

Unequal temperaments circulate again

I enjoyed Mark Lindley's review of Patrizio Barbieri's book *Enharmonic instruments and music 1470–1900* ('A great microtonal survey', *Early Music*, xxxvii/3 (2009), pp.481–3). I must take exception, however, to a sentence by Lindley on p.482: 'The section on 12-note equal temperament includes a page and a half on this other topic (with an appropriately dismissive footnote about Bradley Lehman's hypothesis regarding Bach's use of an unequal circulating temperament)'.

It is clear that Dr Lindley himself thinks my hypothesis ought to be disdained, and he has now said so in this journal three times over the past several years; however, Barbieri's book says nothing of the sort. The footnote in question does not evaluate my work at all, 'dismissively' or otherwise, but simply states factually that the hypothesis exists, in passing. Its full text, from p.297 of Barbieri's book, is: 'As far as Johann Sebastian

Bach is concerned, the latest hypothesis on his use of an unequal circulating temperament has been advanced by Lehman 2005, and his proposal is discussed by Lindley & Ortgies 2006, and O'Donnell 2006'. That is the same objective footnoting treatment that Barbieri's book gives to all the other materials that are similarly ancillary to his argument.

Barbieri's bibliography on p.574 then mentions the page numbers of only the first printed section of my article; not the printed part 2, or any of the web-based material (further analyses and musical examples, all integral parts of my argument). Obviously, Barbieri has not engaged with my entire argument, if any of it; and therefore, Lindley is overstepping the mark with the assertion that Barbieri's footnote is 'appropriately dismissive'. Eighteenth-century circulating 12-note keyboard temperaments simply are not a strong feature (let alone a focus) of Barbieri's book, as Lindley correctly points out. It says nothing about my work, one way or the other, that Barbieri did not take the time to evaluate it closely.

Another scholar who has cited only the first printed section of my article was Peter Williams, in *J S Bach: a life in music* (2007); his objections published in that book were obviously not based on serious consideration of the entire piece. Isn't this like walking out of a concert at the interval, then publishing a 'review' of the unheard second half?

To set the record straight: my hypothesis is built on close observation of the enharmonic requirements in Bach's music, especially where he called for more than 12 differently named notes in the same composition. For example, the WTC's C major prelude and fugue need both an A \flat and a G \sharp . The keyboard musician must find a workable compromise to play both those notes with the same key lever. Bach's drawing on the title-page, then, shows his practical recommendation to establish those appropriately compromised pitches. The student is expected to work by ear with simple step-by-step adjustments, and no mathematical apparatus, towards this everyday skill in keyboard musicianship. Bach's music lays out the problem of playing keyboard music through all possible major or minor scales: all of the installed pitches must fit reasonably into their melodic and harmonic contexts, even when they get renamed through modulation. I believe Bach's drawing then shows how to achieve that, hands-on: the naturals F–C–G–D–A–E (i.e. forming the home base of the C major hexachord, C–D–E–F–G–A) are in their ordinary regular positions, and the remaining six notes are set as careful and specific compromises. The music and the drawing all serve as parts of the evidence towards a reconstruction of Bach's practice(s).

My work is primarily in musical analysis within historical context, as Bach's expertise with intonation was a practical musical skill (not speculative mathematics). I play all of the WTC myself on harpsichord, clavicord and organ, listening closely for the interactions of temperament and scales in their musical context. Lindley's argumentation about my work, in print, has been against a straw-man representation of things I did not say. In November 2008 in this journal, he dismissed my hypothesis as if it presented 'a secret mathematical formula' that Bach 'conveyed cryptically in the decorative loops'. To Lindley, apparently, Bach's drawn loops are merely 'decorative' and serve as no useful evidence to him; and I have allegedly imputed a 'secret mathematical formula' onto Bach, despite my direct point that Bach was not into the dry mathematical stuff.

Lindley's longer article, November 2006, also had similar problems: dismissing evidence, mischaracterizing the thrust of my argument, choosing contemptuous-sounding words such as 'daft' and 'outlandish', misguessing at my 'implicit premises', and trivializing the idea that Bach *could have* expressed a meaningful practical method within that allegedly 'decorative' drawing. I have a full rebuttal to that article on my own website, www.larips.com.

I am grateful that scholars such as Barbieri, Williams and Lindley at least cite the existence of the hypothesis. If there is to be meaningful academic dialogue on this topic, however, I believe it ought to use a valid and fair process of argumentation: study the *entire* piece, engage with its practical and historical observations and represent it accurately.

BRADLEY LEHMAN
Virginia

doi:10.1093/em/cap104

Advanced Access published on November 3, 2009