20	35·4	33·2	19·64
17	121·53	35·5	34·0	19·70
22	121·38	35·5	34·1	19·74
24	120·01	35·4	35·4	19·72
Oct. 1	120·62	35·1	34·5	19·46
2	120·29	35·1	34·8	19·40
3	121·31	35·0	33·7	19·33
4	124·41	34·9	30·5	19·26
6	120·60	51 30 34·8	51 28 34·2	+19·10
Mean Latitude of Instrument from Autumn Observations			51 28 33·6	

Remarking that the mean results for Geographical Latitude of the Instrument (determined from observations made when the Aberration of the star had respectively its largest + value and its largest − value) agree within a fraction of a second, I think myself justified in concluding that the hypothesis of Professor Klinkerfues is untenable. Had it been retained, the Aberrations to be employed in the corrections would have been increased by + 15″ and − 15″ respectively, and the two mean results would have disagreed by 30″.

The latitude of the instrument from these observations is about 51° 28′ 34″·0. The position of the instrument, as measured on the Observatory Map, is 340 feet south of the Transit-circle, a spatial distance corresponding to about 3″·35. The latitude of the Transit-circle being taken at 51° 28′ 38″·4, the geodetic latitude of the instrument is 51° 28′ 35″·05, an agreement closer than I expected, consideration being given to the form of the ground. It appears very probable that at the place of the Transit-

circle, on the north brow of the hill, the zenithal direction is disturbed towards the north and the astronomical latitude is too great.

There is only one point in this investigation upon which a doubt can be suggested as possible, namely the evaluation of the micrometer-scale. It was thus conducted :—The micrometer-plate contains 26 wires, and the fixed part of the instrument contains 25 crosses, each interval being nearly 256″. With this arrangement every wire-interval is measured with great ease, and the whole series of 25 intervals is accurately obtained in terms of the micrometer. By placing the instrument in a proper position, the same intervals are obtained in time of the star's transit, which is easily converted into arc. The comparison of these gives the value of micrometer-divisions which has been employed.

The following verification, of somewhat inferior accuracy, has been made by measures of the instrument. It appears that the ray of light passes through 0·9 inch of glass, 35·3 inches of water, and 0·8 inch of air, nearly (the measure of the last being slightly uncertain). Remarking that the dividing surfaces are horizontal and plane, it is easily seen that the micrometer-scale ought to be such as is due to an air-telescope whose length in inches $=\frac{0 \cdot 9}{1 \cdot 6}+\frac{35 \cdot 3}{1 \cdot 336}+0 \cdot 8=27 \cdot 8$ inches. And from this, with observation of transit of the star, it was found that the measure of 25 intervals of wires ought to be 0·8693 inch: as measured with a pair of compasses, it was found sometimes 0·871, sometimes 0·875. The agreement is fully as close as can be expected from the rudeness of the operation, and shows distinctly that there can be no error of principle in the method of evaluing the micrometer-scale.

V. "Magnetic Survey of the East of France in 1869." By the Rev. S. J. Perry and the Rev. W. Sidgreaves. Communicated by the President. Received July 13, 1871.

(Abstract.)

This paper contains the results of a series of magnetic observations taken in the east of France during the months of August and September 1869, and is a continuation of the paper on the survey of the west of France, published in the Philosophical Transactions for 1870, p. 33.

No change was made in the observers, nor in the methods of observation, during the two surveys; and the only alteration in the instruments was the substitution in 1869 of a Jones theodolite in lieu of the small altazimuth by Cook used in 1868.

Observations were made at twenty-one stations in the following order:— Paris, Rheims, Metz, Strasbourg, Issenheim, Dôle, Mont Rolland, Dijon, Lyons, Avignon, Marseilles, Monaco, Montpellier, Grenoble, N. D. de