In Defense of Introspectionism: A Response to DeBellis

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DeBellis (2009) argues that introspectionist music theory, as represented in my book *The Cognition of Basic Musical Structures* (*CBMS*; Temperley, 2001), makes an ungrounded assumption that the intuitions of the author are shared by other theorists and by experienced listeners generally. But this inductive leap is motivated by experimental and corpus evidence showing that, with regard to basic structures such as meter, listeners largely *do* hear things in the same way; and this assumption is confirmed in *CBMS* by corpus tests in which my analyses are compared to those of other listeners. DeBellis suggests that what I was doing in *CBMS* was not accessing unconscious representations, but rather bringing new representations into existence. But this implies either that experienced listeners in general do not represent meter at all, or that they arrive at metrical analyses by a pre-theoretic process that is not available to music theorists; neither of these options is plausible.

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In his article "Perceptualism, not introspectionism: The Interpretation of Intuition-Based Theories" (in this issue of *Music Perception*), Mark DeBellis offers a thoughtful and provocative critique of the "introspectionist" approach to music cognition—an approach in which the theorist seeks to describe his or her mental representations of music through introspection. DeBellis is concerned with what he calls "intuition-based" music theories. While he includes Lerdahl and Jackendoff's *Generative Theory of Music* (1983) (hereafter *GTTM*) in this category, DeBellis's main focus is on my work, especially my book *The Cognition of Basic Musical Structures* (2001) (hereafter *CBMS*), which, as he notes, is strongly influenced by *GTTM*. DeBellis presents three objections to the introspectionist approach. He then proposes an alternative interpretation of intuition-based theories, in which the intuitions reported are not introspections, but reports of perceptual judgments about music.

I will call DeBellis's three objections (in the order that he presents them) the Conscious/Unconscious Objection, the Communication Objection, and the Trained/Untrained Objection. While the first objection is, in a sense, the most fundamental, my response to it depends on my response to the other two, so I will address it last.

Under the introspectionist approach, the theorist makes claims about their mental representations of a piece based on introspection, and the reader of the analysis then compares these claims to their own intuitions, again via introspection. (I think DeBellis and I agree that an intuition is essentially the conscious experience of a mental representation.) The problem, according to DeBellis, is that there is no guarantee that the theorist and the reader attach the same meanings to the analysis; this is the Communicaton Objection. DeBellis illustrates this objection using Lerdahl and Jackendoff's (1983) theory of pitch reduction. In presenting this theory and discussing the intuitions on which it is based, Lerdahl and Jackendoff (hereafter L&J) suggest that the position of an event in a pitch reduction indicates its "structural importance" (which they distinguish from "surface salience"); DeBellis wonders whether readers "will have a common, sufficiently determinate understanding of the term [structural importance]" (p. 124). L&J also suggest, as a criterion for pitch reduction, that "each level should sound like a natural simplification of the previous level"; again, DeBellis questions whether this will necessarily mean to readers what it means to L&J.

It is odd that DeBellis chooses pitch reduction to illustrate a critique aimed primarily at my work, since I do not deal with pitch reduction at all in *CBMS*.[1] In defending my methodology, I am more comfortable using theories that I have advocated and developed in my own work. Let me then make my case with a kind of

[1]DeBellis's other examples concern the *GTTM* theory of grouping structure. While I do give some attention to grouping in *CBMS*, my view of it is very different from that of *GTTM*; my model of grouping is non-hierarchical, and applies only to monophonic music.

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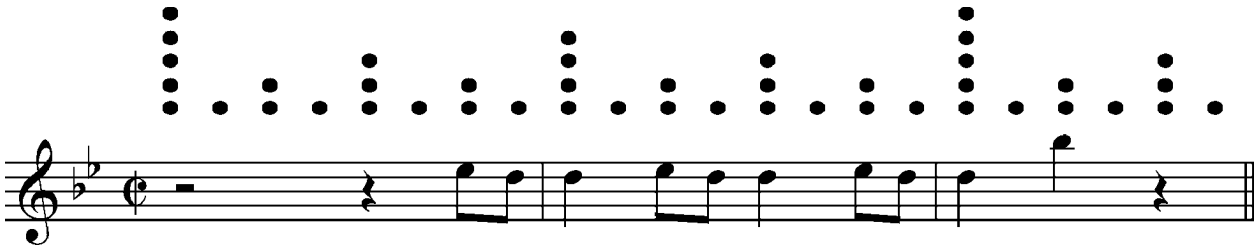

FIGURE 1. Mozart, Symphony No. 40, first movement, mm. 1-3. The metrical grid is indicated above the staff.

structure that is addressed, and viewed in much the same way, in both *GTTM* and *CBMS*: metrical structure. A metrical structure, as proposed in *GTTM* and assumed in *CBMS*, is a framework of several levels of beats of varying degrees of strength, where beats are points in time; beats are inferred from various kinds of accents in the musical surface but may not always coincide with accents. The metrical structure (or "metrical grid") for the opening of Mozart's Fortieth Symphony is shown in Figure 1. It seems to me DeBellis's Communication Objection could very well be made with regard to meter as well. In positing a certain metrical structure for the passage in Figure 1, I am making certain claims about my mental representations: That I infer a quarter-note-level beat at the onset of the first note of the melody, a one-measure-level beat at the third note, and so on. A reader may find this analysis in accord with his or her own intuitions, but how do we know that the reader attaches the same meaning to "beat" as I do?

DeBellis calls the Communication Objection a "skeptical worry": it questions the reasoning behind my claims about musical mental representations (and thus, the claims themeselves). DeBellis's third objection, which I call the Trained/Untrained Objection, is a skeptical worry as well. This objection concerns the claim—made by both L&J and myself—that the representations we posit are valid not only for other music theorists but for "experienced listeners" in general, those familiar with tonal music but not necessarily formally trained. DeBellis argues that there is little justification for this move. Again, the case of metrical structure can serve as an illustration: How can I be sure that untrained listeners infer the same metrical grids from pieces that I do, or indeed, that they infer metrical grids at all?

To respond to DeBellis's objections, we must consider what intuition-based music theories are meant to accomplish; once more, I will use metrical structure as an example. Like any theory, the metrical theory of *GTTM* and *CBMS* is put forth to explain certain facts. Some of these facts are intuitions that the theory explains quite directly: We might feel, for example, that a certain note in the music feels more accented than another note.[2] Other intuitions may be explained in more indirect ways: for example, we might posit metrical structure as a way of explaining why certain points in a melody feel like better ending points than others (Palmer & Krumhansl, 1987), or why a particular setting of text to the music sounds better than another (Halle & Lerdahl, 1993), or why we feel a change of harmony at a certain point (assuming a theory such as *CBMS* in which harmony is influenced by meter), or why certain musical segments sound parallel and others do not (Temperley, 1995). Other relevant facts come from behavioral studies in music psychology. When people are asked to tap to music, they tap in certain fairly consistent ways; if asked to tap at a slower or faster rate than their normal one, they will find a regular pulse that is again fairly consistent across listeners (Parncutt, 1994; Drake, Penel, & Bigand, 2000). To take another example, Povel and Essens (1985) find that some rhythmic patterns are much more easily learned than others, and hypothesize that the easily learned patterns are those that strongly induce a single metrical structure. (Much of the experimental research on meter, such as Povel and Essens's study, has used participants without extensive formal training in music theory.) Yet another source of evidence is corpus

[2]We must be careful here, since there are different kinds of accent: What is at issue here is *metrical* accent (a strong beat), rather than *phenomenal* accent (something in the musical surface that draws attention, such as a long note or loud note). While phenomenal accents generally correspond to metrical accents (indeed it is largely the phenomenal accents that convey the metrical accents to the listener), they can also conflict (creating syncopation). L&J argue that the failure to make this distinction has led to confusion in rhythmic theory in the past. (This may be exactly the kind of "misunderstanding" that DeBellis has in mind.) Because of this, we should be cautious about attaching too much weight to intuitions about accentuation itself; it may be wiser to rely on other kinds of intuitions that arise from metrical structure, such as intuitions about how to tap to a melody or how to set text to it.

data—representations of meter in notated music. Music notation normally indicates meter up to the level of the measure (with barlines and time signatures), and this can be taken as an indication of the *composer's* metrical hearing.

This body of data—intuitive, experimental, compositional—both motivates and confirms the metrical theory of *GTTM* and *CBMS*. In the first place, it motivates the basic theory of metrical grids—an idea first proposed in *GTTM* as a general way of accounting for rhythmic and metrical phenomena. Secondly—and here is where introspection comes in—these data offer many specific examples of listeners' intuitions, allowing us (L&J and myself) to compare these intuitions to our own. (Corpus data are especially useful in this regard, but experimental data are valuable as well, especially with regard to untrained listeners; for example, in Povel and Essens's (1985) study, we may examine specific patterns and compare participants' intuitions about their complexity to our own.) As we usually find our own intuitions to agree with those of other listeners, this seems to indicate that in general—not always, but most of the time—people hear meter in the same way. Once this assumption is made, we then can take our own intuitions as representative of experienced listeners in general. These intuitions then serve as our primary data in theory-building—for example, in identifying the principles whereby metrical structures are inferred from note patterns (which is actually our main concern). This inductive leap from our own intuitions to other listeners seems justified on the whole, but it certainly is not infallible. Clearly there are some differences in music perception between trained and untrained listeners, and sometimes there are disagreements among experts as well. It is therefore important, once our theories have been proposed, to test them against the intuitions of others. L&J encourage experimental testing of their theory in *GTTM* (pp. 5, 333), and some relevant work has been done relating to the metrical component; for example, Palmer and Krumhansl (1987) find a strong correlation between metrical strength and judgments of melodic closure. In *CBMS*, I present corpus tests of the metrical theory, in which its predictions are compared to metrical analyses represented in music notation.

If the structures and analyses proposed in *CBMS* were supported by introspective evidence alone, then DeBellis's objections would be perfectly valid and would cast serious doubt on the project. But this is far from the case. Indeed, I make it very clear in *CBMS* that the theories put forth there are motivated by external evidence, and are subject to testing and confirmation from such evidence (see especially pp. 6-7). Each of the kinds of structure presented in *CBMS*—meter, melodic phrase structure, contrapuntal structure, harmony, and key—is proposed to explain a body of relevant evidence. In each case, I found a high degree of correspondence between other listeners' intuitions and my own, and thus took my own intuitions about analyses of pieces as representative. And in each case, this approach was vindicated by corpus analyses comparing other people's intuitions (composers or other music theorists) to my own.[3] Admittedly, much more testing is needed here, especially with regard to untrained listeners. But I believe that the basic logic of *CBMS*—introspectionist methodology, motivated by and then tested against other evidence—is sound.

To return to DeBellis's skeptical worries: The kinds of evidence discussed above do not *prove* that the theories in *CBMS* are correct, nor could they ever do so. It is always possible that two listeners who display the same behaviors and report the same intuitions represent things mentally in completely different ways. But no theory can ever be proven correct. We are justified in accepting whatever theory predicts the totality of the evidence most accurately and parsimoniously. And until better alternatives are proposed, I believe that we are justified in accepting the theories in *CBMS* as the best explanations for the relevant facts.

The Conscious/Unconscious Objection

Earlier I suggested that claims about mental representations could be justified, in part, by their explanatory power in regard to intuitions: for example, we might posit metrical structure to explain our intuitions about the most natural way to tap to a melody. One might also appeal here to intuitions about metrical structure itself: That is, one could simply say, "metrical structure is intended to explain (and may therefore confirmed by) listeners' intuitions of metrical structure." Note that I did *not* make this argument. This brings us to DeBellis's first objection, which I call the Conscious/Unconscious Objection. This objection concerns the claim, made repeatedly by L&J and myself, that the kinds of mental

[3]Actually, the corpus tests in *CBMS* do not directly involve my own intuitions, but rather, the output of the computational implementation of my theory. But I designed the theory to match my intuitions (though it does not always do so exactly); thus, if there is strong agreement between the theory's output and other listeners' intuitions (and the tests show that there is), this is probably because my intuitions match those of other listeners.

representations at issue in *GTTM* and *CBMS* are generally unconscious. DeBellis asks, if the representations are unconscious, how can they be accessed via introspection? DeBellis focuses in particular on a passage in a 1999 essay in which I discuss the use of introspection in music cognition research:

> This [the use of introspection] may seem problematic, in view of the fact that the mental processes and structures involved are generally held to be unconscious. But it seems reasonable to suggest that such structures might be made conscious through sustained introspection, or, perhaps, inferred from other representations that are more readily accessible (Temperley, 1999, p. 69).

DeBellis questions the idea that unconscious representations "might be made conscious through sustained introspection."

The status of musical structure with respect to consciousness is an extremely difficult issue. I admit that I have been somewhat inconsistent about it and I am not entirely certain about it even now. I believe it is basically right to say that the kind of structures discussed in *CBMS*—meter, harmony, and the like—are unconscious. Consciousness implies a vivid, direct, awareness of something—like a note being loud or a flower being red. I do not feel that I have this kind of awareness of metrical grids and harmonic analyses as I hear a piece—even if I am *thinking* about those things. DeBellis is quite right, also, to challenge my suggestion that unconscious representations "might be made conscious through sustained introspection." (I had doubts about this suggestion myself after proposing it; it does not appear in *CBMS*.) I would, however, stand by the second part of this sentence (quoted above): one can make *inferences* about unconscious representations from the conscious representations (intuitions) that they give rise to. In the case of meter, for example, I may not have direct intuitions about metrical structures. But I do have intuitions about whether note A is metrically weaker or stronger than a neighboring note B; I may feel that note A is a more natural location for a stressed syllable (if a text were set to the melody), or that note A is a more natural place to tap or conduct a beat. And it seems justified to posit metrical structure as a kind of unconscious representation that gives rise to these conscious intuitions.

Let us suppose, then, that metrical representations are unconscious, but that one can infer such representations from conscious intuitions of various kinds. In the years since I first encountered *GTTM*, I have learned to do this quite quickly and almost effortlessly. But if this process of inference becomes fast and effortless enough, so that I immediately know what metrical grid I am forming for any piece, is this really so different from being conscious of it? The immediacy and ease with which we can access metrical representations also seems to justify the use of terms such as "hearing" and "perceiving"—I *hear* the meter of a piece in a certain way—which I frequently use in *CBMS,* as do many other people. (It might also justify our saying that we have intuitions *about* meter, even if they are not intuitions *of* meter.) Perhaps we could say that consciousness is a matter of more-or-less rather than all-or-nothing: metrical structures are immediate enough that we may speak of perceiving them, yet not quite as present to awareness as colors and sounds.

There is another part to the Conscious/Unconscious Objection. DeBellis writes:

> [T]here is virtually no scientific understanding of the mechanism that would underlie [a process] . . . that transforms unconscious representations into conscious ones while reliably preserving their content. (p. 123)

Thus the problem with introspectionism lies not only with the idea of accessing unconscious representations, but also in the fact that, if we do access such representations, we may change them. How do we know that our introspective activity is not affecting the unconscious representations that we are trying to describe?

This objection, too, cannot be taken lightly; but I believe the introspectionist approach can withstand it. This conviction is due largely to my memory of my past experiences of music. That is to say: I have intuitions about pieces (the relative accentuation of notes and so on) that seem to confirm the reality of metrical structure as part of my own cognition (whether these intuitions are *of* metrical analyses or an indirect result of them is not important here); and I am fairly confident that I have had these intuitions for a long time, since long before I started studying L&J's theory, or indeed, before I started studying music theory at all. The reader may well be skeptical of this: How can I be sure that I remember my earlier musical experiences accurately? What is crucial here is that, while in most cases I believe my metrical analyses have not changed much, in a few cases they have. As it happens, Mozart's Fortieth Symphony provides a case in point. I have known this melody since childhood; but I can remember that I used to hear it with the downbeat of m. 2 stronger than the downbeat of m. 3, as in Figure 2. When the possibility of an "odd-strong" hearing (one with odd-numbered measures stronger than even-numbered ones) was brought to my attention,

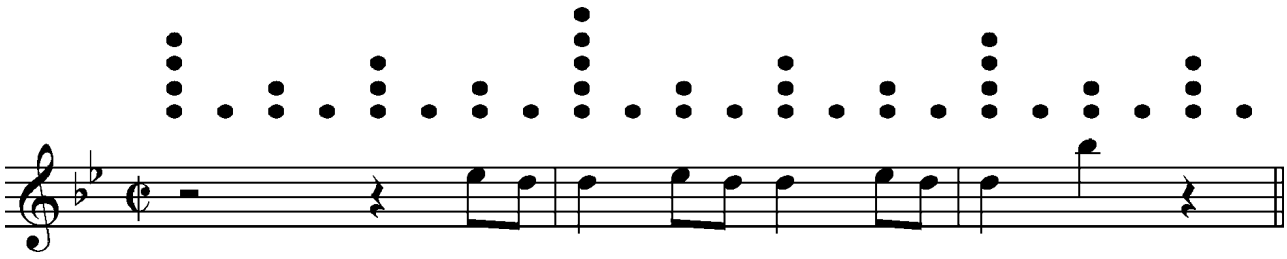

FIGURE 2. Mozart, Symphony No. 40, first movement, mm. 1-3, with an alternative metrical grid.

I remember this was quite a relevation and definitely changed my hearing of the passage. In short: my memory tells me that I used to hear most pieces the same way that I do now, but that, in a few specific cases, my hearing has changed. Clearly, then, I am not subject to a general illusion of constancy with regard to my metrical hearings. This leads me to believe that my memories about this are largely accurate; and my memory tells me that my mental representations of meter and other basic musical structures have not been greatly altered by my analytical and theoretical activities.

As I have said, I find the issue of consciousness very difficult and even now I am not sure I have it right. It seems to me, however, that this is not really the *central* issue. What is ultimately of interest is the psychological reality of the structures and principles in *GTTM* and *CBMS* for experienced listeners in general—whether conscious or unconscious. I do maintain that I am able to access my preexisting metrical representations—whether the process is direct or indirect—without changing them; and I further maintain that the inductive leap from my metrical representations to those of other listeners, when backed up by other evidence, is justified.

Perceptualism

The Communication and Trained/Untrained Objections are objections to the conclusions drawn from introspectionist work; they challenge the logic of attributing introspectively-obtained analyses to other listeners. The Conscious/Unconscious Objection, on the other hand, is an objection to the *methodology* of introspectionism. According to DeBellis, when L&J and I think we are accessing unconscious mental representations through introspection, we really are not doing that at all. In that case, what are we doing? If the analyses put forth in *GTTM* and *CBMS* are not reports of our unconscious representations, what are they?

To answer this question, DeBellis presents an alternative "interpretation" of the theories in *GTTM* and *CBMS*, which he calls "perceptualist." Under the perceptualist interpretation, the analytical process represented in *GTTM* and *CBMS* is not one of introspection—trying to gain access (directly or indirectly) to unconscious representations—but rather, one of studying musical objects themselves:

> On such a [perceptualist] model, when theorists talk about intuitions about musical structure they are talking about . . . the perception or detection of structural properties of the music; and when they write or assent to an analysis they are reporting what they perceive: they are reporting a perceptual judgment about the music. (p. 125)

Introspection, DeBellis writes, "suggests a static situation whereby the representation is already present in the mind, and only comes to be known or clarified through introspection." (p. 127) Under the perceptualist view, however, "intuition is an active process, more plausibly understood as bringing structured percepts into existence" rather than "as the inspection of a structure already present." (p. 125) According to DeBellis, then, the analytical method used by L&J (in *GTTM*) and myself (in *CBMS*) actually involved *changing* our analyses of pieces, not accessing them. We could not have been accessing our unconscious representations, according to DeBellis, because it is *impossible* to do so.

Let us assume for a moment that DeBellis is right: the metrical analyses I arrived at in building the theory in *CBMS* involved bringing new analyses into existence, not accessing preexisting analyses. (We will stay with metrical structure as an example, though everything that follows applies just as well to any of the other kinds of structure discussed in *CBMS*: phrase structure, contrapuntal structure, harmony, and key.) The question is, what exactly was in my mind before I did these analyses—and more importantly, what is in the minds

of other experienced listeners, most of whom have not gone through the same perceptualist process that I have?[4] (Recall that experienced listeners are those who have had extensive exposure to tonal music, but may not have formal music training.) There are two options here, and it seems to me that neither one is plausible. One is that experienced listeners generally do not hear (mentally represent) metrical structure at all. Recall that metrical structure was first proposed as a way of explaining a large body of intuitive, experimental, and compositional evidence. If we assume that listeners generally do not mentally represent meter, we have simply abandoned our explanation for this data. We no longer have an explanation for why listeners find some rhythmic patterns more complex than others, why they tend to tap to music in certain ways, and so on. Surely we would not want to take this step unless there were compelling reasons to do so, and as far as I can see, none has been offered.

The second option is to say that experienced listeners *do* hear metrical structure, and the analyses that I propose in *CBMS* are, in general, valid for such listeners ("valid" in the sense that the analyses describe their hearing). How then did these analyses arrive in these listeners' minds? Presumably they arrived there through the kind of musical experiences that listeners generally have—essentially by listening to music and processing it in a largely unconscious and pre-theoretic way. (The analyses could not have arrived in listeners' minds by the kind of "perceptualist" work that DeBellis claims L&J and I do in our books—work involving conscious, explicit manipulation of music notation, metrical grids, prolongational trees, and the like—because most listeners have no conscious knowledge of these things.) But can we really expect that a hearing that I arrived at only through highly specialized analytical work will be valid for untrained listener X? This seems unlikely. (If it were true—if I needed special work to arrive at an analysis that X arrived at simply through ordinary listening—then perhaps X should be the music theorist and not me!) Thus, perceptualism either requires us to abandon a very good explanation for a large body of data, or requires an assumption that music theorists need to go to special efforts to obtain analyses that other listeners acquire much more naturally.

[4]When I say that an analysis is present in a listener's mind, I mean it *would* be present if they encountered the piece. This is surely common usage in cognitive science; for example, we say that people perceive a sentence or a visual scene in a certain way, though they would only perceive it if they actually encountered it.

If I am right that listeners arrive at metrical analyses through a largely unconscious and pre-theoretic listening process, it seems most likely that *I* arrived at them this way, as well, and that what I was doing in *CBMS* was accessing preexisting representations rather than forming new ones. (Here I am taking the inductive leap in reverse—extrapolating to my own case from the case of listeners in general.) Thus, the implausibility of the perceptualist view of *CBMS* seems to leave the introspectionist view as the only real possibility. And this relates to my earlier response to the Conscious/Unconscious Objection. That is, the fact that my metrical analyses generally seem to be shared by other listeners (as shown by corpus tests and the like) seems to suggest that these analyses were present in my mind before I did the work in *CBMS*, and that I accessed them without changing them.

Whether or not I am doing perceptualism in *CBMS*, perceptualism is at least something one *could* do. That is, one could analyze music, not with the aim of accessing unconscious representations, but with the aim of changing one's representations—"detecting structural properties in the music." Here again, it is interesting to consider the question of *purpose*. I suggested earlier that the goal of introspectionist music theory is to explain facts of musical intuition and behavior, and to describe the mental processes and representations that give rise to these phenomena. The larger goal, as I have suggested in *CBMS* (p. 325) and elsewhere (1999, pp. 81-82), is to extend these explanations to "higher levels" of musical experience—enjoyment, meaning, and emotional response—and ultimately to answer the (still almost totally mysterious) question of why anyone wants to listen to music at all. This, as I see it, is the goal of music cognition as a whole, and this is why I argue in *CBMS* that the book is best regarded as belonging to the field of music cognition.

Let us ask the same question about the perceptualist approach to music theory: what is its goal? DeBellis does not answer this question directly. There are several clues, however, as to what his answer might be. At one point, DeBellis notes correctly that observations arrived at through the perceptualist method may not be automatically imputed to experienced listeners in general; such imputation requires that other kinds of evidence be brought to bear:

> Consulting theorists' intuitions may be a useful heuristic for the discovery of plausible claims about the perceptual states of experienced listeners, but it does not substitute for the proper justification of them. (p. 128)

Here DeBellis seems to be suggesting that a perceptualist analysis, an analysis not previously present in the theorist's

mind but arrived at only by special work, may turn out to be valid for experienced listeners in general, or may at least provide a "useful heuristic" in this regard. I have already suggested that this is not plausible. If our goal is to characterize the hearing of experienced listeners in general, the perceptualist method is clearly not the way to do it. Whatever perceptualist theory is good for, it is not good for *that*.

So, what is perceptualism good for? Under the perceptualist model, it seems to me, the aim is not really to characterize anyone's hearing of music. Rather, the aim is (or should be) to arrive at a new hearing that is more satisfactory than what we had before—the richest, most fully informed hearing that we can achieve. This is a perfectly valid and worthwhile goal for music analysis. But it is as different from the introspectionist goal as it could possibly be. Perceptualist analysis does not, in any interesting sense, attempt to describe listeners' hearings or mental representations (except in the trivial sense that any report of something that I perceive is a description of my mental representation of it). It therefore has nothing to contribute to an explanation of musical experience or behavior. It is not music cognition, nor indeed, any other kind of cognitive science. Rather, it is an investigation of something in the outside world; it is no more cognitive science than, say, geology.

The introspectionist and perceptualist approaches to music theory, it seems to me, correspond more or less exactly to what I have elsewhere called "descriptive" and "suggestive" music theory (Temperley, 1999). Descriptive/introspectionist theory seeks to describe mental representations and explain music cognition and experience; suggestive/perceptualist theory seeks to describe pieces of music, with the aim of enhancing musical experience. As I suggest in my 1999 essay, the goals of descriptive and suggestive theory are not only different but conflicting. Descriptive theory seeks to describe how people hear (mentally represent) music, and fails if it does not do so; suggestive theory fails if it *does* describe the listener's current hearing, since its aim is to give them something new to hear. DeBellis cites this essay a number of times, but makes no reference to the suggestive/descriptive distinction that is the thrust of the essay—a distinction that seems very relevant to the current discussion.

Now DeBellis is certainly aware of the difference between the introspectionist and perceptualist approaches; he coined the terms, after all. But I am not sure he appreciates the magnitude, the fundamental nature, of the difference between them: in particular, the fact that the two approaches have entirely different goals. DeBellis repeatedly describes perceptualism as an "alternative" to introspectionism. But the term "alternative" usually implies an alternative way of achieving an agreed goal, rather than an alternative goal. At another point, DeBellis compares *CBMS*-style intuition-based theory to theoretical linguistics, noting that while both rely on intuitions, theoretical linguistics does not rely directly on intuitions of structure, and thus avoids one of the problems with introspectionist music theory. But how does perceptualism fit in with theoretical linguistics? Is the aim of linguistics to "attend to structural properties" of language, to "bring structured percepts" about language "into existence"? It is not. The goal of linguistics, rather, is that of introspectionist theory: describing and explaining people's mental representations, not changing and improving them. (I believe there is virtually unanimous agreement among linguists on this point; see for example Chomsky, 1980, pp. 11-24, 189-97.) By endorsing both perceptualism and theoretical linguistics, DeBellis seems to suggest that they are similar in purpose; it seems to me this is not the case at all.

At the end of his essay, DeBellis discusses at some length whether there could be an "intuition-based cognitive music theory" and seems optimistic about this possibility (though he emphasizes that theories of music cognition do not *have* to be intuition-based). Since he has just rejected the introspectionist interpretation of intuition-based theory in favor of the perceptualist one, I take it he is assuming the perceptualist interpretation here. But as I have argued, the goal of perceptualism is quite different from that of cognitive science. Cognitive science is concerned with characterizing mental processes and representations; perceptualism is concerned with discovering new properties of things in the outside world.

It is true that, in practice, the line between perceptualism and introspectionism can sometimes become blurred. A theorist may present an analytical observation about a piece that was arrived at through introspection; but to a specific reader, the observation may be a new perception, one that significantly alters their hearing of the piece. (This then would be introspectionist methodology serving a perceptualist goal.) In cases where that happens (and certainly it sometimes does), the inductive leap of introspectionism—the assumption that everyone hears things the same way—has clearly failed. In the case of meter, for example, lower metrical levels clearly have psychological reality across a broad range of listeners; higher levels (above the measure) may be more subjective and dependent on advanced training (my own experience with Mozart's Fortieth Symphony, discussed earlier, is a case in point). Still, descriptive and suggestive theory are, in

principle, distinct projects with different goals. I see nothing to be gained from conflating them—especially in a discussion that is especially focused on the goals of music theory and analysis.

Conclusions

On one key point in DeBellis's essay, he and I are in agreement. Introspectionist methodology makes an inductive leap from the theorist's intuitions to those of other listeners (both other theorists and experienced listeners generally). This leap can not be justified by any logical or commonsense argument, but requires confirmation from other sources—confirmation that other listeners' mental representations of pieces really do coincide with ours in the relevant ways. I believe I make this point fairly clearly in *CBMS*; and the corpus tests in *CBMS* show that, to a considerable extent, the inductive leap is justified. But DeBellis shows, much more clearly than I did in *CBMS*, the importance of obtaining this confirmation and the problems that can arise if one does not; and in this sense, his essay performs a valuable service. DeBellis is also right to challenge the notion that the musical structures discussed in *CBMS* are unconscious, yet accessible through introspection. Thinking about this objection has led me to a new position on the issue that I think is more satisfactory, though perhaps still not fully satisfactory.

DeBellis goes further than urging caution about the conclusions drawn from introspectionist theory; he actually claims that introspectionist theory cannot be done, and is not done in *CBMS*. Here I disagree; I maintain that introspectionist theory is possible, and that this is what I was doing in *CBMS*. If we assume that the analyses reported in *CBMS* represent new perceptions arrived at through special analytical work (as DeBellis claims) rather than pre-theoretic hearings, we can no longer explain the high degree of agreement between my analyses and those of other listeners—unless we assume, implausibly, that other listeners arrived at my "perceptualist" analyses through a pre-theoretic route.

Perceptualist music theory is a perfectly valid and, indeed, valuable pursuit. But DeBellis does not seem to realize, or at least does not adequately acknowledge, the fundamental difference in purpose between introspectionism and perceptualism. The aim of introspectionism is to understand and ultimately explain musical experience; the aim of perceptualism is to improve musical experience. It seems to me crucial to recognize this distinction, if we are to think clearly about the goals of music theory and music cognition.

Author Note

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