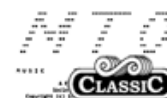




Response to Parncutt

Thomas R. Demske



REFERENCE: <http://www.mtosmt.org/issues/mto.95.1.2/mto.95.1.2.demske.html>

KEYWORDS: similarity, perception

[1] Richard Parncutt proposes a perceptual approach to similarity analysis based on, “average subjective judgment of global similarity by a representative group of listeners.” Even if it were possible to achieve a general consensus on such a standard, I would be uncertain about how to apply it in musical contexts.

[2] The “modified harmonic fluctuation” model of Messiaen’s chord succession described in my essay was a rhetorical expedient. Most readers tentatively accepted the possibility of a connection between perceived breaks in surface continuity and low (whatever that means) REL, ASIM, and ATMEMB values. Competing clusters, based on different pivots or on different cutoff points relative to a single pivot, could thus presumably be evaluated according to how well they conformed to perception. (Recall that evaluation in general, and not perception in particular, was the focus of the essay.) But there are many basic difficulties here. One lies in identifying precisely what percepts might be appropriate testing grounds for the evaluation. (Cf. paragraph 16 in the essay.) Another is that of isolating the “similarity” relationship component from other factors contributing to a goal percept. (Cf. paragraph 17 in the essay.)

[3] My working assumption throughout was that REL and comparable functions have something to do with perception. However valid that assumption may or may not be, the two problems mentioned above remain even for functions more securely grounded in that area. If I understand Professor Parncutt’s paragraph 6 correctly, he is suggesting a table lookup function (?), where the table entries have been determined empirically through experimentation. I suppose that this means asking subjects to rate “global similarity” for $29 \times 29 = 841$ pairs of chords (or $841 \times 2 = 1682$, to check for immediate order effects). Perhaps the chords would be sounds, extracted from a single performance—maybe normalized somehow, maybe not. It might even be possible (maybe!) to explain what “global similarity” means, so that subjects would have some idea of what to shoot for.

[4] By construction, the proposed function should have something to do with perception. But how far would that “something” extend? Given competing clusters of the 29 chords under the proposed function, our hope would be to select only good clusters by listening to the piano ostinato. What to test against is the first decision: smooth progressions, surface grouping boundaries, shifts in large-scale harmonic region? Whatever we decide will likely require considerable extrapolation

in order to relate it to the exhaustive process of discrete chord pairings used in deriving the function; each step in the extrapolation increases the distance between the function's application and its perceptual grounding. Next, given a goal percept, would we necessarily reject a clustering because it conflicts with the percept? Other factors not addressed through the experimental binary comparisons could take control in such situations—contour changes; local tessitura; rhythm; clarinet, violin, and cello parts; voice leading. (Recent mto-list exchanges on enharmonicism seem especially relevant to me here.) The problem is in determining how far mitigating factors are operative, and how far they should be taken into account, when judging similarity-based boundaries according to perception.

[5] David Lewin suggested in a recent mto-talk post that we drop the “similarity” label when referring to functions like REL, RECREL, etc. I suspect a wee bit of tongue-in-cheek here; I also doubt whether the SMT language police budget allows opening a new front in the continuing war on objectionable signifiers. So, what I suggest instead is that we recognize “context-free similarity” for the oxymoron that it is. (On a volunteer basis, of course.) Similarity presupposes a context. The context of REL is a particular intellectual apparatus. The context of (what I understand to be) Professor Parncutt's proposed similarity measure is a particular experimental setting. I would not at all suggest abandoning “similarity” functions. Like all reasonably well-developed constructs, they hold nice potential for theorists.⁽¹⁾ What I do suggest is applying more energy toward understanding their limits.⁽²⁾

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Footnotes

1. See, for example, Chapter 6 of Marcus Castren's oft-mentioned dissertation, “RECREL: A Similarity Measure for Set-Classes” (Ph.D. diss., Sibelius Academy, 1994). Also, Allen Forte obtained remarkably interesting results some twenty-plus years ago with his R0, R1, R2, and Rp relationships (*The Structure of Atonal Music*, Yale University Press, 1973). Those relationships have been much maligned in the subsequent similarity literature. I think a more sympathetic re-evaluation, especially of Forte's analytical applications, could prove very illuminating.

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2. Richard Hermann's response reached me only as I finished writing this. I will reply (if appropriate) after studying it.

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