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RENAISSANCE MUSIC AND EXPERIMENTAL SCIENCE

BY STILLMAN DRAKE

The influence of early modern science on musical theory and practice has not gone unnoticed by historians of music. Professor Claude Palisca has called particular attention to the impact of scientific thought on the music of the late sixteenth and early seventeenth centuries. His "Scientific Empiricism and Musical Thought" concludes with this summary: "It would be wrong to conclude from this exposition that the sensualism and freedom of early Baroque music can be ascribed mainly to the liberalizing force of scientific investigation. . . . Scientific thought did reveal, however, the falsity of some of the premises on which existing rationalizations of artistic procedures had rested. . . . By creating a favorable climate for experiment and the acceptance of new ideas, the scientific revolution greatly encouraged and accelerated a direction that musical art had already taken. Finally, the new acoustics replaced that elaborate conglomeration of myth, scholastic dogma, mysticism and numerology that had been the foundation of the older musical theory, giving it a less monumental but more permanent base."¹

The reciprocal influence of musical theory and practice upon early modern science, however, has been neglected by historians of science. This is somewhat surprising when we consider that music in medieval education held an equal place in the quadrivium with arithmetic, geometry, and astronomy as the recognized mathematical disciplines. For that reason alone, music might be expected to have remained rather closely associated with mathematics and science, and to have shared in their sudden transformations during the late Renaissance. I am convinced that that was indeed the case, and that the origins of the experimental aspect of modern science are to be sought in sixteenth-century music, just as its mathematical origins have been traced to the ancient Greek astronomers and to Archimedes.

In saying this, I have in mind the emergence of physics as a distinct separate study, no longer an integral part of philosophy, after the work of Galileo. The new physics, in which mathematics and experiment replaced logic and authority, was so successful as to become a sort of model for other sciences, at least so far as method was concerned. It is, of course, possible to trace the lineage of physics as far back as one wishes. Any form of intellectual activity that exists in one

¹C. Palisca's article in *Seventeenth Century Science and the Arts*, ed. H. H. Rhys (Princeton, 1961), 91-137.

generation can be found to have some counterpart in the preceding generation, and it is instructive to detect and study these. On the other hand, it is no less instructive to recognize the occurrence from time to time of fundamental changes in patterns of intellectual activity. One way to do this is to select some pattern in modern times, and to move back until an essential element in the pattern disappears, or at least becomes of negligible importance in the writings of men who pursued the activity under study.

It is easy to trace modern physical science back in an unbroken line to Sir Isaac Newton, who unquestionably linked experimental evidence in his work with mathematical laws, in a way that is highly characteristic of science today. Many historians of science feel that it is proper to trace this same unbroken line further back, to Galileo. Others, who would pursue it yet further, are soon confronted with definite evidence of discontinuities, to say the least. The most influential historians of science (e.g. Pierre Duhem, Alexandre Koyré) contend that the work of Galileo was merely a continuation of lines of thought laid down in the Middle Ages by men whose work subsequently fell into obscurity until very recent times. In that way a link with classical antiquity is found, through commentators on Euclid and Aristotle who flourished in the Middle Ages. But if Galileo's work was a continuation of medieval thought, there seems to have been some interruption, for it is hard to find any sixteenth-century writers on mechanics whose conception of the relation of mathematics to experimental evidence even resembles that of Galileo. Hence, if we want to go farther back than Galileo in an unbroken line, we have to proceed with caution.

Obviously the unbroken line reaching back from the present will end wherever we discover the disappearance from physics of either mathematics or experiment. Now, the sixteenth century marked a notable increase of emphasis on and development of mathematics, ushered in by the publication and translation of ancient Greek works such as those of Euclid, Archimedes, and Pappus of Alexandria. And since mathematics pervades all typically sixteenth-century writings on mechanics, our quest turns automatically into a search for the origins of experiment. But here we shall need some kind of definition of experiment in its characteristically scientific sense.

For such a definition we can eliminate at once the concept of experiment that is associated with the name of Sir Francis Bacon; that is, the idea that we can arrive at the true explanation of things by the systematic and patient accumulation of observed facts, without any preconceived theory. In Bacon's view, the explanation would ultimately force itself upon us as a result of this procedure. That may be true, though in many matters it is open to doubt; but whether it is true or not, it has really nothing to do with science. The mere accumu-

lation of data, useful though its results may be, does not constitute scientific knowledge as such. I shall cite a single example in illustration of this. One of Galileo's "Two New Sciences," published in 1638, was the science of strength of materials. There had been an impressive accumulation of information on that subject by earlier builders and engineers. But it was not such knowledge that gave rise to that science, or ever would have given rise to it, though that knowledge can be seen to fit into Galileo's new science. The new science was mathematical in character, and it developed not out of engineering, but out of the theoretical science of mechanics, and it was derived by Galileo from the Archimedean law of the lever. So in eliminating the Baconian definition of experiment, I am not denying the existence from time immemorial of accumulated observational data. Still less am I denying the usefulness of such activities. I mean only that it is not the Baconian type of experimentation that did, or could, account for the rise of modern science.

The kind of experimentation which interests historians of physics is the deliberate manipulation of physical objects for the purpose of corroborating by their behavior a definitely preconceived mathematical rule, or for the purpose of discovering a mathematical rule applicable to their behavior. Some physical objects are not amenable to manipulation—e.g., the heavenly bodies—so in their case deliberate selections of precise observations are made, and comparisons of them serve in place of manipulation. Thus some kind of tinkering with external measurements, directly or indirectly, is involved. Such an investigation, with an exact rule in question, for the purpose of seeing whether that rule is obeyed, or of discovering some exact rule, constitutes scientific experimentation. In physics and astronomy, "exact rule" means some mathematical expression; in other sciences, a rule may be considered precise without its being mathematically expressed. But it is always the testing of exact rules by some kind of external activity that distinguishes experimental method in science from various other possible approaches to knowledge and truth.

It is therefore natural for historians of science to be interested in discovering when, and under what circumstances, this approach began its uninterrupted prosecution down to the present time. It is equally natural for them to suppose that, so far as physics is concerned, experiment in the sense of manipulations to confirm or establish a law began in mechanics.

Proceeding thus, it is tempting to suppose that the first physical experiments were concerned with relatively simple measurements of equilibrium and of motion; for example, with the balancing of weights and the measuring of speeds by comparing distances and times. Suppositions of that kind are so easily made, and so intrinsically plausible

that they tend to go unquestioned. It is thus that people reconstruct the invention of the wheel by supposing that it must have evolved from the use of logs or rollers in the moving of heavy objects. Fortunately for those who enjoy that kind of speculation, the invention of the wheel goes so far back that they need not fear contradiction, though they cannot adduce the slightest positive evidence in favor of their unanimous conjecture. A historian who may try to discover the origin of scientific experimentation, however, is less fortunate. In unbroken succession, this really does not go very far back in time; not even as far back as the invention of printing, let alone of writing. Hence he may expect to see any assumption or conjecture he makes about the origin of scientific experimentation challenged, and he is obliged to look for something that can be supported by some kind of recorded evidence.

With these things in mind, let us take a new look at the idea that scientific experimentation began in mechanics. A very simple and fundamental law in mechanics is the law of the lever. Of course, that law may have been discovered by a Baconian accumulation of observations, but then again it may not. The law is a mathematical statement, and it is perfectly possible that nobody ever knew it in that form before Archimedes. Levers may have been used long and widely without their exact law having been known, and that law may have been deduced by someone who never used a lever, or at least never experimented with it. Certainly Archimedes made no use of experimental induction in his proof of the law of the lever, nor do his postulates suggest an experimental origin. I say this not to suggest that there was no scientific experimentation in antiquity, but to show that the most natural conjectures about its role in the origin of mechanics may be quite misleading. In any event, the law relating distances and weights for the lever was both discovered and mathematically formulated so long ago that it would have been simply silly to perform any experiments to verify it as late as the sixteenth century. Not only that, but the law of the lever is so simple and so far-reaching, once it is known, that there was also little point in performing actual experiments to determine the laws of any of the other simple machines. They could be much more effectively discovered and demonstrated mathematically from the law of the lever. And that, I am quite certain, is exactly the attitude that was taken by writers on mechanics in the sixteenth century.

Reliance on mathematical reasoning had by then driven out of physics any feeling of need for experimentation that may once have existed. We shall see presently what the analogous situation was in music. The reason I am quite certain of the situation in mechanics is the following.

Pappus of Alexandria, writing in the fourth century of our era, had

derived mathematically a law for the force required to drive or draw a heavy body up an inclined plane. His law was quite mistaken, because he had employed a false assumption, but the mathematical derivation was very ingenious and complicated, and it had the lovely aura of remote antiquity to recommend it to sixteenth-century mechanicians. From the theorem of Pappus, there easily followed a law for the equilibrium of bodies suspended on different inclined planes, also false, of course. Now, it happened that the correct law of equilibrium on inclined planes had been stated in the thirteenth century by Jordanus Nemorarius, who did not know the work of Pappus. In 1546, Niccolò Tartaglia published the medieval theorem, with some improvements in its purported proof. So all later writers were confronted with two different laws, each of which was accompanied by an attempted mathematical demonstration. If ever there was an occasion for experimental test, this was it; moreover, the deciding experiment would have been very easy to perform. But that is not what happened. In 1570, Girolamo Cardano, who was certainly familiar with the correct medieval theorem and who in all probability also knew the work of Pappus, ignored them both and published a brand-new law for inclined planes, which had to be in error, since the medieval theorem was correct. Seven years later Guido Ubaldo del Monte published the first comprehensive work on mechanics. An astute critic of his predecessors, he certainly knew the correct theorem of Jordanus, and probably also knew the erroneous theorem of Cardano; yet he adopted in his own book the false (but ancient and elegant) theorem of Pappus. These events are hardly explicable if the idea of experimentation in mechanics, to test a preconceived mathematical rule or to discover a new one, is assumed to have been prevalent in the sixteenth century.

I might add that even in the later work of Galileo we find evidence of the use of experiment only to confirm a preconceived mathematical law, and not of its systematic use to discover new laws. That step came after his time, as a logical extension of his work. But we are interested in tracing the probable origin of Galileo's application of experiment (in the scientific sense) to mechanics, which means that it is time to turn to the history of music.

In the sixteenth century, music and mechanics were more obviously closely related sciences than they are today. Tartaglia wrote in his revised preface to the first vernacular translation of Euclid ever published: "We know that all the other sciences, arts and disciplines need mathematics; not only the liberal arts, but all the mechanical arts as well. . . . And it is also certain that these mathematical sciences or disciplines are the nurses and mothers of the musical sciences, since it is with numbers and their properties, ratios, and proportions that we know the octave, or double ratio, to be made up of the ratios

4:3 and 3:2, and it is similarly that we know the former [that is, the interval of the fourth] to be composed of two tones and a [minor] semitone, and the latter [that is, the perfect fifth] to be composed of three tones and a minor semitone. And thus the octave (or double) is composed of five tones and two minor semitones; that is, a comma less than six tones; and likewise we know a tone to be more than 8 commas and less than 9. Also, by virtue of those [mathematical] disciplines, we know it to be impossible to divide the tone, or any other superparticular ratio, into two equal [rational] parts [in geometric proportion], which our Euclid demonstrates in the eighth proposition of Book VIII.”²

But if, about the year 1550, the sciences of music and mechanics were alike in their purely mathematical character, the relations of those two sciences to the practical arts that bore the same names were totally different. Musical theorists were in possession of mathematical rules of harmony that they believed must be very strictly followed in practice. It would be unthinkable for musicians to depart from those rules, lest the very basis of music be destroyed. Musical theorists, moreover, received a good deal of respectful attention from musical practitioners and even had a certain authority over them. This was certainly not the case with theorists in mechanics. Musicians quite frequently studied under musical theorists, but engineers did not study under mechanical theoreticians. And if there were any theorists of mechanics who believed that their rules must be strictly followed in practice, this would have been in the sense that failure to observe the rules would result in wasteful use of materials or in the collapse of buildings, not in the ruin of architecture.

There was a further difference between the arts of music and mechanics in the sixteenth century, a difference that probably has a bearing on the events which (in my opinion) led to the origin of experimental physics. This was the fact that commencing about 1450, and quite markedly after 1550, musical practice underwent fundamental changes, while mechanical practice did not. Those changes in musical practice brought about a real need to expand or alter musical theory, and with it a need for critical examination of its basis and its claims to correctness. No such need was felt by engineers. It was just as easy to test the mathematical rules of music in practice as it would have been to test those of mechanics; the difference in applying or neglecting such a test lay only in the feeling of need. Furthermore, the tests could be carried out in music with a great deal more accuracy, for no sixteenth-century mechanical measurement came even close to the precision of the trained ear of a musician. I believe that

²*Euclide . . . diligentemente rasettato . . . per . . . Nicolò Tartalea* (Venice, 1543), has a shorter and incorrect reference to musical proportions.

that may very well still be true today, if we restrict ourselves to mechanical measurement and do not bring in electronic devices.

Finally, one might go so far as to say that the only possible means of detecting errors in, and ultimately of overthrowing, incorrect musical theories, was the appearance of experimental evidence against them. This was perhaps not equally true of errors in theoretical mechanics, which were in fact corrected by the detection and elimination of certain false assumptions, rather than by the revision of the whole theoretical structure of the science. But a discussion of that would lead us too far afield.

The heart of ancient musical doctrine was an arithmetical theory of proportion credited to the Pythagoreans. The very existence of musical consonances and dissonances was ascribed in it to certain numbers, related as ratios. That is, the cause of consonance itself was thought to be the so-called sonorous numbers. According to the Pythagorean rule, the musical intervals of the octave, fifth, and fourth were governed by the ratios 2:1, 3:2, and 4:3. Music owed the possibility of its existence to these ratios, which are made up of the smallest integers. Here we have a marvellous example of the use and abuse of mathematics and experiment. It is said that Pythagoras discovered these ratios as a result of his having noted, in good Baconian fashion, the different tones given out by hammers of different weight when striking an anvil. The story is certainly apocryphal, but observations of string-lengths would have led to the ratios. Having obtained their simple numbers in this or some other empirical way, ancient theorists put them in the place of any further experimentation, and derived from them an elaborate theory of musical intonation. In time, the arithmetical theory was seen as transcending in authority its original source in pleasant sensation. The consequences were not serious for a very long time, but by the late sixteenth century it was no longer possible to maintain the ancient idea of simple numerical ratios as the cause of harmony. We must pause to see how that came about.

Ancient Greek music, though it bore the name “harmony,” was largely innocent of harmony in its present musical sense. Singing was chiefly homophonic, and when it was accompanied by instruments, they were played in unison with the voice, or, at most, in the octave. The fitting-together that was implied by the word “harmony” was not a fitting-together of different melodies or of parts sung simultaneously, as with us, but a fitting-together of succeeding notes so as to preserve consonance within the recognized *tonoi*, ancestral to the medieval modes, which differed in an essential way from our keys. These facts, coupled with the limited range of the human voice, and the practice of remaining within the selected mode during a given song, made it possible to decide on the intervals that were to divide the octave without imposing limitations on the composition of music.

By the thirteenth century, however, the simultaneous singing of two or three airs had long been in vogue, and thereafter much ingenuity was exercised in the composition and arrangement of motets in such a way that separate voices might interweave without clashing. The use of perfect fourths and fifths exclusively as chief accents, or points of repose, fitted in very nicely with this purpose, and the momentary appearance of imperfect consonances, as thirds and sixths had come to be regarded, did not disturb the ear so long as they were not carried to excess.

In time, however, deliberate violation of the ancient rules began to be attractive; the ears of singers and of listeners came to recognize a sort of general harmonic flow, though the form of composition remained polyphonic. Along with this came the development and multiplication of instruments, used at first to accompany voices, but later coming to be played together with or without voices. Instruments introduced a new element, because unlike voices, most instruments are incapable of producing whatever note the player desires with the exactitude of the human voice. Lutes, recorders, and viols, for example, unlike trombones and the later violins, are limited to definite sets of notes by fretting or by the positions of wind-holes. Organ pipes emit specific notes that cannot be varied in pitch by the organist, just as the harp and most of the keyboard instruments are governed by fixed string-lengths. Inevitably, problems arose over the proper tuning of instruments, particularly when different kinds of instruments began to be played together, which brings us to the sixteenth century.

The fretting of early stringed instruments had led quickly and naturally to tempered scales for them. Octaves simply must be in tune; even the untrained ear cannot tolerate much variation for the octave, which is heard instinctively much the same as unison. Intervals of the fourth and fifth together must also make up the octave, and while it is nearly as easy to tune perfect fourths or fifths as it is perfect octaves, you cannot make several strings agree over a range of two or three octaves by tuning them in perfect fourths or fifths. There is that plaguey "comma" that Tartaglia mentioned, and it has to be divided up in some way. For lutes and viols, this is accomplished by tempering, usually by making each successive fret interval $17/18$ ths the length of the preceding lower fret, a rule advocated by Vincenzo Galilei late in the sixteenth century. Organs, however, seem to have been tuned by the mean-tone system from an early period, certainly by the fifteenth century. Recorders, on the other hand, were generally tuned in C or F, depending on size, in just intonation. But temperament, mean-tone tuning, and just intonation do not make use of precisely the same intervals.

The nature of the difficulty that gave rise to those various intervals is purely mathematical in a sense; the numbers 1, 2, 3 . . . are very use-

ful for counting, but they do not form a continuum. Musical sounds do form a continuum, and hence the whole numbers, even when they are formed into fractions, have a limited application to musical sounds. The set of fractions that will divide a given octave into seven different tones will obviously divide a different octave into seven analogous tones; but if we give them names in the ordinary way, those which bear the same names will not all be in unison or separated by exact octaves. As Tartaglia remarked, and as the ancients knew, the ratio 2:1 for the octave and the ratio 3:2 for the fifth implied the ratio 9:8 for the whole tone between the fifth and the fourth. This in turn implied too small a value for the exact semitone. As Simon Stevin, the Flemish counterpart of Galileo, wrote, "The natural notes are not correctly hit off by such a division. And although the ancients perceived this fact, nevertheless they took this division to be correct and perfect, and preferred to think that the defect was in our singing—[which is] as if one should say the sun may be wrong, but not the clock. They even considered the sweet and lovely sounds of the minor and major third and sixth, which sounded unpleasantly in their misdivided melodic line, to be wrong, the more so because a dislike for inappropriate numbers moved them to do so. But when Ptolemy afterwards wanted to amend this imperfection, he divided the syntonic diatonic in a different way, making a distinction between a major whole tone in the ratio 9:8 and a minor whole tone in the ratio 10:9, a difference that does not exist in nature, for it is obvious that all whole tones are sung as equal."³ By "sung," Stevin meant "sounded" in general, and his final remark is rather an exaggeration.

Regarding the musical string as a continuum, Stevin announced flatly that rational proportions had nothing whatever to do with music, and declared that the proper division of the octave into equal semitones was as the twelfth root of two. It is in this way that we tune pianos today; it enables us to play in all keys without seriously disturbing the ear in any. His conclusion was, however, not experimental but purely mathematical; one might say that it is only accidental that it opened the door to modern harmony. I shall say no more about it here except to mention that Stevin, in a characteristic aside, considered the whole problem to have arisen from an inadequacy of the Greek language: ". . . the Greeks were of the most intelligent that Nature produces, but they lacked a good tool, that is, the Dutch language, without which in the most profound matters one can accomplish as little as a skilled carpenter without good tempered tools can carry on his trade."⁴ For Galileo, the book of Nature was written in mathematics; for Stevin, the book of mathematics was written in Dutch.

³*The Principal Works of Simon Stevin, 5, Music*, ed. A. D. Fokker (Amsterdam, 1966), 431–33.

⁴*Ibid.*, 433.

And so, finally, we come to the sixteenth-century controversy over musical tuning that created modern music, with experimental physics as a by-product. In one corner stood the mathematical theory of antiquity, which took sonorous number as the cause of concord and asserted that number must *govern* string-length, or the placement of wind-holes, or the like. In the other was the human ear, with that curious taste for pleasant sounds that goes along with—or at least once went along with—the composition and performance of music. They were in conflict, as Stevin observed, and with or without the Dutch language, that conflict had to be resolved. The question was, which was to be master, numbers or sounds? The conduct of the debate is instructive, because it is symbolic in many ways of the debates that created the modern world, and of many that are still going on in it. For it was at once a battle of authority versus freedom; of theory versus practice; of purity versus beauty, and so on. Curiously enough, the issue was partly decided by antiquity; that is, by the recovery of an ancient treatise that was largely neglected by theorists before the sixteenth century. Nothing helped a good cause in that century like the discovery that some ancient writer had already thought of it. Copernicus was careful to mention some ancient writers, e.g., some Pythagoreans, who were said to have subscribed to the motion of the earth. It was of great help to musicians in their struggle for emancipation from the syntonic diatonic tuning of Ptolemy to be able to cite the view of Aristoxenus that when all was said and done, the ear of the musician must prevail.

I should like to remark at this point that in a paradoxical way, the struggle to free and broaden music in our own time is a mirror image of the struggle that freed and broadened it in the seventeenth century. Electronic computer music has much in common with the program of the late Renaissance conservatives who fought to preserve the mathematical beauty of sonorous number and superparticular ratios against the mere pleasure of the human ear. The argument of Gioseffo Zarlino, roughly paraphrased for its philosophical content, was that there could be only one correct tuning, established by the mystic properties of numbers, and if that put limitations on instrumental music, then so much the worse for instruments. The voice must govern instruments, and must in turn be governed by divine proportions of numbers. It is amusing that the argument for the divine right of theory, used at one time to impose narrow bounds on music, is now used to support the abandonment of all bounds, and with them all mere sensory criteria for music. Theory before pleasure, once conservative, is now avant-garde.

On purely theoretical grounds, Zarlino advocated the extension of

the Pythagorean ratios up to the number 6, from the ancient 4. This would allow as consonances, in addition to the fourth and fifth, the major sixth as 5:3, the major third as 5:4, and the minor third as 6:5. He was also willing to allow the minor sixth as 8:5, but that finished the list; no other consonances were admissible. The number 6 was, after all, the first perfect number—that is, the lowest number that was equal to the sum of its factors—so the sonorous numbers could safely be extended that far, but no further. Zarlino's *Harmonic Institutions*, published in 1558, did away with all rivals of the syntonic diatonic tuning, such as temperament and the mean-tone system, as theoretically unjustifiable. His treatise on composition thus limited music to polyphony as before, though this was given a somewhat larger range. Method had been perfected. Mathematics was saved by the *senario*, as Zarlino called his six-based ratio system, but harmony was stillborn if his system prevailed.

About five years later, Zarlino's scheme was subjected to criticism, both mathematical and experimental, by G. B. Benedetti. Benedetti has long been recognized among historians of science as the most important by far of the Italian precursors of Galileo in mechanics, but only recently Professor Palisca has called attention to his achievements in the physics of music. These are contained in two letters written to Cipriano da Rore at Venice, probably in 1563, and published in 1585 together with Benedetti's more famous contributions to mathematics and physics. First, he showed by means of examples that a strict adherence by singers to Zarlino's ratios for consonances could result in a considerable change of pitch within a few bars. Benedetti argued from this that singers must in fact make use of some kind of tempered scale in order to preserve even the most elemental musical orthodoxy with regard to pitch of opening and closing notes.

Next, Benedetti proceeded to examine the problem from the standpoint of physics. Instead of relating pitch and consonance directly to numbers, that is to string-lengths and special ratios, he related them to rates of vibration of the source of sound. Consonance, he asserted, was heard when air waves produced by different notes concurred or recurred frequently in agreement; dissonance, when they recurred infrequently or broke in on one another. Thus strings vibrating in the ratio 2:1 would concur on every other vibration; in the ratio 3:2, on every sixth vibration, and so on. The direct cause of consonance was thus related to a physical phenomenon, which in turn bore a relation to certain numbers; but the numbers as such were not regarded by Benedetti as the cause of the phenomenon of consonance. Pursuing this reasoning, he suggested an index of consonance, formed by multiplying together the terms of the ratio. The smaller this product, the greater the consonance. Viewed in this light, consonance

and dissonance were not two separate and contradictory qualities of sounds, but rather they were terms in a continuous series without sharp divisions. A similar view was soon to emerge for heat and cold, speed and slowness, and other ancient physical concepts. Also, by Benedetti's index, it followed that the traditionally abhorred subminor fifth, with the ratio 7:5, was in fact a better consonance than the minor sixth, with the ratio 8:5, which Zarlino himself had allowed even though it lay outside his *senario*. Obviously this was not just a further modification of the old theory, but a fundamental attack on the very basis of that theory.

In framing these ideas, Benedetti had had recourse to experiment; that is, to the deliberate manipulation of physical equipment in order to test a preexisting mathematical theory. The physical equipment consisted of a monochord, on which a movable bridge permitted the division of a single stretched string into any desired ratio, and allowed the measurement of the ratio to a fair degree of accuracy by measurement of the lengths of the two sections, which could then be sounded simultaneously by plucking in order to determine the tonal effect. Benedetti's basis for asserting that string-lengths were proportional to frequencies of vibration is not stated; it may or may not have been experimental. But the appeal to physical results over the authority of a mathematical theory that had been developed over many centuries was in any case a novel event. Experiments similar to Benedetti's, though probably not directly inspired by him because he did not publish them, will presently be shown to have been intimately connected with Galileo's work in mechanics. First, though, I want to comment on the step-by-step nature of the process, for Benedetti's results were in an important sense only a small start toward the modern physical science of musical acoustics.

Benedetti's brief writings on music did not remove mathematics from musical theory; they merely changed its role. For his contemporaries, as for the ancients, numbers were the cause of harmonious sound in a totally different way from that in which Benedetti saw them as related to that cause. Others regarded numbers as ruling the nature of sound; Benedetti formulated a physical theory of sound that was capable of explaining the association of certain numbers or ratios with certain tonal effects. But Benedetti himself was not a composer, and therefore did not concern himself with the possibility or desirability of expanding the range of acceptable tonal effects. He seems not even to have noticed the phenomenon of partial vibrations. These things were soon to come. What Benedetti contributed was in the nature of a new explanation of known effects, rather than a basis for the exploration of new ground. Thus to the question, "Why is the fifth more harmonious than the major third?" the classical answer was of this form:

“Because the ratio 3:2 contains two sonorous numbers, and the ratio 5:4 does not.” Zarlino’s reply might have been: “The ancients were mistaken; both are equally harmonious, for all numbers within the *senario* are sonorous numbers. But since sonorous numbers are the cause of harmony, the slightest departure from the ratios named must be avoided, or dissonance will result.” Benedetti’s reply would have been, “Because harmony proceeds from agreement of vibrations, which occurs every sixth time for the fifth, and only every twentieth time for the major third.” But to the question, “What can we do to widen the scope of harmonious music?” no one gave an answer. To Benedetti’s opponents, the question was unthinkable; and to Benedetti, the first man who would have been able to answer, the question never occurred; he was a scientist and not a musician.

It is virtually certain that Zarlino, who opposed any tempering of the vocal scales, never heard of Benedetti’s demonstration that such tempering was necessary in practice and theoretically justifiable. But Zarlino was opposed in print by a former pupil of his, Vincenzo Galilei, father of Galileo. In 1578 Galilei sent to Zarlino a discourse in which the departures of practicing musicians from the tuning recommended by Zarlino were stated and defended; there was no escape from a tempered scale in the music of the late sixteenth century. Zarlino paid no attention to this attempt of Galilei’s to refute his theory; indeed, he appears to have attempted to suppress the printing of his former pupil’s book, which nevertheless appeared much expanded, in 1581. Galilei, who had long followed Zarlino’s teaching, began to question it only after he had learned from Girolamo Mei, the best informed man in Italy on the music of the ancients, that among the ancients themselves there had been musicians who questioned the absolute authority of mathematical theory over sense. It was Mei who invited Galilei to put to actual test certain doctrines of the old theory. The result was Galilei’s abandonment of Zarlino’s teaching and his publication of the contrary view in favor of tempered scales.

Zarlino, far from accepting these criticisms, counterattacked in 1588 with his final book on music theory, the *Sopplementi musicali*. Though he did not mention Galilei by name, he quoted from his book and identified him as a former pupil. Galilei lost no time in replying; in 1589 he published a little volume which he dedicated, with obvious sarcasm, to Zarlino. The first part of this book is merely polemical and personal, but the balance of it is of the greatest interest with respect to the beginnings of experimental physics. Galilei was, first of all, outspokenly against the acceptance of authority in matters that can be investigated directly. He did not even accept the idea that some musical intervals were natural, or that any were consonant by reason of their being capable of representation by simple proportions. Any sound was

as natural as any other. Whether it pleased the ear was quite another matter, and the way to determine this was to use the ear, not the number system. Zarlino had extolled the human voice as the greatest musical instrument, being the natural one, and concluded that musical instruments, as artificial devices, were bound by the laws of the natural instrument. Galilei replied that instruments had nothing to do with the voice, and made no attempt to imitate it; they were devised for certain purposes, and their excellence could be determined only by the degree to which they successfully carried out those purposes. Mathematics had no power over the senses, which in turn were the final criterion of excellence in colors, tastes, smells, and sounds.

For a tuning system, Galilei advocated an approximate equal temperament as determined by the trained ear. Here we should recall that Stevin, about the same time, went still further; he boldly declared that ratios and proportions had nothing to do with music and that the proper and ideal intervals were those given by the twelfth root of two. Stevin, like Benedetti, failed to publish his discussion of music, but it is likely that he was influential in his own country.

It is in Galilei's final refutation of Zarlino that we find for the first time the specific experimental rejection of sonorous number as the cause of consonance. Referring to the celebrated opinion of Pythagoras that the small-number fractions are always associated with agreeable tones, Galilei stated first that he had long believed this, and then said that he had determined its error by means of experiment. The ratios 2:1, 3:2, 4:3 will give octaves, fifths, and fourths for strings of like material and equal tension but of lengths in these ratios, or for columns of air of similar lengths. But if the lengths are equal and the tensions are varied, then the weights required to produce the tensions are as the squares of these numbers. In a later, unpublished manuscript, he added that the cubes would have to be calculated where volumes determined the sounds. Thus, he said, the ratio 9:4 was just as closely associated with the fifth as the ratio 3:2; and by implication, so was the ratio 27:8—ratios which were simple abominations to the Pythagorean and Ptolemaic numerologists who believed in sonorous number as a cause.

In addition, Galilei experimented with strings of different materials and weights, discovering that unison cannot be consistently obtained between two strings if they differ in any respect whatever. Stringing a lute with strings of steel and gut, he found that if these were brought into agreement as open strings, they would not be in perfect agreement when stopped at the frets.

The removal of number magic from musical theory performed a doubly significant service. First, it deprived number of causal properties; second, and perhaps more important, it called attention to the real significance of number as it referred to a specific dimension, such

as length, surface, and volume. The trouble with the older musical theory was not only that it failed to provide anything fruitful for expanding musical practice, but also that by purporting to give an explanation in mystical numerical terms, it tended to prevent the direct study of the actual application of number to the material instruments of music. A similar double error pervaded most of physics; not only did the erroneous physical principles of Aristotle fail to agree with observation, for example with regard to falling bodies, but their very existence discouraged the search for correct principles.⁵

The experiments of Vincenzo Galilei, like those of Benedetti, were true scientific experiments in the sense in which we have defined that term: the manipulation of physical objects for the purpose of verifying a mathematical rule preconceived as applicable to their behavior, or for the purpose of discovering a rule involved in that behavior. Whether music inspired the first such experiments in an unbroken line to the present remains to be seen. If others preceded them, it would seem that they must have been independent of them, for it is hard to see how Benedetti and Galilei might have attacked musical theory experimentally under the influence of some earlier experimental attack on some other mathematical theory. And to establish the probable continuity of their work with the application of experiment to mechanics, which has had such profound consequences for all science, we may now turn briefly to Galileo himself.

Galileo was enrolled at the University of Pisa in 1581, his father having selected for him a medical career. Two years later he became deeply interested in mathematics under the guidance of a family friend, Ostilio Ricci. The chair of mathematics at the University appears to have been vacant when Galileo was there. His new interest distracted him from the study of medicine, and in 1585 he left the University without a degree. During the next five years he lived mainly in Florence, giving some private instruction in mathematics and commencing on researches that obtained for him the chair of mathematics at Pisa in 1589. Now it was precisely during those years, and particularly in 1588–89, that Vincenzo Galilei is most likely to have carried out many of the experiments in refutation of Pythagorean music theory that he never published, but that survive in manuscripts among Galileo's papers. It seems to me extremely likely that Galileo was himself involved in his father's experiments, some of which he appropriated and published many years later in his *Two New Sciences*. Galileo was an accomplished amateur musician, instructed by his father, and as a young mathematician he could hardly have remained indifferent to

⁵C. Palisca, 130; cf. V. Galilei, *Discorso intorno alle opere di Gioseffo Zarlino* (Venice, 1589; facs. repr. Milan, 1933), 127–28.

what his father was doing in the measurement of tuned strings and the examination of Pythagorean musical numerology. Thus the conception of experimental verification of mathematical laws in physics, which is often illustrated in Galileo's books, may very well have been inspired by his father's work during the years in which he had just left the university and was developing his own mathematical skills. It was precisely at this time that Galileo applied experiment to an ancient theory; he devised a hydrostatic balance and reconstructed the reasoning of Archimedes in the famed detection of the goldsmith's fraud in making the crown of Hiero of Syracuse.

If we consider the nature of Vincenzo Galilei's experiments, we gain also a possible clue to Galileo's early interest in the pendulum, a device with which his future work in physics was to be intimately connected. One of the most interesting of his father's experiments concerned the determination of the numbers associated with particular musical intervals under different conditions of the production of tones. You will recall that the celebrated ratios $3/2$ for the fifth, $4/3$ for the fourth, and $2/1$ for the octave were related to string lengths, given strings of the same material and diameter. It was Vincenzo Galilei who remarked for the first time, in his final diatribe against Zarlino, that the same ratios did not hold at all for the weights that must be used to stretch a given string to the equivalent pitches; here, the inverse squares of the ratios would hold, and to raise the pitch an octave, where half the length would suffice, quadruple the weight was required. The experiment is not particularly difficult, but however it is carried out, an observer can hardly escape the phenomena of the pendulum. This is obvious if one thinks of suspending two strings of equal length and size, and weighting one with four times the weight attached to the other. In order to set up the experiment, let alone to elicit a tone, say by plucking, the strings and their weights are bound to be set in at least a slight swinging motion. If Vincenzo Galilei used instead the apparatus commonly illustrated for the false Pythagorean rule, the pendulum effect would also occur; here, parallel strings are stretched over a flat bed, weights being applied to the ends hanging down from a terminal bridge. The application of different weights to these relatively short vertical strings would set them swinging and would invite attention to the lengths and periods of oscillation.

Galileo's observations of the pendulum were already reflected in his earliest contribution to the analysis of motion and of the law of falling bodies. About 1590 he composed a treatise on motion, which was never published. In this treatise he attempted to analyze the motions of bodies on inclined planes, and this was done in a manner that treated inclined planes as tangents to a circle. By 1602, we find him discussing the motions of bodies along arcs and chords of circles, and

it is quite evident that he and his patron, the Marquis Guido Ubaldo del Monte, were doing some experimental observations of those motions. Galileo's decision not to publish his first treatise on motion seems to be related to his discovery, by experimental test, that the rules he had deduced for speeds on inclined planes were incorrect. By 1609, after various false starts that can be traced in his letters and notes, he had got to a point where he planned to publish a systematic treatise on motion, having ironed out the earlier false assumptions by means of a combination of mathematical reasoning and simple experimental tests. But in 1609 his attention was diverted to the newly invented telescope, and as a result his science of motion was not published until many years later, in 1638.

As one would expect, Galileo's first use of experiment in physics was limited to rough and simple tests to see whether the rules he had worked out mathematically were actually followed. As time went on, he took a greater interest in devising experiments to corroborate or illustrate his science of motion. The extent to which he actually conducted experiments such as those he described late in life is a matter of debate, but there is no question that they served as models to his pupils and his readers.

Such I believe to have been the origin of experimental method in physics in the sense of the unbroken thread leading from the late sixteenth century to the present. The first conscious experiments to test a preexisting mathematical theory were probably the musical experiments of Benedetti and Vincenzo Galilei. They were extended into mechanics by Galileo, whose pupils Castelli and Torricelli carried them on over into hydraulics and phenomena of air pressure; refined by Pascal and Boyle experiment led to the gas law. Boyle's law is said to have been the first scientific law to be experimentally discovered. Yet Vincenzo Galilei's discovery that the weights required for producing tensions corresponding to given pitches are as the inverse squares of lengths must have been empirical. In any event, the manipulation of physical equipment set up to test a mathematical law had come much earlier than Newton, even earlier than Galileo; and it came because of the conflict between numerology and physics in the field of music.

The desire for ever-increasing precision in experiment, a necessary counterpart of the new method in science that sought certainty in the reconciliation of sense experience and mathematics, was strikingly evidenced by Marin Mersenne, the friend of Descartes and the spokesman of Galileo in France. Mersenne is best known for his *Harmonie Universelle* of 1636–37, a monumental work on the theory and practice of music in which the role of the science of mechanics is much emphasized. It was Mersenne who carried out with great care the experiments on falling bodies and on bodies descending along inclined planes

which Galileo mentioned but only roughly described in his books.⁶ The intimate linkage of music to mechanics, mathematics, and experiment that is made in Mersenne's work tends to increase the probability of my general thesis concerning the origin of experimental physics.

I should like to conclude with a remark about a totally different relationship between Renaissance music and modern science, in an area where experiment in the sense of deliberate physical manipulation is not possible; that is, in astronomy. This second musical approach to science found expression in the work of Johannes Kepler, an almost exact contemporary of Galileo. Kepler was deeply motivated by precisely the kind of faith in the domination of the universe by numerical harmonies in astronomy that Galileo's father fought to destroy in music. Little interested in mechanics, Kepler devoted his life to the discovery of that Pythagorean music of the spheres that ought to be produced by the celestial motions. His goal turned out to be as illusory as that of the rule of musical harmony by exclusively small-number fractions. In the course of his work, however, Kepler discovered the mathematical laws that do describe the planetary motions. Those laws destroyed the heavenly spheres themselves, replacing circles with ellipses and uniform motions with varying speeds, much as physical laws destroyed the Pythagorean musical ratios. With varying distances from the sun, the planets were freed from monotony; but they could only grind out, on Kepler's most favorable calculations, a dreary succession of uninteresting scale-passages. Nevertheless, in his unending quest for better results and his devotion to precision of measurement, Kepler established a new physics of the heavens as firmly in mathematics and observation as Galileo founded a new terrestrial physics in the same solid base.

The fountainhead of Renaissance music was thus at least partly responsible for the emergence not of experimental science alone, but of a whole new approach to theoretical science that we now know as mathematical physics. It is well known that Sir Isaac Newton accomplished the synthesis of Kepler's astronomical laws and Galileo's science of motion, setting the pattern of modern physics that unifies terrestrial with cosmic events. In retrospect, it seems not to have mattered greatly which side the scientist took, provided only that he was deeply interested in the musical controversies of the Renaissance. What did matter was that the older mathematical theories of music were capable of exact test by experiment and precise observation. That was a key which, in the hands of Galileo and of Kepler, opened the door to modern science.

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⁶Mersenne, *Harmonie Universelle* (Paris, 1636–37), I, 112; *Les nouvelles pensées de Galilée* (Paris, 1639), 188.