SOUND RADIATION FROM THE VIOLIN—
AS WE KNOW IT TODAY

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INTRODUCTION

To the best of my knowledge, the first measurement of sound radiation from a violin was by Backhaus in 1928 [1] (which makes this endeavor exactly as old as I am). Having now been asked to summarize the field "as we know it today" on the occasion of Erik Jansson's sixtieth birthday, I decided not to try to enumerate all the data on the multiplicity of violins that is today available, nor to present a complete history of everything that has ever been done in the field. Instead, my purpose will be to discuss the options that are available in designing such a project, to list the various types of difficulties that each of them entails, and to inquire how the accessibility of different methods has changed over this period.

One general remark: most people would accept the statement that "the function of a violin is to make sound" as essentially tautological. It seems to follow from this that the *quality* of a violin must somehow be encapsulated in *how well it makes sound;* therefore, a sufficiently complete measurement of quantities like radiativity is all that one needs to define the difference in quality between one violin and another. We shall return at the end of the paper to a discussion of where that reasoning is insufficient.

METHOD OF EXCITATION

In most early experiments, the violin was excited by what is surely the most obvious method — with a bow. Yet the very first measurement [1] of the radiation field was quite ambitious, in that it set itself the task of exploring not only the field's overall magnitude but also its directionality. For this purpose, and in the absence of the kind of electronics that can capture the signals from a large number of microphones simultaneously, either the microphone or the violin had to be rotated around an axis passing through the violin. This required the violin's excitation (including, if possible, its phase) to be kept absolutely steady for the length of time that it takes the apparatus to make a complete revolution; and, if the results are to encompass a complete sphere rather than just a single circle, much longer than that. That is an unreasonable demand to make of any bowing machine, let alone of a human player. For that reason, Backhaus [1] (who used a small mechanically driven turntable in an anechoic chamber) experimented with electromagnetic excitation, but unfortunately gave us only a scant glimpse into his actual setup. It seems clear, from his reproduction of the oscillographic signal photographically captured as the violin rotated, that the stimulus was not sinusoidal (he refers to it as *elektromagnetische Anzupfung*, or "electromagnetic plucking"), but I, at least, was not able to figure out what exactly it was.

The big difficulty, as Backhaus reports it, was lack of stability. This is, indeed, understandable if his method consisted of an electromagnetic drive (in which the magnetic field of a permanent magnet surrounds some region of the metallic string and an oscillating current is passed through the string) whose externally controlled frequency is made to coincide with a string resonance. Because of the very high Q associated with such a resonance, a slight uncontrolled drift in tuning is difficult to avoid.

For this reason, Backhaus's experiment was a failure. The best that he could produce — after what was obviously a great deal of work — is one single curve at 3300 Hz (the fifth partial of the E-string) for half of a circle, and with no phase information. As far as the data itself is concerned, it appears reasonable enough; but since the frequency in question is deep in the region of directional tone color, where the angular dependence is a very rapid function of frequency [2], it is difficult to judge it in a detailed way.

After Backhaus, researchers went back to bowing, sometimes by hand, sometimes by machines of varied design [3, 4]. A return to electromagnetic excitation was made by Watson, Cunningham, and Saunders in 1941 [5], with the instability that plagued Backhaus obviated by damping all the strings; the excitation could now be viewed as applying a force directly to the bridge. Considerably later, the damped string electromagnetic driver was further developed by Dünnwald [6], who used it extensively; before him, Meyer had accomplished a similar purpose with an electromechanical actuator [7].

With the development of digital methods, yet another mode of excitation appeared: the force hammer. Here the bridge is laterally impacted by a mallet that contains a piezoelectric force sensor in its head, producing an exact electronic record of the impact force as a function of time; simultaneously, an appropriately positioned microphone obtains a similar record of the sound signal as a function of time. A Fast Fourier Transform performed on the two signals then gives us both the applied force and the resulting sound in the frequency domain, after which the usual assumption of linearity allows us to divide one by the other and thus obtain the appropriate radiativity function as a function of frequency. This method was used by Bissinger and Keiffer [8] in obtaining the total radiated power.

A rather different method, based on the principle of reciprocity, was first published by Weinreich in 1985 [9]. Here the violin is put into vibration by an external loudspeaker, and the motion of the bridge detected with small accelerometers; by the principle of reciprocity, one can then compute the field that would be radiated if a known lateral force were applied to the bridge, just as the vibrating string in fact applies it. In the 1985 paper the interest is primarily in rather low frequencies, when the behavior of the violin is well parameterized by a small number of multipole moments (specifically, the monopole moment and three components of the dipole moment), and all the computation is directed toward that end; in fact, however, the same method is straightforwardly applicable to the simple directional analysis of the field, by placing the driving loudspeaker in any desired direction from the violin and measuring the bridge motion then.

The great advantage of the reciprocity method is that, in terms of size and weight, a transducer that detects motion can usually be made much smaller and lighter than one which exerts a force (or imposes a motion; in later work [2], we have replaced the accelerometers with a phonograph pickup stylus resting on the bridge, thus reducing both the load on the violin and the possible damage to it to negligibility.)

In spite of the availability of these various newer excitation methods, Wang and Burroughs [10] returned to a mechanically bowed setup, using a continuous belt meticulously hand sewn with rosined horsehair. They justify the considerable trouble involved in building and maintaining such a "bow" by observing that it allows them to excite, in addition to the more obvious transverse motion of the string, the "correct" amount of torsional motion as well. Although it is true that torsional motion, by exerting a torque on the bridge notch instead of a force, would modify the degree to which various shell modes are excited by the bowed string, the quantitative importance of this effect is (in my opinion) doubtful. Nonetheless, it would certainly be interesting to test this question by using the apparatus of Wang and Burroughs and varying the diameter of the string.

THE SAUNDERS LOUDNESS CURVE

The considerations are different, however, for the case of the so-called *Saunders loudness curve* [3, 11]; Saunders himself refers to it as the *total intensity curve.* In the original experiment described by him, the chosen gamut of the violin is covered in semitone steps twice: the first time by bowing as softly as possible, the second time as loudly as possible. The desired curve is then obtained by averaging the two results for each note. The reason this situation is in a different category from the others that we have considered is that both the soft limit and the loud limit are presumably functions not of the violin alone but of the violin *when it is bowed;* it is, in other words, a composite function of the violin's ability to radiate sound and of the bow's ability to set the string into smooth vibration. In fact, available theories of the two limits attribute their existence to quite different mechanisms; this is obvious when you consider that the loud limit must involve nonlinear dynamics of the driven string, which cannot play a role in the determination of the soft limit.

According to Saunders the difference between the two curves, which can be identified with the *dynamic range* of the instrument, is pretty much constant at 30 dB. However, if that were precisely true there would, of course, be no sense in measuring them both separately. Indeed, later applications of the loudness curve [12, 13] drop the soft measurement and concentrate entirely on the loud.

If the exactly constant offset between the two, regardless of which pitch is chosen on which instrument, were an absolute fact, there could be only one conclusion: the ratio of the two *string amplitudes* corresponding to the soft and loud limits must be independent of the bridge admittance as seen by the string; otherwise, since the two limits are determined by quite different laws, it would be an absolute miracle if they tracked each other so precisely. The fact that the loudness curve alone (or the "softness curve" alone) does vary, both between notes and between instruments, would then have to be attributed to the difference in how good those instruments (or the different notes on one instrument) are at converting string vibrations into sound. If that were the case, there would really be no point in taking the loudness curves at all, one of the more modern excitation methods being completely sufficient.

However, I personally do not believe that this is the case except in a first approximation. Saunders himself remarks that there are some variations in the dynamic range, but that they are "due probably to the player." This remains an interesting area to which some of the modern bowing machines should be able to provide good answers.

DIRECTIONALITY OF DATA

With regard to the kind of attention paid to direction by various researchers, their data can generally be divided into three categories: unidirectional, total, and directional.

In spite of the fact that the very earliest work on the subject, that of Backhaus [1], made a serious (even if unsuccessful) attempt to determine directional properties, both Saunders [3] and Meinel [4], who followed him, seemed somewhat unclear on the subject. Both indicate the necessity to choose the direction of measurement (that is, of microphone placement) carefully and to keep it constant as data was taken — in other words, they are well aware that the direction *matters;* yet both of them are comfortable limiting their data to a single direction, as though a single direction can validly describe "the sound of the violin in general." The same comment applies to Dünnwald [6], who explicitly chooses a direction "typical of listeners' position in a solo concert," but collects his data in an anechoic chamber, which hardly duplicates what listeners in any concert hear.

In fact, the choice of direction is not crucial at frequencies below about 850 Hz, where the radiation pattern is close to isotropic. On the other hand, at high frequencies the location of peaks and valleys as a function of frequency can be very different for different directions [2].

In terms of the nomenclature introduced at the beginning of this section, we thus have to classify the data of Meinel, Saunders, and Dünnwald as *unidirectional.*

At the other extreme from *unidirectional* data is *total* data, which reports the sound power radiated, not in some chosen single direction, but throughout the 4ð solid angle that surrounds the violin. One way to collect such data is in a reverberation chamber, a space with highly reflecting walls which, in effect, cause the microphone to hear every sound a number of times coming from different directions. This was the method adopted by Jansson and his colleagues for the purpose of obtaining so-called long-time average spectra [14, 15, 16].

"*...*understanding can only come from the *perception of patterns.*"

Bissinger and Keiffer [8] measure total radiated power (still as a function of frequency) by a different method: placing the violin in an anechoic chamber, they use the force hammer method to obtain the radiated field at each of 266 microphone positions situated on a sphere of 1.2 m radius. They then averages the results (after throwing away the phase information, of course) to obtain information about the total radiated power, presuming that 266 is, for practical purposes, equivalent to infinity. How good that presumption is depends, of course, on the wavelength of the sound, which in principle must be large compared to the spacing of microphone positions. If it goes to the other extreme, becoming small compared to the spacing of at least some of the microphone positions, it becomes easy for features of the radiation pattern to "sneak through" between microphones and thus not be detected. It would appear for the situation of Bissinger and Keiffer that the cutoff frequency would be somewhere in the region of a few kilohertz.

Note that any data covering more than one direction simultaneously necessarily require throwing away phase information.

DISPLAY OF DATA

In my first research proposal to the National Science Foundation in the field of violin physics, which I submitted in 1977, I undertook to record detailed measurements of the radiated acoustic far-field amplitude per unit force exerted on the bridge as a function both of frequency and of the two spherical angles. Although the proposal was approved a little over a year later, and a number of exploratory measurements were done, the experiment itself was never completed — primarily because I realized I had no good method of displaying the results in a way that would carry visual meaning, and I was not interested in filling my shelves (let alone journal pages) with tons of meaningless printouts.

It should be emphasized that the importance of a good display method is a lot more than simply saving space. After all, the scientist, contrary to some popular opinion, is driven not by a *thirst for knowledge* but by a *thirst for understanding;* and understanding can only come from the *perception of patterns.* Such a perception, in turn, requires data to be presented in a form that *our senses can make sense out of.* In our case, this means using a three-dimensional space (since the independent variables are frequency and the two polar angles), somehow placing at each point of that space a mark that contains information on two dependent variables (such as amplitude and phase of the radiation field at some large fixed distance from the center of the violin). It also, incidentally, requires computing graphics power such as was not readily available in 1977.

The most recent violin results that attempt such a display are contained in the elegant holographic work of Wang and Burroughs [10], who supplement the common presentation methods by the use of (a) perspective and (b) color. In principle, the first effectively adds one dimension, allowing the use of three independent variables; whereas the second allows each point to show three separate dependent variables, since color is usually thought of as three-dimensional (represented, for example, by hue, intensity, and value; or by red, green, and blue components). In fact, however, such a use of color would be incompatible with perspective, since the coloring of the plane nearest the viewer would completely block anything that is behind it.

That is not, however, in any case the scheme that Wang and Burroughs attempt. First of all, for reasons that we shall mention in a minute, they do not wish to limit themselves to the far-field radiation field, but are interested in visualizing it as a function of radius also; in other words, they deal with three, rather than two, independent space variables (which they choose as *X*, *Y*, and *Z*). Accordingly, they are forced to abandon the frequency as a component of the display, instead presenting a number of separate pictures each labeled with a frequency. In their case, this does not represent a loss, since their frequencies are determined as the partials of a bowed string, which are in any case well separated.

Secondly, Wang and Burroughs do not present either the pressure amplitude or pressure phase of the field but rather the intensity vector, a quantity easily obtained if both of the more primary quantities are known, as they surely are in a holographic experiment. (It is not possible, however, to reverse this transformation.) They do this both by color and by drawing arrow vectors, concentrating on one plane at a time to avoid the non-transparency problem that we mentioned above. The rationale behind reporting only the intensity appears to be to look for "hot spots" which act as dominant sources of energy (although, as the authors both indicate and illustrate, they may sometimes be sinks rather than sources). The implications of such "hot spots" are, however, unclear, since the intensity vector at a point is the product of pressure and velocity there, but it is not at all evident that the pressure at a given location may not be *caused* by motion of the shell somewhere completely different.

More specifically, in order for the velocity pattern to be localized in some region of the shell, the size of that region must be of the order of the wavelength of wood bending waves at the frequency in question; whereas in order to localize the pressure in that region — that is, to attribute the pressure to local motion — the wavelength of sound waves must not be larger than the region. This means that the air wavelength must be smaller than the wood wavelength, which requires that the frequency be higher than the coincidence frequency. If we accept Cremer's estimates [17] for the coincidence frequency of the violin shell as about 5 kHz parallel to the grain and about 18 kHz perpendicular to the grain, this condition is never satisfied in the range treated by Wang and Burroughs.

FREQUENCY VS. MODE

Although historically knowledge of violin modes developed early, it was realized only slowly that as far as understanding the vibration of the violin system is concerned, *modes are all there is.* I believe that the breakthrough came with the work of Schelleng [18], who pointed out that the relatively high radiativity plateau between the A0 mode and the next large peak (the B1), as contrasted with the deep valley between B1 and B2, is due to the A0 and B1 having, in his terms, "opposite polarity." The terminology is, in a way, unfortunate, because the "oppositeness" is not really a characteristic of the modes themselves but of the *radiativity* of the modes when probed at a particular point — namely *at the side of the bridge* (which corresponds to the direction of the force applied by the vibrating string); nonetheless, Schelleng's understanding makes it clear that the plateau is not due to any kind of "background level" because nothing of the kind exists (other than the tails of other modes, of course). There is no "background" because *modes are all there is.*

This "radically modal" point of view suggests that frequency, the (continuous) independent variable that had always been considered the *sine qua non* of responsible radiation measurements, should really be replaced by the (discrete) variable of mode number (or, better, mode *name,* since in many cases the modes do not form a naturally ordered set). Of course mode radiation parameters are experimentally determined by measuring things as a function of frequency (or, in the case of impulse responses as when a force hammer is used, as a function of time); once they have been determined, however, we can always deduce the behavior of the system at any given frequency from those mode parameters, the characteristic frequency of each mode, and the strength with which that mode is represented in the particular quantity in question. The variation with frequency is then determined by those numbers plus modal response functions, which are universal. (In mathematical terms, we would say that a function is here being specified by the location of its poles and the values of the corresponding residues, rather than by giving the value of the function for every value of the independent variable.)

"...modes are all there is."

As an example, if we wished to specify the angular distribution of radiation from a violin, which may be changing rapidly with frequency, we would only need to describe the angular distribution radiated by each mode, plus the strength with which that mode is excited by the particular method of excitation used. One may remark that the space of modes is not only discrete, but in most practical cases there are, in effect, only a finite number of modes that contribute to the result, so that the simplification involved in using mode instead of frequency as the independent variable is enormous. Bissinger [8] was the first to introduce these ideas into radiation measurements. Even though the particular quantities with which he deals are relatively simple in that they are averaged over angles and so eliminate two independent variables, his papers do correctly introduce the "radically modal" point of view, which will, I believe, characterize all future work. A closely related, but less explicitly formulated, approach characterizes the work of Weinreich, Holmes, and Mellody [19], whose results, even though they are presented as a function of frequency, call the reader's attention primarily to the behavior of modes. By contrast, Wang and Burroughs [10] represent the older point of view: since their data is collected at discrete and well-separated frequencies that have no connection to the frequencies of particular modes, they do not allow any "natural" interpolation of their frequency dependence which, for "radically modal" data, can be done exactly.

FREQUENCY CONTROL IN MEASUREMENT

In his 1928 paper, Backhaus [1] complained about the great instability of his system when the violin string was excited electromagnetically, presumably at one of its resonant frequencies; and we earlier commented on the difficulty of avoiding such instability when one is trying to sit on top of a resonance of very high Q. Although the primitive state of 1928 electronics probably did not allow him to do it, today that would not have been a problem: we would simply lock the driving frequency to that of the string (for example, by sensing the phase difference between the driving signal and the phase of the oscillation). Exciting the string by bowing accomplishes the same thing, since the bow does not impose its own frequency but locks to that of the string.

Of course the radically modal point of view, which we now advocate, would in any case seek to avoid an externally imposed frequency regardless how stable; but the same idea of locking the drive to the normal mode of a string can also be used to lock it to a normal mode of the violin body (this will generally require having a few sensors in different places on the violin whose outputs are combined with appropriate phases and amplitudes to favor one mode over neighboring ones). Naturally, if the aim were to measure radiation field properties, it would be wise to choose a driving method other than a loudspeaker, such as the method of Meyer [7] or of Dünnwald [6]. If the signal returned by the sensors is given back to the driver in a positive feedback loop, one is, in effect, increasing the Q of that mode compared to any others; if the Q reaches infinity, not only will that be the only mode excited, but it will also go into steady spontaneous oscillation so that appropriate modal characteristics, such as patterns of the radiation field, can be measured at leisure. In practice, of course, it is not possible to adjust the Q so it is exactly infinity; rather, it is necessary to adjust the feedback so Q is greater than infinity (that is, negative), which will make the oscillation grow. One must then also add to the circuit a soft nonlinear saturation characteristic that will make the oscillation amplitude reach a stable steady state (that such a method can actually be made to work was demonstrated to me by Eric Arnold, in my own laboratory, around 1980).

***"...it is the composite "machine" of violin + violinist and not the violin alone* that is the source of the sound."**

CONCLUSION: IS RADIATIVITY EVERYTHING?

At the beginning of this paper we pointed out that, since the only function of the violin is to make sound, one might conclude that the characteristics of its sound-making activity — that is, its radiativity, when measured as a function of all relevant independent variables — ought to, when correctly interpreted, completely determine the quality of the instrument; and therefore people interested in violin quality can pretty much stop measuring anything else.

The fallacy in that reasoning becomes obvious, however, the moment we consider that the most distinguished Stradivarius or Guarnerius is capable also of making sounds that are absolutely horrid, as one can easily convince oneself by handing it to a four-year-old child and asking him to play (or just imagining the experiment, which avoids putting a very expensive instrument at risk). The point is, of course, that it is *the composite "machine" of violin + violinist and not the violin alone* that is the source of the sound in question; and whereas it is straightforward to feed some input in the way of printed music to this composite and attempt to judge the quality of what comes out, in itself that provides relatively little help to the violin maker.

That is the reason why *radiativity is not everything;* rather, the quality of a violin is determined by a host of properties including not only the input impedance at the string (which reflects in important ways on the "feel" of the string when it is bowed) but also seemingly primitive factors like the exact angles at which the fingerboard is planed, which contributes to the comfort of the violinist's fingers and, hence, the impression that more energy can be devoted to making beautiful music and less to fighting the violin. Indeed, it is surely true that not all of the influences perceived by the violinist are yet known to us, let alone understood or measured.

Nonetheless, among the properties that determine violin quality, radiativity remains privileged: a good violinist may be able to overcome almost any of the interactive quantities that he/she directly senses, but if an instrument is, for example, incapable of producing any sound above 4 kHz, there is little that even the greatest virtuoso can do about it. That is why, in my opinion, the study of violin radiativity will continue to occupy a central place in the ongoing research on this most fascinating instrument. ■ **CASJ**

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