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Are There Any Bad (or Good) Transformational Analyses?

by Michael Buchler

Abstract. This article considers two related issues: how we envision methodology and evaluate success in transformational approaches to analysis. Inasmuch as methodology drives analysis, as Rings (2006) suggests, we might regard the transformational toolbox as collectively comprising a less robust analytical methodology than does, for example, Lerdahl and Jackendoff's *A Generative Theory of Tonal Music* (1983). Indeed, to use *GTTM* terms, we might say that transformational methods offer only well-formedness, not preference, rules. But when a method lacks preference rules, how can its applications be evaluated? If all well-formed analyses are equally good—or at least valid—then criticism (which is routinely considered integral to our field) becomes impossible and this article's title question becomes pertinent.

Keywords and phrases: Transformation, analysis, criticism, Klumpenhouwer, Lewin.

Introduction

Let's assume that the answer to the title question is emphatically "yes!" Certainly no one would want to say that there aren't good examples of *any* particular methodology. And if there are good examples, there surely must be bad (or less good?) examples—unless one imagines that transformational analysis, or any other way of going about analysis, offers a fail-safe system for generating brilliant musical insights. If answering the title question here in the opening paragraph seems a bit too easy, rest assured that a majority of this essay investigates two important follow-up questions: "How do we know?" and "How can we tell a bad transformational analysis from a good one?"[1]

One might reasonably wonder why I am focusing particularly on transformational analysis. In part, it was at the implicit invitation of Henry Klumpenhouwer, who, in his published response to my 2007 article "Reconsidering Klumpenhouwer Networks," pointed out that "some of the problems Buchler has with K-nets are at root problems with the analytical philosophy K-nets seem to embody, but are by no means limited to that approach alone."[2] Indeed, while my objections to the way recursive structures conflate pitches (or pitch classes) and transformations were innate to K-nets, my comments about K-net audition, pitch-class-based inversion, and transformational structuring were not. Those broader arguments might well be leveled against all manner of transformational analysis that operates in pitch-class space (and that's surely a large majority of it).

Klumpenhouwer's response also led me to contemplate the epistemology of transformational analysis and also of Fortean (Cartesian) set-theoretical analysis and its associated tools, and I do believe that much of this essay

[1] Admittedly, "good" and "bad" are inherently polemical terms that are easy to problematize, but they will be useful in ways that will become obvious later in this essay.

[2] Klumpenhouwer (2007, §2).

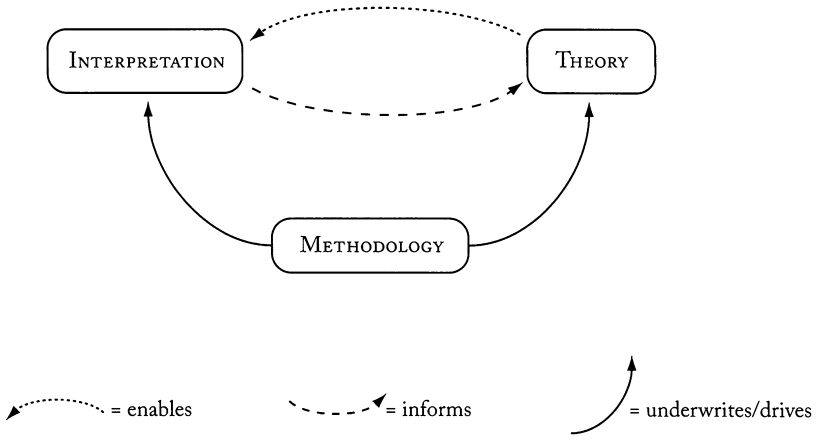


Figure 1. Rings's network representation of Lewin's "project" (Rings 2006, 115).

applies equally to both analytical perspectives. I am not, however, especially interested in engaging in a criticism of either transformational or Cartesian ways of theorizing. Both approaches have led us to new and deeply informative ways of navigating our musical universes and more ways of imagining compositional spaces. In short, this is not an essay about abstract theorizing; it is about how we apply these approaches to music analysis and how we engage those analyses as a community of scholars. At the risk of sounding defensive, it bears repeating that I have no interest in either limiting analysis or prescribing proper (and therefore proscribing improper) ways of performing analysis.

1. Analysis and Methodology

Music analysis is often portrayed as inherently interpretive—a creative venture as much as (or more than) a search for some larger Truth. One can find agreement from writers as diverse as David Lewin, Joseph Kerman, and Marion Guck that when we perform an analysis, we are interpreting or "reading" a musical work. It also seems relatively uncontroversial that we music theorists generally try to analyze music in ways that conform to some set of methodological expectations. "Methodology" is a word that Lewin and Klumpenhouwer (among many others) have used when discussing analytical technique and it plays prominently in Steven Rings's (2006) essay on (*Journal of Music Theory* called it a review of) David Lewin's three books.[3] Rings cleverly encapsulated Lewin's analytical and theoretical philosophy by constructing a kind of meta-transformational network of Lewin's so-called "project." His network is reproduced in Figure 1. In beautifully Lewinian fashion, Rings's network is simple, immediately apprehensible, and yet it seems deeply evocative, inviting us to find applicability not explicitly drawn out in the accompanying prose and challenging us to ask questions about the nature and spirit of transformational theory and analysis (note that Rings uses "interpretation" as a broadly humanistic term that includes "analysis").

I was initially quite taken by this network, but a footnote that appeared four pages later caught me by surprise and led me to reassess my understanding of what it represented.

> The polemical upheaval such abstraction can cause was recently on display in *Music Theory Online* 13/3 (September 2007), as various writers responded to Michael Buchler's (2007) critique of Klumpenhouwer networks. Tacitly at work in many of the responses was an effort to articulate the methodological principles that seemed violated by Buchler's proposal. The responses to Buchler from Henry Klumpenhouwer (2007) and Shaugn O'Donnell (2007) can be read as the most explicit attempts to "reattach the Methodology node" of Figure 1 [Figure 1 of this essay] to the questions of theory and interpretation circulating in the discussion.[4]

If Shaugn O'Donnell and Henry Klumpenhouwer were trying to reattach that Methodology node, had I somehow detached it? In this footnote, Rings was surely referring to Klumpenhouwer's and O'Donnell's assertions that I had corrupted the ontological nature of K-nets by reshaping them into dual transformations. In essence, that was my simpler representation of the same musical information that

[3] E.g., at the beginning of the second chapter of *Musical Form and Transformation*, Lewin said that his analysis of Stockhausen's *Klavierstück III* "is intended partly as a methodological model" (Lewin 1993, 16).

[4] Rings (2006, 119).

K-nets model.[5] To my mind, while I had certainly challenged the overall utility and practicality of K-nets in analysis and shown that one could arrive at K-nets' "hyper" transformations through simpler means, I didn't think that I had broadly challenged the notion that there *is* a methodology at play.

But, while reading Rings's essay, it dawned on me that I truly might not know what counts as "methodology" in this context. More specifically, I found (and still find) myself wondering what kinds of things one can find in a methodology node and how it is that this "methodology" thing underwrites or drives both theory and especially analysis. My sense is that our expectations for analytical methodology differ widely depending both upon what sorts of music we examine and what technique(s) we use. In common-practice tonal music and also in popular music that follows some harmonic syntax, one's methodology might well inform how one labels harmonies, whether and how one accounts for voice-leading patterns, and the ways in which one applies any pre-existent schemata that highlight some normative tonal or formal structure.

Whatever one might think about its applicability, practicality, or underlying philosophical basis, we could probably all agree that Lerdahl and Jackendoff's (1983) *A Generative Theory of Tonal Music* ("*GTTM*") amounts to one of the most robust analytical methodologies of our time. In particular, Lerdahl and Jackendoff's distinction between preference rules and well-formedness rules simultaneously establishes clear expectations for what an analysis should look like, what goals it should aspire to attain, and what results can be regarded as normative or special in a rather wide body of music.

If the "rules" for Schenkerian analysis are less specifically articulated in treatises (both by Schenker and others), there are still many norms that can nevertheless be divined through a study of Schenkerian analysis as practiced in the pages of our journals, textbooks, and conference handouts. A textbook such as Cadwallader and Gagné's (2007) *Analysis of Tonal Music* surely lays out far more preference rules than well-formedness rules, but both types are there in spirit, if not in name. "Don't cross slurs" or "don't invent new *Ursatz* forms" might be considered Schenkerian well-formedness rules; "only include inner voices when they provide important information" or "don't connect instances of î from cadential six-four chords to tonic chords" might be preference rules.[6]

Both Schenker and Lerdahl and Jackendoff offered us some basic tools for formatting and communicating an analysis, some expectations of analytical outcome, and a general sense of a correct and incorrect manner of application—all of which might be understood as not only informing a methodology for tonal music analysis, but also providing a means for analytical criticism. What, then, are the materials, tools, and goals of transformational analysis? Transformational analysis of one sort or another seems to have supplanted or subsumed pitch-class set theory (or set-complex theory) as the predominant methodology currently applied to non-tonal works and it has become increasingly prominent in analyses of chromatic tonal music (e.g., Rings 2007, 2011). But what advice does it (or what advice do its practitioners) offer us about how we can or should either apply it or critique its products?

Some might claim that merely asking these questions suggests a narrow-mindedness that is destructive to the analytical enterprise. I can imagine readers asking: "If we have an outcome in mind from the get-go, why bother with the analysis?"; "Isn't it better not to be bound by our methodologies?"; and other such questions. I won't argue that the analysis of atonal music should or can be either as all-encompassing or as reliant on normative syntax as is tonal analysis. (Indeed, Lewin conceded something akin to that point in his second book, *Musical Form and Transformation*.[7]) But, I will argue that the *kind* of methodology that governs most (particularly non-transformational) tonal analysis is fundamentally different from the *kind* of methodology that governs transformational analysis. Specifically, it seems clear (especially after reading and re-reading the responses to me by Klumpenhouwer, Nolan, and O'Donnell) that inasmuch as Lewin, Forte, (John) Rahn, Cohn, or Klumpenhouwer (among many others) defined a method for atonal analysis, it is one that contains (at least primarily) tools, not guidelines for their application.[8] In other words (in *GTTM* words): the methodologies governing transformational analysis appear to consist only of well-formedness rules, not preference rules. That said, it is worth noting that John Roeder (2009) proposed a very coherent set of eight preference rules that blend a desire for salient segmentation with a desire to show motivic self-similarity by focusing only on a single family of objects to be transformed.

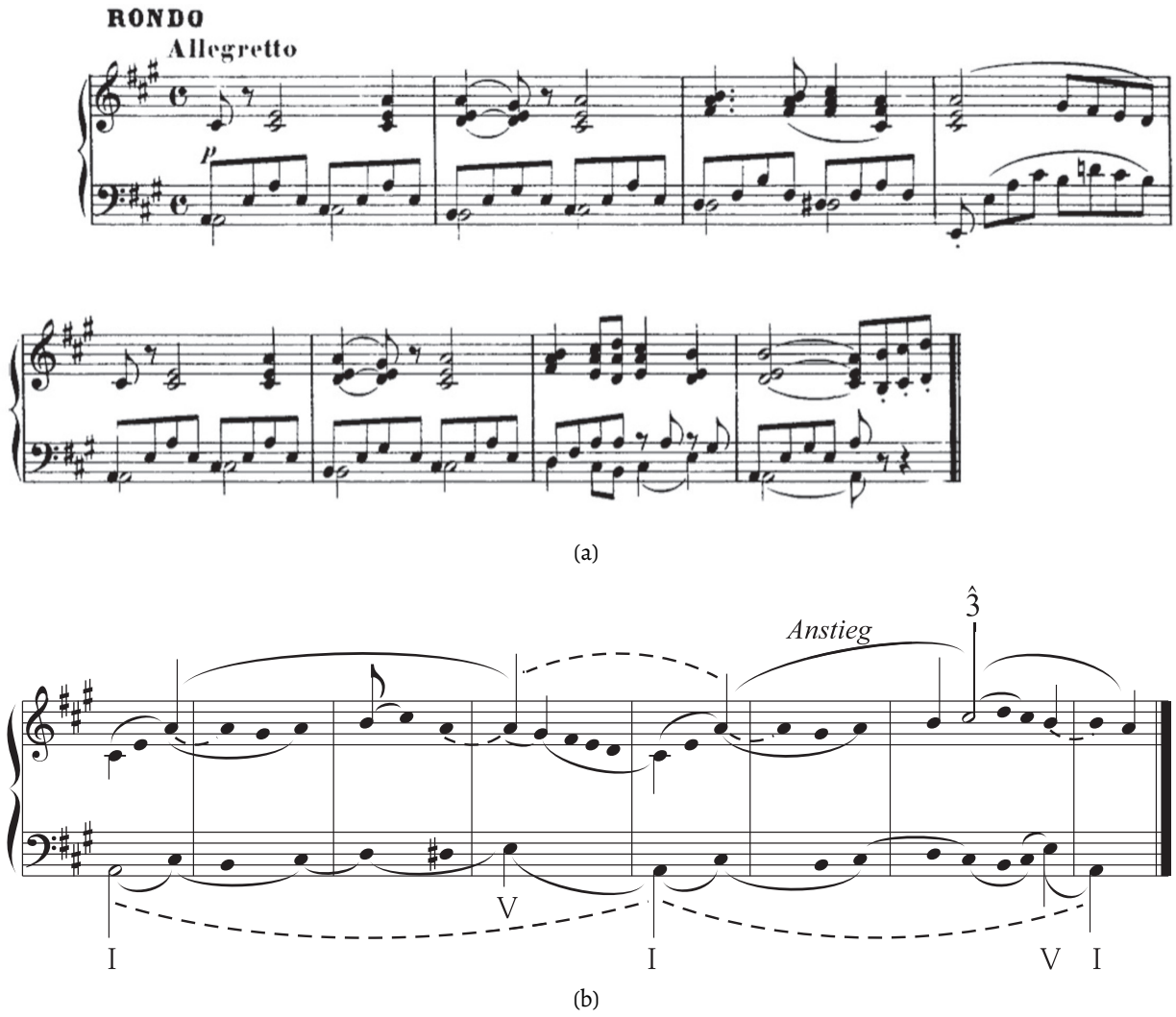
Among Roeder's preferences are that we "choose the most aurally salient analytical objects that will still belong

[5] Buchler (2007, §§5–31).

[6] I could imagine people arguing that the last rule seems more formative than preferential.

[7] In referring to some of his own "dissatisfactions" he had with his own analysis of Stockhausen's *Klavierstück III*, Lewin remarked that "if we demand that all music that we examine be on the aesthetic level of the great tonal masterworks, and that all the theoretical equipment we invoke be at the level of sophistication and power that tonal theory has achieved after two and a half centuries of intense development, we will not get very far in coming to terms with the music of our recent past" (Lewin 1993, 44).

[8] In 2009, John Roeder more bluntly claimed that Lewin never articulated a transformational methodology for analysis (§1.3.).

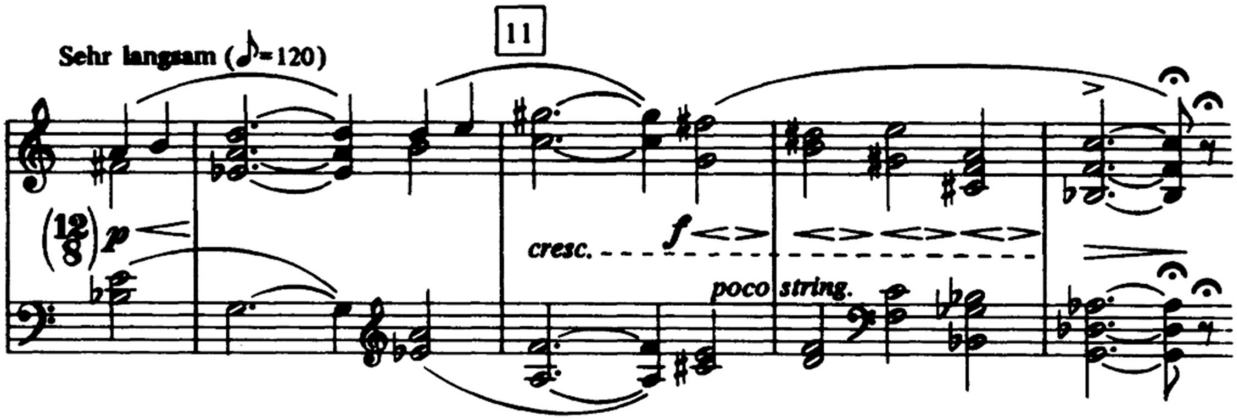


Example 1. Schubert, Piano Sonata, D. 959, mm. 1–8: (a) Score and (b) a (terrible) Schenkerian analysis.

to a single family and have an economical transformational structure," "choose transformations that may be applied to other families of objects in the same composition," "choose an object family that is complete (including all objects that appear in the piece) but minimal (not entailing many objects that do not appear)," and "choose transformations that occur prominently and repeatedly" (2009, §12.1). I find Roeder's guidelines to be helpful, but I imagine that abiding by them faithfully would restrict one to a relatively small canon of post-tonal compositions (or compositional excerpts). Certainly, most of David Lewin's own atonal analyses adhered to only some of these rules.

My previous observation about well-formedness versus preference rules leads to the central point of this essay and, indeed, the essay's title: if there are no commonly accepted preference rules, can analyses be considered unsuccessful if they adhere to all basic well-formedness rules? As I said at the outset, I believe that they can, but I also believe that our evaluations should be founded on both intersubjective notions of musicality as well as on well-formedness criteria, and that is something that we have been reluctant to do. I recognize that some readers might wonder why we have to evaluate at all. After all, there are no uniform standards and goals for analysis and we are all (thank goodness!) captivated by different musical features and swayed by different sorts of arguments. But if you are on an editorial board or a conference program committee or are asked to write a recommendation letter or a review for a tenure or promotion case, then it's essential that you feel equipped to evaluate analyses that are placed before you. This is certainly not about finding Truth; it's about working within a critical discipline.

It is easy to envision a failed Schenkerian analysis that nevertheless adheres to every notational convention



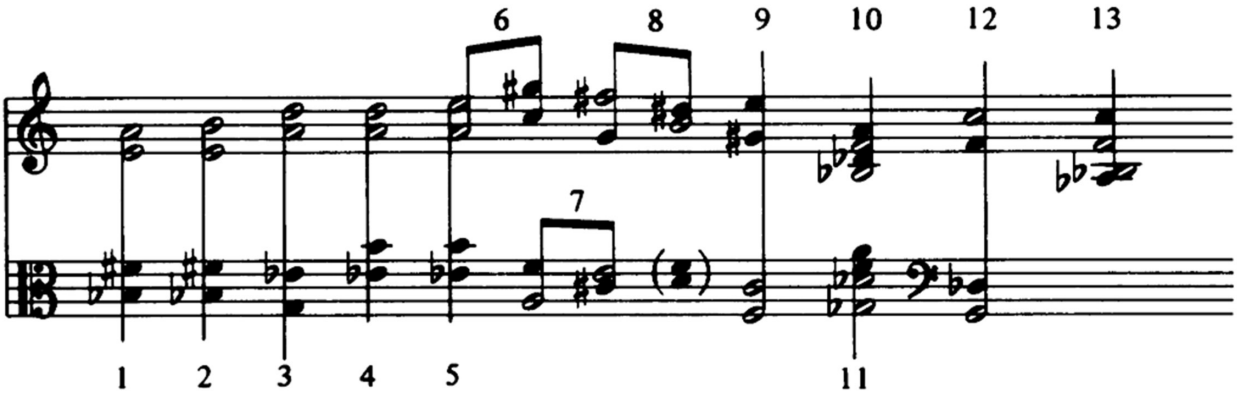
Example 2. Example 1 from Lewin (1994): The chorale-like segment from Schoenberg's Op. 11, No. 2 (mm. 9–13).

and features no internal conflicts. Example 1 shows the first eight bars of the last movement from Schubert's late A-major piano sonata, D. 959, and a less-than-brilliant Schenkerian analysis of the passage. Consider the musical interpretations that this analysis entails. I feel certain that a vast majority of *Intégral* readers could produce a litany of ways in which this is a terrible analysis. I've even used Example 1 in my own Schenkerian analysis class as an error-detection exercise. It misreads harmony and phrase structure, it shows no regard for meter, it shows a prolongation of î at the cadential six-four in the first phrase, the alleged head tone is actually ornamental, and there are considerably more musical problems. On the other hand, there are also no conflicting slurs, no orphaned tones, and at first glance—if one did not recognize the specific musical passage analyzed—I imagine that it could appear to be a reasonable analysis. But the point here is that it *isn't* a reasonable analysis and we all know that. And any program committee or editorial board would (I hope) rightly reject a paper that featured this as anything other than a negative example. It badly violates our collective musical principles and in ways that we can readily describe without relying on that old pronouncement, "I just don't hear it that way."

I wonder how often committees of experts claim that a well-formed transformational analysis has failed on similar grounds. From both personal experience (serving on such committees) and the sort of "word on the street" that one hears from trusted colleagues, I doubt that it happens very often. David Lewin is often lauded as one of the best and most sensitive musical analysts of our time. I like and admire a tremendous amount of his work, but I also think that some of his (and many other scholars') post-tonal analyses have simply escaped critical scrutiny on anything other than structural grounds. A particularly impressive theory node, in other words, sometimes appears to blind us to the interpretation node. Or, perhaps more befitting Rings's grand network: the arrow from the interpretation node to the theory node seems to have disappeared.

In his 1994 K-net tutorial, Lewin was ostensibly not only teaching us how to use Klumpenhouwer networks—after all, he did that in his 1990 *Spectrum* article—but also demonstrating their analytical utility. Indeed, in his prose Lewin makes multiple claims about audition, and this is not uncommon in his analyses. I will address two central issues with this analysis: the first has to do with Lewin's segmentation, the second with the goals of his transformational method. Of course, segmentation is always an issue in analysis, so it might seem tangential to raise here, but when you can't buy the segmentation, it is awfully difficult to digest the analysis.[9] Example 2 shows the passage Lewin analyzes: the chorale-like segment from Schoenberg's Op. 11, No. 2. Example 3 shows Lewin's segmentation into thirteen chords that he later uses to form K-nets. Comparing the score and Lewin's segmentation can be a bit tough in his article. The musical example (my Example 2) is shown on p. 80 of Lewin's article, his segmentation (my Example 3) appears on p. 87, and from that point on the analysis makes little reference to the musical surface. Example 4 directly compares the music and Lewin's segmentation. If indeed Lewin chose a chorale-textured passage because its segmentation would be relatively unambiguous, his segmentation clearly departed from those obvious expectations. In addition to the segments that overlap parts of two harmonic events (e.g., segments 1 and 2 share three pitches), there is also a trio of orphaned pitches that do not make it into his segmentation at all: two that are acknowledged by Lewin's parentheses and one that isn't (although it is a pitch-class duplication, so perhaps this is intentional).

[9] Michiel Schuijer makes that point repeatedly in his book, *Analyzing Atonal Music*.



Example 3. Example 9 from Lewin (1994): Thirteen segments from the Schoenberg excerpt used as the basis of a K-net analysis.

I don't know why some of Lewin's segments feature complete chords as heard in both hands, while others combine elements from different chords (not always even segregating right-hand and left-hand dyads). Because transformational methodologies generally relate sets of equal size (even if they duplicate pitch classes), it appears that once Lewin elected to focus on tetrachords, he was methodologically driven to divide larger sets into overlapping tetrachords in order to bring them into the system. However, large chords aren't the only places that Lewin chose to double-count pitches. What unsettles me most about this segmentation is that the list of thirteen chords implies that there are thirteen temporally distinct events, which clearly is not the case. Lewin also displayed his chord segments in an unusual treble-clef-plus-alto-clef system, which subtly obscures both the degree to which he has double-counted pitches and the degree to which he has conflated right- and left-hand pitches. Although it isn't comfortable to say so about such a (deservedly) venerable analyst, his segmentation seems extraordinary and extraordinary claims really do call for extraordinary justification.

Klumpenhouwer claimed that Lewin wasn't necessarily trying to bring out aspects of the music that are immediately (or perhaps ever) audible.[10] Indeed, Lewin might have simply been trying to demonstrate a methodology without making interpretive claims about the music at hand; perhaps he was simply "[searching] for the sake of searching."[11] (This gives weight to my earlier claim that Rings's arrow from the interpretation node to the theory node might not have always been present.) Whatever his intended purpose, Lewin laid bare his analytical agendas early in the tutorial article, and they fundamentally involved tetrachordal segmentations and the goal of relating the large cadential chord to the more obvious tetrachords.

Agenda 1: To formulate an overall view of the chorale, we must somehow relate the 4–19[0148] sets of its middle to the 4–16s[0157] and 4–Z15s[0146] of its opening.

Agenda 2: The cadence chord of the chorale must be integrated into that view.

Agenda 3: So must the verticalities at the end of measure 11 and the beginning of measure 12, chords that strongly project diminished triads. Example 5 [not reproduced in the present article] showed to some extent how the left and right hands of the chords fit into a scheme of 4–19[0148] sets, but the effect of the chords as vertical tonalities cannot be ignored. Furthermore, the $\{D_4, F_4\}$ of the chord at the beginning of m. 12 was not addressed by Example 5; this dyad has yet to be integrated into any overall harmonic view of the chorale.[12]

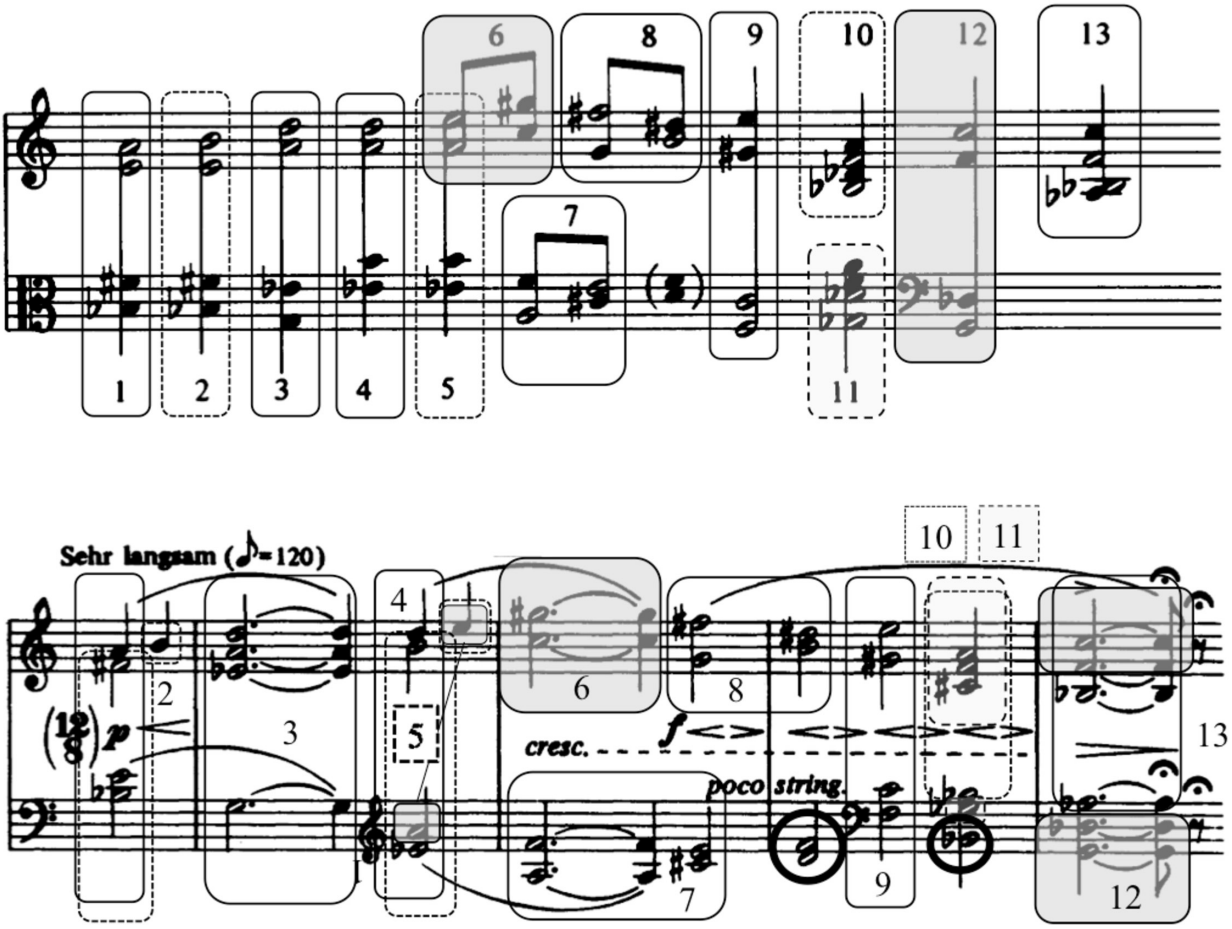
His means toward accomplishing these stated goals involved dividing the six-note chord into two overlapping tetrachordal subsets, which could then be introduced into the transformational network.

Any two analysts might disagree over a particular segmentation and one should be careful about claiming any monopoly on the truth or on musicality. But I don't simply disagree with Lewin's unusual segmentation; I wonder why one would want to construct a transformational network to relate the chords in this passage. As illustrated in Example 5, there's a clear transformational moment as this passage begins. The gesture leading into m. 10 is replicated a fourth higher beginning at the anacrusis to m. 11. But the arrival chord in m. 11 is rather startlingly different from the arrival chord on the downbeat of m. 10. In m. 10, we have another instance of (set class) 4–16[0157]; in m. 11, we land on a real-

[10] Klumpenhouwer (2007, §21).

[11] Schoenberg (1978, 1–2), quoted by Lewin (1987, 12) and re-quoted by Klumpenhouwer (2007, §21).

[12] Lewin (1994, 86).



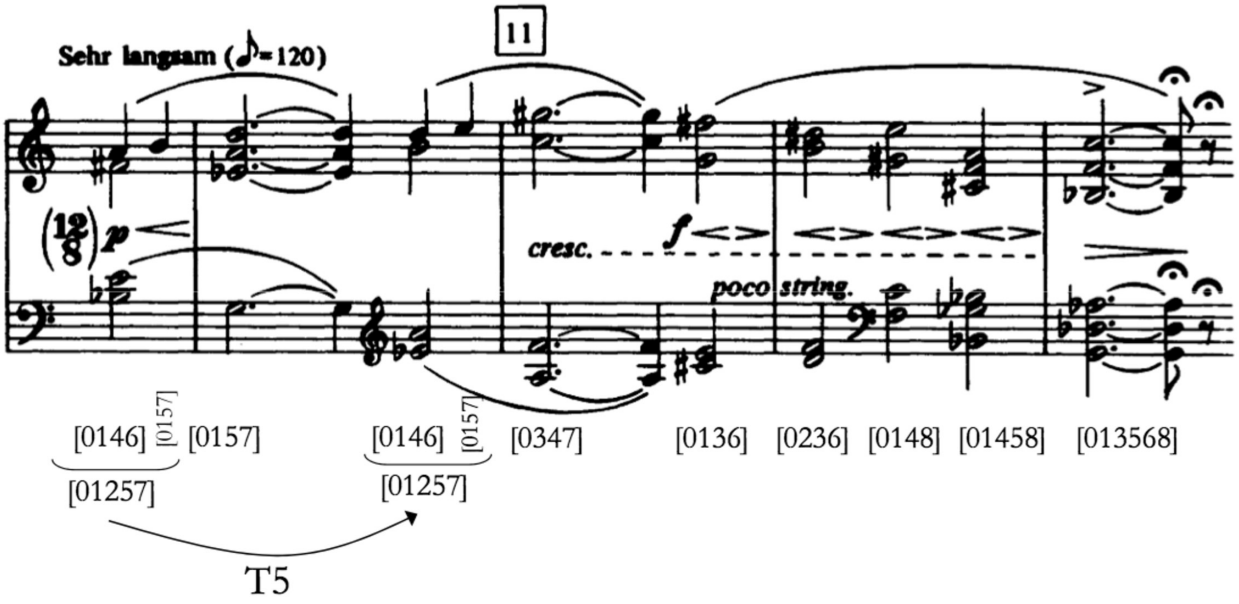
Example 4. How Lewin's (1994) segments (numbered and shaded in the first system) appear in the score (shown in the second system). Circled notes on the score are apparently omitted from Lewin's segmentation.

ization of 4–17[0347]. To frame this less abstractly: the opening bar-and-a-half strongly projects harmonic fourths—both perfect and augmented. In Example 6, I have produced yet another analytical representation of this passage, now replacing set-class labels with chord spacing designations. The concentration on typical second-Viennese sonorities (particularly trichordal segments with adjacent perfect and augmented fourths) is disrupted in m. 11 when we land on a chord that features minor sixths in both hands separated by a perfect fifth. Indeed, most of the chords in this chorale could be heard as projecting some interval, but not perfectly. Each intervallic projection (to borrow from Howard Hanson's lexicon) also includes some other corrupting interval, usually a different chromatic form of the same diatonic interval.

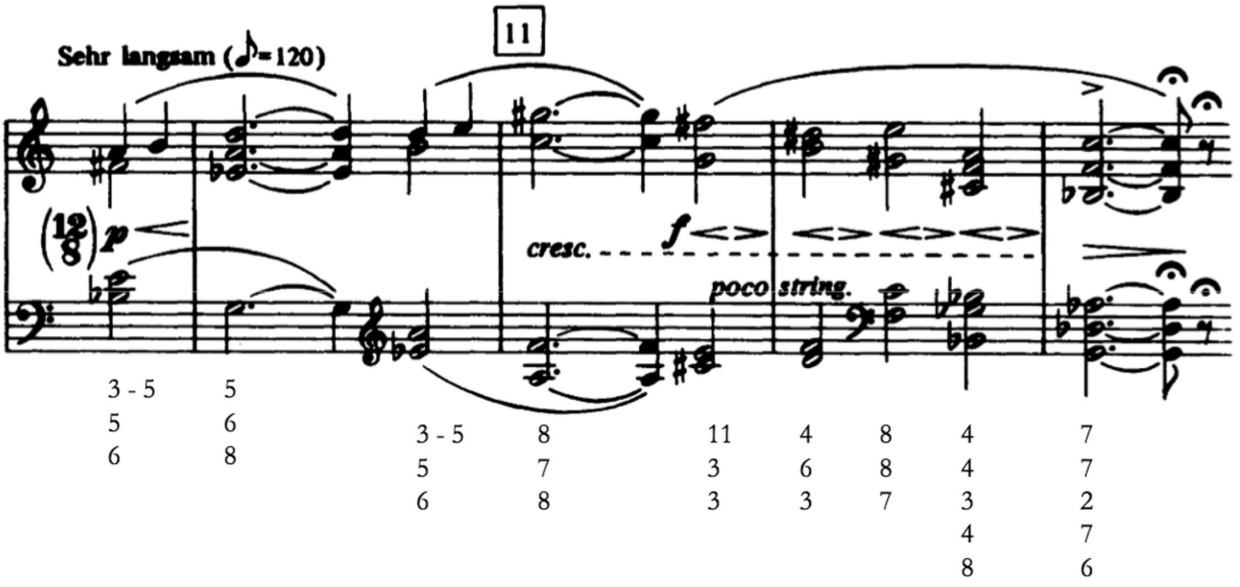
If I wanted to spin the harmonic information in Example 6 into a transformational narrative, I could imagine inventing a transformation to move from one form of a fourth to another; perhaps a transformation to move from one cyclic set type to another. After all, transforming an augmented triad to a quintal harmony would have great utility in describing the cadential passage. But such a transformation would not only fail to account for every note in my segmentation of the passage, it would provide yet another way of imbuing canonical status on dissimilar-sounding harmonic constructs, and it wouldn't ultimately produce a better explanation of what I hear. To borrow David Temperley's terms, such an analysis would be "suggestive," but not (cognitively) "descriptive" (2001, 8–9).

Lewin himself observed that "the passage is clearly a single phrase, yet its harmonic structure sounds diffuse. That is a significant aspect of its aesthetic effect, and we shall take some time to explore more precisely some of its diverse features. Then we shall approach a question which arises *naturally* from this context: is there some way in which we can sense the harmonic field as unified, rather than diverse?" (Lewin 1994, 79; the italics are mine).

It isn't only K-nets that can make canonical connections among diverse harmonies. Other transformations—especially contextually defined ones and split or fuzzy



Example 5. A view of Schoenberg's chorale, emphasizing harmonic set classes.



Example 6. A view of Schoenberg's chorale, emphasizing chord spacing.

operations—can also accomplish the same goal. In particular, transformational analyses that invoke pitch-class inversion when such axes are not reflected in the music can easily seem distant from one's musical experience.

Of course, sometimes we have good reasons for showing that musical entities that seem dissimilar in one dimension are related in some other. For that matter, we might sometimes consider differentiating canonically related entities that appear to be musically disparate (as Lewin compellingly does in his "Appassionata" reading in *GMIT*). I am certainly not looking to dictate what is and isn't significant. However, I have observed that it's very easy to become bedazzled by analyses that rely upon graphic technology and mathematical (or simply formalistic) transformations. Such tools (especially those that take us to high degrees of abstraction) can appear to be significant but bear little relation to musical notation or experience. I think David Lewin

struggled with this problem, and those struggles are evident in a number of his essays and book chapters.

It is worth revisiting the question of why I am principally discussing transformational (and largely post-tonal) analysis rather than *all* formal post-tonal analysis. In part, it's because transformational techniques implicitly necessitate finding relational patterns and often of building networks, and that this motivation seems even more (dare I say) methodologically driven than set-complex or similarity-based analysis. The simple aim of set-complex analysis is to show that a composer is working with a particular collection of related musical building blocks, regardless of how or whether those blocks might be mapped onto or into one another. But, of course, that simple aim can also lead theory to take a particularly strong hand in driving interpretation.

In classic transformational analysis everything must be the same type to enter into the analysis, so there's a strong temptation to make the "type" as broadly defined as possible in order to organically include a maximum number of analytical segments.[13] In other words, for better or worse (or for better *and* worse), set-complex analysis—and particularly similarity analysis (which, early in my career, was my own stock and trade)—sets less lofty analytical goals. In much atonal repertoire (especially from the first half of the twentieth century), it is relatively easy to find recurrences of the same set types, or members of the same extended set-class families, as defined by relatively abstract constructs like genera, Fourier groups, K/Kh complexes, and similarity measures. It is more difficult to find works or even passages where all of the elements can be shown to be transformations of one another. And once we abide by some assortment of transformational restrictions (such as those preference rules laid out by Roeder), we are still left to make a case for why someone should care that a certain musical entity, however abstractly defined, moves along a particular transformational path (to invoke a common spatial metaphor).

Undoubtedly, that case is easier to make if the path is circular, returning us back home after a certain number of steps, or if it's processive, meaning that we continually encounter the same pattern of transformations or the same kinds of motion as we follow that path through the music. Two very compelling examples of processive transformational analyses are Julian Hook's (2008) key signature transformation analysis of Michael Torke's *The Yellow Pages* and John Roeder's (2003) depiction of the triadic sequence in Arvo Pärt's *The Beatitudes*. In both cases, the transformations are not only clear, but they're also readily audible. They are not, however, immediately obvious. Part of the brilliance of Hook's and Roeder's transformational readings is that they both clarify and simplify processes that might otherwise be tough to recognize. Returning to Temperley's (2001) terms, these transformational readings by Hook and Roeder appear to act both descriptively and suggestively.[14]

When constructing transformational narratives about non-process-driven music, it is often possible to make the music *seem* processive by showing, as Lewin did, that a diverse progression can, somehow, appear unified. I approach such analyses skeptically. I'm willing to be convinced—I *love* being convinced by creative analyses—but it will (and it should) take more than a pretty graph and the invocation of transformational metaphors.

2. A Case for Analysis, a Basis for Analytical Criticism

Henry Klumpenhouwer criticized me for mentioning that some analytical claims are "arbitrary" or "inconsistent." He clearly believes that we should learn from all analyses and shouldn't have to criticize particular readings as good or bad, and, viewed uncynically, his is a very humane and generous perspective. We are, thankfully, a polite discipline and Klumpenhouwer's view plays to our communal sense that it is considered bad form to claim that someone's transformational (or simply atonal) analysis is unconvincing or, worse, unmusical. And yet, as I mentioned earlier, we rarely hesitate to criticize Schenkerian analyses on those same grounds. The effect, as I see it, is that certain analytical methodologies have gained critical immunity. I have suggested some possible reasons behind this: a positivistic interface that lacks musical immediacy, reliance on formalisms that seem infallible and that relatively few people in our discipline truly understand well enough to criticize, and a lack of any sort of intersubjective analytical preference rules.

John Rahn wisely argued that musical theories are not necessarily vacuous if they are not falsifiable (1989, 149–150). We do not need and should not spend our time seeking some objective standard for good and right analysis. (I am rather far from the closed-minded puritan that Henry Klumpenhouwer made me out to be in 2007.) However, I believe that as a community we ought to acknowledge that a sensitive

[13] Notable exceptions to the usual condition that transformed items be members of the same general type of object can be found in Julian Hook's work on cross-type transformations (Hook 2002, 2007).

[14] One danger of applying Temperley's descriptive/suggestive dichotomy is that (as Temperley himself pointed out), these terms aren't actually dichotomous. Another danger is that these terms might be read as suggesting that analyses have failed if they don't meet some cognitive/descriptive standard. Personally, I generally only find myself caring about whether I can "hear" (or even learn to hear) a particular analysis when the analyst makes implicit or explicit cognitive or perceptual claims.

musical reading should aim to be more than simply well formed. A novel may exhibit impeccable grammar, a rich vocabulary, and use sophisticated literary devices yet still garner criticism if it lacks a compelling and coherent narrative. We should feel similarly empowered to challenge analyses that use impressive graphic interfaces and seem unassailably well formed if they do not also make any compelling and coherent musical arguments. Conversely, I believe we should resist any well-learned urges to dismiss analytical work that lacks tightly formalized charts and graphs or that does not account for every note in a passage.[15]

As I said in the K-net debates that appeared in *MTO* in 2007 and 2008, I am not interested in criticizing theory that stakes no analytical claims. I find tremendous value in abstract theorizing, which can certainly lead to new ways of thinking about music and particularly to fruitful ideas for composers. But I have argued that whenever theory is applied to music, analysts ought to say something (however implicitly) about both the music and also the methodology at play. When we aspire only to the former cause, our use of analytical theory might be superfluous; when we aspire only to the latter cause, our engagement with the music can seem superfluous. When no preference rules can be found in our operative methodology node, we should assiduously try to bring our own interpretations to the fore and should try to help others understand what musical features led us to wield transformational tools. In turn, our community should try to look beyond the tools and should open-mindedly support and engage all sorts of analytical interpretations with critical responses.

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[15] Elsewhere, I have argued that we should exercise a freer hand in reading ornamentation in post-tonal music (Buchler 2014).

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