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Abbreviations:	
GMO <i>Grove Music Online</i> , ed. D. Root < http://0-www.oxfordmusiconline.com >.	
MGG2 <i>Die Musik in Geschichte ud Gegenwart</i> , ed. L. Finscher < http://www.mgg-online.com >	
ODNB <i>Oxford Dictionary of National Biography</i> , ed. L. Goldman < www.oxforddnb.com >.	
RISM <i>Repertoire internationale des sources musicales</i> .	

CORRESPONDENCE

Unequal Temperaments: Revisited

Response to 'Unequal Temperaments', a review by Bradley Lehman of Claudio Di Veroli, *Unequal Temperaments: Theory, History and Practice (Scales, Tuning and Intonation in Musical Performance)*, Second revised edition (eBook, Bray Baroque, Bray, Ireland 2009), published in this journal, volume 3, part 2 (2009), 137-163.

CLAUDIO DI VEROLI

'The book is certainly worth its price, and more. I hope that this review itself leads to serious scholarly and practical discussion of the musical and theoretical points raised; and to a third edition of *UT*?' (p. 163). Reading this, the last sentence of the review, it may seem that Dr Lehman offered a benevolent list of suggestions for improvement. Unfortunately this is not the case: his lengthy review is ostensibly not an analysis of rights and wrongs in the *Unequal Temperaments eBook (UT)*, but a fully-loaded diatribe against it. Lehman strongly objects to the book's layout, formatting, organization, language, mathematics, tables, sources, historical presentation, tuning methods: everything. In the two introductory pages, he boldly deplores (a first in my 64 years of a very active scientific and musical life) my purported 'disorganized thought processes', 'weak standard of scholarship', 'mathematically-based points ... based on faulty premises' and so on. Except for a few short comments on 'Sections I found especially valuable', the review's sections bear headings such as 'Overemphasis on beat rates and mathematical precision ...', 'Unrealistic expectations for practical musicianship', 'Hasty or erroneous conclusions, apparent biases ... misleading information' and 'Points that appear too lightly-researched'. Are Dr Lehman's scathing criticisms justified? How can somebody with a Ph.D. supervised by world-famous scientists, followed by decades of dedication to early music instruments and interpretation, publishing works consistently praised by highly respected musicians and musicologists, have eventually failed so miserably? How can one reconcile the high praise received from knowledgeable readers of *UT* (not to mention reviews, including those published by the British Clavichord Society and Early Music America) with the distinctly negative appraisal presented by Lehman? To find out, let us scrutinize the review's main points, mostly checking them against published information that the reader can easily verify.

Format and editor

Lehman begins the review of *UT* with his opinions on 'The Format' (pp. 137-138), declaring that the 'page layout often looks amateurish'. The reader can easily check Lehman's mastery of the matter by examining the visual design he has produced

for his own main website, <<http://www.larips.com/>>. Actually, *UT* closely follows the recommendations made by specialists about material meant to be read on a computer screen rather than on a printed page. Moreover, when printed, *UT* has been found to be ‘a pleasure to read’ in the review by David Hitchin in the *British Clavichord Society Newsletter* (October 2009). This section of Lehman’s review also objects to the book’s contents (as discussed below) and ends thus: ‘these are problems that a competent outside editor ... would have handled, if the book had one’. A remarkable assertion considering that the text of the second edition (i.e. that under review) was thoroughly checked by someone very knowledgeable on tuning and temperaments: Fred Sturm, of the University of New Mexico, USA.

Rough draft

Also in ‘The Format’ section, Lehman dismisses *UT* as very similar in content to my first (similarly titled) book on temperament, published in 1978:¹ where necessary, I shall hereafter distinguish between the volumes as *UT* 2009 (the volume under review) and *UT* 1978. Lehman deems *UT* 2009 ‘a rough draft ... as if Di Veroli is not fully committed to writing an entirely *new* book’ (p. 138). Indeed later in the review he goes even further: ‘Di Veroli does not want to deal extensively with anything that he did not already know about or use in the early 1980s, when his first book was greeted so enthusiastically by musicians’ (p. 144). At the risk of stating the obvious, the historical past does not change over time: only our understanding of it does, sometimes. Anybody collating my two *UT* books—scores of musicians have—will find that, though the general organization has been largely preserved, *UT* 2009 is a thoroughly rewritten work, which fully incorporates the musicological advances of the intervening thirty years. Note also the contradiction: the review implies that *UT* 1978 and *UT* 2009 are similar, while at the same time the former is very good and the latter very bad.

The Internet

The reviewer argues that ‘Di Veroli often relies on Internet chatter, web sites, and vague rumours in preference to citing peer-reviewed and published work ...’ (p. 137) and that *UT* ‘teaches the reader (implicitly) to trust the Internet ahead of trusting books, academic journals, and libraries’ (p. 138). Both assertions are completely unrelated to the actual content of *UT*. Moreover, because of the web’s informality and lack of peer-review, Lehman finds it ‘questionable whether such Internet-based material should be included at all in a book or eBook’ (p. 138). I am afraid that many writers do it nowadays: a book review is arguably not the best place to question generalized and accepted present practices.

‘Valuable’ sections

¹ *Unequal Temperaments and their Role in the Performance of Early Music* (Buenos Aires, 1978).

‘Sections I found especially valuable’ (pp. 138-139): in these few paragraphs Lehman only approves of two aspects of *UT*:

1. *Barbieri*: ‘At these and numerous other points, *UT* refers the reader to the excellent scholarly work of Patrizio Barbieri ... I had not known much about it before this year’ (p. 139). Lehman deplores my ‘weak scholarship’, yet this admission seems to reflect poorly on *his* scholarship, because Prof. Barbieri is widely considered one of the leading modern scholars on the history of temperament.
2. *Fretting*: The treatment in *UT* is found by Lehman to be ‘excellent’ (p. 139). Needless to say, this is the matter that most readers of the review—the members of the Viola da Gamba Society in particular—are likely to check in the book and have the knowledge to judge for themselves.

Jorgensen

In the section of the review ominously headed ‘Overemphasis on beat rates and mathematical precision, ahead of *musical* listening skills’, the spectre of the late Prof. Jorgensen looms large in assertions such as ‘Di Veroli’s and Jorgensen’s approach ... within their calculation-based paradigm’ (pp. 139-140), ‘... heavy reliance on Owen Jorgensen’s speculations’ (p. 149) and ‘... over-reliance on Jorgensen’ (p. 149). This is surprising indeed. Of all the things that could be said about my work, I never imagined somebody would seriously, and repeatedly, assert that I have ever agreed with—or indeed followed—Prof. Jorgensen’s ideas and methods. The evidence on the contrary is there for everybody to check. As early as 1980, *UT* 1978 was described by the temperament scholar Prof. Rudolf Rasch as ‘the opposite’ of Jorgensen’s *Tuning the Historical Temperaments by Ear* (Marquette, MI, 1977).² Actually *UT* 2009 includes clear and detailed rebuttals to the main proposals of Jorgensen, including (a) his thesis about prevalence of unequal temperaments up to the twentieth century and (b) his practice of tuning and modifying historical temperaments by means of equal-beating, which *UT*—in spite of Lehman’s assertions to the contrary—does *not* follow, and indeed openly criticizes in Section 12.7. As elsewhere, here Lehman also digresses and explains at great lengths how he prefers doing things differently from *UT*, using the review as a propaganda vehicle for his own ideas.

Tuning by beats

This practice only became widespread a century ago, as explained in detail in *UT*, which—uniquely in the literature—includes for most temperaments three tuning methods: one based on counting beats; another on estimating beats by interval comparison but without counting; and finally, one following traditional pre-nineteenth-century methods. All this notwithstanding, Lehman misrepresents *UT*

² R. Rasch, ‘Owen Jorgensen: *Tuning the Historical Temperaments by Ear* (1977) and Cl. Di Veroli: *Unequal Temperaments and their Role in the Performance of Early Music* (1978) (book reviews)’, *Stimulus* (1980), 32-37.

as upholding that ‘beat rates ... and checks (by beats) is the only way to deliver sufficient accuracy in practice I [Lehman] disagree with the principle that tuners *must* use them to deliver acceptable work’ (pp. 139-140). This is outrageous, for I have never written nor believed such a thing: suffice to read *UT* where, on p. 140, I explain how *non-beat-rate* methods achieved good tuning precision in the eighteenth century and remarkable accuracy in the early nineteenth century. Lehman further insists in belittling my tuning methods by the naïve expedient of simply describing his own. In the process, he argues at length against the accuracy of tuning a keyboard using beat rates, a fact verified for over a century by the daily practice of piano, organ and harpsichord tuners. If Lehman now wants to prove everybody else wrong, he should use better arguments than simply describing his own tuning methods; moreover, he should refrain from using a book review for this—futile—attempt.

Beat rates

Lehman also discovers fault with *UT* because he ‘find[s] beat-rate sequences difficult to memorize’ (p. 140). Why should anybody memorize beat-rates? *UT* clearly states that the tuner should read the beat rates from a printed page (see the box at the end of p. 201 in *UT*). In the same passage, Lehman also writes that ‘instructions based on beat rates work accurately only at one particular pitch level’. Not so: Section 12.6 of *UT* proves that beat rates for $A=415$ Hz are fully satisfactory in tuning practice, all the way from $A=392$ to $A=447$. Furthermore, if the utmost accuracy is desired, the Table in p. 184 of *UT* shows how to achieve it, for pitches from $A=392$ to $A=466$, by simply using a metronome with different speeds. Shouldn’t a reviewer read the book first?

Non-keyboard instruments

In Lehman’s discussion of ‘Unrealistic expectations for practical musicianship’ there are five points to note:

1. *UT* asserts that “[violin] Fingering accuracy in any circular temperament is not difficult to master” On the next page, that statement is contradicted by this one: “Playing in tune with an equally-tempered keyboard is difficult. Playing in tune with an unequally tempered one is impossible” Which is it to be? (p. 141). The apparent contradiction is not in *UT*, but in the reviewer’s out-of-context quote, because he overlooked a very significant detail: in *UT* the second statement is preceded by the following clause: ‘When a Romantic or modern violin player follows Pythagorean intonation’. Pythagorean intonation is indeed truly incompatible with unequal circular temperaments.
2. The review states that on p. 317 of *UT*, where the 31-division is discussed, ‘[t]here is a forbiddingly complicated map of the fingerboard presented as something to learn directly ...’ (p. 141), further implying that *UT* introduces unnecessary mathematical tools in violin fingering. Yet, after the map, *UT* clearly explains that ‘some Baroque sources advocated it as

standard violin practice, including charts similar to Fig. 16.2.5 above'. The maps that Lehman dubs 'forbiddingly complicated' are actually historical!

3. 'It is a quixotic pursuit, this misguided expectation that all the notes by singers and non-keyboard instrumentalists ought to agree exactly with a keyboard's pitches, all the time' (p. 142). Nowhere in *UT* is the matter stated in those terms, but rather as trying to achieve an intonation in agreement with the keyboard. This is certainly not 'quixotic' and, as stated in *UT*, it was the advice of important Baroque musicians, not to mention leading modern performers such as the renowned gamba viol player Wieland Kuijken. He observed that 'When you have to play [the viol] with the harpsichord ... you just have to try to play in the same temperament as the harpsichord, however it is tuned'.³
4. 'Bruce Haynes addressed this issue thoroughly in 1991 ['Beyond Temperament: Non-Keyboard Intonation in the Seventeenth and Eighteenth Centuries'. *Early Music*, 19 (1991), 357-382], showing that most seventeenth and eighteenth century [sic] musicians probably did *not* try to match keyboard temperaments' (p. 142). This is not what Haynes wrote: on p. 362 he concurs with *UT* showing how some musicians did and others did not. Besides, Haynes's widely known and interesting paper was largely superseded by Barbieri's much more extensive work on the matter, often quoted in *UT*.
5. 'I hope that Di Veroli does not believe that violins ever had frets' (p. 148). Before writing such a disrespectful remark, Lehman should have read p. 315 of *UT*, where I explain why and how I use the term 'fret' for violins.

Keyboard tuning

'Is anyone honestly able to "count alternatively 7 beats in a second and 8 in the next second", working with a timekeeping device? ... *UT* [p. 266] asks the reader to observe one second that has 7 beats and another that has 8, accurately' (p. 142). As above ('Tuning by beats'), it is surprising to find that a self-professed tuning specialist seems unfamiliar with everyday modern keyboard-tuning practices. Firstly, piano tuners have used 'timekeeping devices' maybe as early as Helmholtz's classical treatise, *On Sensations of Tone*.⁴ Their use certainly became widespread in the early twentieth century: William Braid White specifically recommended the metronome for beat rates.⁵ Secondly, Lehman seems unaware that these beat rates and even higher ones are in common use at present: White prescribed rates as high as 10 per second.⁶ Finally and most importantly, the review fails to mention that *UT* does not ask for any accuracy here. On the contrary, the text acknowledges the

³ W. Kuijken and C. Hogwood, 'Wieland Kuijken and Christopher Hogwood on the Viol', *Early Music*, 6 (1978), 4-11: quoted in *UT*, 308.

⁴ *Die Lehre von den Tonempfindungen als physiologische Grundlage für die Theorie der Musik* (Heidelberg, 1862), trans. by A. J. Ellis as *On the Sensations of Tone as a Physiological Basis for the Theory of Music* (London, 1875 and later editions).

⁵ *Piano Tuning and Allied Arts* (London, 1917; Boston, 5/1946), 85.

⁶ *Ibid.* 73.

difficulty, and suggests an alternative in the previous paragraph: ‘Due to the very fast beat rates for the major thirds, the reader may find it easier to reverse the orders of the pairs in the scheme below’ (*UT*, p. 266), thus making an accurate beat-counting no longer necessary.

Mathematical apparatus

‘Reading the sections about “Homogeneous Meantone” and “Attenuated Meantone” (1/5 and 1/6 syntonic comma systems, respectively), I miss the simpler sense that the seventeenth and eighteenth century [sic] musicians in actual practice flattened the 5ths and sharpened the 3rds as much as sounded acceptable to them’ (p. 143). The reviewer may have *missed* it, but is there more than once, for example: ‘Several different variants of standard meantone with fifths tempered by less than 1/4 S.c. were described in ancient times (some with precision, others quite obscure)’ (*UT*, p. 75); and ‘Leopold Mozart ... and many other 18th-century sources ... the “nine commas” dictum ... being best approximated by either the 1/5 S.c. or the 1/6 S.c. variants’ (*UT*, p. 77).

Lehman also dismissively notes that ‘[i]f some theorists in the past were adamant about mathematical or theoretical precision, to the same degree as Di Veroli is now, this did not necessarily concern any ... practical musicians ... [who] could make their music without needing to know *any* such mathematical apparatus!’ (p. 143). This is an amazing straw man argument. Any reader of *UT* can check that the book uses maths for the same purpose as ancient theoreticians, i.e. to explain and substantiate assertions on the properties of temperaments. Nowhere does *UT* state, or even suggest, that mathematics were used (or needed) in the daily tuning and performance practices during the seventeenth and eighteenth centuries.

Standard French Temperament

‘Presenting what he calls “Standard French” temperament ... [Di Veroli] asserts that “Music with extreme modulations or just many accidentals is likely to sound very dissonant.” Instead of saying it is “likely to” sound dissonant, why did he not do some hands-on testing with extant music, and report some concrete results?’ (p. 144, n. 25). I have three observations on this. First, there was no reason for Lehman to be dismissive about *UT* naming “Standard French” a temperament based on “Standard meantone” diatonic fifths. Second, Lehman is again misreading my book, where ‘likely to’ (*UT*, p. 109) clearly means that some music sounds very dissonant, while other music does not. Third, even more disturbing is Lehman’s disregard for published information. In Section 21.3 of *UT*—and also in other publications of mine—anybody can read about my long experimentation with French temperaments, including public performances. I still have a printed concert programme of mine (a duet of Baroque flute and harpsichord in the Universidad del Salvador in Buenos Aires) dated 3 December 1977, which testifies that the Baroque-model instruments were tuned to the *tempérament ordinaire*. This shows how my experience tuning and playing this most important tuning system spans more than 32 years, *pace* Lehman. As for Lehman’s request to ‘report some concrete results’, any reader—though for some unfathomable reason not the

reviewer—can find them in *UT*, on pages 108, 109, 111, 113, 159, 161, 162, 166, 228, 308, 309 and 406 through 409.

Neidhardt and nicknames

[R]egarding Neidhardt's ... "Big City", we do not get to see its recipe by tempered 5ths' (p. 144). The reviewer may 'not get to see it', but it is duly shown in *UT* on p. 128, Fig. 9.7.1 (last of the two green curves) and discussed in the 'Circle of Fifths' paragraph later on the same page. Lehman also objects to my criticism of Neidhardt's temperaments (incidentally, a criticism shared by other modern writers), but does so by presenting his personal feelings as evidence: 'I have tested most of Neidhardt's 1732 temperaments hands-on; they often sound terrific ...' (p. 145). Indeed, *terror* is what they produce in anybody trying to tune them using the methods of their time, and *UT* explains why in Sections 9.7 and 21.6; these sections are easy to spot in the CONTENTS searching for 'Neidhardt': this also shows how—again—the reviewer ignores information which is very easy to find in *UT*. Lehman further deplores that '*UT* guides the reader to favour a restricted set of only a few generalist solutions' (p. 145). Of course it does! And the reason why is explained in the book's MISSION STATEMENT on p. 14 (and elsewhere). Anyway, *UT* includes keyboard tuning instructions for 25 different temperaments, certainly not a 'restricted set'.

On the subject of meantone 'nicknames' (pp. 143-144), Lehman strongly criticizes *UT* for calling 1/4 S.c. meantone 'standard': yet this is the common usage in the modern literature on early music, and was also common ancient usage in some countries, e.g. *temperamento comune* in Italy during the seventeenth and eighteenth centuries. More generally, Lehman objects to the assignment of nicknames to meantone variants in *UT*, even though the reason is clearly stated in the book: this time in a special box on p. 69.

Vallotti's major thirds

'When tuning Vallotti's temperament step-by-step, we are told to "Check that g-b is a very good major third (but not pure)", and similarly for f-a and c-e. However, in the presentation of regular 1/6 comma, where these 3rds are practically the same size as in Vallotti's (within 0.5 cent), they are called "good but noticeably sharp". This implies a noticeable distinction of quality ... casting in good light the temperament the author endorses (Vallotti) and denigrating the one he doesn't' (pp. 145-146). Firstly, I do not 'endorse' temperaments: *UT* clearly states that they are mostly good or bad only with reference to the different music to be played. As for 'within 0.5 cent', it is indeed a very small amount! Could I be so biased? Let us check. Vallotti has 1/6 Pythagorean comma diatonic fifths, producing good major thirds tempered by 5.87 Cents, as shown in *UT*, p. 123 [check: $d_{III}=4x dV + Sc = 4x(-1/6Pc) + Sc = 4x(-23.46/6) + 21.51 = 5.87$]. The 1/6 Syntonic comma meantone has instead the good major thirds tempered by 7.17 Cents, as shown in *UT*, p. 76 [check: $d_{III}=4x dV + Sc = 4x(-1/6Sc) + Sc = 4x(-$

21.51/6)+21.51=7.17]. The difference between 7.17 and 5.87 is not ‘the same size ... within 0.5 cent’ as claimed by Lehman, but almost three times as much.

Intervals and consonance

‘In the “General Laws on Consonance and Beat Rates” [*UT*, p. 23] Di Veroli explains: “An interval is consonant if and only if the ratio between the two fundamental frequencies is equal to the ratio of small integer numbers.” ... By this standard, a major 10th (5:2) and a major 17th (5:1) are considered ‘more consonant’ than a major 3rd (5:4). This is at odds with Di Veroli’s later assertions ...’ (p. 146). It is not: as stated in the first paragraph on p. 21 of *UT*, all the treatment of interval consonance is restricted to intervals smaller than an octave. This is further clarified elsewhere: in the box on superparticular ratios on p. 22; in the title of the table on p. 24; and in the text on p. 25. I insist, a book reviewer should first read the book.

Equal temperament

‘There are some overstatements about the necessity of equal temperament’ (p. 147). Lehman criticizes my advocacy of equal temperament for music dated from *c.*1750, and also for some music as early as 1742. Against this he offers his personal experience with his own ‘Bach temperament’. Actually *UT* could as well be criticized for the opposite excess, because the book—in agreement with Lehman—supports playing J. S. Bach’s music with decidedly unequal temperaments, while two eminent scholars such as Profs Rudolf Rasch and Mark Lindley have argued that Bach’s keyboard temperament may have been equal temperament, and this for important works famously written decades before 1742.

Almost-equal temperament and Jorgensen (again)

‘[T]he book asserts what “some” or “most” nineteenth century [sic] tuners would do (with regard to the amounts of inequality that would have been tolerated in the musical taste of the time). It does not supply a single citation of support, or any description of the reasoning behind the statement’ (p. 149). Here the disregard for the contents of the book under review is blatant. It will suffice to refer to *UT*, pp. 138, 139, 142 and 172, where the full rationale for Almost-Equal temperament is explained, including historical sources (namely an eighteenth-century quote and a nineteenth-century description of a very similar temperament).

The review also states that the purported deficiency ‘appears to come from heavy reliance on Owen Jorgensen’s speculations ...’ (p. 149). This bold assumption goes directly against documented evidence: the Almost-Equal temperament (first published in *UT* 1978) was already included in typescripts sent by the author to prospective publishers back in 1976, one year before the first work on temperaments by Jorgensen was available.⁷ When, in his 1980 review (see note 2

⁷ *Tuning the Historical Temperaments by Ear* (Michigan, 1977).

above), Prof. Rasch compared *UT* 1978 with Jorgensen's treatise he concluded that the two books were 'written totally independent from one another' (my translation, *op. cit.* p. 32). As a further confirmation, I have a letter from Prof. Jorgensen in which he agrees that this was indeed the case. More assertions in the review about the purported 'over-reliance of *UT* on Jorgensen' are discussed further below.

Weak scholarship and Suppig

'Section 19.7 [*UT*, p. 368] asserts erroneously that Friedrich Suppig's manuscript about temperament was "published" in his lifetime. It was published only as recently as 1990, as a facsimile edition, with a historical essay by Rudolf Rasch' (p. 150). I wrote: 'Suppig's "Labyrinthus Musicus ..." was published together with his "Calculus Musicus"...' I stand corrected for my slip of the pen: I should have written 'put' instead of 'published'. But Lehman goes further: 'This is merely one example where Di Veroli relies only on secondary information, or on his own misreading of it (in this case, a 1984 article by Rasch), instead of taking a closer look at sources' (p. 150). This is Lehman's main attempt to show my 'weak scholarship'. However, the 'historical essay' to which he refers⁸ includes the full contents of Rasch's 1984 article, thus the 'secondary information' and 'source' in this particular case are both in the same publication! And I could not possibly ignore all this when I wrote *UT*: Prof. Rasch can attest that he kindly sent me copies of both publications back in 2007.

1/6 comma meantone

On pp. 150-153 Lehman argues at length about the important historical use of the 1/6 syntonic comma (S.c.) meantone temperament. Let us check his assertions and misrepresentations:

1. Among the features of 1/6 S.c., Lehman repeatedly mentions 'the pure 45:32 tritone' as paramount, yet there is no evidence in eighteenth-century sources that this feature was ever considered relevant.
2. Lehman is the first writer known to me to argue that adding two pure intervals produces a pure interval, and indeed to describe as 'pure' an interval with the ratio 45:32.
3. Lehman never mentions that the 'pure tritone' is actually 7:5 (*UT*, p. 376).
4. Lehman refers to a particular vogue of 1/6 S.c. meantone that was restricted geographically and chronologically. Around the middle of the eighteenth century—mostly in Northern France and Germany—many temperaments coexisted: strings would follow either a meantone variant or the very different Pythagorean intonation, while accompanied by keyboards that were either circular or equally tempered. Indeed, equal

⁸ F. Suppig, *Labyrinthus musicus' and 'Calculus musicus': Facsimile of the Manuscripts, Paris, Bibliothèque du Conservatoire Rés. F.211-212 (Dated Dresden, 24 June 1722)*, ed. R. Rasch, Tuning and Temperament Library, 3 (Utrecht, 1990).

temperament was ideal to avoid the resulting cacophony, wrote d'Alembert in 1752; this matter is fully treated in *UT* on pp. 138, 149, 174 and 475.

5. In 1753 Leopold Mozart—mentioned by Lehman as supporting the general use of 1/6 S.c., and fully discussed in *UT*—wished violinists to learn this system as an exercise, but in actual ensemble practice he recommended tuning the open strings to the keyboard and following its tuning, which for his readers in his time was mostly an approximation to equal temperament.
6. Amidst the well-known mid-eighteenth-century French academic *querelles* about the meantone variant to be preferred, 1/6 S.c. was sometimes advocated, but most musicians expressed their preference for the traditional 1/4 S.c., notably among them Michel Corrette.⁹
7. Lehman refers to Telemann and his multiple-division theoretical writings, but ignores that in practice Telemann composed cantatas for solo trumpet in Eb, chamber music with lots of enharmonies, and harpsichord pieces with no fewer than seven flat/sharp pairs (including E# and B#). Playing this music with any non-circular temperament such as 1/6 S.c. meantone (see also the next section, 'Duffin') inevitably produces frequent and truly unplayable discords. Wolves are extinct in Europe: perhaps this is the way to have them back.

Duffin

On p. 147, and elsewhere, Lehman professes his full approval for the work of Prof. Ross Duffin, who advocates in his writings the use of 1/6 S.c. meantone as a circular temperament. This was known not to be the case already in Baroque times, so much so that in the 1740s Riccati was busy producing radical changes to obtain a circular system, that later evolved into what we call today 'Vallotti's temperament'. The non-circularity of 1/6 S.c. meantone is easy to prove, as done with full details in *UT* (pp. 77 and 123). All this is conveniently ignored by Lehman, who uses Duffin's book, *How Equal Temperament Ruined Harmony*,¹⁰ as an argument against *UT*, criticizing the latter—incredibly—for not following Duffin(!).

It is worth noting that *UT* is not alone in criticizing Duffin: Dr Ibo Ortgies recently published a review of Duffin's work in which, among other objections, it is stated that 'he makes little distinction between the theoretical concepts and the actual practices of tuning and temperament at a given time'. Ortgies concluded that 'the background of some important historical and tone-systematical principles of intonation is not correctly presented'.¹¹

⁹ See P. Barbieri, 'Il "migliore" sistema musicale temperato: "Querelles" fra Estève, Romieu e altri accademici francesi (c.1740-60)', *L'Organo*, 27 (1991-1992), 31-81, esp. 54-55; see *UT*, p. 82.

¹⁰ *How Equal Temperament Ruined Harmony (and Why You Should Care)* (New York, 2007).

¹¹ 'Not Quite Just: Review of Ross Duffin, *How Equal Temperament Ruined Harmony*', *Early Music*, 35 (2007), 452-454, at 453 and 454 respectively.

Nine commas

‘The entire Section 6.4.6 is about “nine commas in a tone”, and it says: “Unfortunately it can be shown that the dictum [of ‘nine commas in a tone’] is not true for any existing or conceivable meantone temperament.” This is absurd, because it is true both for regular 1/6 Pythagorean comma, and for its practical extension, the 55-note division of the octave! Di Veroli is so firmly fixated on promoting and analysing syntonic comma schemes that he has missed this’ (p. 152).

Lehman seems to be in utter confusion here. First of all, please compare these statements with the section above on ‘Vallotti’: on p. 146 of the review Lehman accuses me of ‘endorsing’ Vallotti’s 1/6 *Pythagorean* comma scheme vs 1/6 *syntonic* comma meantone. Vice versa, now he accuses me of promoting *syntonic* comma schemes against *Pythagorean* ones! *Which is it to be?* Neither of course: Chapter 8 of *UT* is devoted to ‘French’ *syntonic* circular systems and Chapter 9 to ‘Good’ *Pythagorean* ones, clearly explaining differences and implications in Sections 8.1, 9.1 and 11.10.

As for my full discussion of the *Nine commas* dictum (*UT*, pp. 379-381), my results are not ‘absurd’ but actually very accurate. It is Lehman who is ‘so firmly fixated’ on arguing against *UT* that he has missed the initial full definition of the dictum in *UT* (p. 379) as three simultaneous statements. For meantone temperaments, ‘NIC: A Tone is subdivided into Nine Commas. / SEM: The Chromatic and Diatonic Semitone have respectively 4 and 5 commas. / ENH: A sharp is 1 comma lower than its enharmonic equivalent flat’. As shown on p. 380 of *UT*, the Dictum implies the following values in Cents: NIC=193.56, SEM=86.03 and ENH=21.51. If instead of the syntonic comma we used the Pythagorean comma, as kindly suggested by Lehman, the values for the Dictum would be respectively 211.14, 93.84 and 23.46 Cents. Calling V the pure fifth and VIII the pure octave, for any regular temperament, NIC=2V-VIII, SEM=7V-4V VIII and ENH=|12V-7V VIII|. These formulas, applied to 1/6 P.c. meantone, yield respectively 196.09, 86.31 and 23.46 Cents: only the last number coincides, proving that, in 1/6 *Pythagorean* comma meantone, only ENH coincides with the dictum, while—pace Lehman—NIC and SEM diverge much more than in 1/6 *syntonic* comma meantone.

The review is further in error where Lehman describes my statements on the dictum as ‘absurd’ because he finds the dictum strictly true for the 55-note division (p. 152). It is not. In *UT* I wrote that the 55-division provides ‘ENH best approximated ... also gives a reasonable approximation to NIC and SEM’ (*UT*, p. 381). The 55-division (where a ‘comma’ is $1200/55=21.82$ Cents) best approximates a tone with 9 ‘commas’, thus its size is $9 \times 21.82=196.4$ Cents, significantly different from the $9 \times 21.51=193.6$ Cents produced by the dictum using the syntonic comma, as duly shown in *UT*. Even more interestingly, 196.4 is

almost 15 Cents away from the $9 \times 23.46 = 211.1$ Cents we would get if we followed Lehman and used the Pythagorean comma instead!

Schlick

‘[T]his very early ... system is placed inconspicuously near the back of the book, rather than given centralized attention for its virtues’ (p. 152). I suspect that the reviewer is referring here to some other book: in *UT*, Schlick is one of the five main temperaments assigned an individual section in Chapter 9 on IRREGULAR ‘GOOD’ TEMPERAMENTS: it could not be more ‘centralized’! And ‘inconspicuous’ it is certainly not, for it gets one of the most extensive treatments in the book: searching the CONTENTS for ‘Schlick’ sends the reader to three places with a total of eight full pages (pp. 116-117, 236-237 and 410-413), more than those devoted—say—to Werckmeister III, a temperament as important historically and much more relevant in performance practice.

Vallotti/Young

On p. 152 Lehman objects to quite a few points in *UT* where, in my opinion, we actually agree. Then he criticizes my definition—commonly found in modern works—of Young No. 2 as a rotation of Vallotti, on the grounds that ‘while such a rotation looks easy enough on paper, it actually involves moving half of the notes: 6 out of 12’. This is certainly true, but as a criticism of *UT* is absolutely unfounded. Whenever a temperament can be easily produced on a keyboard as a retuning of another one, the matter is clearly stated in *UT* in Chapter 13, where the ‘original’ schemes have lines in a different colour. This does not happen in the sections on Vallotti/Young (13.16 and 13.17). Nowhere in *UT* is it stated or implied that Vallotti can be easily retuned/rotated as Young No. 2 or vice versa.

Lehman’s Bach temperament

Remarkably, the reason for the rage of Dr Lehman against *UT* is clearly explained in his review: he was upset at finding that his own main contribution to temperaments is *not* included among the handful of tunings *UT* recommends to modern players. Lehman naïvely complains that *UT* ‘pointedly avoids evaluating my work ... in any meaningful way’ (p. 153). Actually, Lehman’s writings suggest that by ‘meaningful’ he means to agree with his ‘absolutely compelling’ reasons for his ‘Rosetta Stone’ Bach temperament discovery, i.e. his own interpretation of the ‘WTC squiggle’ as a tuning system.¹² Let me summarize where I disagree with Lehman in this respect:

1. A book review is not the place for a reviewer to engage in polemics against the author.

¹² Published as a two-part essay, ‘Bach’s Extraordinary Temperament: Our Rosetta Stone’, *Early Music*, 33/1-2 (2005), 3-23 and 211-232.

2. It is outside the stated goals of my book—and a physical impossibility—to evaluate in any detail every one of the scores of modern proposals for Bach’s temperament. Lehman has no reason to complain: *UT* devotes almost two pages (pp. 130-131) to his own proposal!
3. Lehman is free to deem his own creation very important, and to call it ‘Bach temperament’, but leading scholars have published strong arguments in disagreement with his creature.
4. *UT*, p. 130, clearly states that only a particular issue in Lehman’s temperament is dealt with, because the fundamental weaknesses of his work have already been addressed (in spite of Lehman’s rebuttals) in an article by Lindley and Ortgies (duly mentioned on p. 130 of *UT*), which focuses on exposing the ‘many weak links in [Lehman’s] chain of reasoning [in his ‘Rosetta Stone’ article]’.¹³
5. Lehman also notes in his review that the ‘main argument presented [*UT*, p. 131] against my temperament is ... that “E-G# is his *worst* major third, and the thirds surrounding it are also quite bad.” This is preposterous and short-sighted criticism, because only a few pages later *UT* presents the “WTC Optimal+” solution that Di Veroli has worked out ... [which] has exactly the same size E-G# as mine, although the book doesn’t present that fact directly (why not?)’ (p. 153). Unfortunately, Lehman fails to distinguish between absolute and relative deviation of an interval with respect to its neighbours in the Circle of Fifths. The difference arises because Lehman’s ‘Bach temperament’ is significantly less unequal than ‘WTC Optimal+’. The relevant point is that in the latter the worst major tonality is F# major (6 sharps), which Bach demonstrably used much less often than Lehman’s E major (4 sharps). The all-important A major triad is also much better in WTC Optimal+.
6. It is also worth noting that *all* the historical circular temperaments (as well as modern reconstructions such as Kellner and Barnes) have been either ‘symmetrical’ (the mistuning of major thirds increasing at the same ‘speed’ towards either flats or sharps) or slower towards the sharps, sometimes very obviously so. This is the case in d’Alembert and most of Neidhardt’s systems, some of them with their worst major thirds located two positions further clockwise than expected by symmetry. Lehman’s proposal is unique in running straight against this systematic historical trend: his worst major thirds are located two positions *counter clockwise* from symmetry (see *UT*, p. 130, Fig. 9.7.2).
7. A fundamental fallacy in his review is that Lehman compares his ‘Bach temperament’—which he strongly upholds as the best ever proposal for Bach—against my WTC Optimal+ which he dubs my ‘champion’ (p. 153). This is seriously misleading, for *UT* very explicitly *does not advocate at all* the use of WTC Optimal+ in performance practice, for Bach or for any other music. (More on this below under ‘WTC Optimal’.)

¹³ ‘Bach-Style Keyboard Tuning’, *Early Music*, 34 (2006), 613-624, at 613.

Couperin's temperament

On p. 148, Dr Lehman produces a novelty: the first objection to my accurate reconstruction of François Couperin's organ temperament:

1. Lehman boldly describes it variously as 'pseudo-seventeenth-century instructions' and a 'non-historical temperament', in spite of the fact that, after their initial publication almost three decades ago, those instructions were confirmed by their significant similarity to the temperament published by Lambert Chaumont with his organ works of 1695, barely five years after Couperin published his organ masses (see *UT*, p. 105, Fig. 8.2.5). (Interestingly, I have been recently informed that the well-known French organ maker Formentelli has found in a seventeenth-century organ in France intact original fluework, which is tuned in a very similar system).
2. Later (pp. 154ff.), Lehman objects again to my Couperin temperament. Here he presents an assortment of his personal feelings as arguments against my proposal (which carefully correlates consonance with frequency of use of major thirds in F. Couperin's *Messe pour les Paroisses*, for organ). Lehman writes that my proposal has the non-meantone notes 'crudely moved' (p. 154): yet a full research work and six full pages in *UT* (pp. 394-399) were devoted to their rationale and optimal fitting to Couperin's *Messe*.
3. Lehman states that he remained unconvinced after trying my Couperin temperament ... on a *harpsichord*! He should have used the right instrument, because Couperin's temperament was meant to enlarge the meantone compass avoiding however most of the prominent dissonances otherwise produced by the loud *Tierces* of Baroque French *organs*. Lehman should have read my clear explanations about this matter in *UT* at pp. 64, 100, 104, 197, 394 and 395.
4. Eventually Lehman suggests (for the fourth time! *Quousque tandem abutere, Catilina, patientia nostra?*) his own idea instead: 'the assumed basis of 1/4 comma tempering in the naturals is entirely wrong Couperin had to have better circulating systems' (p. 154). Here Lehman's qualification of 'better' is unfounded: anybody minimally versed in French Baroque organs knows—as clearly stated in the above-quoted pages from *UT*—that a tuning based on wider fifths, and the consequent wide major thirds, would yield continuous discords with the loud French organ *Tierces*, a most important stop in the palette prescribed by Couperin and other French Baroque organists.
5. Lehman's idea is further contradicted by well-known evidence that a diatonic 1/4 S.c. was the rule in all French (and most European) organs well into the eighteenth century. In France, concrete proposals to change over to 1/5 S.c. or other meantone variants were first made years *after* F.

Couperin published his Masses, and were counterbalanced by many musicians attesting that $1/4$ S.c. kept being the preferred solution for decades (Corrette specified ‘un quart de Comma’ for eight fifths in 1753, and the $1/4$ S.c. was described as common fare in French organs by Dom Bédos and others *c.*1770). Again, this made lots of sense for French organs with their prominent *Tierras*. Lehman’s suggestion that François Couperin (in his late teens when he composed the *Masses*) had his imposing instrument at Saint-Gervais (or any other organ) fully retuned against sensible, widespread and well-established norms, is extremely unlikely and is not borne by any evidence.

Digression: Rameau’s temperament

Lehman’s review is full of digressions. Let me digress once too, especially after discussing Couperin, because elsewhere Lehman has applied his peculiar ideas on French Baroque temperament to Rameau, no less. In his recent webpage, which he recommended in the HPSCHD-L online forum in March 2010, Lehman writes: ‘Rameau’s 1726 ... $1/4$ comma division ... does not work well in practice; it has to be a gentler division such as $1/6$... to sound sufficiently smooth in Rameau’s music ... the history books are mistaken ... Further evidence (admittedly circumstantial) away from $1/4$ comma is in ... Rameau’s *c.*1728-9 volume of harpsichord music ... He carefully described the theoretical “difference of one Quarter-tone” between such pairs, theoretically, but emphasized that they are exactly the same key on the keyboard’.¹⁴ In the passages quoted Rameau clearly explained how one of the meantone tenets—differentiating sharps from flats—had to be modified in order to make the temperament enharmonic: in no way can this be construed as ‘evidence away from $1/4$ comma’.

Even worse, having dubbed current musicology ‘mistaken’, Lehman is telling us how he *feels* that $1/4$ comma *ordinaire* is too unequal for Rameau and why, therefore, it must have been another temperament with wider fifths. Here very obviously—as already observed in the aforementioned article by Lindley and Ortgies—Lehman gives his feelings priority over the historical record, which in this case consists not only of the works by Rameau he quotes, but also of the other contemporary French sources on the *temperament ordinaire*, all uniformly advocating $1/4$ comma diatonic fifths yielding pure major thirds.

German vs Italian

‘*UT* is weak on ... modern German language sources’ (p. 148). Now Lehman tries a low blow, knowing (from an online forum where I once asked for a translation) that my German is lacking. He also deplors that, among the huge corpus of modern publications on temperament, many of which are duly mentioned in *UT* (including indeed a few in German!) with a bibliography of 165 entries, six further

¹⁴ <http://www-personal.umich.edu/~bpl/larips/practical.html#rameau_ordinaire> (accessed 16 December 2010).

ones should have been included that Lehman deems ‘especially important’ (p. 148). This is ridiculous nitpicking: let me quote my heading in *UT*, p. 449: ‘LITERATURE CITED – This is not an exhaustive bibliography, but only the full list of published works cited in this treatise Specialised bibliographies are available online. The most complete one ... with about 6,000 entries’. Besides, I find that at least one of Lehman’s six ‘especially important works’ has basic flaws. Another one, dated 1985, is referenced on p. 149 (n. 57), where Lehman asserts that it makes ‘Section 19.9 [of *UT*] ... unnecessary’. Why? *UT* is not a research report but a treatise, which includes many topics already treated in other modern publications. Section 19.9 is indeed necessary and, more significantly, it is based on a manuscript of mine dated 1975 and abridged on p. 240 of *UT* 1978, thus *published seven years before* the source quoted by Lehman!

As for languages, nobody is perfect. I am only fluent in English, Spanish, Italian and French, and I also understand Portuguese, Latin and the German of the Gospels. I find that in temperament matters it is better to be fluent in English and Italian rather than German, because most ancient and modern German texts on temperament are today available in very reliable English translations, while some relevant ancient documents as well as many of the all-important works by Prof. Barbieri—which Lehman acknowledges as ‘excellent scholarly work’ (p. 139)—are only available in Italian.

Fifths and Thirds

The originality of Lehman’s ideas cannot be denied. He objects to evaluating a temperament based on fifths and thirds thus:

- a) ‘There are other problems from over-reliance on Jorgensen as well, especially the heavy bias [of *UT*] toward analysing temperaments with regard to their 5ths and 3rds ...’ (p. 149). (Jorgensen cannot be blamed for this as we will see below.)
- b) ‘*UT* focuses on almost nothing but 5ths and major 3rds ...’ (p. 151). (This shows that Lehman did not read Chapter 6 of *UT*.)
- c) ‘Di Veroli’s spreadsheets are fine, but ... in their analyses of temperaments for comparison they focus almost exclusively on 5ths, major 3rds and minor 3rds’ (p. 163).
- d) In the section headed ‘Getting beyond *UT*’s insufficient analytical methods’ Lehman begins with ‘several case studies that show why the analytical methods in *UT* are insufficient, where it merely measures 5ths and major and minor 3rds’ (p. 159).
- e) ‘Analysis of major 3rds is a superficial way to assess temperaments for real music. It is more important to look at the sizes of steps within diatonic scales ...’ (p. 155). (This is used against Barnes and indirectly against all modern writers on the subject.)

This personal opinion of Lehman, which he repeats *ad nauseam* in the review, is in contradiction with the historical writers on unequal temperaments; from the Middle Ages to the nineteenth century, even through their fierce polemics, they all

agreed and wrote that the main intervals to be discussed and gauged in tuning work—theoretical and practical—were fifths and thirds. A compelling and widespread historical case—well-known to early music performers and tuners—is described in *UT* (pp. 85 and 153): the drastic change implied in all the sizes and proportions of the diatonic scale did not prevent most musicians throughout Europe from switching from Pythagorean intonation to meantone temperament in early Renaissance times, in order to achieve the desired good major thirds.

Interestingly, under the heading ‘Reference of temperament recipes based on 1/12ths of the Pythagorean comma’, Lehman produces a table of 13 ‘temperaments mentioned either in *UT* or in this review’ (p. 158-159). Oh, surprise: for the description and comparison of the temperaments he utilizes—invariably—the much-maligned deviations of the fifths, preceded by a clever method to deduce the much-maligned deviations of the major thirds!

WTC Optimal

I will not enter here a polemic on a topic fully discussed in my book. According to Lehman, ‘Di Veroli presents the recipe as a cycle of 5ths, but doesn’t display the all-important (to him!) set of the 12 major 3rds that turn up in it’ (p. 157). Oh yes I do: please read the main contents of Figs. 9.7.3 (*UT*, p. 134), 21.9.1 and 21.9.2 (*UT*, p. 427).

Lehman further finds that ‘*UT* gives an exceedingly complicated method to set ‘WTC Optimal+’ by ear, requiring the user to count ... beat rates. I have worked out a much simpler method ...’ (p. 158). Thanks for your contribution, Dr Lehman, but *UT* carries simple-no-beat-rate methods for historical temperaments only: WTC Optimal+ is a modern hypothesis that *UT* specifically does *not* recommend for practical use.

Barnes

‘I am surprised that Di Veroli still champions Barnes’s method, in light of its devastating defects’ (p. 156). Actually, *I am surprised that Lehman* has now changed his opinion on a system, which a few years ago, in an online forum, he described as a ‘reasonable result’. (Yes, I read in the review that he disapproves of quoting web pages, but it is there where most of what Lehman has written is to be found). Further, I am curious as to what Lehman means by ‘devastating effects’, given that Barnes’s temperament is Vallotti with a minimal improvement: one note slightly changed so that only two major thirds are Pythagorean. Both tuning systems have now been in widespread use for decades by world-renowned soloists, ensembles, tuners and instrument makers, without them reporting any ‘devastating effects’ and indeed with excellent and unsurpassed results when performing the music of J. S. Bach and many other late Baroque composers. In his comments on pp. 155ff. Lehman conveniently ignores the full rebuttals, against Barnes’s detractors, which mostly comprise p. 132 of *UT*, the book he is purportedly reviewing.

Finally, Lehman states that ‘The Barnes temperament was then derived by trial and error, rather than systematically from the data ...; this is not properly scientific procedure’ (p. 155). This statement demonstrates a very limited view of scientific method: were this the only way, arguably most of the scientific and technological achievements of the twentieth century would not have occurred. Back to temperaments: only in a few cases it is possible to apply *deduction* to derive a historical temperament from the data; quite often, other methods are needed. Barnes used *induction*, the very scientific modelling procedure of inductive hypotheses formulation *followed* by verification against data. This is explained in many modern books.¹⁵

Customized temperaments

UT describes the prevalence of the *tempérament ordinaire* in eighteenth-century France, something very well documented and agreed upon by current musicology, but not by Lehman who includes it under the heading ‘Getting beyond *UT*’s insufficient analytical methods’ (pp. 159-160). In that section he observes that the *ordinaire* is inadequate for Leclair, a composer not mentioned in *UT*. Indeed, the *ordinaire* was by far the most common system in use by Baroque French composers, but certainly not the only one, as clarified throughout *UT*. Further, on p. 150 of *UT* it is clearly stated that the book discusses ‘general tendencies and practical solutions, mostly disregarding secondary exceptions/variants which, as is well known and agreed upon, were manifold in ancient times’. Leclair is far from being the typical French Baroque composer, writing in late Baroque times with strong elements of Italian style.

More alarmingly, Lehman uses the music by Leclair, Corelli and others to argue that ‘when selecting a keyboard temperament ... it does not suffice ... to apply some generalist solution’ (p. 160): he argues that for each composer one should find out the ideal individual temperament and follow it in practice. The inevitable consequence is that, during every public recital, keyboard tuners (and non-keyboard players) should be busy changing the temperament as different composers are successively played. Am I wrong? Or is it Dr Lehman who has ‘unrealistic expectations’ and who advocates ‘quixotic pursuits’? One of the stated goals of my *UT* books has been precisely to help to resolve this type of issues. It is well known, and well documented on record booklets, that the leading modern performers of early music have always played following a handful of the ‘generalists solutions’—often using the same temperament for many years—that Lehman now boldly deplors. If he really believes that musicians should all change their ways and that eventually audiences would appreciate (and hopefully also pay for) multiple keyboard retunings during public recitals, Lehman should present his proposal in a more suitable context than a book review, which—for the umpteenth time—is arguably not the appropriate forum to launch such a radical reform in modern performance practice.

¹⁵ One example will serve for several: P. Rivett, *Principles of Model Building: The Construction of Models for Decision Analysis* (Chichester, 1972), esp. ch. 1: ‘The Model in Science’.

Lehman has a point or two, or has he?

[Di Veroli] does not mention in detail any of the Marpurg or Sorge temperaments ...' (p. 144). Not quite: Section 21.7 (*UT*, p. 420) is fully devoted to Marpurg's temperaments, though I failed to include a circular one that may deserve some scrutiny. Anyway, Marpurg's treatment of unequal temperaments (1776) is only of academic interest, being too late as a source for Baroque tuning and also—very significantly—because Marpurg was a staunch supporter of equal temperament! Lehman also objects (p. 149) to the vagueness of my sentence on mid-eighteenth-century German theoreticians and harmonic waste (*UT*, p. 45): he certainly has a (minor) point here, but this issue and the omission of Marpurg's circular system were already spotted before mid-2009, when I added the required entries (re *UT*, pp. 45 and 420) to the 'Errata and Addenda' file in my *UT* website, <<http://temper.braybaroque.ie/>>.

Conclusion

In decades of reading about tuning and temperaments I have never encountered a text with so many inaccuracies as Lehman's *UT* review; nor have I ever read a book review with such a blatant disregard for the actual contents of the work under scrutiny. My *UT* treatise is certainly far from perfect, but the faults reported by the reviewer are simply not there.

In summary: (1) Dr Lehman's review ignores most of the topics in *UT* that have been acknowledged—by other reviewers and knowledgeable readers—as important contributions. (2) The review often digresses for pages on end, expounding Lehman's personal opinions in open contradiction with current musicological thought and tuning experience. (3) Most of the criticisms in the review are based on quoting out of context in such a way as to grossly misrepresent the actual content of *UT*. (4) The remaining criticisms are based on Lehman's personal ideas and calculations that have been demonstrated to be in error.

Notes on the Contributors

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RICHARD CARTER grew up in a musical family, playing the 'cello, but was dissuaded from studying music and took a degree in Physics at New College, Oxford. Dissatisfied with the career which unfolded, he spent twenty years living and working on the English canals. Increasing interest in early music and historical performance led to him taking up the viol and baroque 'cello, with encouragement and guidance from Stewart McCoy, Alison Crum and Catherine Finnis. Since moving to Austria in 2002 he has devoted himself to early music, supporting the teaching and performing activities of his partner, Johanna Valencia, and running a small publishing venture, *Oriana Music*, with a special emphasis on lyra viol and viol music for beginners. He is a founder member of the Vienna-based viol consort *Almayne*, and a former editor of this journal (2009).

JOHN CUNNINGHAM received his Ph.D. in 2007 from the University of Leeds, where he studied with Peter Holman. His first book, *The Consort Music of William Lawes, 1602-1645*, was published by Boydell and Brewer in July 2010.

CLAUDIO DI VEROLI grew up in an Italian family in Buenos Aires, Argentina, where he was trained by a distinguished group of European musicians. In the early 1970s he lived in Europe, where he was strongly influenced by Colin Tilney in London and Hubert Bédard in Paris. He was granted access and practised extensively on the antique keyboards in the Fenton House (London) and the Paris Conservatoire Musée Instrumental (now Musée de la Musique). With a Ph.D. in Statistics from Imperial College London (supervised by Profs. E. M. L. Beale and David R. Cox) he returned to Buenos Aires, where he pioneered the performance of Baroque music based on ancient practices. Considered a leading harpsichordist and specialist in Baroque interpretation in South America, he has been Professor of Harpsichord and examiner of the Organ course at the Conservatorio Nacional in Buenos Aires. For decades Di Veroli has carried out research in Baroque music interpretation, ancient keyboard fingerings and unequal temperaments, often applying state-of-the-art scientific and computer tools. His writings have been endorsed by leading musicians such as Gustav Leonhardt, Igor Kipnis and John Barnes. They include four books—two on tuning and temperament, one on Baroque keyboard technique and a recent one on Baroque keyboard interpretation—and more than 40 papers. He now lives in Bray, just south of Dublin, Ireland. Recent teaching practices include short courses and masterclasses in Uruguay, Argentina and Italy.

American-born harpsichordist and organist MICHAEL FUERST comes from Madison, Wisconsin, where he studied organ, mathematics and German. He continued his musical studies in harpsichord with Arthur Haas at the Eastman School of Music, where he was the assistant to lutenist Paul O'Dette. A Fulbright Scholarship enabled him to study with Robert Hill at the Staatliche Hochschule für Musik in Freiburg. He is a member of various groups that specialize in seventeenth-century music, including Chelycus (which has recorded works of Andreas Oswald), and the Hamburger Ratsmusik (which was awarded the Echo Klassik prize in 2006). He worked at the University of Würzburg as a member of a Deutsche-Forschungsgemeinschaft-financed musicological research project to study sources of seventeenth-century German instrumental music. His doctoral dissertation on the *Partiturbuch Ludwig* is nearing completion. Amid an active