

## Are There Any Bad (or Good) Transformational Analyses?

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### [slide 1—title]

Let's get this out of the way. Yes, I think that there *are* bad transformational analyses—and good ones, too. Okay, so that settles the title question. But the real questions that I want to address in this short talk are: “how do we know?” and “how can we tell a bad transformational analysis from a good one?”

You might 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 article “Reconsidering Klumpenhouwer Networks,” said “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.” Indeed, while my objections to the way recursive structures conflate pitches 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 certainly most of it.)

Klumpenhouwer's response also led me to contemplate the epistemology of transformational analysis and also of Forteian set-theoretical analysis. In other works—in *Lewin's* words—much of what I will discuss applies both to transformational and Cartesian methodologies, but I should say at the outset that I am not especially interested in engaging in a criticism of transformational or Cartesian ways of theorizing. If anything, I believe we should do more theorizing, find more ways of navigating our way around our musical universes, and more ways of imagining compositional spaces.

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Writers as diverse as David Lewin, Joseph Kerman, and Marion Guck have claimed that when we perform an analysis, we are *interpreting* or *reading* a musical work, and we music theorists generally do this in terms that conform to some methodological expectations.<sup>1</sup> “Methodology” is a word that Lewin and Klumpenhouwer use often when discussing analytical technique and it plays prominently in Steve Rings's recent essay on (*JMT* called it a review of) David Lewin's three books. Rings cleverly encapsulated Lewin's analytical and theoretical philosophy by constructing a kind of meta-

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<sup>1</sup> One could argue that methodological expectations are even more robust in other fields, such as ethnomusicology, than they are in music theory.

transformational network of Lewin's so-called "project." **[slide 2]** In beautifully Lewinian fashion, Rings's network is simple, immediately apprehendable, 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 left me scratching my head a bit. **[slide 3]** In it, Rings suggested that, in my critique of Klumpenhouwer networks, I had effectively tried to detach "the methodology node." Or, more accurately, he suggested that, in their responses to my critique of K-nets, Shaugn O'Donnell and Henry Klumpenhouwer had suggested that I had tried to detach "the methodology node." Rings was surely referring to their claims that I had corrupted the ontological nature of K-nets by reshaping them into dual transformations. (That was my simpler representation of the same musical information that K-nets model.) To my mind, while I had certainly challenged the overall utility and practicality of K-nets in analysis, I didn't think that I had broadly challenged the notion that there *is* a methodology at play.

And then it dawned on me that I might not actually know what counts as "methodology" in this context. More specifically, I wonder 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 music we examine and what technique 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 you might think about its applicability, practicality, or underlying philosophical basis, we could probably all agree that Lerdahl and Jackendoff's 1983 *A Generative Theory for Tonal Music* ("GTTM") amounts to one of the most robust methodologies of our time. In particular, their 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. Within their system, preference rules can be thought of as providing many windows for interpretive freedom.

If the "rules" for Schenkerian analysis are less specifically articulated in treatises (both by Schenker and others), there are still many norms that can nonetheless be divined through a study of

Schenkerian analysis as practiced in the pages of our journals and textbooks and in conference handouts. A textbook like Cadwallader and Gagné's *Analysis of Tonal Music* (2007) surely lays out far more preference rules than well-formedness rules, but both types are there in spirit, if not in name.

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 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, 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 suggest that merely asking these questions suggests a narrow-mindedness that is destructive to analysis. 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 so forth. I won't argue that the analysis of atonal music should be or can be either as all-encompassing or as reliant on normative syntax as is tonal analysis. (Indeed, Lewin conceded almost exactly that point in his second book *Musical Form and Transformation*.) But, I will argue that the *kind* of methodology that governs most (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 or Forte or Rahn or Cohn or Klumpenhouwer defined an analytical method, it is one that contains only tools, not guidelines for their application.<sup>2</sup> More specifically and with direct reference to *GTTM*: the methodology governing transformational analysis appears to consist only of well-formedness rules, not preference rules. That said, it's worth noting that last year in *Music Theory Online* [slide 4] John Roeder proposed a very coherent set of preference rules that combine 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. I realize that you can't read that very well, so let me highlight just the first few guidelines: "Choose the most aurally salient analytical objects that will still belong to a single

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<sup>2</sup> In 2009, John Roeder more bluntly claimed that Lewin never articulated a transformational methodology for analysis. (*MTO* 15/1, §1.3.)

family and have an economical transformational structure. 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). Choose transformations that occur prominently and repeatedly.” (Roeder 2009)

I think Roeder’s guidelines are helpful, but I imagine that abiding by them faithfully would restrict one to only a small canon of post-tonal compositions (and 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 my talk and, indeed, my talk 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 think that our evaluations should be founded on our intersubjective notions of musicality, not merely on well-formedness, and that is something that we have been reluctant to do. I know, some of you might be wondering why we have to evaluate at all. Everyone has different standards and goals for analysis, everyone is captivated by different sets of musical features, 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. We are, after all, a critical discipline.

It is easy to envision a failed Schenkerian analysis that nevertheless adheres to every notational convention and features no internal conflicts. **[slide 5]** Here are the first eight bars of the last movement from Schubert’s late A-major piano sonata, D. 959. Let’s listen **[play]** Now, consider this less-than-brilliant Schenkerian analysis of the passage. **[slide 6]** Let me play that again. You might want to follow my sketch. **[play]** I’m sure that you all could recite a litany of reasons that *this* a *terrible* analysis. It misreads harmony and phrase structure, it doesn’t respect meter, it shows a prolongation of scale-degree 1 at the cadential six-four in the first phrase, the alleged head tone is actually ornamental, and there are considerably more musical sins. *But* there are no conflicting slurs, there are no orphaned tones, and at first glance—if you hadn’t either seen or heard the music—I’ll bet that it would 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 violates our collective musical principles—badly—and in ways that we can readily describe without relying on that old meme, “I just don’t hear it that way.”

Can you imagine a committee of experts saying that a well-formed transformational analysis had failed on similar grounds? You might be able to *imagine* it, but I don't honestly think 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 many of his post-tonal analyses have simply escaped any critical reading.

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 rather demonstrating their analytical utility. Indeed, in his prose he makes multiple claims about audition, as he does in many of his analyses. There are two central issues that I will address: 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 perhaps it's a bit tangential to this talk, but when you can't buy into the segmentation, it's awfully difficult to digest the analysis. (Michiel Schuijjer makes that point repeatedly in his recent book, *Analyzing Atonal Music*) **[slide 7]** Here is the passage Lewin analyzes, the chorale-like segment from Schoenberg's op. 11, no. 2. **[play]** **[slide 8]** And here is Lewin's segmentation into thirteen chords that he later uses to form K-nets. **[slide 9]** Now let's compare score and segmentation, which is tough to do in the context of the article. The musical example is shown on p. 80, Lewin's segmentation appears on p. 87, and from that point on the analysis makes little reference to the musical surface. Let's go through how Lewin's chords actually appear in the music. **[at my talk in Indianapolis, I clicked through and the various colored bubbles appeared one at a time. It isn't easy to see on slide 9, but chord 6 spans three different time points]** and then there are a trio of orphaned pitches (circled) that don't make it into this segmentation. 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).

I'm not sure why some 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 methodology generally requires sets of equal size (even if they duplicate pitch classes), 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 bothers me most about this segmentation is that the list of thirteen chords might imply thirteen temporally distinct events, which clearly isn't the case. Lewin also displayed his chord segments in an unusual treble-clef-plus-alto-clef system, which disguises 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, his segmentation seems extraordinary and extraordinary claims really do call for extraordinary justification.

Klumpenhouwer claimed that Lewin wasn't really trying to bring out aspects of the music that are immediately audible, perhaps not audible at all, but rather trying to demonstrate a methodology. But, as both Roeder and Rings have observed, Lewin seemed conflicted at best on this issue. **[slide 10]** Lewin laid bare his analytical agenda early in his tutorial article, and it fundamentally involved tetrachordal segmentations and the goal of relating the large cadential chord to the more obvious tetrachords. But his means to accomplish this involved dividing the six-note chord into two overlapping tetrachordal subsets, which could then be introduced into the transformational network.

Beyond simply disagreeing with Lewin's unusual segmentation, I wonder why exactly one would *want* to construct a transformational network to connect the chords in this passage. **[slide 11]** At the beginning, there's a clear transformational moment as the gesture in the pick-up to m. 10 is replicated a fourth higher at the pick-up 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 [0157], in m. 11 we land on a realization of [0347]. **[slide 12]** To put things somewhat less abstractly, the opening bar-and-a-half really projects harmonic fourths—both perfect and augmented. (You can see that I've replaced set-class labels with chord spacing designations.) This concentration on typically second-Viennese sonorities, 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 this 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.

**[slide 13]** 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. Emphasis 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 operations—can also be used to accomplish the same goal. In particular, transformational analyses that invoke pitch-class inversion when such axes are not reflected in the music can seem quite distant from musical experience.

Of course, sometimes we *do* have 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 seem musically disparate (as Lewin compellingly does in his “Appassionata” reading in *GMIT*). I am certainly not looking to prescribe what is and isn’t significant, but I want to point out 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 apparent in a number of his essays.

It’s worth revisiting the question of why I’m principally talking about transformational analysis rather than about *all* formal post-tonal analysis. In part, it’s because transformational techniques implicitly necessitate finding relational patterns and 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 set of musical building blocks, regardless of how or whether those blocks might be mapped onto or into one another.

In 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. In other words, for better or worse (or for better *and* worse), set-complex analysis—and particularly similarity analysis (which was my own stock and trade)—sets less lofty analytical goals. In a lot of atonal repertoire (especially from the first half of the twentieth century) it’s 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, and K/Kh complexes; it’s less easy to find works or even passages where all 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 still have to make a case for why someone should care that a certain musical entity moves along a particular transformational path (to invoke a common 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 kind of motion as we follow that path through the music. (I'm thinking about Jay Hook's key signature transformation analysis of Michael Torke's *The Yellow Pages* or John Roeder's depiction of the triadic sequence in Arvo Pärt's *The Beatitudes*, to mention just two examples, both of which are not only clear but audible).

When we construct transformational narratives about non-process-driven music, we often make the music *seem* processive by showing, as Lewin did, that a diverse progression can, somehow appear unified. I personally approach such analyses with a higher degree of skepticism. I'm willing to be convinced—I *love* being convinced by creative analyses—but it's going to take more than a pretty graph and the invocation of transformational metaphors.

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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, that is a very kind and generous perspective. We are, thankfully, a polite discipline and Klumpenhouwer's perspective plays to our communal sense that it's considered very bad form to claim that someone's transformational analysis is unconvincing or, worse, unmusical. And yet, as I mentioned, we rarely have any problem criticizing Schenkerian analyses, and *most* of the worst ones don't make it onto conferences or into journals and books (of course, some of the best ones don't either). The effect, as I see it, is that certain analytical methodologies have gained critical immunity. I have suggested some reasons behind this: a positivistic interface that lacks musical immediacy and that allows one to rely upon fanciful musical segmentation, 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: 149-50). We do not need and should not spend our time seeking some objective standard for good and right analysis. However, I believe that as a community we ought to



acknowledge that a sensitive 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 technologies and seem unassailably well-formed if they don't 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.

As I said in my earlier K-net debates, I am not interested in criticizing theory that stakes no analytical claims. I think that abstract theorizing is valuable and can certainly lead to new ways of thinking about music and 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.<sup>3</sup> 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.

**[slide 14]**

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<sup>3</sup> David Lewin was especially good about doing this in his analyses of tonal music; it is in the atonal analyses where the theory node sometimes seems to dominate the interpretation node.

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