ivory circle. The needle, previous to beginning to count the vibrations, was drawn 50° or 60° from the magnetic meridian by another needle, and left to oscillate. When it had reduced its arc of vibration to 30°, the counting of its vibrations was commenced, and terminated at 5°. It usually took between 300 and 400 vibrations to reduce the arc of vibration to this limit, occupying from 12 to 16 minutes. Four of the needles, with an apparatus in duplicate, were sent to the author from Professor Hansteen of Christiana, to be employed in comparative experiments in various parts of Great Britain. They were vibrated in Edinburgh by Captain Basil Hall and Lieut. Robert Craigie, and the results are set down with the rest in this paper. The needles being returned, were also used in the experiments between Paris and London. The two remaining needles were made by Dollond, of the same size and form as Professor Hansteen's. The author then relates his experiments, which were made on the 3rd of December, about seven weeks previous to his departure for Paris, in the garden of the Horticultural Society at Chiswick; and on the 15th of January, at Thornfold Park, near Tunbridge Wells; and on the 30th of January, in the garden of the Royal Observatory at Paris.

An opportunity occurring, three of the needles were sent to England early in April, and, with one sent by Captain Hall from Edinburgh, were vibrated by Captain Chapman, R.A., in the garden of the Horticultural Society, and returned to Paris.

Professor Hansteen's four needles were always kept separate, but those by Mr. Dollond together, and nearly in contact. To try the effect of separation, these were separated from the 14th of March to the 30th of April, and being then again tried in the same place as before, their times of vibration were found unaltered.

The author then states, in the form of an abstract, the results of the several experiments, all the details of which are subjoined in the form of tables. The mean of all gives a ratio of horizontal directive force in Paris greater than in London, in the ratio of 1068 : 1000; and on the supposition that the dip in London is 69° 45′, and in Paris 67° 58′, the ratio of the intensity of directive force on the dipping-needle comes out greater in London than in Paris, by about 15 parts in 1000.

On the Resistance of Fluids to Bodies passing through them. By James Walker, *Esq. F.R.S.E. Communicated by* Davies Gilbert, *Esq. M.P. V.P.R.S.* Read May 31, 1827. [*Phil. Trans.* 1828, *p.* 15.]

The object of this paper is to explain a new mode of measuring the resistance of fluids, which has of late become more than formerly an object of research owing to the introduction of steam navigation.

The resistance of a fluid *per se*, is, theoretically speaking, as the square of the velocity; but, independently of friction and viscidity, this theory is only applicable to the case of a body entirely and deeply immersed. If it float on the surface (as a boat), the elevation of the water in front, and its depression behind, disturbs the exactness of

this law, and renders theory inapplicable; while experiment, as the author remarks, has not yet supplied the defect.

The author devotes the first part of his paper to a consideration of the experiments made by Bossut and other members of the French Academy in 1776, 1778, and by the London Society for the Improvement of Naval Architecture in 1793 and 1798. Both these sets of experiments he regards as inconclusive, partly from the small size of the floating body and the small velocities used, but chiefly from the inadequacy of the means of measuring the actual resistance, and the sources of error arising from the rigidity of the cords used, the friction of pulleys, and that of the line itself dragged through the water, the moving power being a weight suspended and descending uniformly.

In the experiments which form the object of this paper, all these sources of error (which, in some cases, amounted to three times the resistance to be measured,) were avoided by the simple contrivance of estimating the strain exerted on the boat, at every instant, by a spring weighing machine immediately attached to it, through which the tension of the cord was of course transmitted, and which measured the actual tension exerted in overcoming the resistance of the boat, unmixed with any of the other causes of the destruction of power.

The apparatus employed is illustrated by drawings. The experiments were made in the East India Import Dock, whose size and depth are such as to allow no resistance arising from the sides or bottom of the dock. The boat being drawn at each experiment over $\frac{2}{10}$ths of a mile, the time of passing over $\frac{1}{10}$th was carefully noted, and the tension or resistance read off and registered every two seconds. The velocity was preserved uniform by applying the power of men turning a barrel in measured time by the swing of a pendulum.

Four sets of experiments were made on boats of 18 and 28 feet in length, variously loaded, and on a Thames wherry, with velocities from 2 to $5\frac{1}{2}$ miles per hour; and the conclusion from them all is, that the resistance increases in a higher ratio than as the square of the velocity.

Mr. Walker concludes this paper with a comparison between the effect of moving power applied on a rail-road and on a canal, which from these experiments appears to be reduced to equality at lower velocities than if the resistance to the boat were as the square of the velocity.

On the Corrections in the Elements of Delambre's *Solar Tables required by the Observations made at the Royal Observatory, Greenwich. By* George Biddell Airy, *Esq. M.A., Fellow of Trinity College, Cambridge, and Lucasian Professor of Mathematics in the University of Cambridge. Communicated by* John Frederick William Herschel, *Esq. V.P.R.S.* Read December 6, 1827. [*Phil. Trans.* 1828, *p.* 23.]

The author was desired by the Board of Longitude to examine the discordancies between the right ascensions of the sun, as observed at