Reply to Erkki Huovinen

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In a rare moment of insight in his reply to my admittedly harsh review of his book, *Pitch-Class Constellations: Studies in the Perception of Tonal Centricity*, Erkki Huovinen notes, "I thus understand that Agmon's criticisms are directed towards my work principally to the extent that it represents experimental music psychology in general." Indeed, I would never have bothered to review what Huovinen characterizes in his reply as "just a doctoral dissertation," had I not felt that, *to the extent that the work in question represents experimental music psychology in general*, it tells a very sad story concerning the present state of this discipline.

In the central part of my review, " 'Psychological Reality' and the Lure of Objectivity" (137–40), I expose the biases of Huovinen's ostensibly "objective" methodology. In place of any serious argument in his defense, Huovinen presents in his reply a lengthy synopsis of his book as the first of what he refers to as "two arguments for the mental reality of diatonicism." The synopsis adds little of substance to what an intelligent reader can already glean from my review. Be that as it may, one can only commend Huovinen for the disclosure that immediately follows the synopsis: "I now feel that there are some difficult problems in this argument."[1]

The errors that Huovinen makes in his reply are many, beginning, in fact, with a poor choice for a title. As he correctly points out (albeit with a wrong page reference), on p. 141 of my review I state that according to his study (and contrary to popular belief), diatonicism is not an essential component of tonal perception. As I immediately explain, I base this statement (in part) on the very last sentence of his book:

> . . . the empirical results of the present study, *while questioning the role of fixed pitch collections and fixed measures of stability*, still leave no doubt about the importance of centricity in that flexible process of making subjective sense out of musical pitch environments that can be called tonality" (371, emphasis added).

1 Huovinen claims that he does not know what has "shattered for good" my "initial trust in . . . [his] scientific impartiality" (Agmon 2004, 132). However, I should think it is pretty clear that I mean the statement in his book that mistakes sarcasm for argument in its reference to "traditionally bound music-theoretical explanations," a statement quoted in my review on p. 135. Apparently, the best Huovinen can do in response to my charge that he is far less objective than he pretends is to attempt to turn this charge against me, insinuating that I have "intentionally misrepresented" the data displayed in Table 6.3.1 of his book "while converting them to percentages" (see Agmon 2004, Example 1). However, as I clearly explain in an accompanying footnote, the distortion of data *originates with Huovinen*, who rounds off to the nearest integer the *average* number of subjects selecting (across trials) a given pitch class TC. (Indeed, I *inform* the reader that, for precisely this reason, my calculated percentages "may be slightly inaccurate.") Instead of further undermining his stature as an objective scientist by making his insinuation, Huovinen could have much more reasonably provided the relevant numbers in his reply, since overall, only he has access to them. As it happens, in Figs. 6.4.3, 6.4.5, and 6.4.6 of his book, he provides the averaged percentages of *string-bounded* responses for *six* of the sixteen set-classes in question. I present these percentages below, together with the corresponding percentages given in Example 1 of my review in parentheses. The reader can judge whether or not my "tampering" (*sic!*) with the results makes them "favorable" to my views: 5–24A: 6.6 (7.05), 12.7 (12.94), 31.3 (30.58), 22.9 (22.35), 8.9 (8.23); 5–24B: 7.8 (8.13), 15.1 (15.11), 19.3 (18.6), 15.1 (15.11), 30.1 (29.06); 5–29A: 16.9 (16.09), 9.0 (9.19), 10.2 (10.34), 9.6 (9.19), 38.0 (36.78); 5–29B: 14.5 (14.11), 14.5 (14.11), 25.3 (24.7), 13.9 (14.11), 16.9 (16.47); 5–30A: 4.8 (4.76), 20.8 (20.23), 26.2 (26.19), 11.4 (11.9), 20.8 (20.23); 5–30B: 5.7 (5.88), 10.2 (10.58), 30.4 (29.41), 11.7 (11.76), 21.4 (21.17).

Indeed, in the final section of my review (141–43), I challenge Huovinen's conclusion that diatonicism is not an essential component of tonal perception, sketching a theory of diatonicism that, if true, would render certain results from his Experiment 5 irrelevant.

The strategy by which Huovinen opts to meet my challenge is as odd as it is self-defeating. On the one hand, he finds a cautiously worded statement from his book, and insists that this statement is equivalent to the statement that, according to his study, diatonicism *is* an essential component of tonal perception. This is the essence of his first "argument for the mental reality of diatonicism," the lengthy book-synopsis that follows notwithstanding. On the other hand, in his second "argument for the mental reality of diatonicism," he shifts the debate to my ground, finding flaws with, what *he* understands is my notion of diatonicism. On both counts, however, Huovinen misses the mark.

HUOVINEN'S FIRST "ARGUMENT FOR THE MENTAL REALITY OF DIATONICISM"

The reader will decide whether statement (A), "This [i.e., the results from the weighting vector analysis] suggests that the subjects may, indeed, have relied on long-term memory representations derived from diatonic music (in a wide sense),"[2] is equivalent, as Huovinen insists, to statement (B): According to Huovinen's study, diatonicism is an essential component of tonal perception.[3]

Let us, however, allow Huovinen to have it his way, and assume that (A) above is indeed equivalent to (B). Note that (B), which negates my statement that, according to Huovinen's study, diatonicism is *not* an essential component of tonal perception, is equivalent to "critically verifying the psychological relevance of the diatonic scale," that is, to verifying "the mental reality of diatonicism."

The first thing that comes to mind as a result of this exercise is that if (A) is indeed equivalent to (B), then Huovinen's question ". . . why should one try to empirically determine whether the diatonic scale figures as a mental schema in organizing the listener's perception if one already knows this from music theory?" remains open. Indeed, why? That Huovinen has little trust in the claims of music theorists does not seem to alter the fact that, after much ado, he will merely "verify" these claims. Does Huovinen seriously believe that music theorists should sleep better, now that their claims have been "verified" by "methodologically rigorous music-psychological experiments"?[4]

However, a much deeper problem renders Huovinen's claim that (A) is equivalent to (B) untenable, and that is the presumption that theories *can*, in fact, be verified. Given the references in my review to Karl Popper's seminal contribution to the philosophy of science (see notes 4 and 9), not to mention Huovinen's own appeal to Popper's authority in a different context towards the end of his reply (of which more will be said later), one would think that he *knows* that theories can only be proven false, never verified. However, time and again in his reply, Huovinen makes it abundantly clear that this fundamental insight into the nature of the scientific inquiry is something that he simply cannot, or will not understand.

Consider my allegedly "jaw-dropping conclusion," that "this is the first time a music psychologist has rejected outright

2 Huovinen 2002, 348, restated in Huovinen 2005.

3 Compare statement (A) with the much stronger statement with which the same paragraph begins. "In sections 1.1 and 1.2, we saw how various theories concerning the perception of tonality can be criticized for their *uncritical acceptance of diatonicism as the basis for all tonal hearing.* . . . Indeed, the experiments reported in this study have suggested many common aspects of tonal perception that cannot be accounted for *by theories which put the diatonic scale or a comparable structural description in the minds of the listeners and use such templates as if they were logical inference rules*" (emphasis added). Huovinen suggests that I have confused his methodological premises as his conclusions. Given the in-depth analysis of his methodology that I present, this is absurd.

4 Huovinen 2005, note 4.

the relevance of music theory to the study of tonality" (Agmon 2004, 131). Any reasonable reader will agree that my "conclusion" is hardly more than a paraphrase of Huovinen's own statement, quoted one sentence earlier in my review, "one of the pervasive features of the present work has been precisely the claim that a 'coherent concept of tonality' should not be based on criteria expressed in the concepts of music theory" (Huovinen 2002, 370). That Huovinen finds my paraphrase of his statement so utterly astonishing can only be attributed to his misunderstanding of the term "theory." Of course he has not rejected music theory *as a methodological tool*; this much, one would hope, Huovinen understands from my review. Nevertheless, he definitely has rejected the claims of music theory *as theories*, that is, as inherently un-verifiable ideas that nonetheless may *conceivably* be falsified.[5]

Consider also Huovinen's statement that his book ". . . addresses problems [concerning the mental reality of music-theoretical constructs as organizers of human musical perception] that simply would not exist if Agmon . . . were right in his claim: 'music theory, by and large, *is* a theory of mind'."[6] Since this statement is conditional ("*if* Agmon . . . were right"), let us pretend for a moment that I *am* right, and music theory, by and large, is indeed a theory of mind. Does it follow that "problems . . . concerning the mental reality of music-theoretical constructs as organizers of human musical perception" do not exist? Not at all. Huovinen apparently cannot comprehend the difference between the claim that music theory, by and large, is a theory of mind, and the absurd claim that all musical theories, *as theories of mind*, are a priori true. Only the *true* musical theories, as theories of mind, will have what Huovinen calls "mental reality." As I clearly explain (in the wake of Chomsky) in my review, "since music cannot be reasonably located anywhere else in the world except the human mind, almost any musical theory, implicitly or explicitly, is 'psychological'; *whether the theory is 'real' or not is indistinguishable from the question of whether it is true or not*."[7] How does one *know*, one might ask, that a musical theory is true (and is therefore "mentally real")? Well, one doesn't. However, somewhat like the presumption of innocence in the legal domain, so long as one is unable to show that a given theory *is false*, one assumes it to be true.

Be that as it may, Huovinen fails to provide any serious argument to counter my claim that music theory, by and large, is a theory of mind. Towards the end of his reply, however, he appeals to Popper's pluralistic philosophy by which "there are three worlds," the physical, the mental, and the "third world" of "possible objects of thought," and suggests that music belongs to all three.[8]

5 Huovinen makes the prevalent mistake of interpreting Popper's notion of falsifiability in empirical rather than logical terms. This attests once again to his misreading of my review, given that I warn against this mistake in note 4.

6 The reference is to Agmon 2004, 138. See also Huovinen's statement, "if . . . [Agmon] means that all previous researchers in music psychology have simply accepted the claims of music theory straight away as claims concerning the human mind, I sincerely hope that he is mistaken."

7 Agmon 2004, 138, emphasis added. To make the analogous statement in reference to any of the natural sciences would be trivial indeed. A theory in physics, for example, has "physical reality" only to the extent that it is true. Huovinen writes that "in accepting these claims [that music theory, by and large, is a theory of mind] . . . [Agmon] loses the possibility of developing a lot of interesting music theory, being tied to the claim that our theoretical constructs *necessarily have mental reality as organizers of our musical perception*" (emphasis added). This is like saying that a physicist loses the possibility of developing a lot of interesting physical theory, being tied to the claim that his theoretical constructs are true (have physical reality).

8 Huovinen (2005) refers to Popper's "third world" as "the cultural world of symbols, theories, and artifacts." However, according to Popper (1979, 154), ". . . the third is the world of intelligibles, or of *ideas in the objective sense*; it is the world of possible objects of thought: the world of theories in themselves, and their logical relations; of arguments in themselves; and of problem situations in themselves."

As far as I am aware, Popper himself never considers the status of music *vis-à-vis* his three worlds. However, from his discussion of human language, it is quite clear that music is hardly a "third-world" object:

> And in so far as language contains information, in so far as it says or states or describes anything or conveys any meaning or any significant message which may entail another, or agree or clash with another, it belongs to the third world.[9]

As to whether music is a *first*-world object, this is essentially a terminological question. For example, one could reasonably maintain that acoustical signals per se are not "music," so long as they have not been processed by a human mind. Let us suppose, however, that Huovinen's Popper-inspired idea concerning the multifarious nature of *music* is correct. Music *theory*, common sense would then seem to dictate, although, say, a "symbolic construct" itself, *addresses* the physical, "cultural," *and mental* worlds.[10] Huovinen's conclusion, however, is rather different. "As a symbolic entity," he states, ". . . a musical theory may have many aspects that are beyond reach for a human comprehender."[11] Huovinen is consistent at least, for he complements the peculiar logic of this statement with the equally novel notion that "what makes the task of the music psychologist interesting" are "exactly" (*sic*) the humanly *in*comprehensible aspects of musical theories (*and* compositions), aspects which, for some obscure reason, he sees as the most notable attributes of their ostensible symbolic nature.

HUOVINEN'S SECOND "ARGUMENT FOR THE MENTAL REALITY OF DIATONICISM"

As we have seen, in his first "argument for the mental reality of diatonicism" Huovinen misses the opportunity for meeting me on *his* ground, namely, the falsification of theories by experimental means. This he does by arguing that his study not only does *not* establish that diatonicism is not an essential component of tonal perception (which, by any reasonable reading of his text, is counter to fact), but also that it *does* establish *the opposite* of this statement (which is counter to standard notions concerning the nature of the scientific inquiry). As his second "argument for the mental reality of diatonicism," Huovinen shifts the debate to *my* ground, music theory. This is an odd move, particularly by someone self-proclaimed to be interested in music theory primarily as a methodological tool.

Let me point out at the outset that, compared to the first "argument," the *logic* of Huovinen's second argument is somewhat less shaky. Huovinen constructs a "transformational" theory that reflects *his* understanding of what I mean by "hearing a collection of pitches diatonically." He then shows (or at least can be interpreted as showing) that this theory (supposedly, mine) *approximately* satisfies Popper's notion of "unfalsifiability"; therefore, it is hardly a scientific theory. In constructing his theory, however, Huovinen not only reveals once again his limited understanding of my text, but also his limitations as a music theorist.

To be sure, Huovinen can hardly be faulted for not being a very sophisticated music theorist; after all, "music theorizing" is not his primary interest.[12] However, Huovinen cannot,

9 Popper, 1979, 157. According to Popper, "human language . . . belongs to all three worlds."

10 *Cf.* Popper 1979, 157–58: "It was the Stoics who first made the important distinction between the (third-world) objective logical *content* of what we are saying, and the *objects* about which we are speaking. These objects, in their turn, can belong to any of the three worlds: we can speak first about the physical world (either about physical things or physical states) or secondly about our subjective mental states (including our grasp of a theory) or thirdly about the contents of some theories, such as some arithmetical propositions and, say, their truth or falsity."

11 Huovinen's complete statement reads, "as a symbolic entity, *a musical composition* or a musical theory may have many aspects that are beyond reach for a human comprehender" (emphasis added).

12 Contrary to Huovinen's complaint, there is nothing "insulting" (nor, as far as I can see, "catchy") about the opening remark of my review, "anyone tempted to infer from the title of Erkki Huovinen's *Pitch-Class*

on the one hand, show little interest in, let alone respect for, music theory, and on the other, believe he is entitled to adjudicate whether such works as my 1989 and 1996 *JMT* articles are just pretty "symbolic construct[s]," or have any standing with respect to the mental world. Indeed, had Huovinen studied these and other relevant works carefully, or paid closer attention to the statements I make in my review, he could never have assumed that *his* theory is anything like what I might have in mind.[13]

Let me quote, then, the paragraph from my review that sketches my conception of diatonicism.

> What does it mean to hear a given collection of pitches diatonically? Let me suggest somewhat simplistically . . . that to the extent that conventional staff notation (including accidentals) does justice to one's hearing, then one hears diatonically. For staff notation respects what I believe is an essential attribute of diatonicism, namely, a seven- as well as a twelve-element "octave." Of course, someone who believes, as Huovinen does, that staff notation is merely an arbitrary convention, that enharmonic distinctions are "confusing" and "non-objective," and that, in general, the diatonic scale is just a "pitch collection," would disagree (Agmon 2004, 141).

How can anyone understand from this paragraph that my notion of hearing "a given collection of pitches diatonically" (henceforth, "diatonic pitch-class representation"), whatever its exact content might be, can be cast exclusively in the language of twleve-tone theory?

Example 1, based on Fig. 5–2 from my unpublished 1986 Ph.D dissertation, "Diatonicism, Chromaticism, and Enharmonicism: A Study in Cognition and Perception," should give a clearer picture of what I mean by diatonic pitch-class representation.[14] The notion is based on the assumption that the diatonic scale is used as a mental template in assigning diatonic representations to pitch classes. This assumption runs contrary to Huovinen's reference to "common aspects of tonal perception that cannot be accounted for by theories which put the diatonic scale or a comparable structural description in the minds of the listeners and use such templates as if they were logical inference rules" (Huovinen 2002, 347–48). Indeed, another template assumed to be used in the same process is the set of eleven *diatonic intervals* P1, m2, M2, m3, M3, P4, P5, m6, M6, m7, and M7.

The diatonic template (in this case, the "white-note" collection), ordered as a "cycle of fifths," is given in the example's left-hand column. Although conventional letter notation is used for the sake of simplicity, readers familiar with my *JMT* articles will know that any such letter-name is equivalent to an *integer-pair* (for example, C = (0, 0), D = (2, 1), etc.); the same holds for diatonic intervals. (The example and its discussion are simplified in several other respects as well that, for present purposes, are not crucial.) The uppermost row in the example represents the twelve pitch classes, also ordered "by fifth" (that is to say, by pitch-class interval 7). Readers familiar with my 1990 *Music Perception* article will know that I regard pitch classes and scale steps as distinct entities. Pitch classes are *perceptual* (that is, psychoa-

Constellations: Studies in the Perception of Tonal Centricity that the author is inclined to music theorizing is likely to be disappointed." The purpose of this remark is to caution the reader that, in the work under review, appearances can be misleading. Although he concedes, "there is a point in what Agmon says," strangely enough, in citing my remark Huovinen excises the reference to the title of his book.

13 Huovinen (2005, note 4) accuses me of "carefully picking out" from his extensive bibliography "one of the rare references to a *non*-scientific practice manual," commenting, "of all theory texts Huovinen seems to be fond of [it]" (Agmon 2004, note 1). However, there was no need to "carefully pick out" Huovinen's reference to Gorow (2000) from his handful of references to music-theory texts. Someone who begins his study by criticizing such classic texts as Aldwell & Schachter, 1989 (see Huovinen 2002, vi, 8), should not be surprised that his approval of Gorow (2000) on p. 42, is so very conspicuous.

14 Some of my publications (e.g., Agmon 1989 and 1990) are based on my dissertation, yet none to date has dealt with its ventures into the chromatic and enharmonic realms. Unlike Huovinen, I have neither published my dissertation, nor sent it to a "major academic journal" for review.

Diatonic Template \ Pitch-Classes	5	0	7	2	9	4	11	6	1	8	3	10
F	F	C	G	D	A	E		G♭	D♭	A♭	E♭	B♭
C	F	C	G	D	A	E	B		D♭	A♭	E♭	B♭
G	F	C	G	D	A	E	B	F♯		A♭	E♭	B♭
D	F	C	G	D	A	E	B	F♯	C♯		E♭	B♭
A	F	C	G	D	A	E	B	F♯	C♯	G♯		B♭
E	F	C	G	D	A	E	B	F♯	C♯	G♯	D♯	
B		C	G	D	A	E	B	F♯	C♯	G♯	D♯	A♯

EXAMPLE 1. *Diatonic pitch-class representation. After Agmon 1986, Fig. 5–2 (p. 189).*

coustical) entities, whereas scale steps (and diatonic intervals, for that matter) are more abstract mental entities to which I refer by the term "cognitive."

With exactly seven exceptions, at the intersection of any row with any column of Example 1 one finds an element of the white-note collection, or a chromatically altered element of that collection. This element is the "cognitive," diatonic representation of the given pitch class (that is, the pitch class that labels the given column), *as computed relative to the given scale-step of the mental template* (the scale step that labels the given row). For example, the diatonic representation of pc 8 *relative to C* is A♭, and is computed as follows. In equal temperament, the *sound* of C is pitch-class 0. The *pitch-class interval* from pitch-class 0 to pitch-class 8 is 8. There exists *exactly one* member of the *diatonic-interval* template, namely m6, the sound of which in equal temperament is pitch-class-interval 8. Finally, C transposed by m6 is A♭. By comparison, the diatonic representation of pitch-class 8 *relative to E* is G♯. This is because (1) the pitch-class interval from pitch-class 4 (the sound of E in equal temperament) to pitch-class 8, is 4; (2) the unique member of the diatonic-interval template the sound of which in equal temperament is pitch-class interval 4, is M3; and (3) E transposed by M3 is G♯. Note that in relation to D, pitch-class 8 *has no diatonic representation*. This is because the diatonic-interval template is assumed to contain exactly eleven members, and among these, the sound of none, in equal temperament, is pitch-class interval 6. As the reader can see, any pitch-class from the set {0, 2, 4, 5, 7, 9, 11} has exactly one diatonic representation, namely {C, D, E, F, G, A, B} respectively (assuming, of course, the white-note collection as mental template). By contrast, any pitch-class from the set {1, 3, 6, 8, 10} has *exactly two* diatonic representations, namely {C♯/D♭, D♯/E♭, F♯/G♭, G♯/A♭, A♯/B♭} respectively. Overall, against twelve distinct pitch classes, there exist exactly *seventeen* (7+2·5) distinct diatonic representations.

In my review of Huovinen's book (141–42), I suggest that subjects of Experiment 5 may have represented the pitch-class set {9, 0, 1, 3, 4, 7} diatonically (by my definition) relative to the "two-sharp" diatonic template as {A, B♯, C♯, D♯, E, G}, thus challenging Huovinen's claim (2002, 257) that his subjects have not heard this set diatonically (by his definition) *at all*. Since *relative to the two-sharp template* pitch-class 0 can be diatonically represented as either B♯ *or C*, and similarly, pitch-class 3 as either D♯ *or E♭*, my suggestion obviously involves an implicit assumption. Listeners, I assume, prefer to represent a given pitch-class set diatonically in such a way that for any chromatically altered scale-step there exists a *non*-altered scale-step in the representation, such that the diatonic interval from one to the other is m2 or M7. Borrowing a familiar term from music psychology, I shall refer henceforth to any chromatically altered scale-step in a given diatonic representation of some pitch-class set that satisfies this constraint as *anchored*.[15] For example, in my suggested hearing of {9, 0, 1, 3, 4, 7}, B♯ is anchored to C♯, and D♯ to E (anchoring can also take place in the "descending" direction).

What does Huovinen mean in his reply when he characterizes *his* understanding of my theory of diatonicism as "transformational"? Huovinen, who thinks exclusively in terms of twelve-tone theory, means that I take (say) a six-element subset of some member of Forte's set-class 7–35, a subset conceived as a subset of a diatonic scale, and then I transpose (say) two of its elements by pitch-class interval 1 or 11, a transposition conceived as chromatic alteration. The only constraint on this transformation is that the number of elements of the *transformed* subset cannot be smaller than that of the original one. Huovinen shows that the number of all such transformed subsets is relatively large; therefore, he argues, my theory is vacuous, or at least nearly so.

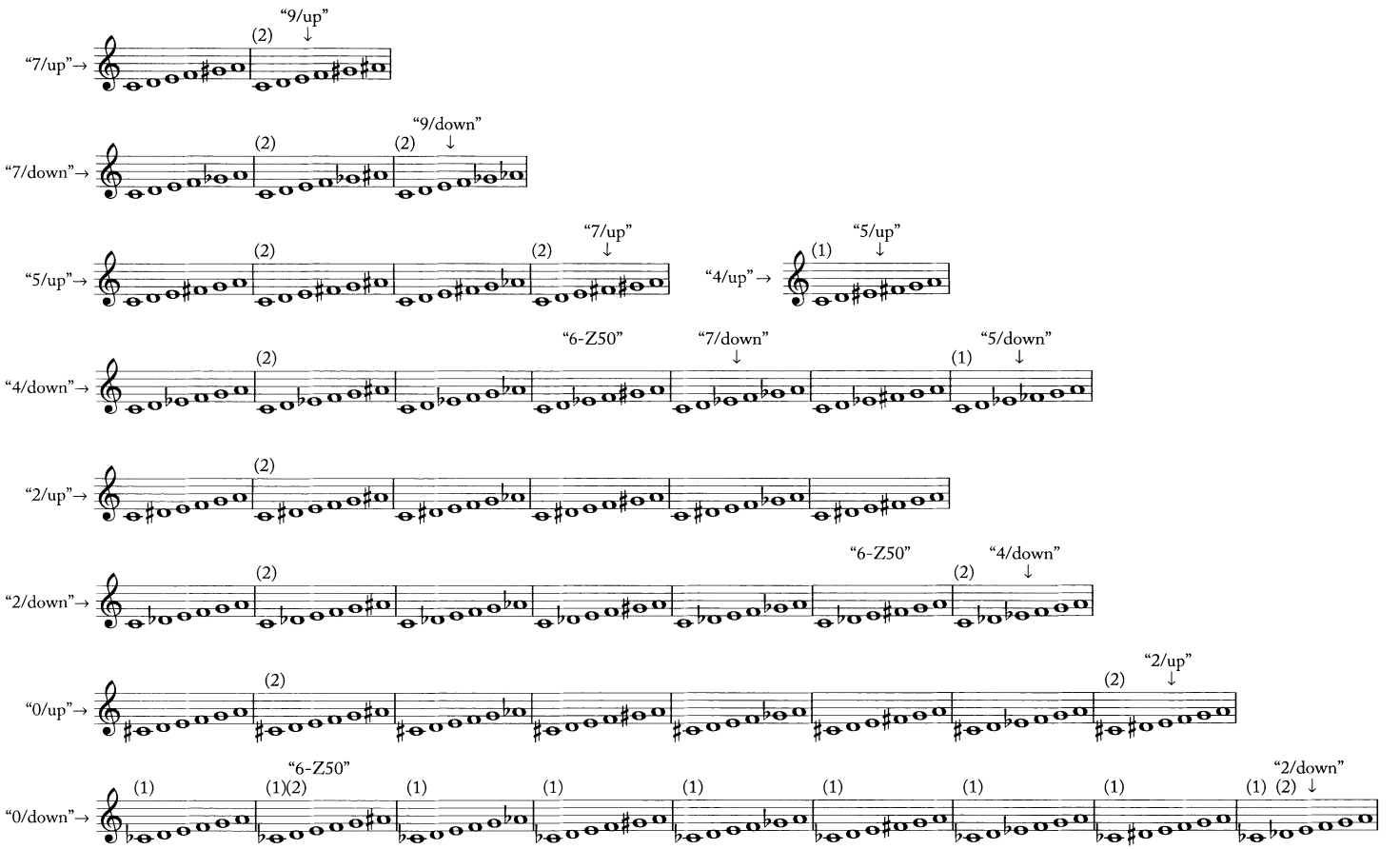
15 The term "anchoring," though not exclusively in the sense offered here, was introduced in Bharucha (1984).

Example 2 is *my* representation of the 31 distinct "transformations" of the pitch-class hexachord {0, 2, 4, 5, 7, 9} that Huovinen believes can be represented diatonically according to my theory. Although pitch-class set labels are replaced, for obvious reasons, with staff-notated sets of notes, Example 2 is arranged similarly to Huovinen's Example 6, for easy comparison. To save space, however, empty row-column intersections are omitted, and therefore the position of some transformations is slightly shifted relative to their position in Huovinen's corresponding example.

Note that eleven out of Huovinen's 47 representations contain elements that do *not* appear in Example 1 (there are more representations than there are pitch-class sets, since some sets are represented more than once); these representations are indicated in Example 2 with a numeral "1" in parentheses. For example, any representation in the "0/down" row has an impossible C♭. Thus, this entire row can be eliminated.[16] One can similarly eliminate the representation in row "4/up" and column "5/up" with its impossible E♯, as well as the "4/down-5/down" representation with its impossible F♭.

All representations labeled in Example 2 with a numeral "2" in parentheses can be eliminated for a different reason: they contain an unanchored chromatically altered scale step. For example, all representations in column "9/up" have an unanchored A♯; similarly, in "2/down-4/down," E♭ is unanchored. (I do not consider the C♭'s in row "0/down" as "unanchored," since a C♭ representation of pitch-class 11 relative to the white-note diatonic template is impossible in the first place.) In all, 22 of Huovinen's 47 representations can be

16 Huovinen does not inform us whether the original set {0, 2, 4, 5, 7, 9} is conceived, specifically, as a subset of the "white-note" diatonic collection, or the "one-flat" collection. If one assumes the one-flat diatonic collection rather than the white-note collection as template, C♭ becomes available; however, A♯ becomes *un*available. Thus, the argument holds in general.



EXAMPLE 2. *Diatonic representations of the "transformations" of {0, 2, 4, 5, 7, 9} given in Huovinen's (2005) Example 6. Impossible representations are indicated with (1) or (2).*

eliminated.[17] Since the 22 include 14 representations that exhaust all instances of *seven* distinct pitch-class sets, this means that only 24, not 31, out of the eighty transpositional hexachord types, can be *represented diatonically* as transformations of the hexachord {0, 2, 4, 5, 7, 9}.

When Huovinen states "when we carry out a similar procedure for all hexachord subsets of the diatonic scale, it turns out that no less than 61 of all the 80 transpositional hexachord types can be transformed from them in the manner that was demonstrated in Agmon's example," he makes no less than three mistakes. First, as we have seen, his numbers for the individual hexachords are exaggerated. Second, he adds together transformations of *different* hexachords, thus rendering his own notion of "transformation" vacuous. Finally and most importantly, Huovinen's trivial notion of transformation is *not* equivalent to my notion of diatonic pitch-class representation. For example, the "6–Z50" transformation has two *distinct* (that is, transpositionally *non*-equivalent) "legitimate" representations (see Example 2). Moreover, since one representation of a given transformation can be "legitimate" (e.g., the "4/down-7/up" or "2/down-5/up" representations of 6–Z50), and another "illegitimate" (e.g., the "0/down-9/up" representation of the same set-class), *even had all 80 transpositional hexachord types been represented in Example 2*, this would not have posed a threat to my theory. The distinctions that my theory makes are simply far more refined than what twelve-tone theory allows, its usefulness as a methodological tool notwithstanding.[18]

Huovinen concludes his second "argument" for the mental reality of diatonicism by urging "professor Agmon" "to do what is usual in science: to show how some rival hypothesis bestows higher probability on the observed phenomenon than the one he attacks." However, as his first "argument" amply demonstrates, the misunderstanding of the nature of the scientific endeavor is Huovinen's, not mine. Science, once again, is about theories, and the constant testing of *whether they withstand falsification*.[19] As I point out on p. 142 of my review, "as far as I can see, there is nothing in Huovinen's findings to falsify such a theory" (i.e., my theory of "what it takes to hear music tonally"). If Huovinen believes otherwise, he should certainly speak out; however, he is not entitled to pass his trivialization of other people's theories as falsification.

Lest readers get the false impression that I do *not* believe that empirical science, in some sense, "is based on observation," let me conclude my response to Huovinen by addressing this issue.

Note that the statement "empirical science is based on observation" does not imply that observations *imply* theories, in any logical sense of the term. As Popper has taught us,

17 This number can be reduced further by eliminating all representations with either G♭ or A♯. As may be seen in Example 1, the G♭ (respectively, A♯) diatonic representation of pitch-class 6 (respectively, pitch-class 10) is computed as such *in relation to only one member of the diatonic template*, and is therefore inherently weak. Note that when I say that a given representation in Example 2 "can be eliminated," this holds *only in relation to the given mental template*, in this case, the "white-note" collection {C, D, E, F, G, A, B}.

18 Since Huovinen does not seem to understand Popper's notion of "unfalsifiability" (see note 5), when he finds to his delight that the theory that he attributes to me implies that "nearly everything can be 'heard diatonically'," he fails to realize that the flaw in question is logical rather than empirical. As a result, his celebration (beginning with "Does this mean that 76.25% of all hexachords" and ending with "the explanation of experimental results") is beside the point. This includes, in particular, his reference to the remark on p. 141 of my review, "Huovinen . . . assumes what may conveniently be tested, ruling out a priori alternative hypotheses," which has nothing to do with the problem at hand.

19 Given the unimaginably vast number of *possible* "observed phenomena" in the universe (e.g., that intelligent yet hostile beings from a distant galaxy are aiming right now a highly sophisticated star-destroying weapon at the sun), a "probability" for any *particular* observation (e.g., that the sun will rise tomorrow), unless it is indistinct from zero, simply cannot be defined. It is unfortunate that Huovinen characterizes my criticisms as "attacks." If science has any "method," it is the method of criticism, self-criticism included.

science is deductive, not inductive. Huovinen misunderstands, of course, this crucial point, for in footnote 1 he states that "this sort of 'mental reality' [of how the structure of the diatonic scale is internalized as a mental template that organizes the listener's perception] does not imply and, more importantly, *is not itself implied by* the fact that the person is able to intellectually describe the scalar structure or to use it in theoretical arguments" (emphasis added). The "mental reality" to which Huovinen refers is simply not "implied by" *any* type of observation, including his own, "methodologically rigorous music-psychological experiments" (see Huovinen 2005, note 4).

Given this state of affairs, a practical question naturally arises: *which* of the *potentially infinite* observations upon which a theory "may be based" (including, by the way, such observations as Huovinen dismisses in the statement just cited), is the scientist to prefer? In many of the natural sciences, the answer to this question seems obvious enough: the preferred observations are those obtained by controlled laboratory experimentation. However, while it is clear why an immunologist, say, may need sterile laboratory conditions to study the interaction of germs with the human body's immune system, it is not at all clear why someone interested in the "mental representation of tonal music," at least in the present, preliminary state of our understanding of what this notion means, will "close his eyes," so to speak, to any evidence *other* than that which his (ostensibly!) sterile laboratory produces. Popper, as usual, is very clear on this point:

> I dislike the attempt, made in fields outside the physical sciences, to ape the physical sciences by practising their alleged "methods"—measurement and "induction from observation." The doctrine that there is as much science in a subject as there is mathematics in it, or as much as there is measurement or "precision" in it, rests upon a complete misunderstanding. On the contrary, the following maxim holds for all sciences: Never aim at more precision than is required by the problem in hand (Popper 1956, 7).

Consider again my claim that subjects of Huovinen's Experiment 5 *may* have heard the pitch-class set {9, 0, 1, 3, 4, 7} in terms of the "two-sharp" diatonic template (Agmon 2004, 141). D and B, Huovinen counters, "the obvious candidates for a tonal center in the two-sharp diatonic collection," "only received 2.3% and 2.9% of the responses, respectively." Well, perhaps had Huovinen's experiments been less prejudiced *against* out-of-stimulus TCs in the first place, these percentages would have been higher. How can Huovinen even *pretend* that these percentages are meaningful, when he himself instructed his subjects "to listen for a suitable TC *in the pitch strings*"?[20]

However, the point I wish to make is not that Huovinen is a sloppy experimenter, and therefore his results are "contaminated"; this will *always* be the case, no matter how carefully he plans and conducts his experiments. My point is rather that for some strange reason Huovinen is *convinced* that he needs the equivalent of a 200-inch telescope, when all he really needs is the naked eye coupled with some good-old common sense. How much "precision" does it take to establish that {9, 0, 1, 3, 4, 7} *can* be heard as a chromatically embellished dominant-seventh in D?[21]

20 Huovinen 2002, 108, emphasis added; see also p. 248. Indeed, it is surprising that Huovinen's subjects chose D (or B) as TC at all, given that the stimulus material was constructed in such a way that encouraged the hearing of two diatonically unrelated triads, a diatonic representation of {0, 3, 7}, and a diatonic representation of {9, 1, 4}. Ironically, Huovinen's prejudice against out-of-stimulus TCs stems directly from his *obsession* with "the scientific method." For further discussion see Agmon 2004, 139–40 (note 13 in particular).

21 Huovinen (2005) believes that "the D major reading would be rather fantastic in the light of my other experiments where it was found that pitch classes becoming 'sandwiched' between two other pitch classes are largely avoided as tonal centers. . . ." However, the D here is *not* "sandwiched," let alone "*doubly* sandwiched between two upper and two lower neighbors" as Huovinen—who apparently does not grasp the difference between D♯ and E♭—states. On the other hand, one of the most common chromatic progressions by which a *minor* tonic, in particular, is established—the evidence of Huovinen's experiments notwithstanding—is ♭$\hat{2}$–♯$\hat{7}$–$\hat{1}$.

I sincerely hope that Huovinen will not try to protest at this point that introspection is an utterly "unscientific" method of data collection. He certainly did *not* demonstrate that his own "methods" are any more objective, let alone that they lead to deeper insights concerning the mental representation of tonal music.

In closing, then, whatever the prospects are for any "fruitful" interaction between music theory and music psychology, the path that Huovinen follows is a sure dead-end. The final word in this sad tale, however, should be Popper's:

> Objectivity is not the result of disinterested and unprejudiced observation. Objectivity, and also unbiased observation, are the result of criticism, including the criticism of observational reports. For we cannot avoid or suppress our theories, or prevent them from influencing our observations; yet we can try to recognize them as hypotheses, and to formulate them explicitly, so that they may be criticized.[22]

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22 Popper 1956, 48; originally in parentheses.