To Biot Not to Be: The Sound of Success

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Abstract

I argue that the central quantitative derivation of the speed of sound equation was first discovered not by Laplace, but by his younger colleague Jean-Baptiste Biot, through consideration of how empirical facts about the behavior of heat plug into Lagrange's wave equation.

1 Correcting the Definitive Correction

How fast does sound travel in a given medium? The resolution of this question eluded some of the most powerful physicists of the 18th century. The usual story goes: Newton's prediction that $v = \sqrt{P/\rho}$ did not match experiments of the time[1]. He tried in each successive edition of his *Principia* to correct this error (Newton, 1999, 772-778), but without success. Newton was followed by a venerable tradition of botched attempts, including the likes of Euler (1926), Lambert (1768), and Lagrange (1788). Finally, in a short address to the Académie des Sciences, Pierre Simon de Laplace exposed that acoustic phenomena do not take place at a constant temperature, and on this consideration produced the definitive correction:

$$v = \sqrt{\frac{P}{\rho}\frac{c_\rho}{c_v}}. \tag{1}$$

where c_ρ and c_v are the specific heats of air at constant pressure and constant volume, respectively.

[1] Following a common convention, P/ρ is the ratio of pressure to density of the medium.

I would like to argue for one revision and one addition to this story. First, the revision: while Laplace did play an important role in the correction of the speed of sound equation, the central quantitative calculation was in fact discovered by Laplace's junior colleague, Jean-Baptiste Biot. The study of Biot's result then leads to this addition: the trick behind the correction of the speed of sound equation was to plug a carefully operationalized characterization of adiabatic heating into Lagrange's wave equations, and solve for the speed of sound.

My case is made in two main sections. In the first, I review Laplace's (1816) suggestion that the correction of the speed of sound equation was founded on three assumptions about the nature of heat. I show that all of these assumptions were anticipated by Biot (1802), but that we have good reason to suspect the collaboration of Laplace and Biot on each other in the years leading up to 1802. In the second section, I describe how Biot derived an early version of the correct speed of sound equation, and discuss the meaning and motivation behind each of Biot's assumptions.

2 An Epiphany of Nebulous Origin

Laplace's (1816) address to the Académie des Sciences contains no technical details, and I do not know of any surviving text or manuscript in which Laplace carries out the derivation of the speed of sound equation[2]. Instead, Laplace rather cryptically suggests that the central insight of behind this derivation was to draw on three facts about the behavior of heat[3]. These facts may be summarized as follows[4]

L1. compression of air $\Rightarrow$ increase in temperature;

L2. increase in temperature $\Rightarrow$ increase in elasticity;

L3. at a constant temperature: elasticity $\propto$ density.

[2]Nevertheless, a great deal of recent scholarship proceeds under the impression that the essential correction of Newton's speed of sound equation was carried out by Laplace. For example, see: Finn (1964), Cohen (1999, pp. 186), Psillos (1999, 119-120), and Chang (2002).

[3]I have provided a translation of this intriguing little article in an appendix (see page 12), and will be referring to this translation. All other quotations from non-English sources are translated freely into English.

[4]There is also an additional, "innocuous idealization" suggested by Laplace, that during a single sonic vibration the increase of temperature is negligible.

Let us begin with a discussion of the experimental and theoretical bases for these claims, in order to better understand their appearance in Biot's work. Then I'll sketch Biot's expression of these claims more than a decade earlier, and suggest that the speed of sound equation might have developed out of a collaboration between these two physicists. Finally, I'll consider the what can (and cannot) be inferred about the role of particular ontological commitments about the nature of heat in this derivation.

2.1 Experimental and Theoretical Basis

Of Laplace's three assumptions, L1 requires the most discussion. In his (1816) statement of L1, Laplace writes that "[t]he heat released by the approach of two neighboring molecules of a vibrating arial fiber elevates their temperature" (Apx. B, pp. 13). By 1800, this claim was experimentally well supported. Delamétherie (1798) reported that air bubbles would sometimes become trapped in water pipes running down into the mines; in doing so, this air would have become highly compressed. When this air was finally released above ground, the accompanying dilation would be so extreme as to deposit ice on nearby objects. Similar experiments confirming the relationships between compression and temperature variation were reported by others, such as Darwin (1788).

As some historians have pointed out[5], Laplace's theoretical understanding of heat gave him good reason to posit L1. Laplace was a famous proponent of the view that heat was a subtle fluid called *caloric*. This view, which was sketched by Lavoisier and Laplace (1783, 151) and developed robustly by Laplace (1821), allowed for heat to exist in more than one state. In brief: a quantity of *free* or *sensible* caloric was proportional to temperature, and could be measured with a thermometer. Caloric in this state was self-repelling, so that regions of high-density caloric tended to transfer heat to regions of low-density caloric, in accord with Newton's law of cooling. However, under the right conditions, caloric could also become chemically combined with regular matter, thus occupying volume but not affecting temperature. This was called the *chemical* or *latent* state of caloric[6].

A calorist in Laplace's tradition thus had the following explanation of Delamétherie's

[5]For example, see Fox (1971), Chang (2002), and Chang (2004).
[6]For a more thorough treatment of Laplace's caloric theory, see (Fox, 1971).

mine result. A quantity of air is compressed at the bottom of a pipe of water, the volume is decreased, and a certain quantity of latent caloric is "squeezed out" into a sensible state. This sensible heat dissipates as the bubble of air travels up the pipe. When the bubble finally escapes into the atmosphere at the top of the pipe, the air dilates dramatically, creating a region of low-density caloric. This region is quickly infused with the dense caloric in neighboring regions, resulting in a temperature drop and consequently an ice deposit on nearby objects.

Claims L2 and L3 were well known at the time, and are of considerably less interest. The former was a result known to Newton: an increase in temperature "increases the elasticity of air" (Laplace, 1816, Apx. B, pp. 13). The latter is another well-known law: for particles at constant temperature, "their mutual repulsion increases in inverse proportion to their distance" (Laplace, 1816, Apx. B, pp. 13).

So let me summarize. Laplace has suggested that three assumptions led him to the speed of sound: the experimental/theoretically motivated conclusion that compression increases temperature, two well-known laws, and an innocuous idealization. This is perhaps the origin of the just-so story I told at the beginning. But on the face of it, this account cannot be complete, because no mention at all has been made as to the role of the *ratio* of specific heats c_ρ/c_v in Laplace's equation, as opposed to just some arbitrary constant k.

Unfortunately, a more detailed account of Laplace's reasoning is not available, and so I will have nothing to say about how this particular ratio was historically derived. However, I do think that a significant improvement of the account that Laplace has given can be significantly improved, by reintroducing the role of Jean-Baptiste Biot.

2.2 Biot achieved a similar result from the same three facts

In 1802, some fourteen years before Laplace's address to the Académie des Sciences, Jean-Baptiste Biot published an article on the theory of sound[7]. Putting off the details of Biot's result (which I promise to return to in section 3), let me draw attention to a few passages from this article. Consider three assumptions that Biot makes:

[7]A translation of this article is produced in Appendix C of this paper, to which I refer in citing Biot.

B1. "when we condense atmospheric air, it loses part of its latent heat, which passes to the state of sensible heat" (Biot, 1802, Apx. C, pp. 18);

B2. "part of this heat... raises the temperature of the fluid, and influences its elasticity" (Biot, 1802, Apx. C, pp. 19);

B3. For a constant temperature, elasticity is a function of density, given by $\phi(\Delta) = gnH\Delta$, where g, n, and H are constants (Biot, 1802, Apx. C, pp. 21);

These assumptions are of course effectively identical to the three assumptions L1-L3 outlined by Laplace above.

Biot uses these assumptions in the derivation of an equation for the speed of sound, given by

$$v = \sqrt{gnH}\sqrt{1+k} \tag{2}$$

where $1+k$ is a constant related to medium through which the pulse travels (Appendix C pp. 23). But by B3, $gnH = \phi/\Delta$. So switching momentarily to modern notation for elasticity and density, it seems that Biot has derived:

$$v = \sqrt{\frac{P}{\rho}K}. \tag{3}$$

But of course, this is just the famous correction to the speed of sound equation 1, in which $K = c_\rho/c_v$. But of course, the fact that distinction between the specific heats in this ratio was not known by the year of Biot's publication in 1802.

Given the surface similarity between Biot's result and that of Laplace, it seems very likely that Laplace drew largely on Biot (1802) in the development of his (1816) paper. What I would now like to argue is that the reverse is also true; that is, that Biot drew largely on the ideas of Laplace in the development of his (1802) result.

There is of course no question that Laplace had *some* influence on Biot's result; Biot (1802) opens by recognizing Laplace's suggestion that he examine how **B1** might influence the speed of sound. But how deeply did this influence run? Two manuscript sources suggest that Laplace may have had a quite formative influence on the development of Biot's derivation.

The first is a well-known anecdote by Biot (1858), published late in his life, in which he recalls a close relationship with Laplace in the years leading up to 1802. In it, he describes their frequent visits, in which Biot would occasionally bring his work to discuss with Laplace, from mixed differential equations to physics. Unfortunately, Biot's doting recollection of Laplace may not a particularly reliable source. However, a second source provides further evidence that the aging Biot was not mistaken. Several letters written from Biot to his old teacher Lacroix confirm that Biot was in contact with Laplace: "I wrote to Cit. Laplace yesterday to tell him I had finished his summary..." (Biot, 1799-1800, letter no. 2). Although few details about his relationship with Laplace are mentioned, one can reasonably infer that there may have been some diffusion of information between these men.

Thus, although we cannot say for certain whether or not there was an open collaboration between Laplace and Biot on the speed of sound equation, it seems clear that each had some influence on the other. The upshot is that although it still isn't obvious how Laplace arrived at his (1816) result, he can learn something new about his derivation by studying the derivation of Biot.

2.3 The Role of Ontology

There is a level of these derivations that I have quietly left out until now. Yet due to some recent literature on the subject, some mention should be made of the role of ontology in this discovery. Notably, it is possible to play up the role of ontology in Laplace's three "facts." On the one hand, one can insist with Hasok Chang that a material understanding of caloric "played a crucial role" in the success of Laplace's derivation (Chang, 2002, 909). This view would correspondingly focus on Laplace's theoretical explanation of the elevation of temperature during compression; this view certainly set him apart from many earlier failed attempts. On the other hand, one can follow Stathis Psillos in claiming that Laplace's derivation "was not essentially dependent" on a material theory of heat (Psillos, 1999, 113). Instead, Psillos focuses on the elements of Laplace's ontology that were "preserved" in later theories.

I believe it is rather appropriate to play down the role of ontology altogether here, not only from the perspective of Laplace, but from the perspective of Biot as well. First, there is a sense in which Laplace could have taken L1-L3 to be *independent* of any particular ontology. In their (1783) memoir on heat, Lavoisier and Laplace made the fascinating suggestion one might only adopt principles that hold for both caloric and dynamic theories of heat, writing that "[i]n our ignorance of the nature of heat, we are left to carefully observe its effects, which principally consist in the dilation of bodies, the rendering of fluids, and the conversion into vapor" (Lavoisier & Laplace, 1783, 153-154). They even went so far as to suggest a schema for translating between these two ontologies:

- Free caloric ⇔ *Force vive*;
- Combining of caloric ⇔ Loss of *force vive*;
- Disengaging of heat ⇔ Augmentation of *force vive*

(Lavoisier & Laplace, 1783, 154). And while this view is perhaps more characteristic of Lavoisier's writing than that of Laplace[8], it is not unreasonable to suppose that Laplace was aware of it in the decades that followed. Thus, I see no way to reasonably infer that Laplace's work relied on a particular view about the nature of heat.

This is a claim about what we cannot infer; now I would like to suggest that the study of Biot's work indicates an additional, positive claim: the derivation of the speed of sound equation was motivated above all by operational considerations. That is, while Biot made use of the language of a material theory of heat, his central definitions, assumptions, and arguments all revolve around particular measurements and the relations between them. To appreciate this, let us now engage in a more careful analysis of Biot's derivation.

3 A hot new suggestion, a hot new result

The essence of Biot's derivation may be sketched in the following simple terms. Biot assumes, apparently following Laplace's suggestion, that a single vibration of a passing sound wave results in a proportional increase in elasticity. The constant of proportionality is taken to be

[8] For example, Lavoisier cautioned a few years later: "we are not even obligated to suppose that caloric is a real substance; it is sufficient... that it be any kind of repulsive cause that separates the molecules of matter, allowing us to imagine its effects in an abstract and mathematical way" (Lavoisier, 1789, 19).

a property of the fluid through which the sound wave travels, which can be calculated from measurements of air using barometers and thermometers. Biot then considers an infinitesimal component of this elasticity, and integrates to find the total elasticity of the fluid as a sound wave passes through it. He then follows Lagrange line by line in order to produce a solution to Lagrange's equation of motion for waves corresponding to the value of elasticity that he has found. The result is the velocity of sound described above.

I see no particular reason to walk through the steps of this calculation in any finer detail. However, we can learn something about what should be added to the three assumptions emphasized by Laplace, by examining the particular assumptions required for Biot's derivation. These assumptions may be broken down into three categories: physical assumptions (assumptions about empirical matters of fact), mathematical conventions (definitions and equivalences), and idealizations ("harmless" approximations made for ease of calculation). Let us now briefly review each of them.

3.1 Assumptions of Biot's Derivation

Physical Assumptions.

P1. Lagrange's equations for the wave motion of a fluid (Lagrange, 1788, Section II.9) provide an acceptable model when the temperature of the fluid is constant.

P2. The initial velocity of the particles of the fluid are small (enough that they may be modeled as initially tangent to their acceleration vector).

P3. The compression of a unit volume of air by a sound wave results in a positive change in density for that unit, call it s.

P4. A change in density of s per unit volume results in a *proportional* total change in temperature, call it $T = \beta s$.

P5. A change in temperature results in a *proportional* increase in elasticity per unit temperature, call it $100\gamma T$, or simply $100\gamma\beta s$.

Rather than enumerate an enormous list of rather banal physical assumptions, let us take such obvious assumptions as Newton's laws as implicit. Then the first two assumptions P1 and P2 provide a model of the phenomenon. Lagrange's derivation of the desired equations of motion from a least-action principle is significantly more economic than Newton's analysis, which made rather clumsy use of the pendulum law. Both models give rise to Newton's original speed of sound equation when isothermal conditions are assumed, and so Biot had good reason to choose Lagrange's method over Newton's in P1. Indeed, Biot defines his variables just as Lagrange does, following Lagrange's derivation line by line in producing a solution to the equations of motion[9]. Assumption P2 is then a claim about the initial conditions on this model.

The new assumptions that Biot introduces, indicated above in boldface, have describe the way that the elasticity of air changes under compression. Interestingly, Biot could have simply replaced P3-P5 (together with M1 below) with a simpler principle, which says that the compression s of a unit volume of air by a sound wave gives rise to an increase in elasticity of ks per unit temperature. Biot's more elaborate definition is an illustration of his central concern with relations between particular measurements.

Mathematical Conventions.

M1. In order to summarize P3-P5, define $k = 100\gamma\beta$, a constant specific to the medium of air. Then the a medium of initial elasticity ϕ that is compressed too a density of s per unit volume will have a resultant elasticity given by $e = (1 + ks)\phi$.

M2. Choose units such that the initial density of air is 1.

M3. Express elasticity ϕ as a function of density Δ as follows: $\phi(\Delta) = gnH\Delta$, where g is the acceleration due to gravity, H is the height of a mercury barometer, and n is the ratio of the density of mercury to that of air. Once we have assumed Newton's equations, this is indeed nothing more than a convention (for proof, see Appendix A).

The first two conventions are simply for ease of calculation, so let us make the following observation about M3. While M3 essentially follows Lagrange's definition, Biot has

[9] As a result, an essential strategy in reading Biot's paper for the first time is to read it side by side with Lagrange (1788, Section II.9).

added the term n, which amounts to an insistence that the barometer in his calculation be a mercury barometer. Lagrange's suppression of this term is natural from a mathematical perspective, in that it really doesn't matter what fluid is inside a barometer. One might as well assume for the purposes of calculation that the fluid is of the same type density as the substance that it is measuring. I take this as a further sign that Biot's central preoccupation is largely with the operational measurements of a phenomenon.

Idealizations

I1. Assume the density of a fluid through which sound travels is uniform.

I2. Assume that when a compression s is due to a sound wave, terms of order s^2 may be neglected in calculation.

I3. External forces (other than those effected by the passing sound wave) may be neglected.

Idealizations are a part of every mathematical model, and the three idealizations above turn out to be especially useful in Biot's calculation. I1 is approximately true in a small region of atmosphere such as that which a sound wave typically travels through. Similarly, I2 and I3 provide reasonable approximations in the case of a sound wave, and allow Biot to follow Lagrange in producing an analytic solution to the equations of motion.

3.2 A Guiding Principle?

Biot's assumptions certainly don't betray any particular emphasis on ontology, and his derivation does not seem to be guided by any particular ontological commitment as to the nature of heat either. Instead, if any guiding principle helped his hand, it would seem to be the principle that each quantity appearing in the derivation be carefully operationalized, that is, parameterized by particular measurable quantities. We saw this in two places in the discussion of Biot's assumptions above. But Biot's own discussion of his physical principles has this same flavor (see page 18-20).

Although Biot does give brief treatment of heating due to compression in the language of a caloric theory, he spends most of his energy discussing particular examples of it. He gives a long explanation of the behavior of thermometers in the chamber underneath a vacuum machine, followed by a discussion of ice deposits of the type mentioned earlier. This is not to suggest that Biot was an operationalist in the 20th century sense of the term. Rather, I claim that if any principle seems to have guided Laplace, it was careful attention to the measurement of a phenomenon.

4 Conclusion

Biot's derivation of the speed of sound equation could not have achieved success in its time. For when it came time to plug in the numbers and calculate the speed of sound, Biot in the end assumed that $c_\rho = c_v$, and produced a result that was inconsistent with experiment. However, the structure of his result was essentially correct, and it certainly constitutes the first quantitative calculation of the type that Laplace (1816) described.

Laplace's close association with Biot, together with the similarity of the calculation that Laplace (1816) described and Biot (1802) performed, suggest that Biot's calculation can actually provide some new insights into how the correction to the speed of sound equation was achieved. Those new insights, I have argued, are the following. The essential realization was indeed compression produces adiabatic heating. The trick in calculating this effect to assume a series of tiny increases in elasticity, add all of them up, and plug the new value for elasticity into Lagrange's wave equation. And if there was any principle that guided this feat, it was the careful attention to particular quantities measurable with barometers and thermometers.

Still, even this does not constitute complete explanation of how the speed of sound equation was discovered; Laplace's derivation of the ratio of specific heats is nowhere to be found. Thus, a more satisfying explanation awaits further research.

Appendix A

Biot's expression of elasticity as $\phi = gnH\Delta$ is a quantity that we now refer to as *hydrostatic pressure*. This is a convention based on the operation of a barometer, which may be seen through the following simple argument.

Recall that by Biot's definitions, ϕ represents elasticity, g the acceleration due to gravity, $n = \Delta_m/\Delta$ the density of mercury divided by the density of air, and H the height of a barometer. Let A be a lateral slice of area of such a barometer.

The force of gravity acting on the volume of mercury AH is given by $F_1 = \Delta_m AHg$, and the force acting on the mercury to air pressure is $F_2 = \phi A$. But this system is inertial, and so these two forces are equal:

$$\phi A = \Delta_m AHg.$$

Dividing by A and substituting $\Delta_m = n\Delta$, this reduces to

$$\phi = gnH\Delta$$

which is the desired result.

Appendix B

On the speed of sound in air and in water[10]

By M. Laplace

(Read before the Academy of Science on the 23rd of December, 1816.)

Newton, in the second book of Mathematical Principles of Natural Philosophy, gave the expression for the speed of sound. The manner in which he derived it is one of the most remarkable traits of his genius. The speed according to this expression is a little less than one sixth of the speed determined by careful experiment by members of the Academy in 1738, in experiments done with great care. Newton had already recognized this discrepancy in the experiments of his time, and tried to explain it. But modern discoveries about the nature of atmospheric air have destroyed this explanation, along with those proposed by various other mathematicians. Fortunately, these discoveries present us with a phenomenon which appears to me to be the true cause of the discrepancy between the observed and the calculated speed

[10] Translation of Laplace (1816).

of sound, which most mathematical physicists have now accepted. This phenomenon is the heat that air develops when it is compressed. When one raises the temperature of air while maintaining constant pressure, only one part of the caloric received actually produces this effect; the other part, which becomes latent, produces an increase in volume. It is this part which is released [se developpe] when one compresses the expanded air and reduces it to its original volume. The heat released by the approach of two neighboring molecules of a vibrating arial fiber elevates their temperature, and gradually diffuses into the air and the surrounding bodies. But this diffusion and propagation [irradiation] occurs extremely slowly relative to the speed of the vibrations, and one can assume without noticeable error that, in the period of a single vibration, the quantity of heat stays the same between two neighboring molecules. Thus the molecules, in coming together, are repelled all the more: first, because as their temperature is assumed to be constant, their mutual repulsion increases in inverse proportion to their distance; and second, because the latent caloric which is released [se developpe] increases their temperature. Newton had only considered the first of these two causes of repulsion, but it is clear that the second cause must increase the speed of sound, since it increases the elasticity of air. In entering this into the equation, I arrive at the following theorem:

> The actual speed of sound is equal to the product of the speed given by the Newtonian formula and the square root of the ratio of the specific heat of air under constant atmospheric pressure and varying temperature to its specific heat at a constant volume.

If we now assume, like many physicists, that the heat contained in a quantity of air under constant pressure and varying temperature is proportional to its volume (which cannot be far from the truth), the square root above becomes the square root of the ratio of the difference of two pressures to the difference of the quantities of heat which develop two equal volumes of atmospheric air with respect to these pressures, in passing from a given temperature to a lesser one, with the smallest of these quantities of heat and the smallest of these pressures being taken as units.

Wanting to compare this theorem to experimental fact, I was fortunate enough to find the observational conditions assumed by my theorem among the numerous results of the interesting work that Mr. La Roche and Mr. Berard have done on the specific heat of gas. These able physicists have measured the quantities of heat which are released from two equal volumes of atmospheric air when the temperature is lowered by about 80 degrees; one quantity is compressed by the weight of the atmosphere, the other compressed the same weight increased by thirty-six hundredths. They found that the heat released relative to the greater pressure was 1.24, with the heat relative to the smaller pressure being taken as a unit. In order to calculate the actual speed of sound it is thus necessary, by the previous theorem, to multiply the speed deduced by Newton's formula by the square root of the ratio of 36 hundredths to 24 hundredths, or by the square root of 3/2. At a temperature of six degrees, this formula gives 282.42 meters per second. In multiplying by $\sqrt{3/2}$, we get 345.45 meters per second. Members of the French Academy found it to be 337.18. The difference between these two results might amount to experimental uncertainty. But the smallness of this discrepancy unarguably establishes that the difference between the observed speed of sound and the speed calculated by the Newtonian formula is due to the latent heat released by the compression of air.

It is the result of all this that, given constant pressure, if one increases a given volume of air by raising its temperature, and then reduces it by compression to its original volume, it will release in this compression one third of the heat employed in its expansion. It remains for physicists to determine, by direct experiment, the ratio of the specific heats of air under constant pressure and at a constant volume; a ratio which we have just found to be 1.5. The speed of sound, as observed by members of the French Academy, gives 1.4254 for this ratio; perhaps, in light of the difficulty of direct experiments, this speed is the most accurate means of obtaining it.

I have concluded that the speeds of sound in rainwater and sea-water are equal to 2642.8 and 2807.4 meters per second. These numbers were calculated on the basis of the experiments of Canton on the compression of these liquids, and with regard only to the linear decrease of the dimensions of the compressed volume. I recognized that it is necessary to consider the total decrease of this volume, and thus that the preceding numbers must be divided by $\sqrt{3}$, reducing them to 1525.8 and 1620.9. Hence, the speed of sound in fresh water is four and a half times larger than in air.

Appendix C

On the Theory of Sound[11]

By the cit. Biot, associate of the National Institute

The cit. Laplace having suggested that I examine the influence that variations in temperature accompanying the dilations and condensations of air would have on the speed of sound, and if possible to seek through this consideration a reconciliation of experiment and calculation, I performed on this subject, after the aforementioned, the research that I am submitting to the Institute.

We know that the theory of Newton on the propagation of sound gives a velocity noticeably smaller than experience. The geometers have since formulated this theory in a more rigorous form, and they have arrived at the same results. It is not uncommon to see the author of the Principia produce in this way, as if through some sort of inspiration, consequences that more rigorous calculations have since almost always confirmed.

This agreement of the geometers in finding the speed of sound to be smaller than that given by experiment has made them think that either the calculations are founded on imprecise data, or that we have failed to account for some necessary phenomenon [circonstances]. One thus sought to supply plausible reasons for this discrepancy; however, we must agree that they were founded on more or less doubtful hypotheses, which can no longer endure after the discoveries of modern chemistry on the composition of the atmosphere. According to Newton, the observed difference was due to what was unaccounted for in the calculation of the volumes of the integral particles of air, which he regarded as [[174]] instantaneously transmitting sound through the medium of solid bodies. In assuming these molecules to be of nontrivial dimensions as compared to their separation, he thought that we needed to add [ajouter] the space that they occupy to that which gives the calculated speed of sound. Since this differs by one ninth from the experiments of which Newton made use, he gave air

[11] Translation of Biot (1802). Original pagination is indicated with [[brackets]]. Editorial comments given in footnotes.

molecules the density of saline substances, and by calculating their value on this assumption, arrived a little closer to the appropriate correction. But it is not difficult to see that this correction is founded on precarious hypotheses, and that appear discredited [infirmées] through the observation of a great number of phenomena.

Today, it is doubtful that air owes its elasticity to the nature and diameter of its particles; it is rather the caloric in which it is dissolved. Nevertheless, we accepted this hypothesis, in our ignorance of that which concerns the fundamental [intime] nature of bodies. Nothing supports the idea that the molecules of air have the same density as saline substances, and Newton could only have chosen this assumption because it led to the conclusion that he wanted to obtain. In fact, if he had given another density to these molecules, for example that of gold, he would have found their diameter 24 or 25 times smaller than their separation [distance mutuelle], which would have been insufficient [loin de suffire] to square experiment and calculation. Moreover, if the molecules of air occupy the ninth part of the interval that separates them, as Newton assumed, then the intensity of light through the atmosphere would have to be much weaker than it really is. And the condensations that we can bring about in this fluid, either through cold or by mechanical means, would have noticeably altered its transparence, which doesn't occur in any way whatsoever. Finally, if one notes that it is proven by several chemical facts that the even the densest substances still have an infinitude of pores, it appears natural to think that the dimensions of the molecules of air and of other elastic fluids are infinitely small in comparison to their mutual distances, and as a consequence that we must not attribute the failure of the theory to these considerations.

Newton did indicate the interposition of vapors suspended in air as another cause, though one of minor influence. He saw them as not playing any role in the movement of the fluid through which sound is propagated. It seemed to Newton that these circumstances, in diminishing the quantity of material to move, must increase the speed of sound; in assuming that atmospheric air [[175]] was one tenth vapor, he deduced an increase in speed of one twentieth. But this cause, which is less hypothetical than the aforementioned, was by itself far from enough to explain the failure of the theory, and moreover was rebuked by experiments that members of the Academy of Sciences performed in 1738; it was found that the thickest fog did not alter the speed of sound in any noticeable way. I stopped arguing against Newton's explanation, because it seemed to me to be contrary to the discoveries of chemistry in recent times. However, no matter how strong the proofs that I have given might appear to me, I propose them only with hesitation [defiance]. When one believes to have discovered an error in the work of a man so great, and such a sage observer of nature, one must doubt and study for a long time, for fear of erring oneself.

One also finds some research by Lambert on the subject that interests us, in the 1768 Memoirs of Berlin. He argued against various hypotheses that had been drawn on to try to bring theory and experiment into accord, but then proposed another that is no more admissible. He took atmospheric air to be charged with a quantity more or less like vapor, and other heterogeneous materials much heavier than air itself. These small aqueous or saline masses, but without elasticity, are supported by the force of cohesion between the molecules of the atmosphere alone, like little drops of mercury on water. And their specific heaviness being 700 or 800 times greater than that of the molecules of air, they appear, because of their small volume, to lodge themselves in the spaces between the former [ces dernières]. These strange aqueous particles would only hinder and suppress the undulation of air, without themselves contributing to the propagation of sound, and as a result they would have to be removed in the expression of the density of air that was introduced in the calculation.

All this assumes that water dissolves in air, is always in a liquid state there, and has the proper specific heaviness for this state; as it happens, the contrary is now well proven. We know that air becomes lighter in dissolving water, and as a consequence this liquid, in so passing to the elastic fluid state, takes a volume of which the specific heaviness is less than that of air itself.

Lambert, halted by the difficulty of separating the weight of [[176]] air from that of the vapors that he assumed to be suspended there, took the opposite approach, and set himself to determine, after the experiments made on the propagation of sound, the quantity of vapor suspended in the atmosphere. He found that they must form more than a third of its weight, does not match experiments that have since been made on the composition and decomposition of this fluid, as it exists on the surface of the earth.

Lambert again published a memoir in 1772, where he was concerned with both the speed of sound and astronomical refractions. But it is based on the same hypotheses that we have just reviewed, and he even employs those results in his calculations.

After having spoken of the attempts that have been made to reconcile the theory of the propagation of sound with experiment, I am going to part from the well-established facts, and expose a cause by which it seems very possible to explain the discrepancy. But I must first explain the principles on which the calculation is based.

These principles are very simples; we know from experience that the elasticity of air is proportional to its density. On the basis of this fact alone, we calculate the speed with which undulations must propagate in this fluid, and we find a result of 915 feet per second, while experiments find it to be 1033.

Since there is nothing in the derivation that is not rigorous, it must necessarily be that the law that serves as its basis is in need of modification, at least when we apply it to the successive condensations and rarefactions of air in the formation of sound.

It is a known fact to physicists that when we condense atmospheric air, it loses part of its latent heat, which passes into the state of sensible heat; and that on the other hand when we rarify it, it takes back a portion of the sensible heat, which it converts to the state of latent heat. The mercury in the thermometer under the container of a pneumatic machine falls several degrees when we empty it, and on the other hand it increases under the container of the machine during compression. These effects cannot be attributed to the dilation or the compression of the bowl of glass of the thermometer subjected to the experiment, for I am assured that if we let the condensation or the vacuum remain for some time, equilibrium is reestablished, and the mercury returns to its previous degree. In order to get an idea of the temperature that air takes in these experiments, and of the quantity [[177]] of heat that is absorbed or released [dégage], one must observe that in general, the thermometer only exactly indicates the degree of heat of a body with which it is in contact to the extent that one can regard it as infinitely small with respect to this body, and neglect the influence of its presence on the temperature that one wants to observe. As it happens, that is far from true, when one carries this out under the container of a pneumatic machine or of a compression machine. So even a thermometer that is very-small, for example like those that are adapted to hygrometers, still has a considerable mass in comparison to that of the volume of air in which it is immersed. It also happens that for equal dilations, the variation of the thermometer decreases with the dimensions of the containers, so that in taking them to be strongly-small, it becomes completely unnoticeable. If in addition we account for the influence on the reestablishment of equilibrium that must come from that the partitions of the containers, and the machines themselves of which we make use, we can easily sense that the changes in temperature effected by this little mass of air must be considerable, since they make the thermometer that is immersed there vary by several degrees.

These effects appear great in the mines, where a great quantity of condensed air is employed by the refinement apparatuses. When contact with the surrounding atmosphere is reestablished, the air absorbs enough heat so as to deposit a part of the water dissolved there in the state of ice, in the dilatation that restores it to its natural state.

In the propagation of sound, the successive condensations and dilatations of air must necessarily cause, in the particles which they effect, some very-small variations in temperature of the same class, analogous to those whose existence we have just recognized; and these variations must influence their elasticity. As a consequence, the law according to which the resilience [ressort] of air is proportional to its density only appropriate in the state of rest, when we let the fluid return to the temperature that it had before the change in volume that it was given. And in the state of movement, where condensations and rarefactions occur in short intervals, it becomes necessary to take account of the corresponding variations in temperature.

It is calculation that can make us appreciate the influence of this cause on the speed of sound with exactitude. But before establishing this, it is necessary to determine the quantity of heat that becomes sensible in a given condensation of air, or more precisely, [[178]] the part of this heat that raises the temperature of the fluid and influences its elasticity, for it is possible that a portion of it escapes in radiant form; since the latter always happens in a change of temperature, the released caloric [calorique dégagé] does not encounter a body capable of stopping it and absorbing it.

With the means that we have to rarify and condense air, it would be extremely difficult to directly evaluate the quantity of heat that it absorbs or that it emits [dégage]. However, the corresponding variations in temperature must increase with the changes that are effected on the volume of air; we will regard them as proportional to these changes. This will be observably true, above all in the theory of sound, where the condensations and rarefactions are comprised of limits of small extension, and we will then try to determine the coefficient of this proportionality, on the basis of the experiments done on the propagation of sound.

Theory of the propagation of sound in taking account of the heat developed by the changes in volume of the particles of air.

Let p, q, and r represent the speed of the particles of the fluid parallel to three rectangular axes x y z, speeds that we assume to be very-small. Let us call ϵ the elasticity of the particles, δ their density in the state of movement, and X Y Z the forces applied to them. We will have the following three equations to determine the undulations of the fluid:

$$\Delta\left(\frac{dp}{dt}+X\right)+\frac{de}{dx}=0; \Delta\left(\frac{dq}{dt}+Y\right)+\frac{de}{dy}=0; \Delta\left(\frac{dr}{dt}+Z\right)+\frac{de}{dz}=0. \tag{4}$$

$$\frac{d\Delta}{dt}+\frac{d\Delta p}{dx}+\frac{d\Delta q}{dy}+\frac{d\Delta r}{dz}=0. \tag{5}$$

(See the *Mecanique analytique*, page 496 and on[12].)

When we disregard heat, the elasticity ϵ is a function of density Δ, and if in addition $Xdx+Ydy+Zdz$ is an exact differential, the preceding formulas are reduced to the integral of a single partial differential equation. We ask if it is the same, when we go beyond [a égard aux] these circumstances that have been indicated.

Let us suppose that in a constant state, the elasticity is [[179] some function of the density, call it $\phi(\Delta)$. We represent the density of the particle of air under consideration by Δ', before it is perturbed [ébranlée]. Since the duration and the extension of the vibrations are assumed to be very-small, the molecule will pass suddenly from the density Δ' to the density Δ, and we will have:

$$\Delta=\Delta'(1+s) \tag{6}$$

[12] See (Lagrange, 1788)

s being the strongly-small [fort-petite] quantity, which will represent the condensations when it is positive, and the dilations when it is negative.

Let β' be the variation in temperature corresponding to a very-small condensation, as for example $1/100$ would be, β' being expressed in thermometer degrees, so that the variation corresponding to the small condensation s will be very nearly represented by $100\beta' s$, or simply βs by defining $100\beta' = \beta$. If in addition γ represents the increase in elasticity for a small variation in temperature, as would be, for example, $1/100$ of a degree, then $100\gamma\beta s$ will represent the increase in elasticity for the condensation s, an increase that can be expressed by ks, in defining $100\gamma\beta = k$.

In virtue of these modifications, the expression of the elasticity ϵ during the movement will become

$$\epsilon = (1 + ks)\phi(\Delta). \tag{7}$$

We have in addition

$$\Delta = \Delta'(1 + s). \tag{8}$$

If then the initial density Δ' is variable for different particles of fluid, like for example that which takes place in fluids under the action of weight, ϵ will not be a function of Δ alone; consequently, the function $\frac{d\epsilon}{\Delta}$ will certainly not be an exact differential, and it would no longer be of any use in integrating the equations 4 and 5.

Following other authors who have treated the theory of sound, we assume in the remainder of this work that the initial density of the fluid is uniform and equal to unity. This is the case for the atmosphere, when one considers no more than one layer of little thickness, and for this reason it is also the case for the propagation of sound in the horizontal direction.

So in calling g gravity, n, the ratio of the density of mercury to that of air, and H the height of the barometer, we have[13]:

$$\phi(\Delta) = gnH.\Delta \tag{9}$$

[13] Following a common convention of the time, Biot sometimes uses a period a (.) in mathematical expressions as shorthand for parentheses around the term that follows it.

[[180]] and since $\Delta = 1 + s$, this implies

$$\epsilon = gnH(1 + ks)(1 + s) \tag{10}$$

which, in neglecting quantities of order s^2, reduces to $\epsilon = gnH(1 + (1 + k)s)$. One derives from this

$$\frac{d\epsilon}{\Delta} = gnH(1 + k)\frac{ds}{1 + s} \tag{11}$$

which, integrating and simplifying, gives[14]

$$\int \frac{d\epsilon}{\Delta} = E \tag{12}$$

$$E = gnh(1 + k)ln(\Delta) \tag{13}$$

So if we multiply the first term in equation 4 by dx, the second by dy, the third by dz, and if we insert them, in assuming $X = 0$ $Y = 0$ $Z = 0$ since we are neglecting accelerative forces, we have

$$-dE = \frac{dp}{dt}dx + \frac{dq}{dt}dy + \frac{dr}{dt}dz. \tag{14}$$

The second expression[15] must then be an exact differential, which demands that the function $pdx + qdy + rdz$ enjoys the same property. This condition will be satisfied in the case at hand, since the initial speeds p q r are considered to be very-small. Thus, let

$$pdx + qdy + rdz = d\phi. \tag{15}$$

From this we derive $p = d\phi/dx, q = d\phi/dy, r = d\phi/dz$, and as a consequence we will have[16]

$$E = -\frac{d\phi}{dt} \tag{16}$$

[14] E here serves as a dummy variable introduced by definition, following the notation of Lagrange (1788, 498). Note also that, like Lagrange, Biot's original notation for the natural logarithm of Δ is written $l.\Delta$. Here and throughout this translation, I have replaced this notation with $ln(\Delta)$.

[15] Here, Biot is referring to the entire right side of equation 14.

[16] In the original text, equations 16 and 17 were printed so close together that they seemed to possibly be part of the same expression. However, study of the calculation shows that Biot clearly intended them as two separate equations, and so this is clarified here by placing the them on two separate lines.

$$ln(\Delta) = -\frac{1}{gnH(1+k)}\frac{d\phi}{dt}. \tag{17}$$

However, equation 5 can be put in the form

$$\frac{d.ln(\Delta)}{dt} + \frac{d.ln(\Delta)}{dx}p + \frac{d.ln(\Delta)}{dy}q + \frac{d.ln(\Delta)}{dz}r + \frac{dp}{dx} + \frac{dq}{dy} + \frac{dr}{dz} = 0 \tag{18}$$

and thus, in substituting in the values of $ln(\Delta)$, p, q, r, and neglecting second order quantities, we have

$$\frac{d_2\phi}{dt^2} = gnH(1+k)(\frac{d^2\phi}{dx^2} + \frac{d^2\phi}{dy^2} + \frac{d^2\phi}{dz^2}). \tag{19}$$

(For these formulas, see *Mécanique analytique* by Lagrange, second part, p. 500.)

In taking k to be null, we would have the ordinary equation for the propagation [[181]] of sound, which has been given by Euler and Lagrange, in neglecting the heat rendered sensible by the condensation o f air. This consideration has the sole consequence of augmenting the coefficient gnH in the ratio of $1+k$ to unity, k always being a positive number. Yet we know, and it can be demonstrated directly, that it is on this coeffecient that the speed of the propagation of sound depends, which is expressed by $\sqrt{gnH}$ when heat is neglected. As a consequence, our formulas will give for this speed

$$\sqrt{gnH}\sqrt{1+k}, \text{ or } (915 \text{ feet})\sqrt{1+k}. \tag{20}$$

It will thus be larger than in ordinary theory; but to appreciate this difference, it is necessary to know the value of k.

We do not yet have any direct experiences that make known the variations of the elasticity of air for very-small changes in temperature, such as those that enter in the preceding formulas. We only know, from the experiences of Amontons, that the resilience [ressort] of air increases by one third for an increase in temperature equal to 80° by Réamur's thermometer. If the rate [marche] of this increase is assumed to be uniform, which is the simplest assumption, then one will have 1/240 for each degree, which gives $100\gamma = 1/240$, and consequently $k = \beta/240$. β is thus the variation in temperature corresponding to a compression equal to unity. We admit along with some physicists that the bodies in the dilations or the condensations that we produce release [dégagent] or absorb enough heat so that it would be necessary to remove it from them or give it to them, in order to reduce them naturally to the volumes that we make them occupy. In this case, if it is assumed that all the heat rendered sensible by compression goes toward raising the temperature, without being dissipated at as radiant heat, it will be easy to determine β. For it was found in some very-precise experiments by Guy-Lussac on the dilation of gases, that an increase of 80° in temperature gives a dilation equal to 0.35, such that in the preceding supposition one would have $\beta = 80°/0.35 = 228°$. On this hypothesis, this would be the heat that becomes sensible when air is condensed by double. One would thus have $k = 0.95$, the speed of sound thus given by $915\sqrt{1.95} = 1227.73$.

[[182]] This result is much greater than that which is given by experience, and the difference might come from the hypothesis, which we have just admitted is not completely exact, or from one part of heat rendered sensible by compression dissipating in radiant form without raising the temperature of air. But this example is still appropriate in showing the great influence of this cause, and how much it is necessary to take account of it here.

If it is not possible for us to directly appreciate with exactitude the quantity by which the temperature of air varies for a given compression or dilation, we can determine it with great precision with the help of the preceding formulas, and in departing from experiments made on the propagation of sound. If 1038 feet is taken for this speed, which is the value found in 1738 by the members of the academy of sciences, one will have

$$1 + k = \frac{(1038)^2}{(915)^2} \tag{21}$$

$$\text{so } k = 0.2869, \tag{22}$$

$$\text{which gives } \beta = 68.856° \tag{23}$$

which is to say that when the volume of air is condensed by double, its temperature is decreased or increased by about 69 degrees by Réamur's thermometer; and this quantity will not appear too large as compared to that which the thermometer tells us, if it is recalled that we only deal with very-small quantities of air in contact with highly conductive heat walls [parois], and that the thermometers that we make use of still have a considerable mass, as compared to that of the air where they are immersed.

Other results on the speed of sound will give other results on the quantity of heat. For example, in assuming this speed to be 1080 feet, which is the largest that has been found, one finds

$$k = 0.3922 \tag{24}$$

$$\beta = 94.3 \tag{25}$$

and that the variation in temperature would surpass 94°. We will adopt the first result as preferable, because it is deduced from a greater number of experiments made with care and with appropriate devices. But we see through this correspondence, between the increase in the speed of sound and that of the heat that compression renders sensible, how necessary it is to take account of the link between these two phenomena, and how easy it is to reconcile calculation and experiment through this consideration, in one of the most important theories of mathematical physics.

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