Klopsteg Memorial Lecture (August, 1992): What science knows about violins—and what it does not know

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This is the edited text of the Klopsteg Lecture delivered to the Summer Meeting of the AAPT on August 13, 1992. It sketches the current state of knowledge about the violin—at least as seen by the author—in two parts, *Physics of the Bowed String* and *The Violin as a Radiator of Sound*, punctuated by a number of "meditations" about the nature of scientific knowledge.

I. FIRST MEDITATION

If we want to ask ourselves what exactly it means to "apply physics" to a problem, we need to keep in mind that physics is rather different from the other sciences because, unlike them, it is not defined primarily by its subject matter. So, for example, we would agree that a problem validly belongs to biology if it deals with living things, or to geology if it deals with rocks, but no corresponding definition exists for physics. Instead, physics is delineated, not by its subject matter, but by the methods of thought that a physicist uses.

Take, for example, the case of the weather, whose prediction obviously involves thermodynamics, aerodynamics, electrodynamics, mechanics: by any definition based on subject matter, it would certainly qualify as physics. Yet until fairly recently—specifically, until the advent of weather satellites, massive daily balloon launchings, and great monstrous supercomputers—physicists did not involve themselves much with the weather for one very simple reason: the problem was too hard. Put more soberly, the methods that physics uses were unable to make any headway. Faced with this kind of a situation, physicists simply pronounced weather prediction as "not physics."

In fact, one can view the whole history of science—beginning with Greek attempts at "rational" descriptions of natural phenomena, up through medieval theological speculations, the birth of what was called "natural philosophy," and finally the physics that we know today—as a gradual culling away of subject matter that did not fruitfully yield to that particular style of analysis; in this progression, physics is nothing other than that part of human speculation which remained. It is not that other subjects, such as philosophy and theology, are less interesting or exciting—in some ways they may be more so—but that progress in those areas must be attempted by other methods.

That is why it often strikes us as so pathetic when newcomers, such as those who work in the fields that we have come to call the "social sciences," come along and say, "Look: these methods have been so successful for the physicists; let us apply them to our own areas of interest and see whether we could not attain similar successes." To a physicist, that is going backwards. A physicist says: "We have been there earlier in our history, and we found that it does not work."

But I still have not told you what "good physics" is, and particularly not in a way that would define what one might mean by "good violin physics." That is a point to which we will need to return.

II. PHYSICS OF THE BOWED STRING

A. Sticking and slipping

For purposes of physical understanding, we can visualize the complete violin, as sketched in the top part of Fig. 1, decomposed into two sections according to the scheme shown in the bottom part of the same figure. The section on the left consists of the strings, whose vibrating length is defined at one end by the bridge, on the other by the "nut;" they are held under carefully tuned tension between the tailpiece at one end and the pegs at the other. The strings (or, more exactly, the string which is being played at a given moment) is put into vibration by the action of the bow, which exerts a force on it. The other section of the violin, as we conceptualize it, consists of the wooden shell plus the enclosed air. The vibrations of the string are transmitted to this second section via the bridge; in turn, the shell and enclosed air interact with the sound medium represented by the surrounding air, so that sound waves are sent out from the instrument. In this first part of the presentation we will outline some aspects of the physics of the interaction between the bow and the strings, that is, the process that is responsible for putting the left section of Fig. 1 into vibration.

When a physicist contemplates such a process, the first question that naturally arises is: How is the rectilinear motion of the bow converted into oscillatory motion of the string? Many people will consider that question answered by saying that we are dealing with a "stick-slip oscillation:" the vibration of the string is driven by the fact that it first sticks to the bow, moving, in other words, along with it; but when the force between the two exceeds some kind of frictional limit it "lets go" and *slips* in the opposite direction, finally being grabbed by the bow once more. These two régimes, the sticking régime and the slipping régime, alternate, so that each complete period of oscillation contains first one, then the other.

Yet a little thought will reveal that such a description omits some important features, as we can see by contemplating the simple stick-slip system shown in Fig. 2. Here a rough-surfaced conveyor belt is imagined to be moving on two pulleys with constant speed v_0, and a mass m, connected to a fixed wall with a spring of force constant k, is placed on top. We can make our model especially simple, without sacrificing any of its important properties, by calling the maximum force of static friction F_0, and imagining that the force of sliding friction vanishes altogether. The mass will then be dragged along by the conveyor belt until the displacement is equal to F_0/k, whereupon it will let go; subsequently (since we assume the force of sliding friction

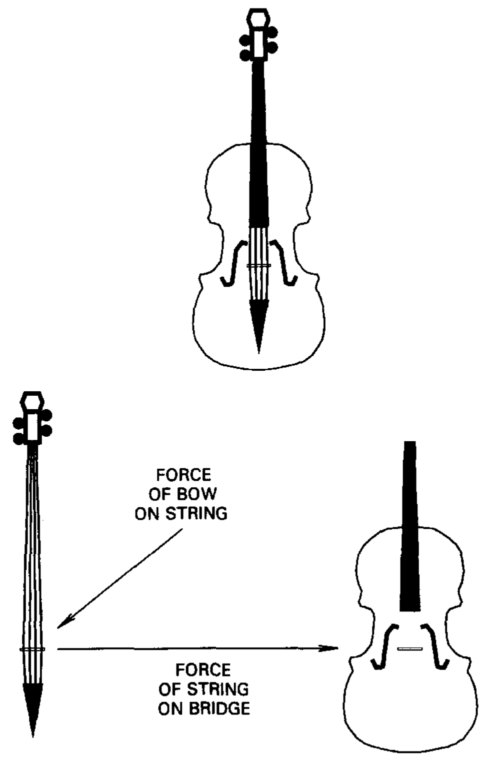


Fig. 1. For purposes of our discussion, the complete violin (top) is conceptually decomposed into strings (on the left) and body (on the right). The strings are held under tension between the pegs at the top and the triangular "tailpiece" at the bottom; their vibrating length is defined by the nut (or else the player's finger) and the bridge, and they are set into vibration by forces exerted by the bow. The bridge, in turn, shown in both parts of the violin, exerts forces on the wooden shell of the violin and, more indirectly, on the enclosed air.

to vanish) its motion will be that of a free oscillator, whose initial conditions are to have a displacement F_0/k and a velocity v_0. Such a motion is, of course, sinusoidal in time, and will continue until the velocity of the mass again matches that of the conveyor belt, when it will be "recaptured" and the cycle will repeat.

A simple calculation shows that the pattern of the resulting motion will depend on a parameter which we can call the *coupling constant* and represent by Γ. It is defined as the ratio

$$\Gamma = \frac{F_0/k}{v_0/\omega_0},$$

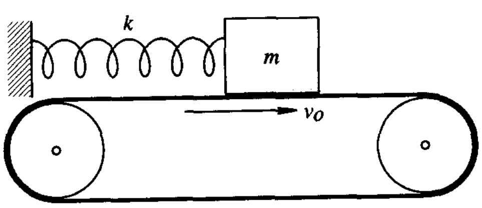


Fig. 2. A prototypical stick-slip oscillator: a mass m is dragged along toward the right by a conveyor belt moving with speed v_0 while attached to a fixed wall by a spring of force constant k. This type of system is, however, a very poor model for a bowed string.

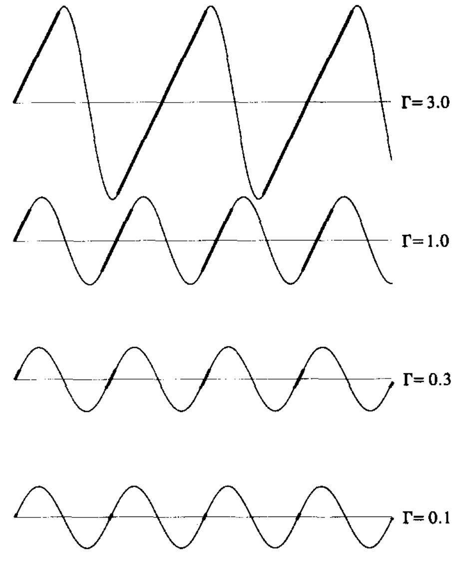


Fig. 3. Displacement as a function of time of the system of Fig. 2 for various values of the coupling constant Γ. The time axis spans exactly four periods of the harmonic oscillator if it were sliding on a frictionless surface. The decrease of actual frequency of oscillation with increasing Γ, mild for small Γ, becomes enormous when that parameter exceeds 1.

where the fraction in the numerator is the displacement at which the mass lets go of the conveyor belt, and the one in the denominator is the displacement amplitude with which the corresponding free oscillator would have to vibrate so as to have a velocity amplitude of v_0 (here ω_0 is the radian frequency of the free oscillator). The motions of this system for a few possible values of Γ are shown in Fig. 3; in this case Γ is varied by keeping k, v_0, and ω_0 constant and changing F_0. Clearly, the value of the coupling constant determines the "duty cycle," or the respective fractions of the period occupied by the sticking and slipping régimes; the larger Γ is, the smaller the relative time spent in "free" vibration. It is also clear from the figure (as can, of course, be calculated in explicit algebraic terms) that the overall period will vary with Γ; this variation is weak for small Γ but becomes quite strong for large Γ.

Now it is a very simple exercise to match the parameters of such a model to the typical conditions of a bowed violin string, with the result that Γ is by no means small (values in the range of 10 are entirely reasonable); at the same time, we know that the frequency with which such a bowed string oscillates is very accurately the same as the free string would have (and which is easily observed if we pluck the string instead of bowing it). Musically, this fact is extremely important. You may be aware that the frequency of a vibration determines the pitch of the musical note which is perceived, and that the interval of a semitone, e.g., from C to $C\#$, represents a frequency change of less than 6%, so that a shift of even a fraction of 1% would, by musical standards, constitute an outrageous error in intonation. We conclude that the model of Fig. 2 cannot, at face value at least, be applied to the mechanism by which the bow interacts with the string.

Although we will find it advantageous to approach our modeling from a different direction, it may nonetheless be interesting to remark what, exactly, went wrong in the case

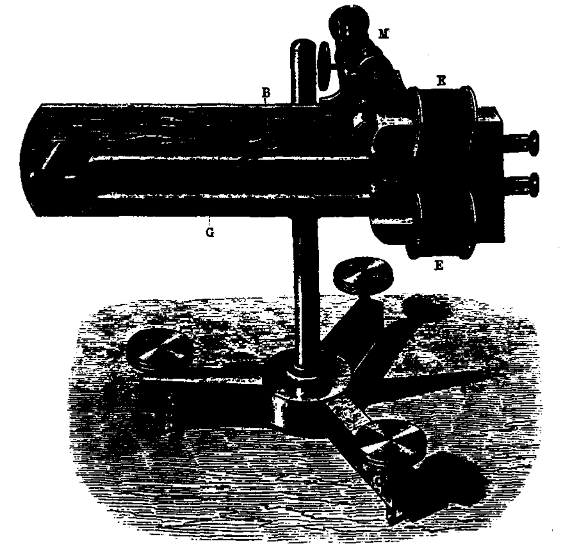


Fig. 4. Helmholtz's "vibration microscope," from an illustration in his book *Die Tonempfindungen (Sensations of Tone)* first published in 1862.

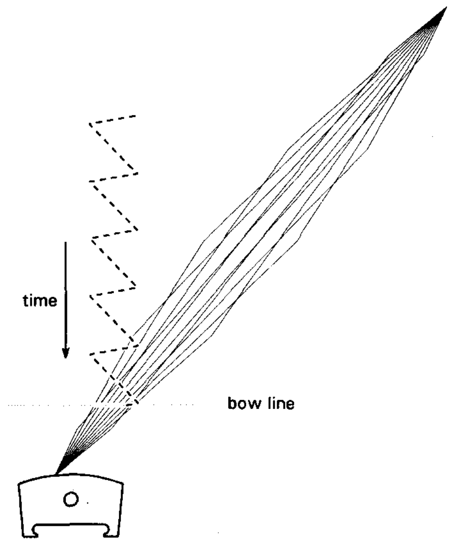


Fig. 5. Helmholtz motion of a violin string depicted as an imagined multiple exposure from a violinist's-eye point of view. In this perspective sketch, the bridge is the nearest point, and the positions of the string at various instants, all of which lie in a horizontal plane, are shown at the same time. The violinist's bow is imagined as moving (to the right) along the dotted line. Also shown, in the form of a dashed line, is the curve that would be drawn by the string if a vertical sheet of magic recording paper moved upward through the bow line without affecting the string's motion. On this record, time would run downward (because the paper is moving upward), and the displacement of the string would appear as an asymmetric triangular wave sometimes called a "sawtooth."

we are considering. It turns out that an absolutely crucial property of the stretched string, as it appears in this context, is that it has not one but many proper frequencies, and that these proper frequencies are harmonic—that is, integer multiples of the lowest one. In other words, it is necessary to consider the string not as a single harmonic oscillator but as a collection of them; or, equivalently, as a continuous medium having not one, but an infinity of degrees of freedom.

B. Helmholtz motion

Historically, the first physicist to do a serious, quantitative study of bowed-string motion was Helmholtz, working in the second half of the last century. The nature of his apparatus is pictured in Fig. 4, reproduced from his book which was first published in 1862. Not shown in this diagram is the violin itself, which is held vertically (like a cello); affixed to some point of the string is a small grain of starch, which is illuminated by a strong light. It is the resulting very bright small white spot that is observed through the vibration microscope which the figure depicts. It consists of a microscope whose objective lens is attached to one prong of a tuning fork; this tuning fork is electrically driven (by a mechanism like that of an electric buzzer) so that it vibrates sinusoidally along a vertical line segment at the same time that the grain of starch, attached to the string, is vibrating horizontally. What is seen by an observer looking through the microscope is, accordingly, a two-dimensional curve which, if the frequency of the tuning fork is related to that of the string by a ratio of small integers, will have the character of a stationary (or almost stationary) Lissajous figure. In a way, this device can be considered a precursor of our modern oscilloscope, except that the sweep (which is here vertical) has a sinusoidal wave form instead of the more modern sawtooth; as a result, considerable interpretation is required to obtain from the observed luminous curves an actual picture of the displacement of the string as a function of time.

The conclusion which Helmholtz reached, on the basis of meticulous observations at a number of places on the string, is that the bowed string is executing *Helmholtz motion*, as we call it today; and although the intervening one and a third centuries have considerably refined our knowledge of the phenomenon, its essential nature is still acknowledged to be as Helmholtz described it. In "ideal" Helmholtz motion, which can be shown to be a possible motion for an infinitely flexible free string without any energy dissipation, the string takes the form, at each instant of time, of two straight-line sections joined by a corner or "kink;" as time progresses, the kink moves along a parabolic path from one fixed end of the string to the other and then returns on the opposite side, this motion of the kink taking place at the speed of transverse waves in the string. Thus the round-trip time of the kink, which is $2L/c$ (L being the length of the string and c the wave speed), is the time after which everything repeats—that is, it is the *period* of the Helmholtz motion. We note that, according to elementary physics, this is exactly equal to the frequency of the fundamental normal vibration of the string, all other normal modes having frequencies which are integer multiples of this fundamental one. As a result, any free motion at all (e.g., the motion which results when the string is plucked), being a superposition of normal modes each vibrating with its own frequency, will have that same period of $2L/c$ (since the passage of that amount of time will allow each mode to have gone through an integer number of its own proper periods, causing the overall motion to start from the beginning).

Figure 5 depicts the Helmholtz motion as an imagined

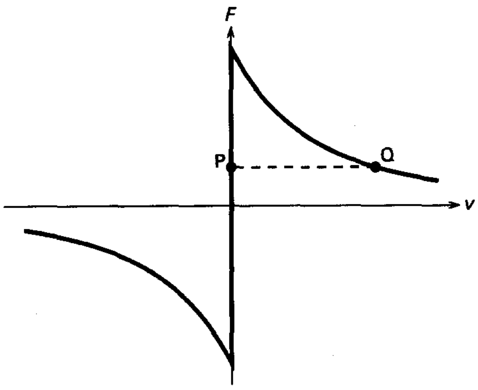


Fig. 6. Hypothetical characteristic giving the force between the bow hairs and the string as a function of their relative velocity. The vertical section represents sticking: at zero relative velocity, any force between a negative and a positive limit is possible. The curved sections represent slipping: as the relative velocity grows, the frictional force decreases. In Helmholtz motion, the string alternates between the operating points P and Q, which must lie on the same horizontal line.

multiple exposure from a violinist's-eye view. Shown also, in the form of a dashed line, is the motion of one point in the string, graphed as displacement (horizontal) vs time (vertical). We see that this displacement also consists of straight-line sections, that is, constant-velocity sections, corresponding in alternation to the time during which the kink moves from the observation point to one end of the string and returns, and to the round trip in the other direction. If the observation point coincides with the bowing point, it will be useful (as we shall see in a minute) to interpret those two sections as the "sticking" and "slipping" parts of the cycle, but it is important to understand that, for ideal Helmholtz motion, the same type of displacement-time curve would be observed at each point of the string. What is different among different points is the duty cycle and the two corresponding velocities: at each point, the duty cycle, or fractional time spent moving in each of the two direction, is determined as the fractional position of that point along the string measured from each of the endpoints; and the two constant velocities must, of course, be in inverse proportion to those fractions, since the overall displacement in the course of a period must be equal to zero.

Although it may feel comforting, in this way, to have discerned why the sticking and slipping parts of the cycle each have a constant velocity, and why the frequency of motion is (unlike the oversimplified model of Sec. II A!) independent of any "coupling constant," this "discernment" must still, at this point, be viewed as illusory, since all we have stated so far is that the Helmholtz motion is a possible motion of the free string without damping and without external forces applied. But in fact, of course, the bow *does* exert a force, according to some frictional characteristic such as is sketched in Fig. 6. Here the force exerted by the bow on the string (or, more correctly, the force exerted by the string on the bow; the other is, by Newton's Third Law, the exact negative of it) is shown as a function of the velocity of the string relative to the bow hair. Typical of this characteristic is that at very low relative velocities the force rises to comparatively high values; the graph shows this initial large slope as infinite, that is, it depicts the corresponding part of the curve as a vertical segment. When the situation is such as to place the actual operating point on this segment, we describe it as a "sticking condition," that is, one in which the relative velocity of the string and bow is zero or nearly zero. As the relative velocity increases away from zero, the frictional force is shown as decreasing again; these are the points that correspond to a slipping condition, in which the velocities of the string and bow hair can differ as much as they wish.

A moment's thought will reveal why a frictional characteristic of this general type, in which slipping (or "sliding") friction is less than sticking (or "static") friction, is necessary for Helmholtz motion to be possible. If we describe the sticking and slipping parts of the motion by a pair of "operating" points on the graphs, such as P and Q in Fig. 6, the two must lie on a horizontal line, that is, they must correspond to forces which are equal both in magnitude and in direction; otherwise, since the distances which the string covers in the two directions are equal (otherwise there would be a net displacement in the course of a period, which is nonsense), the work done by the bow in the sticking section would not be cancelled exactly by the negative work done in the slipping section. This would imply that the bow transfers a net amount of energy to the string in the course of a period, which is impossible if there is no dissipation (as we have assumed) and a steady motion has been attained. But it is clear that two separate intersection points for which the force is the same can exist only if the curve increases to a maximum and then decreases again.

As you are probably aware, a frictional characteristic that comprises separate sticking and slipping régimes, with the force larger in the former than in the latter, is generally typical of what we call *dry friction*. So, for example, if there is a large crate on the floor and you lean on it in an attempt to make it move, you find that up to a certain point it remains oblivious to your effort; but once the motion begins, the same force exerted by your leaning on it can be sufficient to produce considerable acceleration, perhaps enough to make you fall on your face if you are not careful. By contrast, what we call wet, or *lubricated*, friction is a situation in which the frictional force increases monotonically from being zero at zero velocity. This latter situation is exemplified by standing on a dock next to a large boat which is not properly tied up. If you lean on the boat ever so slightly, it will begin to creep away, albeit very slowly; so that if you are not paying attention you can once more end up in an embarrassing situation. In the case of violin bow hair, a slight layer of oil, such as your little brother or sister might put on it by touching it with greasy paws, will be enough to make it useless for obtaining a sound.

What, then, would be the motion of a violin string, still considered dissipationless, under the action of a bow? Clearly, the "pure" Helmholtz motion is no longer possible, since it is a solution of the equations of motion with *no* force applied; yet the correct answer is actually quite simple since, as we have proved, the forces exerted by the bow during slipping and during sticking are identical. The situation is, accordingly, that of a string which is free except for *an absolutely constant* force applied at one point. If nothing else were happening, such an absolutely constant force would (like a localized weight hung from a clothesline) produce a string with a stationary kink at the location of the bow. Superposing the two solutions, one a solution of the homogeneous (that is, free) equation of motion, the other a "particular integral" of the inhomogenous one, we conclude that a possible motion of the dissipationless bowed string is the classic circulating kink of the Helmholtz motion plus a second, stationary kink at the position of the bow. It is this combination that is today considered as the basic motion of a bowed string.

C. Uniqueness and stability

The foregoing approach at best establishes that the Helmholtz motion, as experimentally observed, is possible. It does not give any grounds for believing that it is the only one possible; nor does it bring any evidence to show why, if other motions are possible, the Helmholtz motion is stable—that is, continues to be executed once it is begun.

With regard to the first point, the answer is clearly revealed by handing a violin and a bow to a novice and requesting that an attempt be made to play the instrument. We all know from painful experience that the resulting assortment of screeches, squeaks, and squawks is certainly *not* the regular and periodic motion that we have been discussing; in other words, the Helmholtz motion does *not* have any physical uniqueness. In fact, this simple observation illustrates a rather important danger associated with doing physics on a system which has been chosen for study on the basis of other, essentially extraneous, reasons, and that is to confuse the universe of *musical* possibilities with the universe of *physical* possibilites.

As regards the *stability* of Helmholtz motion, the situation is more complicated. Experimentally, of course, we know that it is stable, in that a violinist, having once begun a note, is able to continue it by simply drawing the bow smoothly across that string, in spite of the fact that infinitesimal perturbations must always be arising. In fact, to describe a motion as "stable" is synonymous with saying that, as a result of the appropriate laws of dynamics, any infinitesimal perturbation which does arise subsequently decays back toward insignificance rather than growing.

Unfortunately, when we try to apply this kind of logic to the ideal Helmholtz motion, we reach the embarrassing conclusion that *theoretically, the Helmholtz motion is always unstable.* This conclusion follows, in fact, directly from an examination of the frictional characteristic of Fig. 6, where this motion is described as one that jumps back and forth between the two operating points, namely the sticking point P and the slipping point Q. Now if an infinitesimal disturbance is superimposed, it is easily seen that, since the slope of the curve at Q is negative, an increase of velocity will be accompanied by a decrease of force; differentially, this is the condition we describe as a "negative resistance," whose property is to feed energy into the perturbation rather than dissipating it. On the other hand, the sticking point P has an infinite slope, which (like a rigid wall) corresponds to an "infinite resistance;" it means that any perturbation will be reflected from it with no change in energy. Thus the only two possibilities—depending on details of form and phase—are for the perturbation to gain energy or to remain unchanged, so that the interaction of the perturbation with the bow must, on the average, cause it to grow.

Faced with a situation in which experiment and theory clearly contradict each other, we are forced to consider what factors in the real situation our model has ignored; there is, in fact, no shortage of these, including: imperfect reflection at the end supports; finite width of the bow; presence of torsional string motion in addition to transverse motion; and some others, none of which I can reasonably go into as part of this talk. Suffice it to say that the problem of stability of Helmholtz motion remains one in which active research is continuing.

D. Computer simulation

Computer simulation as a method of research lies in some ways in between theory and experiment; in other ways it represents a completely new type of research activity, made possible only relatively recently by the development of computers whose great speed and enormous memory allow them not merely to do things faster than we could otherwise but to engage in projects which would normally be considered totally impossible, in the sense that to attempt an equivalent calculation "by hand" would not merely take much longer than any researcher's lifetime but might well exceed the expected lifetime of our civilization as a whole. In doing a computer simulation of a bowed string, we must have, or at least think we have, a theoretical picture of the underlying mechanics; but instead of then attempting to solve analytically the very complex nonlinear coupled partial differential equations, we take, so to speak, "the point of view of the string itself," in that we assign to it appropriate initial conditions and then allow its time development to proceed incrementally from one moment to the next, these moments being spaced sufficiently closely in time to approximate the continuous behavior of what actually occurs.

One such program which I developed ten years ago I named VIOLUNIX, since that was my first experience with the UNIX operating system. Its details are not important, especially as considerable work has been done in this area since; but the general scheme is for the computer to store two arrays, signifying the two waves which exist simultaneously on the string, one propagating in each direction. When the time advances by one unit, the arrays advance, their leading elements being "reflected" at the corresponding string ends (of which one stands for the bridge of the violin and the other for the violinist's finger) by being transferred to the other array. (In this case it is not necessary to assume that no dissipation exists at the supports, since a reflection coefficient smaller than unity is easily incorporated.) Finally, the program identifies three contiguous string locations as the places where the bow, taken to consist of three bow hairs, applies its forces, thus modifying the string's motion. These forces are computed from the corresponding string particle's instantaneous transverse velocity with the aid of a "frictional characteristic" to which the computer has access in the form of a table lookup, and which is programmed to correspond to whatever form the researcher has in mind (such as, e.g., the one of Fig. 6). By letting this computation proceed, we not only get a picture of the finally attained steady motion, but can equally well, and without distinction, study the various transient phenomena that occur.

We show in Fig. 7 (top curve) the string motion computed by VIOLUNIX for the situation in which the bow starts at rest and accelerates uniformly to a maximum speed; we see how at first the string merely deflects, then breaks into oscillation. As seen in the expanded view of this transient section (bottom curve of Fig. 7), it does not take long before rather good Helmholtz motion, as signified by

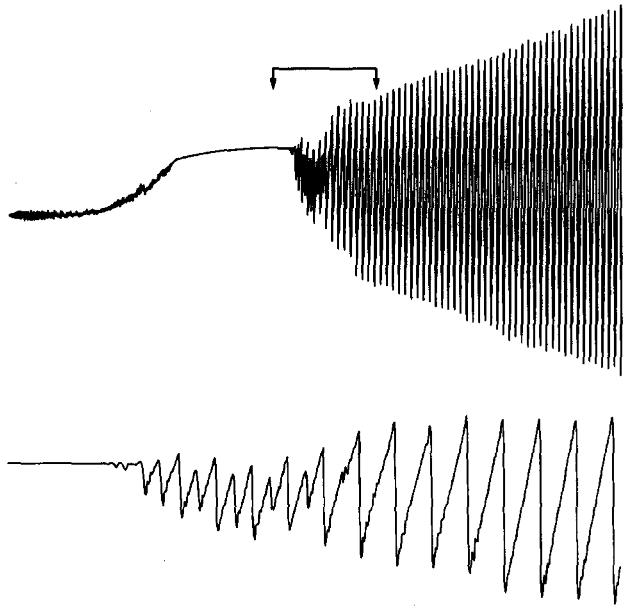


Fig. 7. String behavior computed by the program VIOLUNIX; what is plotted is the slope of the string at an endpoint, which translates into transverse force exerted on the bridge, as a function of time. The string is tuned to 500 Hz, and the upper graph represents a time interval of 200 ms; the bow is assumed to start from rest and gain speed linearly with time. The smaller interval extending from 86 to 120 ms (indicated by the arrows over the top graph) is shown expanded in the lower graph.

the sawtooth behavior, is established—and we must keep in mind that Helmholtz motion was in no explicit way programmed into this. But we also notice that the initial oscillation, although resembling a sawtooth shape, is taking place at twice the frequency of the string—that is, the bow is exciting not the fundamental but the second harmonic; after just a few cycles the fundamental makes its appearance and quickly takes over. This phenomenon is actually rather familiar; when one listens to it, one recognizes it immediately as what we normally call "a squeak." In fact, my experience listening to these "synthesized" sounds made it painfully obvious that in programming the computer simulation I had programmed the violin but had made no attempt to program the violinist! As a result, my first attempts sounded very much like a more or less normal violin being played by a four year old.

III. SECOND MEDITATION

At the end of what we called our "First Meditation," I left you with something of a puzzle: what is it about an investigation into the behavior of a violin which makes it, or fails to make it, "good physics?" In fact, as I understand it, the criterion is simple: *If you have been enjoying the discussion I have been presenting to you, then it is good physics*; in other words, it is the reaction of professional physicists that determines the quality of physics research. To many people, such a subjective criterion appears terribly undependable and frivolous, but (as I see it) what makes it not at all frivolous is precisely the fact that it is so utterly dependable; that is, that the judgment of professional physicists with regard to whether something is, or is not, good physics is so very consistent that it must be viewed as properly defining a real underlying objective quality.

Unfortunately, this kind of reasoning is terribly difficult to explain to an outsider, which accounts for the enormous misunderstandings about the nature of science that characterize our broader culture, and which are constantly propagated and reinforced by those in the media whose job it is to cover scientific subjects. Because of the general assumption that science is directed toward practical ends, people are always asking me: have you discovered the Secret of Stradivarius yet? And if not, what exactly is your research *good for?*

To understand why such questions tend always to miss the mark, it may be helpful to clarify the important distinction between a *violin maker* and a *violin physicist* (a difference that does not, by the way, preclude the same person engaging in both activities). The distinction is very simple. A violin maker's aim is to produce a better violin, and if in the process he obtains some scientific insights, that is gravy; whereas to the violin physicist, the situation is reversed. Although I, a violin physicist, have perennial secret dreams of discovering an important breakthrough in the process of violin manufacture—and the commonly used phrase "Secret of Stradivarius" is nothing but a metaphor for such a discovery—that is not the criterion by which I judge my success or failure which is, rather, determined by the reactions I get from an otherwise uncommitted audience at a physics colloquium (or, what is closely related, the peer reviews of research proposals which I submit to the National Science Foundation).

Sadly, violin physics has acquired something of a bad reputation among violin makers because of the way its nature has been misrepresented, often by the physicists themselves. That is why you often get comments from highly talented makers to the effect that "scientific methods are not of much use—I tried them and they did not help at all." Usually, what is hiding behind such a statement are two basic misunderstandings: first, that "scientific methods" means having a lot of oscilloscopes and audio oscillators and spectrum analyzers sitting around one's workshop; second, that science *claims* to provide effective methods for good violin building, which in fact it does not.

IV. THE VIOLIN AS A RADIATOR OF SOUND

A. Static and dynamic response

It is a commonplace that a vibrating string, interacting directly with the air around it, will radiate only an infinitesimal amount of sound, so that the understanding of the violin as a musical instrument must involve the discussion of the rest of it—the part other than the string itself—in the process of radiation. What we are talking about is, of course, the wooden shell of the instrument, as well as the air enclosed inside that shell (which communicates with the outside via the two openings known as "*f* holes"); in other words the section show on the bottom right of Fig. 1.

The importance of considering the frequency-dependent dynamics of both the wood and the air is made clear if we first discuss the simplest case, in which we imagine the frequency of string vibration to be so low that, under the action of the oscillatory force which the string exerts on the violin bridge, the shell and the air behave in a manner that is essentially static, following instantaneously the changes of force as they occur. Under those circumstances, a force applied at the bridge will cause some distortion of the shell with a consequent change in its volume; however, whatever volume change does occur in the shell, a corresponding amount of air will flow in or out of the *f* holes, so that the total volume of shell plus air will remain exactly unchanged. I like to refer to this rule as the "toothpaste effect," because of its analogy to what happens when you squeeze a tube of toothpaste: the volume of toothpaste which emerges will be exactly the same as the decrease in volume of the tube, so that the overall volume remains constant.

Now it is a fact that, at least at long wavelengths (which correspond to low frequencies), the radiation of sound is dominated by what is called the *monopole moment* of the source, which is precisely the amplitude of the overall volume change that it experiences; hence we conclude that if the body of the violin behaved statically, no sound radiation at all would occur. In other words, to try to imagine that the wood and air of the violin body behave statically is not merely a poor approximation numerically, but an approximation which totally destroys the problem we wish to examine.

On the other hand, if we ask at what frequency (apart from any radiative considerations) we would expect the static model to fail anyway, we might estimate where the shell might be expected to have its first *resonance*, which is, of course, the frequency at which effects of inertia (statically negligible) become comparable with effects of elasticity. We can get some idea of the Hooke's Law force constant by imagining that we put a 1 Kg weight on the center of a violin's belly—where the bridge exerts its forces—and measure its resulting displacement; in order of magnitude, we might see 0.1 mm or so. On the other hand, imagining the system in free vibration and visualizing the mass of wood involved, our estimate might be in the range of 10 g. Putting those numbers into the formula for the natural frequency of the corresponding oscillator gives us an answer in the vicinity of 500 Hz, right at the low end of the important audio spectrum. This makes it clear that to treat the violin motion as quasistatic would, in any case, be nonsense: we must, instead, formulate the behavior of the system dynamically, as the response of its various *normal modes* to the forcing stimulus.

In fact, the toothpaste effect (known more formally as the *sound hole sum rule*) is overcome, and the violin enabled to radiate, precisely because the wood motion and the air motion have different natural frequencies, causing the exact cancellation between them to be destroyed. Typically, the lowest air mode has a frequency around 290 Hz, and the lowest wood mode (as we just saw) more like 500 Hz, so that the toothpaste effect is broken as soon as the playing frequency begins to approach 200 Hz or so. (For reference, the lowest note that a violin can play, the open *G*, has a frequency of 196 Hz.)

To work out the consequences of this point of view, we first review the response of a single normal mode as a function of frequency. Consider, for example, the wood motion by itself; let its modal frequency be 500 Hz, and let it have a *Q* of 45. (As a reminder, the "*Q*" of a mode is the ratio of its modal frequency to the width of its response peak at a height of $2^{-1/2}$ of the maximum; it is also the ratio of the amplitude of the elastic restoring force to the amplitude of the dissipative force, when the driving frequency is at the center of the resonance, that is, equal to the modal frequency.) Figure 8(a) shows the displacement amplitude as a function of frequency, when the amplitude of applied sinusoidal force is kept constant. At low frequencies the behavior is static; that is, the displacement follows Hooke's law, being proportional to the applied force and independent of frequency. As the modal frequency is approached, we observe the phenomenon of *resonance*, whereby the displacement amplitude (still for the same given force) rises to a much higher value. Finally, at very high frequencies the motion is governed by the inertia, which means that it is the acceleration rather than the displacement that is proportional to the force; accordingly, the displacement amplitude now drops as the inverse square of the frequency.

The same figure shows, in its bottom (shaded) part, the phase of the displacement relative to the applied force, also as a function of the driving frequency. At the low-frequency end, the displacement, being governed primarily by elasticity, is naturally in phase with the force; that is, at each instant the displacement is in the direction of the applied force. By contrast, in the high-frequency limit, that is, at driving frequencies well above the modal (or "resonant") frequency, it is the acceleration which is in phase

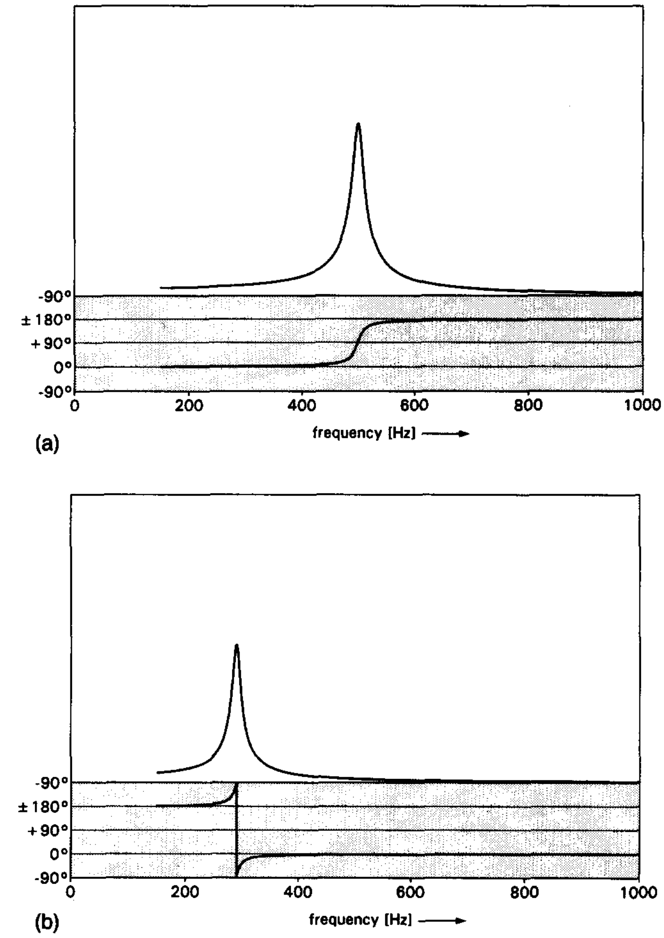


Fig. 8. Monopole moment as a function of frequency, for fixed force amplitude applied to the bridge, of (a) a typical "wood mode" and (b) a typical "air mode." The upper section of each graph shows the amplitude of the monopole moment; the lower section is its phase relative to that of the applied force.

with the applied force; the displacement is then (according to the well-known behavior of simple harmonic motion) 180° out of phase. At the exact center of the resonance, the phase difference is 90°.

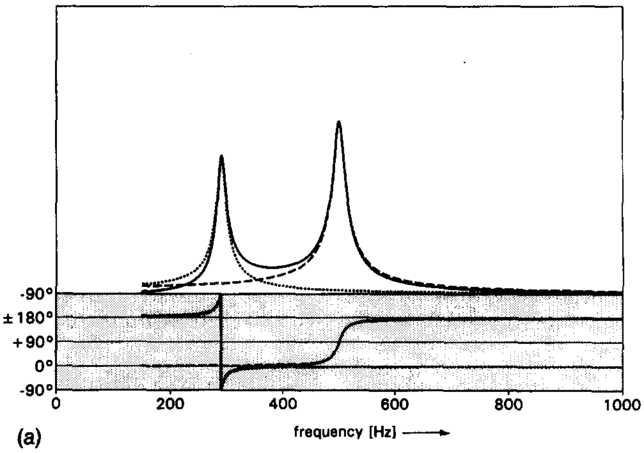
B. "Positive" and "negative" modes

In this lowest "wood mode," or "body mode," an upward displacement of the section of the body that lies immediately under the bridge corresponds to an overall "inflation" of the body volume, that is, a positive monopole moment. Accordingly, the same graph shown in Fig. 8(a) can also be interpreted as the *amplitude* of the *monopole moment* as a function of frequency, for a fixed amplitude of driving force applied at the bridge.

Let us now shift our attention to the lowest air mode, whose characteristic motion consists primarily of an alternate inward and outward flow of air through the *f* holes. Viewed as a harmonic oscillator, we have a system whose mass is essentially the mass of air in and around the *f* holes (because that is where the kinetic energy is concentrated), and whose "spring" comes from the air confined inside the body of the violin, which is being compressed when the flow through the *f* holes is inward and expanded when it is in the opposite direction. Although the mass involved in this mode is much smaller than the wood mass that we considered in connection with the lowest wood mode, the force constant is also a great deal smaller, as a result of which the modal frequency comes out comparable to (in fact, lower than) that of the wood mode, typically around 290 Hz. This lowest air mode is often called the "Helmholtz mode," not because Helmholtz considered it specifically but because it is similar to the vibration of what is called a "Helmholtz resonator" such as an empty glass bottle.

If, however, we want to construct a graph of monopole moment vs frequency for this air mode, in analogy with Fig. 8(a) for the wood mode, we must consider a crucial difference: whereas for the wood mode a downward static force on the bridge region causes a compression of the violin body, and hence a decrease in its overall volume, in the case of the air mode a downward static force applied to the violin shell at the bridge causes air to be *expelled*, thus *increasing* the effective volume of the system. (That is precisely the origin of the sound hole sum rule, or toothpaste effect: with both modes present, the low-frequency monopole moments of the two exactly cancel.) As a result, the graph of monopole moment amplitude vs driving frequency, for constant applied force at the bridge, is as shown in Fig. 8(b). Its general appearance is similar to Fig. 8(a), except that the phases are all shifted by 180°. If these two modes—the single wood mode and the single air mode—were the only ones present, the sound hole sum rule would require the low-frequency amplitudes in Figs. 8(a) and 8(b) to have exactly the same magnitude.

Figure 9(a) shows the superposition of such a pair of modes explicitly. The two previous graphs are copied as, respectively, a dashed and a dotted line; the sum of the two is shown as a solid line. We note, first of all, the cancellation as the frequency goes to zero, which we have been remarking on; we note also that, as each modal frequency is passed, the phase moves upward by 180°. Another important feature is that in the region between the two resonances there is a plateau: the contributions of the two modes add instead of cancelling. This is due to the fact that, while they "intrinsically" have opposite signs, this is the region where one of them has already experienced its 180°-phase shift but the other has not. Stated differently, the air mode is already in its inertia-limited régime but the wood mode is still in its elasticity-limited régime. I suspect that the appearance of this "plateau" may be an important factor in "leveling out" the frequency response of a violin.



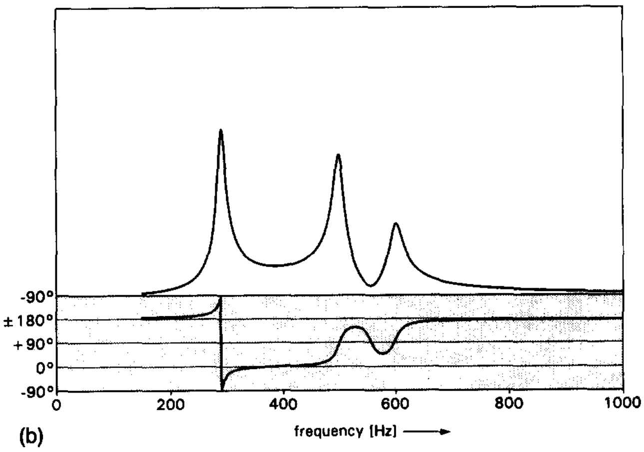


Fig. 9. (a) Composition of the two moments of Fig. 8 (solid; the dashed and dotted curves repeat those of the previous figure). We note especially the plateau between the two peaks. (b) Composition of three modes, one negative and two positive. Note the dip between the two positive peaks and the retrograde phase.

Finally, Fig. 9(b) shows a superposition of *three* modes, namely, the two shown in the previous figure plus one additional one at 600 Hz, whose amplitude is assumed to have the same sign as the wood mode at 500 Hz. The sound hole sum rule now requires the Helmholtz mode to be cancelled at low frequencies by the sum of the other two, and in this figure we have adjusted its strength so as to satisfy that condition. But what is especially interesting—and may possibly be important in determining the musical properties of an instrument—is the behavior at frequencies between the middle and the highest mode: we now observe a radical dip in the radiativity, rather than the plateau characteristic of the region between the Helmholtz mode and the first wood mode. The explanation is, of course, clear; since the middle and highest modes have strengths of the same sign, they will cancel, rather than add, when the lower one is already inertia limited but the higher one is still elasticity limited. It is worth noting that an even clearer signature of this kind of behavior is the *retrograde phase*, that is, a phase slope which is negative instead of positive.

This type of behavior suggests that it may be important to distinguish modes whose strengths have opposite signs. Although it is tempting to call one type "airlike" and the other "woodlike," by extension of the nature of the lowest two, it seems to me that this would be a misleading nomenclature, since all modes, and especially the higher ones, are in fact combinations of air motion and wood motion, and the actual sign of the monopole strength is determined as a balance between the two; moreover, in the higher modes, which have some nodal lines in the wood motion, even that wood motion itself will have both positive and negative contributions. Accordingly, I prefer to refer to the two types simply as negative and positive modes. The general rule that will prevent the occurrence of regions of retrograde phase, and the corresponding sharp dips in the radiativity amplitude, is then that *positive and negative modes should alternate in frequency.*

The type of quantity that we have been plotting, defined as the monopole moment developed by a violin per unit force amplitude applied to the bridge, is called its *radiativity*. Figures 10(a)–10(c) show three examples of experimentally measured monopole radiativity data, corresponding to three violins of widely different qualities. It is interesting to note that a retrograde phase region somewhere in the 450–550 Hz range appears in all three. Since in this region the density of modes is still low, it would be possible, in principle, to identify the exact nature of all of them; armed with such knowledge, plus a knowledge of physics (or the collaboration of a physicist), a violin maker could adjust the individual modal frequencies up or down. Whether this could result in an appreciable improvement in instrument quality is, at this time, not definitively known, although some strong suggestions in that direction have been made.

C. Synthesis of violin sound

As we saw in Sec. II D it is possible, on the basis of the theory we have developed in this discussion, to program a computer to integrate the appropriate differential equations so as to obtain a simulation of the actual motion of a bowed string. In the same way, our discussion of radiativity, including measurements on an actual violin, can be used to simulate the sound field produced for any given time dependence of the force applied to the bridge. The simplest case is that of a violin which is simply tapped, that is, to which a force pulse is applied, since the computation of the resulting sound does not depend on any understanding of the string dynamics. From there we can proceed to the computation of the sound produced by a violin which is plucked, that is, played "pizzicato." Here the string dynamics does enter, since we have to understand not only the ideal motion of a plucked string but also how the vibration dies away with time; but the dynamics of the interaction with the bow, which is considerably more complicated, can still be omitted. Finally, we can combine everything we have studied in order to synthesize the full violin sound.

(The original presentation of this paper included at this point a tape recording of the following synthesized sounds, constructed on the basis of the above considerations.

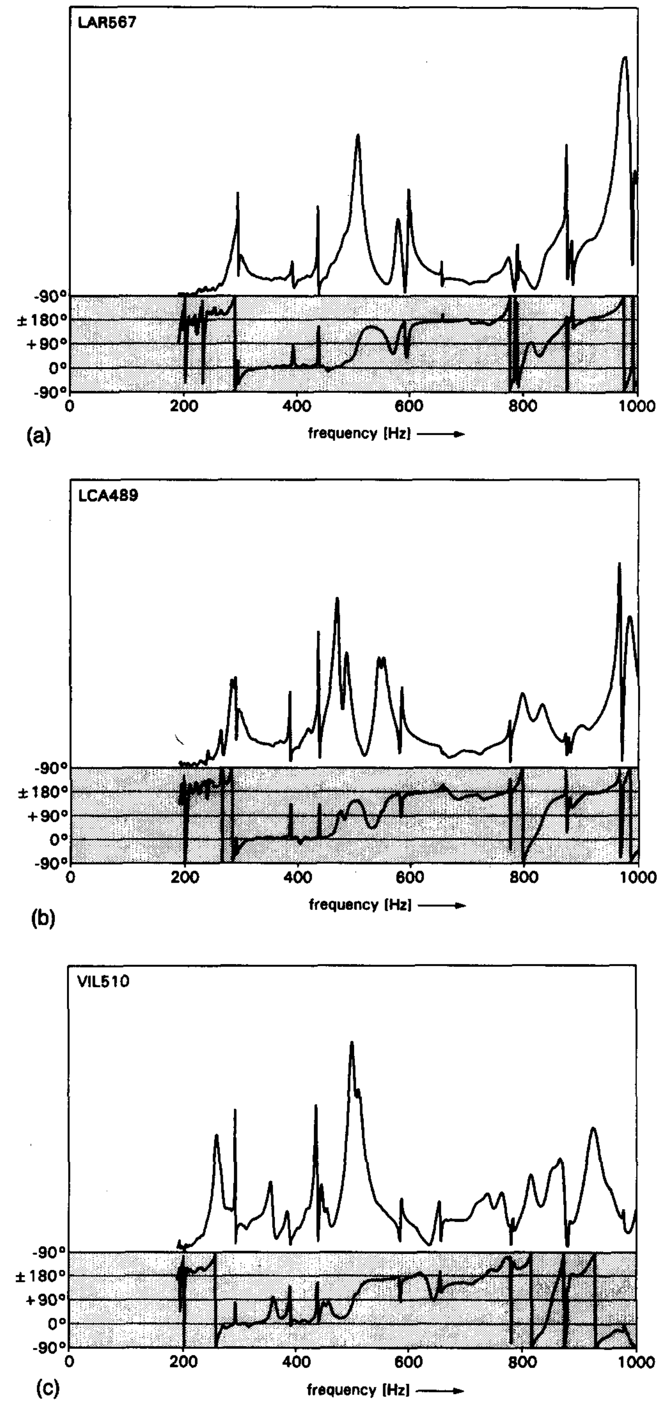


Fig. 10. Measured monopole radiativities of three actual violins between 190 and 1000 Hz. The three differ radically in quality

(1) The sound of a violin being tapped with pulses of various lengths.

(2) A *pizzicato* passage from a Bach trio sonata.

(3) The same passage *arco*, or bowed. The sound is that of a novice whose bowing technique is very poor; this should come as no surprise, since we spent a great deal of time programming the violin and none programming the violinist.

(4) As a lighthearted variation, we played the same pas-

sage with random errors in frequency. This adds a certain realism, since the bowing quality of a beginner is now combined with appropriately poor intonation.

(5) A three-octave scale, starting with the *G* below middle *C*, the lowest note a violin can play.

(6) Finally, we returned to the Bach passage, but this time "accompanied by" a synthetic piano which the author had worked on some time earlier. This piano had also been programmed on the basis of "first principles," but no discussion of it was presented.)

V. THIRD MEDITATION

We have spent by far the greater portion of our time on the first part of the title, seemingly ignoring the second altogether. There are excellent reasons for this.

First of all, from where we stand, we can certainly see the horizon, but not what is beyond the horizon, so that addressing the question of what we do not know is, at best, trying to describe our horizon. Exciting as that can be, we will finally be forced to acknowledge that the horizon is only one dimensional. In other words, describing what we do not know is really no different from describing the outer reaches of what we do know.

Second—and more importantly—the question of what we do not know is difficult for a physicist because of what we pointed out in our first meditation, namely that physics is defined, not by its subject matter, but by the methods it uses. Therefore, trying to state what physics we do not know is equivalent to compiling a list of questions to which we would like to know the answers, and to which we think that the methods of physics are applicable; by definition, this would itself be a fundamental contribution to the research. Again, stating what we do not know becomes an enumeration of things we *do* know. I think that is why people sometimes say that engineering is an activity involved with finding correct answers, but physics is an activity involved with finding correct questions.

VI. WHAT SCIENCE DOES NOT KNOW

A. Judging excellent violins

After the thoughts I presented in Sec. III it may seem paradoxical that, in trying to formulate what science does not know about violins, I myself come up with the statement that what we lack is the Secret of Stradivarius. I use that phrase more metaphorically, however, as I shall now try to explain.

No knowledgeable person will deny that a certain Antonio Stradivari, working in Cremona during the decades around 1700, produced some very, very fine violins; at the same time, no knowledgeable person will deny that throughout the past three centuries there have been makers—not, perhaps, great hordes of them, but nonetheless some—producing very fine instruments, right down to our own day. Indeed, the task of making precise comparisons borders on the meaningless, especially in view of the fact that a genuine Stradivarius, with its great museum value and even greater mystique, can command an enormous price on the market.

But, you may say, why not simply compare the sounds of a number of instruments, by having a qualified jury express its opinions after listening to each of them played behind a screen? Unfortunately, a violin, unlike, for example, a piano, does not produce any sound, or at least not any "violin sound," unless it is played by a violinist; and to test dependably the sound of a very good violin obviously calls for the services of a very good violinist. But what, then, is the jury hearing, the violin or the violinist? Because the truth is that, as can be easily verified, if you give a virtuoso violinist the junkiest instrument in the world, the sound that comes out will very quickly bring tears to your eyes; but the conclusion to be drawn is not that quality makes no difference. Regardless of the effect produced on the listeners, the violinist himself will be keenly aware of his instrument's low quality, because of the way it "has to be fought" to produce any kind of decent sound—whereas a fine violin gives the illusion of "playing itself," as many players will attest. For this reason, if there is a concert to be given, the good violinist will choose the best instrument he (or she) can afford, thus minimizing the energy drain that goes into controlling the instrument and leaving more to be devoted to playing the music itself.

In this way it becomes clear that which of a number of violins is "the best" can be defined only in relation to a particular violinist: it is the one which responds best to that particular player's taste and manner of playing. In fact, it is well known that very good violinists can, and do, disagree on which of a number of violins is the best.

B. The new secret of Stradivarius

But in spite of the ambiguities which exist in judging the quality of outstanding instruments, it remains true—so true, in fact, that it is often forgotten—that there is no ambiguity at all on a coarser level. In particular, if we hand any experienced player a violin and ask that it be classified into one of three categories: (a) "student instrument;" (b) "decent professional instrument;" or (c) "fine solo instrument," the judgment would not take more than about 30 s, and the opinions of different violinists would coincide absolutely. (Perhaps "absolutely" is an overstatement, since borderline cases are always possible; but at least there would not be any question about *which* border such an instrument is on.) The consensus that can be found on this matter, no matter how independently and discreetly the questions are asked of the different experts, establishes that we are dealing with a quality which is *objective*, and which can therefore, in principle, be translated into physical specifications, determined by tests and measurements that can be defined in purely physical terms. But the tantalizing fact is that *no such specification which successfully defines even coarse divisions in instrument quality is known.* It is this extremely frustrating fact to which I refer as "The New Secret of Stradivarius."

In fairness, I should state that many people have tried their hand at this question, and bits and pieces have, at times, appeared to emerge. But—so it seems to me—in order to constitute a valid physical understanding, any alleged answer, whether partial or total, would have to include a physically functional specification of *what it is that a violin is supposed to do*, a specification whose nature is such that, in principle at least, it does not exclude the possibility of an instrument being constructed in the future which is better than any that have been built in the past. The evaluation of a particular instrument would then consist of a simple measurement of the degree to which it does do what it is supposed to do. But I emphasize that the original specification must be *in terms of physically mea-*

surable properties; to "be responsive under one's finger," "have a beautiful sound which carries," "speak clearly and easily," and other such expressions which are no doubt meaningful but have not been translated into physical properties, cannot be considered to answer the question I am posing.

VI. CONCLUDING MEDITATION

Many years ago, just after I finished presenting a colloquium on piano physics at Michigan State University, an anonymous student handed me a sketch he had made while listening to me. It was entitled "Great Moments in Physics #42: Galileo Begins the Study of Musical Instruments," and showed the great man himself dropping a piano and a saxophone side by side from the top of the Leaning Tower of Pisa.

At moments of discouragement, I have been known to look at that picture and wonder just how far we have come from such an apocryphal beginning. Yet the truth is that we have come an enormous distance. The trouble is that the nature of research is forever to be doing something that we do not know how to do and, as soon as we have learned how to do it, to stop doing it and look for a new problem; this means that a researcher's mind is forever fixed on what has *not* been achieved—which, by the standards of the world, means being condemned to a life of perpetual discouragement. That this is not the way that we researchers perceive it is one of the great miracles of human creativity, and the primary reason that we love our work as much as we do.

ACKNOWLEDGMENTS

That this paper is completely devoid of references is entirely a matter of the style in which it was presented, and is by no means meant to imply that all the results quoted are the author's own. On the contrary, I am, like every scientist, infinitely indebted both to the myriad of others who have contributed to our knowledge and understanding, and to the generally open and sharing atmosphere that exists in our profession, making good research possible. By contrast, the many occasions on which I have, in the course of this presentation, gotten out on a limb are occasions for which I myself accept complete responsibility. I would like also to acknowledge the support of the National Science Foundation, which over the years has furthered my research immensely. Readers who would like to pursue the subject to a deeper level will do well to begin with the book *The Physics of Musical Instruments* by N. H. Fletcher and T. D. Rossing, which contains not only excellent presentations but extensive bibliographies as well.

Fluctuation and dissipation in Brownian motion

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An analysis of Brownian motion based upon a "Langevin equation" form of Newton's second law provides a physically motivated introduction to the theory of continuous Markov processes, which in turn illuminates the subtle mathematical underpinnings of the Langevin equation. But the Langevin approach to Brownian motion requires one to *assume* that the collisional forces of the bath molecules on the Brownian particle artfully resolve themselves into a "dissipative drag" component and a "zero-mean fluctuating" component. A physically more plausible approach is provided by a simple discrete-state jump Markov process that models in a highly idealized way the immediate effects of individual molecular collisions on the velocity of the Brownian particle. The predictions of this jump Markov process model in the continuum limit are found to precisely duplicate the predictions of the Langevin equation, thereby validating the critical two-force assumption of the Langevin approach.

I. INTRODUCTION

Brownian motion is the motion of a macroscopically small but microscopically large particle that is subject only to the collisional forces exerted by the molecules of a surrounding fluid. If M denotes the particle's mass and $V(t)$ its instantaneous velocity, then the traditional way of analyzing Brownian motion[1–4] is to begin with the Newton's second law equation

$$M\frac{dV(t)}{dt} = -\gamma V(t) + f\Gamma(t). \tag{1}$$

Here γ is a positive constant called the drag coefficient, $\Gamma(t)$ is an entity called the Gaussian white noise process (which will be discussed more fully later), and f is a constant whose value remains to be specified. The physical interpretation of Eq. (1) is that the particle is subject to two kinds of forces: a steady dissipative drag force