Laplace and the Speed of Sound

By Bernard S. Finn *

FOR A CENTURY and a quarter after Isaac Newton initially posed the problem in the *Principia,* there was a very apparent discrepancy of almost 20 per cent between theoretical and experimental values of the speed of sound. To remedy such an intolerable situation, some, like Newton, optimistically framed additional hypotheses to make up the difference; others, like J. L. Lagrange, pessimistically confessed the inability of contemporary science to produce a reasonable explanation. A study of the development of various solutions to this problem provides some interesting insights into the history of science. This is especially true in the case of Pierre Simon, Marquis de Laplace, who got qualitatively to the nub of the matter immediately, but whose quantitative explanation performed some rather spectacular gyrations over the course of two decades and rested at times on both theoretical and experimental grounds which would later be called incorrect.

Estimates of the speed of sound based on direct observation existed well before the Newtonian calculation. Francis Bacon suggested that one man stand in a tower and signal with a bell and a light. His companion, some distance away, would observe the time lapse between the two signals, and the speed of sound could be calculated.[1] We are probably safe in assuming that Bacon never carried out his own experiment. Marin Mersenne, and later Joshua Walker and Newton, obtained respectable results by determining how far they had to stand from a wall in order to obtain an echo in a second or half second of time. But Mersenne also used a gun, comparing the time of travel of the flash and the report; and all of the rest of the experiments listed below used this same technique (with the possible exception of Robert Boyle, who did not give reasons for his value).

By 1660 the Florentine Academy had made careful measurements using a gun at a distance of about a mile. The results gave a value for the speed of sound of 1077 Paris feet, or 1148 English feet per second. From this point on the experimental values agreed rather closely, and no one seems to have questioned them seriously during the succeeding century and a half of debate. Nevertheless, we should note that most of these measurements were made without close attention to temperature, pressure, and moisture content of the atmosphere, or wind velocity, and it was really not until the

---

* Smithsonian Institution.

[1] Francis Bacon, *Sylva Sylvarum, or a Natural History,* edited by William Rawley (London, 1627). Century 2, Section 209.

ISIS, 1964, Vol. 55, No. 179.

nineteenth century that these factors were regularly and explicitly taken into account.[2]

MEASUREMENTS OF THE SPEED OF SOUND PRIOR TO 1800

Publication Date	*Experimenter*	*Speed (English feet per second)*
1636	Mersenne	1036[3]
1636	Mersenne	1470[4]
1644	Roberval	600[5]
1666	Accademia del Cimento	1148[6]
1677	Cassini	1152[7]
1685	Boyle	1200[8]
1687	Newton	920-1085[9]
1698	Walker	1305[10]
1708	Flamsteed, Halley	1142[11]
1708	Derham	1142[11]
1738	Cassini de Thury	1107[12]
1739	Cassini de Thury	1096[13]
1744	Blanconi	1043[14]
1745	La Condamine	1112[15]
1751	La Condamine	1175[16]
1778	Kästner, Mayer	1106[17]
1791	Müller	1109[18]

Isaac Newton's rather involved calculation of the speed of sound appeared in the first edition of his *Principia mathematica*.[19] He obtained the value 968 feet per second, not unreasonable in the light of experiments to date.

---

[2] The effects of temperature and pressure variations could be estimated if one assumed they were important only in the Newtonian formulation; wind velocity was generally recognized as being important, even if left unmeasured. The importance of other factors was discussed well into the nineteenth century: for instance, the intensity and pitch of the source.

[3] Marin Mersenne, *Harmonie universelle* (Paris, 1636), p. 214. See also: J. M. A. Lenihan, " Mersenne and Gassendi — an Early Chapter in the History of Sound," *Acustica*, 1951, *2*: 96-99. No attempt has been made to convert numbers to standard temperature or pressure; in almost all cases the data are insufficient to do so. Some values may vary slightly from those reported elsewhere because of minor differences in conversion factors. Some attempts at standardization are made in Lenihan, " The Velocity of Sound in Air," *Acustica*, 1951, *2*: 206-207.

[4] Marin Mersenne, *De l'utilitié de l'harmonie* in *Harmonie universelle* (Paris, 1636), p. 44.

[5] Marin Mersenne, *Cogita physico-mathematica* (Paris, 1644), p. 140. Also Isaac Newton, *Philosophiae naturalis principia mathematica* (London, 1687), pp. 370-371.

[6] Accademia del Cimento, *Saggi di naturali esperienze fatte nell'Accademia del Cimento* (Florence, 1666), Waller translation (London, 1684), p. 141.

[7] See, for instance, Jean Baptiste Du Hamel, *Regiae scientiarum academiae historia* (Leipzig, 1700), p. 169. The measurement is often referred to much earlier than this, usually as giving 180 toises per second.

[8] Robert Boyle, *Essay on Languid Motion* (London, 1685).

[9] Isaac Newton, *op. cit.*, pp. 371-372.

[10] Joshua Walker, " Some Experiments and Observations Concerning Sounds," *Philosophical Transactions*, 1698, *20*: 434.

[11] D. William Derham, " Experimenta et observationes de soni motu, aliisque ad id attinentibus," *Phil. Trans.*, 1708, *26*: 3, 32.

[12] César François Cassini de Thury, " Sur la propagation du son," *Mémoires de l'Académie*, 1738, p. 135. Also " Sur la vitesse du son," *Histoire de l'Académie des Sciences*, 1738, p. 3.

[13] César François Cassini de Thury, " Sur les opérations géométriques faites en France dans les années 1737 & 1738," *Mémoires de l'Académie*, 1739, pp. 126-128.

[14] Ioannes Blanconi, " Observationes physicae variae," *De Bononiensi scientarium et artium institute atque academis commentaris Bologna*, 1744, *2*: 365-366.

[15] Charles Marie La Condamine, " Relation abrégée d'un voyage fait dans l'intérieur de l'Amérique méridionale," *Mémoires de l'Académie*, 1745, p. 488.

[16] Charles Marie La Condamine, *Journal du voyage fait par ordre du Roi à l'Equator* (Paris, 1751), p. 98.

[17] A. G. Kästner and J. T. Mayer, *Göttingesche Anzeigen von Gelehrten Sachen*, 1778, p. 1145.

[18] Gotthard Christoph Müller, *Göttingesche Anzeigen von Gelehrten Sachen*, 1791, pp. 1593-1594.

[19] The speed of sound, u, is equal to $\sqrt{E/\rho}$, where E is the elasticity and ρ is the density

When it came time to publish a second edition of the *Principia,* new values for the density of air gave Newton only a slightly different value, 979 feet per second,[20] for the speed of sound. However, in the light of new experiments, especially those of John Flamsteed and Edmund Halley, he was convinced that this was too low. A solution could be found in his theory of the particulate nature of matter. Newton proposed that the particles of air had diameters equal to one tenth their mutual separation. The particles would then have to vibrate only 90 per cent of the distance he had previously supposed or, alternatively, the sound moved through the particles (10 per cent of the distance) at infinite speed. The speed of the wave would thus be 1088 feet per second, and the presence of water vapor might increase this to 1142.

Before leaving Newton, and without going into detail, we should note that his initial calculation rested on a number of hypotheses. Chief among these were assumptions that the elasticity of the air was a linear function of the sound intensity and that the particles of air oscillated in simple harmonic motion. More than a century was to pass before both of these questions were subjected to a thorough scrutiny.

The eighteenth-century physicists dismissed Newton's explanation for the difference between measurement and theory in what was now the speed-of-sound problem; but even the best of them could do no better. In 1727 Leonhard Euler thought he had a correction which would give theoretical values between 1069 and 1222 feet per second, depending on the temperature.[21] But by 1759, when his best calculation was 894 feet per second, he was forced to admit: "We know that sound is transmitted in one second through almost 1100 feet, and no one has yet discovered the cause of this excess over theory." [22]

Also in 1759, Lagrange calculated the speed of sound without making Newton's assumptions of the harmonic nature of air particle vibrations. Surprisingly, the result of the computation was not affected. Lagrange

of the medium. $E = -v\,(dp/dv) = \rho\,(dp/d\rho)$; $v =$ volume. Therefore $u = \sqrt{dp/d\rho}$. We can apply Boyle's law ($p = k\rho$) to obtain $u = \sqrt{p/\rho}$. Newton did not realize that heat was developed in compression and that the sound vibrations took place so fast that this could not escape but instead raised the local temperature and thus also the pressure. We can calculate this by assuming the more complete gas law: $pv = R\theta$. Then

$$pdv + vdp = Rd\theta.$$

For any heat process, the change in heat,

$$dQ = (\partial Q/\partial v)\,dv + (\partial Q/\partial p)\,dp.$$

Therefore, the specific heat at constant pressure:

$$C_p = (\partial Q/\partial \theta)_p = (\partial Q/\partial v)\;(dv/d\theta)$$
$$= (\partial Q/\partial v)\,R/p.$$

And the specific heat at constant volume:

$$C_v = (\partial Q/\partial \theta)_v = (\partial Q/\partial p)\;(dp/d\theta)$$
$$= (\partial Q/\partial p)\,R/v.$$

In an adiabatic process, $dQ = 0$, allowing us to derive that $vdp/dv = pC_p/C_v$. Substituting this into the expression for the speed of sound, and noting that $\rho = m/v$:

$$u = \sqrt{(p/\rho)\;(C_p/C_v)}\,.$$

Since $C_p/C_v = 1.42$ for air, we can see why Newton's calculation fell 20 per cent short of the experimental value for the speed of sound.

[20] Isaac Newton, *op. cit.* (2nd ed.; London, 1714), pp. 343-344.

[21] Leonhard Euler, *Dissertatio physica de sono* (Basel, 1727), in *Opera omnia,* edited by E. Bernoulli, R. Bernoulli, F. Rudio, A. Speiser, series 3, vol. I (Berlin, 1926), pp. 183-196; the calculation appears on pp. 186-187.

[22] Leonhard Euler, "De la propagation du son," *Mémoires de l'Académie des Sciences de Berlin,* 1759, published 1766, *15*: 428-507; see especially p. 443.

found solace in concluding that " one should not be surprised that theory differs a little from experiment; for we know that experiments complicated enough cannot furnish data simple and free of extraneous conditions, as demanded by pure analysis." [23]

At least by 1802 Pierre Simon, Marquis de Laplace, had resolved, qualitatively and to his own satisfaction, the old Newtonian dilemma. It was very simple. When the sound wave compressed — then rarefied — the air, the simple form of Boyle's law did not hold because the temperature did not remain constant. Under compression, for instance, heat was liberated. Because of the speed with which the compression-rarefaction process took place, this heat did not have time to dissipate; thus the local temperature was raised, the local pressure was raised, and the speed of sound was that much greater than what Newton had predicted.

All this was first revealed to the scientific world by Jean Baptiste Biot in 1802. Laplace had asked his young protégé to discover " the influence that variations of temperature, which accompany the dilations and condensations of air, might have on the speed of sound," [24] and to try to conciliate calculation with experiment. Biot could not carry out his charge because he lacked the necessary data. However, he could and did calculate the amount the temperature would have to rise under compression to produce the observed speed of sound. He let the speed equal $\sqrt{(p+k)/\rho}$, where k was the change in pressure due to the change in local temperature. To evaluate k he was able to refer to the recent nonadiabatic experiments of J. L. Gay-Lussac which measured volume as a function of temperature at constant pressure. To achieve the necessary 40 per cent increase in pressure, Biot calculated that the temperature would have to rise 69° (Réaumer scale).

Five years later S. D. Poisson addressed himself to this problem in a paper delivered to the École Polytechnique. He calculated that a volume compression of 1/116 would increase the temperature one degree. This figure became a standard in the literature.[25]

Biot and Poisson could calculate how much the adiabatic heating (or cooling) would be under Laplace's hypothesis. But as a check on the theory this was a rather fruitless approach as long as no one had measured what the temperature changes actually were. Confirmation of Laplace would have to come from new experiments — experiments not on the speed of sound, but on heat.

Measurements of the specific heat of air had been reported as early as 1783 by Laplace and Antoine Lavoisier [26] and in 1788 by Adair Crawford.[27]

---

[23] J. L. Lagrange, " Récherches sur la nature et la propagation du son," *Turin mémoire*, 1759, in *Lagrange oeuvres*, vol. 1, edited by J. A. Serret (Paris, 1867), pp. 131-132.

[24] J. B. Biot, " Sur la théorie du son," *Journal de physique*, 1802, *55*: 173-182.

[25] S. D. Poisson, " Mémoire sur la théorie du son," *Journal de l'École Polytechnique*, 1808, *7*, cahier 14: 325. For evidence of the use to which this number was put, see Sadi Carnot, *Reflexions sur la puissance motrice du feu* (Paris, 1824) and T. H. Kuhn, " The Caloric Theory of Adiabatic Compression," *Isis*, 1958, *49*: 137-138.

[26] P. S. Laplace and A. Lavoisier, " Mémoire sur la chaleur," in P. S. Laplace, *Oeuvres complètes*, vol. 10 (Paris, 1894), pp. 149-200.

The meaning of " specific heat " was straightforward: the amount of heat necessary to raise a quantity of air through a given temperature difference. And it mattered little what notions one had about the nature of heat, as Laplace and Lavoisier pointed out. Crawford made his measurement by heating two identical brass containers, one evacuated, the other filled with air, and plunging them into identical baths. He measured the temperature increase of the baths and calculated the specific heat of air as 1.79 (compared to the same mass of water as 1.0). The precision of this method was severely limited by the relatively large heat capacities of the brass containers. Laplace and Lavoisier, on the other hand, passed air through a coil in their calorimeter. They obtained a specific heat value of 0.33. It is significant that none of these experimenters considered it important that the one measurement had been made with the air at constant volume, the other with the air at constant pressure. Incidentally, modern values for the two specific heats are 0.173 and 0.242 respectively.

More than a quarter of a century later Laplace again evidenced an interest in specific heats. In 1807 the young chemist Gay-Lussac reported the first of a series of experiments with the eudiometer designed to measure relative specific heats in various gases. He wrote that Laplace and C. L. Berthollet showed particular interest in his work.[28] Results were inconclusive, however, and Laplace — if he ever had such a plan — did not attempt to support his views on the speed of sound with Gay-Lussac's measurements.

In the early nineteenth century there was a great deal of interest among the French scientists in the subject of specific heat. This interest stemmed from various sources, among them desire for more knowledge of chemical combination (heat of reaction and specific heat were thought to be closely connected), interest in more efficient steam engines, and speculation on the value of absolute zero. Georges Cuvier, writing probably in 1813, indicated briefly how this led to a prize being offered by the Académie des Sciences,[29] the " Grand Prix des sciences physiques ou naturelles " for 3000 francs, with terms as follows: " Determine the specific heats of gases, particularly those of oxygen, azote, and some compound gases, comparing them to the specific heat of water, etc." [30] There is no evidence that there was any stimulus from the speed-of-sound problem. The prize was proposed in January 1811, to be awarded in 1813. Two important papers were submitted, both of which contribute to our story. One, by F. Delaroche and J. E. Bérard, appeared in 1813 and won the prize; the other, by Nicolas Clément and Charles Désormes, was not published until 1819.

Delaroche discussed prior work before revealing his own experimentation. His reasons for rejecting the method of Crawford are interesting. He wrote:

---

[27] Adair Crawford, *Experiments and Observations on Animal Heat, and the Inflammation of Combustible Bodies* (2nd ed.; London, 1788).

[28] J. L. Gay-Lussac, " Premier essai pour déterminer les variations de température qu'éprouvent les gaz en changeant de densité, et considérations sur leur capacité pour le calorique," *Mémoires de physique et de chimie de la Société d'Arcueil*, 1807, *1*: 181-182.

[29] *Mémoires de l'Institut National*, 1812, published 1816, *2*: lxxxi-lxxxvii.

[30] Ernest Maindron, *Les fondations de prix à l'Académie des Sciences* (Paris, 1881), p. 61.

" This procedure, besides its lack of precision, has the inconvenience of not giving the *specific heat* of gases, in the sense that we have given to that word, since the gases, thus closed up, can neither dilate nor condense." [31] Thus he made a rough distinction between the constant-volume and constant-pressure processes; furthermore, the constant-volume process was assumed to give a value for the specific heat not particularly important—in fact, not really the specific heat at all.

The substance of the Delaroche-Bérard experiment was to supply gas at a constant pressure, heat it, and allow it to cool while flowing through a calorimeter. The specific heat measured for air was 0.2667; other reasonable values were obtained for oxygen, hydrogen, water vapor, etc.

SPECIFIC HEATS PER UNIT VOLUME AS MEASURED BY DELAROCHE AND BÉRARD

Gas	*Delaroche and Bérard*	*Modern*	*Gas*	*Delaroche and Bérard*	*Modern*
Air	1.0	1.0	N_2O	0.89	0.79
H_2	12.3	14.1	C_2H_4	1.58	1.46
CO_2	0.82	0.77	CO	1.08	1.04
O_2	0.88	0.91	Water vapor	3.2	2.0
N_2	1.03	1.06	Air at 1.36 atmospheres	1.24	1.36

By a slight alteration in their apparatus they were able to obtain one measurement for the specific heat of air at a higher pressure. They found that by increasing the pressure 35.83 per cent (from 74.05 to 100.58 centimeters of mercury) the heat given off by equal volumes of gas increased 23.96 per cent.[32] No attempt was made to repeat the experiment or to try for other pressures. Delaroche and Bérard did not try to calculate the speed of sound, but they did something rather interesting. They made reference to Laplace, assumed that his reasoning on the discrepancy between theoretical and experimental values for the speed of sound was correct, and thus were able to use the calculations of Poisson plus some of their own numbers to come up with a value (− 318°) for absolute zero.

A second entry for the Grand Prize of 1813 ran afoul, if one would believe the authors, of some scientific politics. For our purposes there were two interesting parts to the experiment of Clément and Désormes.[33] In the first they had a closed air-filled glass vessel connected to a water manometer and a vacuum pump. The internal pressure was reduced about one centimeter of mercury below atmospheric pressure. A valve was opened long enough to allow equalization of internal and external pressures, then quickly closed. As heat dissipated from the enclosed gas, the manometer indicated a decrease in internal pressure, giving a measure of what had been the increase in temperature. In the second part of the experiment they measured

---

[31] F. Delaroche and J. E. Bérard, " Mémoire sur la détermination de la chaleur spécifique des differens gaz," *Annales de chimie*, 1813, *85*: 84.

[32] *Ibid.*, 132-138.

[33] Charles B. Clément and Nicolas Désormes, " Détermination expérimentale du zéro absolu, de la chaleur et du calorique spécifique des gaz," *Journal de physique*, 1819, *89*: 321-346, 428-455.

the relative specific heats (at constant pressure) of several gases, though without comparing them to water. (This last fact would seem to be reason enough not to give them the prize.) This second part of the experiment can be compared to the work of Delaroche and Bérard, though it was a good deal simpler in its mechanics. A flask of gas was immersed in a constant-temperature bath, allowing the gas to expand against a water manometer; the rate of expansion was a measure of the specific heat. By a slight alteration in the procedure they were able to measure the relative specific heats of air at three less-than-atmospheric pressures.

In 1822, Gay-Lussac and J. J. Welter measured the ratio of the specific heat at constant pressure (C_p) of air to the specific heat at constant volume (C_v). The details of their experiment were never published, but apparently they did essentially the same thing as Clément and Désormes in the first of the two experiments mentioned above. The ratio obtained was 1.3748.[34]

From a modern point of view only the measurements of the ratio C_p/C_v performed by Clément and Désormes, then by Gay-Lussac and Welter, should have been of any value to Laplace. Yet it is a fact that his first successful calculation of the speed of sound was made using the Delaroche-Bérard data. We might content ourselves by saying that the experiment was faulty, that Laplace's theory was faulty, and by some coincidence of the type often found in the history of science the two completely canceled each other. But there is more to it than that. Laplace's theory was faulty in large part because he tailored it to the Delaroche-Bérard numbers; that is, he constructed his theory to fit the best available data. In fact, he constructed several such theories. Unfortunately, the crucial experimental number was faulty in just the right way to make them possible. Let us then look briefly at how Laplace fitted his theoretical notions of heat to the speed-of-sound problem. Such a look is particularly rewarding because his view fluctuated considerably during two decades — an indication of the difficulties inherent in the problem of heat in the early nineteenth century.

Laplace's first published comments on the speed of sound appeared in Note V of Berthollet's *Statique chimique*.[35] Here he advanced the view that the quantity of heat in a gas at a given temperature was proportional only to the volume and was independent of the density of the elementary particles. His reasoning was that when the gas was compressed to, say, half its volume, the density of particles at the surface would double. If the repulsive force between particles remained constant, that would be just enough to balance the external pressure. But the free caloric was also reduced by half. Laplace assumed there were only two places for it to go. It did not become latent because then the repulsive force between particles would have changed; therefore it must have been released to raise the temperature of the immediate surroundings. In a nutshell: when the gas was compressed to half its volume, half of the free caloric was released.

---

[34] P. S. Laplace, "Note sur la vitesse du son," *Annales de chimie*, 1822, *20*: 200.

[35] C. L. Berthollet, *Statique chimique* (Paris, 1803), pp. 245-247. Also in P. S. Laplace, *Oeuvres complètes*, vol. 14 (Paris, 1912), pp. 329-331.

No sooner had this analysis been sent to press than Laplace detected an error. His correction appeared in Note XVIII of the same book.[36] The change in surface area was not proportional to the change in volume but rather to the two-thirds power of the volume. If the pressure were increased eight fold, the density of particles on the surface would be multiplied by four. Thus the repulsive force of each particle against the external pressure would have to double. Indeed, the repulsive force of a particle at a given temperature would be proportional to the reciprocal of the distance between particles. Under compression some of the free heat became latent in order to make up the repulsive force. In a nutshell: when the gas was compressed to half its volume, less than half of the free caloric was released; the rest (Laplace did not estimate how much) became latent in order to make up the increase in repulsive force: " which agrees with experiment and with the observed speed of sound."

In December 1816, almost four years after the experiments of Delaroche and Bérard, Laplace read to the Académie des Sciences a remarkable paper in which he used their results to calculate the speed of sound. He noted once again that the repulsive force between molecules was inversely proportional to their separation. Furthermore he was quite explicit in stating that in order for the process to have any effect on the speed of sound, the vibrations would have to take place fast enough so that the heat evolved would not have time to escape, but rather would raise the temperature of the molecules involved (as we would say, the process is adiabatic), a condition which he felt certain held. Then, " the true speed of sound is equal to the product of the Newtonian formula times the square root of the ratio of the specific heat of air at constant pressure and at different temperatures to its specific heat at constant volume." [37] This is an eminently correct statement. It is interesting to see how he arrived at it and how he applied it to the Delaroche data.

Laplace's basic ideas, clearly expressed in the 1816 memoir, were identical with those of his second 1803 statement. Pressure depended inversely on the distance between molecules and was related in some undetermined way to the temperature.

In Laplace's words, freely translated:

> When the temperature of the air is raised, at constant pressure, only part of the heat is used to raise the temperature; the rest serves to increase the volume. This latter part of the heat is liberated when the air is reduced to its primitive volume by an increase in pressure. When two air molecules come close together in a vibration, the heat released raises their temperature and tends to radiate out into the nearby area; but if this happens very slowly relative to the speed of vibration, we can suppose that the amount of heat remains essentially the same. So, as the two molecules approach, they meet a resistant force first because the repulsive force is proportional to $1/D$ at constant temperature, and second because the latent caloric which

---

[36] C. L. Berthollet, *Statique chimique*, pp. 552-553. Also in P. S. Laplace, *Oeuvres complètes*, vol. 14, p. 332.

> develops increases their temperature. Newton only considered the first of these, but obviously the second cause must increase the speed of sound since it increases the pressure.[37]

Thus Laplace could relate his own adiabatic system to a constant-pressure heat process, and Newton's isothermal system to a constant-volume heat process. To make the pressures proportional to the specific heats, he apparently assumed that for equal volumes at different temperatures the pressure was proportional to caloric content; this neither follows from nor contradicts the rest of his theory.

Delaroche and Bérard had measured the specific heat of air at two pressures. There is no indication that Laplace thought it important or even realized that these were constant-pressure specific heats. Crucial to him was the assumption that they were proportional to the caloric contents of the gas at the two pressures. The difference in the heat content, at constant volume, had therefore been measured as 24 per cent for a change in pressure of 36 per cent. Thus, he could assume that if a quantity of air were heated at constant volume to increase its caloric by 24 per cent, the pressure would increase by 36 per cent. On the other hand, if the air were allowed to expand at constant pressure back to the original temperature, the amount of heat absorbed should theoretically be 36 per cent. Hence the ratio of the specific heats was $.36/.24 = 1.5$. Laplace multiplied the Newtonian value for the speed of sound by $\sqrt{1.5}$ and obtained 345.35 meters per second, to be compared with an experimental value of 337.18.

Laplace was well pleased with the agreement, as well he might have been. Before going on, however, we might note that the whole analysis was quite fortuitous since the data of Delaroche and Bérard were in error. Instead of 1.24 they should have obtained 1.36 for the relative specific heat of air at the higher pressure, an error of less than 10 per cent in their measurements which became a very important 50 per cent in the way it was used by Laplace.

Five years later, in 1821, Laplace had developed a completely different theory of heat.[38] But, in what might be considered a classic example of the manner in which multiple theories are available in science for the explanation of a given number of data, the new theory explained all the phenomena embraced by the old, including the faulty Delaroche-Bérard experiment.

Every molecule was surrounded by an amount of heat, c. The repulsion between molecules, and therefore the pressure, was proportional to c^2. Thus, if the pressure was increased by, say, a factor of two, the amount of heat necessary to sustain the new pressure would increase by a factor of only $\sqrt{2}$ if the temperature remained constant. As a result, heat would be evolved. Applying this to the speed-of-sound problem, Laplace noted that

---

[37] P. S. Laplace, "Sur la vitesse du son dans l'air et dans l'eau," *Annales de chimie,* 1816, *3*: 238-241. Alsoin, *Oeuvres complètes*, vol. 14, pp. 297-300.

[38] P. S. Laplace, "Développement de la théorie des fluides élastiques et application de cette théorie à la vitesse du son," *Connaissance des temps, 1825,* 1822, also in *Oeuvres complètes*, vol. 13 (Paris, 1904), pp. 291-301.

the amount of heat liberated would be too great if the process were completely adiabatic. So he blatantly assumed that the compressions and rarefactions did not take place fast enough to prevent some radiation — in other words, the process was not completely adiabatic.

A year later he modified this last, rather surprising view. The compressions and rarefactions did take place adiabatically; the excess heat, instead of escaping, became latent.[39] He assumed that there was an amount of heat $(c + i)$ associated with every molecule, but the pressure was proportional only to c^2. Here c was the free heat surrounding the molecule, while i was the latent heat and was somehow associated with the state of the molecule. This interpretation fitted very nicely with the experimental values for the speed of sound, in light of the data reported by Delaroche and Bérard.

Laplace's reasoning went this way. If the process were adiabatic, and if there were no change in the latent heat, the velocity of sound, u, would be $\sqrt{2p/\rho}$. As Laplace had pointed out in 1821, this was too high. At that time he had suggested that the excess heat might escape. Now he postulated a change in the latent heat, so that the formula could be rewritten as

$$u = \sqrt{\frac{2p}{\rho} \cdot \frac{\partial(\rho c)}{c\partial\rho}}\,.$$

We might note that the change in c would be the negative of a change in i in an adiabatic process. From the Delaroche experiment Laplace deduced that $\partial(\rho c)/c\partial\rho$ was equal to 0.8; he then contented himself with the statement that this gave him approximately the observed speed of sound.

Two questions arise: what sort of agreement did Laplace actually get? and how did he obtain the value 0.8? The answer to the first is easy. He stated his most recent value for the Newtonian calculation as 282.4 meters per second, which, when multiplied by $\sqrt{1.6}$, gives 358 meters per second. This is really not very good agreement with the experimental value which he cited as 337.2 meters per second, which is probably why he did not complete the calculation of the theoretical value.

The answer to the second question is not so easy. Laplace gave no indication of how he interpreted the Delaroche data in terms of his new theory. I suggest the following as at least a plausible reconstruction of his thought process.

Delaroche and Bérard measured the specific heat of air at two pressures. Assume that the specific heat is proportional to $(c + i)$, where i is an unknown function of pressure, and c^2 is proportional to the pressure. When they increased the pressure (hence also the density) by a factor of 1.36, c should have been increased by $\sqrt{1.36} = 1.165$. Since they measured 24 per cent increase in the specific heat, $0.075c$ must have been absorbed into a change in i. Therefore,

$$\frac{dc/c}{d\rho/\rho} = \frac{0.075}{0.36} = 0.20, \text{ and } \frac{\partial(\rho c)}{c\partial\rho} = 0.80.$$

---

[39] *Ibid.*, p. 300.

One year later, in 1823, Laplace presented his final calculation of the speed of sound. It was essentially independent of any theory of heat.[40] He used the measurements of Gay-Lussac and Welter, which he judged more accurate than the similar measurements of Clément and Désormes. Since they had measured the change in pressure resulting from the heat produced in an adiabatic compression, their reported ratio could be applied directly by Laplace to the speed-of-sound problem. The ratio was 1.3748, giving a speed of 337.144. Corrected for water vapor, this was raised to 337.715, which could then be compared with the newly determined Bureau of Longitude value, 340.889 meters per second.[41]

The old theory, we might note, had not been abandoned. Indeed, Laplace found support for it in these new results. He started with an expression for the ratio of the specific heats:

$$\frac{C_p}{C_v} = \frac{(c+i)_p}{(c+i)_v} = \frac{\rho \, \frac{\partial v}{\partial \rho}}{p \, \frac{\partial v}{\partial p}}.$$

Since ρ, density, is inversely proportional to volume, it was an easy derivation to arrive at the expression: $(v'/v) = (p'/p)^{\gamma}$, where $p < p'$ and γ is the ratio of the specific heat at constant pressure to the specific heat at constant volume. It is important to note that for Laplace C_p was proportional to the volume. Delaroche and Bérard had measured C_p at two different pressures. The ratio of the pressures was 1.36, the ratio of the measured specific heats was 1.24. Laplace calculated that $1.36^{1/1.3748} = 1.249$. He considered this good confirmation of his views.[42]

In recapitulation, Laplace saw the source of error in the speed-of-sound calculation at least as early as 1802; he used the terminology " ratio of specific heats " by 1816. But we should note that in neither of these instances did the theory of heat play an important role, and his statements remain valid today. It was in searching for experimental values of specific heat ratios that difficulties arose. Here the theory of heat could be decisive. Where today we would dismiss the results of Delaroche and Bérard as both irrelevant and in error, Laplace was able to fit them conveniently into two different theories in three different fashions. In each case he could believe in good conscience that he was supporting solid theoretical views with the best available experimental evidence. Fortunately, he was also able to support his theory of sound transmission with data independent of his notions of heat, thus making it easier for the former to survive when the latter fell into disrepute.

Neither the great name of Laplace nor the reasonableness of his basic assumptions nor the close agreement between experiment and theory were sufficient to solve immediately for the scientific world the problem of the speed of sound. Instead, he seems to have brought the problem to the fore,

---

[40] P. S. Laplace, *Traité de mécanique céleste*, vol. 5 (Paris, 1823), book 12, pp. 99-160.

[41] *Ibid.*, pp. 140-141.

[42] *Ibid.*, pp. 142-143.

where it became fair game. And the debate was rather heated at times over the next half a century. To understand this we should realize that there were two fundamental defects in the Laplacian view. One was his theory of heat. Laplace had tied his solution to half a dozen interpretations of the nature of caloric. And in each case he was able to discard without so much as a murmer the previous explanation as he expounded the virtues of a new one. Others, naturally, had even fewer compunctions about disregarding the latest views of the French savant. A second defect was even more crucial. Certainly heat was produced by compression; but what really happened to this heat for a sound wave in air? Laplace assumed there was no time for it to escape (though even he departed from this view briefly in 1821, as we have seen). To others this seemed highly unlikely. Surely some of the heat had time to radiate away, in which case the effect predicted by Laplace would be inadequate even if his theory of heat were true.

To give a detailed account of these other views would involve us too deeply in the problems of caloric in the early nineteenth century. A brief description of some of the more interesting notions will have to suffice.

Poisson, expectedly, supported Laplace's hypothesis. In 1823, without committing himself to the theory of heat, he calculated the speed of sound using the experimental value of C_p/C_v found by Clément and Désormes. The result was only slightly inferior to that of Laplace. Other early supporters were P. L. Dulong[43] and Gerrit Simons.[44]

In 1812 Johann Friedrich Benzenberg found support in his own experiment, and in John Dalton's law of partial pressures, for the idea that sound traveled at different speeds through different constituents of the atmosphere.[45] It might go 800 feet per second through oxygen or CO_2, but at a faster rate through water vapor. Thus a sharp tone might be heard over two or three seconds if the observer were sufficiently far away. The measured speed of sound would obviously be that in the fastest constituent.

John Herapath developed a kinetic theory of heat in the early 1820's, obtaining expressions quite similar to those derived by Laplace. As a result, he accused Laplace of plagiarism in 1823, at the same time criticizing his use of the stolen ideas. In 1830 Herapath extended his criticism to Laplace's theory of sound. At the very beginning of a complex derivation where the theorems "speak for themselves" he introduced an extra $\sqrt{2}$ into the Newtonian formula under the radical sign. The result was a Newtonian speed multiplied by $\sqrt{1.414}$, and a calculated speed markedly closer to the experimental value than Laplace's. We would interpret this as due to the fact that $\sqrt{2}$ happens to be closer to the true value of C_p/C_v (1.42) than

---

[43] P. L. Dulong, "Récherches sur la chaleur spécifiques des fluides élastiques," *Annales de chimie*, 1829, *41*: 113-158.

[44] Gerrit Simons, "On the Theoretical Investigation of the Velocity of Sound, as Corrected from M. Dulong's Recent Experiments, Compared with the Results of the Observations of Dr. Moll and Dr. Van Beek," *Phil. Trans.*, 1830, *120*: 209-214.

[45] J. F. Benzenberg, "Ueber den Einfluss der Dalton'schen Theorie auf die Lehren von der Geschwindigkeit des Schalls," *Annalen der Physik*, 1812, *42*: 155-162.

was Gay-Lussac's experimental number. Herapath considered it a clear defeat for the Frenchman's " ingenious, but very questionable, hypothetical assumption."[46]

James Ivory and Henry Meikle supported Laplace but found both theoretical and experimental reasons to believe that the key factor ought to be 4/3 instead of 1.3748, in spite of the better experimental agreement provided by the latter. Ivory declined to place much faith in experiments " which require the measurement of very minute variations of length with extreme accuracy."[47]

In 1837 William Ritchie considered Laplace's hypothesis " gratuitous and improbable " and proceeded to revive the old Newtonian suggestion.[48]

Further attempts to dislodge Laplace were made by Richard Potter and J. J. Waterston in the 1850's. Appealing to the atomic theories of Dalton and Herapath respectively, both managed to introduce a factor $\sqrt{3/2}$ into the Newtonian formula without benefit of the adiabatic heating effect. James Challis attacked Laplace's hypotheses and solved the problem " exclusively on hydrodynamical principles."[49]

But the tide was beginning to run strongly in the other direction. Laplace's basic assumption was too plausible to be denied. By using his mechanical equivalent of heat, James Joule calculated how much the temperature of air would rise under compression with a result that " fully confirms the theory of Laplace."[50] Excellent summaries of Laplace's argument, disentangled from the antiquated theories of heat, were provided by William Rankine, G. G. Stokes, and John Le Conte.[51] They carefully noted that Laplace's solution was sufficient to explain the speed of sound within probable error, though there was still the possibility that the explanation lay elsewhere. It was still only an assumption that the radiative process took place slowly enough to make transmission in air an adiabatic process. However, the burden of proof was now clearly with the opposition, and they had not provided it.

---

[46] J. Herapath, " On the Velocity of Sound and Variation of Temperature and Pressure in the Atmosphere," *Quarterly Journal of Science*, 1830, pp. 168-169.

[47] James Ivory, " On the Laws of the Condensation and Dilation of Air and the Gases, and the Velocity of Sound," *Philosophical Magazine*, 1825, *66*: 11.

[48] W. Ritchie, " An Attempt to Account for the Discrepancy Between the Actual Velocity of Sound in Air or Vapour, and That Resulting from Theory," *Royal Society Proceedings*, 1837, *3*: 458.

[49] Richard Potter, " The Solution of the Problem of Sound, Founded on the Atomic Constitution of Fluids," *Phil. Mag.*, 1851, *1*: 101-104, 317; J. J. Waterston, " On the Theory of Sound," *Phil. Mag.*, 1858, *16*: 481-495; James Challis, " On the Theory of the Velocity of Sound," *Phil. Mag.*, 1851, *1*: 406.

[50] James Prescott Joule, " On the Theoretical Velocity of Sound," *Phil. Mag.*, 1847, *31*: 115.

[51] William John Macquorn Rankine, " On Laplace's *Theory of Sound*," *Phil. Mag.*, 1851, *1*: 225-227; G. G. Stokes, " An Examination of Possible Effect of the Radiation of Heat on the Propagation of Sound," *Phil. Mag.*, 1851, *1*: 305-317; John Le Conte, " On the Adequacy of Laplace's Explanation to Account for the Discrepancy Between the Computed and the Observed Velocity of Sound in Air and Gases," *Phil. Mag.*, 1864, *27*: 1-32.