REFLECTIONS ON STRINGS

Mark Wilson

> There is hardly a paradox without utility. Thoughts that matter little in themselves may give occasion to more beautiful ones.
>
> —*Leibniz*[1]

THOMAS Kuhn, in his influential "A Function for Thought Experiments,"[2] poses a dilemma:

> [In] a real thought experiment, the empirical data upon which it rests must have been both well-known and generally accepted before the experiment was even conceived. How, then, relying exclusively upon familiar data, can a thought experiment lead to new knowledge or to a new understanding of nature?"

He correctly rejects the thesis that thought experiments invariably operate to expose prior conceptual confusion. With respect to his chosen example—Galileo's "trough paradox"—Kuhn argues that the older conceptions of motion which Galileo sought to dislodge could in fact be *correct* within a possible world simpler than our own. Hence, by normal criteria of self consistency, "conceptual confusion" cannot be the sole explanation of a thought experiment's efficacy. Kuhn accordingly turns to the background empirical data. Such facts, he argues, may sometimes resist successful accommodation within our accepted conceptual framework, although we can be blind to this discrepancy until the thought experiment brings it to our attention. Kuhn is thus able to link thought experiments to the conceptual changes he later labeled "paradigm shifts."

This implicit dichotomy between conceptual muddle and empirical adequacy, however, cannot handle the full range of thought experiments which have arisen in the history of physics. There are cases, one of which I'll discuss here, which seem fueled almost entirely by considerations of mathematical harmony. Typically no question of conceptual confusion *per se* is at stake. In our example, it will be the position *dislodged* by the thought experiment[3] which has the greater claims to rigor and conceptual clarity. Contrary to Kuhn, overlooked empirical considerations seem unable to explain the puzzle's efficacy either. Indeed, many writers will assert, on *a* *priori* grounds, that no empirical data could have any conceivable bearing on our puzzle.[4]

In fact, Kuhn would clearly wish not to be saddled with the conceptual muddle/overlooked data dichotomy, simply because it smacks too much of the analytic/synthetic distinction which he rejects. Unfortunately, we do not seem to have yet found the tools in philosophy of language with which to characterize adequately the workings of most armchair experiments. At present, we seem forced to utilize distinctions which, in more philosophically enlightened moments, we would prefer to reject. As matters now stand, the shortfall inherent in Kuhn's analysis of thought experiments provides philosophy of language with both a puzzle and an important clue for future research. My purpose in this note is simply to sketch the key features of a classic thought experiment which illustrates the undiagnosed elements neatly.

The outcome of a better account of thought experiments might probably contribute to a more adequate historiography of science as well. It is notorious that Kuhn's focus upon "conceptual revolutions" tends to minimize, albeit inadvertently, the importance of the great advances on the mathematical side of physics realized in the Eighteenth and Nineteenth centuries. Typically this work is all swept under the rug of "normal science." This dismissal is apt to puzzle anyone who has seriously looked at what the Nineteenth century physics actually proposed in its physics—Kelvin's "vortex atoms"; Abraham's massless universe, etc.[5] Such ideas, evaluated in terms of crude shock value, seem as "revolutionary" as anything which came before or later. What is true, however, is that such ideas naturally suggest themselves as one works out the mathematical implications of continuum physics (thus Kelvin was inspired by Helmholtz' vorticity theorem). What is left out in Kuhn's picture of science, it seems to me, is an awareness of how easily the dogged pursuit of mathematical rigor alone can lead one naturally into the most novel and unexpected terrain. The thought experiment I will now describe illustrates this pattern in a simple[6] if somewhat circumscribed way.

In the 1740's, Jean d'Alembert and Leonhard Euler both worked on the theory of the vibrating string[7], such as found in a guitar or violin. They both agreed on the proper equation for the system (discovered by d'Alembert); their "thought experiment" puzzled about what should happen, according to their equation, if a string was released from a *plucked* initial position, that is, with a triangular starting configuration. This problem had already been raised in 1713 by Brook Taylor who concluded that "the adjusting process [was] beyond the scope of mathematical description."[8] Most general histories of mathematics include some discussion of the Euler/d'Alembert dispute, usually with emphasis on the role the problem played in the slow historical progression to the modern treatment of function.[9] With respect to issues of *functionality*, d'Alembert seems unduly dogmatic, proceeding "on grounds so arbitrary that it would be unjust to metaphysics to call them metaphysics."[10] Our concern, however, is with another side of the dispute, where d'Alembert seems (almost) totally reasonable. In fact, with the most minimal modifications, one can find defenses of d'Alembert's position in many modern texts, without much indication that any alternative view is viable.[11] Nonetheless these same texts frequently apply theory in ways compatible only with Euler's more liberal stance. Without prior warning, a modern physics student can be easily brought to feel the bite of the d'Alembert/Euler puzzle. I will accordingly present the thought experiment in contemporary terms, dropping purely historical aspects of the original dispute.

Our problem is perhaps the simplest of a broad class of cases where an unexpected disharmony arises between *analytical* and *geometrical* descriptions of nature. Galileo optimistically announced the new era of mathematical treatments of nature in these famous words from *Il Saggiature*:

> Philosophy is written in this great book which is always open before our eyes—I mean the universe—but it cannot be understood unless one first learns the language and distinguishes the characters in which it is written. It is a mathematical language and the characters are triangles, circles and other geometrical figures, without which it cannot be understood by the human mind; without which one would vainly wander through a dark labyrinth.[12]

Galileo's beloved triangles lead to unexpected difficulties in many physical contexts. Euler's equations for non-viscous fluids are expressed in terms of the derivative of the local velocity of the substance. A solid obstacle placed in the stream will cause the fluid to flow around it. When the fluid rejoins itself in the wake of the obstacle, the flow from each side will have acquired different velocities, leading to a line of velocity discontinuity in the wake.13 This means that the original equations, which predicted this flow, must be nonsensical (through lack of a well defined velocity derivative) at the line of discontinuity. So a combination of natural geometrical assumptions plus our analytic description (Euler's equations) lead to a breakdown in the conditions necessary for the validity of the equations. In this case we can conclude that Euler's equations do not have unlimited validity. Our string case begins with a conflict of this kind, but the resolution is rather different.[14]

The dispute between d'Alembert and Euler likewise involves the unexpected failure of a necessary derivative. In his first paper, d'Alembert looked at a small section of a stretched string and applied a differential form of Newton's Second Law:

$$df = P \frac{d^2y}{dt^2} dx,$$

where P is the density (assumed constant) and y is the transverse displacement of the string. Under plausible assumptions about the tension T of the string, d'Alembert found the magnitude of the force to be

$$T\frac{d}{dx}\left[\frac{dy}{dx} \Big| \sqrt{1+\left(\frac{dy}{dx}\right)^2}\,\right]dx$$

The resulting equation is nonlinear and hard to solve, but if the slope of the string remains small,

$$\left(\frac{dy}{dx}\right)^2$$

can be dismissed as negligible. Setting $\frac{1}{c^2} = \frac{P}{T}$ as we derive the ubiquitous (one dimensional, linear) wave equation: $\frac{d^2y}{dx^2}$.

D'Alembert's achievement here was of great historical importance, because of the early introduction of a *partial* differential equation and the light it sheds on Newton's Second Law, (*a.k.a.* the Momentum Principle).[15] Although one finds a repetition of this derivation in most introductory texts, conceptually it is rather confusing. In particular, d'Alembert has assumed that the operative forces will always push portions of the string vertically back and forth in the y-direction. Why shouldn't the string sometimes displace *longitudinally* in the x-direction as well? A more satisfactory approach will allow such motions, but its derivation will call upon more sophisticated physical principles.[16]

D'Alembert also found the general solution for his equation, which Euler subsequently improved through a representation in terms of initial conditions, *viz.*

$$y(t) = \frac{1}{2}\left[g(x+ct) + g(x-ct)\right] + \frac{1}{c}\int_{x-ct}^{x+ct} h(z)dz$$

where g is the string's initial configuration and h its initial velocity. Apparently, Euler was inspired to seek such a presentation through analogy with the data requirements of ordinary differential equations, a perceptive if not totally trustworthy insight. Now the "thought experiment" is this: what happens if one released a string plucked at spot *A* from rest? Suppose the string is infinitely long.[17]

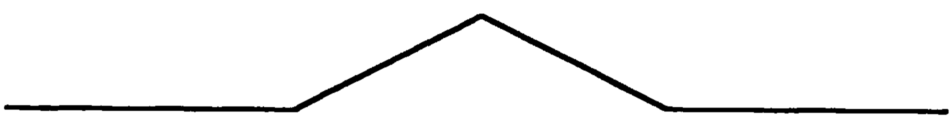

FIGURE 1

Euler's solution tells us that two half sized copies will depart to the left and right:

FIGURE 2

We get the table-top figure in the period when the two little triangles overlap. If the string is fixed at two points, Euler's solution predicts (in a manner to be discussed later):

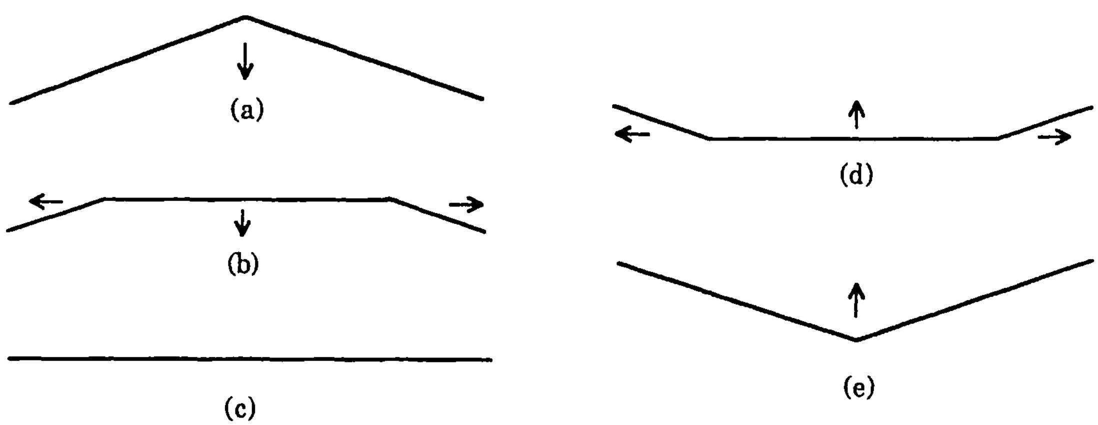

FIGURE 3

This may be surprising; what about the sine wave forms which plucked guitar strings are supposed to display? Real life strings are subject to further damping forces which we didn't include in our force calculations; such forces can reduce the string quickly to an approximately sin wave configuration. But without damping forces, d'Alembert's string can't lose energy and will follow its jerky pattern forever.

The predictions we have just made were based upon Euler's *solution* to the wave equation. But look again at the differential equation itself; it seems to require that at every spot x the operative force on the string should be proportional to

$$\frac{d^2y}{dx^2}.$$

But at the plucked spot (and at both subsequently moving corners) this quantity is not defined because even

$$\frac{dy}{dx}$$

is undefined. So Euler's plucked string "solution" seems to violate—or, at least, render nonsensical—the very equation it was supposed to solve! Worse yet, the total effective force making the string move must be concentrated at the problematic corners (there is no force where the string is straight). Yet it is exactly here that d'Alembert's equation doesn't apply. Furthermore, the corner must be experiencing an infinite acceleration, or else the applied force would be zero.[18] So it looks as if Euler is relying upon a differential equation that completely misdescribes all relevant forces acting upon the string. This, in essence, is d'Alembert's objection to Euler. It is easy to see, incidentally, that the same objection applies to strings with shapes less extreme than our corners, e.g., joined parabolas with continuous first derivatives but jumps in the second derivative. Much of the actual discussion between d'Alembert and Euler concerned these smoother cases.

Before considering how d'Alembert's objections can be answered, we should first ask why Euler (or anyone else) should *care*. His own explicit considerations frequently hinge upon expedience—the wave forms which are easiest to study happen to violate d'Alembert's strictures. Jan Mikusinski supplies a modern defense of this nature:

> We could say that in reality the string never bends at an acute angle and [y(x,t)] can always be regarded as a function with continuous second derivatives… In practice, however, such bends often lie within the limits of measurement error: consequently it is unprofitable to consider minute details, particularly since their introduction to the calculation would complicate it greatly without making the final result any more accurate. Under such conditions it is even desirable to neglect in the calculations slight disturbances which are not essential to the characterization of the whole phenomenon.[19]

In particular, in Euler's study of reflection waves such as echoes (see below), it proved most convenient to study the movement of very simple shapes violating d'Alembert's smoothness requirements.[20] In fact, Euler actually employed a primitive version of the δ-function in such a context.

Euler's position can be strengthened in a variety of ways. If one had a solid triangle to work with, why couldn't one *mold* the string into the forbidden initial position? Real life strings possess some stiffness which resist bending, but such forces were not included in our differential equation. Hence the string has no mechanism to straighten itself out. This argument is closely allied to the fluid mechanics paradox discussed above (the analogy is even closer for the assignment of geometric boundary conditions to the two dimensional wave equation, such as applies to a stretched membrane).

One of the great early successes of the wave equation was its ability to successfully describe wave reflection from a fixed end point, a problem which had puzzled physicists for some time. Suppose a wave is traveling towards an end point A.

FIGURE 4

D'Alembert and Euler constructed an imaginary continuation of the string beyond A thus:

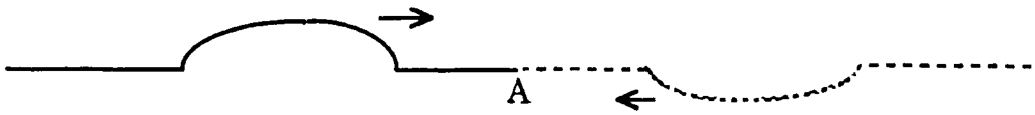

FIGURE 5

Notice that the entire figure satisfies the wave equation everywhere. As the original wave form moves right past A, the imaginary inverted wave on ones left and becomes "real." This technique can be used to successfully predict a wide variety of reflection behavior. The key to its success lies in keeping the wave equation true everywhere. But consider point A. Some force, undescribed in the wave equation, must be responsible for keeping A fixed. D'Alembert, if he is to be consistent, must object to the foregoing treatment of reflection if his objection to corner solutions is sound. But then his theory will be robbed of its greatest success! Accordingly, if we overlook the wave equation's lapse at A, we should pardon the corner as well.[21]

Finally, if we were to return to the original wave equation, without the simplification that

$$\left(\frac{dy}{dx}\right)^2 \approx 0$$

we would find that it almost always converts smooth initial configurations (i.e., with the right number of derivatives) into configurations which lack them.[22] In short, objectionable corners will spontaneously form even from initial conditions which lack them. Such behaviors are called "shocks" after the related phenomenon in gas dynamics. The unavoidability of such behavior was not realized until a 1848 paper of Stokes (and he was mistakenly talked out of it by Kelvin).[23] The theory was taken up in earnest by Riemann, Rankine and Huginiot and has continued to attract the interest of eminent mathematicians to this day. The deepest reasons for allowing Euler's corners stem from the requirement that physics must accommodate shock waves, although neither d'Alembert nor Euler were aware of such considerations. With d'Alembert's *linear* form of the wave equation, shocks do not form spontaneously but once created, Euler's general solution describes how they will move.

But merely having a *motivation* for a set of solutions doesn't entitle one to them. To get a sense of where we now stand *vis á vis* our thought experiment, let us ask ourselves whether the string might assume a configuration with *no* derivatives anywhere, as in Weierstrauss' famous construction. If we allow the possibility, Euler's general solution will move it along the string just as easily as it moves the corner configuration. This possibility may sound completely wild, although some writers have suggested that Brownian motion might knock the string into such a shape.[24] But it is hard to see how the logic of Euler's position prevents such possibilities.

I might parenthetically interject that historically the majority of mathematicians were converted to Euler's corner solutions through their almost surpetitious appearance in the guise of Fourier series.[25] Such considerations are rather extraneous to the logic of the dispute however.

How did Euler justify his solutions? His own answer is rather unclear—he claims that the sharp corners wouldn't behave very differently from a legitimate, smoothly rounded corner:

> If one were to dull the corner in the [problematic] derivative an infinitesimal amount and properly rejoin the two points, one would be able to evade the source of all these difficulties. The change which this would bring to the original line will be infinitely small and will change nothing in the original state of the string nor that which determines its motion.[26]

As it stands, this sounds like a justification for *neglecting* strings with corners, on the grounds that their omission would make no difference. In fact, as we saw in the Mikusinski quote above, it is often very hard to determine whether a scientific author is defending a class of solutions as *idealized* approximations to the world's true solutions or claiming that the extension class represents the proper set of real solutions. Each position requires defenses of quite different sorts. For *idealization*, one must show that one's "idealizations" remain close, in some sense, to the behavior of the real solutions. For *extension*, one must try to reformulate one's underlying physical principles so that the extension class becomes a natural product of these principles. Although it is virtually certain that, if pressed, Euler would favor the extension view—see the quote from Truesdell below—he nonetheless frequently accepts justifications more in line with an idealization defense.

In particular, Lagrange in 1759 worked out an atomistic model of the string and "derived" d'Alembert's equation in a limit as the number of masses go to infinity. He claimed—and Euler applauded him for it—that this showed corners could appear in the limit because the lines between the atom's centers in each finite model were polygonal. Of course, one could prove that a circle has corners by this sort of argument. In Gallavotti's vigorous modern presentation of Lagrange's model, one emerges with *infinitely differential* (C∞) strings in the limit—a requirement which would sustain d'Alembert even at his most "unreasonable." Obviously, the mere taking of "limits," without further checks, gives one great leeway to introduce or suppress all sorts of desired qualities.[27] If one really believes in a Boscovitchean atomistic universe—as Lagrange and Euler did not—then some more plausible finite model might be used to justify the wave equation as an idealization. But if one keeps to a continuum view of physics, Lagrange's derivation represents a step backwards from d'Alembert's.

The presumption that classical mechanics should be founded upon a point mass foundation is encountered frequently in philosophical circles and textbook presentations. However, the mere fact that the *real* world breaks up into atoms does not entail that the *classical* world must, especially if the only way that classical macroscopic materials such as strings can then be granted their expected classical properties is to acquiesce in "limiting case" arguments of dubious character. Many of the objections of Duhem *et al.* against loose appeals to "molecular" models in Nineteenth century physics were concerned with this lack of rigor and, as such, remain sound.[28]

A more promising approach in the "idealization" tradition is to blame the problem on the dissipation and/or stiffness terms we left out of the equation. Such terms keep real strings rounded (sometimes) but we can neglect them in an "idealization" if their contribution is small. The classic work in this vein is by L. Oleinik. It is hard to see why nature will always conspire to include such ameliorating terms however.

Behind some of Euler's considerations is the presumption that his extension solutions will always be close in behavior to regular solutions. In the case of shocks, this is not true—shocks move through the string faster and force entropy changes which smooth solutions do not.

It might be expected that d'Alembert at least would have favored the "idealization" view. Actually he did not; he claimed that initial shapes with corners must be "the most ordinary, and perhaps the only ones that have ever existed for vibrating strings."[29] His considered opinion was that his own equation was worthless for describing nature! This was a general theme of his:

> Geometry, which should only obey physics, when united to the latter sometimes commands it. If it happens that a question which we wish to examine is too complicated to permit all its elements to enter into the analytical relation which we wish to set up, we separate the more inconvenient elements and substitute for them other elements less troublesome, but also less real. We are then surprised to arrive, notwithstanding our painful labor, at a result contradicted by nature, as if after we have disguised, truncated or mutilated it, a purely mechanical combination will return it to us.[30]

As Truesdell remarks, d'Alembert seemed to work out equations only to prove the worthlessness of an analytical description of nature.

In any case, Euler clearly favors the extension view, despite his occasional lapses, into "idealization." Truesdell comments:

> The modern answer [to our thought experiment] is that the basic field laws are integral equations: at points where their solutions are sufficiently smooth, these integral equations are equivalent to differential equations, but at singular loci they give rise to algebraic conditions which restrict the limiting values of the solutions of the differential equations as the singularity is approached. This is the only basic idea of continuum physics which Euler did not have...[His official response is inadequate but] in practice Euler actually uses a quite different idea. Once he has the general solution, he discards the differential equation altogether. That is, Euler takes the functional equation, rather than the differential equation, as the complete mathematical statement of the physical principles of wave propagation, apart from boundary and initial conditions... We may formalize his approach as Euler's *extension principle for physical laws*: In regions of sufficient smoothness physical laws are to be stated by differential equations; let their general solution by arbitrary functions be regarded known; then it is the resulting functional equations, with the arbitrary functions perhaps only piecewise smooth, which are to be taken as the general mathematical statement of the original physical laws. Because of the now customary and more general method of integral equations, this extension principle has only historical interest. But from it we see that Euler, in addition to being the first to conceive a field theory ruled by partial differential equations, was also the first to realize that these equations can fail to be sufficient.[31]

Unfortunately one cannot generally rely upon general solutions for extending differential equations, for they are rarely forthcoming. A popular modern technique relies upon integration by parts (or Green's theorem) to extend a large family of equations. Let us restrict our attention to a string with fixed ends *A* and *B*. Multiply the wave equation by an arbitrary smooth function Ø which vanishes (along with its derivatives) at *A* and *B* and integrate. Note that Ø is not required to be a solution of the wave equation itself. Result:

$$\int_a^b \int_{t_0}^{t_1} \varnothing \left[\frac{d^2y}{dx^2} - \frac{1}{c^2}\frac{d^2y}{dt^2} \right] dtdx = 0$$

Integrating by parts and dropping terms vanishing because of Ø and y's behavior at *A* and *B*, we get

$$\int_a^b \int_{t_0}^{t_1} y \left[\frac{d^2\varnothing}{dx^2} - \frac{1}{c^2}\frac{d^2\varnothing}{dt^2} \right] dtdx = 0$$

The differential operator has been transferred over to the unproblematic Ø. Any regular solution y will make this formula true for all admissible Ø. If we now allow the class of "weak solutions" to include any y with this property, whether it is a regular "strong" solution or not, we will have extended the wave equation in a rigorous way which will now encompass Euler's corner solutions. In fact, if distributions are introduced in the manner of Laurent Schwartz, the old wave equation can be reinterpreted as a distributional equation in exactly our new "weak solution" sense.

As this stands, this all seems like a formalistic trick, although the technique can be linked to the venerable Principle of Virtual Work in mechanics.[32] In nonlinear cases, however, this extension destroys the unique dependence of solutions upon initial conditions and one will need to find extra conditions, often derived from surprising sources (e.g. thermodynamics) to pick out the right weak solution.[33]

The most common reaction to these problems is to maintain that it is fundamentally a mistake to try to formulate physical laws in terms of *differential* equations at all, but one should begin with *integral* forms instead, a result upsetting to certain philosophical opinions.[34] Actually a problem as simple as billiard ball impact gives independent motivation for such a change. In such a situation, "F=ma" cannot make sense because the acceleration is not defined at the moment of impact t_i. One must instead say that the total force exerted over an interval of time (t_o, t_1) containing t_i must equal the total change of momentum experienced in the same interval, i.e.

$$\int_{t_0}^{t_1} F\, dt = m \left[\frac{dx}{dt} \right]_{t_0}^{t_1}$$

where

$$\left[\frac{dx}{dt} \right]_{t_0}^{t_1}$$

represents the *jump* in velocity between t_o and t_1. Specializing to our string, we write

$$\int_{t_0}^{t_1} T \left[\frac{dy}{dx} \right]_b^a dt = \int_b^a P \left[\frac{dy}{dt} \right]_{t_0}^{t_1} dy$$

If y is a regular smooth solution, the duBois-Reymond Lemma allows us to convert this to the familiar differential form; otherwise we get the "jump condition"

$$\left[\frac{dy}{dt} \right] = \pm C \left[\frac{dy}{dx} \right].$$

This is the "algebraic condition" Truesdell alludes to above.[35]

Unfortunately, this change in foundations means that we no longer have enough equations to set a determinate, "well posed" physical problem in the case of the nonlinear string. Just as in the "weak solution" treatment, we must cast about for further conditions to isolate a unique solution. In general, the technicalities involved are fearsome,[36] but in the context of our string, thermodynamics provides an appropriate missing condition, *viz.* the Clausius-Duhem inequality (closely related to the Second Law of Thermodynamics). I find it quite remarkable that the resolution of a "mechanical" seeming problem like the movement of a string should force one to introduce thermodynamic ideas, where this addition is required by simple considerations of equation counting. In short, classical continuum mechanics does not have the "closure" one might naively expect. Again some of Duhem's hostility towards point mass mechanics seems to stem from related considerations. As such, the evolution of the mechanics of strings neatly illustrates my initial contention that considerations of mathematical harmony alone can force great changes in the foundations of a discipline. And there is nothing in our history that could not have been foreseen by a mathematician (with the combined brilliance of Euler and Riemann) firmly ensconced in his or her armchair. Clifford Truesdell labeled Euler's rejection of the principle (derived from Leibniz) that "nature does not make jumps" "the greatest advance in scientific methodology in the entire century."[37] This may rather overstate our problem's importance, but it is clear that we cannot understand the efficacy of thought experiments until we can account for the flood of changes that derive from this innocuous seeming puzzle.[38]

NOTES

1. Quoted in H. J. M. Bos, "Differentials, Higher-Order Differentials and the Derivative in the Leibnizian Calculus." *Arch. Hist. Exact Sci.* 14, vol. 1 (1974).

2. Thomas Kuhn, *The Essential Tension* (Chicago: University of Chicago, 1977), p. 241.

3. I.e., d'Alembert's.

4. E.g. Henri Poincare.

> Now the numbers the physicist measures by experiment are never known except approximately; and besides, any function always differs as little as you choose from a continuous function. The physicist may, therefore, at will suppose that the function studies is continuous, or that it is discontinuous; that it has or has not a derivative; and may do so without fear of ever being corrected, either by present experience or any future experiment. We see that with such liberty he makes sport of difficulties which stop the analyst.

"Analysis and Physics," in *The Value of Science* (New York: Dover, 1958), p. 83.

5. Cf. P. M. Harmon's comments in *Energy, Force and Matter* (Cambridge: Cambridge University Press, 1982), pp. 10-11.

6. That is, the original problem can be *stated* simply enough. Its possible resolutions, which I'll discuss at the paper's end, involve many subtleties and still represent an area of active investigation. Here my account will be sketchier and somewhat oversimplified.

7. In almost all details of what I am to present here I follow Clifford Truesdell's two great treaties *The Rational Mechanics of Flexible or Elastic Bodies* 1638-1788 in Leonhard Euler, *Opera Omnia* Series II, Vol. 12 (Lausanne: 1954) and his 1956 Introduction to Vol. 13, Series II of the same work. D'Alembert's original paper is "Recherches sur la courbe que forme une corde tendre mise en vibration," *Historie de l'Academie Royle, Berlin* 3, 1747; Euler's is "Sur la vibration des corbes" in *Opera Omnia* Series II, Vol. 10. The discussion is excerpted in D. J. Struik, *A Source Book*

in mathematics, 1200-1800 (Cambridge: Harvard University Press, 1969), pp. 351-68.

8. John T. Cannon and Sigalia Dostrovsky, *The Evolution of Dynamics* (New York: Springer-Verlag, 1981), p. 18. Taylor provided the force analysis D'Alembert later followed but was unable to convert into a proper differential equation.

9. A good treatment in this vein is U. Bottazzini, *The "Higher Calculus"* (New York: Springer-Verlag, 1986). D'Alembert's position is often described as allowing only analytic functions in mathematics; in truth, it would be more correct to say that he didn't see how the tools of the differential calculus could be meaningfully applied to non-analytic functions, such as geometry might study. For an excellent account, cf. A. P. Youschkevitch, "The Concept of Function Up To The Middle of the Nineteenth Century," *Arch. Hist. Exact Sciences* vol. 7 (1976).

10. Truesdell, "Introduction," *op. cit.*, XLII.

11. Cf. A. L. Fetter and J. B. Walecka, *Theoretical Mechanics of Particles and Continua* (: McGraw-Hill, 1980), Chapter 7. These same authors implicitly shift to Euler's position when they discuss shocks in chapter 9.

12. Translation from Giovanni Gallavotti, *The Elements of Mechanics* (New York: Springer-Verlag, 1983), p. 2.

13. Cf. L. Prandtl and O. G. Tietjens, *Fundamentals of Hydro- and Aeromechanics* (New York: Dover, 1957), p. 215.

14. The hydrodynamic situation has many further complexities which I cannot discuss here. Furthermore equations in physics can "self destruct" without benefit of non-smooth initial or boundary conditions. Most notorious, perhaps, is the prediction of black hole singularities from the equations of general relativity (which don't make sense there). The appearance of shocks in the full non-linear equation for the string is an example of breakdown, largely independent of special initial or boundary conditions. In the *linear* case which Euler and d'Alembert discussed, corner singularities are induced only courtesy of rough initial or boundary conditions, which I have loosely labeled "geometry" here. But the phenomenon of shocks shows that, although the dispute first presents itself as a conflict between geometry and analysis, other factors need to be considered in an adequate resolution.

15. As surprising as this claim may sound, the modern reading of "F=ma" begins here, not with Newton. Cf. Truesdell's comments in *Essays in the History of Mechanics* (New York: Springer-Verlag, 1968), Chapter V.

16. Cf. H. F. Weinberger, *A First Course in Partial Differential Equations* (New York: Blaisdell, 1965), Chapter 1. A more sophisticated approach, allowing Euler's solutions, can be found in Stuart S. Antman, "The Equations for Large Vibrations of Strings," *American Mathematical Monthly*, vol. 87.

17. In the drawing the figures are drawn too steep to satisfy the small amplitude assumption, but this could be easily corrected. It might be remarked, in light of footnote 24, that Euler's general solution is often used to move wave forms that can't possibly satisfy this assumption, e.g. square waves or delta functions. On the other hand, if our string happens to be made of the right kind of material, the wave equation is exact and the small amplitude requirement can be waived. Cf. Joseph B. Keller, "Large Amplitude Motion of a String," *American Journal of Physics*, vol. 27, pp. 584-86.

18. This argument is taken from Fetter and Walecka, *op. cit.*, p. 219.

19. *Operational Calculus*, Vol. I (Pergamon, 1983), p. 216. Mikusinski is actually concerned to defend singular "δ-function" type solutions to the wave equation in the sense of Laurent Schwartz' distributions or his own convolution quotients. As I remark below, it is often hard to determine whether a scientist is defending a class of solutions as merely acceptable "idealizations" or as correct in their own right. Although it is common to regard δ-functions as "idealized limits" of real

functions, Mikusinski and other advocates of "singular functions" can sometimes be read as claiming that such solutions are no more "idealized" than the regular solutions. Unfortunately it is very hard to find a clear discussion of these issues, which supply our string problem with an added depth. *Cf.*, however, T. P. G. Liverman, *Generalized Functions and Direct Operational Methods* (Englewood Cliffs: Prentice-Hall, 1962), p. 55. An excellent history of this range of issues, with pertinent comments on Euler's own inclinations in these directions, is Jesper Lutzen, *the Prehistory of the Theory of Distribution* (New York: Springer-Verlag, 1982).

20. Less defensible aspects of d'Alembert's position made these constraints more severe than appear here.

21. This *ad hominem* on Euler's behalf is my own invention. As an argument that d'Alembert must be prepared to accept concentrated loads on the string, it seems sound, but its general approach to boundary conditions cannot be ratified. In general, the whole subject of boundary conditions clearly merits closer philosophical scrutiny than it generally receives.

22. This description is misleading in that, when shocks are close to hand, we are not warranted in decoupling the transverse vibrations of the string from its longitudinal motion. But we still get a spontaneous breakdown of the equation's validity. The genius of the whole theory of shocks is how it continues the prediction of string behavior past these failures. A good discussion of the physics can be found in L. D. Landau and E. M. Lifshitz, *Fluid Mechanics* (Pergamon, 1984), part IX. A modern introduction to the mathematical side of things is Joel Smoller, *Shock Waves and Reaction-Diffusion Equations* (New Yrok: Springer-Verlag, 1983). The theory of distributions, mentioned above, is closely allied with this work.

23. "On a Difficulty in the Theory of Sound," in *Mathematical and Physical Papers* (Cambridge: Cambridge University Press, 1883).

24. Cf. the speculations in B. B. Mandelbrot, *The Fractal Geometry of Nature* (New York: Freeman, 1984); the view traces back to Norbert Wiener's treatment of "white noise." As I indicated in footnote 19, an important class of extension solutions are those connected with the theory of distributions—in particular, square wave pulses (which can be induced by sharp impulsive hammer blows). Many books discuss these, often in the context of Fourier analysis, taking various positions (pro and con) depending upon how the string energy is computed, whether the small amplitude assumption is bothered with and so forth. See, for example, Cornelius Lanczos, *Linear Differential Operators* (New York: Van Nostrand, 1961), 8.7. Unfortunately adequate discussion of these issues would take us quite far afield.

25. Cf. Joseph Fourier, *The Analytic Theory of Heat* (New York: Dover, 1955), p. 230.

26. "Sur le Mouvement d'une Corde," *Opera Omnia 10*. Cf. Truesdell, *Rational Mechanics, op. cit.*, p. 285.

27. Gallavotti, *op. cit.*, 4.5. Lagrange's model for transverse vibrations hangs his "atoms" on a massless chord. Taken as a finite difference scheme for a partial differential equation, Lagrange's approach can be most valuable for existence proofs, numerical calculation, etc. But it is not suitable as a candidate for a true picture of nature.

28. Cf. Truesdell, "A Program of Physical Research in Classical Mechanics," in *Continuum Mechanics I* (: Gordon and Breach, 1966).

29. Quoted in Truesdell, *Rational Mechanics, op. cit.*, p. 288.

30. "Essaie d'une nouvelle theorie de la Resistance des Fluides."

31. Truesdell, *Rational Mechanics, op. cit.*, p. 43.

32. Stuart S. Antman and John E. Osborn, "The Principle of Virtual Work and Integral laws of Motion," *Arch. Rat. Mech. and Anal.*, 1979.

33. Even more surprising, blind application of the extension technique can lead to paradoxical results because mathematically equivalent differential equations will extend to different classes of weak solutions. Cf. Smoller, p. 253.

34. E.g., Bertrand Russell, "On the Notion of Cause," in *Mysticism and Logic* (Doubleday, 1957). I might also mention that, in a more general way, related failures of smoothness in boundary conditions for elliptic equations cause difficulties for the program of "nominalizing Newtonian Gravitational Theory" (actually only the static case) announced in Hartry Field, *Science Without Numbers* (Princeton, 1980).

35. First worked out for the string by Christoffel in 1876. Cf. *E. B. Christoffel*, edited by P. L. Butzer and F. Feher (Birkhauser Verlag, 1981). For the details, see Erich Zauderer, *Partial Differential Equations of Applied Mathematics*, (New York: John Wiley and Sons, 1983).

36. Cf. comments in Antman, *op. cit.*

37. *Rational Mechanics*, p. 248. A good history of the philosophical side of these questions is Wilson Scott, *The Conflict Between Atomism and Conservation Theory 1644 to 1860* (London: MacDonald, 1970).

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