where *p* is the pressure, *g* the density, and *q* the difference between the total quantity of heat which a given mass of air may then contain, and that which it contains at a tem­perature and pressure chosen arbitrarily. Also *k* is still a constant, expressing the ratio of the specific heats. We here state the matter in something like the more simple language and notation of M. Poisson, who, in the *Annales de Chimie for* August 1823, has readily deduced that equation from the two leading principles employed for it by Laplace, and which alone are essential to it, viz. the law of Ma riotte, and the *constancy* in the ratio of the specific heats.@@1

From integrating the preceding equation on general principles only, and without having regard to the subject in hand, it could only be inferred that *q* is some indetermi 2

nate or arbitrary function of —. But such eminent mathematicians as Laplace and Poisson, instead of being content with this general view of the matter, ought to have perceived that the conditions of the question, particularly the two principles just specified, and with which they had set out for the purpose of finding *q* in terras of *p* and *g, ne­cessarily* required *q* to be of the form A + B Q log·/?—Iθg∙g)>

where A and B are constants ; because this expression, independently of its being the first to present itself in the integration, is the only form or function of the above general integral which would be free from inconsistency or compatible with those two leading principles. So far, how ever, were they from attending to or being aware of this circumstance, that supposing themselves perfectly at liberty ι

*J)IC ,*

to adopt any particular function of —, they made choice *0*

*8*

I ,

of *q—* A + B (26667 + 0) *ps* 1 ; where A and B are arbitrary constants, and *θ* is the temperature, because *g* has been eliminated by the law of Mariotte. This form was adopted by these philosophers, that it might agree, as they supposed, with the hypothesis that the expansions of air under a constant pressure are proportional to the corresponding increments of absolute heat. But it is easily shown that such *a* hypothesis is quite incompatible, not only with the two leading principles, but with that very form which was on its account purposely given to *q,* as was first point ed out in the Edinburgh Philosophical Journal (for October 1826, p. 335), and afterwards more explicitly in article Hygrometry (vol. xii. p. 113), where a similar question has been consistently solved by a far more simple process of rea soning.

The precise value of the abovementioned ratio of the specific heats is considered an important element in the theory of sound. The experiments of Desormes and Clement give 1·354; those of Gay Lussac and Welter, 1·3748; and those described under the article Hygrometry, 1·3333; all of which seem to be smaller than what would reconcile the theory with the actual velocity of sound. But we shall afterwards see reason for supposing that there is still another source of acceleration, which has hitherto been over

looked. In the *Annales de Chimie* for June 1829, and *Mé­moires de l'Academie* (tome x. p. 147), M. Dulong treats at considerable length on the specific heat of elastic fluids ; and with the view of obtaining the ratio in question for each of them, he assumes as demonstrated, that the actual velocity of sound, in any elastic fluid whatever, has to the velocity computed by Newton’s formula the subduplicate ratio of the specific heat of that fluid under a constant pressure to its specific heat under a constant volume. But since it is only in air that the velocity of sound can be obtained by direct means, Dulong availed himself of a method previously employed by others, though by no means with complete success, as is evident from the discordance of their results. It consists in determining the velocity of propagation from the musical note rendered by a given cylindrical tube, and from the measured distance between two consecutive nodal sections or positions of minimum vibration. The pitch of the note gives the number of vibrations in a given time, and consequently the time of propagation over the measured interval, and therefore the velocity of the sound. By following a particular process, Dulong was enabled to give great precision to this method, which he practised first on atmospheric air, with the view of testing the soundness of the scheme ; and by many successive trials he obtained a series of results, each of which, instead of exceeding, fell short of the velocity determined by direct observation. On finding therefore that this method failed in affording quite so great a ratio as he had expected or wished, Dulong abandoned it altogether, and finally adopted 1∙421 as the ratio for air. This he obtained merely from taking the mean of a great number of the direct experiments which have been made on the velocity of sound in the open atmosphere, though it is usually quoted by others as the result of Dulong's own experiments with the musical notes from tubes. The same empirical mode of determining the ratio, which, however, does nothing for clearing up the theory, has been prosecuted still more closely by Dr Simons, in the Philosophical Transactions for 1830. When the number thus obtained is substituted in the formula, it certainly answers admirably ; and no wonder, for it is just obtained by reasoning in a circle, which is fit to reconcile every sort of discordance.

There can be no question that great ingenuity and superior analytical skill have frequently been displayed in theoretically investigating the velocity of sound ; but an important oversight, and which probably is the principal source of the remaining discrepancy between the observed and the theoretical velocities, seems to attach to all the researches of this kind with which we are acquainted, in that they are conducted upon two assumptions which are so very incompatible with each other that both cannot be true, if indeed any of them is strictly so. In the first place, it is assumed that the particles of the air, at least during a calm, vibrate accurately in the direction in which the sound is propagated. Secondly, that the air, during these vibrations, preserves or acts strictly in its fluid character. That the investigations involve the first of these assumptions, is what no one will for a moment dispute ; and that they also proceed upon the second, is evident from the circumstance that the pressure of the air in such researches is always taken as

@@@1 In the Philosophical Magazine for April 1827, it has been attempted to deduce the same equation from these two principles, incongruously coupled with a third, but with which they are utterly incompatible ; and that is the needless assumption, that, under a constant pres sure, the variations of absolute heat in air are proportional to those of its temperature by the common scale. But so much does the resulting equation savour of the incongruity of the data, that it has been accompanied with an admission of its being only true in one particular state of the variables, and therefore totally unfit fur integration. All this might have been allowed to pass, had it not been done with the view of palming the same fatal defect upon the production of Laplace and Poisson, which however is of an essentially different character. For the equation which has been deduced from the two leading principles alone, by the accurate and consistent method of Poisson, as abov cited, and also given in the Philosophical Magazine for November 1823, neither involves the needless assumption, nor has any dependence on a par­ticular state of the variables, and so is equally true in every case. Nor was it till after having legitimately obtained that equation, that Poisson adopted any such assumption at all on this occasion, though, as we shall presently see, he unfortunately has done so in the sequel of bis memoir, and thereby introduced all manner of inconsistencies.