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MUSIC THEORY AND 'GOOD COMPARISON': A VIENNESE PERSPECTIVE

Nicholas Cook

Most contemporary theorists feel uncomfortable about ascribing significance to inaudible relationships in music; we tend to assume that there should be some meaningful relationship between analysis and auditory experience. There is no obligatory reason why this should be so. The idea that what is significant in music should coincide with what is perceptible in it is not a universal one; according to Dahlhaus[1] this is no more than a dogma of the last two centuries or so, while even today there are some types of non-Western music whose structural organization lies principally in the physical actions involved in performance, rather than in the sound.[2] And David Lewin has recently argued that as theorists we place too much emphasis on perception as distinct from the broad range of activities through which people express their responses to music.[3] But the fact remains that, in the West today, it is the perception of musical sound that is generally considered to be paramount in defining the meaning of a piece of music. Hence one of the most crucial questions we can ask about any theory of music—one which bears directly upon the validity which we can ascribe to it—is how it relates to the perceptual experience of the listener. In this article I try to show the problems inherent in any simple answer to the question, and to outline a rationale for the practice of analysis that overcomes these problems.

I

The simplest possible relationship between analysis and auditory experience would be if the analysis consisted of a list of "things to hear". Essentially this is what the much-abused formal analyses produced around the turn of the present century consisted of: first the first subject, then a bridge passage, then the second subject and so forth. Now there is no doubt that people can be quite easily taught to hear these things. Some fifteen years ago, Alan Smith[4] carried out a series of experiments in which seventh-grade students were given instruction in classical forms. They were then played curtailed recordings of classical works and had to identify the structural point at which the music stopped. The seventh-graders performed quite well. Perhaps more revealing, however, was a further series of tests which Smith carried out, in which music majors were tested in the same way. In the first test, in which they did not know what they would be required to do, these more advanced students performed poorly. But in subsequent tests—when they knew what was expected—they did well. What this shows is that people who know how to track the musical form as they listen—how to identify the first subject, the transition and so on—do not in fact do so in the ordinary way. And this is perhaps not surprising, given what many theorists, following Schenker, see as the superficial and even meaningless nature of these traditional categorizations of musical form.

Schenker's attack on the categories of traditional formal analysis rather resembles a phenomenological reduction. He "brackets" them in the same way that phenomenologists strip off the sedimented accretions of historical or theoretical knowledge, so that the underlying phenomenon will reveal itself to consciousness. But Schenker's theory of music is not in fact a phenomenology in any normal sense. He reduces music, not to a phenomenon, but to a theory of structural levels. Schenker believed that only the genius is directly aware of music's large-scale structure—of the *Urlinie* and its transformations, that is to say. People who lack genius cannot become aware of this directly; they can only do so through the mediation of the conceptual representation of the music provided by the analyst.[5] So it would not be correct to think of Schenker's analyses as attempts to represent the manner in which listeners ordinarily perceive music; as Ruth Solie says, "Schenker predicates his notion of totality not upon perceptual mechanisms in the observer, but upon the work of art itself".[6] I shall return to this at a later point.

Our contemporary interest in Schenkerian analysis, however, derives largely from the fact that it does somehow seem to reflect the auditory experience of music. And in the adaptation and extension of Schenkerian theory offered by Lerdahl and Jackendoff in their *Generative Theory of Tonal Music* this perceptual basis is made explicit, for the authors describe their work as a model of "the organization that the listener attributes to the music

he hears".[7] Though this listener—the "experienced listener", as Lerdahl and Jackendoff sometimes call him—is an abstraction (because individuals may vary in the manner in which they hear a piece), the theory is put forward as "an empirically verifiabe or falsifiable description of some aspects of musical organization, potentially to be tested against all available evidence from contrived examples, from the existing literature of tonal music, or from laboratory experiments". So far the only such tests to have appeared in print are those by Irène Deliège,[8] and they do provide some confirmation of Lerdahl and Jackendoff's model of tonal perception. But two qualifications need to be made. In the first place, Deliège's subjects were given sheets on which each note of the music was shown as a dot, and asked to indicate the segments into which the music fell. So they were not simply listening; they were making decisions on the basis of a visual representation of the music. And this raises a problem to which Burton Rosner called attention in his review of Lerdahl and Jackendoff's book, in which he said that what the authors really seem to be talking about are

> the intuitions of a sophisticated listener *who has the score of the piece!* The present theory (and others that claim perceptual validity) generally rest on the tacitly accepted but little noticed use of scores. . . . Lerdahl and Jackendoff are not alone in assuming that complex hierarchical trees in *visual* form are isomorphic to the auditory results of listening, which necessarily occurs across time and involves memory. This assumption seems very dubious psychologically, when carried to larger levels.[9]

Rosner's mention of larger levels raises the second point I mentioned. Deliège's tests were concerned only with the perception of small-scale structure, on the level of phrases rather than extended formal sections. Now Lerdahl and Jackendoff treat large-scale structure on the model of small-scale structure; like Schenker, they model the unity of a movement on the unity of a phrase. But it is by no means obvious that listeners hear movements as integrated structures in the same way as they do phrases. Rosner elaborates this point in an article co-authored with Leonard B. Meyer, and draws attention to an important distinction between hierarchical theories of music and the linguistic theories with which they are often compared:

> The top node of a grammatical tree is an immediately observed datum: a sentence or an utterance. . . . The lowest nodes in music-theoretic tree structures, however, represent a datum: an actual stretch of music. Quite often, only fragments of it are held in memory. . . . We cannot therefore believe that the increasingly higher nodes, which represent ever more rarified selections, form the core of music perception.[10]

And the general point that Rosner and Meyer are making extends beyond strictly hierarchical analysis. The concept of large-scale tonal closure—the idea that a movement or even a multi-movement work should begin and

end in the same key—is fundamental to our understanding of classical form, but experiments which I have published elsewhere[11] suggest that listeners only have a direct perception of tonal closure when the time-scale involved is in the order of a minute or less. In other words it seems that when we talk about the tonal coherence of movements lasting several minutes, we are not talking about what people actually hear at all (or at least most people). Yet it is this kind of coherence which forms the primary focus of the majority of analytical studies of music. In this way the perceptual reality of what the music theorist is saying seems to become parlous just at the point that it becomes of the greatest aesthetic interest.

Eugene Narmour and Alan Keiler,[12] among others, have pointed out the implausibility of Schenkerian theory as a theory of perception (something which, as I said, it was never originally intended to be). The basis of their criticisms is that Schenker's middleground and foreground are only defined by the background, so that the perception of the parts presupposes the perception of the whole—a model of perception which it is difficult, if not impossible, to maintain. Both Narmour and Keiler have accordingly been trying to develop models of musical structure which are genuinely reductive, in the sense of showing how the listener can derive a structural interpretation of the music from the observed phenomena of the musical surface.[13] But the best-known attempt to model musical structure in this manner is the one that Benjamin Boretz published in six issues of *Perspectives of New Music* under the general title "Meta-Variations".[14]

The basis of Boretz' system is what he called the "qualia" of pitch and time-ordering, which constitute the building-blacks of musical perception. These are structural categories: they represent the smallest difference in pitch or time that is of significance within a given musical culture. The smallest significant interval in serial music, for instance, is the tempered semitone; a flat D♭ is still a D♭, and as such categoricaly distinct from a C, even a sharp one. (In another culture, however, the flat D♭ might represent a different structural category from the in-tune one.)[15] Now it is obviously important that people should be able to discriminate between different structural categories—it would never do to have music in which there were a hundred different structural values to the semitone. But this has nothing to do with the musical structure as such. It is simply a question of the general psycho-acoustical constraints within which music must operate, and as Babbitt (whose influence is felt throughout *Meta-Variations)* puts it, "the discovery and formulation of these constraints fall in the province of the psycho-acoustician".[16] In other words a rigid methodological division is made between the psycho-acoustician's investigation of auditory perception, and the music theorist's investigation of musical structure.

In line with this, the rest of *Meta-Variations* consists of formalized models of some of the ways in which the qualia of pitch and time-ordering can be organized in order to yield different types of musical structure.

There is hardly any direct consideration of the manner in which listeners perceive these structures; the theoretical assumption seems to be that as long as the building-blocks of musical structure—the qualia—are perceptible, any structure built out of them ought to be perceptible too.[17] And Babbitt makes such an assumption explicit when, speaking of serial music, he says that the transformations of a series S "require for the perception of their relation to S merely the ability to identify interval classes".[18] Now this remark is in flat contradiction with the results of a considerable number of experimental studies which have shown that listeners are not, in fact, generally able to perceive such relationships.[19] And this is not surprising, because Babbitt's statement involves an illegitimate generalization between two quite different contexts of listening. It is worth going into this in some detail.

In his writings, it was Babbitt's avowed aim to develop a scientifically adequate approach to music theory, that is to say one whose terms could be defined in a rigorous, abstract manner.[20] But people's subjective experiences of music cannot be analyzed in rigorous, abstract terms. Scores, on the other hand, can. To this extent, Babbitt's analyses of music are, in actuality, analyses of musical scores. And scores consist, among other things, of specifications of pitch and time-point. However, when 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. Of course there are certain circumstances, for instance in the ear-training class or the psychologist's laboratory, where people make a special effort to hear music in terms of distinct notes and rhythmic values. But even under such conditions, there are severe limits in the extent to which this can be done; there is a drastic asymmetry between the music that listeners can cope with as an ear-training exercise, and what they can appreciate in the concert hall.

If the notes in the score do not represent things that people ordinarily hear, then, do they have some kind of objective reality? A pianist plays notes, in the sense that he presses keys. But singers do not sing notes in this sense; indeed they sometimes glide from one note to another in such a way that it becomes hard, or even arbitrary, to say where one note stops and the next one starts. In such cases, or in the case of jazz saxophone playing, or a rapid scale played on the violin—or any number of avant-garde pieces—the notes cannot be said to be in the sound at all in any objective sense. As Mary Louise Serafine puts it, they "arise only as a result of reflection *upon* music and notation *of* it".[21] In other words the score does not directly correspond to any psychological or acoustical reality. It is not even an approximation to reality. Instead it is a model, or a metaphor,[22] based on a comparison between the experience of musical sounds in an actual musical

context and the judgements of pitch or interval that would be made if the same sounds were heard individually. What Babbitt has done is to confuse the primary and the secondary subjects of the metaphor—that is, to treat the model of ear-training on which the score is based as if it were the musical reality that it represents.[23]

If in a significant sense the formalized music theory of Babbitt and Boretz is a theory of musical scores, then the same is also true of both Schenker and Schoenberg. Boretz' notorious statement that "sounds . . . are not part of music, however essential they are to its transmission"[24] echoes something that Schoenberg told Dika Newlin in 1940:

> Music need not be performed any more than books need to be read aloud, for its logic is perfectly represented on the printed page; and the performer, for all his intolerable arrogance, is totally unecessary except as his interpretations make the music understandable to an audience unfortunate enough not to be able to read it in print.[25]

What this implies—and there are other statements by Schoenberg which tend to the same conclusion—is that the work exists as an ideal entity essentially distinct from its acoustic realization. And Schenker spelled this out more specifically in an article from *Das Meisterwerk* called "Let's do away with the phrasing slur".[26]

Schenker's main purpose in this article is to distinguish between the editorial phrasing slur—which is simply an instruction for immediate execution—and the legato slurs found in the autographs of the master composers, which (according to Schenker) constitute an essential part of the musical structure. Indeed the basic proposition Schenker is putting forward is that "the masters' manner of notation represents the most complete unity of inner and outer form, of content and symbols" (p. 55). For this reason, he continues, the composer's "struggle over notation always goes hand in hand with a struggle over the content; but once the content is worked out, then the only possible notation is also immediately present" (p. 74). For Schenker, then, the score is not a representation of the music; it *is* the music. In a musical sense, therefore, a score is realized not through being performed, but through being understood as an organic whole, an expression of the *Urlinie.* This explains Schenker's otherwise strange comment about the analytical graphs in *Der Freie Satz* that "the graphic representation is part of the actual composition, not merely an educational means".[27] And it is in this sense—a decidedly Hegelian one—that, as Ruth Solie said, Schenker's theory is based not on the listener's perceptions, but on the work of art itself.

We have arrived at a paradox. Babbitt and Boretz' attempts to develop a rigorous theory for music—one which will meet the demands of scientific discourse—turn out to be based on a conception of the score that makes more sense in terms of nineteenth-century metaphysics than twentieth-

century psychology. Nor is this the only way in which supposedly scientific explanations of music turn out to be based on aesthetic presuppositions of limited historical or geographical scope. John Rahn justifies what he calls "the presumptuous assumption that our interest is primarily focused on particular pieces of music' on the grounds that 'after all, we never musically listen to anything else".[28] But, unless we are willing to gloss "musically" as "analytically" (in which case the statement becomes more or less circular), this is not as obvious as Rahn implies. For, as Patricia Carpenter has shown,[29] the concept of the musical work as a reified entity is one that only developed around 1800; and, in Dahlhaus' words, though the concept of the composition seemed "self-evident in the nineteenth century according to the letter of the aesthetic law, it was of restricted validity and always in peril from the context of actual musical behaviour".[30]

Certainly the tests I mentioned earlier, in which listeners failed to respond to large-scale tonal closure, would support this last remark. Were the subjects of these tests musically listening to particular pieces of music, to borrow Rahn's words, in such a way that they experienced the particularity of a work as a consequence of its structural organization? Apparently not; and this means that, if these listeners experienced what they heard as compositions, this must have had as much to do with their aesthetic preconceptions—the interpretative attitude with which they approached what they heard—as with the music's structure. Or perhaps they experienced the music in the same way as background music, and not as constituting particular pieces at all—not "musically", to use Rahn's term. Either interpretation is uncomfortable from the theorist's point of view. In the one case the unity of the work lies in the province of historian or even the sociologist, since it is the result of changing aesthetic attitudes; in the other case there is no work, and hence no focus for music theory at all. A better account of the relationship between perception and music theory is needed if the discipline is not to collapse into the history and sociology of aesthetics on the one hand, and the psychology and pedagogy of note-to-note structure on the other.

II

Of course there is an easy way to solve these problems, though it means dispensing with the image of a scientific music theory. This is to maintain that the purpose of analysis is not to reflect how people listen to music, but to explain how they ought to. Such was Schenker's position. Like Hanslick, Schenker was reacting against what he saw as a decline in Western musical culture, a decline that stemmed from a failure of hearing. Schenker says in *Das Meisterwerk* that "theorists as well as performers . . . plod along from one passage to the next with the laziest of ears and without the slightest

musical imagination. All they hear is the constant change between tonic and dominant, cadence after cadence, melodies, themes, repetitions, pedal point".[31] It is this that Schenker's work was intended to combat. And for Schenker, of course, there was no question of his imposing his own personal tastes or values upon other musicians: the *Urlinie* and its transformations were not something he had dreamt up to express or rationalize his own aesthetic viewpoint, but part of the objective structure of the musical masterwork.[32] But if we cannot today accept Schenker's Hegelian views, then the idea that analysis says how music should be heard becomes problematic. A prescriptive stance may easily appear to be purely authoritarian (*this* is how you should hear the music), with the only alternative being an unbridled relativism (this is how I hear the music; you can hear it as you like).

What we need is a rationale for adopting Schenker's analytical methods while rejecting his epistemology. And a clue to how such a rationale might be formulated can be found in the concept of *Darstellung* as developed by other writers in Vienna during the early decades of the present century[33]—just the time that Schenker was bringing his own theories to fruition. Schenker seems to have been unaffected by these broader intellectual developments. Schoenberg, however, was not. In the first chapter of his *Harmonielehre* he attacks the work of previous theorists who put forward what they claimed to be the natural laws governing music. Schoenberg does not deny that such natural laws exist. But nobody, he says, has yet discovered what they are, and he adds, "I believe they will not be discovered very soon".[34] However that does not mean that there is no place for music theory. Schoenberg continues:

> Efforts to discover laws of art can . . . , at best, produce results something like those of a good comparison: that is, they can influence the way in which the sense organ of the subject, the observer, orients itself to the attributes of the object observed. In making a comparison we bring closer what is too distant, thereby enlarging details, and remove to some distance what is too close, thereby gaining perspective. No greater worth than something of this sort can, at present, be ascribed to laws of art. Yet that is already quite a lot.

And he goes on to outline the pedagogical value of this kind of theory:

> What we do achieve can be enough, if it is given as a method of teaching, as a system of presentation *(Darstellung)*—a system whose organization may aim, sensibly and practically, towards the goals of instruction; a system whose clarity is simply clarity of presentation, a system that does not pretend to clarify the ultimate nature of the things presented

—as, of course, Schenker's theories do.[35]

If Schoenberg put forward his concept of *Darstellung* by way of a criticism of other writers in the field, the philosopher Ludwig Wittgenstein—

another Viennese citizen—turned the same arguments against his own earlier work. Wittgenstein's *Tractatus Logico-philosophicus,* which was first published in 1919, was an attempt to reduce language to its formal essentials; and like Boretz' *Meta-Variations,* which belongs to the same intellectual tradition,[36] it makes use of propositional calculus for this purpose. When he first completed the *Tractatus,* Wittgenstein believed that it provided answers to all the principal problems of philosophy; he said as much in his preface to the work. But in later years he came to think that the elaborate system of the *Tractatus* had, in fact, amounted to no more than a system of presentation. Janik and Toulmin put it like this (p. 190):

> The propositional calculus had attracted Wittgenstein, in the first place, as the intellectual instrument required for a fully rigorous "critique" of language in general. By the time he had finished, it turned out to have given him only the scaffolding for an elaborate metaphor. Unless one *saw* the possibility of modeling "facts" by "propositions" having the same "real logical form", no independent demonstration was possible to prove that the propositional calculus can be used to describe real "states of affairs".

And Wittgenstein also put forward a critique of Freudian psychotherapy which ran along similar lines. According to Freud, the function of the therapeutic process was to uncover events which had actually occurred in the patient's childhood and which, through being repressed, had caused a neurosis. Once the events had been made conscious, the neurosis would disappear—rather in the manner of a physical symptom that disappears when the underlying medical condition has been treated. Now Wittgenstein did not doubt that Freudian psychoanalysis might work. But he rejected Freud's concept of causality. He argued that the very concept of mental illness was no more than a metaphor—a "good comparison", to use Schoenberg's phrase, between physical symptoms and behavioral abnormality. He argued that the childhood events supposedly discovered during the therapeutic process had probably never happened and were certainly not the cause of the patient's problems. But the psychoanalyst's reconstruction of them was valid, all the same, to the extent that it was accepted by the patient and so helped him to come to terms with his predicament.

Like Freudian psychoanalysis, Schenkerian analysis is based on a metaphor. In Schenker's case the comparison is between the note-to-note structure of Fuxian counterpoint and the freely elaborated surface of real music.[37] Technically speaking, one might say that Schenkerian theory consists of a number of transformations which may be invoked in order to account for the discrepancies between a particular piece of music and the rigid note-to-note specifications of Fuxian counterpoint. And the effect of this is, as in the case of Freud's theory, to render intelligible something that is problematic (large-scale musical structure in Schenker's case, abnormal behaviour in Freud's) through formulating it in terms of something familiar

(Fuxian counterpoint in the one case, illness in the other). To say this is not to say that the same laws govern large-scale musical structure and Fuxian counterpoint (or abnormal behaviour and illness); it is just to make a comparison. We are dealing with what Schoenberg called a system of presentation, something that does not—or at least should not—pretend to clarify the ultimate nature of the things presented.

It is easy enough to see what this means in principle. But what does it mean in practice? Consider the status of consecutive fifths in Schenkerian analysis. These are forbidden under all circumstances in Fuxian counterpoint, which deals only with the musical surface. Following Brahms' lead,[38] Schenker regarded foreground consecutives as legitimate when they result from the interaction of different structural levels; in such cases, Schenker argued, the consecutives are apparent rather than real. But he accepted the traditional prohibition of real consecutives. Now Schenker did not see his theory as a metaphor; he believed that there are natural laws which operate equally at the level of large-scale and that of note-to-note structure.[39] So one might expect to find an equally strict prohibition of consecutive fifths when these are generated at a single structural level in the middleground. In the event Schenker is more pragmatic, saying that forbidden intervallic successions may be found in the middleground, but that "it is then the task of the foreground to eliminate them".[40] In practice, however, both Schenker and present-day Schenkerians tend to avoid middleground consecutives.

To take a specific example, how would you analyze the second half of Schubert's song "Das Wandern"? (Figure 1a shows a précis of the score.) The fall of the fundamental line from $\hat{3}$ to $\hat{2}$ is supported by a sequential bass line F♯-G-E♮-F♮, giving an intervallic progression 6-3-6-3 between the outer lines [b]. But, as the slurs and the Roman letters in [b] indicate, the F♯ and E♮ are merely approach tones to the G and F; they have a subordinate role. Therefore at a more remote level the structure is as shown in [c]; the melody arpeggiates the harmonies VI and V, which in turn support the descent of the fundamental line. But now we have glaring consecutive fifths between the outer parts. The analysis looks uncouth. Would you not avoid graphing the piece this way, instead either including more foreground detail so that the fifths are disguised—as in [b]—or interpreting the structural motion differently, for instance as in [d] or [e]? But in what sense do these alternative analyses make better musical sense of Schubert's song than the one shown in [c]? (Why is the V harmony in [d] given precedence over the VI, when each receives precisely the same support at foreground level? Do you really hear a structural dominant in mm. 17–20, as [e] implies, with their rocking alternation of F and B♭?) I would maintain that [c] is not in itself a less accurate formulation of the tonal structure of "Das Wandern" than [d] or [e], but that, because of the consecutive fifths, it is less satisfactory as an expression of that structure in terms of the metaphor

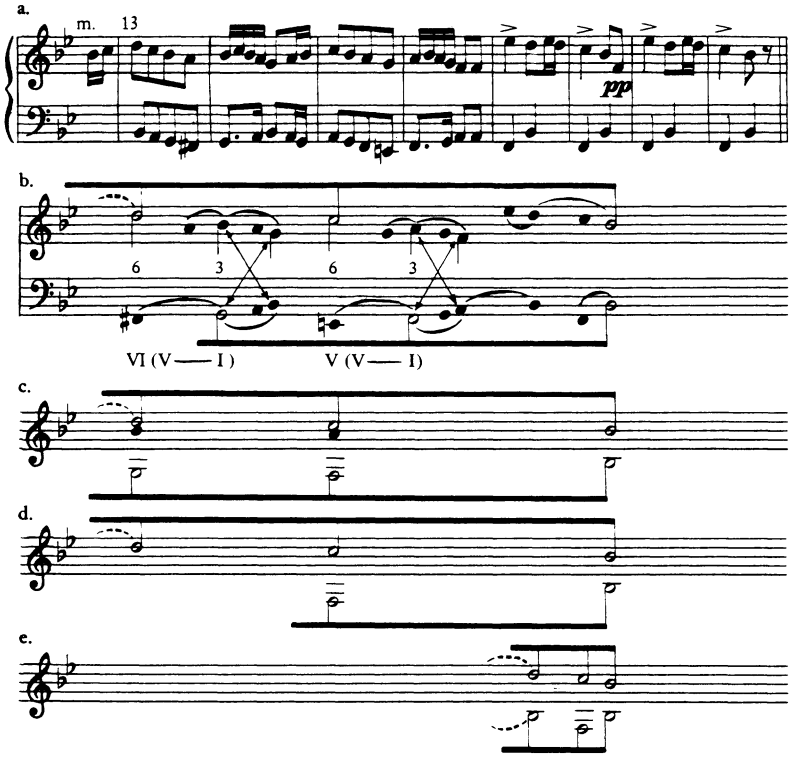

Figure 1

of Fuxian counterpoint. It makes the music look ungrammatical and, therefore, incoherent. But this is not because the middleground consecutives contravene any natural law of musical organization. It is because they run counter to the representational means adopted in Schenkerian analysis. They spoil the comparison between Schubert's song and Fuxian counterpoint.

If a Freudian explanation acquires its validity through being accepted by the patient, a Schenkerian explanation is validated when its reader accepts it as a satisfying account of the music in question. (To put it this way perhaps makes it sound like just a matter of taste. But a Schenkerian analysis does not simply present an interpretation; it provides reasons for the interpretation, implicitly if not explicitly.) Now the idea that an analysis is validated through the reader's acceptance of it may seem weak and subjective by contrast with the criterion of verifiability, as discussed by Brown and Dempster. As a matter of fact there is a good deal of scientific work that does not adhere to the verifiability criterion; the social sciences, for instance, generally adopt the less rigorous criterion of replicability,[41] and in a sense the reader's validation of Schenkerian analysis could be considered as falling under this heading. But what I would prefer to emphasize is the larger trend in musical studies away from explanatory criteria borrowed from the sciences. Kenneth Gourlay expresses this in a statement that might have been designed specifically to counter what Babbitt said about scientific language and method: "the assumption that there is only *one* scientific method", he writes, "results in analogous attempts to apply it in humanistic fields by eliminating the personal and subjective, as in anthropology or sociology, without fully considering whether it is applicable".[42]

What Gourlay is essentially saying is that the ethnomusicologist needs to recognize the centrality of his own role in cultural interpretation. Dahlhaus says much the same thing from the historian's standpoint: "whatever precision it is within our powers to attain will never be reached by leaving ourselves, the observers, out of the picture, but solely by making the observer's position an integral part of the agreement reached on the cognitive process chosen and the results obtained".[43] And this is also the basic premise of Richard Taruskin's critique of the performance practice movement.[44] In each of these cases, the study of music is being viewed as a creative interaction between the musicologist and the music in question, in which the musicologist seeks to define or redefine the music and, in so doing, constructs a cultural identify for himself.[45]

Wittgenstein stressed the necessity of "seeing" how the propositional calculus of the *Tractatus* could be applied to real-life situations, and any metaphor has to be "seen" in the same sense as a joke: that is to say, not conceptualized in analytical terms, but grasped through what could be called a kind of inner performance. Schenkerian analysis lends itself to this kind of inner performance because it builds on the familiar foundations of

species counterpoint. This makes it relatively easy for a trained musician to read a Schenker graph, not in the way one reads an abstract argument, but in the way one reads a score—in terms of the fusion of aural perception and imagination that we refer to as "hearing".[46] A Schenkerian analysis, in other words, yields up its meaning when it is experienced, or rather when the music is experienced in terms of it. In saying this, I want to suggest that the significance of an analysis lies not so much in the *product*—that is to say, the published graph or table—as in the actual *process* of writing or reading it. (David Lewin makes the same point more figuratively when he refers to the products of analysis as "ski tracks tracing the poetic deeds that were the perceptions themselves".[47]) 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.

One of these is the distinction that Narmour and Keiler emphasize, and which I mentioned earlier, between background-to-foreground and foreground-to-background derivation. Keiler—who is not hostile to Schenkerian analysis, as Narmour is—uses tree structures as a way of formalizing the transformations that are shown more impressionistically in a conventional Schenkerian graph. Now this can obviously be useful as a way of clarifying just what is involved when we talk, say, about interruption technique; if we are going to use theoretical models then we want to have a clear conception of what the models actually are (though it might possibly be argued that some of the formal distinctions that tree-structure representation forces one to make do not correspond to musically significant distinctions). But what is questionable is whether there is any virtue beyond this in distinguishing, as Keiler insists that we should, between "the structural analysis of a piece"—that is to say, the formalized interpretation shown in the tree structure—and "the informal reductive strategy of the analyst".[48] In formal terms, it may be true that Schenker's system is generative and not reductive.[49] But the analyst does not begin with the fundamental structure (how would he know which form of it to choose?) and then elaborate it step by step. He is more likely to begin by reducing the score to a series of middleground formations, only considering the background at a later stage. He may begin by taking $\hat{3}$ as the primary tone in order to see how such an interpretation will work out, but to do so is only to set up a hypothesis which is liable to be set aside at any stage. In this way, doing a Schenkerian analysis involves a constant alternation between background-to-foreground and foreground-to-background derivation. Can we not maintain that the substance of the analysis lies precisely in this process of testing alternative interpretations and seeing how they illuminate or contradict the details of the surface—a process which is recapitulated when a reader works through an analytical graph, rather than simply accepting it as some

kind of statement of fact? If so, Keiler's claim that "Schenker's graphic notation . . . constitutes a theoretical language that still requires considerable decipherment" (p. 227) is open to the retort that we realize its meaning every time we do Schenkerian analysis. And, after all, it was not Schenkerian analysis that prompted Wilson Coker to ask "but . . . will we be able to interpret what our theories mean?":[50] it was the use of formalized representations of musical structure—representations that do not make use of conventional notational symbols and so are harder to "hear" in musical terms.

A more important problem, perhaps, for contemporary music theory is the issue of universalism versus particularism, which Brown and Dempster discuss at length in their article. Their basic contention is that there is an "essential contradiction between forming general laws of music"—that is to say, universalist or scientific explanation—"and explaining the uniqueness of individual compositions".[51] Now there are a number of comments that might be made about this. For one thing, not all scientific enquiry is concerned with laws that are universal in their application, particularly in the social sciences; in this respect Brown and Dempster's characterization of scientific method seems a little old-fashioned. But a more important objection relates to Brown and Dempster's characterization of particularism. They cite the following statement from *Meta-Variations* as an example of particularism (p. 82):

> The criterion for "completion" of a "unit of syntactic structure" is and must be external with respect to any individual utterance in language; in music, such units of "completion" may again be contextually determined on the basis of single instances from "internally", or "implicitly" defined criteria.

This is a particularist point of view, they say, because it implies that "an individual art work both wholly determines its analysis or interpretation and does so because it also determines its own best method of analysis or interpretation" (p. 82). But Boretz is not saying that the general principles governing the analysis—for example, that it should be concerned with identifying points of completion at different structural levels, and rationalizing the relationships betweeen them—have to be created anew in each instance; he is merely saying that the particular way in which the principles apply will vary from one case to another. (The basic point of *Meta-Variations* was, after all, to outline a theory of music that was broad enough to allow for such varied applications.) We can make an analogy with psycholinguistics here. The phoneme is defined in functional terms as a unit of linguistic structure, but it also has an acoustic aspect. In acoustic terms any given phoneme is defined by a certain combination of what Roman Jakobson called 'distinctive features' (for instance grave/acute or nasalized/non-nasalized). Now the general principles of phonemic structure are applicable to all languages. But the particular combinations of distinctive features

that are significant vary from language to language; the principles have to be applied differently in different cases, and as a result the psycholinguist needs to determine the relevant features contextually, that is to say in terms of each individual language. This does not stop psycholinguistic theory from being scientific.

We can focus the issue of universalism versus particularism by considering how it applies to Schenkerian analysis. Making a Schenkerian reduction involves generalization: the deeper the structural level, the more of the composition's individuality is discarded, until the background is reached, at which point the composition has been generalized out of existence. In this sense Schenkerian theory is not at all particularist, in Brown and Dempster's terms. Furthermore, Schenker based his theory on laws which he considered universal in their application, though most people today would regard them as of limited chronological and geographical scope; it is presumably because of this universalism that Brown and Dempster cite Joseph Kerman's criticisms of Schenker as an example of the kind of attacks that can be directed against scientific music theory—so implying that Schenkerian theory is not only universalist but also scientific. On the other hand, Schenker himself repeatedly said that his theories should not be thought of as scientific, instead emphasizing what he called the analyst's "obligation toward particularity".[52] And many contemporary theorists would agree with William Benjamin's view that "the great strength of Schenkerian theory lies in its ability to characterize an individual tonal work in terms which highlight its uniqueness and, especially, its uniqueness at higher levels".[53] So there seem to be reasons for regarding Schenkerian theory as both universalist and particularist. We need to go into this in a little more detail.

Kerman's main criticism of Schenkerian analysis is that it "repeatedly slights salient features in the music".[54] For instance, he complains that Schenker's analysis of "Aus meinen Thränen spriessen", the second song from *Dichterliebe,* completely ignores one of the song's most telling features: the cadences that come at the end of each couplet. (Even more telling, perhaps, is the fact that the vocal line never resolves to the tonic; only the piano does.) Now at one level what Kerman says is of course perfectly true, just as it is true that Schenkerian theory more or less ignores rhythmic structure. But at another level neither statement is true at all. As I have argued elsewhere,[55] a good Schenkerian analysis presents the results of a careful consideration of rhythmic factors, though it does so implicitly; and Schenkerian theory also provides a framework within which a more explicit rhythmic theory can be developed (which is why so many of the most important contributions to rhythmic theory in recent years have come from committed Schenkerians like Carl Schachter, Maury Yeston and William Rothstein). In the same way, Schenker's graph of "Aus meinen Thränen spriessen" may not show any specific consideration of the repeated cadences, but the fact that they disappear in the reduction should not be

taken to mean that they have no significance; it means that, despite their prominence at surface level, they do not have a structural role in tonal-functional terms. And that, surely, is one of the reasons for their telling effect.[56] Similarly, while the failure of the vocal line to resolve may not be explicitly mentioned, it is thrown into sharp relief against the norms of voice-leading represented by Schenker's graph, and so emerges from the analysis as a striking discrepancy - as something which runs counter to normal expectations, which is of course exactly what it is. A great deal of the value of Schenkerian analysis, it seems to me, lies precisely in the discrepancies that arise between the analytical representation and the familiar surface of the music in question.

A convenient illustration of this is Schenker's middleground sketch of the opening theme from Beethoven's Sonata, Op. 90 (see Figures 2 and 3).[57] A surface reading of this theme would revolve around the half cadence at m. 16, which is marked by a pause sign, and followed by a I6 chord moving through II7 and V toward the close. But both the dominant harmony and the caesura disappear without trace in Schenker's sketch. Instead, he reads a tonic prolongation extending right up to the II7 chord. In the same way, he suppresses the registral and dynamic contrasts that are particularly characteristic of this theme; what are much the highest notes of the passage (the Es at mm. 16–17 and 20–1) appear in Schenker's sketch as an inner voice. One can easily imagine what Kerman might have to say about this! But we do not need an analytical method to point out the registral contrasts; they are among the most immediately obvious features of the music. 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 (Beethoven marks the continuation "in tempo"). Again the sketch does just this. Schenker's analysis, in other words, reveals the divergence between surface design and underlying structure; it shows how the music is animated by the tension between foreground and background, whole and parts.[58]

Each level of a Schenkerian analysis represents a stylistic norm, or a more or less systematically derived model, against which the elaborations of the next level stand out in all their particularity. The function of the analysis, then, is not to reduplicate the composition in question; it is to focus attention on its individual qualities. And this means that it is wrong to judge an analysis according to how directly it *mirrors* the surface of the music, with its tunes and silences and abrupt changes of texture. What matters is the extent to which it *illuminates* the surface. Schoenberg's wisecrack (if he ever actually made it[59]) about his favourite bits of the "Eroica"

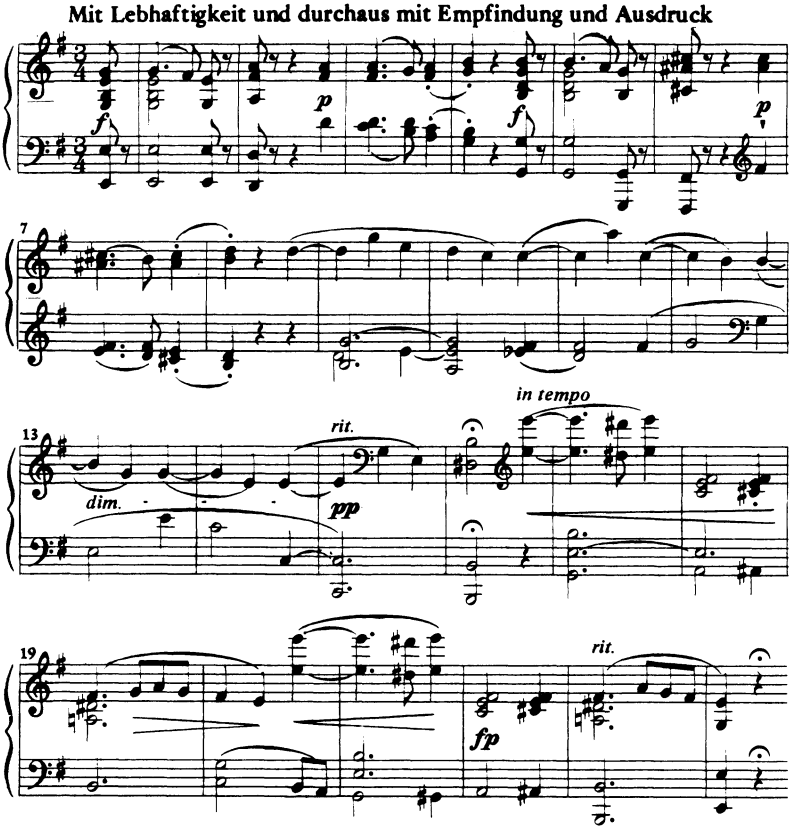

Figure 2

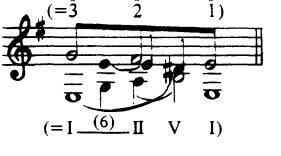

Figure 3

Symphony being in the tiny notes of Schenker's graph is all very well, but it misses one of the main points of the analysis, which is to show how highly salient features of the music arise as purely surface elaborations (something, incidentally, that seems to parallel Nottebohm's remark, based on his studies of the sketches, that Beethoven's creative faculty "often rose to its greatest heights only at the last moment"[60]). Schenker's 'Eroica' analysis does not reduplicate the salient features of the listener's experience; it interprets them in terms of the metaphor of large-scale contrapuntal structure. In fact it precisely fulfils the criteria that Schoenberg himself set out for a "good comparison", in which "we bring closer what is too distant, thereby enlarging details, and remove to some distance what is too close, thereby gaining perspective".

Stephen Davies remarks that, in general, analyses "are best to be seen as illustrations of the theory [that informs them], rather than as evidence for it".[61] This seems to me to go to the heart of the supposed dichotomy between universalism and particularism. Davies is suggesting that the main purpose of general principles—theories—is to articulate the analyst's insights into individual works. This is rather like historical writing, in which general principles are on the whole invoked as a starting point for the consideration of the individual instance, and not the other way round.[62] That is to say, the historian's general principles are to be understood not as universal laws, but as systems of presentation. And when, exceptionally, such principles are elevated into laws, so that proving their validity becomes the historian's primary concern, the result is the now discredited kind of historiography represented by Spengler's or Toynbee's writings. Schoenberg of course lumped Spengler and Schenker together in his caustic essay "Those who complain about the decline",[63] and it is certainly possible to view Schenker's analyses as being designed to prove the validity of his theory of musical structure (a theory which, in Schenker's view, rendered all other theories obsolete). In their article, Brown and Dempster talk a good deal about the ways in which Schenkerian theory can or cannot be proved to be correct. My complaint is not that this is illegitimate, nor that it is unfair to Schenker, but simply that it is not the most profitable way for us to approach what Schenker had to say.

To regard a Schenkerian analysis—or indeed any other type of analysis—as a demonstration *that* a given piece is unified is less illuminating than regarding it as a demonstration of *how* the piece is unified, or for that matter of how it is not unified but rather characterized by irreducible structural contrasts.[64] And when we use the word "demonstration" in this sense we mean something quite different from a mathematical demonstration, which demands to be accepted (or rejected) on the grounds of its strictly logical coherence. One cannot reasonably reject a mathematical proof just because one does not like it. But the same does not apply to a Schenkerian analysis, or to any other analysis that can be "heard" in musical terms. Of course we

are influenced by the arguments an analyst puts forward, by the corroboration provided through surface detail, register and so forth. But this is not enough in itself. Davies writes that

> The analyst who *sees* in the score relationships which cannot be heard by anyone will not convince us that he has exposed the source of a work's unity. But the analyst whose analysis allows us to hear relationships of which previously we were unaware may well convince us.[65]

However this does not go far enough, either. We do not accept an analytical interpretation just because we can "hear" it. (After all, it is easy to "hear" the most preposterous structural relationships if one wants to.) We accept an analytical interpretation because we find it persuasive; that is, because it satisfies the particular demands that led us to the analysis in the first place—demands such as, in Benjamin's words, "the need to memorize a work, the desire to relate it to other works, or the simple impulse to understand it better".[66]

III

"Choosing our words with great care", writes Kerman, "we might say theory deals with those aspects of music that might be thought analogous to vocabulary, grammar, syntax, and rhetoric in the field of language".[67] Theorists like Keiler or Lerdahl and Jackendoff think of music theory more or less on the model of linguistics. But the rationale for analytical practice which I have outlined would imply an analogy not so much with linguistics but with literary criticism, the thrust of which (as Judith and A.L. Becker put it) is "to understand, not to explain".[68] Other ethnomusicologists, such as Kenneth Gourlay,[69] have also endorsed literary criticism as a research model, while Anthony Newcomb has recently applied techniques explicitly derived from literary criticism to the study of Schumann's music.[70] And in the field of musical historiography, Leo Treitler expresses a similar conception of the musicologist's work when he writes that "verifiability as the measure of lawfulness yields ground to intelligibility, coherence, potential explanatory power".[71] Writers like Newcomb and Treitler—and one might cite Kerman and Gary Tomlinson as well—aim primarily at the reconstruction of musical meaning through a full consideration of the context in which the music arose, and they tend to deplore the way in which theorists ignore the historical context of the music they analyze. Treitler, for instance, complains that "prevailing modes of structural analysis are antihistorical, in two respects: they decontextualize their objects in their rationalistic treatment of them; and they are taught and practiced without notice taken of their own historicality or, in general, of the role that particular models play in the organization of understanding".[72]

Nobody can claim that this charge is wholly unwarranted; after all, music is never heard outside some interpretative context, whether that in which it originated or another, and to this extent its meaning is obviously determined contextually. But the issue is not as simple as it might seem. Dahlhaus writes that "it is quite defensible in methodological terms for us to isolate an object so long as we do not question the reality of the connections from which it has been extracted".[73] Of course the composition suffers a loss of meaning in this process. But the loss of meaning may not be so great as to override the methodological gains, particularly when one is dealing with a culture as institutionalized and reified as that of Western art music.[74] Indeed I would argue that such a methodology is indispensable not only from the theorist's point of view—after all, it defines what he does—but also from the historical musicologist's. For, as Dahlhaus and Treitler have made abundantly clear, historical facts do not just exist; they are created through historical interpretation. And this applies with particular force to the musical work, which is not something given but rather (as Dahlhaus puts it) "dissolves into a source, an authentic text, a composer's intention and a historian's notion as to the musical significance of the acoustical substrate sketched out by the text or realized according to the guidelines laid down within it".[75] In other words, the musicologist constitutes the musical work through his own interpretation of it, through conceiving of it as an aesthetic object. And to do this involves taking up some kind of theoretical position vis-à-vis the work, whether the theory involved is explicit or merely implied, because it is only by virtue of some kind of theory that a piece of music can be conceived as a discrete entity. In this way any study of the music of the past—to the extent that it maintains the concept of the work at all, rather than breaking up into reception history—is by definition theoretical as well as historical. To say this is not to claim primacy for theory over history, as Schenker did when he outlined the way in which the history of music would have to be rewritten in the light of his theories.[76] (One shudders to think what such a history would have looked like; maybe Schoenberg's remarks about Spengler were not so far off the mark.) It is merely to endorse Treitler's own statement that "as investigative procedures, neither analytical nor historical methods can be absolutely prior to the other. They inform one another in a continuous circle".[77]

The central proposition I have put forward in this article is that music theory acquires validity not, like scientific knowledge, from being verifiable, but from serving some useful purpose—in enabling the analyst to arrive at an interpretation, communicate an insight, or resolve a problem. And I have cited music history in order to exemplify the kind of way that theory can be useful and even indispensable within the broader context of musical studies. But of course there are many other examples that could have been cited too. Theoretical thinking about music is important just to the extent that it leads to better musicology, ethnomusicology, composition, or performance. Theory, in short, can best be justified by practice.

NOTES

1. Carl Dahlhaus, *Analysis and Value Judgment* (New York: Pendragon Press, 1983 [*Analyse und Werturteil,* Mainz, 1970, trans. S. Levarie]), p. 54.
2. See for instance Bell Yung, "Choreographic and kinesthetic elements in performance on the Chinese seven-string zither", *Ethnomusicology* 28 (1984): 505–17, and John Baily, "Musical structure and human movement", in Peter Howell, Ian Cross and Robert West (eds.), *Musical Structure and Cognition* (London: Academic Press, 1985), pp. 237–58.
3. David Lewin, "Music theory, phenomenology, and modes of perception", *Music Perception* 3 (1986), especially pp. 374–91.
4. Alan Smith, "Feasibility of tracking musical form as a cognitive listening objective", *Journal of Research in Music Education* 21 (1973): 200–13.
5. Sylvan Kalib, "Thirteen essays from the three yearbooks *Das Meisterwerk in der Musik* by Heinrich Schenker: an annotated translation" (Ph.D. diss., Northwestern University, 1973), ii, p. 293.
6. Ruth Solie, "The living work: organicism and musical analysis", *19th Century Music* 4 (1980): 151.
7. Fred Lerdahl and Ray Jackendoff, *A Generative Theory of Tonal Music* (Cambridge, Mass.: MIT Press, 1983), p. xii.
8. Irène Deliège, "Grouping conditions in listening to music: an approach to Lerdahl and Jackendoff's grouping preference rules", *Music Perception* 4 (1987): 325–59.
9. Burton Rosner, *"A Generative Theory of Tonal Music* by Fred Lerdahl and Ray Jackendoff" (review), *Music Perception* 2 (1984): 289–90. See also Eric Clarke, "Theory, analysis and the psychology of music: a critical evaluation of Lerdahl, F. and Jackendoff, R., *A Generative Theory of Tonal Music", Psychology of Music* 14 (1988): 15–16.
10. Burton Rosner and Leonard B. Meyer, "The perceptual roles of melodic process, contour, and form", *Music Perception* 4 (1986): 37.
11. Nicholas Cook, "The perception of large-scale tonal closure", *Music Perception* 5 (1987): 197–205. In these tests, listeners heard pieces of music in two versions, one of which was tonally closed while the other was not; they were required to say which version was more coherent, created a stronger sense of completion, etc.
12. See Eugene Narmour, *Beyond Schenkerism: the Need for Alternatives in Music Analysis* (Chicago: Chicago University Press, 1977) and Alan Keiler, "On some properties of Schenker's pitch derivations", *Music Perception* 1 (1983): 200–28.
13. See in particular Narmour's "Some major theoretical problems concerning the concept of hierarchy in the analysis of tonal music", *Music Perception* 1 (1983): 129–199. Lerdahl and Jackendoff's theory is also reductive in this sense, because their prolongational reduction—whose background-to-foreground formation corresponds to Schenker—is complemented by time-span reduction, which works the other way round.
14. For a full bibliographical citation see note 3 of Brown and Dempster's article.
15. See Boretz' "Nelson Goodman's *Languages of Art* from a musical point of view", in Benjamin Boretz and Edward T. Cone (eds.), *Perspectives on Contemporary Music Theory* (New York: Norton, 1972), p. 35. (Orig. publ. in *Journal of Philosophy* 67 [1970]: 540–52.)

16. Milton Babbitt, "Past and present concepts of the nature and limits of music", in Boretz and Cone, op. cit., p. 9.
17. I do not mean to say that Boretz doesn't care about perceptual viability: "I believe there's a real world out there, because not all of my fantasies work" ("Two replies", *Perspectives of New Music* 15/2 [1977]: 242). But the decision as to whether an analytical interpretation "works" is a purely intuitive one; the theory itself is not concerned with perception at all.
18. Milton Babbitt, "Twelve-tone invariants as compositional determinants", in Paul Henry Lang (ed.), *Problems of Modern Music* (New York: Norton, 1962), p. 120. (Orig. publ. in *Musical Quarterly* 46 [1960]: 246–59.)
19. A summary of such experiments will be found, with references, in Jana K. Millar, "The Aural Perception of pitch-class set relations: a computer-assisted investigation" (Ph.D. diss., North Texas State University, 1984). Millar's list should be supplemented by Paul Pedersen, "The perception of octave equivalence in twelve-tone rows", *Psychology of Music* 3/2 (1975): 3–8, and Carol L. Krumhansl, Gregory J. Sandell and Desmond C. Sergeant, "The perception of tone hierarchies and mirror forms in twelve-tone serial music", *Music Perception* 5 (1987): 31–78.
20. See Babbitt's famous statement about scientific language and scientific method, quoted at the beginning of Part I of Brown and Dempster's article.
21. Mary Louise Serafine, *Music as Cognition: the Development of Thought in Sound* (New York: Columbia University Press, 1988), p. 60. A parallel argument to the one I have put forward, leading to a similar conclusion, would be to contrast the Euclidean representation of time in the score with the multiplicity of temporal perceptions revealed through the phenomenological analysis of music; David Lewin discusses this in "Music theory, phenomenology, and modes of perception". See also Boretz' "What lingers on (, when the song is ended)", *Perspectives of New Music* 16/1 (1977), especially pp. 107–8.
22. On the score as a model see Henry Martin, "Modes of explanation in analytical discourse", *Perspectives of New Music* 15/2 (1977): 174–191. On models and metaphors see Christopher Lewis, "Mirrors and metaphors: reflections on Schoenberg and nineteenth-century tonality", *19th Century Music* 11 (1987): 26–42.
23. This argument is presented in more detail in my book *Music, Imagination, and Culture* (forthcoming from Oxford University Press).
24. Benjamin Boretz, "Nelson Goodman's *Languages of Art*", p. 34.
25. Dika Newlin, *Schoenberg Remembered: Diaries and Recollections (1938–76)* (New York: Pendragon Press, 1980), p. 164.
26. Translated in Kalib, op. cit., ii, pp. 52–83.
27. *Free Composition* (New York: Longman, 1979 [*Der Freie Satz*, Vienna, 1935, trans. E. Oster]), p. xxiii.
28. John Rahn, "Logic, set theory, music theory", *College Music Symposium* 19/1 (1979): 114.
29. "The musical object", *Current Musicology* 5 (1967): 56–87.
30. Carl Dahlhaus, *Schoenberg and the New Music: Essays by Carl Dahlhaus*, trans. D. Puffett and A. Clayton (Cambridge: Cambridge University Press, 1987), p. 221.
31. Kalib, op. cit., ii, p. 170.
32. "I was given a vision of the urlinie, I did not invent it!" (Kalib, op. cit., ii, p. 218). I have discussed this issue in more detail in "Schenker's theory of music as ethics", *Journal of Musicology* (forthcoming).

33. See Allan Janik and Stephen Toulmin, *Wittgenstein's Vienna* (New York: Simon and Schuster, 1973), especially pp. 183–4. *Darstellung* is of course a perfectly standard German word meaning "model" or "representation" and does not necessarily carry the particular interpretation I am discussing here; for instance, Schenker called his study of Beethoven's Fifth Symphony a *Darstellung*.
34. Arnold Schoenberg, *Theory of Harmony* (London: Faber, 1978 [*Harmonielehre*, Vienna, 1911, trans. R. E. Carter]), p. 10.
35. Schoenberg's account of "good comparison" seems to echo Goethe: "Thinking by means of analogy . . . has the advantage that it concludes nothing and aims at nothing final. Induction, on the other hand, is easily tainted; it holds a predetermined goal before its eyes and sweeps true and false on before it as it works toward that goal" (from *Maximen und Reflexionen;* translated by William Pastille in "Ursatz: the musical philosophy of Heinrich Schenker" [Ph. D. diss., Cornell, 1985], pp. 87–8).
36. It is extraordinary how much twentieth century analytical thinking has its origins in Vienna. Boretz, who was particularly influenced by Carnap, was working in the tradition of Viennese positivism. Schenker came from what is now Poland but worked in Vienna, and his first-generation disciples were Viennese. So were Schoenberg and the main analysts who worked under his influence (Keller was born in Vienna; Réti was born in Serbia but studied and worked in Vienna). Perhaps this reflects the enduring influence of Eduard Hanslick, who was the first professor of music at the University of Vienna, and whose writings in effect demonstrated the necessity for structural analysis, without clarifying in what specific ways it might be done; all the analysts I have mentioned might be seen as trying, in different ways, to carry out Hanslick's program. As for style analysis, the program for this was spelt out by Hanslick's successor at the University, Guido Adler.
37. Or more precisely, the freely elaborated surface visible in the score of the piece in question. To the extent that the score itself is (as I said above) a metaphor or model, we are dealing with a double metaphor, a model of a model (cf. Martin, "Modes of explanation in analytical discourse", p. 183).
38. In his *Octaven u. Quinten u. A.*, which Schenker edited and annotated (English version "Brahms's Study, Octaven u. Quinten a. A., with Schenker's Commentary Translated by Paul Mast" in *The Music Forum* 5 [1980]: 1–196).
39. "The principles of voice-leading, organically anchored, remain the same in background, middleground and foreground, even when they undergo transformations" (*Free Composition,* pp. 5–6).
40. *Free Composition,* p. 56. For a discussion of Schenker's contradictory statements about middleground consecutives, see William Benjamin, "Schenker's theory and the future of music", *Journal of Music Theory* 25 (1981): 163–4.
41. See Robert Dentan's "Response to Field and Roseman" in *Ethnomusicology* 28 (1984): 464.
42. Kenneth Gourlay, "Towards a reassessment of the ethnomusicologist's role in research", *Ethnomusicology* 22 (1978): 10.
43. Carl Dahlhaus, *Foundations of Music History* (Cambridge: Cambridge University Press, 1983 [*Grundlagen der Musikgeschichte*, Cologne, 1967, trans. by J. B. Robinson]), p. 88.
44. See for instance Taruskin's "On letting the music speak for itself: some reflections on musicology and performance", *Journal of Musicology* 1 (1982): 338–349.

45. The phrase is taken from Arthur Nestrovski, "Music theory, Saussure, *Theoria", In Theory Only* 10/6 (1988): 10.
46. Maybe, as Benjamin seems to imply in "Models of underlying tonal structure: how can they be abstract, and how should they be abstract?" *(Music Theory Spectrum* 4 [1982]: 48), the difficulty of "hearing" music in terms of such abstract analytical models as set theory is simply a consequence of their not being built on such familiar foundations. At the same time, I have difficulty in imagining quite what it might mean to "hear" the difference between, say, a K and a Kh relationship. But it is easy to be too glib about this. There is a continuum from "hearing" to abstract conception, and perhaps all one can sensibly propose is that no analytical model should be more abstract than it has to be in order to communicate the analyst's interpretation of a work.
47. "Music theory, phenomenology, and modes of perception", p. 382. This, it seems to me, is really Boretz' position too. Despite the scientific appearance of Boretz' earlier writings (the later ones, to put it mildly, look different), Brown and Dempster's characterization of him as a would-be scientific theorist obscures what is surely the primary aim of his work: to contribute to the experienced "vividness (in the senses of individuality, lucidity, and depth) of musical works" ("Two replies", p. 241). Perhaps we would all have understood *Meta-Variations* better if it had been called "Poetics of Music Theory in the Form of Six Lessons".
48. "On some properties of Schenker's pitch derivations", p. 224.
49. After all, Schenker introduced his earliest full-scale reductive graph (in the *Erläuterungsausgabe* of Op. 101) with the words: "Here are shown the lines that Beethoven's imagination followed". And his method of presentation in *Der Freie Satz* follows a generative, not a reductive, sequence.
50. Wilson Coker, "Richmond Browne (ed.), *Music Theory: Special Topics"* (review), *Music Theory Spectrum* 4 (1982): 130.
51. Brown and Dempster, p. 84. This is reminiscent of Lewin's argument, in "Behind the beyond: a response to Edward T. Cone" *(Perspectives of New Music* 7/2 [1969]: 62), that "analysis is really antithetic to theory"; analysis, that is to say, focusses on the individual case whereas theory is concerned with generalization. But, as Lewin implied and as Cone made explicit in his reply (pp. 70–2), the antithesis is a matter of emphasis rather than of underlying principles. It is not, in other words, a question of contradiction.
52. Kalib, op. cit., ii, p. 82. Elsewhere in *Das Meisterwerk* Schenker writes that "The general . . . easily seduces man into becoming complacent, into taking no further pains with the particular. Continued misperception of the particular unsouls, so to speak, the understanding of the general; it no longer ripens into truth; it solidifies into a schema" (translated in Pastille, "Ursatz", p. 159).
53. "Models of underlying tonal structure", p. 36.
54. Joseph Kerman, *Musicology* (London: Fontana, 1985; American edition, *Contemplating Music,* Cambridge [Mass.]: Harvard University Press, 1985), p. 82. Kerman's remarks about "Aus meinen Thränen spriessen" will be found in "How we got into analysis, and how to get out", *Critical Inquiry* 7 (1980): 311–31; for Schenker's analysis see Allen Forte, "Schenker's conception of musical structure", *Journal of Music Theory* 3 (1959): 1–30, reprinted in Maury Yeston (ed.), *Readings in Schenker Analysis and Other Approaches* (New Haven, 1977): 3–37.
55. Nicholas Cook, *A Guide to Musical Analysis* (London: Dent and New York: Braziller, 1987), pp. 61, 77.

56. What Kerman might have reasonably complained about is the fact that Forte did not draw attention to this in his commentary on Schenker's analysis. The trouble with Kerman's attack on Schenkerism, as he calls it, is that he tends not to distinguish between the analytical methodology on the one hand, and the manner in which it is used—which may be sensitive or insensitive, critical or uncritical—on the other.
57. Schenker's sketch, shown in Fig. 3, is taken from *Der Freie Satz,* Ex. 109a, (1).
58. See Pastille, "Ursatz", pp. 67ff., 134. Present-day Schenkerian analysis, it seems to me, tends to put too much emphasis on unity *per se,* rather than on the dynamic relationships between different levels.
59. See Milton Babbitt (ed. S Dembski and J. N. Straus), *Words about Music* (Madison: Wisconsin University Press, 1987), pp. 139–40.
60. Gustav Nottebohm, *Two Beethoven Sketchbooks: a Description with Musical Extracts* (London: Gollancz, 1979 *[Ein Skizzenbuch von Beethoven,* Leipzig, 1865, and *Ein Skizzenbuch von Beethoven aus den Jahre* 1803, Leipzig, 1880, trans. J. Katz]), p. 98.
61. Stephen Davies, "Attributing significance to unobvious musical relationships", *Journal of Music Theory* 27 (1983): 204.
62. Patrick Gardiner, *The Nature of Historical Explanation* (London: Oxford University Press, 1961), p. 90.
63. Translated in *Style and Idea: Selected Writings of Arnold Schoenberg,* ed. L Stein, with translations by L. Black (London: Faber, 1975): 203–4.
64. See David Neumeyer's "Reply to Larson", *In Theory Only* 10/4 (1987): 34.
65. "Attributing significance to unobvious musical relationships", p. 212.
66. "Models of underlying structure", p. 28.
67. *Musicology,* p. 13.
68. Judith and A.L. Becker, "Response to Feld and Roseman", *Ethnomusicology* 28 (1984): 455. See also Boretz' remarks in "What lingers on", p. 106.
69. "Ethnomusicologist's role", pp. 25–6.
70. Anthony Newcomb, "Schumann and late eighteenth-century narrative strategies", *19th Century Music* 11 (1987): 164–74.
71. Leo Treitler, "On historical criticism", *Musical Quarterly* 53 (1967): 191.
72. Leo Treitler, "'To worship that celestial sound': motives for analysis", *Journal of Musicology* 1 (1982): 159.
73. *Foundations of Music History,* p. 27.
74. See Harold Powers, "Language models and musical analysis", *Ethnomusicology* 24 (1980), p. 8.
75. *Foundations of Music History,* p. 35.
76. *Free Composition,* p. 27.
77. '"To worship that celestial sound"', p. 155.