

Mathematics, music, and experiment in late seventeenth-century England

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The Scientific Revolution saw many subjects given new scrutiny, with attempts to use mathematical, mechanical, or experimental modes of explanation to gain understanding of them. One of those subjects was music. Already a tradition of mathematical study of musical intervals stretched back through the middle ages to ancient Greece, where the emphasis had been on ratios of the lengths of strings that formed particular musical intervals. In the seventeenth century there were new mathematical techniques and new kinds of mechanical explanation that could be applied instead (Wardhaugh 2006). There were also new experiments and experimental instruments. In this chapter I will discuss those instruments, in the particular context of late seventeenth-century England.

The Royal Society, founded in 1660, provided a meeting-place for diverse approaches to music, and was a potential source of legitimization for the few studies of music that incorporated experiments. I will discuss below some of the musical experiments performed by the Society: they included the use of a very long string to find the absolute frequency of musical vibrations; the use of a short string to display relationships between the lengths and tensions of strings and their musical pitch; the use of a vibrating glass to display patterns of standing waves; the use of a toothed wheel to demonstrate the effect of particular ratios of frequency; and finally an experimental musical performance using specially

modified musical instruments. None of these was an ‘experiment’ in the more modern sense of producing knowledge or testing a theory; each in fact displayed quantitative knowledge which some or all of those present already possessed.

In this chapter I will consider four instruments close to the boundary between ‘mathematical instruments’ and ‘musical instruments’. As well as throwing that boundary into relief, these instruments illustrate a range of issues that arose when scholars attempted to make sense in the new seventeenth-century context of the mathematical musical tradition they had inherited.

THE MUSICAL COMPASS

Fig. 7.3.1 shows an instrument, of sorts, called the Musical Compass. It is a paper instrument, of the type known as a ‘volvelle’: that is, it consists of two sheets of paper attached together at a single place so as to rotate. The upper sheet is circular, and both are printed. Only one copy of the ‘compass’ now survives, as the final page of a pamphlet printed in London in 1684. The pamphlet is anonymous but internal evidence strongly suggests that its author was Thomas Salmon, an Oxford-trained clergyman who had studied with the mathematician John Wallis and who had a lifelong interest in music theory.

This device dates from a period when Salmon was trying out different strategies for musical tuning. It was surely modelled on similar devices for navigation or astronomy, which were sometimes made of paper but more often of brass. Volvelles, such as those by Fernandez (1626) and Cantone (1668), were also sometimes used in non-mathematical music theory at this period, but I do not know of an English example, and it seems relatively unlikely that Salmon would have seen either of these (from Lisbon and Turin respectively).

Whatever its inspiration, this is a clear example of an ‘instrument’ that embodies a theory, containing a considerable amount of information in a small space. How does it work? The rotating paper disc lists the string lengths for each note of a one-octave scale, for a string of length 1000 units, so that 1000 units represents the lowest note of the octave and 500 the highest. On the page beneath are printed the letter-names of notes, and a second set of string lengths. Rotating the disc allows the mobile string lengths to be variously aligned with the fixed note names. The ‘compass’ was probably intended to be used for building or modifying a musical instrument, to facilitate the placing of frets on a set of equal-length strings tuned to different pitches. To fret a D string, for example, the user would line up the 1000.00 on the volvelle with the D on the page, and read off the string lengths of other notes from the device. It would also be necessary to multiply these lengths by a constant factor depending on the actual length of the string in question: a table printed beside the ‘compass’ illustrates this for a string of length thirteen units.

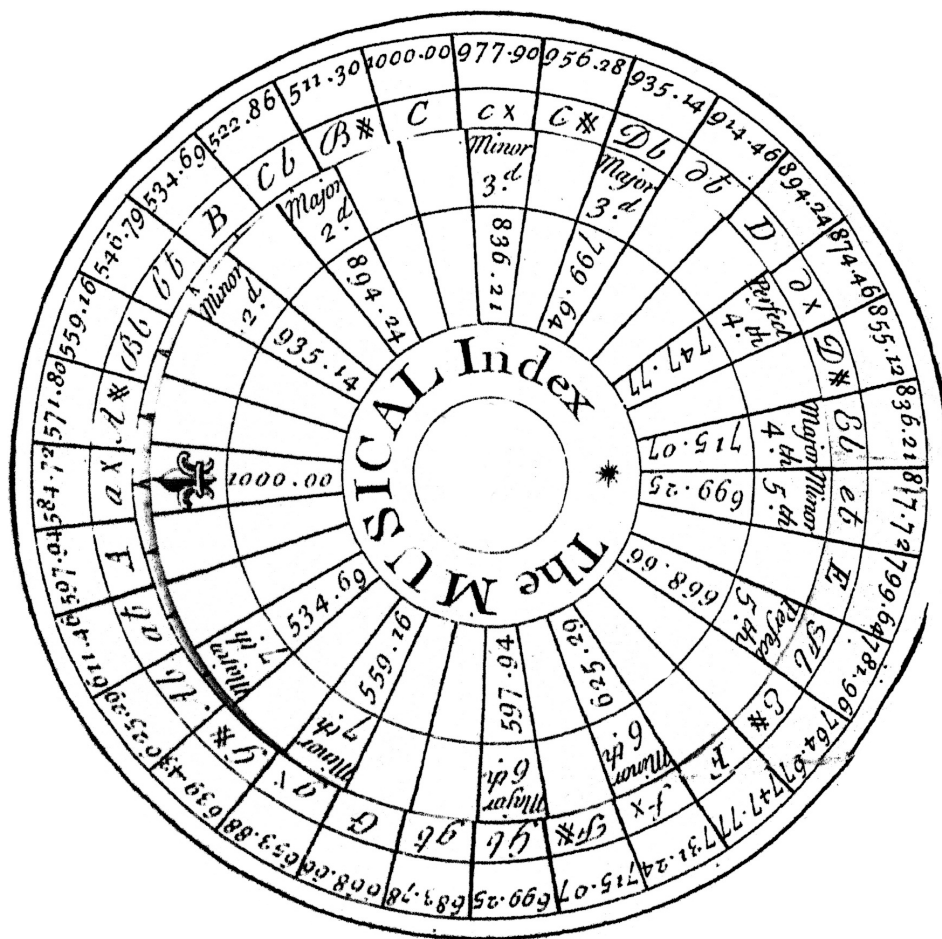


Figure 7.3.1 The 'Musical Compass'. By permission of the British Library

Also marked on the moving page are the names for the relative positions of notes within the key: major second, major third, perfect fourth, and so on; and when the device is rotated these move to correlate with the fixed letter names. In D, for example, the major second will be E and the perfect fourth G. So the 'compass' also illustrates the variable relationship of the major or minor scale with respect to absolute pitches, an emerging feature of music theory more generally at this time.

Inspection of the string lengths on the 'compass' reveals that they divide the octave into thirty-one equal parts. A thirty-one-pitch octave had been proposed in the fourteenth century by Marchetto of Padua, and had made occasional appearances since then: in this scheme, which was also used by Christiaan Huygens, the tone and semitone were taken as $5/31$ and $3/31$ of an octave respectively, good

approximations to their pure values (Huygens 1986; Marchetto 1985; Herlinger 1981, 193; Barbour 2004, 117–121).

The ‘Compass’ apparently caught the attention of the English mathematician Brook Taylor, who was the first to provide a mathematical description of the vibrating string by (in effect) solving its differential equation. Among his papers are found, dismantled, two ‘musical compasses’. They are more elaborate than Salmon’s, each with not one but three moving parts.¹ Although we do not know exactly when or how Salmon’s work came to Taylor’s notice, their similarity to the earlier ‘musical compass’ is too great for them to be independent of it. They probably date from the 1720s, around the time Taylor was working on other aspects of mathematical music, and after his 1714 analysis of the vibrating string.

The first of Taylor’s musical compasses has circular moving parts with, starting from the middle, scales based on the division of the octave into twelve, fifty-three, and nineteen apparently equal parts. These divisions are not arbitrary; they can result from quite natural criteria for scale construction derived from successive approximations for the relationship between the octave and the perfect fifth (see Wardhaugh 2006, 71–80). Taylor’s second musical compass also has three moving parts, each of which divides the octave into fifty-three. Each whole compass is about ten centimetres across. All the markings are by hand, unlike Salmon’s printed compass. There are a total of eighteen spare compass parts, most with no markings at all, probably reflecting an intention to make further compasses with different combinations of octave divisions.

Taylor’s musical compasses are hard to interpret, but it is clear that he was interested in constructing devices consciously based on Salmon’s musical compass and which would demonstrate the variable alignment of different letter names and relative positions within the scale, in at least three different equal divisions of the octave. There is no evidence that Taylor was interested in a correlation with string lengths, although these could have appeared on a lost page to which the moving discs and rings would have been fixed.

Salmon’s compass illustrates both the use of decimals to quantify pitch and the precision with which that quantification could be realized. He gave string lengths to five figures, a degree of precision wholly unrealizable with seventeenth-century instruments and which scattered references in the sources tell us was known to be wholly imperceptible by at least the conscious faculty of hearing. Taylor’s compasses illustrate mathematical music in a purer form: they appear to be not a tool for instrument building but an aid to the conceptualization of pitch and the comparison of different musical scales which were in discussion among theorists.

These are instruments of display: they display first the belief that music was a mathematical art, and second the mathematical description of a specific musical

1. Cambridge, St John’s College Library, Classmark U 19: Brooke Taylor Papers (unfoliated).

tuning or group of tunings. The constraints of a rotating circular device were perhaps uniquely suited to theories of tuning in which the octave was equally divided, in which the depiction of the scale would have a high degree of rotational symmetry.

Other paper instruments for the mathematics of music existed. One was the 'Grand Scale', produced and demonstrated by the musician and theorist John Birchensha in the 1660s. Founded on a very elaborate table of string lengths, this instrument appears to have had a primarily didactic function, as a way to teach Birchensha's music students about his personal theory of musical tuning. Intermediate between Salmon's aids for instrument building and Taylor's apparently private tools for musical theorizing, the 'Grand Scale' would have formed the centrepiece of a whole musical treatise, which was unfortunately never completed (Wardhaugh 2006, 117–118, 273–292; Field and Wardhaugh 2008). An idea of what the 'Scale' might have looked like is given by a table of thirty-four pitches made by the English mathematician John Pell in 1665, possibly in response to a request from Birchensha: each pitch has a ten-digit integer string length, its prime factorization, a reference number, and up to three further factorizations indicating its relationships with other notes.²

LONG STRINGS

While these paper instruments displayed already-established (or assumed) relationships between pitch and number, there were also instruments whose function was more ambivalent. One of the most prominent was the long string.

It had been known since antiquity that when simple ratios were realized in the lengths of vibrating strings, the pitches produced by those strings formed consonant musical intervals. Vincenzo Galilei extended this observation to the thickness and tension of the strings (Galilei 1589; Cohen 1984, 82–83).

His son Galileo Galilei quoted and discussed these results, but said that 'it is quite impossible to count the vibrations of a sounding string, since it makes so many of them' (translated in Galilei 1974, 144). The relationship between the dimensions of the string and its frequency, important for the coincidence theory of consonance, therefore remained a matter of surmise, or of argument from analogy. (The latter approach was pursued in particular by Galileo.) The first person to lengthen the string so that its vibrations could be counted was apparently Marin Mersenne, a French member of the order of Minim friars, who recorded in 1636 that a string making the pitch G '*bat 168 fois l'air... dans le temps d'une seconde minute*', 'strikes the air 168 times... in a second' (Mersenne 1636, 169).

2. London, British Library, Add MS 4388, ff. 14r–37v: John Pell, notes and calculations on music, f. 37v.

This was based on extrapolation from the absolute frequency of a string of $67\frac{1}{2}$ feet (about 20.5 m), which he estimated at two cycles per second when tensed by a mass of half a pound (about 230 g).³ When this string was reduced in length to five inches (12 or 13 cm) it made the G in question (Mersenne 1636, III 169; Dostrovsky 1974–75, 198). The figure of $G = 168$ implies $A \approx 377$, which is too low to be credible. But Mersenne was relatively uninterested in absolute frequencies or in numerical precision: he was keener to encourage the reader to experiment for himself, and to show that frequency could in principle be measured.

The reason that these apparently relatively simple experiments had not been performed before was probably to do with the theories of sound that prevailed. Although it would not have been difficult, at any time from Pythagoras onwards, to slow down a musical string by lengthening or slackening it, the incentive to do so was absent because the string's vibrations were not held to be directly associated with the sound's pitch. Of the classical sources only one, the Euclidean *Sectio canonis*, described frequency as essential to pitch: most ancient writers apparently thought of frequency as only accidentally associated with a sound's pitch, both frequency and pitch being caused by the particular speed of the sound's travel through the air (Barker 1989, 190–208, esp 192 and n 2; also 107 n 39). Even in the late seventeenth century the fairly astute observer, Claude Perrault, argued that the visible vibrations of a string were larger in size and much slower in frequency than the invisible vibrations which constituted sound (Perrault 1680, 40–41, 62, 78–84, 113–7).

Long-string experiments were also performed by Walter Charleton, the English physician and natural philosopher, which he described in his *Physiologia Epicuro–Gassendo–Charletoniana* (1654), a work mainly concerned with the presentation of his controversial atomism. He began by using a slack string to show that vibrations of very low frequency were individually visible but not audible, those somewhat faster became invisible but produced 'a certain dull stridor', and vibrations of sufficient frequency created a pitch (Charleton 1654, 222). Next he established the relationship between frequency ratios, as opposed to string length ratios, and intervals: 'Fasten a long Lute-string at one extreme on a hook nayled to a wall, and suspend a small weight at the other; then strike the string at convenient distance above the weight'. If the initial vibrations were slow enough, it could be confirmed that halving the length of the string doubled their frequency. Turning to a string whose vibrations were audible as pitches, the same exercise of halving the string's length would produce a rise in pitch of an octave, 'and thence you cannot but concede, that the Acuteness of this half of the sonant chord, above that of the whole sonant chord, is caused only by the doubly

3. Metric equivalents can only be approximate, because of the lack of standardization of weights and measures in the seventeenth century.

more frequent Percussions of the Aer, and proportionate strokes of the Sensory'. Similar arguments would associate the other musical intervals with their proper ratios (Charleton 1654, 222–223). He extended his observations to the effect of changing the string's tension, and went on to explain how the coincidence of strokes could produce consonances. But he did not attempt to establish the absolute frequency of particular musical notes, as Mersenne had.

Pierre Gassendi, Charleton's source for the long string experiments as for the *Physiologia* generally, referred to Mersenne's work on the speed of sound (Gassendi 1649, 418–419; see also Gassendi 1658, 38–39, although this text would not have been known to Charleton in 1654). And when the experiments were repeated in 1664 at the Royal Society of London, Charleton was present. There is therefore a plausible line of transmission of the long string experiments from Mersenne their originator, through Gassendi and Charleton to the Royal Society. Since Charleton was also named by the Stationers' Register as the translator of Descartes' *Compendium musicae* into English, he emerges as quite substantially responsible for bringing musical science to England in the 1650s and 60s, an aspect of Charleton's career which has not previously been recognized (van Otegem 1999, 199; *Transcript*, I 402).

The long string experiments of the Royal Society were stimulated by contact with the musician and theorist John Birchensha (see above), who appeared at a meeting later that summer. The person who actually set up the string, built the monochord, and performed the experiments was Robert Hooke: he was the Society's Curator of experiments at the time, and his role in the musical experiments is also implied by a brief reference in a letter to Robert Boyle (Boyle 2001, II 292). Here 'G Sol. Re. Ut.' is the name of a single note: the G which, in modern notation, lies in the top space of the bass clef stave.

[6 July 1664] An experiment was made to measure the velocity of a sounding string, or to determine how quick the vibrations thereof are in a certain space of time. There was taken a brass wire of 136 foot long, of $\frac{1}{32}$ of an inch diameter; and weighing this string, extended by a weight of $3\frac{3}{4}$ lb. + 1lb. 10 ounces, and being made to vibrate in the middle, its vibrations were found to be half seconds. Then being stopped in the middle, and the half of that made to vibrate in the middle, was found twice as swift, or to vibrate quarter seconds: whence the length and vibrations appeared reciprocal. . . . Then farther stopping the wire within one foot of the end, and striking that short part, it was guessed to give a note of G. Sol. Re. Ut; which was to be experimented by a pipe at the next meeting. So that it seemed, that the velocity of the vibration of a string tuned to G. Sol. Re. Ut. is two hundred seventy-two times in a second (Birch 1756–57, I 446).

A similar account appeared in a letter of Moray to Christiaan Huygens two weeks later, adding that a mass of 4 lb 7 oz was initially used and then 'adjusted' so that half-second vibrations would result (Huygens 1888–1950, V 95). But the

figure of 272 cycles per second for G is inconsistent with any contemporary pitch standard. For the long string described one would expect on theoretical grounds a frequency of about 0.82 cycles per second, not 1, and for the short string 112 (or 224, correcting for the fact that the Society took 1 cycle to contain 2 strokes). The latter is somewhat more plausible than 272 cycles per second as a late seveneenth-century G: the most likely source of error seems the estimation of ‘half seconds’ (Charleton took his pulse as a standard for seconds in his experiment), although the Fellows’ estimation of G cannot be relied upon (Morse 1948, 84, 169; Dostrovsky 1974–5; Haynes 2001).⁴

The next week the experiment was repeated, and the results agreed with those of the first trial (Birch 1756–57, I 449). A week later again, a monochord was set up ‘to know the diversity of notes by’:

[20 July] The brass wires were extended upon a long square box of four feet long, one with a weight, the other with a pin, till they became unisons. Then the one being stopt in the middle with a moveable bridge, the two halves on either side were unisons to one another, and one of them an eighth higher than the other, which was not stopt. (Birch 1756–57, I 451)

There followed divisions in the ratios 1:2, producing a fifth with the unstopped string, and 1:4, producing a double octave between the two stopped parts. The next week the effect of weights was investigated:

[27 July] ...one wire being extended by five pounds weight, the other was tuned to an unison with it; and then the same string being stretched with a weight of twenty pounds, it was found just an octave higher: which shews, that the weight is in a duplicate proportion to the sound or vibration (Birch 1756–57, I 456).

The final experiments before Birchensha’s appearance are the most interesting:

[3 August] Two strings being tuned unisons, one of them was stopt at one third, and the lower end of it gave a fifth, and the shorter end was an eighth higher than the longer. Then one of the strings was so stopt, as to make it a note [that is, a whole tone] higher than the whole; and the proportion of the shorter to the whole was found less than 9 to 10. Then the string was stopt a third higher, and the proportion was found as 3 to 4. This was estimated so by the ear. (Birch 1756–57, I 456)

The ear became increasingly important during the series of experiments. On 6 July the ear was used to ‘guess’ the absolute pitch produced by a given string, but distrust of the ear was expressed by the promise to check it using a pipe at

4. Frequency $\nu = (\sqrt{T/\rho\sigma})/2l$ where T = tension, ρ = density, σ = cross-sectional area and l = string length: here l = 136 feet = 41.45 m, T = 19.67 N (= wg where w = 5lb 6 oz. = 5.375 lb = 2.006 kg, and g = 9.80665), ρ = 8600 kg/m³ and σ = 4.948×10^{-7} m² (= πr^2 where r = 1/64 in. = 3.969×10^{-4} m). For the wire described the non-linearity due to stiffness is only a few percent.

the next meeting. The implication on 13 July—'the experiments of determining the velocity of the vibrations of a brass wire, to afford a certain sound, was prosecuted, and agreeing with what was made at the last meeting ...'—was either that that pipe had not been supplied, or that it had confirmed the ear's judgement.

On 20 and 27 July the ear was asked to confirm that certain string-length ratios indeed produced the expected musical intervals. This did not place any great reliance on the ear, particularly since the intervals involved were limited to the octave, fifth, and double octave. It is possible that the pipe referred to earlier was in fact used to check these intervals also, although a question would have arisen about the accuracy of the pipe's realization of musical intervals: since the pipe was not mentioned, it probably did not feature.

Finally, on 3 August, the procedure was reversed: instead of measuring out ratios and checking their musical result, two musical intervals were set up 'by the ear' and the corresponding ratios measured. This use of vibrating strings, to move from musical interval to mathematical ratio, featured in the Pythagorean legends of the discovery of harmonious ratios, and was mentioned as an unrealized possibility by Ptolemy, but this is a very unusual instance of its actual performance at any period.

The ratios that this experiment produced are also of interest, although it is frustrating that the Society did not go on to generate more of them. They suggest, broadly speaking, that the intervals the Fellows of the Society produced 'by ear' were close to those of the 'mean tone' tuning, and relatively distant from the Pythagorean values which most mathematical theorists of the period might have expected the ear to prefer. At the end of this experiment the Fellows again expressed distrust of their own ears, asking Taylor to bring John Birchensha to the next meeting: the outcome showed that what the Society wanted from Birchensha was to make use of his musical ear.

[10 August] Mr Birchensha being accordingly called in, tuned the string by his ear, to find how near the practice of music agreed with the theory of proportions.

This was exactly what they had been trying to find out the previous week: the emphasis was on the superior ability of Birchensha's ear to do it.

The effect was, that he could not by his ear distinguish any difference of sounds (upon the moving of the bridge) above half an inch, especially in the fourths, thirds, and tones.

Assuming they were still using the four-foot monochord of 20 July, moving the bridge by half an inch from any of the intervals named would produce an error of between 20 and 25 cents, that is up to a quarter of a semitone. This is not a huge amount, but it should have been audible to a competent musician.

Whereupon it was resolved, that a virginal should be as exactly tuned, as could be done by the ear, and then the monochord examined by it. (Birch 1756–57, I 457)

So Birchensha's ear was rejected in favour of an instrument, which presumably would be more reliable. Given an accurately tuned pair of virginals all the experimenters' ears would have to do would be to judge when certain notes produced by the monochord were unisons with notes on the musical instrument. Unfortunately the fatal flaw was implied as soon as this strategy was mentioned: the virginal itself must be tuned 'by the ear', and it would therefore provide no more reliability than the ear itself. It is not recorded that anyone pointed this out, and the proposed trial was never performed in any case. Birchensha's own suggestion, to use a bass viol, hinted that the brass-stringed monochord had made it hard 'to distinguish the musical notes': but although the viol was accepted, no (experimental) work was apparently done with it (Birch 1756–57, I 460).

The long string provides an intriguing example of an experimental instrument which was clearly conceptually derived from musical instruments but was kept distinct from them in practice. It was an instrument which could be operated satisfactorily by the Fellows of the Royal Society, who made no special claim to specifically musical skills (although many of them may well have possessed such skills, as Penelope Gouk (1999, 23–65) has documented). It generated an experimental programme in which distrust of the ear was a key problem, mentioned explicitly on more than one occasion: a programme which failed when it was demonstrated that even the ear of a professional musician could not (though perhaps due to the limited abilities of the individual concerned) provide the level of mathematical precision the Fellows desired.

Depictions of a vibrating string, or even the strings themselves, had long been didactic tools for the display of theories about tuning: the depiction of the monochord and the discussion of that depiction were the standard means to display musical theories during the Middle Ages and Renaissance. The conceptual reversal of the string so as to determine, rather than display, the relationship between pitch and string length, is a striking development.

Comparable experiments with other instruments are elusive. Later in 1664 the Royal Society obtained, at Huygens' suggestion, a flat plate of bell metal 'for the trial of the vibrations of hard bodies sounding'. Their intention was to obtain several such plates, of different sizes, to determine how their sound depended on their size. But the specimen produced was 'found useless for the experiments' (perhaps it was cracked), and the project did not proceed (Birch 1756–57, I 460, 475). The plate was found among Hooke's possessions at his death in 1703 (Hunter and Schaffer 1989). Attempts to find absolute frequency using sounding pipes were rare until the work of Joseph Sauveur in Paris in the final decade of the seventeenth century (Sauveur 1984). Huygens attempted such an experiment using organ pipes—he calculated frequency from pipe length and the speed of sound, which he found experimentally—but I know of no English example (Dostrovsky 1974–75, 201).

THE TOOTHED WHEEL

One other device that was used by Hooke and others, specifically to display the coincidence theory of consonance, was the toothed wheel, the teeth of which were made to strike against a fixed metal plate as the wheel turned. It has been linked to Francis North's *Philosophical essay of musick*, which contained a diagram illustrating the coincidence theory by plotting pulses at various frequencies along a horizontal 'time' axis, clearly displaying on the page the various rates of coincidence among different pairs of frequencies (North 1677 (unpaginated plate variously placed in different copies); Kassler 2004). Roger North reported in his biography of his brother Francis that Hooke converted this diagram 'into clockwork':

and made wheels, with small ligulae, in the manner of coggs, which moving each upon its pinn, as the wheel turned, struck upon an edg, one after another equably; the wheel turning slow the pulses were distinguishable, and had no other vertue; but then turning swifter, the distinction ceased, and a plain musicall tone emerged. This for one[;] then, another wheel was contrived to strike 3 to 2 (for instance) and as the distinction begun to fail, and continuation took place, one might hear a consort 5th coming on, and setting in the manifest accord so named (North 1995, 250; Chan, Kassler, and Hine 1999, 73).

Another description emphasized coincidences of strokes:

some wheels should strike together puls for puls, and others in proportions, as $1/2$, $3/2$, etc.... He would begin to turne slow, and so long the pulses were distinct, and he could discern them, as smiths at anvill, without any other idea; but then coming to a mighty swiftness, the consonance called fifth (for instance,) which is $3/2$ which sort of demonstration of the nature of musicall accords is irrefragable [irrefutable].⁵

And another had more specific details of the ratios available:

The ingenious Mr. Hook, made an engin of wheels that made pulses in any musical proportion, as 2, 3, 4, 5, or 6 to 1 and so 3 to 2 and the like.⁶

For North, this device illustrated that a continuous sensation in general resulted from a series of separate events too frequent to be distinguished. It also confirmed the already-known relationship between particular frequency ratios and musical intervals, and lent further plausibility, if any were needed, to the coincidence theory.

5. London, British Library, Add MS 32537 ff. 66–109: Roger North, 'Hasty essay', quote ff. 91v–92r, see Chan, Kassler, and Hine (1999, 49–170); Kassler (2004, 72–3).

6. London, British Library, Add MS 32546, ff 33–90: Roger North, essay 'The world', quote ff. 33r–v, see Chan, Kassler, and Hine (1999, 72).

In fact Hooke was already working on sound wheels in March 1676, and his diary records that he received material from Francis North only the following November; North's book was published the next year (Hooke 1968, 223). Conceivably Hooke had seen the sheets of North's *Essay* as they came from the press, but I think it more likely that Hooke's development of the musical wheel preceded his contact with North's ideas (Chan, Kassler, and Hine 1999, 73; Gouk 1999, 210 and n 62).

In 1681 the device was shown to the Royal Society:

an experiment of making musical and other sounds by the help of teeth of brass wheels, which teeth were made of equal bigness for musical sounds, but of unequal for vocal sounds. (Birch 1756–57, IV 96)

Although incorporating perhaps the earliest attempt at sound synthesis, this demonstration did not include what might now seem the most obvious use of the brass wheel: to establish the absolute frequency of specific notes. To turn the wheel at a known speed would have been quite easy using a gearing mechanism, and the sound could then have been matched with a pitch from a musical instrument. This would certainly have been more accurate than the use of the progressively shortened string for the same purpose.

Another use of the brass wheels could have been to falsify the 'coincidence theory' of consonance (the term is not a seventeenth-century one, though it catches the essence of the idea). This theory proposed that the aural experience of consonance, the 'blending' of certain pairs of pitches, could be accounted for by conceiving pitched sounds as associated with regular series of pulses, the frequency of which was linked to the sound's pitch. If the frequencies of two sounds formed a simple ratio of whole numbers, many pulses in the two series would coincide. This persuasive explanation had first been proposed in letters of 1563 and a publication of 1585 by the Italian music theorist Giovanni Battista Benedetti (Benedetti 1585; Cohen 1984, 75). Though seriously flawed because of its failure to consider what in modern terms would be called the phase of the two sets of pulses, the theory was considered unproblematic by writers in the later seventeenth century, when it became a routine set-piece at the beginning of treatises on mathematical music. It could support a commitment to the founding of music theory on a mechanical basis, but did not necessarily do so (contrast North 1677 and Holder 1694).

Robert Hooke's apparatus could have shown easily that matching of phase was unnecessary for the production of consonance, thereby falsifying the coincidence theory. But although only one of North's three descriptions explicitly mentioned the two sounds' being in phase, we have no positive evidence that he ever considered trying out unphased sounds.

It has been repeatedly suggested that Hooke's sounding wheel demonstrated for the first time the correctness of the identification of musical interval with relative

frequency (Gouk 1999, 208; Dostrovsky 1974–75, 199). It is not clear whether it did any more in this respect than the long strings of Mersenne, Gassendi, Charleton, and the Royal Society already had: indeed, it still left open the possibility that, as Perrault was to suggest in the 1680s, visible vibrations and their frequency were only accidentally related to pitch, the vibrations which actually constitute sound being much smaller and faster (Perrault 1680). A remark made by Hooke to Christopher Wren and William Holder (another mathematical music theorist) in 1676 that ‘the vibrations of a string were not Isocrone [of constant period] but that the vibration of the particals was’ hints that he, too, was considering a similar possibility (Hooke 1968, 211). This is ambiguous, but it certainly suggests that although Hooke considered sound a series of strokes of some kind (at this date, almost certainly not waves) with a definite frequency, he did not necessarily identify this frequency with that of the sounding body’s visible vibrations. This might account for his apparent uninterest in establishing the absolute frequency of visible vibrations in the case of the wheel. From other references in his diary Hooke seems to have been developing his theories of sound throughout 1676, but unfortunately we have very little more information about their content (see Kassler and Oldroyd 1983; Gouk 1980).

Huygens, on the other hand, did use a toothed wheel to measure absolute frequency, in about 1682. He drove a small toothed wheel with teeth from a larger wheel using a driving belt, and calculated the frequency of the sound produced as 547 cycles per second. He judged the pitch to be the same as the D on his harpsichord (Huygens 1888–1950, XIX, 375–376; Dostrovsky 1974–75, 199–201). This is a plausible figure: it implies $A = 410$, which is well within the range of pitches in use at the time (pitches around $A = 400$ were normal for instruments used at home) (Haynes 2001). It is unlikely that this appearance of a toothed-wheel experiment shortly after Hooke’s was a coincidence: Huygens had been in touch with the Royal Society via Moray, and it is possible that lost letters in that correspondence transmitted the idea for the experiment.

Somewhat later Brook Taylor, whose ‘musical compasses’ I discussed earlier, performed similar toothed-wheel experiments (as with ‘G. Sol. Re. Ut’ above, ‘A la mi re’ is the name of a single note, in this case the A that falls within the treble clef stave in modern notation):

6 March 1712/13

I applied a quill to the crown wheel of my chamber clock, and making it fast to one of the [pillars] of the clock, I let the works run down for 7 minutes and by my Harpsichord I found the quill to sound A la mi re in alt., and by the works of the clock the quill struck 766 teeth per second.⁷

7. Cambridge, St John’s College Library, classmark U 19: Brooke Taylor papers (unfoliated); see Cannon and Dostrovsky (1981, 19).

He also matched this pitch, two octaves lower, with that of a wire whose frequency he was able to calculate from its length, tension, and density. The fact that the prediction matched the frequency observed in the wheel he seems to have taken as confirmation of his analysis of the vibrating string, which he had presented to the Royal Society the previous year (Taylor 1713).

The experiment implies that Taylor's harpsichord had $A = 383$, a very low value, but one which Taylor's careful experiment and the agreement with his theoretical prediction (which is correct for the length, tension, and density he gives) oblige us to take seriously.⁸ Of course, matching the wheel's pitch on a harpsichord involved rounding to the nearest semitone and therefore introduced an error of up to half a semitone. Three days later Taylor repeated the experiment with a different frequency and pitch, and found that this result was consistent with the first.⁹

The toothed-wheel apparatus differs from the long string in the crucial respect that it is not derived from a musical instrument, and its operation is consequently still further from the exercise of distinctively musical skills (and the sound produced by a cog striking a brass 'edge', though pitched, is unlikely to have been a recognizably 'musical' one). The only role for the ear here was in recognizing when two sounds were at the same pitch.

The diversity of the uses of this apparatus by different experimenters is therefore striking: for Hooke this was another instrument for theory display, but for Huygens and Taylor it was a more genuinely experimental apparatus, capable of producing data which in Taylor's case constituted a meaningful check on a quantitative theory. That theory, though, dealt with the vibration of strings in general: it was not a theory of music. Next I turn to a rare and valiant attempt at experimental verification of a specifically musical theory.

MODIFIED VIOLS

Thomas Salmon, whom we met above as the maker of the first musical compass, pursued an interest in mathematical music theory throughout his life. In 1672–73 he was involved in a controversy about the reform of musical notation: his short book on the subject, *An essay to the advancement of musick*, was violently attacked in print by Matthew Locke, organist to the Queen's chapel (Salmon 1672a; Locke 1672). Two later volumes in the dispute were more concerned with trading insults than resolving strictly musical questions (Salmon 1672b; Locke 1673). Although

8. He gives length = 12.3 inches, weight = 12 ounces and density = 1 grain per foot, which implies frequency = 382.4 Hertz (correcting for Taylor's terminology which introduces an extra factor of two in frequencies).

9. Cambridge, St John's College Library, classmark U 19: Brooke Taylor papers (unfoliated); see Cannon and Dostrovsky (1981, 19).

a storm in a teacup, the dispute gave both Salmon and Locke the opportunity to explain at length their respective ideas about the nature of musical knowledge and the proper way(s) to acquire it.

Strikingly, Salmon incorporated public ‘experiments’ into the study of music. ‘I don’t know’, he wrote, ‘what to request more advantageous for [the scheme’s] acceptance, than an Experiential tryal’ (Salmon 1672a, ‘epistle’, iii). After Locke’s challenge he appealed to experiment again as a way to establish the superiority of his scheme: ‘which I have experienc’d before several judicious persons’ (Salmon 1672a, 61). This recalls the Royal Society’s semi-public trials and its appeals to a group of reliable observers. Locke, by contrast, was not interested in public experiments: when he did refer to experience it was to individual, private experience.

The point of Salmon’s trials was to persuade hearers of the excellence of the scheme and thereby result in modifications to musical practice: ‘surely, ‘twere well worth the while for Instruments to be contriv’d accordingly... for the excellency of Musick’ (Salmon 1672b, 20). A review in the *Philosophical Transactions* endorsed the scheme, and again ‘recommended it to publique practise’ (Anon 1671–72, 3095). Perhaps the emphasis on experiment had in part been a strategy to arouse the Society’s interest.

Musical hearing had an important role in Salmon’s project: it was supposed to be able to recognize harmonic excellence reliably. Salmon believed that ‘God hath created a peculiar faculty of hearing, to receive harmonious sounds, clearly different from that by which we perceive ordinary noises’, although he declined to speculate on whether this faculty was physiological or resided in the soul (Salmon 1672a, 2). A difference between the ordinary and musical hearings was their sensitivity to small deviations from pure tuning:

The Keys of an *Harpsichord* [sic] are now tuned in a common diluted proportion... though a vulgar ear may not be able to judg the difference... yet there will be a dissatisfaction, though it be not evident in what particular to complain. (Salmon 1672b, 20)

Salmon felt that, unlike his opponents, he was concerned with ‘the true nature of music’, and often returned to the desire to unite theory and practice (Salmon 1672b ‘epistle’, [i], 12–13). But he always assumed that mathematical theory led practice: music ‘consists in proportions’, it ‘is a combination of sounds as they are proportioned in numbers’, therefore it is ‘part of the *Mathematicks*’ (Salmon 1672a, 2; Locke 1673, 7). For Salmon (as for his Greek sources, most notably Ptolemy), aural recognition of a musical interval and intellectual computation of a ratio were two different ways of apprehending the same thing. Eventually, Salmon explicitly outlined a whole method for musical science; he would:

- (1) establish the mathematical givens, supposedly by doing experiments with strings, but in reality by study of other theorists;

- (2) use these to produce a mathematical division of the octave into smaller musical intervals;
- (3) relate that division to practical knowledge by establishing how these intervals correspond to steps of the scale;
- (4) invite the reader to check these intervals by measuring them out on a string and comparing them with his expectations;
- (5) organize persuasive public performances using his scheme; and finally,
- (6) the scheme would be widely adopted in practice.

(Salmon 1672b, 248)

Although experiment was prominent here, it was not meant to test but to persuade. The checking of the harmonic intervals by the reader was meant to allow the reader to persuade himself the scheme was correct, not to allow Salmon to correct it if it were found wrong: in fact, Salmon did not even say that he had performed the checks himself. And the point of the public demonstration was to persuade others, not to check whether the mathematics had worked (Salmon 1672b, 6). (One might say that the reader and later the audience were invited to become witnesses to the correctness of Salmon's mathematics and its applicability to music: a strategy which arguably owed something to writings of Boyle and others which Salmon may have read.)

By contrast, here is Matthew Locke's statement about how to acquire musical knowledge.

All Creatures that have Ears are apprehensive of Sounds, but not of distinguishing them; those, whose Ears Nature hath prepared for Practical Music, by dividing and sub-dividing a String (for Example) come to experience their difference and distances; and from thence, by comparing them, to Tones, which (the Ear having distinguished into Consonants and Dissonants) they Arithmetically divide to the greatest quantity Practicable...and thence...advance to That we call *Composition*, the Mother of all Vocal and Instrumental Musick.

...

More of the Mathematicks than this, Sir, (excepting what belongs to the Mechanical Part thereof for the Making Instruments) signifies nothing to us.... You have...quitted the Field of *Practical Musick*, and run for shelter to the *Nature and Causes of Sounds*, which properly belongs to Philosophy. (Locke 1673, 15–16)

Salmon's career after the dispute with Locke was less explosive (partly, perhaps, because Locke died in 1677). I discussed above the pamphlet probably by him, 'The Musickall Compass', about musical tuning and notation, which appeared in 1684. In 1688 he wrote a book on tuning. He corresponded with John Wallis, and there are manuscripts of treatises by Salmon very similar to his 1688 tuning theory

in both London and Cambridge: the Cambridge manuscript is among Newton's papers (Salmon 1688; Salmon 1705).¹⁰ The 1688 book was in fact endorsed by both Newton and Wallis.

All this is by way of prelude to, and to shed some light on, my fourth example of a mathematical musical instrument, a different type of modified viol. In each of these tuning texts Salmon described in detail a method for realizing his preferred tuning on stringed instrument. By contrast with a normal instrument, on which straight 'frets' govern the position of the fingers across all of the string, Salmon proposed a scheme which placed corresponding notes at slightly different positions on each string (Salmon 1688, foldout). At the end of his book were scale diagrams for the system, which could in principle be transferred to the relevant part of an instrument, the fingerboard, quite easily: Fig. 7.3.2 shows an example. An advertisement in the *London Gazette* in 1689 offered to modify lutes according to Salmon's scheme (see Tilmouth 1961, 8).

Salmon continued to promote his new fretting at the Royal Society until shortly before his death, and in 1704 a musical 'experiment' was finally performed at a meeting of the Society under his direction:

Two Viols were Mathematically set out, with a particular Fret for each String, that every Stop might be in a perfect exactness: Upon these, a Sonata was perform'd by those two most eminent Violists, Mr *Frederick* and Mr *Christian Stefkens*, Servants to his Majesty; whereby it appear'd, that the Theory was certain, since all the Stops were owned by them to be perfect. And that they might be prov'd agreeable to what the best Ear and the best Hand performs in Modern practice, the famous *Italian*, Signior *Gasparini*, plaid another Sonata upon the Violin in Consort with them, wherein the most compleat Harmony was heard.¹¹ (Salmon 1705, 2069)

The ear did not produce knowledge here; it was simply asked to assent to knowledge already possessed. If the musical compass was an instrument for the visual display of musical theories, the modified viols enabled their aural 'display'. It is not clear what would have happened if the assembled company had failed to judge the harmonies they heard to be excellent: perhaps Salmon would have blamed the performers or listeners rather than revised his mathematical theories; but it is hard to imagine he would have allowed the trial to go ahead if he were not confident of its result.

One of the oddities of Salmon's tuning scheme was that it contained at least five different sizes of semitone, whose detailed arrangement depended on the key in which the performer wished to play. Salmon suggested, in fact, that several

10. Oxford, Bodleian Library, MS Eng Lett C 130 ff. 27–8: letters, Thomas Salmon to John Wallis, 31 December 1685; Wallis to Salmon, 7 January 1685/6; Cambridge, University Library, Add. MS 3970, ff. 1–11: Thomas Salmon, 'Division of a monochord' (copy); London, British Library, Add MS 4919, ff. 1–11: [Thomas Salmon?], 'The practicall theory of musick ...' (copy: diagrams only are in Salmon's hand).

11. See also Royal Society Journal Book X, 97, 102.

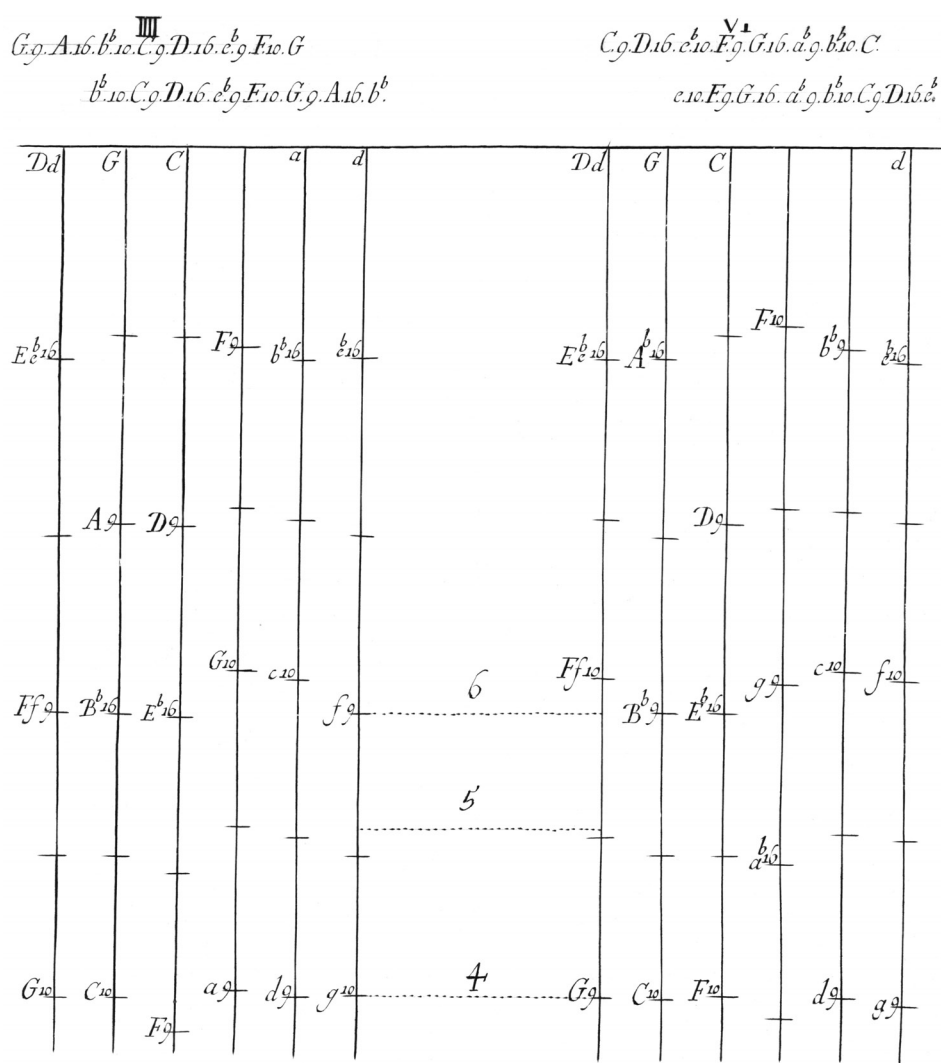


Figure 7.3.2 Where to place the frets on a viol (Salmon 1688, foldout). By permission of the British Library

different fingerboards be constructed, and a different one attached to the instrument each time a piece of music in a new key was to be played. This raises obvious questions about the feasibility of using his scheme to play real music, where key changes within continuous sections of music were hardly unusual by this period. But the problems of producing satisfactory intonation from a viol in a variety of keys were not trivial, and unusual fretting schemes are by no means unknown in viol technique: slanted, curved, even split frets (Crum and Jackson 1989, 159–163). What is revealing about Salmon's scheme is its avoidance of such practical and workable compromises, in favour of embodying in a musical instrument

a mathematical perfection which may not even have been fully translated into sounds: the details of how the fingers are placed on the strings in relation to the frets can also be used to modify the pitch, and Penelope Gouk (1982, 230–231) has suggested that in Salmon's trial the performers may have used such a technique to cancel the effect of his unusual placement of frets. Although he achieved contact of a kind with the world of professional musical performance, he seems to have remained ignorant of the interplay between playing and listening by which good tuning could be achieved even with theoretically imperfect instruments, and indeed of the level of ad hoc negotiation which may be involved in such a thing as 'good tuning' for both players and listeners.

Apart from this failure of communication—a complex and recurring issue in the mathematical music theory of the period—perhaps the most remarkable feature of Salmon's theory of music is that both he and ultimately the Royal Society apparently thought that a meeting of the Society was an appropriate place to demonstrate it. Salmon's modified viols differ from the other experimental instruments which I have discussed by being very closely related to ordinary musical instruments: even after their mathematical modifications they remained real musical instruments which would be played, for preference, by professional musical performers. They were not pieces of experimental apparatus to be operated by the Curator of experiments. But the very closeness of Salmon's scheme to real musical performance, and the fact that musical skill was required to judge it, caused problems of interpretation which perhaps account for the fact that Salmon's musical experiment was entirely unique in the early history of the Royal Society.

CONCLUSION

These four very different instruments used for the mathematical study of music do not by any means exhaust the contents, or the idiosyncrasies, of 'harmonics' in late seventeenth-century England. 'Harmonics' raised distinctive and probably unique problems about the relationship of mathematical knowledge to the senses and to instruments and, during this particular period, writers about the mathematics of music also laboured to achieve a workable relationship between musical practice and the new experimental practices of early modern science. One conclusion which these four instruments illustrate is that those writers failed to achieve a consensus.

The relationship of music to the mathematizing impulse which arose in the seventeenth century was complicated by the fact that music was already considered a branch of mathematics. Each of these instruments reflected a belief that music was inherently mathematical, a programme of study which aimed to update mathematical basis of music, and uncertainty about how that was to be achieved.

These instruments illustrate how, in the context of the new theories of knowledge of the seventeenth century, mathematical music was exceptional, particularly in its relationships with mathematics, with the ear, and with musical practice. Questions about the reliability of the ear and its ability to produce knowledge were complex in the seventeenth century. Some musical scientists assigned the ear as nearly as possible no role at all, working mathematically in terms of pure reason and simply asserting that their results corresponded to real music: the musical compass illustrates such an approach, though not in its most extreme form. Others permitted the ear the role of recognizing mathematical excellence when it was rendered audible: as in Salmon's demonstration with modified viols. Others tried to find a role for the ear in producing knowledge, as fleetingly occurred during the long string experiments at the Royal Society, but which proved problematic.

If the object of study in musical science was seriously considered to be musical sound, it was necessary for it to rely on musical practitioners, who alone could produce musical sound. Any experiments or experimental instruments used were liable also to rely on the skills of musicians and musical instrument makers. Musical knowledge was difficult to embody except in musical instruments and performances.

But this was not a situation in which the kind of objectivity sought in other sciences could be attained or even envisaged. A scientific instrument is typically supposed to make an experiment more 'objective', since 'an instrument cannot be prejudiced or passionate', but it is far from obvious that this could ever be said of a musical instrument or its operator (Hankins and Silverman 1995, 229). This is why Salmon's performance with modified viols was so exceptional, with its close dependence on real musical instruments and musical skills both of production and of judgement.

The relationship of mathematical theory to musical practice was problematic also because that theory remained prescriptive rather than descriptive. The few attempts to create mathematical descriptions of contemporary practice were hampered by the lack of suitable instruments and by lack of confidence in the ear's ability to turn sounds into numbers. The perception by practitioners that mathematical theory was therefore irrelevant to them may well have contributed to theorists' failure to build a relationship with practitioners which might have solved these problems.

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