

**Mark DeBellis.** *Music and Conceptualization.* Cambridge: Cambridge University Press, 1995. 163pp.

**“Hearing is Believing?”**

*Music and Conceptualization* is a deceptively slim volume of six chapters. Although the last three appeared in previous publications in slightly different form, the book as a whole follows a coherent line of argument and investigation.<sup>1</sup> At first blush, DeBellis's main audience would appear to be readers of analytic philosophy; his prose style is philosophical in tone and he cites more philosophers than he does music theorists and musicologists. Also, there are relatively few musical examples, and those that do appear are seemingly simple music-analytic statements like “I hear that chord as a dominant” (more on the deceptive simplicity of such statements in a moment). *Music and Conceptualization*'s strongest repercussions, however, are for the music theory and musicological community. For DeBellis has lobbed the academic equivalent of a neutron bomb into the midst of the discipline of music theory. Unlike its military counterpart BeBellis's bomb has the reverse tactical effect: it destroys most of the theoretical edifices, but leaves their partisans standing. For when we have finished digesting his arguments, we are left with our conceptualizations about music, but no longer have the comfort of believing that they represent a veridical account of the music. Indeed, upon finishing *Music and Conceptualization* it is no longer clear what exactly a “veridical account of musical structure” could or would be. While DeBellis's initial intent may have been to use a few examples from music (and music theory) to attack various claims in philosophy (which he has done with varying levels of success), his end result raises a number of hard questions for music theory and analysis.

The first chapter, on “Hearing Ascriptions,” introduces the reader to the kind of music-analytical claims that DeBellis is worried about, claims which take the form “Some listener *S* hears musical event *x* as an *F*” (for example, “I hear that pitch as a dominant”). DeBellis makes a crucial distinction at the outset between these sorts of *hearing* ascriptions versus

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<sup>1</sup> Chapter 4 first appeared in *Current Musicology* 55 (1993), Chapter 5 in *The Interpretation of Music: Philosophical Essays*, ed. Michael Krausz (Oxford: Oxford University Press, 1993), and Chapter 6 in *Philosophy and the Arts*, Midwest Studies in Philosophy, edited by Peter A. French, Theodore E. Uehling, Jr., and Howard K. Wettstein, vol. 16 (South Bend, Ind.: University of Notre Dame Press, 1991). Some of the central ideas of Chapters 2 and 3 were incorporated in his paper on “Conceptual and Non-Conceptual Modes of Music Perception” which was presented at the 1995 annual meeting of the Society for Music Theory.

*analytical* or *structural* ascriptions of the form “*x* is an *F*” (for example, “that chord *is* a dominant”) or “Passage *P* has some property *X*” (for example, “that phrase is a prolongation of  $\frac{3}{2}$ ”). The ramifications of this distinction are explored in the following two chapters, where DeBellis discusses various ways of musical hearing by experienced or “theory-laden” listeners who are readily able to make hearing ascriptions along the lines of “I hear that pitch as a dominant” versus their naive or untutored counterparts who cannot.

This raises the question: “Do musically-trained listeners hear music differently than their untrained counterparts?” The simple answer is “yes,” but what exactly is the nature of this difference? Others have noted that naive listeners are nonetheless aware of musical syntax and structure, as they can recognize wrong notes, feel appropriate senses of closure and non-closure, characterize the music’s expressive character in a nuanced fashion, and so forth.<sup>2</sup> But rather than distinguish between “intuitive” versus “conscious” experiences of music, DeBellis rightly notes that it is not different “levels of hearing” (whatever that might mean) which differentiate naive versus analytically-skilled listeners, but different kinds of *conceptualization* (p. 6). Different kinds of conceptualization give rise to different kinds of hearing ascriptions. Through an examination of these ascriptions we may gain some sense of the differing perceptions and conceptions of music which underlie them.

When one makes a hearing ascription such as “I hear that chord as a dominant,” one is not only describing one’s perception; one is also voicing a perceptual belief (p. 26). That is, in making such a statement (assuming one is not deliberately lying), one is saying two things, (a) that one perceives certain tones to be present, as well as (b) what one believes those tones are doing in terms of their functional role within the musical texture. Thus, when I say “I hear that chord *as* a dominant” it thus follows that I also believe “that chord *is* a dominant” (note the grammatical difference in the two statements). For the theorist, hearing is believing. But what if I am unable to make a hearing ascription of the form “I hear that chord as a dominant”? What belief(s) do I hold about the music in that case? In the second chapter, “Musical Hearing as Weakly Non-Conceptual,” DeBellis argues that what is perhaps going on is that while the naive

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<sup>2</sup> See, for example, Peter Kivy, *Music Alone* (Ithaca, N.Y.: Cornell University Press, 1990), Fred Lerdahl and Ray Jackendoff, *A Generative Theory of Tonal Music* (Cambridge: MIT Press 1983), Malcolm Budd, “Understanding Music,” *Proceedings of the Aristotelian Society Supplementary Volume* 56 (1985): 233–48, and Leonard B. Meyer, *Emotion and Meaning in Music* (Chicago: University of Chicago Press, 1956).

listener may *hear* that chord as a dominant, she does not know (and hence cannot believe) that it *is* a dominant. Note that our theory-laden listener and our naive listener both hear the same thing (namely the chord-as-dominant); it is just that the theorist knows what it is she hears while the naive listener knows not.

It is important to note that under the framework of weakly non-conceptual hearing, the naive listener and the theorist *do* have different beliefs about what they hear (even though they hear the same thing). DeBellis illustrates this difference by presenting an example which goes back to Frege. Consider the two sentences:

- (1) The Evening Star is in the sky.
- (2) The Morning Star is in the sky.

These two sentences “present different information values, since it is possible to believe (1) and doubt (2)” (p. 30). That is, if you don’t know (due to astronomical ignorance) that “the evening star” and “the morning star” both are ways of referring to the planet Venus, then you are likely to accept (1) upon seeing Venus in the sky in the evening, and reject (2). DeBellis then reminds us that while the *information* denoted by (1) versus (2) remains the same, the *modes of presentation* for that information differ (p. 30). The upshot for our naive (weakly-nonconceptual) listener versus our theorist is not that the music (read “Venus”) is different for the two, but that the mode of presentation of that information is different. By an appeal to the epistemological level of mode of presentation DeBellis is able (as others have done) to have his listeners hear the same thing, but have different conceptions of it.

Unfortunately, DeBellis never makes clear what the mode of presentation of a given note or chord is to the naive listener. Consider again the Venusian example. Our astronomer looks up and sees Venus; our music theorist listens and hears a Dominant. Our naive skywatcher looks up and sees the Evening Star; our naive listener listens and hears . . . (?). DeBellis does not finish the analogy. I would suppose that this missing mode of presentation could be: “the dissonant chord that usually comes right before the last chord of the phrase.” In which case the analogy would be that the naive listener hears “pre-cadential dissonant chord #1” versus “dissonant chord #2” while the theorist recognizes that both are  $V^7$  chords. But there are of course problems here, because chord #1 might be a  $V^9$ , while the other just a  $V^6$  with a suspension (since “the dissonant chord that usually comes before the last chord of the phrase” is a rather fuzzy set), in which case there really are two different chords, albeit in the same functional category. As we shall see, DeBellis addresses this problem in the

next chapter. But there is another problem with DeBellis's weakly non-conceptual account of naive listening which should be noted before we move on.

Inherent in the morning star versus the evening star example is an essentialist worldview: there really is just *one thing* (that is, Venus) which gives rise to the two phenomena. I hasten to add that this is the proper epistemic perspective for doing good astronomy. But there is a nominalist worldview to be heard from as well. For of course the morning star does not shine in the evening—that is precisely the salient difference in the appearances of Venus. Music is not astronomy; listening to music is not quite the same thing as stargazing. In a musical context a nominalist worldview gains plausibility. Let us compare essentialist versus nominalist frameworks for DeBellis's hearing ascription(s). For a musical essentialist, the hearing ascription "I hear *x* as a dominant" means that she hears that chord as one of manifold instantiations of a particular *w*-structure. In philosophical parlance, the listener's sense of the identity of a particular sonic token flows from his or her knowledge of an abstract type to the particular chord at hand (at ear?). DeBellis takes this position explicitly (p. 17). For a musical nominalist, however, the hearing ascription "I hear *x* as a dominant" means that this particular instance is judged to be sufficiently similar to all the other instances in a collective set of chords, and it is that set which is labeled "dominant." Thus to the nominalist, "dominant" means something like "the sum of all contexts which map the term *dominant* to the same locus in my knowledge-structure of music." Note that for the nominalist the identity of a particular sound-instance flows from the particular instance to the larger musical category in which it is placed. In the case of a theory-laden listener this category might well define a particular chord or scale step; for our naive listener this could be the "dissonant chord that comes right before the end of the phrase."<sup>3</sup> Note that a nominalist approach does not negate DeBellis's central argument in chapter two: naive listeners and theorists could both proceed in nominalist fashion, and simply be making different sets of connections. Indeed, a nominalist could characterize that difference by saying that the theorist has a more fine-grained set of *relata* in his mental representation of music.

In chapter three, which is not surprisingly titled "Musical Hearing as Strongly Non-Conceptual," DeBellis reveals his own reservations with the

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<sup>3</sup> For a defense of the nominalist approach to musical structure see Nelson Goodman, *Languages of Art* (Indianapolis: Hackett, 1976); for a critique of Goodman see Lydia Goehr, *The Imaginary Museum of Musical Works: An Essay in the Philosophy of Music* (Oxford: Clarendon, 1992).

argument presented in chapter two. If naive listening were only weakly non-conceptual, then we could happily maintain that naive listeners and theorists both hear the same music but give different reports of that experience given their different degrees of musical knowledge. In chapter three, however, DeBellis gives a convincing argument, informed by research in music perception and cognition, that naive listeners and theorists simply do not hear the same music.

To illustrate the gap between these two kinds of listeners DeBellis gives the following familiar example:

**Example 1.** The “Star-Spangled Banner” (after DeBellis’s example 3.1)



Here we have the opening measures of the Star-Spangled Banner, a tune that is familiar to many theorists and naive-listeners alike. As such, both kinds of listeners have mental representations of the melody, since both can (we shall assume) sing it correctly, recognize wrong notes, and so forth. A theorist, moreover, is able to relate the tones of the melody to each other in a particular way, namely in terms of their scale-step attributes. So the theorist’s mental representation of this tune includes the component:

“sol–mi | do–mi–sol | do, mi–re | do–mi–fi | sol . . . “

This representation allows the theorist to recognize that the first and last notes of the phrase are the same; in both cases the theorist can make the hearing ascription “I hear that note as *sol*.” But the naive listener cannot do this, and so, according to DeBellis:

An ordinary, untrained [i.e., naive] listener may easily verify by armchair methods that he cannot reliably distinguish instances of  $f^1$  [i.e., *sol*] from non-instances. . . . the listener cannot reliably tell, in general, when two pitches of the melody are the same—as on the italicized words in “*Oh-oh* say can you see, by the dawn’s early *light*”—and when they are different.” (p. 62)

What does it mean, then, that naive listeners cannot recognize when certain pitches recur in a melody? Under a weakly nonconceptual mode of hearing we assumed that both theoretically-trained and naive listeners recognize the recurrence of identical musical objects, even though the latter group cannot cast this recognition in terms of a hearing ascription. But

under a strongly non-conceptual mode of hearing it means more than just that they cannot make a pair of hearing ascriptions of the sort "I hear note  $x$  as an instance of pitch  $P$ " and "I hear note  $y$  as an instance of pitch  $P$ ." Rather, it means that naive listeners do not even notice the melodic recurrence *at all*. This failure of recognition short-circuits the possibility of any beliefs about the identity of pitches  $x$  and  $y$ , and thus demarcates a fundamental gulf between our two classes of listeners. Hearing, then, is not believing, at least not for the naive listener.<sup>4</sup>

I would offer two small counter-arguments to DeBellis at this point. The first is that DeBellis strongly relies on the chroma theory of pitch representation for framing a sense of "what knowing the tonal identity of a particular pitch" is in the mind of the theoretically trained listener.<sup>5</sup> Under this theory there is a unique location in our mental "pitch-class space" for each pitch-class in a given key. So, for example, in B $\flat$ -major, all instances of pitch-class F map to the same location in terms of their chroma position. But are all occurrences of F phenomenally and perceptually the same? Indeed, are all instances of *sol* really the same in example one? (Compare F as the fifth of the anacrustic tonic versus F as the root of a V chord which is melodic goal of the phrase, prepared by a secondary dominant.) DeBellis may be cornered here by psychological theories of pitch perception that are based on crude readings of music theory by "musically naive" psychologists. My second counter-argument is to point out that naive listeners are, of course, able to recognize thematic and (in many cases) motivic recurrence. Thus on some level both theoretically-trained and naive listeners perceive and can report "I hear those notes as THEME 1" and "I hear these notes as THEME 1, too." There may be an analogy to language and linguistic perception here. By definition, competent speakers can recognize word recurrence as well as differentiate words like *date* versus *gate*. However, when confronted with the /d/ versus /g/ consonant sounds in isolation, all one hears are strikingly similar chirps, as it is impossible to distinguish the individual consonant phonemes outside of their morphemic environments. The moral here is that just because one cannot distinguish structural elements on a micro-level it does not follow that one does not perceive those elements. In a similar fashion, while

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<sup>4</sup> William Thomson, in his article "The Harmonic Root: A Fragile Marriage of Concept and Percept," *Music Perception* 10, no. 4 (1993), makes similar remarks about "the dramatically different results obtained from [musically] trained and untrained subjects in empirical studies of interval perception . . . Those who have not endured some objectification [due to music-theory training] of pitch perception cannot be expected to discriminate among sensory inputs they do not 'hear'" (p. 405).

<sup>5</sup> See DeBellis, pp. 11–13, and for additional background, David Butler's *The Musician's Guide to Perception and Cognition* (New York: Schirmer, 1992), 52–55 and 115–20.

naive listeners may not be able to identify individual tones, they may nonetheless have a sense of their import in being able to recognize and contrast larger musical shapes.

These two counter-arguments, while tempering DeBellis's position regarding strongly non-conceptual listening, do not eliminate it. For DeBellis truly seems to have found a level of structure (and hence a level of hearing ascriptions) on which theoretically-trained listeners perceive things that untrained listeners simply do not. This connection between theoretical training and perception then leads us to Chapter Four, "Is there an Observation-Theory Distinction in Music?" which is the centerpiece of the book.

Chapter Four begins with a parable first told by the philosopher of science Norwood Russell Hanson: "[I]magine Tycho Brahe and Johannes Kepler standing on a hill, watching the sunrise. Tycho believed the sun moved around a fixed earth; Kepler held a heliocentric conception. . . . Do Kepler and Tycho see the same thing in the east at dawn?"<sup>6</sup> At issue here is the extent to which one's beliefs about the world can and do influence one's perception of the world. Implicit in Hanson's parable is the idea that what you believe does influence what you see: "Seeing is a 'theory-laden' undertaking."<sup>7</sup> Thus as you acquire new and different beliefs about the world, your fundamental perception of the world changes. The contrary position is that our perception(s) of the world and the beliefs inferred from those perceptions (as well as the perceptions and arguments of others) remain separate. In other words, observation is "theory-neutral." While philosophers have long debated this issue, recently it has been the subject of an ongoing exchange between Paul Churchland (who argues for theory-laden perception) and Jerry Fodor (who argues for theory-neutral, or to use his term, "encapsulated" perception).<sup>8</sup> DeBellis uses their exchange as a point of departure for his own contribution to this debate, which is to engage these philosophical issues in the context of music theory and musical perception. As one would expect from the previous chapters, DeBellis is sympathetic with Hanson and Churchland, and

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<sup>6</sup> Norwood R. Hanson, *Patterns of Discovery* (Cambridge: Cambridge University Press, 1958), 5, cited in DeBellis, p. 80.

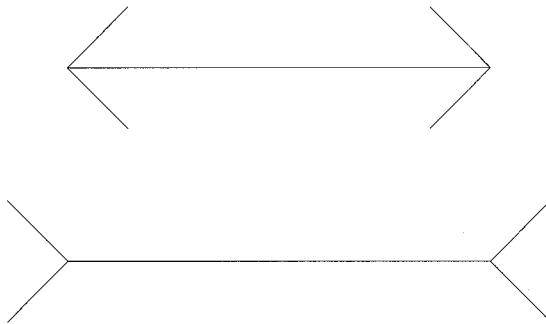
<sup>7</sup> Hanson, *Patterns of Discovery*, 19, cited in DeBellis, p. 80.

<sup>8</sup> DeBellis specifically cites Paul M. Churchland's *Scientific Realism and the Plasticity of Mind* (Cambridge: Cambridge University Press, 1979) and "Perceptual Plasticity and Theoretical Neutrality: A Reply to Jerry Fodor," *Philosophy of Science* 55 (1988): 167-87, as well as Jerry A. Fodor's *The Modularity of Mind* (Cambridge: MIT Press, 1983) and the articles "Observation Reconsidered" and "A Reply to Churchland's 'Perceptual Plasticity and Theoretical Neutrality,'" both reprinted in Fodor's *A Theory of Content and Other Essays* (Cambridge: MIT Press, 1990).

argues (as we shall see) that conceptual listening is theory laden; as a result the bulk of the chapter is a critique of Fodor.

Very briefly, here is Fodor's argument for theory-neutral observation, along with a particular example. Most perceptual systems (which Fodor calls "input systems") are *modules*, which means that they have a certain set of properties: they are domain specific (different modules for different sensory modes, along with specialized modules for language, certain aspects of visual processing, etc., including one for music); they are fast and mandatory in operation (for example, we can't but help instantly see the characters RED as a word, rather than as a series of shapes; likewise, we hear speech sounds as *words*, rather than as mere sounds); we have limited access to modules from higher levels of cognition; modules break down in characteristic ways (as evidenced by cases of neurological aphasia); their outputs are conceptually "shallow;" and they are informationally encapsulated.<sup>9</sup> Fodor gives the following example to illustrate modularity with respect to visual perception:

**Example 2.** Müller-Lyer Optical Illusion



In the familiar Müller-Lyer illusion the two line segments appear to be of different lengths, even though they are in fact the same, as a moment with a ruler will confirm. Yet even though we "know" and believe that these two lines are the same length, we still see them as different. For Fodor this is robust evidence that cognitive systems are modular, as your higher-level knowledge ("the lines are the same length") does not change your perception.

DeBellis goes about critiquing Fodor's claim in the following way. First,

<sup>9</sup> DeBellis, p. 86; see also Fodor, *Modularity of Mind*, 47–101.



DeBellis rightly notes that the output of perceptual systems (as described by Fodor) has a semantic content: it is about features or states of the world and, as such, is couched in a certain “vocabulary” relevant to the perceptual domain (p. 88). For example, the output of the visual module in the Müller-Lyer illusion involves the content that “the bottom line is longer than the top line” (that this content is false is precisely what makes this an optical illusion). Thus DeBellis asks, “Is that vocabulary radically malleable in response to the theories we hold?” (p. 89). DeBellis’s claim is that in the case of music, it is. Persons who have had some ear-training in the context of courses in music theory will produce hearing ascriptions which employ theoretical terminology along the lines of: “I hear that note as a leading tone,” or “I hear that chord as an “upside down” German sixth, with #4 in the bass and ♭6 in the soprano,” to give a few examples. And, says DeBellis, such a theoretically-indoctrinated listener:

is apt to respond . . . spontaneously and without much ratiocination . . . she hears a piece under a certain music-theoretic description and will give that description in describing what she hears. There is simply no principled basis on which to say that trained listeners do not hear chords as tonics and dominants in as full-blooded a sense as that in which ordinary perceivers see tables and chairs. . . . (p.103)

Before attending music theory classes, we did *not* make such hearing ascriptions, nor did we employ music-theoretic vocabulary as product of our musical perception. Acquiring music-theoretic knowledge seems to change in the vocabulary of our perceptual systems. As DeBellis rightly notes:

[F]or most people [elementary ear training] is *work*: it is not a trivial matter of learning to apply labels, but entails the development of new perceptual abilities . . . . If ear training did not consist in the development of such concepts, but consisted (say) merely in the association of already-possessed perceptual concepts with verbal labels, then it would be as easy as learning to type. . . . (pp. 64–65)

Therefore, if music perception involves a music module, then it cannot be encapsulated in the way Fodor would want it to be.

DeBellis scores many points contra Fodor, but I have a number of questions regarding some of the moves DeBellis makes in mounting his attack. First, as the quote given above rightly notes, ear training is not the same as acquiring a theory; learning to hear melodies under the rubric of movable do (which translates “three blind mice” to “mi-re-do”) isn’t the same thing as studying *Stufentheorie*. Note how the analogy between Brahe

versus Copernicus is one between the student who knows *Stufentheorie* versus one who does not, or perhaps even between rival theories of tonal function (Marpurg versus Kirnberger?). In other words, Hanson's parable is not about two rival practices of skywatching. This does not mean that DeBellis may well be on to something with respect to the ways that "background" theories may guide our analyses and our perceptions. But in the case of ear training, one could argue that no *theory* is involved here at all—simply a set of strongly reinforced perceptual associations.

Second, DeBellis never really refutes Fodor's case with respect to the Müller-Lyer illusion. If Fodor is wrong, then why *cannot* one learn to see the two parallel lines in the illusion as equal in length? Furthermore, there are aural/musical analogs to optical illusions. To give but one example, there is the aural illusion of endlessly rising (or descending) tones described by Shepard.<sup>10</sup> Shepard prepared tones whose frequency components are composed of a set of octave-related harmonics whose amplitudes are determined by a bell-shaped spectral envelope. Shepard found that when an ordered cycle of tones was played, subjects perceived an ever-ascending series of tones, even though each pitch-class was represented by the same tone (that is, when "C" comes round again at the end of an ascending scale octave it sounds an octave higher, even though it is simply a repetition of the initial tone, etc.). This perception remains even once you understand the nature of the tones and their spectral components. Thus some aural/musical perceptions seem to be encapsulated in a manner analogous to the Müller-Lyer optical illusion.<sup>11</sup>

What then is the nature of the output of the music module? Perhaps rather than saying that its output vocabulary takes the form of hearing ascriptions such as "that chord is a dominant" or "that note is a leading tone," aural illusions such as Shepard's tell us that the semantic content of

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<sup>10</sup> Roger N. Shepard, "Circularity in Judgments of Relative Pitch," *Journal of the Acoustical Society of America* 36 (1964): 2346–53. For a convenient summary and CD illustration of this and other aural illusions see David Butler, *Musician's Guide to Perception and Cognition* (New York: Schirmer, 1992).

<sup>11</sup> For example, see Eugene Narmour, *The Analysis and Cognition of Basic Melodic Structures* (Chicago: The University of Chicago Press, 1990) for extensive discussions of low-level interval perception and the categorical distinction between large versus small intervals. See Van Noorden, "Temporal Coherence in the Perception of Tone Sequences," Ph.D. diss., Technische Hogeschool Eindhoven, The Netherlands (1975), Albert S. Bregman, *Auditory Scene Analysis: The Perceptual Organization of Sound* (Cambridge, Mass.: MIT Press, 1990) and "Auditory Scene Analysis: Hearing In Complex Environments," in *Thinking in Sound: The Cognitive Psychology of Human Audition*, edited by Emmanuel Bigand and Stephen McAdams (Oxford: Clarendon, 1993), 10–36, as well as Butler, *Musician's Guide to Perception and Cognition*, for discussions of auditory stream segregation.

the music module is more along the lines of “this line is ascending” or “that is a large interval” or “that note belongs to that part of the musical texture.” Notice that here we still have a semantic content for the output of music module which can be determinably true or false (again, our *false* sense that a series of Shepard tones is perpetually ascending is precisely what makes it an auditory illusion), and this is a content which we perceive as robustly as we do tables and chairs. DeBellis seems to be talking about another level of description/ascription with respect to the musical object, one which lies above the output of this very shallow module. I hasten to add, though, that DeBellis is quite right not to focus on the “is the music ascending or descending” level of ascription, for music theory does not operate on that level. Moreover, DeBellis is also correct in recognizing that after a music-theory education/indoctrination, we begin to hear musical phenomena on a higher level in a similarly automatic and visceral way. For example, if someone plays the pitches C–E–G–B $\flat$  we can reflexively hear that chord as a V<sup>7</sup> of F (we can sing the following tonic triad, etc.). Moreover, we can be tricked into thinking we are hearing something (that same C–E–G–B $\flat$ -as-V<sup>7</sup>) when it turns out to be something else (that is, a Gr6 of E minor). So our theoretical indoctrination effects this “higher level” (a middle level, let us say) of musical description and ascription.

Here then is a more complex counter to DeBellis’s argument against Fodor. DeBellis notes that the study of music theory/ear training causes listeners to alter the vocabulary of their hearing ascriptions. Such a change of vocabulary is indicative of a change in the operation(s) of the music-perception module, and therefore music perception is not perceptually encapsulated. Q.E.D. This argument is sound. But there is another way to interpret DeBellis’s presentation of these facts. Rather than claiming that the change of vocabulary entails modification of the music module, one could alternatively posit that what ear-training involves is the construction of a new module for processing sounds in very particular (read musical) contexts.<sup>12</sup> Fodor admits that module-construction may involve learning, as in the acquisition of a module which recognizes the phonemes of a

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<sup>12</sup> The auditory context may be even more particular than this. That is, in the context of aural skills classes, under carefully proscribed conditions, music students are able to make listening ascriptions in a properly theory-laden form. DeBellis does not ask whether or not these same students’ listening ascriptions are similarly theory-laden outside of the aural skills classroom, or several years after they have finished their musical education, etc. While there may be parallels between the kinds of ascriptions listening subjects make in psychological laboratories and in music theory classrooms, one should be cautious in generalizing from these specialized contexts to ordinary listening.

particular language, and he implies that some modules require indoctrination: in learning to read, you come to see CAT as a word, rather than a series of shapes.<sup>13</sup> Moreover, we do not learn to read by simply being exposed to texts; we need the help of a reading teacher. Similarly, the reading module does not utterly displace other visual modules (such as shape-recognition modules), but simply delivers the appropriate output in the appropriate context. If we can learn to read (and in so doing build a word-recognition module) why cannot we also learn to hear musical sounds in a somewhat different way? It is not outside of Fodor's own account that such learning can and does occur, and that it precisely consists in the construction of new, domain-specific modules.

Whether we build new modules (pace Fodor), or modify the vocabulary of a given module (pace DeBellis), or if we don't use modules at all (pace Churchland), the upshot of the lack of encapsulation for music theory is serious. For the end result is functionally the same: music-theory training alters musical perception. This raises serious questions in terms of the theory-neutrality of perception in the case of music. As DeBellis notes: "The specific issue raised by this argument is whether trained musical observations ever play an ineliminable role in theory confirmation in music" (p. 108), and "Do theories of music actually rely for the evidential support on theory-laden observations? I maintain that the answer is 'often, yes'" (p. 110).

In other words, music theorists are rarely neutral observers when arguing for and/or defending their theories and analyses. DeBellis claims that the lack of neutral observation need not be a fatal flaw for music theory, for one may still determine the rational bases for theory choice under these conditions (p. 108). But I am not so sure. "Rational" in this case seems to mean a theory which is consistent and which conforms to some sense of internal logic. But the larger problem of having one's observations driving one's theory (and vice-versa) would seem to entail a kind of deep-level epistemic circularity: we hear what our theories lead us to believe we ought to hear, and in hearing that way, come to more strongly believe in the veracity of our theories. To put it another way, if earlier (in Chapters Two and Three) DeBellis seems to say that music theory does not "carve the music at its joints," given the gulf between theory-laden and non-conceptual listening, then it would now seem that in fact music theories tend to generate the very musical skeletons whose joints they then carve.

Chapter Five, on "Theoretically Informed Listening," optimistically tries to mop up after Chapter Four. Its essential point is that the aesthetic effi-

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<sup>13</sup> Fodor, *Modularity of Mind*, 47–52.

cacy of a particular theory (the instrumental value of a theory in enhancing and enriching the listening experience) is not dependent on whether it is objectively true or false. DeBellis proceeds here via an exegesis on Kivy's discussion of various modes of listening (which is itself an exegesis on the different approaches to listening typified by the characters Mrs. Munt and Tibby from E. M. Forster's *Howard's End*).<sup>14</sup> DeBellis makes good use of Kivy's (well, originally Forster's) account of the experience of Mrs. Munt (who merely "taps surreptitiously when the tunes come") versus that of Tibby (who "holds the full score open on his knee"). In DeBellis's terms, Mrs. Munt is a strongly non-conceptual listener while Tibby's listening is theory-laden. In his chapter, "It's Only Music: So What's to Understand?" Kivy is clearly pulling for Mrs. Munt; he argues that she is fully aware of the music's effects even if she is ignorant of their syntactic causes.<sup>15</sup> While being careful to avoid claiming that Tibby's listening experience is more determinant than that of Mrs. Munt, DeBellis does claim that Tibby's kind of listening is richer (p. 125). Tibby has "a deepened perception of a [structural] property for what it is, [and] that is central to his increased appreciation. And with this comes a deepened pleasure in the music" (p. 129).

Theorists may well be encouraged at this point, but rough sailing lies ahead. For DeBellis notes that the efficacy of a theory in deepening the perception of a structural property is not dependent on the truth or falsehood of either the premises of the theory or their application in a particular analytic context. This follows from the argument in the previous chapter: given that there are no theory-neutral observers to adjudicate music-theoretic claims, one cannot know whether a given theory is "objectively" true or false. This being the case, DeBellis adapts an instrumentalist approach in judging music theories, and he uses Rudolf Reti's theory of motivic relationships as a case in point. While Reti's theories and analyses have received due criticism,<sup>16</sup> DeBellis resuscitates Reti's analyses in arguing how they could provide an enhanced listening experience for a listener like Tibby:

Wouldn't the discovery that the rhythmic repetition has nothing to do with unity thus sensed rob his pleasure of a certain justification?

In my view the answer is negative . . . [E]ven if the rhythmic repetition has nothing to do with unity as the naive [strongly nonconceptual] listener senses it, it has much to do with *Tibby's* experi-

<sup>14</sup> Kivy, *Music Alone*, vi; cited in DeBellis, p. 117.

<sup>15</sup> Kivy, *Music Alone*, 93–123.

<sup>16</sup> And most germane in this context is the treatment they receive from Kivy, *Music Alone*, 130–42.

ence of unity, since he has paid so much attention to it. The falsity of a theoretical assumption that plays a causal role in someone's arriving at a certain aesthetic situation will not undermine the situation. . . . For the listener, such assumptions are a ladder one might kick away; hence, it would be beside the point for Tibby to worry about their truth or falsity. (pp. 130–131)

This “whatever theory gets you through the night” argument, though sound, gives me little comfort. For it is a depressing prospect if (a) one cannot adjudicate theories because to do so requires theoretically-informed listening, which tends toward *modus ponens*, and if (b) what matters most to the users of a theory/analysis is its ability to enhance their listening experience. Musical analyses enrich the listening experience by providing interpretive and/or explanatory frameworks for the musical events.<sup>17</sup> Now I am all for analyses being entertaining, but there is some way in which I think (or at least I want to think) that I value a “true” but perhaps mundane analysis to a rollicking tall tale. Leonard Meyer has noted that we really do value truth in aesthetic contexts, for “why should a work of art once found moving and valuable become a worthless curiosity when it is discovered to be a forgery?”<sup>18</sup> Meyer responds to his question by examining how our beliefs about a work of art influence our perception of it. For we do not simply attend to its beauty and form in the abstract, but do so in a richly embedded cultural context. In a similar fashion, it would seem to me, we also evaluate the theories which inform our aesthetic understanding: we believe in a particular theory in large part because we believe it to be true and of sound premises. If we believe that an analysis is based upon a faulty theory or theoretical premise, we then devalue or even discard that analysis. The recent debate(s) over Susan McClary's reading of the first movement of Beethoven's Ninth Symphony is an obvious and symptomatic case-in-point.<sup>19</sup> Few would deny that her reading told an interesting and

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<sup>17</sup> For a thorough examination and critique of this process see Marion Guck, “Analytical Fictions,” *Music Theory Spectrum* 16, no. 2 (1994): 217–30.

<sup>18</sup> Leonard B. Meyer, *Music, The Arts, and Ideas: Patterns and Predictions in Twentieth-Century Culture* (Chicago: University of Chicago Press, 1967), 55. David Huron has made similar points regarding the way we value truth in musical analyses in his review of Nicholas Cook's *Music, Imagination, and Culture* in *Music Perception* 12, no. 4 (1995): 479.

<sup>19</sup> Susan McClary, “Getting Down Off the Beanstalk” in her book *Feminine Endings: Music, Gender, and Sexuality* (Minnesota: University of Minnesota Press, 1991), 112–31; see Pieter van den Toorn, *Music, Politics, and the Academy* (Berkeley: University of California Press, 1995) for a well-known response. McClary's analysis is chosen here as it is convenient and well-known exemplar; to engage fully even a few of the epistemic and ontological issues that her (or any other) feminist reading of musical structures raises would take us well beyond the scope of the present essay.

challenging story about this movement, casting its tonal and formal structures in a new, gendered light. The problem (well, at least one problem) was not their instrumental value; clearly, these analyses were a “good read.” Rather, the problem was that for some theorists these readings (and/or the theories which stood behind them) just were not true. But if DeBellis is right, then concerns about truth are more or less beside the point, and just as much beside the point for Schenkerians and set theorists as they are for feminists and semioticians. There may be much to hear, but ultimately very little in which we may, from the austere perspective of analytic philosophy, legitimately believe.

In the sixth and final chapter, “Conceptions of Music Structure,” DeBellis leaves us with some observations regarding competing theories and analytical disagreements. As DeBellis politely puts it our theoretical disagreements may often “proceed from [the] diverse purposes of music theory” (p. 139), and in this chapter he discusses what some of those purposes can be. DeBellis uses the familiar opening measures of Mozart’s K. 331 as a case-in-point, an especially useful example given the many varying readings of its tonal and rhythmic structure. DeBellis takes up a particular question that arises in a Schenkerian context: does this piece open with a  $\hat{3}$ -line, or with a  $\hat{5}$ -line? And what exactly does it mean to claim that this piece opens with a  $\hat{3}$ -line, anyway? First, DeBellis points out that we do not just mean “these measures could be generated from an underlying  $\hat{3}$ -line,” since we would have little disagreement with the counter-claim that they could have been generated by an underlying  $\hat{5}$ -line. We usually mean something more, and DeBellis next notes that there are at least two ways in which we may mean something more (p. 137). If we hold an *intentional conception* of musical structure, the claim “K. 331 opens with a  $\hat{5}$ -line” has the structure of a  $\hat{5}$ -line in virtue of its being perceived as such. As DeBellis notes, this is “an essentially phenomenological analysis of structure” (p. 139), and:

[W]hat is central to the [intentional] account is that structure is something to be *heard*. . . . In such cases, analysts seem to rely primarily on the introspection of the objects of their listening experience in arriving at structural descriptions. Or rather: however they arrive at such descriptions, the test for their *validity* seems bound up with how the music is heard. (p. 141)

The other conception, which DeBellis calls the *causal conception*, is grounded on a structure or structural property being demonstrably present, whether or not that property is directly audible. While the claims of set-theoretic analysis are an obvious target here (and one which DeBellis strikes), to give another example DeBellis also points out how structural recurrences

in a Machaut chanson (“Douce dame jolie”) may be present but unnoticed, especially to the listener unacquainted with the conventions of the virelai (pp. 144–46). Though DeBellis does not specify how such claims are made, it seems clear that in these latter cases the analytical argument involves a demonstration of structures and/or structural relationships which are uncovered through knowledge of the compositional process (as might be revealed in sketch studies), or through knowledge of generic conventions (as in knowing the virelai schema, for example), or through some theory of musical structure which may serve as an analytical discovery procedure (take your pick). As noted in the previous chapter, once an analysis is suggested, and if it seems plausible, then one may well begin to hear these causally-conceived properties that were previously inaudible, precisely because the analyst has paid so much attention to them. The causal conception may therefore evolve or devolve to the intentional conception in its diachronic application. Moreover, there is a sense in which causally conceived structural properties are ultimately validated through how the music is heard. In his virelai example DeBellis notes that while the listener may be unaware of the AbbaA formal paradigm, this plan strikes an appropriate balance between the presentation of recurring music versus new text, and thus “as a consequence the eventual recurrence of A delights the listener and somehow seems right and inevitable” (pp. 145–46). Though the structure itself is not perceived, its effects—here the propriety of a repetition—*are* perceived. So there seems to be a sort of indirect intentional validation of those analyses which are grounded in a causal conception of musical structure.

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DeBellis’s observations on the differences between analytically-skilled vs. naive listeners have been made by a number of other researchers, especially those who work in the area of music perception and cognition. In a posting to the “MTO-talk” list Richard Parncutt summed up the difference between psychologists and music theorists in the following fashion:

Psychology is, or aims to be, about what people *really experience*, whereas theory is about what theorists want people to experience, or agree that people should experience, or hypothesize that people might experience.<sup>20</sup>

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<sup>20</sup> *Music Theory Online*-talk, 25 June 1996; archived at <ftp://boethius.music.ucsb.edu/pub/smt-list/smt-talk>. (Italics mine.)



Parncutt's own research has involved the application of Terhardt's work on the perception of acoustical spectra to the problem of the perceptibility of the triad roots.<sup>21</sup> Many theorists are aware of the work of Nicholas Cook, whom DeBellis mentions in passing (p. 148).<sup>22</sup> Cook has demonstrated that many listeners, including those we would describe as musically experienced (that is, having studied an instrument for many years, though not having studied much music theory or history), simply do not attend to such things as formal repetition, tonal return, and so forth. The overall conclusion from Cook's work is that for most people, all listening (like all politics) is local. Large-scale structures and structural relationships do not seem to impinge on their sense of closure (or lack thereof), drama, and so forth. Joseph Swain also makes similar points:

1. Most complex perceptual objects are not universal, but are dependent on a community for which they are real and practical; those outside the community can be said to be incompetent in perceiving such objects.
2. The practical value that a community attaches to an object can be completely conventional and inseparable from "belief." If a community believes that a unity of key can determine aesthetic coherence, then such a belief might influence acts of perception.
3. Such beliefs and values can be taught . . . .
4. The significance of perceptual objects depends in part on who the successful perceivers are and on the nature of their activities within a culture.<sup>23</sup>

Finally, William Thomson in his tellingly titled essay, "The Harmonic Root: A Fragile Marriage of Concept and Percept," summarizes a great deal of work on pitch and interval perception in music theory and music psychol-

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<sup>21</sup> See, for example, Richard Parncutt, "Revision of Terhardt's Psychoacoustical Model of the Roots of a Musical Chord," *Music Perception* 6, no. 1 (1988): 65-93.

<sup>22</sup> Much of Cook's research is summarized in his *Music, Imagination, and Culture* (Oxford: Clarendon, 1990).

<sup>23</sup> Joseph P. Swain, "Music Perception and Musical Communities," *Music Perception* 11, no. 3 (1994): 317-18.

ogy, including that of Parncutt.<sup>24</sup> As Thomson notes, “empirical studies of interval perception have fallen short of confirming the phenomenal reality our concepts describe so confidently.”<sup>25</sup>

Thus DeBellis’s observations regarding the gap(s) between music theory and “real” musical perception are nothing new. What he adds to this growing discussion is a more nuanced view of the gap between music theory and musical perception. After reading DeBellis it becomes clear that a weakly non-conceptual conception of musical listening serves as the principal working hypothesis of musical psychology. This same assumption is also present in the work of many music theorists. For example, Lerdahl and Jackendoff explicitly claim that “the goal of a theory of music to be a *formal description of the musical intuitions of a listener who is experienced in a musical idiom.*”<sup>26</sup> But as DeBellis shows, there are numerous problems with the weakly-nonconceptual conception of the naive listener, and thus the claims of what listeners are “really hearing” in various psychological tests of music perception need to be taken with a large grain of salt. Furthermore, DeBellis has further shown that there is more at issue than simply the difference(s) in conceptualization between naive versus experienced listeners. There may also be insurmountable gaps between theoretically-informed listeners, that is, within the ranks of the theoretically initiated.

Theorists, indoctrinated into a particular way of listening, really do hear and experience the music in the way they claim they do. Or at least, they believe that they do, and since our knowledge of music is mediated through our ascriptions of perceptual belief, it follows that both “real” ascriptions (that is generated from the bottom-up output of some perceptual mechanism, whether it is modular or not) as well as “constructed”

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<sup>24</sup> William Thomson, “The Harmonic Root: A Fragile Marriage of Concept and Percept,” *Music Perception* 10, no. 4 (1993), 385–416. Thomson gives summaries of Robert I. Hurwitz, “An Investigation into the perception of root in harmonic intervals,” Ph.D. diss., Indiana University, 1970; A. J. M. Houtsma and J. L. Goldstein, “The Central Origin of the Pitch of Complex Tones: Evidence from Musical Interval Recognition” *Journal of the Acoustical Society of America* 51, no. 4 (1971), 520–29; Ann K. Blomback and R. T. Parrish, “Acquiring Aural Interval Identification Skills: Random vs. Ordered Grouping,” *Journal of Music Theory Pedagogy* 2 (1988): 113–31; Helen Brown, “The Interplay of Set Content and Temporal Context in a Functional Theory of Tonality Perception,” *Music Perception* 5, no. 3 (1988): 219–50, and Parncutt, “Roots of a Musical Chord.”

<sup>25</sup> Thompson, “The Harmonic Root,” 385.

<sup>26</sup> Fred Lerdahl and Ray Jackendoff, *A Generative Theory of Tonal Music* (Cambridge: MIT Press, 1983), 1 [italics in original]. See DeBellis (pp. 5–6 and 13) for critiques of the notion of “intuitive” experience in general and Lerdahl and Jackendoff’s thesis in particular.

ascriptions (projected from higher cognitive levels onto the perceptual field) have equal epistemic validity. Indeed, it is not at all clear how one could distinguish one from the other. As DeBellis points out in his discussion of theory-equivalent hearing (pp. 39–42 and *passim.*), in the case of the expert listener (one who is heavily laden with theory) the distinction between hearing ascriptions and structural ascriptions is effaced: to those listeners “I hear  $x$  as a dominant” and “ $x$  is a dominant” epistemically converge. Thus Parncutt’s characterization is unfair to the theory-laden listener, since it implies that theorists do not really hear things like complex grouping structures, large-scale tonal resolutions, linear unfoldings, and so forth.

While we thus may take heart in an argument that allows us to believe that we really do hear what we believe we hear, one does not need to read DeBellis to know that there are various fissures—often contentious ones—within the theory and musicological communities. It is a depressing thought, however, to realize that if DeBellis is right, then these ideological fissures may give rise to phenomenally different experiences of music which only further entrench ideological differences. For of course different schools of music theory hold different conceptions of musical structure, and it is these very conceptions which get cashed out in various analyses. Remember too, as DeBellis points out, for most theorists it is the ear that is the final arbiter of analytic validity. It is yet even more depressing if one follows DeBellis’ observations further. Given its inherent lack of objectivity, music theory can never ascribe to be a kind of science; perhaps all that theory can ascribe to be is a kind of solipsism. Ultimately all we can do is come up with coherent accounts of our own listening ascriptions, since our individual conceptions of music are the product of each of our uniquely contingent accretions of theory and musical experience. Different music theorists will have different experiences and beliefs about the “same” piece of music, and to some extent these differences may be incommensurable.

Now most music theorists (including yours truly) would like to believe that in our theory-making and in our analyses we describe and debate “real” musical phenomena and “real” structural relationships. When we argue for a particular set of structural tones in a given passage, or when we claim to have sorted out the grouping structure in another, we believe that we are talking about ontologically distinct musical structures. With the aid of various theoretical perspectives and analytical methods we think we *can* “carve the music at its joints” and therefore produce an arguably “true” exegesis of a musical structure and its workings. Moreover, we hope that our analyses will lead others to *hear the musical structure in the way we suggest* and thus be convincing. Is this just a fool’s errand? Why bother to produce a detailed analysis of the middleground structure of a Chopin

prelude, or the hypermetric organization of a Robert Johnson blues song, if no one else can really hear these things?

I want to close by suggesting that it is not a fool's errand, and indeed suggest that DeBellis's book contains a few rays of hope among its more sobering observations regarding the claims of music theory and analysis. First, if I am right in defending at least part of the Fodorian model for music perception, then the very shallow, modular levels of musical perception may be more important than we might think. For these shallow, encapsulated levels, while resistant to musical style and enculturation, still have a profound impact on the way we hear and conceive the most basic of musical structures. Few music theorists have given these levels their due, but the work of Eugene Narmour is a notable exception.<sup>27</sup> Narmour has shown how the output of a low-level module(s) for melody and rhythm constrains both the formation of higher-level structures as well as the application of our knowledge of musical syntax and style which inheres in those structures. Narmour's work thus is rooted in the most common (indeed, deeply cross-cultural) aspects of musical perceptions and understanding. As such, it may provide solid common ground for work within our own musical/musicological sub-culture. A second source of optimism comes, ironically, from DeBellis's most troubling observation/argument, namely that the farther away an analysis is from the perceptual surface of the music, the more tenuous its claims of neutral observe-ability and hence the more attenuated its sense of aural validity. While this may seem to lead to a notion of musical-analysis-as-make-believe, DeBellis's distinction between intentional versus causal conceptions of structure may be important here. We should ask on what level(s) of musical structure is each of these conceptions most appropriate(?), and then pursue causally-based explanations (evidence permitting) where intentionally-based accounts are less verifiable.

In this review-essay I have perhaps taken some of DeBellis's careful and well honed arguments and pursued them farther and in directions that DeBellis might not countenance. But to this reader, at least, it seems clear that theorists can no longer be content to see their task as giving exegesis of musical structure based upon and guided by their "analytical intuitions." Rather, our task must be the exploration of various modes of discourse about musical structure. In other words, a little less analysis and a bit more theory. We need to make greater efforts to make our analytic

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<sup>27</sup> See Narmour, *Analysis and Cognition of Melodic Structures*, as well as his *The Analysis and Cognition of Melodic Complexity: The Implication-Realization Model* (Chicago: University of Chicago Press, 1992).

ascriptions (and the listening ascriptions they entail) as mutually intelligible as possible. If we take on this meta-critical pursuit then our chances of escaping from the growing tower of theoretical Babel will increase. If we do not, then, to quote a recent review by David Huron, "Music theory is in deep trouble."<sup>28</sup>

—Justin London  
Carleton College

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<sup>28</sup> David Huron, Review of Nicholas Cook, *Music, Imagination, Culture*, in *Music Perception* 12, no. 4 (1995): 509.