# *"Hey, Wait a Minute!"*

DMITRI TYMOCZKO

Dave Headlam's review is filled with errors—quotations out of context, elementary mathematical and theoretical mistakes, misleading summaries of my aims, and irrelevant objections.[1] To give a sense of their density, I have devoted an entire appendix to correcting a single Headlam footnote. Here, I will focus on some larger issues.

The first thing to say is that I do not blame Headlam for his various confusions and misunderstandings. My book is difficult, presenting a lot of new material while also proposing new ways to think about familiar facts. This difficulty is perhaps compounded by the relative simplicity of my writing, which can convey an illusion of understanding that in turn encourages people to read too quickly. I have seen this happen to both sympathetic and unsympathetic readers. Indeed, on two occasions I have engaged in correspondence with friendly theorists, young and old, who have breezed through my book only to find themselves misunderstanding fundamental issues—the nature of a path in pitch-class space, the relation between voice leadings and line segments, the difference between voice-leading and common-tone retention, and so on. Frustration is not the appropriate response. It is genuinely hard to communicate and absorb new ideas, particularly when they require us to *unlearn* habits of thought that may have been with us for decades.

Second, Headlam and I are looking at each other across a methodological chasm. Headlam belongs to what I think of (with apologies to Ian Quinn) as the "Age of Gizmos." His assumption (it seems to me) is that theorists are supposed to invent idiosyncratic, personal, and occasionally whimsical structures—perhaps using them to compose, perhaps exploiting them for whatever analytical insights they might yield. There is no particular expectation that these structures be deeply related to the procedures animating large quantities of earlier music.[2] By contrast, my aim is to avoid Gizmos, instead devising theoretical structures that elucidate *the implicit knowledge of previous composers*, and which can therefore be expected to deliver *reliable analytical insights non-coincidentally connected to the process of composition.* (I also, of course, want to use these ideas to create new music that expands on and generalizes the procedures implicit in earlier styles.) Chief among these is the notion of "voice-leading distance," a concept that roughly corresponds to "the aggregate physical effort required to move from one place to another on the keyboard." One can be fairly confident that composers like Bach, Mozart, Wagner, Debussy, and Tatum were sensitive to the way two chords could be more or less "close" on the piano keyboard ("finger distance"), and consequently that a powerful model of voice leading could lead to musical insights.

Headlam is upset that I did not provide a comprehensive history of spatial diagrams in musical analysis. I take this to show that he simply does not understand the difference between my own geometrical models and the more *ad hoc* graphical constructions of figures like George Perle and Walter O'Connell. My topic is the way the composerly notion of "voice-leading distance" induces a surprisingly *non-metaphorical* geometry among musical objects such as chords and scales. ("Geometry" need not refer to physical space.) To my knowledge, the first theorist to broach this topic (at least in print) was not Pythagoras, Anonymous II, Zarlino, O'Connell, Bo Alphonce, or any of the other figures Headlam wishes I had discussed. It was John Roeder, whose classic 1984 dissertation described graphs of voice-leading relationships among *set classes.* To be sure, Roeder's graphs were problematic in various ways, and there is a long and interesting story about how his work led to mine, which (*NB*) largely focuses on *chords* rather than *set classes.*[3] (It is not obvious that Headlam is clear on this difference, or the large amount of thought it took to get from Roeder's dissertation to *A Geometry of Music*, or the way in

1 For instance: Headlam misstates the number of "points" on the chromatic Möbius strip (seventy-eight, not 144); more fundamentally, Headlam does not seem to realize that the space is continuous, and hence contains an infinite number of points; to specify points without specifying a scale is meaningless. Later, Headlam says that three-note chord space has twelve faces; the correct number is one, just as a Möbius strip has one edge. (He may mean "cross sections containing twelve-tone equal-tempered chords.") Headlam wrongly says that my Figure 3.11.3b does not contain minor seventh chords (he may be thinking of a related graph in my Appendix C, cited specifically because of its inadequacy). And as I show in the Appendix, Headlam fundamentally misunderstands the notion of a "path in pitch-class space," one of the central concepts underlying the entire book. (A word of warning: all quotations in this essay are to the original version of Headlam's review. In a previous debate, Headlam revised his various claims in response to my objections. Since it is both frustrating and time-consuming to respond to a fast-moving target, I am limiting myself to a single version of his review [Brown and Headlam {2007}, Tymoczko {2007}].)

2 For additional remarks on methodology, see my response to Richard Taruskin in this volume.

3 Roughly: Callender (2004) replaced Roeder's (1984, 1987, 1994) orthogonal axes with simplicial axes that more accurately model voice-leading distances. Cohn (with help from Quinn) generalized Callender's three-note space to four notes (2003), though he left out the multisets and hence the space's unusual topology (Quinn added multisets in unpublished work). Meanwhile, Cohn had much earlier (1997) realized that the traditional *Tonnetz* could be used to model (something almost like) voice-leading distances between major and minor triads; Douthett and Steinbach (1998) described the *Tonnetz*'s "dual graph" and discovered the first category of graphs discussed in my §3.11 (cited therein). I then discovered the second category in my 2004 article on Debussy. Meanwhile, I developed a scale-based algebraic way to

which the new geometry can transform our understanding of pre-existing graphs.[4]) The major figures investigating the intrinsic geometry of voice leading are Roeder, Richard Cohn, Jack Douthett, Peter Steinbach, Clifton Callender, and Ian Quinn, all of whom I cite.[5] Headlam considers this list a sign of "in house" clubbiness, but as far as I can tell it is an accurate representation of intellectual history. To lump these figures with Anonymous II, Alphonce, and Zarlino, who manifestly were *not* concerned with the intrinsic geometry of voice leading, is to muddy the waters, underselling both the unanimity among the people I do cite and their direct relevance to my project. More importantly, it is to miss the fact that these "voice-leading spaces," rather than being arbitrary gizmos, are alternative representations of the constraints and relationships that partially constitute embodied compositional knowledge.

Third, many of Headlam's bibliographical complaints reflect plain misunderstandings of my text. He falsely asserts that my §2.10 is about "maximally even sets," then wonders why I don't refer to John Clough and Jack Douthett. But the section is actually about *acoustically consonant chords,* which tend to be nearly or fairly even rather than maximally so. I discuss *maximal* evenness most directly in §3.11 and this is where Clough and Douthett are cited. (Norman Carey and David Clampitt are cited in §4.4, where I discuss "generated" scales that are also nearly even.) He wonders why I do not cite Howard Cinnamon in Chapter 7 when I propose my thirds-based grammar of elementary tonal harmony. But Headlam does not seem to realize that Cinnamon is describing *equal divisions of the chromatic octave,* and that he explicitly contrasts this with diatonic harmony. Thus Cinnamon is by his own admission irrelevant to Chapter 7. Instead, the relevant section is §8.7, where I discuss a variety of perspectives on chromatic tonality, citing not Cinnamon but his predecessor Gregory Proctor (whose theoretical ideas are in turn cited prominently in Cinnamon's more analytical work). In much the same vein, Headlam notices that Irene Montefiore Levenson once wrote an article about distant modulations, and wonders why I do not cite her in §7.4. Somehow he does not notice that she spends no time at all discussing the topic of that section, which is quantifying key distance (this is, of course, no criticism of Levenson). Elsewhere, he conflates *statistical studies* of tonal music (which could be concerned with any musical feature whatsoever) with *databases of Roman numeral analyses,* incorrectly asserting that Bret Aarden and Paul T. von Hippel provide the latter. Needless to say, it is surprising to find these sorts of errors in a review that attempts to take me to task for ignoring scholarly accuracy.[6]

Fourth, Headlam is upset by my views about Schenkerian theory. Here we just disagree about the lay of the intellectual land. It is true that I, like David Temperley and many music psychologists, think that Schenkerian theory is as yet untested—and potentially problematic in various ways.[7] (And it is true that I do not personally consider large-scale tonal relations to be central to any perceptually relevant definition of "tonality."[8]) But as

formalize "strongly crossing-free" and "individually T related" voice leadings (2008), providing an alternative perspective on many of these geometrical relations. Inspired by connections between this work and that of Callender, I worked out the geometry of chord-spaces (*not* set classes) in arbitrary dimension (2006), spaces that naturally contained the discrete graphs discussed by Douthett and Steinbach, and which "project" down to set-class space. Finally, Callender, Quinn, and I (with help from Noam Elkies, who also helped me with chord spaces) generalized Callender's "trichord space" to arbitrary dimensions and arbitrary combinations of the "OPTIC symmetries," dealing with the thorny issue of multisets and non-Euclidean geometry, uncovering the singular conical topology of set classes, and writing a lengthy article (2008) unifying numerous voice-leading models—a project that continues to this day. (Note, by the way, that publication dates may not correspond to the dates at which articles were written or circulated.)

4 For instance, John Roeder did pioneering work describing the geometry of set classes, but he did not realize that *line segments* in such spaces correspond to (equivalence classes of individually T-related) *voice leadings.* (Nor did he realize that these spaces have a simplicial coordinate system or a sophisticated topology.) In fact, the connection between voice leading and line segments was unclear even in Callender's 2004 article, which focused on situations in which voices literally glide from one pitch to another. This relationship—as well as the concept of "individual T relatedness," necessary for understanding voice leading between set classes—was clarified in my 2006 and 2008 articles. The resulting geometrical perspective in turn enriched our understanding of pre-existing graphs, including Douthett and Steinbach's "Cube Dance" and my own "scale lattice." (For instance, it was not previously understood that *winding number* represented *scalar transposition*, nor that these graphs depicted all *strongly crossing-free voice leadings* between their chords, nor that graphs had to meet very stringent conditions if all distances were to reflect voice-leading distances, nor even how to formalize voice leading, and hence motion in the graphs, using the notion of "paths in pitch-class space.") Thus though some of these structures appeared in earlier articles, the new geometry allows us to understand them in a much deeper way—as discussed in Appendix C of my book.

5 Joseph Straus is an important figure who may belong on this list as well. In my book I grouped him with figures like Lewin and Morris, who favored a more abstract, algebraic, and "transformational" approach, because the geometry in Straus (2003) derives from or overlaps with earlier work by Callender and Roeder (both cited by Straus). Furthermore, unlike Roeder or Cohn, Straus did not notice that tetrachordal set-classes have an essentially *tetrahedral* structure, instead providing a two-dimensional graph.

Similarly, Straus's 2007 paper (listed in the bibliography as Straus [2005], due to delays at the *Journal of Music Theory*) was written at the same time as Callender, Quinn, and I were working on our more general approach (which is cited by Straus). Thus I think of Straus as pursuing an approach that is somewhat closer to the more algebraic work of Lewin and Morris, though certainly touching on geometry. One may of course disagree with my take on history, but this is a matter of judgment.

6 Though sum-classes appear in both my work and George Perle's, they play completely different roles. As far as I can tell, there is no deep or even significant conceptual relationship here. O'Connell's case is more delicate, as his tone lattice (not six-, but three-dimensional, *pace* Headlam) is graph-theoretically identical to the six-note *Tonnetz* (see Tymoczko [2012]). However, neither I nor anyone else was aware of this subtle connection at the time of Headlam's review. On the surface, O'Connell's work seems completely unrelated to mine.

7 Temperley (2011). In my experience, most experimental music psychologists—that is, most people who regularly design, perform, and publish experiments—are keenly aware that Schenkerian theory is potentially in trouble. My views on this subject are heavily indebted to conversations with David Huron and Philip Johnson-Laird.

8 This is, first, because I am convinced by the experiments of Nicholas Cook (1987) and Elizabeth Marvin and Alexander Brinkman (1999), and second, because pieces like "Giant Steps" (Coltrane) and "Sweet Child O'

I say in Chapter 7, everything in my book is fully consistent with a certain strand of "compatibilist" Schenkerianism. Crucially, *almost all contemporary Schenkerians and quasi-Schenkerians, including Matthew Brown, William Rothstein, and Fred Lerdahl, subscribe to compatibilism of this sort.*[9] But while everyone seems to agree with me that there is *some* autonomous harmonic grammar like that in my §7.1, the literature contains hardly any attempts to describe this grammar in detail. This is an extraordinary situation, with scholars paying lip service to a theory they refuse to specify. In my book I chose instead to confront the fascinating local logic animating elementary diatonic harmony, chromaticism, classical modulation, and contemporary scalar procedures. Why should it be offensive that someone would focus on local coherence, particularly if they have a lot to say?

One possibility is that Headlam has been habituated into unconscious contempt for the very questions that motivate me—questions like "Why are some progressions common, and others absent, in standard functional harmony?" For instance, he suggests, as if it were perfectly simple, that ii–IV is absent because of the preference for $\hat{2}-\hat{1}$ motion at cadences. But of course, it is not just ii–IV that is rare, it is *every* ascending-third progression. And if Headlam's story were true, we should expect to find plenty of noncadential ii–IV progressions (e.g., phrase-initial I–ii–IV–I$^6$), as well as cadential ii–IV progressions embellishing $\hat{2}-\hat{1}$ melodies (e.g., I–ii–IV–V–I under $\hat{3}-\hat{2}-\hat{1}-\hat{7}-\hat{1}$). Neither of these is at all common in the classical period, as Headlam would have realized if he had thought about it for even a minute. That he did not bother to think betrays an astonishing lack of interest in the detailed local structure of music—precisely what motivates me to look beyond orthodox Schenkerianism.

Fifth, there is the delicate topic of atonality, likely a major cause of Headlam's animus. It is true that I argued that (a) atonal music is generally less popular than tonal music; (b) the structural principles involved in creating atonal music are less robust, perceptually, than are my five features;[10] (c) listeners often need to acquire a taste for music that abandons the five features; (d) atonal music is unpopular not simply because of the *presence* of novel structural relations among intervals or pitches but also because of the *absence* of the five features; and (e) many atonal organizational techniques—including serialism, set theory, complementarity, and so on—can be adapted to musical styles that exemplify the five features. I would hope that the first two points are uncontroversial (certainly, Milton Babbitt believed them). The third seems relatively commonsensical, while the fourth seems to follow from the third. (The fifth is again straightforward, suggesting that abrasiveness may be *part of the aesthetic aim* in at least some atonal styles.) Headlam, who appears not to understand this argument, refuses to identify where he disagrees, instead pointing out that many atonal composers shifted their attention from pitches to intervals. This is simply a non sequitur.

Personally, I do not think that atonal aficionados should feel threatened by my ideas. (I take this opportunity to remind readers that I actually like and have composed atonal music, and that my own sense of "tonal" overlaps to quite a significant degree with the lay listener's "atonal.") Atonality is a minority taste, but that is no reason to get defensive, or to attack people who dare to point this out. Plenty of things are minority tastes, including smelly cheese, expensive wine, and, yes, clam-chowder ice cream. Indeed, I suspect newcomers would be more attracted to this music if we did more to emphasize its obscure, threatening, deliriously irrational, proto-punk qualities. (I have one friend, an eminent philosophical linguist who studied twelve-tone composition with a young Steve Reich [!], who describes Schoenberg's music as "wonderfully perverse.") We are selling this music short if we pretend that it is just as popular, just as accessible, or just as unthreatening as classical music. Can we instead try to remember the *Unsinn*, the *Weltschmerz*, the *Angst* of a world stunned by scientific revolution, transformed by technology, and torn apart by endless war?

So why does my work seem to engender such powerful reactions among the previous generation? One possibility is simply that it is new, and that the anger cloaks the painful and unfamiliar sensation of confusion. Related to this is the possibility that my musical and intellectual values—which de-emphasize Schenker, Schoenberg, and Forte in favor of Reich, the Beatles, Coltrane, Huron, and Cohn—are inherently threatening to those who grew up in a different world. Still another possibility is that people are responding to the youthful polemicist I once was, rather than the mellower, middle-aged parent that I have become. (Headlam is now the second reviewer who simply disbelieves my repeated claims to like [some] atonal music, assuming that these are lies designed to cloak an unbridled hostility to an entire genre.[11]) But perhaps the most frightening prospect is that music scholarship has

Mine" (Guns N' Roses) seem importantly similar to other tonal pieces, despite their unusual modulatory schemes. One can of course *define* "tonal" as "beginning and ending in the same key" (or "having such-and-such a modulatory plan" or even "being amenable to Schenkerian analysis as I practice it") but this is largely a stipulative or terminological definition with few real consequences. As I say in §1.2, very little hinges on how we use the word "tonality."

9 Rothstein (1992). At page 42 in his *Explaining Tonality*, Matthew Brown notes that the progression I–ii–I$^6$ is disallowed by the principles of functional harmony, yet he never attempts to specify those principles, which seem central to his project of elucidating "functional monotonality." Similarly, in private emails Fred Lerdahl has insisted that he believes in a harmonic grammar of the sort offered in my §7.1. Mastering this grammar is surely a central component of developing listeners' tonal competence. (Indeed, we explicitly teach it in schools!) Yet despite having written *two enormous books* attempting to model "listener competence," Lerdahl never once tries to specify this harmonic grammar, nor even acknowledges that it exists. Worse yet, he makes some very misleading statements, discussed in my book, seeming to imply that these harmonic rules can be entirely derived from principles of melodic attraction.

10 Headlam clearly misses the point of Figure 1.2.2, which is that atonal organizational methods are by themselves much less powerful than tonal organizational methods—a point that Babbitt acknowledged, and that we can certainly investigate in the context of random sequences. I explain this quite clearly in my book.

11 If anything, my current worry is that atonal music and complex tonal music are in the same boat. The big argument, these days, is not between tonal

acquired a partisan or "studio" mentality, wherein it is assumed that we are supposed to fight for the beliefs of our teachers. The goal, evidently, is not to think through problems using reason and musicality, citing those articles that are directly relevant to our work, but rather to think *by way of commentary on earlier thinkers.* This is scholasticism in a nutshell, and I hear its discomfiting echoes in Headlam's outrage that I dare disagree with Milton Babbitt.

It is sometimes said that intellectual progress occurs by "cohort replacement" rather than genuine conversion.[12] I have always hated this idea, believing that change is endlessly possible, no matter how old we are or how set in our ways (this may sound naïve, but I am proud of it). The corollary is that we should never stop trying to explain what we mean, or engaging in dialogue with those who disagree. I fully believe that the geometrical perspective provides powerful new tools for grappling with a very wide range of music—tonal and atonal, old and new, popular and esoteric. I encourage readers to approach my book in an open and joyful frame of mind.

## APPENDIX

### RESPONSE TO NOTE 30

I will now go through a single footnote line by line, just to give a sense of how thoroughly Headlam has misunderstood me. I am going to quote every word of his Note, interspersing my own responses. I could (in principle) provide analogous commentary on many other paragraphs in the review.

DH: *For instance, Chapter 1 begins with some larger questions concerning the term "tonal," including the assertion "Faced with these questions, contemporary music theory stares at its feet in awkward silence" (3). However, even a glance at Bryan Hyer's excellent article on tonality in* The New Grove Dictionary *reveals that Tymoczko's presentation style invents a straw man.*

DT: Hyer's article (which I cite) is chiefly concerned with the various uses of the *word* "tonality"; it is not an attempt to adjudicate between them, or to specify the musical properties that give rise to any one particular sense of "tonality." This latter is my project. Hyer and I actually debated this very point on a panel at a conference in London, agreeing that we were answering completely different questions. He has made "the linguistic turn" where I have not.

DH: *Despite the lack of references, Tymoczko does have some "interesting" notes. In Note 4 (5), he writes that David Wessel apparently correlates the spectral centroid with the perception of pitch; this reference follows the assertion in the text that the eardrum is one-dimensional. The eardrum is referred to as an "area" in acoustics literature, so is 2D, and Wessel, whose article concerns "timbre space," correlates the spectral centroid with timbre (as many other writers have), not pitch.*

DT: What I actually wrote was "the eardrum is, in effect, a one-dimensional system that can only move back and forth," with the words "in effect" and "system" registering that I am adopting an approximate and functional perspective. The real truth is that the eardrum, like all physical objects, is three-dimensional. At some levels of approximation it is useful to treat it as a two-dimensional surface. At a coarser grain, it is useful to treat it as one-dimensional—a system whose position corresponds with air pressure, a scalar quantity. (In technical terms, its configuration space is one-dimensional.) My meaning here would be clear to any reader with even a modicum of interpretational charity, and it baffles me that two grown-ups could find themselves debating this point in a serious journal. Should I also mention the metonymy according to which "the eardrum" is being used to refer to a collection of closely related anatomical structures working in concert?

As for my teacher David Wessel, my sentence refers to the ear's tendency to "group events together that are nearby in pitch." My Note then cites Wessel as suggesting that the variable relevant to auditory streaming is not pitch but spectral centroid. This is indeed what Wessel did: he demonstrated that one and the same sequence of notes can be heard as single melody, or as multiple melodies (separate auditory streams) depending on differences in timbre, and specifically on the spectral centroid.[13] Wessel thinks we misattribute streaming to pitch rather than centroid because for many musical instruments, centroid rises with rising pitch. This is roughly what it means to say that the variables are "correlated."[14]

DH: *Note 20 (15) reports that Tymoczko, in the first person, uses the term "C major" where the white notes imply a tonal center on C, and again "I call this 'the I symmetry'" (33, Note 7) where pitch-class inversion is involved; he might add "I and everyone else."*

DT: In the first Note, I draw a distinction between the "C diatonic" collection and the "C major" collection; this is a terminological fine point, hardly universally observed, and in any case worth pointing out to readers. In the second Note I am pointing out that people use the term "inversion" *to refer to two totally unrelated transformations*, announcing that I will henceforth avoid ambiguity by distinguishing "the O symmetry" from "the I symmetry." As far as I know, this terminology derives from my 2008 article with Callender and Quinn. In both cases, Headlam takes passages where I make a perfectly reasonable terminological distinction ("I say X in this circumstance and Y in that one"), quoting only the second half of the distinction to make me look silly, or to make it seem as if I am somehow taking credit for a common term, rather than just explaining how I am using language in my book. Hard as it is for me to believe, Headlam must either be deliberately distorting what I say, or having difficulty parsing the logic of relatively simple sentences.

and atonal, but between notated and non-notated, or perhaps complex and simple. This is why I conceived of my book as a brief in favor of a certain kind of sophisticated tonality.

12 See Kuhn (1996, 150ff.), which quotes Max Planck's famous remark: "A new scientific truth does not triumph by convincing its opponents and making them see the light, but rather because its opponents eventually die, and a new generation grows up that is familiar with it."

13 See Figure 2 and the preceding discussion in Wessel (1979).

14 In fact, spectral centroid is correlated with *both* pitch and timbre: holding pitch fixed, it correlates with "brightness," while holding instrument fixed it correlates with pitch. There is absolutely no reason to think it should correlate with just one of these variables.

DH: *Notes 18–20 on page 42 cover a lot of ground, but the assertion that previous authors conflate what Rahn (1980) called "directed pitch-class intervals" 1–11 with interval-classes 1–6 is not the case.*

DT: Headlam here shows that he totally misunderstands the notion of "a path in pitch-class space," a point central to my book, and indeed to many of my published articles.

$C_4 \rightarrow C\sharp_4$ and $C_4 \rightarrow C\sharp_3$ are both instances of "[directed] pitch class interval 1" in traditional set theory. By contrast, $C_4 \rightarrow B_3$ is directed interval 11. To confuse directed pitch class interval and interval class would be to confuse $C_4 \rightarrow C\sharp_4$ and $C_4 \rightarrow B_3$ (both interval class 1). This is not my complaint. As I very clearly explain, $C_4 \rightarrow C\sharp_4$ and $C_4 \rightarrow C\sharp_3$ are different paths, represented by the numbers 1 and −11. Headlam thinks I am complaining about the conflation of the numbers 1 and 11 (representing C→C♯ and C→B); in fact I am complaining about the conflation of 1 and −11 (representing different ways of getting from C to C♯; note the all-important minus sign). Someone who misunderstands this point cannot possibly understand what a voice leading is, since voice leadings (as I define them) are collections of paths in pitch-class space. This idea is in turn central to my entire book, and crucial to my differences with earlier theorists such as Alphonce, Morris, and even Roeder (once again a complaint about citations likely derives from a failure to understand what I am saying). I have a hard time understanding how any reader could misunderstand me so thoroughly here, given how much time I spend on the distinction.

DH: *On page 16 Tymoczko reports, in an understatement, that the presence of ficta "may complicate matters somewhat" in regard to finding diatonic scales, with no reference to the literature.*

DT: What I actually say is that "the earliest Western music explored the tonal centers contained within a relatively static and largely diatonic macroharmony," making no mention of the problem of "finding diatonic scales." I then note that the presence of ficta complicates matters somewhat. This seems totally unobjectionable. First, well-known facts do not require footnotes, and the existence of musica ficta is certainly well known. (Its extent is the subject of academic debate, but *any* degree of ficta constitutes a complication of the sort I mention.) Second, there is widespread agreement that, ficta notwithstanding, most medieval and Renaissance music remains *largely* diatonic—as attested by virtually every contemporary recording and critical edition. Does Headlam really want to challenge this? Is it not enough that my statement accurately describes a huge proportion of the pieces collected in standard historical anthologies?[15]

DH: *On page 47, Note 24, transposition is defined as addition and inversion as subtraction from a constant, both well-known and long existing concepts, with the only reference being to "Tymoczko 2008b."*

DT: Headlam wrongly suggests that my Note discusses transposition and inversion of pitch classes. The Note explicitly and unambiguously refers to something more complicated: *pairs of voice leadings* being *individually transpositionally or inversionally related.* These technical terms originate in the cited 2008 article, and are by no means standard. Once again, Headlam thoroughly misunderstands my writing.

Let me close by mentioning a few other problems. Headlam suggests that "a more typical mathematical procedure might be to prove the impossibility of non-efficient voice leadings for near-even collections"; this is unworkable because *any* pair of chords can be linked by *arbitrarily inefficient* voice leadings, as when C major moves to itself by a two-octave motion in every voice (Headlam probably misunderstands this because he doesn't understand paths in pitch class space). At two points Headlam falsely suggests that Anonymous II realized that consonant sonorities were, in comparison to all other sonorities, optimal from a voice-leading standpoint. Headlam tries to establish my clubbiness by the bizarre stratagem of counting bibliographic entries for each author, without regard to the works' size or the number of textual references. (Fred Lerdahl, for example, is the second-most cited theorist in the book, yet he only gets two bibliographical entries, since most of my references are to his monumental *Tonal Pitch Space.*) Even then, however, Headlam neglects to mention that I cite five separate works by Allen Forte, five by Schenker, and four by Taruskin, putting them fourth through sixth in the standings. Later, Headlam argues that my own statistics refute my thirds-based grammar, ignoring the standard distinction (cited earlier in his review!) between *grammatical* and *probable.* Finally, Headlam complains that the term "Neo-Riemannian theory" does not appear in my index, even though the term is so deeply ambiguous—comprising a grab-bag of dualist, transformational, harmonic, and contrapuntal ideas—that it has recently been abandoned by Richard Cohn. And so on.

15 Interestingly, there once *was* a citation here, which my copyeditor suggested removing—demonstrating that one man's pedantry is another's scholarly propriety.

## WORKS CITED


Brown, Matthew. 2005. *Explaining Tonality.* Rochester: University of Rochester Press.

Brown, Matthew, and Dave Headlam. 2007. "Comment on 'The Geometry of Musical Chords.'" *Science* 315: 330.

Callender, Clifton. 2004. "Continuous Transformations." *Music Theory Online* 10 (3). http://www.mtosmt.org/issues/mto.04.10.3/mto.04.10.3.callender.pdf (accessed 11 November 2011).

Callender, Clifton, Ian Quinn, and Dmitri Tymoczko. 2008. "Generalized Voice Leading Spaces." *Science* 320: 346–48.

Cohn, Richard. 1997. "Neo-Riemannian Operations, Parsimonious Trichords, and their 'Tonnetz' Representations." *Journal of Music Theory* 41 (1): 1–66.

———. 2003. "A Tetrahedral Graph of Tetrachordal Voice-Leading Space." *Music Theory Online* 9 (4). http://www.mtosmt.org/issues/mto.03.9.4/mto.03.9.4.cohn_frames.html (accessed 12 December 2011).

Cook, Nicholas. 1987. "The Perception of Large-Scale Tonal Closure." *Music Perception* 5 (2): 197–205.

Douthett, Jack, and Peter Steinbach. 1998. "Parsimonious Graphs: A Study in Parsimony, Contextual Transformations, and Modes of Limited Transposition." *Journal of Music Theory* 42 (2): 241–63.

Headlam, David. 1996. *The Music of Alban Berg.* New Haven: Yale University Press.

Hook, Julian. Review of *A Geometry of Music. Music Theory Online* 17 (3). http://www.mtosmt.org/issues/mto.11.17.3/mto.11.17.3.hook.html (accessed 11 November 2011).

Hyer, Brian. 2002. "Tonality." In *The Cambridge History of Western Music Theory.* Ed. Thomas Christensen. 726–52. New York: Cambridge University Press.

Kuhn, Thomas. [1962] 1996. *The Structure of Scientific Revolutions.* Chicago: The University of Chicago Press.

Lerdahl, Fred. 2001. *Tonal Pitch Space.* New York: Oxford University Press.

Marvin, Elizabeth West, and Alexander Brinkman. 1999. "The Effect of Modulation and Formal Manipulation on Perception of Tonic Closure by Expert Listeners." *Music Perception* 16 (4): 389–408.

O'Connell, Walter. [1962] 1968. "Tone Spaces." Rev. English ed. *Die Reihe* 8: 34–67.

Roeder, John. 1984. "A Theory of Voice Leading for Atonal Music." Ph.D. diss., Yale University.

———. 1987. "A Geometric Representation of Pitch-Class Series." *Perspectives of New Music* 25 (1–2): 362–409.

———. 1994. "Voice-leading as Transformation." In *Musical Transformation and Musical Intuition: Eleven Essays in Honor of David Lewin.* Ed. Raphael Atlas and Michael Cherlin. 41–58. Roxbury [MA]: Ovenbird.

Rothstein, William. 1992. "The True Principles of Harmony: Or, Schulz, Schenker, and the *Stufe.*" Paper presented at the Second International Schenker Symposium, 27–29 March, New York.

Straus, Joseph. 2003. "Uniformity, Balance, and Smoothness in Atonal Voice Leading." *Music Theory Spectrum* 25 (2): 305–52.

———. 2005. "Voice Leading in Set Class Space." *Journal of Music Theory* 49 (1): 45–108.

Temperley, David. 2011. "Composition, Perception, and Schenkerian Theory." *Music Theory Spectrum* 33 (2): 146–68.

Tymoczko, Dmitri. 2004. "Scale Networks and Debussy." *Journal of Music Theory* 48 (2): 219–94.

———. 2006. "The Geometry of Musical Chords." *Science* 313: 72–74.

———. 2007. "Response to Comment on 'The Geometry of Musical Chords.'" *Science* 315: 330.

———. 2008. "Scale Theory, Serial Theory, and Voice Leading." *Music Analysis* 27 (1): 1–49.

———. 2012. "The Generalized *Tonnetz.*" *Journal of Music Theory* 56 (1): 1–52.

Wessel, David. 1979. "Timbre Space as a Musical Control Structure." *Computer Music Journal* 3 (2): 45–52.

*Music Theory Spectrum,* Vol. 34, Issue 1, pp. 144–49, ISSN 0195-6167, electronic ISSN 1533-8339.  DOI: 10.1525/mts.2012.34.1.144