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viewpoint

The Question of Purpose in Music Theory: Description, Suggestion, and Explanation

By David Temperley

In any discipline, it is desirable to have a clear sense of what the goal is. What are we trying to achieve, and how will we know when we have succeeded? Music theory has hardly shirked these "meta-issues." Discussions of the goals of theory and analysis can readily be found; recent debates have centered on the means of confirming or testing a music theory, the relevance of historical and cultural context to analysis, and the relevance of the composer's intentions.[1] Exploration of these issues is to be welcomed, and disagreements need not alarm us when the positions taken are clear and coherent. I will argue here, however, that a serious confusion has been lurking beneath much of this discussion.

Many statements regarding the purpose of music theory may seem unproblematic, if somewhat vague. Probably few would take issue with Claude Palisca's definition in the *New Grove,* which characterizes music theory as "the study of the structure of music" (1980 18:741). Perhaps general agreement would be found as well over the relationship between theory and analysis. An analysis is an investigation of the structure of a single piece; a theory is a more general account of some aspect of musical structure, which guides analyses and is also motivated and informed by them. As Ian Bent has pointed out, however, the problem is where exactly the "structure" of music is to be found (1987: 5). Is musical structure something in the mind of the listener, in which case its elucidation involves the description of (perhaps unconscious) psychological processes and representations? Or is it something that resides in the musical object itself—perhaps, in large part, *not* normally part of the listener's hearing and experience, but revealed by the analyst with the aim of enhancing that experience?

Joseph Kerman, after embracing Palisca's definition of music theory as the study of musical structure, elaborates it as follows:

> When musicians use this term [structure] today . . . they generally mean the structure of total works of art—*what makes compositions work.* (1985: 61, italics added)

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This phrase appears often in general discussions of music theory. But what exactly does it mean? If I ask you how something works—say, part of a car engine—I could be asking, "What does it do?" or I could be asking "How does it do what I already know it does?" In the case of music, I could be saying, "This piece has certain effects on me (an emotional effect, a sense of conflict and resolution, etc.). How is it having these effects?" Or I could be saying, "I don't feel that I'm fully understanding this piece; show me a better way of listening to it so that I can appreciate it more." Consider also the following statement, from Matthew Brown and Douglas Dempster:

> Music theory must also be a rational pursuit. By 'rational' we mean nothing arcane, merely that theory helps us illuminate, elucidate, understand, or explain music. (1989: 65)

Here again, the same ambiguity arises. Does "illuminating" or "elucidating" music mean shedding light on our current hearing of the piece, and how that hearing arises, or does it mean enhancing that hearing in some way? All of these statements, then, are noncommittal between at least two purposes. As I will show, each of these purposes finds wide support in the writings of music theorists. Yet they are not only quite different, but, I will argue, are fundamentally conflicting.

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One possible goal for music theory is clearly reflected in this statement by Fred Lerdahl and Ray Jackendoff.

> We take 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.* . . . By this, we mean not just his conscious grasp of musical structure; an acculturated listener need never have studied music. Rather, we are referring to the largely unconscious knowledge (the 'musical intuition') that the listener brings to his hearing—a knowledge that enables him to organize and make coherent the surface patterns of pitch, attack, duration, intensity, timbre, and so forth. (1983: 1–3)

Leonard Meyer offers a similar view:

> Understanding and enjoying a Bach fugue or a Brahms sonata does not involve knowing about—conceptualizing—cadences, contrapuntal devices, bridge passages, and the like, any more than being entertained by *Hamlet* involves knowing about syntactic functions, prosodic devices, or dramatic means. . . . Listening to music intelligently is

> more like knowing how to ride a bicycle than knowing why a bicycle is ridable.
>
> This is not to contend that education cannot enhance understanding and hence appreciation and enjoyment. . . . And to this enterprise, critical analysis can certainly make an important contribution. But education is not its primary goal. The primary goal of criticism is *explanation* for its own sake. Because music fascinates, excites, and moves us, we want to explain, if only imperfectly, in what ways the events within a particular composition are related to one another and how such relationships shape musical experience. (1973: 16–17)

Work in music theory that embraces this purpose could be described as "descriptive" or "psychological" music theory: it attempts to describe listeners' unconscious mental representations of music. As both these quotes suggest, such work usually aims to account for the perceptions of a fairly wide population of listeners, rather than just those with extensive formal training, although it will normally confine itself to listeners who have had some exposure to the kind of music being studied. Such work might take the form of an analysis of a single piece, describing mental representations of some aspect of its structure. In other cases—as reflected in the Lerdahl and Jackendoff quote, for example—what is sought is a general theory of some aspect of musical perception, a theory that describes listeners' general knowledge of music and the principles whereby they infer certain structures from certain musical inputs.[2] Such a theory, in turn, might allow us to achieve Meyer's goal: *explaining* why it is that a certain piece, or certain musical features in a piece, bring about a certain experience in the listener.

Seen in this way, descriptive music theory could well be regarded as a branch of cognitive science—the loose alliance of disciplines concerned with the study of cognition, including also cognitive psychology, computer science (especially artificial intelligence), neuroscience, and linguistics. Descriptive music theory shares with these disciplines the goal of explaining aspects of human experience and behavior, and the assumption that the way to do this is by positing mental representations. The importance of this assumption in cognitive science cannot be overestimated.[3] To appreciate its centrality, one need only consider the kinds of concepts and entities that have been proposed in cognitive science: for example, edge detectors and primal sketches in vision, tree structures and constituents in linguistics, prototypes and features in categorization, networks and schemas in knowledge representation, loops and buffers in memory, prob-

lem spaces and productions in problem-solving, and so on. All of these are kinds of mental representations, proposed to explain observed facts of behavior or introspection.

The methodology of descriptive music theory is primarily introspective. This may seem problematic, in view of the fact that the mental structures and processes 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. A useful parallel may be drawn here with theoretical linguistics. The reasoning in linguistics is that, while we do not have direct intuitions about (for example) the syntactic structures of sentences, we do have intuitions about whether sentences are syntactically well-formed (and perhaps about other things, such as whether two sentences are identical in meaning). By simply seeking to construct grammars that model these judgments—linguists reason—we will uncover much else about the syntactic structure of the language we are studying (and languages in general). Similarly, seeking to model our introspective judgments about (for example) the metrical structures of pieces, or expectations of melodic continuation, may lead us to posit other mental processes and structures that are not in themselves consciously available.

Of course, unconscious mental representations of music may be—and have been—explored in other ways besides introspection, notably through psychological experiment and computer simulation. Indeed, these other methods have an essential role to play in testing the hypotheses of descriptive music theory. Together, these various approaches can be seen as constituting the musical branch of cognitive science—what has lately come to be known as "music cognition."

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Much other work in music theory embraces a very different purpose from that espoused by Meyer and Lerdahl and Jackendoff. Consider these three quotes, from John Rahn, Marion A. Guck, and Peter Kivy, respectively:

> To analyze music is to find a good way to hear it and to communicate that way of hearing it to other people. (1980: 1)

> I take it that analysis is the means to change and refine hearings and therefore that, when analysts write analytical texts, we are offering readers the possibility of recreating a hearing that we have found worthwhile. (1993: 307)

> It is, I take it, a truism that the point (or a point) of description is to get us to perceive in the music that which we are describing in it. (1989: 10)

Two quotes from Carl Schachter reflect a similar attitude:

> Of course the deeper levels of structure, by definition, are not as readily accessible to direct perception as are events of the foreground. . . . If they were, there would be no point to our analyzing music. (1976: 285–86)

> In the course of analysis, discoveries often occur. One becomes aware of connections that one had not previously perceived even in a confused or not fully conscious way. But once aware of them, one hears them; if not heard, the analysis is meaningless. . . . [M]usical analysis has value only insofar as it helps us to hear. (1976: 311)

We might call this approach to theory the "suggestive" approach. ("Prescriptive" is also a possibility, but this carries a pejorative connotation that is not at all intended here.) By this view, the objective of doing theory and analysis is to find and present new ways of hearing pieces, not to describe the way people hear pieces already. As with the descriptive approach, we might posit a distinction here between theory and analysis. An analysis recommends a hearing of a particular piece; a theory, on the other hand, offers general principles of musical structure which might be applied to many pieces. Whereas a descriptive theory intends to describe some aspect of musical perception or cognition, a suggestive theory seeks to enhance it in some way. Note the conflict between these two purposes: as suggested by Schachter's first quote in particular, suggestive analysis intends *not* to describe structures and relationships that are already being heard. (The conflict between the two is also clear in Meyer's quote, although here, the opposite view is taken: the goal of analysis is explanation, *not* education.)

Two further points are crucial here. I take it that when analysts say they are trying to get listeners to hear new things in the music, these are things that listeners, at present, do not hear *even unconsciously*. If these analysts were trying to make listeners aware of things that they already perceive unconsciously, then, of course, their purpose would be no different from that of descriptive theory; but this is not the sense I get from statements like those quoted above (although it is difficult to be sure).[4] Another point: to the extent that a suggestive theory posits structures or relationships that enhance our hearing of a piece—and, hence, which we did not

hear previously—the theory is not entitled to claim also that these relationships explain effects that the music has on us. To posit a certain feature of a piece of music as an explanation for a psychological effect, it seems to me, implies that that feature is (perhaps unconsciously) being heard or mentally represented. Certainly—as noted earlier in this essay—this is the usual assumption in psychology and other areas of cognitive science (although perhaps some in music theory would take issue with it).[5] To the extent that a theory claims to explain the effects of a piece, then, it must be taken as a descriptive theory rather than a suggestive one.

I believe these two goals characterize the great majority of work in music theory today. They are not, of course, the only goals that might be pursued. One might also regard musical analysis as an objective study of the score of a piece, a search for structures and relationships that seem significant or pertinent without regard for hearing either descriptively or suggestively. This is what Nattiez (1990) has called the analysis of the "neutral level."[6] However, I believe few analysts today would claim such a purpose. Alternatively, one might seek, through analysis, to reconstruct the intentions or thought processes of the composer. Again, I can find almost no *explicit* embrace of this goal in recent theory and analysis. Ethan Haimo (1996: 178) has recently suggested that claims about composers' intentions are often implied in analyses in subtle ways; this is an issue deserving further study. For now, however, we will limit ourselves to the two goals outlined above.[7]

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If it were simply the case that some theorists were pursuing descriptive theory, and others suggestive theory, this would not necessarily be an unhealthy situation. Indeed, in some areas of the discipline, there is a fairly clear allegiance to one purpose or the other. The work of Lerdahl and Jackendoff and Meyer is clearly psychological in orientation, as their quotes above suggest; much pitch-class set theory, I think, is clearly suggestive (consider Rahn's quote, for example). But a great deal of work in theory and analysis simply does not address the issue of purpose; and this is problematic, given the lack of consensus on this issue. The confusion is compounded by the fact that, in some cases, claims for suggestive and descriptive validity can be found for the same theory. The prime case in point is Schenkerian analysis.

The confusion over the purpose of Schenkerian analysis can be traced back to Schenker himself. Many comments can be found in Schenker's writings which seem to reflect a strongly suggestive attitude (although in Schenker's case, "prescriptive" would perhaps be more appropriate): "There is no doubt that the great composers—in contrast to performers

and listeners—experienced even their most extended works not as a sum total of measures or pages, but as entities which could be heard and perceived as a whole" (1979: xiii). "[O]ne can understand that the layman is unable to hear such coherence in music [the coherence of background structure]; but this unfortunate situation obtains also at higher levels, among musicians of talent" (1979: 6). However, listeners not blessed with this special gift can *learn* to hear large-scale structures, and Schenker's purpose is to facilitate this. "Only by the patient development of a truly perceptive ear can one grow to understand the meaning of what the masters learned and experienced."[8] In his discussions of musical structures and relationships, then, Schenker seems to be presenting them as things people should try to hear, rather than describing things that they already hear. However, there are also signs of a psychological attitude in Schenker's writings. *Counterpoint* contains numerous references to psychology and the "psychological effects" of musical patterns, as well as frequent appeals to the way "we hear" something or to the tendencies of "the ear."[9] While the suggestive impulse seems dominant in Schenker's writings, then, there are signs of some ambivalence in this regard.

This ambivalence is much in evidence in more recent Schenkerian analysis as well. A number of recent Schenkerian analysts have adopted a suggestive view, urging that Schenkerian analysis should be regarded as a suggestion for hearing and not as a descriptive theory of perception (though usually without Schenker's insistence that a Schenkerian hearing —indeed, a *particular* Schenkerian hearing—is the only valid one). Forte, for example, has described Schenkerian analysis as "a new way of hearing music" (1977: 6); Schachter's quotes, cited above, reflect a similar view.[10] Others, however, adopt a psychological view of Schenkerian theory. One example is Lerdahl and Jackendoff, whose own theory is greatly influenced by Schenker's ideas; although they do point to a difference in purpose between Schenker and themselves, their acknowledged debt to Schenker's theory seems to imply that it is of great relevance to the "experienced listener" (who "need never have studied music").[11] Even more notable here is Peel and Slawson's review (1984) of Lerdahl and Jackendoff's *Generative Theory of Tonal Music (GTTM),* which compares Lerdahl and Jackendoff's theory to Schenker's throughout (very unfavorably), thus implying that Schenkerian analysis does a better job of fulfilling *GTTM*'s stated goal—describing the hearing of the "acculturated listener" who "need never have studied music"—than *GTTM* itself. Another example is found in John Sloboda's book *The Musical Mind,* where Sloboda offers a lengthy comparison between Schenker and Chomsky. Sloboda finds a number of parallels between the two, and also some differences, but never mentions any difference in purpose between Schenker and Chomsky.

Thus, we can only assume that Sloboda sees the goal (and value) of Schenker's theory as being analogous to Chomsky's, namely, as a description of the mental structures underlying the perception and production of language (or music). In short, Sloboda clearly seems to regard Schenkerian analysis as a psychological theory rather than a suggestive one.[12]

This disagreement in purpose is troubling. It really is a disagreement—although it is rarely acknowledged as such—rather than merely a difference in emphasis, because, as argued above, a single theory can hardly be suggestive and descriptive at the same time: to the extent that it is enhancing listeners' perceptions, it cannot also be describing them. Even more troubling is the fact that some authors seem to claim both purposes for Schenkerian analysis at the same time. Two examples will suffice. Nicholas Cook's article "Music Theory and 'Good Comparison': A Viennese Perspective" is essentially a discussion of the purposes of music theory, and of Schenkerian analysis in particular. Cook begins by questioning the degree to which music theories—among them Schenkerian analysis, set theory, and Lerdahl and Jackendoff's theory—describe the actual listening process. He proposes an alternative goal for music theory, based loosely on the early-twentieth-century Viennese concept of *Darstellung*. By this view, the aim of music theory might be, in Schoenberg's words, to "influence the way in which the sense organ of the subject, the observer, orients itself to the attributes of the object observed."[13] Cook then voices what would seem to be an unequivocal statement of the suggestive purpose of theory:

> And if we accept this view—if we regard an analysis not as an objective representation of musical structure but as a suggestion for how the music can be experienced—then we may find that a number of the problems of contemporary music theory simply evaporate. (1989: 129)

So far, then, Cook would seem to be advocating a shift from the descriptive approach toward the suggestive one. Later, however, his attitude seems to change. In examining a Schenkerian analysis of the first movement of Beethoven's piano sonata op. 90, Cook notes that the middleground structure suggested by Schenker leaves out certain surface features of the music. But, Cook argues, these are obvious anyway.

> What we want an analysis for is to explain the powerful sense of cohesiveness and direction that pervades the discontinuities of the musical surface; and this is precisely what Schenker's sketch does. In the same way, we do not need Schenkerian analysis to tell us that there is

> a break at m. 16; we need it in order to understand why this break seems so curiously evanescent, with the musical motion continuing after it as if nothing had happened. (1989: 132)

Now Cook is suggesting that Schenkerian analysis explains something that we feel about the music. Again, however, to claim that a theory explains the effects of a piece is to present it as a descriptive theory, not as a suggestive one. If Schenker's theory really did (in some way) explain the effects Cook mentions, a strong case could be made that the theory was in some sense describing our mental representations of the piece. But this is a completely different enterprise from coming up with new ways of hearing. It is not only different, but incompatible; how can a single theory possibly be doing both?

Robert Snarrenberg's recent monograph, *Schenker's Interpretive Practice*, also takes up the issue of Schenker's purposes, and the purposes for which his theory might be used. Very early in his discussion, Snarrenberg quotes this remark from Schenker about Hermann Kretzschmar's analysis of Beethoven's Ninth Symphony:

> 'What good is a "guide" if it offers the reader nothing more than what he himself already perceives and knows? . . . "Long measured the way" [a phrase Kretzschmar had used] is undoubtedly the impression that everyone receives from the principal idea; wasn't Kretzschmar's task rather at least to indicate correctly the technical means that led to such an effect?' (qtd. in Snarrenberg 1997: 7)

Snarrenberg elaborates Schenker's comment as follows: "What the readers of a guidebook presumably cannot know readily from their own experience —and what Schenker is convinced readers ought to desire to know—is how the arrangements of tones crafted by a composer can result in anything like a 'trait of suffering' " (1997: 7). Thus, according to Snarrenberg, Schenker sees the goal of analysis not as enhancing people's experience of a piece—listeners already perceive the 'trait of suffering'—but, rather, as explaining how this experience came about. Yet just a few sentences later, Snarrenberg writes:

> Composition and interpretation are complementary activities centered on tonal content. Composers intend to produce effects or responses in others by means of configuring tones in such and such a manner. Listeners hear (or imagine hearing) the presented configuration of tones and respond appropriately. . . . For this complementary relation to hold, composers and listeners must be disposed to re-

> spond in similar ways to tonal configurations. The point of Schenker's interpretive practice is just to bring about that sharing of mental disposition, to do so by bringing noncomposers' minds into line with what he believed to be the mental disposition of the German composers of the eighteenth and nineteenth centuries. (1997: 7–8)

Once again, the purpose has now shifted. Rather than explaining the effects of music on the listener, the goal is now to *improve* listeners' hearing so that they can respond to the music in appropriate ways. One might argue, in Snarrenberg's defense, that he was only trying to explain Schenker's own contradictory and inconsistent purposes. Still, Snarrenberg would have done a service by drawing attention to this contradiction, especially since, as we have seen, it remains very much present in the thinking of theorists today.

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The division I have posed might in some ways seem oversimplified. I have argued that, to the extent that a theory is describing our perceptions, it cannot also be enhancing them. However, a theory might be descriptive in some aspects, but suggestive in others; for example, one might argue that Schenkerian structures are descriptively valid for many people at a local level of structure, but at higher levels (e.g., the *Urlinie*), they are best regarded suggestively. A theory might also have a different status for different people, or for the same person at different times. This last point is a particularly important one, since it indicates what might seem to be a fundamental convergence between suggestive and descriptive theory. Let us consider set theory, which I think is widely construed—and rightly so—as an entirely suggestive theory; that is, it serves to enhance the hearing of people who study it. (Perhaps one should consider it a set of analytical tools rather than a theory, but this does not affect the present point.) Once someone studies set theory, it is descriptive of their hearing (or at least their understanding) of certain pieces. Even then, however, there would be little justification for calling set theory a "psychological theory of music theory students." By the same token, we could call classical mechanics a "psychological theory describing the knowledge of physicists," but this would seem odd. To call something a psychological theory, it seems to me, implies that it has some kind of psychological validity *beyond* what is due to people's explicit study of the theory.[14] We should note also, however, that in the course of studying set theory (again, regarding it for the moment as a purely suggestive theory), one undoubtedly acquires all kinds of tacit and unconscious knowledge that is brought to bear in doing set-theoretical analysis, and this could be studied in a psychological way,

just as psychologists study the tacit knowledge involved in physicists' problem solving. (Whether any existing work in music theory could be regarded as "psychological" in this way is, I think, doubtful.)

In short, a question such as "Is the value of Schenkerian analysis as a descriptive or a suggestive theory?" undoubtedly has a highly complex answer. A theory may be suggestive in some aspects, descriptive in others; and it may be suggestive and descriptive to different degrees for different people. My own view is that the truth about Schenkerian analysis lies somewhere in this middle ground. However, I believe these complexities must be confronted. To simply offer vague and conflicting generalities—or to evade the issue altogether, as much theory does—is not the solution. Such an attitude has led us to a situation of profound confusion, in which the status and value of music-theoretical systems is altogether unclear. For those who are primarily interested in psychological theory, one question of great interest is this: To what extent can we take Schenkerian theory as a successful model of people's perception and cognition of music (beyond what is due to explicit study of the theory) and, hence, as contributing to an explanation of their musical experience? This is not the only interesting question one could ask about Schenkerian analysis, but it is surely one interesting question—those who have never studied Schenkerian analysis include many listeners of classical music today, as well as *all* listeners prior to Schenker—and it is a question to which there is an answer, though undoubtedly a complex one. Until the difference—and essential conflict—between the suggestive and descriptive goals of theory is recognized, however, it is difficult to see how progress can be made on this question.

Another regrettable consequence of this confusion of purpose is that it has led to serious misunderstandings with psychologists. Suggestive music theories are sometimes subjected to unfair criticism, and inappropriate tests, because their purposes are not understood. For example, Eric Clarke criticizes analyses that are based on mathematical relationships such as the Fibonacci Series, which, in his view, do not characterize people's hearing (1989: 11). The validity of this criticism depends entirely on the aim of the analyses in question (he cites none specifically). If the aim of a particular Fibonacci analysis is to suggest to people a new way of hearing a piece, then *of course* the analysis does not characterize their hearing before they read it; it would be a failure if it did. As another example, Cheryl Bruner tested subjects' intuitions about similarities between pitch sets, to determine whether these intuitions corresponded with Robert Morris's measure of pitch-class set similarity.[15] The subjects' responses to set similarity did not correlate well with Morris's measure. Such an experiment seems somewhat misconceived; Morris's set similarity measure is surely best regarded not as a cognitive model, but as a tool for helping analysts find in-

teresting ways of hearing pieces (or, perhaps, for composing music that is interesting to analyze). On the other hand, theorists are so often unclear about their purposes in doing analysis that others must be forgiven for sometimes misunderstanding them. Indeed, Morris himself claims that his measure provides a "rationale for the selection of sets that insure predictable degrees of aural similitude" (1979: 446). This sounds very much like a psychological claim—that his measure predicts the "aural similitude" of sets—which a psychologist might quite reasonably want to test.

My claim that music theory is confused about its purpose might strike some as unfair, for, one might argue, a similar mixture of purposes can be found in other fields as well, including some branches of cognitive science. This is true; however, it is instructive to consider how this situation has been handled in cognitive science. An illustrative example is the study of decision-making. Early theories of decision-making involved highly rational and consistent models, which were assumed to be models of actual human cognition. Subsequent experimental work revealed, however, that human decision-making was frequently not rational in this way. Since then, there has been a clear demarcation in the field between normative models of decision-making, which are highly rational and coherent (and are sometimes used to *aid* people in decision-making—for example, in making choices about medical treatments), and descriptive models, which describe how people actually do make decisions.[16] I suspect that if someone were simply to present a "model of decision-making" without specifying whether it was a model of how people should make decisions or how they do make decisions, this would be regarded as strange. The same is true of artificial intelligence. A research project in AI may seek to model human performance of some task; alternatively, it may simply seek to perform the task with maximum success (perhaps with some practical application in mind). However, there is a very strong awareness in AI that these two purposes are very different and that a system that succeeds at one task may well not succeed at the other.[17] An even clearer case is linguistics, where the distinction between prescriptive and descriptive linguistics was recognized long ago, and it was resolved that the proper domain of linguistic research was the latter rather than the former.[18] This is in contrast to music theory, where there seems to be confusion as to whether the suggestive and descriptive goals are even distinct.

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At this point it might be useful to consider a concrete example. I am currently working on a study of tonality in rock music: What are the factors in rock songs that determine the tonal center? (The problem, in brief, is that the main factors in tonal implication in common-practice

tonal music are absent in rock. In common-practice music, each major or minor key has a unique pitch-class collection—a major or harmonic minor scale—that largely serves to establish it; cadences are also a major factor in key implication. In rock, the same pitch-class collection seems to imply different tonal centers in different cases, and there are no obvious cadences. Thus tonal implication must rely on other factors.) My *modus operandi* is the usual one of descriptive theory. I examine my intuitions as to what the tonal center is in many rock songs, assuming that these intuitions are the same as those of most other listeners. This is, of course, a huge and problematic assumption, as I discuss below. I then search for factors that might explain these judgments. Is the tonal center usually a pitch-class that is particularly prominent in the melody (metrically, durationally, or because of placement at large-scale structural boundaries)? Is it particularly prominent as a harmonic root? Are there perhaps conventional harmonic or melodic gestures which function to establish tonal centers in rock, analogous to cadences in common-practice music? If I am able to come up with a model or algorithm which accurately predicts judgments of tonal center in rock songs using these kinds of information (or others), then I have a theory—a conjectural explanation—for how judgments of tonal center are made.

Suppose my assumption of perceptual uniformity is false: many listeners (including, let us say, some readers of my paper) do not agree with my opinions about what the tonal center is in many rock songs. In that case, the validity of my theory is in doubt, because the data I am trying to explain (my intuitions about tonal centers of rock songs) do not adequately represent what they are supposed to represent (other people's intuitions). Now, it is possible that some of these dissenting readers—readers whose intuitions about the tonal centers of songs disagree with mine—will find my opinions about the tonal centers of rock songs (and perhaps also my arguments about the factors involved in tonicization, in general and in specific cases) to be musically interesting nonetheless; they might even be led to reconsider their own judgments. ("Maybe he's right that the tonal center of this song is C, not G, as I originally heard it; the very prominent C-major harmony supports this view.") In this case, my theory would have some suggestive value, along with whatever descriptive value it may have. So much the better, one might say; it is no disaster if an analysis serves a purpose other than the one for which it was originally intended. However, I think we should be very careful about trying to do both descriptive and suggestive analysis at once, or remaining noncommittal between them, in the hope that something like this might happen. The reason is, simply, clarity of purpose. As authors, we generally try to be clear in our own minds about what we are claiming, and what the basis is for our claims,

and we try to make this clear to our readers as well. Surely this should apply, *a fortiori,* to our underlying purpose. It is true that both descriptive and suggestive theory each involve large and problematic assumptions: in descriptive theory, we hope that others hear things the way we do; in suggestive theory, we hope that others don't hear things the way we are proposing, and will find our new hearing useful. But this is all the more reason for being clear about our purposes, so that the validity of our assumptions can be clearly examined, and the success of our work fairly judged.

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I cannot emphasize strongly enough that my aim in this paper is *not* to recommend descriptive theory over suggestive theory, or to denigrate suggestive theory in any way. My main point has been that analysis that is intended as suggestive cannot claim to explain our musical experience. But the more limited goal which suggestive analysis *can* rightfully claim—enhancing our understanding and appreciation of music—is, in itself, enormously worthwhile. Indeed, a successful suggestive analysis—one that enriches the musical experience of those who read it—is a valuable end in itself, in a way which descriptive analysis is not. I can see no legitimate objection to either descriptive or suggestive theory in itself; what is problematic is the combination of the two.

Nevertheless, I will not deny that my own research interests lie mainly in the area of descriptive theory, and that one of my objectives in this paper has been to make a case for descriptive theory as a coherent and worthwhile enterprise. I will close by addressing two objections that might be posed to this enterprise as I have outlined it here.

The premise of descriptive music theory is that, through introspection of our experience of pieces, we can make claims about our mental representations of music—claims which will be valid not only for ourselves (and other theorists), but also for some kind of broader population of listeners (musicians and non-musicians) familiar with the style. This premise might seem dubious, to say the least. Our listening to music is surely deeply influenced by our theoretical knowledge, knowledge that—in the case of music theorists—is highly specialized and unusual. Even more insidiously, our hearing may be affected by whatever models or ideas we may be currently entertaining. Therefore, one might argue, it is a fantasy to suppose that we can introspectively observe, in some detached way, whether our hearing of a piece is characterized by particular theoretical structures or relationships, because our hearing may well be affected by the very theoretical ideas we are considering. Given the futility of the descriptive approach to analysis (at least through introspection), then, an openly suggestive one is

the only defensible approach. The following statement by Jean-Jacques Nattiez reflects this view:

> Now, the difficulty of the esthetic position [the position of trying to describe music from the listener's viewpoint] in the case of harmonic analysis is that knowledge and a priori theories are one of the controlling factors in perception. We run the risk of being trapped in circular reasoning: from the moment that functional formulas based on the circle of fifths satisfactorily explain harmonic progressions, are we not going to *hear* in terms of the theory? By necessity, analytical thematicization always influences perceptual orientation. (1990: 211)

This is certainly a potential problem with descriptive music theory, one that must be taken very seriously. However, I do not believe it is a fatal problem. Again, an analogy with linguistics may be helpful. Linguists routinely make use of their own judgments about linguistic well-formedness and other things (such as whether two sentences are synonymous, or whether two words in a sentence can refer to the same thing). One might argue that, as a linguist entertains a theory of some aspect of syntax, the theory may well be influencing her syntactic processing of language (particularly since, having formulated her model, she *wants* it to successfully predict cases she considers later); there is, then, a danger of circularity. I think most would consider this a silly objection; our judgments about syntactic well-formedness are not much affected by theoretical knowledge about syntax or anything else. Yes, one might respond, but music is not like language in this respect. Perhaps it is not; this is an empirical question. My only point in making the analogy is to show that there are some highly complex cognitive domains that are not significantly affected by any amount of introspection or theoretical knowledge about them. It is at least a possibility, then, that some aspects of musical cognition remain unaffected as well.

If some aspects of music cognition are little affected by theoretical knowledge, then we may examine them introspectively without fear of changing them, and we may also hope that these aspects are fairly uniform across a population of people with (in some ways) very widely varying backgrounds. But what actual evidence is there for this? Some have expressed doubt that any of the constructs posited by music theorists—even avowedly descriptive theorists, such as Meyer and Lerdahl and Jackendoff—have much relevance to the way even music theorists listen, let alone ordinary listeners. Cook questions the psychological reality of even the most basic aspects of musical structure, assumed by music theorists of all kinds:

> [W]hen people listen to music in the ordinary way, they don't hear pitches and time-points. To be sure, they hear tunes and harmonies, which are broken up on the page into distinct notes, but they do not hear the notes as separate entities and indeed they sometimes do not hear them at all, at least in a manner that directly corresponds to what is visible in the score. (1989: 121)

While Cook seems to accept the psychological reality of tunes and harmonies here, the same argument might equally be applied to these. After all, do we not have to spend years teaching undergraduates to understand —in large part, to *hear*—tonal harmony in the "correct" way?

What needs to be remembered here, however, is that the kind of "hearing" at issue—both in Cook's comments and in my following rhetorical question—is conscious hearing. And what chiefly concerns us in descriptive theory—as in cognitive psychology and cognitive science—is precisely what is *not* conscious. The whole point of studying cognition is that there are many things going on in our minds of which we are not immediately aware, and cannot easily access via direct introspection. Evidence for these unconscious processes and representations must be sought in more indirect ways. In the case of pitches and time-points, we might ask: Is it reasonable to posit the mental representation of pitches and rhythmic values as a means of explaining people's processing of higher-level musical entities—for example, their ability to recognize tunes, or identify the emotional connotations of harmonic progressions (major versus minor, for example)? Indeed, is it even *possible* to explain these phenomena without such low-level representations?

Common-sense reasoning can offer provisional answers to these questions, but ultimately it is music psychology that will decide the psychological reality of music-theoretical structures. There is already a large body of experimental data relating to music cognition, often comparing the judgments of listeners with varying levels of training and musical sophistication. The picture is, not surprisingly, very complex. Many studies have shown significant differences between trained and untrained subjects, and between the representations formed by listeners—even highly trained ones —and those assumed in music theory.[19] I am more struck, however, by the degree to which even untrained listeners reflect knowledge of basic aspects of musical structure—harmony, key, melodic implication, meter, motivic relationships, phrase structure, cadences, and so on—and an ability to interpret them in theoretically sophisticated ways. And it hardly needs to be said that there is much to be learned about even these basic aspects of musical structure: the way they are formed, the way they interact with each other, the way they give rise to higher levels of emotional response

and meaning, and so on—issues to which music theory could greatly contribute.[20]

My aim in this section has simply been to suggest that skepticism about the feasibility of introspective, descriptive music theory—as exemplified by Nattiez's and Cook's comments—may be unfounded. The validity of the descriptive approach remains a somewhat open question, and will probably not admit of an easy answer, but surely it warrants further exploration. And if our goal is truly *explanation*—finding out how music does what it does—it is the only way to go.

Notes

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1. On the problem of confirmation, see Brown and Dempster (1989: 65–106). On the relevance of historical context, see Taruskin (1986: 313–20) and Forte (1986: 321–37). On the relevance of the composer's intentions, see Haimo (1996: 167–99).

2. Meyer (1973: 6–9) distinguishes between "critical analysis" (the exploration of the unique features of a piece), "style analysis" (the study of general features of a style), and "theory" (the study of more general principles of musical structure). In the quote above he is discussing critical analysis, but I believe he would maintain a similar position on the current issue with regard to style analysis and theory as well (see, for example, 1973: 7–8).

3. For discussions of this issue, see Chomsky (1980: 11–24, 189–97) and Fodor and Pylyshyn (1988: 3–71). Fodor and Pylyshyn observe that even in the debate between connectionist and symbolic approaches to cognition—a debate that is in some ways very fundamental—both sides agree on the necessity of mental representations. There have been, and continue to be, alternatives to the representational approach. One is behaviorism; another is the "direct perception" theory of J. J. Gibson (see Bruce and Green 1990: 381–89 for discussion).

4. One might wonder if Schachter's first quote implies that the purpose of analysis is to make conscious—available to "direct perception"—what was formerly unconscious. But the second quote seems to imply that the goal of analysis is to reveal things not heard even unconsciously.

5. DeBellis (1995, chapters 5 and 6) argues that music-theoretic models might be regarded as causal explanations for psychological responses to music, without necessarily being mentally represented.

6. See also Monelle 1992.

7. The purposes just mentioned—studying the score in an objective manner, or uncovering the composer's intentions—interrelate in complex, and not always conflicting, ways with the suggestive and descriptive purposes described earlier. For example, one might argue that seeking to reveal the composer's intentions in

a piece (by analysis, or perhaps by historiographical or other means) is a good strategy for finding an informed and satisfying hearing of it.

8. Schenker 1979: xxii. See also Schenker 1987 1:xviii, xix.

9. See, for example, Schenker 1987, vol. 1, pages 10, 53, 84, 92, 96, 149, 183, 191, and 207. For thoughtful discussions of Schenker's claims and purposes, see Dubiel (1990: 291–340) and Blasius (1996). While both of these authors find fault with Schenker's arguments in various ways, neither one acknowledges what I see as the most serious fallacy in his reasoning: the conflict of purpose discussed here. One might argue that these conflicting claims in Schenker represent differences in purpose between his works, or different stages of his thinking. But this does not seem to be the case; *Counterpoint* contains both suggestive and psychological claims, as my citations show.

10. See also Schachter (1981: 122–23) and Benjamin (1981: 160, 165).

11. For Lerdahl and Jackendoff's discussion of the difference in purpose between Schenker's theory and theirs, see 1983: 337–38.

12. Sloboda 1985: 11–17. Sloboda distinguishes between the linguist, who studies linguistic structure, and the psycholinguist, who studies actual psychological processes involved in language. This characterization is not ideal, since it implies that linguistic structure is something non-psychological, that is, outside the mind. Chomsky quite clearly sees his theories as descriptions of mental structures and processes (see, for example, 1980: 11–24, 189–97). Indeed, Sloboda himself admits that musical and linguistic grammars must be mentally "represented" (16); presumably, a grammar could be taken as a description of these mental representations. At the very least, then, Sloboda seems to consider both Chomskyan theory and Schenkerian analysis to be something like what I call descriptive theories—in any case, certainly not what I am calling suggestive theories. A further point: Chomsky's theories apply to production as well as perception, and Sloboda applies Schenker's theories to production also, taking them as a description of the mental structures involved in composers' creative processes. My concern here, however, is with only perception.

13. Quoted in Cook 1989: 124. Schoenberg here was referring specifically to "efforts to discover laws of art," but Cook points to this as a worthy goal for music theory.

14. Along the same lines, one could object: "But in doing a suggestive analysis, by the time I finish it, it does characterize my hearing. Therefore it is also psychological." Again, a geologist could say the same thing: "By the time I completed my theory of tectonic plates, it described my thinking about them." By this criterion, geology is psychology. The fact that an analysis comes to characterize a theorist's hearing simply through doing the analysis does not make it psychological in any usual sense of the term.

15. Bruner 1984: 25–39. Morris's measure is presented in Morris 1979: 445–60.

16. For discussion, see Slovic 1990: 89–100.

17. For discussion, see Garnham 1988: 8–16.

18. See Lyons (1981: 47–54) and Pinker (1994, chapter 12). One might draw a parallel between the "prescriptive/descriptive" distinction in linguistics and my "suggestive/descriptive" distinction here. However, I do not wish to impugn

suggestive music theory with the negative associations attached to prescriptive linguistics—hence my use of the term "suggestive." (Actually, despite what some linguists say about prescriptive linguistics, the enterprise of enhancing and expanding people's use of language is a perfectly valid and well-accepted one, and is a huge part of our educational system. Thus it is not really clear why the term "prescriptive" should be used or taken pejoratively.)

19. Cook's own experiments on tonal closure—showing that listeners are often unable to detect whether a piece began and ended in the same key—are a sobering case in point (1987: 197–206).

20. An important caveat: In claiming that aspects of music cognition may be largely uniform across a population of listeners, I am not at all claiming that these aspects are innate. Rather, I think it is clear that many aspects of music cognition —even very basic ones—are learned: tonal harmony, for example. But it is perfectly possible that such learning takes place largely from exposure, rather than from explicit theoretical training. Again, the parallel with language is apparent.

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