From a close examination of the twenty-eight principal experiments made in Holland in 1823, Professor Miller has, in the Philosophical Magazine for July 1839, deduced the following as the correct means of the results. The mean interval of the time in which sound travelled 17,669·28 metres is 51∙9873 seconds. The mean temperature during these experiments was 11°∙01 cent. The mean pressures of the atmosphere, and of the vapour in it, were∙74018 and ∙00889 metres at 0° cent. respectively. Now, from Rudberg's experiments (Poggendorff’s *Annalen,* xli. 558 and xliv. 119), which perhaps require to be repeated by others, it appears that on heating dry air from 0° cent. to 100°, under a constant pressure, its volume was increased from 1 to 1·365; hence, at the temperature 6 cent., the pres sure of the atmosphere and of the vapour in it being *p* and *f* respectively, the velocity of sound in English feet per second will be

∕z1 + ∙003650v

10θ°wl ΓΞW )

*P*

This, owing to its depending on a smaller rate of expan sion, exceeds 1089∙5, the number in the table ; but it is chiefly to be regarded as a general expression for the experimental results. The theoretical formula will be found farther on.

With the exception of Benzenberg, who had long ago used the species of clock described below with a conical pendulum, the most of the experimenters were formerly very ill provided with any adequate means of accurately measuring the intervals of time which elapse between the flash and report of the guns, as observed at the opposite station. In 1822, the French academicians employed the stopwatch of Breguet and the chronograph of Ricussec, a species of watch, one of whose hands revolves in one se cond, and can, without needing to stop, be made to touch with its extremity the dialplate at any instant, so as to leave there a dot of ink, merely by suddenly pressing a small lever. In the experiments made in Holland in 1823, a clock was used with a conical pendulum which revolved 69∙433 times in a minute, and was supposed capable of determining intervals to the 100th of a second, by suddenly arresting the index without stopping the clock. By means of these machines it was supposed quite practicable to determine the interval between the sight of the flash and the arrival of the report of a gun, with such precision as to obviate all material error which might arise from this cause. The importance of this, if realized, is evident from the circumstance that each tenth of a second corresponds to 110 feet of distance.

But there is too good reason to suspect that all such ideas of attaining great accuracy in any direct measurements of the intervals of time are perfectly illusory, and that observations made with these machines can scarcely be depended on within a tenth of a second. The machines themselves may be possessed of all the perfection ascribed to them ; but it would seem that the man has yet to be made who would be competent to use them. In Holland, only one centrifugal clock being employed at each station, we have no means of judging of the correctness of the observations made with it. But whoever examines the tables of results given in the *Connaissance des Terns* for 1825 (pages 364—367) will find that three and as often four machines, with as many first-rate observers, were generally employed simultaneously in determining the interval for each shot ; and that instead of their results agreeing within an inconsiderable fraction, as the above ideas of great correctness would imply, they often differ by three, sometimes by four, and in one case by five tenths of a second. Some thing farther illustrative of this will be found under the arti cle Clock and Watch Work (vol. vi. p. 783), where a ma­

chine has been described for determining, by indirect means, but to almost any degree of exactness, the interval in which sound passes over a small distance. The principal part is a strong clock, without compensation or other refinement, and requiring no extraordinary sort of being to observe with it.

Although many eminent philosophers have laboured] much to improve and perfect the various reductions which ‘ it is considered necessary to make on the experimental results, there is still a mistake regarding the exact influence of the wind. It seems always to be supposed that the ef­fects of a steady wind are entirely obviated or compensated by taking the mean of the velocities of such sounds as are simultaneously produced at the two opposite stations, and also reciprocally observed at these respectively. This is in effect the view of the matter taken by Professor Moll. (Philosophical Transactions for 1824, p. 426.) Many other philosophers express the same opinion, and in particular Sir John Herschel, who recommends that all experiments on the velocity of sound be made, if possible, either in calm weather or in a direction at right angles to that of the wind. But so far is the result under this last arrangement from being entirely unaffected by the wind, that the error to which we now allude is then a maximum; and indeed in every case in which the direction of the wind is inclined to the base line, there is still a small error, which cannot be obviated by the reciprocal method.

The earlier experimenters gave themselves no concern about the influence of the wind ; but its effect was in some degree eliminated by the arrangements adopted by the French academicians in 1738, though in so imperfecta manner as to leave c∙nsiderable doubt regarding the accuracy of their result. In order to clear up this point, the Marquis Laplace requested the board of longitude to repeat the experiments, with the precaution of exciting sounds more nearly at the same instant from both ends of the base, in the expectation that the effects of the wind would be there by completely obviated. The experiments were accordingly repeated in June 1822, though with no greater precision in this respect than that the times of firing the oppo site shots still differed about five minutes from being simultaneous. In the experiments made in Holland in 1823, matters were so much better arranged, that the difference in the times scarcely ever amounted to a second. How ever, M. Arago has remarked, that when the wind is very unsteady, or comes in sudden gusts, it may still affect the mean result, especially since a sudden gust may interfere with the one sound, and yet miss the other altogether. But even when the wind is perfectly steady, the reciprocal method cannot be quite correct, unless the wind also blows directly from the one station to the other. In every other case it is more or less inaccurate, owing to the circumstance that sound is not propagated in parallel lines, but issues from its source in lines which radiate or diverge to every side ; so that the same sonorous ray which, during a calm, would pass directly from the one station to the other, will not reach the latter at all when deflected by the wind into a different direction. Had our limits permitted, we should have endeavoured to show that, by applying to this the well known principles of the composition of forces, the mean of the velocities of tire reciprocal sounds will, owing to the action of the wind, come short of the true velocity, by a quantity which will in all cases be nearly expressed by — sin.8 A ; where W is the velocity of the wind, S that of

∣⅛u

sound in still air, and A the angle which the direction of the wind makes with the base line. This correction, though generally very small, may often be several times greater than some which are usually taken into account, especially that arising from the effect of the difference of latitude and of the height of the place above the sea, on the force of gra vity. For such we beg to refer to the Philosophical Trans­