CHAPTER 10

Simon Stevin's Music Theory Revisited: A Dialogue

H. Floris Cohen and Julia Kursell

This chapter is a dialogue in four parts. In the first, Cohen briefly restates the interpretation of Stevin's treatise on music theory at which he arrived in the 1980s – an interpretation that has remained unchallenged to date.[1] In the second part, Kursell nevertheless challenges this interpretation in various respects. In the third, Cohen gratefully accepts some of her objections, and uses others for further clarification. The outcome for the reader, as we suggest in the fourth part, is a fresh view – in part, at least – of what Stevin really intended in his treatise on music theory.

Part I, by Cohen

A private tutoring course in mathematical science, such as the one that Simon Stevin arranged for Prince Maurits, could hardly fail to address the science of music. Music theory was part and parcel of the *quadrivium*, on a par with geometry, arithmetic and astronomy. To be a polymath and *not* to address music theory in addition to these other three parts of the *quadrivium* – now *that* would require a special explanation. Numerous contemporaneous but also slightly later writers, such as Kepler, Galileo, Beeckman, Descartes and Mersenne, wrote about music theory and contributed now and then novel insights to it. So did Stevin – if only to include the subject in the curriculum to which he subjected his supreme commander. As he announced in 1605, in the first volume of *Wisconstighe ghedachtenissen* (*Hypomnemata mathematica*), he intended the second volume to contain a treatise on music theory – he even provided a brief abstract. Yet no such treatise appeared in that second volume three years later, though not for lack of writing one. He even wrote two versions, and he sent the second to an organist of his acquaintance. Alas, Abraham Verheyen responded with a critique as cogent as it was devastating. We may suppose, but cannot be certain, that Verheyen's stern objections were what caused Stevin to withhold his treatise 'Vande spiegheling der singconst' ['On the Theory of Music'] from the public. We do know that the manuscript was read in subsequent decades by a few knowledgeable commentators, notably Isaac Beeckman and Christiaan Huygens. For a few more, such as René Descartes, Stevin's brief abstract in the *Wisconstighe ghedachtenissen* sufficed to provoke the same verdict that would be reached by Beeckman and Huygens, namely, that Stevin had it profoundly wrong. And then the manuscript disappeared, only to resurface in the 1880s with the preparation of Christiaan Huygens' *Oeuvres Complètes*. The treatise was speedily published in a very competent edition.[2] Much had changed in the meantime in the practice and theory of music, however, so that Stevin's core message now appeared in a radically different light. In a naïvely progressivist reading, Stevin came to be hailed as a man who, with remarkable foresight, had not only produced a refreshingly sober-minded plea for *equal temperament,* but had also calculated this now-standard way of tuning modern pianos with far greater accuracy than anyone had ever managed to do so before. It is this caricature that, notably in volume 5 of Stevin's *Principal Works*, has made its way to the English-speaking world. E.J. Dijksterhuis, in his masterly book on Stevin of 1943, did not allow himself to be fooled, yet the posthumous, poorly

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[1] H.F. Cohen, *Quantifying Music. The Science of Music at the First Stage of the Scientific Revolution, 1580-1650* (Dordrecht: Reidel, 1984), 45-74; idem, 'Simon Stevin's Equal Division of the Octave'. In: *Annals of Science* 44, September 1987, 471-488. Every statement made in the account that follows goes back to sources and literature listed and extensively referenced in *Quantifying Music*.

[2] Simon Stevin, 'Vande spiegeling der singconst' (this 1884 edition by Bierens de Haan is vastly superior to the 1966 one by Fokker in vol. 5 of Stevin's *Principal Works*, on the grounds set forth in notes 46 and 48 of *Quantifying Music*).

abbreviated and poorly translated version of his book was not a good vehicle for setting things straight in this regard.

The point is – and Dijksterhuis knew this well – that 'Vande spiegheling der singconst' is not about equal temperament, even though a twentieth-century reader could recognize equal temperament in the figures that Stevin calculated. But the conceptual framework in which he made his calculations is quite different. The treatise is not even about temperament at all; rather, throughout his treatise Stevin insisted that he was computing *the one and only natural tuning system*. And it was precisely this outrageous thesis that none of the contemporaries who read it or heard about it could take seriously.

At the heart of Stevin's treatise is his assertion, supported by many ingenious arguments, that the ancient Greeks had been utterly wrong (and so had everybody else since) in maintaining that consonant musical intervals are given by ratios of the first integral numbers. So, what concept of a consonant interval was Stevin arguing against?

The expression 'consonance' stands for two different musical notes that sound well together. The idea is this: take a string, sound it, then take half the string and sound it again – what you hear is two notes that seem to sound the same, only the pitch of the latter note is higher than that of the former. This particular – very particular – musical interval is known as the octave. In other words: the consonant interval known as the octave is given by string lengths in a ratio of 1:2. In similar fashion, you can produce other intervals that count as consonant, though with not quite so striking an effect of seeming coalescence. The next-best-sounding consonance is produced by using strings whose lengths are as 2:3 – the resulting interval is called the fifth. In ancient Greek musical theorizing, the list of consonant intervals ends with the fourth (3:4). In European music-making from the High Middle Ages onward, and in European music theorizing from the mid-sixteenth century onward, further intervals came to be acknowledged as consonant – the major and minor thirds and sixths, with ratios of 3:5, 4:5, 5:6 and 5:8.

The big problem, in musical practice as well as theory, was (and in a sense still is) that it is mathematically impossible in any tuning system to have all consonant intervals pure at the same time. By way of exception, at least in European tuning systems, the octave, so as to maintain the effect of coalescence, is always tuned to the ratio of 1:2. But *within* the compass of one octave, it is simply impossible to have all of your fifths, fourths, thirds and sixths pure at the same time.[3] How, then, to 'divide the octave'?

About half a century prior to Stevin's treatise, two highly consequential developments had turned this into a burning issue. One was the rise of polyphony; that is, a way of making music that arose in the twelfth century, in which several relatively independent melodies are sung simultaneously by different voices (tenor, alto, etc.). As a consequence, the distinction between consonance and dissonance became a good deal more acute than in the monophonic musical practice of the ancients. The other, much more recent development was the 'emancipation' of the musical instrument, which was being employed more and more often, not only to accompany vocal music, but even for purely instrumental pieces. And whereas both singers and players of many an instrument can adapt to some extent how they produce their notes to what sounds best, players of instruments with fixed pitches, such as the lute and the organ and, a little later, other keyboard instruments such as the virginal and the harpsichord, lack such flexibility. That is why the problem of how to divide the octave became most urgent for lutenists and keyboard players. From the mid-sixteenth century onward, dozens and dozens of divisions of the octave were being proposed and fiercely debated, until the pianoforte came into fashion in the early nineteenth century and the debate was settled in favor of equal temperament.

What concessions to tonal purity are you making, then, when you adopt equal temperament? The concessions are

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[3] The shortest answer to the question of why this is so is that we happen to hear logarithmically, and that powers of figures not divisible by each other can never be equal. For a slightly more expansive, yet still concise explanation of this and several other issues concerning the arithmetic of music and the division of the octave, see Prins and Cohen, 'Knowing Heaven and Earth Through Music', 18, and several more passages.

huge – indeed, the only consonant interval that remains pure in equal temperament is the octave. All of the others – all fifths, fourths, major and minor thirds and sixths – are deliberately mistuned to a smaller or larger but always noticeable extent. 'Noticeable? What do you mean? Is it not the case that few people nowadays are even aware of the out-of-tune-ness of these intervals?' Yes, that is so, and the reason why is that we have become thoroughly accustomed to equal temperament, if only because hardly any alternatives exist anymore. But you need only hear a musical composition performed in some other temperament where the inevitable impurities are distributed differently, in a manner that spares them more, albeit in fewer keys, to realize at once how crude a solution equal temperament has been and how much tonal purity has been lost in the process.

Only, Stevin would not have agreed. In his quite deliberately dissenting view, apart from the octave, the consonant intervals are not given by the ratios of the first few integral numbers 1 to 6. Instead, so he maintains over the length and breadth of his treatise, they are given by what he calls the 'natural', that is, the equal division of the octave. Thus, the consonant interval that we call the fifth is not given by 2:3, but by the twelfth root of one half raised to the power of seven: $\sqrt[12]{\tfrac{1}{2}^7}$. And similarly so for all other musical intervals in the octave.

Stevin argued his case in two principal ways. He sought to prove it, and he went on to support his proof by means of three distinct arguments.

Let us consider his proofs first. These are, inevitably, based on a *petitio principii*, that is, they can be seen in effect to presuppose what they are meant to be proving. However, the proofs are so ingenious that when I investigated the matter some forty years ago, I had real trouble pinpointing precisely where in the deep layers of his proof he had (surely unwittingly) smuggled in the theorem that he had set out to prove. Among these layers is his appeal to one specific, empirical trial, of a kind that has led Dijksterhuis to suspect that Stevin was unable to hear a tone difference of almost half a semitone. The implication (to which I return) is that Stevin would have been tone-deaf to an astonishingly high degree.

Meanwhile, Stevin's typical approach and style of reasoning are captured in a delightful manner in his three supporting arguments. I have called one of these the argument from rationality, another the argument from proportionality, and the final one the argument from nature. To set forth the argument from nature would send us deeply into matters a little too arcane perhaps for the musically uninitiated, so I confine myself here to the first and the second argument.

The argument from rationality states that it is a bad mistake (an ancient Greek mistake, to be sure) to call integral numbers 'rational' and thereby to privilege them over all other, allegedly irrational numbers. To link the consonant intervals up with the first integral numbers is therefore nothing but superstitious number-juggling. If you compare, for instance, the number two with the square root of eight, there is simply no good reason to attribute their incommensurability solely to the square root of eight; hence, it makes no sense to call the square root of eight rather than the number two irrational or irregular or any such locution. Stevin had already made the general point in his early work *L'Arithmétique*; in 'Vande spieghelingh der singconst' he refers to this work and then bluntly states that he is not going 'an donuytsprekelicke onredelicheyt ende ongheschicktheyt van sulcken misverstant hier te leeren duytsprekelicheyt redelicheyt gheschicktheijt ende natuerlicke constighe volmaecktheyt deser ghetalen' ('from the inexplicable irrationality and absurdity of such a misconception to teach here the explicability, rationality, commodity and natural ingenious perfection of these numbers').

In the present case, the English translation provides the non-Dutch reader with a satisfactory rendition of Stevin's nicely concise, late sixteenth-century Dutch. But this is not true of Stevin's next argument, the argument from proportionality, which only a person knowledgeable in Dutch can truly grasp. That is so because it brings out Stevin the linguistic purist. His argument from proportionality is a perfect exemplar of his conviction that Dutch lends itself more readily to scientific objectives than any other language.[4] In Dutch more than in any other language, Greek definitely included and Latin even more so, you can with great facility make composite words, which is an important feat, 'overmidts der dinghen namen daer duer oock hare corte bepalinghen sijn' ('since the names of things are also thereby their brief definitions'). 'Evenwijdig' for 'parallel' already indicates by way of the expression itself that the width ('wijdte') is equal ('even'). 'Stelkunde' for algebra already indicates that this is the discipline that provides expert knowledge ('kunde') of what we 'stellen' ('suppose'). And so on, to the very point where we see Stevin arguing that very long ago, in the 'Wijsentijt' ('Age of the Sages') when all the knowledge was already available that the present age is busily seeking to recover, the one and only language spoken was Dutch.

The question that Stevin raises in 'Vande spiegheling der singconst' is how the Greeks, who were clever enough to hit upon the true, equal division of the octave, came up instead with their childish division according to these ratios of the first few integral numbers, such as 2:3 or 3:4 or 4:5. Why did it require a Dutchman to find out that all consonant intervals flow from the equal division of the octave into twelve semitones? Language is the reason why. The Greeks should have derived the division of the octave from the only proper kind of proportionality, *geometric* proportionality, which unlike so-called arithmetic or harmonic proportionality leads at once to the equal division. But they failed to recognize this. Neither the Greek nor the Latin words express the relationship between the ratios of the terms in question. Greek has λογος for 'ratio', and ἀναλογια for their relationship. Latin, along with all the languages that stem from it, is even worse: it provides no connection at all between *ratio* and *proportio*. But luckily there is one language that can express the connection quite clearly and concisely. This language is Dutch, and the proper word for it has been coined by Stevin himself – 'evenredenheyt' (or, in modern Dutch, 'evenredigheid'). This, Stevin says, is a word 'of infinite power', for it is the 'bepaling sijns gronds' ('the definition of its own essence'). Indeed, 'te seggen dat 6, 4, 3 en dierghelycke [,] Singconstighe everedenheyt maecken [,] daer oneindelycke ydelheden uijt volghen ende besloten worden; Men antwoort duer beweghing van tvoornoemde gheluijt, hier van syn gheen even Rhedens, daerom oock gheen Everedenheyt'. Or, translated into a language that is quite unfit to make this particular point, since all terms in question derive straightforwardly from the very Latin Stevin deemed unfit for scientific discourse: 'from the statement that 6, 4, 3 and the like constitute a harmonic proportion ['everedenheijt'], an infinite series of vanities follows and is made to follow. One responds by pronouncing the aforesaid word: here are no even Ratios, therefore, there is no *Evenredenheyt* ['proportionality']'.[5]

One interesting aspect of this wonderful consideration is that it provides Stevin with the perfect justification for his deliberate deviation from every single account of utterly basic musical theory ever produced from Pythagoras onward, up to and including his own time; only a Dutchman is in a position to grasp what true geometric proportionality is like and that, therefore, the division of the octave into twelve equal semitones is the only properly proportional division!

Let us now return to Stevin's proofs. Leaving out all of the technicalities, as I am doing throughout this brief account to the largest possible extent, the outcome of the empirical trial that forms part of one of his proofs is entirely dependent on how the two keyboards that he needs for it have been tuned. If in equal temperament, the trial confirms the property in need of proof by definition; if in any other temperament, the trial cannot fail to disprove it. The interval in question is the sum of three whole tones, the augmented fourth ('tritone') F-B. If you pile two such tritones upon one another, the outcome is, in equal temperament, a pure octave, but, if tuned such that its constituent intervals are in what

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[4] The remainder of this paragraph and the entire paragraph that follows is identical to a summary description in my 'From *Philosophia Naturalis* to Science'.
[5] On *evenredenheyt,* see also the chapter by Van Dyck in this volume.

anyone other than Stevin calls pure intonation, then the outcome is an interval that deviates from a pure octave to quite a large extent. The size of the difference is close to half a semitone; connoisseurs know it as the 'lesser diesis', which is characterized by the ratio 125:128, and comprises 41.1 cents. This 'cent' unit is a measure for interval size that was invented in the late nineteenth century. Certainly with respect to the octave an ordinary person's hearing is capable of distinguishing between pure and impure from a difference of about 2 cents onward. Therefore I find it very hard to share Dijksterhuis' belief that Stevin would not have been able to hear the difference, which after all is no less than twenty times as large as is needed for any ordinary person. And therefore I assume that if Stevin carried out the trial the way he summarily described it, the two keyboards that he used had (no doubt inadvertently) been tuned in equal temperament, thus yielding a significant portion of the *petitio principii* that (as I noted earlier) Stevin can be seen to have committed in this proof. Indeed, there are reasons for supposing that Stevin failed to see how having the two keyboards in equal temperament was bound to invalidate his own proof, the main reason being that at various places in his treatise he makes it clear that he does not understand what temperament as such is about in the first place.

So what is going on here? Whether Stevin was tone-deaf or not must remain anyone's guess and is not of much inherent interest anyway, but that he lacked even the tiniest smattering of musical sensitivity is a readily observable fact that jumps from every single page of 'Vande spiegheling der singconst'. Except for the sheer interest evoked by the mathematical problems that quadrivial *musica* carries in its wake, music apparently had no personal meaning for him. Stevin's argument can be enjoyed for its freshness; for his sheer boldness in declaring every single predecessor just plain wrong; for the clear-cut mathematical principles he appealed to; for the attractiveness and (up to a point) persuasiveness of many a supporting argument. But what was missing (and this can so easily happen when one is wholly out of touch with one's subject, particularly if it is about human conduct and accomplishment) was at least a residual sense of the absurd. This precious sense can only be acquired from the kind of personal familiarity with your subject that keeps you in check when you arrive at the borderline that separates refreshing unorthodoxy from sheer irreparable madness. The relentlessly mathematical train of thought that pervades Stevin's treatise is, in the present case, a thoroughly perverted train of thought – perverted by a lack of sensitivity for what, at root, is the human subject it addresses. In the final analysis, I regard 'Vande spiegheling der singconst' as a mirror for all scholars: beware that borderline behind which madness awaits, and make sure that you retain a genuine feeling for what you are doing and why you are doing it.

Part II, by Kursell

Dear Floris,

Thank you for sharing these stimulating thoughts and observations with me, even though I am not a scholar of the early modern period. Following the gist of your last remark, and not being an expert on equal footing with you in the relationship between science and music in that period, I will try to reflect on your remarks from the perspective of a scholar of the history of music. I do so, because I have faith in your support for those who aim to break through the confines of established discourse, from which I and others have benefited so much.

I will touch upon three questions. The first concerns what you call the proof of the augmented fourth. Can there be a mathematical proof that includes applicability to a physical referent and – to bring this question into my own domain – can this referent be music, i.e., a culturally defined phenomenon? I would like to propose that language, and more specifically Stevin's peculiar approach to language, shows why the discrepancy between mathematical proof and the practice of tuning did not hinder him from suggesting such an application.

My second question elaborates on the question of the referent. Even if the music that is generally associated with the problem of tuning – and that is to say, polyphonic music for instruments with keyboards or frets – does not lend itself to an application of Stevin's ideas, this does not necessarily mean that this is true of music in general. There might be music for which Stevin's invoking of a natural quality is appropriate. I will suggest that his elaborations on a natural quality of music do hold for singing, and thus for the part of musical culture in the Low Countries that was most widespread, most accessible and also most highly developed. Your expertise will be all the more helpful here, since you so convincingly argue that Stevin's treatise is not about temperament. The question is, then, what Stevin means by natural *tuning*, rather than just natural *singing*.

Referring to your 1987 article to which you refer in note 1, my third question challenges the claim that Stevin instead participates in the debate about *consonance*. As you argued in that paper, one looks in vain in Stevin's treatise for the arguments that are needed to discuss musical consonance. Instead, you proposed that he sought to find a natural *scale*. While this is in line with the assumption that singing – and more specifically, monophonic singing – formed the basis of his argument, this does not fully explain the remarks that Stevin did make about consonance and dissonance. For this, we will have to consider his elaboration on the interval of the fourth. This also allows us to tackle the question you raise about musical sensitivity. The appendix on the fourth points to a specific type of listening that in today's terms would be called spectral as opposed to tonal listening. While I fully agree that we need not diagnose historical figures with any kind of pathological deficiency, I hope that this distinction will shed some light on the potential that Stevin's treatise has for musical discourse.

1.

To begin with, your argument that Stevin only proves what is already contained in his notion of an equidistant scale is convincing. The problem, then, arises from his intermingling of two modes of arguing. On the one hand, he resorts to the demonstration that, when adding two intervals that have been defined according to his own calculus, one obtains another interval that is defined within that same framework. This is a plain syllogism. For a mathematical proof, nothing else is needed. But one should add that if Stevin's claim is true by definition, so is its contestation. When we resort to intervals as they are defined by music theory rather than by Stevin's calculation, we already know from the outset that adding up two of the intervals you indicate will never result in an octave. Two augmented fourths piled upon each other result in a new note: an augmented fourth from C is an F sharp, an augmented fourth from an F sharp results in a B sharp, not in a C. That is the very problem of temperament. Compromises had to be found due to the multitude of note names, and the multitude of intervals that fine ears wish to hear in the music – but which would have resulted in a proliferation of additional keys. Such instruments, as you well know, have actually been built.[6] With a keyboard, even with two keyboards, one can thus neither prove nor disprove Stevin's claim, if one requires the mathematical and phenomenal values to be congruent. When carrying out a division of the monochord that is correct in the terms of Stevin's argument, one will obtain a sound that will not make sense in the terms of such a strict logic of tonality. The problem of tuning can only be mitigated, not solved.

If Stevin understood this, he was not up to comparing a D double flat to a C. Rather, he must have been aware that only his system of steps in the octave produced two notes with the same name when adding three whole tone steps to three whole steps. For this, hearing is not necessary – although this is in fact what Stevin's contemporaries got to hear when they played the most popular instrument of the time, the lute. The alleged predominance of the keyboard instrument is to a certain extent an artifact of the predominance of the piano up to Dijksterhuis' and Fokker's time.

Thus, something else must be at stake in Stevin's argument, and this seems to be an argument about language. As

[6] See for instance Prins and Cohen, 'Knowing Heaven and Earth Through Music', 28-29.

you explain so beautifully, his argument is that Dutch is not better at denoting objects, but at relating terms. In addition, Dutch is not more natural than Greek or Latin because it is a vernacular and thus comes to mind more easily, but because it allows the speaker to relate what needs to be related. This, in turn, enables Stevin to create similar formalisms in language to those he seeks to find in mathematics. For the moment, we can thus say that even if we do not yet have a proper referent for either the language or the mathematical formalism, that is, a referent that is in need of such description, we can at least state that the linguistic and mathematical formalisms are in congruence.

2.

Stevin wrote not about music, but about *singconst*, literally translated in the twentieth-century edition as 'the art of singing'. Although his use of this term and its derivatives is congruent to 'music,' the fact remains that the term refers to singing in the first instance; and so does the first definition, which concerns *de nateurlicke sanck*. The Dutch word 'sanck' (present-day 'zang'; singing) highlights the most widespread musical practice in his time, and it also demonstrates once more a play with derivative morphological forms. To sing need not always be an art, and what is natural about it is perhaps to be found in the activity rather than in the art of singing. Singing, which is mentioned in many instances in the text, undermines the idea that this is a treatise about tuning, since the voice cannot be tuned. Yet, Stevin's treatise does not adhere to the tradition of theorizing vocal music, either, which continued up until the sixteenth century. Natural singing can just as well be understood to refer to some sing-along tune that comes to mind without much thinking and without much musical education. It is not ennobled through application to the heavenly bodies, nor does Stevin discuss any rules of composition. Instead, he picks out the definition of intervals to begin his treatise, and applies this to a mundane understanding of singing.

For many centuries, the very fact that music used intervals and that these could be described as proportions of small integers justified the inclusion of music, or rather *harmonia*, in the *quadrivium* at all. The use of musical instruments began to challenge this basic definition of the musical interval in terms of harmonic proportions only much later, after polyphony was established. Authors such as Gioseffo Zarlino and Vincenzo Galilei became quite specific about what a musician had to do to achieve an equidistant tuning for the frets of a lute. Galilei contested Zarlino's beautiful considerations on how to construct the proper geometrical division of the string, pointing to the fact that such a division does not result in pitches that sound right, because it disregards the reality of the frets, which add tension to the strings. And Zarlino replied that even with the frets being fixed at some particular distance, good musicians would always follow their ear and tweak the pitch so that it would be more harmonious. This is much more concrete than Stevin's account. He neither cared about the sound when he described the division of the *sanglijn*, nor does his elaboration on how to distribute intervals over a keyboard indicate that any actual practice of tuning is involved. I would be curious to hear from you whether Stevin's term 'stellen' for tuning was the regular historical word for the action of tuning. You mention that it was used for algebra, because it resembles the word meaning 'to claim' in contemporary Dutch. Stevin's musical advisor Verheijen duly used it to explain to Stevin why he was wrong.

I would thus claim that Stevin is referring to mundane singing, even in those moments when he mentions more concrete musical and especially instrumental practice. The keyboard, for one, seems to function primarily as a neutral display for the distribution of pitches in his argument. Stevin did not venture to participate in the discussion about how to refer to those notes that are not part of the diatonic system, which he calls 'slit keys', taking the design of contemporary organ keyboards as his model for this designation. When Stevin does use note names beyond the diatonic scale, that is, the notes with names from A to H, he simply continues the alphabet beyond the established letters. Any other choice would have forced him to refer to the note names with the sharps and flats I mentioned earlier, i.e., those dictated by 'pure' intervals.[7]

Concerning the monochord, it is remarkable that Stevin speaks of a *sanglijn*, although he could also have spoken of a 'single string' to refer to this instrument. By his time, the monochord had become a standard item in musical treatises for the visualization of pitches. Nevertheless, it had retained the function from previous treatises of providing support for referring to the elusive quality of the sound. The monochord served to find the correct distances of pitches at a time when there was no other way of referring to them. Stevin's *sanglijn*, while still providing a visualization, refers to a practice that exists without such support.

The most interesting case of a discussion of practice in Stevin's treatise is, in my view, the chapter 'waer in doirsaeck verclaert wort vande onvolmaecktheyt dieder int stellen der orghels ende clavesimbels ghebuert' (179); that is, the chapter that explains the imperfection that occurs when organs and harpsichords are tuned. Stevin not only argued for approximation in music, but he grounded this argument in the inability of the human subject to detect the difference. Singers know intuitively whether they are singing correctly. But as research in experimental psychology, carried out 300 years after Stevin, has shown, what is intuited by a singer as correct intonation can deviate from the (mathematically) correct value by an interval of a third. This is about seven times as much as the two notes that Stevin was unable to distinguish, according to Dijksterhuis. Similarly, a listener can judge a singer to be performing correctly when such deviations are measured. The phenomenon occurs in singers and listeners with decent musical ability and, to a lesser extent, even in individuals with absolute pitch (Abraham 1921).[8] Felt and measured musicality in singing can thus indeed produce deviations that astonished Stevin's modern readers.

3.

Consonance is the topic that made his twentieth-century editors wonder whether Stevin ever noticed the phenomena of distortion that can be observed in intervals that deviate from harmonious proportions. You argue that Stevin's treatise must be seen against the background of the discussion of consonance, rather than of tuning systems. Indeed, musical intervals are defined in terms of consonance and dissonance. A consonance would thus be an interval that is appropriate for music and a dissonance an interval that must be dismissed. This was introduced in Greek music theory and it continued until the point of transition that you highlight. Up to the sixteenth century, music was held to be the art of proportions, consonance in music being one example of this. During the Middle Ages, consonance also became a phenomenon in its own right: the relationship between two simultaneously sounding voices. The treatises accordingly discuss which of these tone relations are appropriate for music and how they should be handled in singing and later in composition. Zarlino famously adjusts this discussion to actual practice, acknowledging the prevalence of triads in the music of his time. And Johannes Lippius, in his *Synopsis musicae novae* of 1612, introduced the term triad, admitting that for the description of such an entity it would be more appropriate to consider the note a 'monad' that enters into a relation of three, rather than to continue speaking of proportions, which require three relations of two elements each for describing just one triad.[9] At this point the concepts of tone and note merge, marking the end of the age of proportions.

The physical explanation of consonance emerged in parallel, as you have explained on many occasions. The distinction between consonance and dissonance in musical practice was by then well established. Physicists could refer to it as a phenomenon: what is it that makes listeners feel that some sounds merge into harmony whereas others do not?

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[7] One may add to this that in Stevin's time, letters beyond *a* to *h* were also used for lute notation, where they indicate finger positions that have no relation to the note names we associate today with the diatonic scale. On this, see the classic description in W. Apel, *The Notation of Polyphonic Music*.

[8] Cf. Abraham, 'Tonometrische Untersuchungen'; on his research into musical intonation, cf. J. Kursell, 'Experimental Cylinders'.

[9] Cf. Lippius, *Synopsis Musicae Novae*; on Lippius cf. Rivera, *German Music Theory in the Early 17th Century*.

Stevin's mathematical model of an equidistant tonal system does not add anything to this. In monophonic singing, the problem is irrelevant. Yet, Stevin does comment on how voices and instruments actually sound together in an appendix on the interval of the fourth.

There, Stevin comments upon a rule in counterpoint that is a corollary of the music that is organized in triads. If the triad is the basis of harmony and in that same capacity a moment of rest, then the role of the fourth changes. Music theory understands a triad to be a combination of tones that piles two thirds upon each other. In modern terms, one could also say that the triad combines a choice of pitches that fit into a Fourier Series, with the lowest pitch being identical to the fundamental of the series. A concept of harmonic rest can thus be said to occur when the lowest pitch is identical to the fundamental. This is not the case when the interval between the lowest pitch and the next pitch is a fourth. Then the third partial, or in other words the fifth to the fundamental, is the lowest pitch. Harmonic theory associates this with tension and thus dissonance.

Even though these relations between rest and tension were only made explicit in late nineteenth-century music theory, they nevertheless help us here to explain what Stevin encountered when discussing the problem of the fourth. In short: music theory declared the fourth to be a dissonance when occurring between the lowest voice and the voice above it. I quote from the English translation: 'The fourth is considered a discord by present-day composers, so that in singing with three or more voices it must not be heard against the lowest part; nay, below two voices it is not suffered at all' (483). What Stevin seems to ignore is that this dissonance is a regular feature in a musical setting. That an interval is dissonant does not mean that one may not use it, but that a combination of tones, which is defined as consonant, must then follow. Stevin nevertheless endeavors to prove that the fourth in this position should not be considered a dissonance at all.

At this point, his argument can be called a proof in the sense you invoke. Stevin himself refers to it as 'daetlich bewijs' (175), which is translated as 'empirical proof'. He now does refer to actual sound, and he even introduces controlled variation of the same set-up to make his point. Starting from the observation that singing in different registers may appear to the singers themselves as unison, he argues that it may not always be clear which voice is the lowest and thus with which the fourth must be seen as discordant. Stevin now invites his reader to think of three voices, two of which sing in an octave, whereas the third joins in a fourth to the lower. Since the first two may consider their sound to be in unison, not noticing the difference by an octave, it becomes unclear to which of them the third is in the relation of a fourth. They may not even hear it as such, but as a fifth. He concedes that this may be the effect of combining human voices and suggests repeating the experiment with a flute as a third element. He writes: 'om deser dynghen oirsake wat breeder te verclaren soo is te weten dat als sulcke twee gheluyden tsamen een eerste ofte dobbel eerste maken dalderscherpste ghehoiren en connen niet sekerlick oirdeelen welck van tween het is' (176). In other words, even the most educated musical ear may be unable to distinguish pitches that join in an octave, so that, consequentially, it becomes impossible to say what the interval between the third tone and this joint sound is.

Stevin's hearing, again, is accurate; although music theorists will argue that the function of the fourth, and that is to say, its embedding in a sequence of sounds, is what matters here. That the fourth will be heard as dissonant means that it will be followed by a consonance. Even if one does not detect any unpleasant effect from this discordant sound, it will reveal itself through the next sound as having been in need of resolution into a consonance. Yet, this is not what Stevin's argument is about. He only states in his peculiar terminology that the octave, for which he uses the term 'double first', is not detected as two sounds. The octave is a duplication of a tone that results from half its string length or, in more modern terms, twice its frequency. There are situations in which such a duplication occurs throughout a musically correct musical setting. Organ stops came to make use of such duplication on a systematic basis, and listeners failed to note any contradictions to the rules of musical setting.[10]

This brings me to your last remark – that Stevin lacked any smattering of musical sensitivity, and that his deliberations about music are thus refreshing for having introduced a surprising topic into his mathematical thinking, but have no value for music at all. I think it is fair to say that he did observe music carefully. He does not prioritize the music of the church or court, but I do not see that this deserves our reproach, since it is in line with the values of Dutch society as it emerged in the wake of Stevin's time. He even seems to have had a fine ear. However, he made a curious mistake: he did not obey the rules of musical discourse. The comparison to Descartes may be fruitful in this respect. Descartes, in my opinion, seems to explain that discourse to himself with a sharp mind and little hint of knowing what this discourse is all about. Stevin, by contrast, trusts his hearing, but he underestimates the power of a discourse to shape our hearing. At times when there is no other reference to sound than language and practice, hearing depends on learning what one is supposed to hear, to an even greater extent than this is the case now that exposure to recorded sound has become the rule. Stevin rigorously denies the discourse, introducing instead his own Dutch terms. By the same token, he introduces a new system of reference. This system includes four components: the musical practice of his surroundings, his brilliant ability to create mathematical formalisms, an awareness of these formalisms being never exactly in agreement with empirical reality, and, last but not least, his sharp ear.

Perhaps what kept Stevin from publishing his treatise was something other than being ashamed of his ignorance; he may have understood that the discourse on music in his own time forbade it.

With my warmest wishes and all my gratitude,
Julia

PS: After having re-read Stevin, I noticed that his 'proof' contains another rather clever element. Comparing the effect of piling two tritones upon each other, he adds dissonance to dissonance, each of which is composed of three steps that cannot count as consonances, either. Which ratio should each tone have? 9:8? 10:9? There is no way to define these steps by their relative 'purity' or pure sound, as they are dissonances in any case. The best way to sing several steps with the same name correctly seems to be to sing each of them in the same way. Perhaps he was interested not so much in a division of the octave in the first place, but in a system that could explain uniform steps that made sense when singing a scale – and still did not contradict the octave. In this regard, he was perhaps merely trying to bring some formalism to this obvious empirical problem, as he did in other fields. He just picked the wrong problem to solve, a problem no one noticed but him, with his self-taught ear. Music theorists, by contrast, always took the purity of consonances as the gauge of their tonal systems.

Part III, by Cohen

Dear Julia,

Your very welcome comments have given me occasion to clarify some points and to change my views on certain others.

Clarification first. My point about Stevin's *petitio principii* is that (as may happen to every mathematician intending to prove some theorem or other) at some hidden layer in each of his proofs he has inadvertently used what looks like just

[10] One might add, though, that Stevin's example is not very telling, as it does not concern triads, in which case the fourth between the two lowest voices only becomes relevant.

a link in the chain of the proof as if it were a given; in other words, that the theorem that he aims to prove turns up in the proof itself. You are surely right that music, even such math-infused aspects of music as the theory of consonance and the division of the octave, does not lend itself to mathematical proof of any kind. But that means only that you and I agree that Stevin was trying to do something that cannot be done. His *petitio principii* is just one symptom of the ineluctable fact that anyone who wanted to prove a certain theory of consonance would be bound to run into trouble of this sort. With respect to this, I will now add something that I left out of my original summing-up: Stevin is not even consistent in his efforts to prove his theory of the consonance of the notes of the equally divided octave, for in one of his definitions of 'natuerlicke sangh' ('natural singing' or 'natural music'), he states that these notes are consonant *by nature*. But if a certain quality is such and such by nature, how could it make sense to seek mathematical *proof* for it?

What I like so much about Stevin's efforts to come to grips with a domain where he believes every predecessor to have been utterly mistaken (they didn't speak Dutch so they couldn't really help it) is the deadly rigor with which he keeps pursuing his reasoning. Take his argument about the so-called 'wolf fifth'. This interval arises from the incompatibility of pure octaves with pure fifths (an incompatibility of the kind that I briefly addressed in the first section). To rise from C by seven *octaves* gets you, obviously, to the c seven octaves higher. But to rise from C by twelve *fifths* gets you, *not* to that same c, but to b sharp.[11] Consequently, the interval formed between that b sharp and the f underneath is not a pure fifth. It howls, which is why it has earned the name of 'wolf fifth'. It may actually be situated at any point, wherever you prefer to leave the circle of fifths open, but so much is certain that if you make music both with pure *octaves* (no exceptions here) and with pure *fifths*, you inevitably get into trouble somewhere. Obviously, it is easier for singers to circumvent the problem in practice than, in particular, for keyboard players with their pre-fixed key notes. The main solution that began to be explored in Stevin's time was some form of temperament. It was not until the decades around 1800, which saw the invention of the *pianoforte*, that this was taken one great step further, thus giving rise to our familiar *equal* temperament – a theoretically possible solution that an eighteenth-century organist by the name of Quirijn van Blankenburg found utterly ludicrous, arguing that if you distribute the inevitable falseness equally you find that, having got rid of the wolf, you have now bred a whole nest of wolf cubs.[12] Some hundred and twenty years earlier, Stevin had not been sensitive to arguments of this kind at all, in that for him no wolf fifth existed in the first place. And indeed, in his own perverse system of *naturally* equal semitones, no wolf fifth can occur by definition. With that deadly consistency of his, he rather attributes all notions of the existence of a wolf fifth to a fundamental misunderstanding, namely, to the deeply ingrained notion that the pure consonances are *not* given by the equal division of the octave but by ratios of the first few integral numbers. So, yes, in Stevin's view there is no wolf fifth, and if you believe you hear one it is due to nothing but your own inability to infer from the gap between theoretical preconception and empirical reality that your preconception is just wrong. 'Your honor: case closed'.

Case closed so much the more because, luckily, there was a witness for the prosecution with impeccable credentials – the Nijmegen organist Abraham Verheyen. His letter to Stevin, faithfully transcribed and printed by Bierens de Haan together with Stevin's two versions of his treatise (the original manuscripts rest in one and the same manuscript box at the Royal Library in The Hague) states in its own terms what is so perversely (and therefore so informatively) wrong with Stevin's argument.

Up to this point, then, I stick to my picture of Stevin as a relentlessly consistent mathematician who, in seeking to

[11] See, and hear, https://www.youtube.com/watch?annotation_id=annotation_2604736211&feature=iv&src_vid=Au2QFQ2ppyg&v=1DUZsQ2by2s

[12] Van Blankenburg, *Elementa musica*, 114-5: '... maar helaas: Elke wolf heeft op zyn vertrek drie jongen gekregen die hij verstrooit heeft over 't gansche clavier; zo dat al wat men speelt, is een concert van jonge wolven'.

drive home a point that by its very nature does not lend itself to mathematical treatment, inevitably, and for most intriguing and revealing reasons, overshoots his mark. So far, so good. But also, no farther than that. You have convinced me that there is a broader way of viewing what Stevin was doing. Quite rightly you remind me that the title 'Vande spiegheling der singconst' may be translated not only as 'On musical theory', but also, and in the first place, as 'On the theory of singing'. Even more importantly, you point out that to consider Stevin's two versions of his treatise from that angle reveals some aspects to which I have been, well, perhaps not deaf but definitely blind.

First of all, I shall consider what you wrote about the problem of the fourth. I pointed out in my earlier work on Stevin that, in the short appendix that he dedicates to the subject, he is explaining all over again that everybody else has been mistaken. But you have made me realize that the grounds for his disagreement really betray a remarkable sensitivity to the kind of simple folk-music he was exposed to on an everyday basis. And of course your point about singers' intuition about their own singing possibly being amiss by no less than a full third is well taken, too. All in all, then, Stevin's concern with music was not quite so exclusively confined to an account of consonance and its application on keyboard instruments, but extended to lute practice and to singing as well.

More importantly still, you keenly observe that there is a quite remarkable novelty in Stevin's treatise – a novelty defined not by a presence, but by an absence; namely, the absence of any reference at all to the music of the spheres. That is definitely not a minor issue; to the contrary, as you rightly point out, just about every learned treatise on music prior to Stevin's took its point of departure in the threefold distinction first made by Boethius in or around the year 510: *musica coelestis* (the harmony, represented by the consonances, between the celestial bodies), *musica humana* (how that harmony is reflected in our own bodies and souls), and *musica mundana* (music actually sung, played, and listened to, all of which thus serves as a representation, an imperfect one of course, of that heavenly harmony). It all goes back, ultimately, to Pythagoras, and even though Stevin is quite explicit in his rejection of a Pythagoras-like approach to numbers, I had not fully realized the consequences until now. Almost contemporaneously with Stevin's career, fierce battles were being fought out in Italy regarding the true nature of consonance, and it was precisely the literally cosmic implications of this that lent the controversy its fierceness. One century later, listening to music had turned forever from a cosmic or at least cosmically-tinged event into what was by and large a purely aesthetic experience. This 'untuning of the sky' was a most incisive process,[13] and I can now see, as I failed to do earlier, that Stevin is, at the very least, a remarkably early representative of that secular shift in sensibility.

I return, finally, to the greater or lesser acuteness of Stevin's hearing and, more generally and more interestingly so, to his musical sensitivity as such. True, I have never gone along with Dijksterhuis' supposition that Stevin must have been tone-deaf to the point of being unable to hear an interval difference of over 40 cents. Still, my mental image of Stevin used to be that of a person to whom music meant little else than one more terrain (belonging as it did to the *quadrivium*) that appeared ready-made for the application of his very considerable mathematical skills. By means of subtle yet effective observations, such as 'Stevin's hearing, again, is accurate', you have managed to shatter that image, and to replace it in my mind with a partially new one. It runs thus. Yes, Stevin did care about music and was to some extent sensitive to it. Yes, he did listen to musical performances (everyday Dutch song in particular). In short, in writing 'Vande spiegheling der singconst', he did know, to some extent, what he was talking about. When the part-musical, part-mathematical problem of consonance came up, however, the mathematician in him ran away with it and arrived at an almost solipsistically wrong-headed yet delightfully rigorous solution, in such a manner that he unwittingly turned whatever aural checks he undertook into hiding places for his own relentlessly mathematical train of thought.

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[13] The allusion is to the title of John Hollander's book *The Untuning of the Sky*. A brief summary of the entire process can be found in Prins and Cohen, 'Knowing Heaven and Earth Through Music'.

With my grateful thanks for your most helpful comments,
Floris

Part IV, by Cohen and Kursell

What does this dialogue add to rethinking Simon Stevin? We propose that a dialogue is a privileged form for doing exactly that: rethinking this difficult author with respect to musical matters. Yet, a problem with Stevin's work on music is that taking it seriously also means reacting to the way in which it fell outside of the musical discourse of his time. Even though other contemporary writers on music and science were unable to escape idiosyncrasy, to some extent, given the enormous changes in music theory around 1600, Stevin stands out in this respect. The most daring writers – from the Neoplatonic Marsilio Ficino and the famous maestro di cappella of San Marco, Gioseffo Zarlino, to Marin Mersenne and Athanasius Kircher – all came up with radically different ideas, terms and explanations of how music's elements formed a coherent system.[14] Yet none of them renounced the basics that Stevin refused to accept. Since then, most authors have chosen to explain this as driven by mathematical inventiveness, rather than musical listening. We thus propose that Stevin should be considered as a listener in the context of the music of his time. After all, is it really so convincing to project the musical reality of the nineteenth century onto an author from 1600, as so many writers on Stevin did? Of course, following the outstanding mathematics he displayed to calculate this alleged purpose can engage one to the extent that one forgets that it is perhaps not. At least this might be closer to a music theorist's than a historian's standpoint; that is to say, Stevin has always been appealing to writers who wished to disclose his potential for saying something on music as a phenomenon whose inner coherence is more important than its audibility. While music theory has taken the license of treating music in this way ever since Stevin's time, this did not satisfy us when integrating Stevin's interest in music into the more general context of rethinking his interest in solving practical problems through mathematics more generally. We concluded that in order to draw attention to the gap between an accepted discourse and a position that falls beyond its scope, we also needed a new form for our own discourse.

A dialogue not only offers two positions rather than one, it also escapes the traditional role of the author as the strong individual who independently draws conclusions from the state of the art. Luckily for us, with some notable exceptions, research on Stevin has been rather silent since Cohen's first writings on him.[15] Given the multitude of positions, the very rich field of early modern music and science has shed light on many of the peculiarities of individual authors, including the question of whether certain shared concerns drove their idiosyncrasies. It is this lack of such shared concerns among Stevin and his peers that we propose to the present reader should be rethought; and we do so in harmony.

[14] See, e.g., Gouk on Fludd, Pesic on Kepler, Buehler on Leibniz, Prins on Ficino.

[15] Philippe Vendrix (ed.), *Music and Mathematics,* is particularly rich in this respect. Among several interesting contributions, the fine paper by Brigitte Van Wymeersch stands out. Although she focuses on Descartes, the concept of the 'nombre sourd' is also interesting in comparison to Stevin. Rudolf Rasch's paper again explores the hypothesis, mentioned above, that Stevin proposes equal temperament.