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A Reply to Peel and Slawson's Review of "A Generative Theory of Tonal Music"

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THEORY FORUM

A REPLY TO PEEL AND SLAWSON'S REVIEW OF A GENERATIVE THEORY OF TONAL MUSIC

Fred Lerdahl and Ray Jackendoff

In their review (*JMT* 28/2) of our book, *A Generative Theory of Tonal Music*, John Peel and Wayne Slawson make a number of criticisms that require a reply. We will leave untouched some side issues and many nuances. For brevity, we will refer to our book as *GTTM* and to Peel and Slawson as P & S.

1. *Organization of the theory.* P & S overlook two basic features of our theory in their initial overview, leading to misrepresentations later on. The first of these has to do with the general organization of the theory. As shown in Figure 1, the theory takes the "musical surface" as input and gives its structural description(s) as output. This construction is accomplished by a set of rules, or "musical grammar," which is intended to model the hierarchical dimensions of musical intuition. Consequently, the theory is psychological in orientation: its principles and constructs are claimed to be a description of the unconscious knowledge that an experienced listener brings to bear in understanding individual pieces of music. This approach leads to important methodological constraints which P & S seem not to have appreciated: (1) each rule of the grammar must be motivated and justified by its ability to explain

numerous particular facts about musical intuition; (2) each rule is required to apply consistently in strength for all contexts; (3) the interaction of each rule with the other rules must likewise be consistent; (4) the rules and the structures that they construct must be psychologically plausible, insofar as there is independent psychological evidence of relevance.

We adhere to these constraints throughout *GTTM*. As will be seen below, their presence rules out P & S's alternative analyses of the passages from Mozart's Piano Sonata in A, K. 331, and Bach's chorale, "O Haupt voll Blut und Wunden." More broadly, and contrary to P & S's claim (p. 282), these constraints cause our grammar to be highly structured. The reader cannot just consult the rule index and apply rules indiscriminately, as P & S seem to believe.

To our knowledge, no other music theory has been organized along the lines of Figure 1. Other "generative" approaches have proceeded in the opposite direction, by starting with a set of abstract axioms and deriving the musical surface, as in Figure 2. These approaches also have not shared our fundamentally psychological orientation with its accompanying methodology (for related discussion, see *GTTM*, pp. 111-112, 288-289). P & S do not note these essential differences; we return to them in a moment.

The second basic feature neglected by P & S concerns the internal organization of our musical grammar. Figure 3 shows the derivational route of the four hierarchical components of the theory. In the diagram, the arrows at (a) carry rhythmic information, the arrow at (b) carries pitch information, and the arrow at (c) conveys a synthesis of rhythmic and pitch information. Observe that the mapping from musical surface to prolongational reduction, the most Schenkerian of our components, is indirect. This would not be the case if pitch information alone were sufficient to derive a prolongational tree. But rhythmic information is also needed—specifically, the time-span segmentation, which itself derives from grouping and meter (*GTTM*, pp. 118-120, 124-128, 146-149). As a result, the pitch-rhythmic synthesis of time-span reduction becomes the proper input to prolongational reduction (pp. 123, 187-188, 213, 220, 227-232).

This complex yet specific mapping from musical surface to prolongational structure is a central claim of our theory, at least as significant as our treatment of grouping and meter (praised by P & S, p. 275). Through this mapping we achieve a rule-based selection of structurally important events, rather than leaving it to the analyst's insight, as in previous approaches.

P & S appear not to understand the complementary roles of time-span and prolongational reduction in our theory, for they criticize our time-span analyses of the Bach and Mozart excerpts when it would be

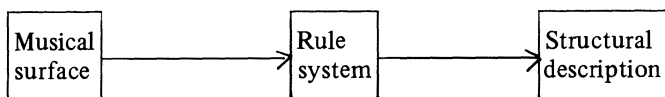


Figure 1

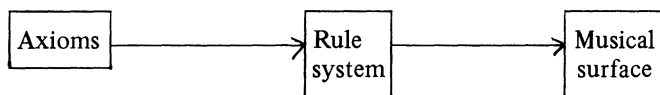


Figure 2

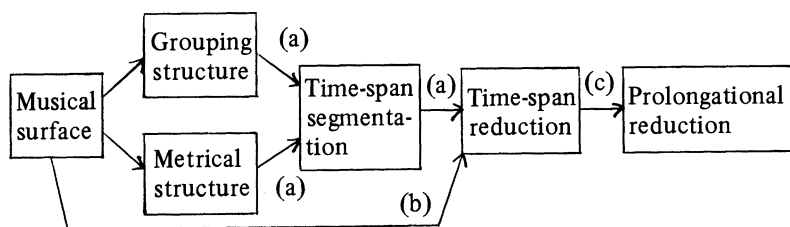


Figure 3

more relevant for their concerns to consider our prolongational analyses. It is especially hard to see how they could have gone so wrong with the Mozart, whose cited time-span reduction (*GTTM*, Example 9.11; P & S, Example 7) was included at this point in our exposition explicitly to set up the immediately following derivation of the prolongational reduction. Further, this same time-span analysis was earlier shown by us to be inadequate in itself, as part of our initial motivation for developing the prolongational component (*GTTM*, pp. 120-122; Example 5.12 is equivalent to Example 9.11). As a result of this confusion, P & S's discussion of the Bach is largely beside the point and that of the Mozart entirely so.

2. *Comparisons with Schenker.* Before considering the Bach and Mozart more closely, we must clear away a recurrent tactic in P & S's review, that of pitting our theory against Schenker's in a manner disadvantageous to us (for example, P & S, pp. 282-287). This tactic is unfortunate for a number of reasons. First, our debt to Schenker (among others) is obvious and acknowledged (*GTTM*, pp. 337-338). We pay him the further tribute of not just imitating him but of adapting some of his concepts to purposes that could not have been imagined in his time; this is a sign of his theory's fruitfulness. Second, P & S do not use Schenker's ideas well. For example, they mistake several particulars of Schenker's analysis of K. 331 (see below), and produce a highly questionable "Schenkerian" analysis of "O Haupt" (P & S, Example 2). And they do not realize the ambivalence of Schenker's attitude regarding the location of the "structural dominant" (P & S, p. 284; see *GTTM*, p. 339, and Oster's footnote in *Free Composition*, p. 37).¹

The third and most important reason against facile comparisons of the two theories is that Schenker's theory is organized along the lines of Figure 2, not of Figure 1. That is, insofar as his theory is formal rather than just a repository of analytic procedures, it proceeds from underlying axioms (the *Klang* and *Ursatz*) through various elaborational rules to the musical surface. He does not intend, as we do, a theory that models the listener's intuition by predicting analyses from musical surfaces. His primary purpose is correspondingly not psychological but aesthetic. As long as the steps of derivation are lawful, actual derivations remain unspecified by *his theory*, this function being taken over instead by the analyst's aesthetic judgment. In the Schenkerian paradigm, then, disputed analyses boil down to value judgments within a systematic framework. In our paradigm, by contrast, unsatisfactory analyses demand improvements in the rule system itself, since it is this rather than the analyst that assigns analyses. There is no Schenkerian counterpart to our musical grammar.

We therefore disagree with P & S's unsupported contention (p. 287) that Schenker's rules are "considerably stronger in their assertions"

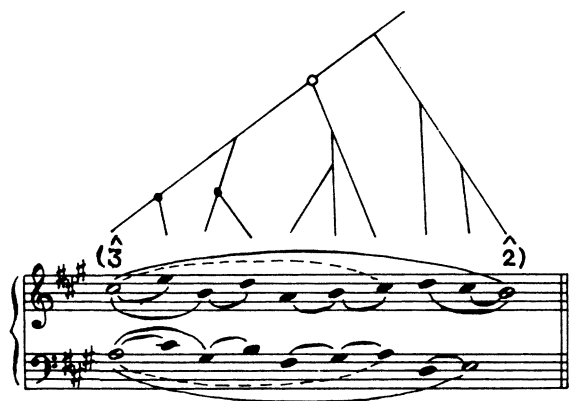
than ours. It would be more appropriate just to say that the two rule systems are hard to compare because of their differing organizations and goals.

It might be interesting, though, to consider for a moment what Schenker's theory would need if it sought to achieve something like the predictive power to which *GTTM* aspires. The following questions would have to be answered. (1) What is the status of Schenker's underlying axioms? Where do they come from and what do they represent? (2) By what criteria is the proper *Urlinie* chosen in any given instance? (3) Why does the musical surface take the specific rhythmic form that it does? How is the rhythmic structure to be characterized? (4) Which derivation of the musical surface, out of the many possible ones, is correct? How is it arrived at?

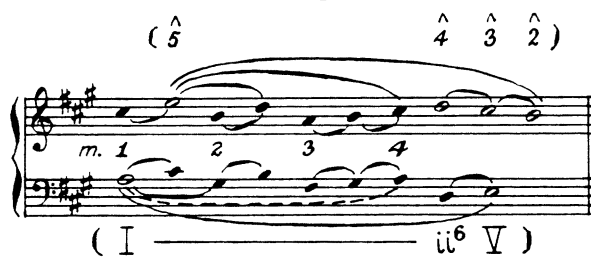
From a psychological viewpoint, the most crucial of these questions is (1), since Schenker seems to claim that humans are innately endowed with *Ursatz*-schema to which they try to fit musical surfaces. Such an attitude does not comport well with contemporary views on the nature of the mind, and is refuted by the historical and ethnomusicological evidence. Schenker aside, we challenge those who propose models along the lines of Figure 2—as P & S do in their chord-grammar fragment (pp. 288–289)—to try to resolve issues (3) and (4) in their own terms. The latter, the limitation of possible derivations, is a particularly thorny problem within such a framework.

3. *Specific issues of analysis.* For a comparison of our Mozart analysis with Schenker's, P & S should have chosen not the time-span reduction but the prolongational analysis (*GTTM*, Example 9.17b), given here as Example 1. The critical question is whether the C# or the E dominates m. 1 melodically. The first choice results in an interrupted $\hat{3}$ -line, the second in an interrupted $\hat{5}$ -line. Our grammar "prefers" the C# because of its bass support (TSRPR 2a), reinforced by its placement in the metrical and grouping structures (TSRPRs 1 and 8). Favoring the E are the weaker principle of relative height (TSRPR 3a) and, perhaps, congruence with motivic structure (*GTTM*, pp. 162, 341; note in particular the E-D-C# in mm. 1–4, answered in rhythmic diminution by D-C#-B in m. 4). This "less preferred" alternative, shown in Example 2 only in our "secondary notation," would require two distinct but related trees, due to asynchronies in structural importance between the melody and the bass. Our theory as presented in *GTTM* does not yet incorporate such an extension, though we discuss the need for it (pp. 116, 274–277).

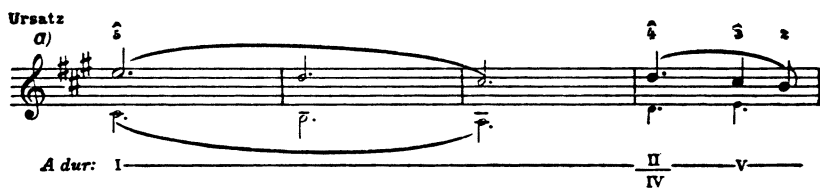
Schenker's analysis, which differs not only from Example 1 but from P & S's misconstruction of Schenker (P & S, Example 6), appears in Example 3 in Schenker's notation.² This version resembles Example 2, but differs in that the structural tonic bass note is assigned (at a



Example 1



Example 2



Example 3



Example 4

certain level) at the downbeat of m. 4 rather than at the beginning of m. 1. Example 3 preserves, as does Example 1, the parallel tenths in mm. 1–4 between the outer voices. By contrast, Example 2 (and P & S's Example 6) suggests the more dubious and nonmotivic voice-leading of perfect to diminished fifths between the outer voices in mm. 1–2.

But Schenker's solution is not without drawbacks. Does the listener really hear—at *any* level—the bass in mm. 1–3 as a structural anacrusis to the A in m. 4? Does he not instead hear the $\frac{3}{1}$ in m. 4 as a prolongational repetition of the opening, as in Example 1? Our grammar cannot even “weakly prefer” the delayed structural bass in Example 3; TSRPRs 6 and 8 and PRPRs 3 and 6 all militate against it. Further, if the theory were somehow bent to accommodate this solution, there would be unacceptable consequences when the rules applied to other pieces. That is, Schenker's treatment of the bass here is idiosyncratic, a result of his desire to keep both parallel tenths and a $\hat{5}$ -line. The $\hat{3}$ -line does not encounter these difficulties.

Behind these alternatives lies the interesting question of whether a reduction should directly reflect motivic structure. Specifically, Schenker insists on the $\hat{5}$ -line because it better projects the descending third-progressions in the melody (E-D-C#, D-C#-B), within which occur ascending third-progressions at smaller levels. While we agree with his motivic analysis, we advocate a greater separation of the hierarchical and motivic dimensions than does Schenker, so that their reinforcing or conflicting interaction can be fully characterized. That is, motivic structure influences reductional structure, as reflected in our parallelism rules (GPR 6, MPR 1, TSRPR 4, PRPR 5), but the two need not be isomorphic (*GTTM*, pp. 286–287, 341). Unfortunately, P & S's commentary is so misplaced that it does not even begin to touch on such meaningful issues.³

P & S's discussion of the Bach chorale is also problematic. They have two substantive objections to our (time-span) analysis. First, they claim that the opening is tonally ambiguous and suggest that the piece is in G major until m. 2 (P & S, pp. 279–281, Examples 2 and 3). This assertion confuses a real-time processing theory with a theory of final-state comprehension. Our theory, like Schenker's and most other music theories, idealizes away from on-line processing to the final state of the listener's understanding (*GTTM*, pp. 3–4).⁴ Thus, even if m. 1 were momentarily heard in G major, it would be correct for our theory (and Schenker's) to analyze it in D major, since the opening is so interpreted in light of the whole. But even in a processing theory the G-major interpretation is implausible from the start: it would entail hearing leaps off the leading-tone (F#!) in both the soprano and the bass, as illustrated in Example 4.

Second, P & S object to our treatment of harmony and voice-leading

in time-spans *p* and *q* in Example 5. In time-span *q*, the selection of head at the quarter-note level leads either to the sequence in Example 6a or to that in Example 6b; in time-span *p*, the choice at the half-note level is between Examples 7a and 7b (*GTTM*, Examples 7.21 and 7.22). The relevant rule is:

- TSRPR 2:** Of the possible choices for head of a time-span *T*, prefer a choice that is
- (a) relatively intrinsically consonant,
 - (b) relatively closely related to the local tonic.

However, in both time-spans there is a conflict between conditions (a) and (b) of the rule, requiring other rules for a decision. On grounds of voice-leading (TSRPR 6, PRPR 3), the theory favors Examples 6b and 7b: 6b affords a stepwise resolution of the bass (C# to D), and 7b reveals a melodic unfolding of members of the tonic triad (F#-A-F#) (*GTTM*, p. 166). In addition, this interpretation shows motivic parallelism (TSRPR 4) in both the bass and the treble (*GTTM*, pp. 166, 131). Finally, the choice of Example 6b promotes optimal left branching in the prolongational tree (TSRPR 6, PRPR 3), further showing a parallelism in harmonic function between beats 3-4 of m. 1 and beats 1-3 of m. 2 (PRPR 5). This interpretation is summarized in the prolongational analysis in Example 8 (with explanatory markings added), which P & S do not cite (*GTTM*, Example 8.31).

P & S (p. 276) suppress condition (a) in their quotation of TSRPR 2, which allows them to maintain that we do not follow strict voice-leading but permit the retention of dissonance at larger reductional levels. They also disregard our discussions of the interaction of conditions (a) and (b) within this rule and of the invariable dominance of this rule over the principle of metrical position (TSRPR 1) (*GTTM*, pp. 160-162). In their alternative analysis (P & S, p. 280), they illegally change the time-span segmentation—a matter of well-formedness not preference rules (the relevant rule is Segmentation Rule 2b)—thereby revealing that they have not grasped one of the deeper points of our reductional theory, the structural counterpoint between the time-span segmentation (including grouping structure) and “prolongational groupings” (*GTTM*, pp. 121-123, 207, 285).

By means of these steps, P & S then erroneously claim that our grammar is so weak that it can generate their utterly different G-major analysis, in which the G chords on beats 1 and 3 dominate m. 1 (P & S, p. 280, Example 3). Crucial here is their assumption that metrical position overrides local harmonic stability (TSRPR 1 over TSRPR 2b), a move that, if systematically applied, would altogether eliminate the possibility of appoggiatura chords in tonal music. But *none* of these steps is feasible: the segmentation rules cannot be tampered with;



Example 5

a

b



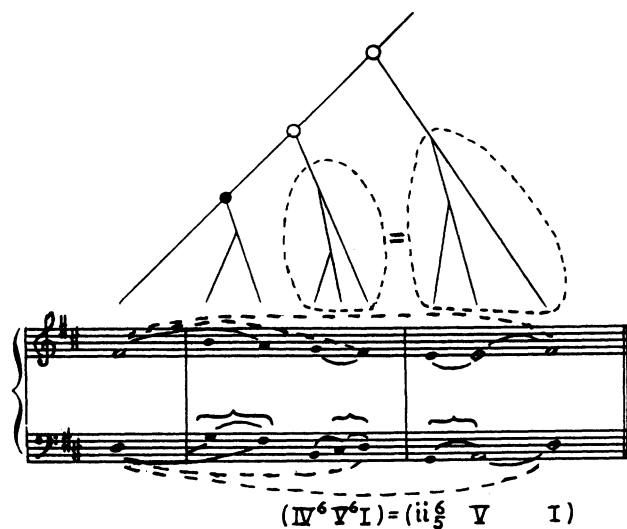
Example 6

a

b



Example 7



Example 8

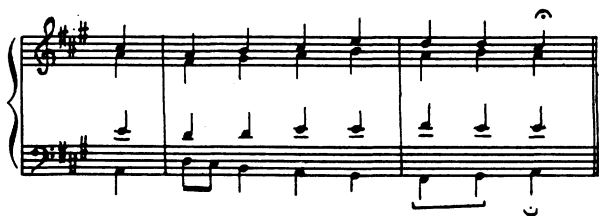
TSRPR 2 overrides TSRPR 1; TSRPRs 4 and 6 cannot be ignored; and a host of factors (TSRPRs 2b, 6, and 8, PRPRs 3 and 6) enforce the domination of the opening D chord. Our grammar is not even remotely weak enough to predict P & S's analysis.

P & S's sole plausible point is their preference for Example 6a over Example 6b, since the IV^6 (disregarding its contrapuntal voicing, which indicates its nonstructural function) nevertheless is a more consonant configuration than the V^6_5 , especially when the outer voices are considered alone. Under either interpretation, however, the melodic G on beat 3 of m. 1 remains a harmonized accented passing-tone between A and F#. Thus, contrary to P & S's assertion (p. 278), the counterpoint in Example 6b is not aberrant. And what would they do with the same progression in Example 9? In the bracketed time-span, would they insist that the IV^6 dominate the V^6_5 ? The rules must apply consistently.

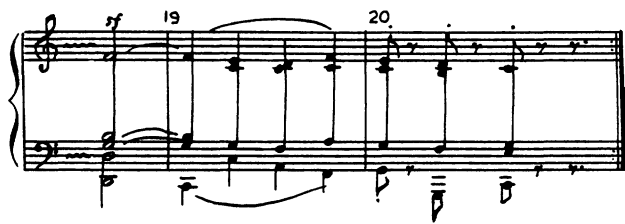
4. *The status of preference rules.* We have treated these matters in detail because they bear on P & S's major principled contention, that our preference rule (PR) system represents "an admission of failure" (P & S, p. 287). There are two aspects to their argument: (1) the PR system is insufficiently predictive; (2) it is intrinsically the wrong kind of rule system. Let us consider these points in turn.

It is true that the PR system is not yet predictive enough. However, it is unreasonable to demand that a theory of such scope be finished in one fell swoop. Throughout *GTTM*, we leave important areas open—for instance, the unrealistic idealization to homophonic textures (pp. 37, 116), the nonexplication of musical parallelisms (pp. 17, 52-53, 286-287), the wholesale assumption of traditional principles of harmony and voice leading (pp. 117, 160, 290), and the incompleteness of the prolongational component (pp. 273-277, 340). Similarly, it was beyond our intent fully to quantify the PRs (pp. 53-55). But this does not mean that the PR system is unstructured. In introducing a new rule, we discuss its range of applicability, its relative strength, and aspects of its interaction with other rules. Thus, as mentioned above, TSRPR 2 overrides TSRPRs 1 and 3, but conditions (a) and (b) of TSRPR 2 compete closely. Through such distinctions in the course of a derivation, Examples 1 and 8 become "preferred," Example 3 and P & S's Example 3 become "not preferred," and so forth. Though a full characterization of the PR system awaits further research, the system is already highly organized.

P & S's more sweeping claim is that, whereas our grammar relies on PRs within the framework of well-formedness rules (WFRs), a generative theory should in principle use only WFRs (P & S, p. 288). We should explain here that WFRs provide admissible, or "grammatical," descriptions; PRs select the most "coherent" among grammatical descriptions. WFRs are categorical: either a sequence is grammatical and

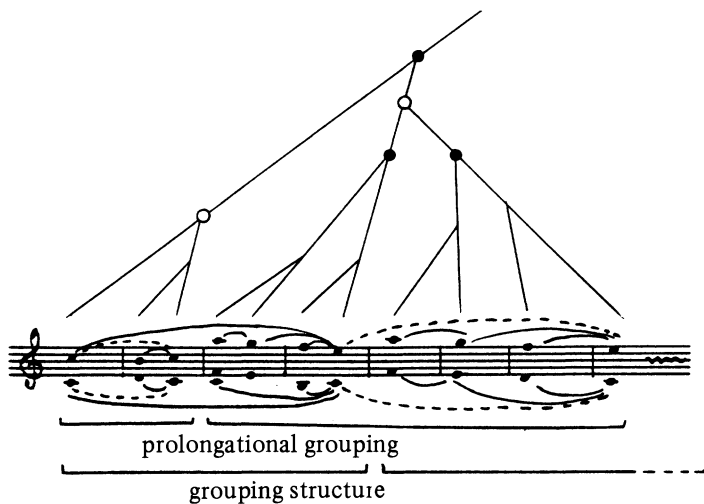


Example 9. Bach chorale: *Du Friedensfürst Herr Jesu Christ*



$ii_3^4 - IV - (I_4^4) - V^7$

Example 10. Beethoven: Sonata, op. 14 (2), II



Example 11

receives a structural description or it is ungrammatical and cannot be assigned a description. PRs are not categorical but weight possible descriptions as more or less plausible. They have not been used in traditional syntactic formalisms in linguistics, and, technically, are an innovation of our theory. However, as we point out in a lengthy discussion that P & S ignore, PR-like principles have been prominent not only in the Gestalt school but in recent research in cognitive science, visual theory, and theoretical linguistics (*GTTM*, pp. 302–330).⁵ Current work in music psychology is also consonant with such an approach.⁶ So if P & S dismiss PRs, they are in effect rejecting much fruitful and central work in the cognitive sciences. It should also be kept in mind that these questions are not purely formal but empirical: how does the brain/mind organize stimuli? The technical evidence from visual studies—conflicting or reinforcing factors triggering at threshold values, heavy parallel processing—appears to support the postulation of PR mechanisms.⁷

The categorical nature of WFRs has consequences for P & S's proposed well-formedness grammar of chord progressions. For example, their grammar rules out a II-IV-V progression (P & S, p. 289). But is this sequence *ungrammatical*—or just unusual? Is the passage in Example 10 a violation of tonal principles in the sense that, say, a II-IV-V sequence would be if the IV were replaced by the tetrachord (0,1,2,3)? In our theory, at least, a II-IV-V progression is grammatical and receives a structural description via the PRs; a II-(0,1,2,3)-V progression is tonally ill-formed and would not receive a description. P & S might fall back on the position that Example 10 involves voice-leading and not a harmonic progression. However, then they would need both well-formed voice-leading criteria and a method for not assigning chord labels to triadic constructions under specified conditions. Regarding voice leading, some criteria may indeed involve well-formedness, such as the resolving of a suspension; however, others are not mandatory but rather more Gestalt-like, such as the filling in of a gap after a leap. The latter can be explicated only by recourse to PRs. As for chord labels, Winograd found that musical passages can have many well-formed roman-numeral analyses (for instance, the beginning of "O Haupt" can be represented in G major!); so he had to develop "semantic rules"—in effect, PRs—to select the most coherent analyses.⁸ We take a similar view. The same argument holds for P & S's claim that a VI cannot replace a I at the end of a piece: does that make Chopin's Mazurka in A Minor, Op. 17 No. 4, ungrammatical? No—it is just unusual, and must be assigned structure by a generative theory of tonal music. P & S's grammar fails even for the examples they offer.

One might ask why musical syntax cannot be as fully captured by WFRs as linguistic syntax. We think there are two fundamental reasons:

unlike language, music is not made up of grammatical categories that combine in highly constrained ways (*GTTM*, pp. 112-113); and music is not limited by such semantic considerations as sense and reference (*GTTM*, pp. 5-6). As a result, music is more able than language to be construed in a multiplicity of ways, in varying degrees of coherence (*GTTM*, p. 9). The relative lack of well-formedness constraints in music is compensated for by the rich and linguistically unparalleled interaction among the various musical dimensions, including the four hierarchical organizations addressed by our theory.

This brings us to P & S's second principled objection, that the Strong Reduction Hypothesis (strict nesting of pitch hierarchies) is too strong a criterion for reductions. They believe the inclusion of "crossed" hierarchies would be more realistic (P & S, p. 290). However, they incorrectly suggest (pp. 290-291) that we want to exclude nonnested structures from music. We in fact call attention to the nonhierarchical aspects of pitch structure (*GTTM*, pp. 7, 9, 52-53, 116-117, 286-287; depending on context, we refer to "motivic-thematic," "linear-motivic," or "associational" organization). The impact of these aspects on hierarchical structure is encoded in the parallelism rules for each component. So the issue is not the existence of nonnested pitch structures but how to treat them theoretically.

We have found in general that the use of crossed hierarchies leads to lack of insight as well as loss of rigor. To deal with situations in which violations of hierarchy seem inevitable, we have included in the grammar a variety of devices (none of them mentioned by P & S), that loosen the strict constraints on nesting: grouping overlap and elision (*GTTM*, section 3.4), metrical deletion (section 4.5), and three kinds of alterations in time-span trees (section 7.2). Further cases of apparently crossed hierarchies may be analyzed as ambiguity between two or more highly preferred structures, each of which is itself strictly hierarchical. Other apparent crossings result from the structural counterpoint between time-span segmentations and prolongational groupings.

For instance, of the passages cited by P & S (p. 290), the supposedly crossed hierarchy in Mozart's K. 545 emerges as a noncongruence between time-span segmentation and prolongational grouping, as shown in Example 11; and the coupled *Ab* in Schenker's analysis of Bach's chorale "Ich bin's . . ." is a part of associational rather than hierarchical structure.⁹ In the future we hope to add a nonhierarchical component which will establish degrees of thematic or associative relatedness among musical passages, thereby fleshing out the already existing parallelism rules (*GTTM*, p. 287).¹⁰

5. *Universals.* P & S's treatment of our views on musical universals and contemporary music is particularly misleading. Our concern is not with cultural but with psychological universals—the mental principles

by which humans organize musical stimuli. This point is treated in some detail in sections 11.1-2, to which P & S make no reference. In the rule index (*GTTM*, pp. 345-352) we distinguish between those principles of our grammar that we think are universal and those that are specific to idioms.

For example, *if* an idiom has harmony, the harmonic aspects of the reductional components come into play; that is, to the extent that the signal permits, the listener tries (unconsciously) to infer harmonic hierarchies. Those rules that pertain to the intrinsic cognitive organization of harmony, such as the building of hierarchies through harmonic stability (TSRPR 2) or prolongational connection (PRPR 3), are postulated as universal. But those rules that concern the particulars of classical tonal harmony, such as the circle of fifths or V-I cadences, are marked as idiom-specific.

Thus, despite P & S's allegations (p. 292), the postulated universal rules are all abstract. For instance, TSRPR 2 (cited above) says nothing about what constitutes relative consonance or closeness; these principles are idiom-specific and must be learned through exposure (*GTTM*, p. 178). If an idiom defines harmonic consonance or closeness in a different fashion, TSRPR 2 will apply in terms of those principles instead of our familiar ones.

In general, three kinds of evidence bear on claims of universality: (1) physiological evidence; (2) cultural ubiquity; (3) unlearnability. Little is known yet about the physiology underlying musical cognition. Historical and ethnomusicological evidence is extremely useful, since it can correct cultural bias, undercutting unwarranted assumptions and perhaps suggesting undetected universals (*GTTM*, pp. 178, 279-280). Though P & S complain about our bias here, they mention nothing beyond a tendentious comparison to Schenker's prejudices (P & S, p. 291).

Our arguments on universality rely most strongly on the third kind of evidence: if a feature of the grammar is remote from the musical surface, this suggests that the feature may not be learned from scratch but is part of innate endowment, though triggered by experience (*GTTM*, pp. 4, 281-282). This sort of argument is familiar in linguistics,¹¹ but P & S have nothing to say about it. Claims of universality are, of course, empirical (*GTTM*, pp. 4-5, 279, 282). However, since they are claims about *grammar*, they cannot necessarily be refuted by superficial examination of the obvious features of other idioms.

Throughout, P & S tend improperly to inject issues of aesthetic value. For example, they go out of their way to paint our theory as biased toward symmetry, even though our prolongational component is no less asymmetrical than Schenker's. The issue in any case is not one of value but of how listeners organize musical surfaces. We offer evidence that in certain respects listeners seek symmetrical structures, everything else

being equal (*GTMM*, pp. 49-50, 99-101, 136-137). A comparable claim in visual psychology would be unexceptionable enough. Is P & S's problem with the term "preference"? If so, we must emphasize that it refers to the noncategorical nature of PRs rather than aesthetic preference (*GTMM*, pp. 42, 336). Or do they just dislike the notion that listeners intuitively organize surfaces in particular ways?

Similarly, our discussion of contemporary music is aesthetically neutral. P & S attribute to us the position that "if a piece of music cannot be demonstrated to possess the nested hierarchies in the four realms they have described . . . then the piece is at best superficial" (p. 291). In fact we take no such position, particularly in the aesthetic sense of "superficial," and we take pains to distance ourselves from aesthetic judgment. Rather, our claim is that some recent music *does not permit listeners to infer* much hierarchical structure, regardless of the technique by which it has been composed. We explore this situation in some detail from the vantage of our grammar, and note that the nonhierarchical dimensions have played a compensatory role (*GTMM*, pp. 296-300).¹² But the absence of heard hierarchy, which is so central to comprehension and memory (*GTMM*, p. 241), is, we believe, a fundamental cause of the difficulty in understanding this music—certainly as important a cause as the social factors against which P & S plead. However, no value judgment is implied. The relation between accessibility and value is at best indirect; nobody has proposed a serious theory about such matters (*GTMM*, p. 301). To maintain a direct correlation would be to argue that some Vivaldi concerto is better than *Pierrot Lunaire* but worse than the latest rock album. We do not take such a view.

NOTES

1. Heinrich Schenker, *Free Composition*, transl. Ernst Oster (New York: Longman, 1979).
2. Schenker, *Das Meisterwerk in der Musik*, vol. 1 (Munich: Drei Masken Verlag, 1925); the same example appears in abbreviated form in Schenker, *Free Composition*.
3. Incidentally, P & S (p. 284) attribute to us the nonsensical view that the event on the downbeat of m. 3 of the Mozart is a "II⁶" preparing the half cadence in m. 4. Evidently the real II⁶ in m. 4 will not do, as P & S are eager to demonstrate our supposed predilection for symmetry (in this case, I-V in mm. 1-2 answered by "II⁶"-V in mm. 3-4). Let us clarify. Although the event on the downbeat of m. 3 is subordinate to the cadence in the time-span reduction, it is deeply nested in the prolongational reduction, as we show at some length (*GTMM*, pp. 227-232). And the function of dominant preparation is represented *only* in prolongational reduction, as a relaxing left branch attached to

the cadential V (*GTTM*, pp. 121–122, 191–192, 340; see the branching for the II⁶ in Example 1 above).

Distortions of this sort are numerous in P & S but are best ignored after a certain point. Where possible, we try in this response to turn the discussion to points of theoretical interest.

4. However, for an exploration into real-time processing, see Lerdahl and Jackendoff, "Toward a Theory of Real-Time Processing of Musical Hierarchies," a paper delivered at the National Meetings of the Society for Music Theory, 1983.
5. See also Ray Jackendoff, *Semantics and Cognition* (Cambridge, Mass.: MIT Press, 1983).
6. Diana Deutsch and John Feroe, "The Internal Representation of Pitch Sequences in Tonal Music," *Psychological Review* 88 (1981): 503–522; Carol Krumhansl, "Perceptual Structures for Tonal Music," *Music Perception* 1 (1983): 28–62.
7. David Marr, *Vision* (San Francisco: Freeman, 1982).
8. Terry Winograd, "Linguistics and the Computer Analysis of Tonal Harmony," *Journal of Music Theory* 12 (1968): 2–49.
9. The Mozart is analyzed in Oswald Jonas, *Introduction to the Theory of Heinrich Schenker: The Nature of the Musical Work of Art*, transl. John Rothgeb (New York: Longman, 1982); see also Schenker, *Free Composition*. For Schenker's analysis of the Bach, see *Five Graphic Analyses* (New York: Dover, 1969).
10. Lerdahl and Jackendoff, "An Overview of Hierarchical Structure in Music," *Music Perception* 1 (1983): 229–252.
11. Noam Chomsky, *Aspects of the Theory of Syntax* (Cambridge, Mass.: MIT Press, 1965).
12. See also Lerdahl, "Théorie générative de la musique et composition musicale," *Le Concept de la Recherche Musicale*, ed. Tod Machover and Célestin Deliège (Paris: Christian Bourgois, 1985).

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