BABBITT, LEWIN, AND SCHOENBERG: A CRITIQUE

GEORGE PERLE

IN HIS brief "Note from the Underground" (PERSPECTIVES, Fall, 1962) Seymour Shifrin expresses certain reservations regarding the success of Arnold Schoenberg's attempt so to organize relations among the transpositions and transformations of his twelve-tone set as to "regain a normative procedure, in some sense analogous to, though certainly not consistent with, the structural norms of tonality." That Schoenberg's late works actually do represent such an attempt there can be no doubt. The *Violin Concerto* is perhaps the most eloquent example. Milton Babbitt's notes for the recording recently issued by Columbia Records and David Lewin's article, "A Theory of Segmental Association in Twelve-Tone Music" (PERSPECTIVES, Fall, 1962) both deal with the work from this point of view. The two essays are more or less complementary: the first surveys the work in its largest dimensions, presenting the overall structural potentialities of the set, a statement of the combinatorial principles that govern Schoenberg's choice of set forms, and even some clues to the compositional means employed to articulate the structural relationships —an altogether formidable achievement considering the restrictions imposed by the function and format of Babbitt's contribution; the second describes in careful and convincing detail how certain "structural harmonic techniques" that are entirely dependent on the serial ordering of the notes of the row are employed in a portion of the first movement. Both authors err in tacitly assuming that their fully documented proof of Schoenberg's "attempt to regain a normative procedure" in the *Violin Concerto* is equivalent to evidence of his success, or even partial success, in achieving this aim. If this "attempt is a crucial one, perhaps the most critically serious of the age," as Mr. Shifrin suggests, and as I believe it to be, then it is urgent not only to recognize the enormous importance of Schoenberg's formulation of the aim and to describe as exhaustively as possible the method by which he hoped to achieve it,[1] but also to recognize

[1] In this connection, as I have pointed out elsewhere, the significance of Babbitt's contributions has been immeasurable, secondary only to that of the work of Schoenberg himself, and his position in the development of twelve-tone theory unique, in that no serious criticism of any phase of his work is thinkable that does not depend on theoretical concepts and a technical language that he himself formulated and developed.

that in his own work he did *not* achieve this aim and to consider carefully the scope and causes of his failure. I anticipate that my criticism of these two studies may be viewed with suspicion by those of my readers who remember the shibboleth of Schoenberg's prewar critics: *Augenmusik!*—a harmless epithet in any case, for the subjectivist's claim to hear what an unfriendly critic maintains can only be seen is ultimately irrefutable, except perhaps by a lie-detector or other psychological test. It is my position that the claims Babbitt and Lewin make for the *Violin Concerto* are contradicted not only when the work is *heard* but also when it is analyzed.

In order to facilitate the following discussion I insert here the table of set forms found in Babbitt's notes. Each row of letters, reading from left to right, presents a different transposition of P, and, from right to left, a different transposition of R. Each column of letters, reading from top to bottom, presents a different transposition of I, and, from bottom to top, a different transposition of RI. The numerical designations are not the ones that Babbitt uses. The first P and R sets that appear in the composition are here designated by "0," and so are the first I and RI sets. Each set form is designated by a number that shows its distance in semitones, counting upward, from the original set form of identical aspect. In the quotations that appear in the remainder of this article I have revised the symbols used, wherever it was necessary, to make them conform to this table of set forms.

						I						
	7	8	1	9	2	4	10	11	5	6	0	3
0	A	B♭	E♭	B	E	F♯	C	C♯	G	A♭	D	F
11	A♭	A	D	B♭	E♭	F	B	C	F♯	G	C♯	E
6	E♭	E	A	F	B♭	C	F♯	G	C♯	D	A♭	B
10	G	A♭	C♯	A	D	E	B♭	B	F	F♯	C	E♭
5	D	E♭	A♭	E	A	B	F	F♯	C	C♯	G	B♭
3	C	C♯	F♯	D	G	A	E♭	E	B♭	B	F	A♭
P												R
9	F♯	G	C	A♭	C♯	E♭	A	B♭	E	F	B	D
8	F	F♯	B	G	C	D	A♭	A	E♭	E	B♭	C♯
2	B	C	F	C♯	F♯	A♭	D	E♭	A	B♭	E	G
1	B♭	B	E	C	F	G	C♯	D	A♭	A	E♭	F♯
7	E	F	B♭	F♯	B	C♯	G	A♭	D	E♭	A	C
4	C♯	D	G	E♭	A♭	B♭	E	F	B	C	F♯	A
						RI						

Lewin points out that three trichords derived from P_3 in the Cadenza are almost immediately restated a semitone higher, thus seemingly representing a statement of P_4, whereas the "real" source of the transposed chords is RI_3. Two important issues are raised: (1) What is the musical significance, if any, of this ambiguity? (2) What criteria determine which of the two set forms is to be regarded as the "real" one? A familiar ambiguity of traditional tonality derives from the extra-contextual equivalence of augmented six-five chords and dominant sevenths. These vertically identical sonorities are differentiated exclusively in their horizontal connections, that is, in their origins and consequences. Perhaps the ambiguity that derives from the identity of trichordal content of P_4 and RI_3 and equivalently related set forms can be made to have, in some sense, some analogous significance—a meaning other than the trivial one of attaching the "correct" label to a given statement of the set. But this "potential enriching structural ambiguity" is here confounded by a host of *non*-structural ambiguities. An immediate reason for regarding this set statement as representative of RI_3 is that the three transposed chords, which quite plainly articulate the first nine notes of P_4, are preceded by, though separated from, the notes A-C-F♯. It would seem farfetched to "explain" the latter as a permutation of the final trichord, C-F♯-A, of P_4. But, so far as the permutation of the segmental content is concerned, such an explanation is completely consistent with Schoenberg's frequent practice elsewhere in this work of linearly exploiting segments of the set as "broken chords" whose components need not adhere to the serial ordering. Since nine notes of the set are stated in the form of three trichords, it seems consistent to regard A-C-F♯ as an unordered linearization of the remaining trichord, except for the further fact that the trichords themselves, if they are regarded as representing P_4 rather than RI_3, are in the "wrong" order, for the fourth trichord appears first instead of last. But permutations of the order of the segments within the set are also found quite frequently in this work. However, as a rule, this "ambiguity" affects the principal hexachordal segmentation rather than the secondary trichordal segmentation.

I do not hesitate to grant that, though we "hear" a transposition of P_3, which we naturally take to represent P_4, the "real" set is RI_3, since no non-structural ambiguities—no reordering of either segmental relations or segmental content—are required by this interpretation. It seems to me that these non-structural ambiguities carry such weight throughout this work as to obliterate the possible effect of any "potential enriching structural ambiguity" deriving from the relation between the two set forms. Nevertheless, the determination that RI_3 is the "real" set rather than P_4 may still be significant. I will consider this question from three different points of view: (1) the strategic role of the diminished triad as a

boundary element of the set; (2) the transpositional relations of set forms employed in the first movement as a whole; (3) the general compositional exploitation of the segmental identities exposed in the passage under consideration.

(1) Since Schoenberg's formal articulations normally present complete set statements or multiples of complete set statements, initial and final elements of the set assume a special importance, for they are often heard as melodic or harmonic incipits and cadences and as typical thematic details. The striking and beautiful use that is made of the rising semitone, with which the prime form of the set begins, and of the inversion of the same, will immediately occur to the reader. The three notes which conclude these set forms and which begin their retrograde aspects tend to function as a highly characteristic *harmonic* unit, the diminished triad. As Mr. Lewin points out (p. 97), "the diminished triad-as-segment plays a strong structural role in the piece." This triad would appear to be so characteristic as an initial element of RI that we might say it determines the set in question to be an RI form rather than a P form, even though it is here articulated in a manner that separates it from the remainder of the set. But the moment we consider this detail in its larger connections we find its "strong structural role" weakened and perhaps destroyed by Schoenberg's often arbitrary derivation of the same harmonic element through the contrived manipulation of non-adjacent elements of the set, so that this *structural* diminished triad is confronted with *non*-structural ones. The first striking exposure of this chord as a boundary element occurs in m. 14, where the orchestra drops out and the solo violin concludes I_0 with the notes E♭-A-F♯. The soloist then introduces (m. 15) the first statement of R_0, with the notes F-D-A♭, still unaccompanied. In m. 16 orchestra and soloist together complete the first statement of R_0 with the following simultaneity: C-F♯-E♭-A. This diminished seventh chord associates very effectively with the diminished triad which had concluded I_0 two bars earlier and which commences the first statement of RI_0 in the following bar, where the diminished triad is again given to the unaccompanied solo violin. As a derivation of R_0, the diminished seventh chord is most confusing, for it is a combination of the sixth, seventh, tenth, and twelfth notes of the set. Thus, we find that what we had taken to be a characteristic concluding element of I_0, the diminished triad E♭-A-F♯, can also be contrived to appear at the conclusion of R_0, by arbitrarily ignoring, at this point, the basic assumptions upon which the structural significance of this harmonic element depends. It may be protested that the set is defined not only serially but also as a collection of segments and that the serial ordering may be violated at times without destroying the integrity of the set. This is quite true, but the diminished seventh chord that we find at the conclusion of R_0 is not derivable from any partition-

ing of the set that plays any role whatever in this particular work. It may also be argued that if revisions of the serial ordering and/or of the segmental content are exploited consistently enough, an additional criterion of validity may be established. The way in which mm. 18-19 are clearly parallel to m. 16 may be cited. There is no question that Schoenberg attempts to validate extra-structural elements by means of surface associations, but the attempt is not successful. The ambiguity that results when associative harmonies are generated by extra-serial as well as serial procedures is not a "potential enriching structural ambiguity." It is the foundational assumptions themselves that become ambiguous. And yet Schoenberg, unlike Berg, obsessively insists on certain procedures that can have no meaning apart from these assumptions, as we shall see.

(2) Throughout the work, each transformation of the set at a given pitch level is regarded as representing a specific harmonic area and is associated only with such transpositions of each of the other transformations of the set as represent the same harmonic area. The basis of this association is the mutual exclusiveness of pitch content of corresponding hexachords of P_0 and R_0, P_0 and I_0, R_0, and RI_0, and I_0 and RI_0, or of any complex of equivalently transposed transformations of the set. If corresponding hexachords of given set forms are mutually exclusive in content, then non-corresponding hexachords of the same set forms are equivalent in content. A given complex, therefore, may be said to comprise such transpositions, and only such transpositions, of the four transformations of the set as will preserve the respective content of two mutually exclusive hexachords. Returning now to the passage in the Cadenza under consideration, we find that RI_3 is a member of the same complex of set forms as is represented by the set statement that immediately precedes it and by the six set statements that immediately follow it, whereas P_4 represents a different complex. This is very good evidence indeed, that the "real" set represented here is RI_3, rather than P_4. Since each complex of set forms is self-contained, one wonders whether or not it is possible to establish any relationship between one area and another. In this connection, Babbitt has the following to say:

> Such complexes dominate exclusively large sections of the composition. For example, the four sets so associated with P_0 (P_0, I_0, R_0, RI_0) are the only sets employed during the first fifty-eight measures of the concerto,[2] and the area thus delineated is normatively closely analogous to a functional tonal area. And when, at measure 59, a new hexachordal area is introduced, it is associated with new thematic materials. The new area is that defined by the hexachords of P_7, a transposition by a

[2] There is a minor oversight here: a series of transpositions occurs in mm. 47 to 52, the original complex returning in m. 52.

perfect fifth of P_0. The transpositional relation may suggest a parallel with the dominant region of tonal "second subjects," but—be that pertinent or not—this particular transposed form (and, naturally, its complement: the perfect fourth) has a singular hexachordal relation to P_0, in that it preserves the greatest number of pitches (four) between corresponding hexachords of any set not in the initial complex. In other words, P_7 is that set which, by a traditionally tested and reasonable criterion of relatedness, carries the work away from the opening area to the most closely related area, and this relatedness is determined completely by the structure—the intervallic structure—of the hexachords of the set.

I fail to understand Babbitt's reference to a "singular hexachordal relation." P_6 also preserves four pitches between corresponding hexachords with P_0, just as P_7 and P_5 do. I also fail to understand why, particularly since the "new hexachordal area . . . is associated with new thematic materials," the relatedness of set forms of one complex to set forms of another complex should only be determined by the content of corresponding hexachords of set forms of a single aspect. The preservation of four pitches between corresponding hexachords of P_0 and R_2, R_3, R_4, R_8, R_9, and R_{10} seems to me, both in principle and in respect to the particular instance of Schoenberg's *Violin Concerto,* as valid a criterion of relatedness as the previously cited connections with different transpositions of P. But if this is the case, the concept of relatedness between different *complexes* on the basis of the number of pitches preserved between corresponding hexachords cannot, in general, be a valid one. For, any two complexes that present maximum relatedness on the basis of the association of P of one with P of the other, will present minimum relatedness on the basis of the association of P of one with R of the other, and vice versa.[3] As a matter of fact, the first set statement in the "new hexachordal area" that commences at m. 59 is R_7, which preserves only two pitches between corresponding hexachords with P_0, the *least* possible number that P_0 can preserve between corresponding hexachords with any set form that is not a member of the initial complex. And the association of P_0 with R_7 seems to me just as probable, or rather just as improbable, as its association with P_7.[4] If the reader will perform the following experiment, he will discover some

[3] An example of what I would regard as validly established relatedness dependent upon common pitch elements among segments of different sets and set forms is found in my article, "The Music of *Lulu:* A New Analysis," *Journal of the American Musicological Society,* XII, Nos. 2-3 (1959), p. 193. *Lulu,* however, as I show in this article, is not based upon the definitive assumptions of Schoenberg's twelve-tone system.

[4] Babbitt assumes, apparently, that where a complex is transposed each hexachordal area will normally tend to be associated with its own transposition rather than with the transposed complementary hexachordal area. I don't know whether or not the validity of this principle can be theoretically confirmed, but it is certainly not confirmed in the given instance.

interesting relationships among the transpositions of the original complex, relationships which seem to me to explain a good deal more about Schoenberg's procedures than Babbitt's theory does. For the subscripts 0 to 11 inclusive, which we have been employing to identify the pitch levels of the complex, substitute the letter names of the notes of the chromatic scale, C to B inclusive. Let us now consider the over-all transpositional relations of the first movement. It will be discovered that most of the movement is "in C," that the next most prevalent "key" is the "dominant," "G," and the third most prevalent the "subdominant," "F." So far, it is difficult to say whether we should suppose the "principal key" to be "C major" or "C minor," but our further investigations fairly well establish the minor mode as primary, for "A♭" represents the next largest area, and after that "E♭" and "B♭." The only "key" that does not appear at all, not even in passing, is "C♯" (nor "D♭," of course, though it does seem that the Neapolitan 6th of C minor might have suggested a "D♭" episode). The distribution of these pitch levels in the movement as a whole confirms the conclusions that follow from this comparison of the relative prevalence of each of them. All in all, except for a brief excursion into "F♯," the "structure" of the *Violin Concerto* seems remarkably "diatonic" for the composer of *Verklaerte Nacht.*

(3) The choice of pitch levels *within* any single complex throughout the work is rigorously dependent, as we have seen, on the principle of the preservation of total hexachordal content among members of the complex. It is equivalence of *trichordal* content between P_4 and RI_3 that gives rise to the ambiguity that we have discovered in the Cadenza of the *Violin Concerto.* If preservation of hexachordal content is a valid basis for set association, why not preservation of trichordal content? The trichordal content of a given set, say P_0, would generate the following complex: P_0 and R_0, P_0 and RI_{11}, R_0 and I_{11}, and I_{11} and RI_{11}. Schoenberg's complete neglect of the *structural* potentialities of the trichordal segmentation of the set[5] reduces his occasional compositional exploitation of this type of segmentation, as at the beginning of the Cadenza, to a casual surface phenomenon, quite devoid of any structural significance and suggesting only the most trivial local associations. I think this criticism applies just as fully to the two measures at the conclusion of the Cadenza, which Lewin singles out for special attention because the trichordal presentation of I_{11} serves as a substitute for the "expected" return of P_0, in that I_{11} and P_0 are trichordally equivalent. The trichordal structure of I_0, which is combinatorially associated with P_0, is just as significant as that of P_0. Yet the sets that are trichordally equivalent with I_0 (P_1 and R_1) are not found in the

[5] For example, the exploitation of two types of complex, one based on the hexachord, the other on the trichord, would have provided a basis for the shifting of pitch levels and for the overall organization of transpositional relations.

first movement of the concerto at all. In the midst of so many structural and non-structural ambiguities, the arousal of "expectations" (in the sense in which this word is normally used in discussions of music) is dubious at best, not to speak of the fulfillment of such "expectations" by means of an "unexpected" appearance, the only one in the movement, of I_{11}.[6]

Schoenberg's insistence on basing every single complex of set forms on the hexachordal relationship, even where his compositional procedures exploit other types of segmentation or where they deliberately obfuscate the hexachordal areas (as in the section beginning at m. 93), is a ritualistic obsession. So is his insistence on somehow including every single pitch element of every set statement, in view of the freedom with which he revises other assumptions of the "system." (In fact, it is precisely these arbitrary consistencies that justify one's continual reference to a "system" in Schoenberg's work, in spite of the "licenses" he allows himself.) These and other obsessions interfere with the "surface similitudes, particularly thematic ones," which, suggests Babbitt, can "provide continuity and association in the first stages" of one's acquaintance with the work. Later stages in my own acquaintance with the work have failed to provide any basis of continuity and association other than these "surface similitudes." I do not believe that either Babbitt or Lewin has succeeded in establishing more than an *intention* on Schoenberg's part to provide a structural basis for continuity and association. Schoenberg's unsuccessful attempt to provide such a structural basis is responsible for certain obvious and fatal flaws in the *Violin Concerto* and in other late works. The attempt itself is more significant and has been more influential than the successes of any other composer of his generation. His failure is one that is worthy of the author of *Moses und Aron.*

MR. BABBITT ANSWERS:

SINCE I shall not presume here to "defend" the Schoenberg *Violin Concerto* against George Perle's charges of "failure" and the possession of "fatal flaws," and shall be concerned with asserting my own virtue only to the extent of attempting to demonstrate that I did not commit the specific, if interesting, sins of which I have been accused, this will be not the extended discussion that may appear warranted but a rather brief

[6] I have considered, and rejected, the possibility that Schoenberg's total avoidance of the P_1 complex, thus excluding the set forms that are trichordally equivalent with his principal I and RI set forms (I_0 and RI_0), and his nearly total avoidance of the I_{11} complex, thus almost completely excluding the set forms that are trichordally equivalent with his principal P and R set forms (P_0 and R_0), is a negative indication of awareness on his part of the significance of the trichordal segmentation. Cf. the conclusion of the preceding paragraph.

note, for all that certain of the issues raised by Perle are, in their implications, important and intricate.

There are at least three ways in which I did not "err" in "assuming that" my "fully documented proof" is "equivalent to evidence of. . . success." I could not have so erred, simply because I never stated or implied that I so assumed. And, surely, nothing is "fully documented" in my necessarily casual and brief program notes nor could there be in so limited an assigned space, any more than there could be of such a work as, say, *Pierrot Lunaire.* And, certainly, I never stated or implied that my notes contained a "proof" of any proposition, since I have assumed always that a proof is the process of deriving a sentence from other so-proven or primitive sentences by means of stated rules of inference. I have no idea what the term "proof" could be taken to mean in other than a logical context, whereas my statements were selected from the infinity of possible true statements which could be made of the concerto and were selected because of their dependence upon certain easily inferable criteria of similarity, which can be "justified" only experientially and can be regarded as reasonable on the basis of rather widely and lengthily tested hypotheses. I cannot admit the application of the term "proof" to a domain where it is clearly inapplicable. Then, Perle's accusation can be recast to read that *had* I presented so documented a "proof" I *would have erred.* By overlooking the counterfactual nature of this new statement, I still stand accused of having proceeded from the logically true to the empirically validated by entailment. However titillating it may be to be charged with the most unlikely of crimes, I must insist that—in this regard—I am still unsullied methodologically, since I not only proved nothing in the logical domain, but made no claims with regard to empirical verification or confirmation. To the extent that I regard my introductory observations as explanatory "analysis" at all, it is only as a selection from a rational reconstruction, whose protocol statements derive their relative incontrovertibility by virtue of their being statements of aural and conceptual capacities which appear to be acquirable. Naturally, I cannot assume responsibility for what is "heard," but only for what can be learned to be "heard." Otherwise, I should be at the mercy of the inadequate training, knowledge, intellectual capacity, and dubious veracity of any listener offered as a counter-example. Therefore, beyond gross physiological considerations with which neither Perle nor I am concerned here, the notion that the "heard" and the "analyzed" are related to each other necessarily as proper subset to the set which includes it properly, is indefensible. Does Perle really wish to assert, as he has, that if a musical entity can't be analyzed then it can't be heard? If he does, then he owes it to us to reveal the secrets of his analytic method, or his new definition of "analysis." The problem of analysis, of course, is that of significance, not of identification.

So I shall interpret Perle's differentiation as merely between that mode of analysis which purports to be the representation of a reported hearing and that which is a rational reconstruction. Since he states his belief that an analysis of the former type is not open to disconfirmation, it is pointless to discuss it here. His disagreement must be with the structure of my rational reconstruction, particularly the fruitfulness of its concepts.

In this respect, although I realize that Perle is employing the editorial "I" in the statement, "I fail to understand Babbitt's reference to a 'singular hexachordal relation,' " and is speaking for those less experienced readers who, when confronted by my notes, will read perhaps too hastily the certainly too compressed statement regarding the singularity in question, I must emphasize that I said "in that it is ," not "completely in that it is" or "entirely in that it is." Briefly, I stated a necessary, not a sufficient condition, in the belief and hope that the experienced would infer the complete basis of singularity and the inexperienced would accept the fact on faith or fatigue—and even welcome the lack of space that prevented further explanation. The further explanation is that, in terms of pitch preservation, S_7 (I employ Perle's relabelling of my rows and columns for the convenience of the reader) and, as I pointed out, its complement S_5, singularly contain, individually, four pitches in common with S_0, and —together—contain all the pitches of S_0. S_6, the other transposition which preserves four elements in corresponding hexachords, does not combine with its complement (itself) to state the total content of S_0. The fact of and, perhaps, the compositional consequentiality of this property can be perceived if one but observes that each element in each of the discrete hexachords of the set is related to at least one other element by the interval of 5 or 7, but that only four elements in each hexachord are related by an interval of 6. Although this suffices to clarify my remark regarding the singularity of S_7 and S_5, I might add that there is a further property—involving only pitch identity within the hexachord, plus the identity of pitch and order number—which differentiates significantly S_7 from S_5: the initial pitch class of I_7, the combinatorially related inversion of P_7, is identical with the initial pitch class of S_0. Perle's stated failure to "understand" why I should insist, allegedly, that "relatedness of set forms . . . should only be determined by the content of corresponding hexachords" is understandable, since I never stated or implied that relatedness should be determined *only* by this criterion. Rather, in a summary introduction, I felt constrained to present only the most immediate and obvious bases of relatedness between set segments. Surely, two segments which are identical in interval succession and possess n pitch classes in common must be adjudged more closely related than two segments which differ in interval succession and possess n pitch classes in common. To assert otherwise is to assert that identity of interval succession is of abso-

lutely no relational significance. Why would the "validity of this principle" have to be "theoretically confirmed" (whatever the operational meaning of this expression may be) beyond this? And, although I explicitly assumed nothing about "association," I am willing to suggest that the most closely related collections "tend to be associated"; indeed, are not the two expressions usually employed as synonomous?

I do wish to thank Perle for bringing to my attention what he gently terms "a minor oversight," but what is actually an ambiguity arising from oversimplification and overcompression. My statement that the "four sets so associated with S_0 are the only sets employed. . ." was meant to convey that this collection of sets is the only such combinatorial collection so employed during the opening fifty-eight measures; the locally generated S_4 and S_8 and their inversionally related sets which occur in mm. 47-52 are not associated with such a collection.

Perle's discovery of what he terms "interesting relationships" which he believes to surpass in explanatory scope those signified by my statements depends upon our assuming, first of all, that the first 58 measures are "in C," for which no corroborative, internal evidence is offered by way of establishing functional interrelations; there is only the statement that the transposition up a perfect fifth places the work in "G," and so on. But "change of key" within a work is secured by the transposition of tonal functions, not—necessarily or usually—of total content. These functional relations may be said to represent scale content associated with the "key" in question, but the scale content is not necessarily compositionally presented, and the transposition of a scale always involves the adjoining of new pitches, which the transposition of a twelve-tone set cannot. Before one can speak of the transposition of functional relationships, one must establish the presence of these relationships. If a transposition is to be taken necessarily to signify a "change of key," then if mm. 17-18 of the "Eroica" Symphony are "in E♭," then mm. 21-22 must be assigned to "A♭," a description which would be regarded as excessively and fruitlessly local by even the most modulatory of analysts. I am tempted to conclude that Perle suggested this interpretation of the concerto in order to view it as a tonal work to which the epithet "change of key" is appropriately applicable as a description of the total function of and means of securing "functional areas." And, since it is agreed generally that the masterpieces of the tonal era arrived at and contained such areas within the conjoined progression of linear and triadic unfoldings of a single functionally oriented structure, thereby achieving qualitative hierarchization rather than the mere parallelism of transposition, the concerto could therefore be adjudged a "poor" or "chaotic" tonal work. (Incidentally, in a work in "C minor," shouldn't "E♭" occur more often than "G"?) Or, if what Perle is attempting to explain is Schoenberg's choice of 7 as the most

prevalent interval of transposition, is he not—therefore—attempting superfluously to explain why Schoenberg chose a hexachord in which 7 (or 5) is the most frequent interval? And to do this by historical analogy is to provide, at best, a biographical conjecture. More fundamentally and concretely, the analogy between the transpositions of a set segment and those of a major (or minor) scale is untenable. The transposition of a scale to complementary levels necessarily results in the same interval or intervals being formed by the newly adjoined pitch classes in both cases; this follows from the fact of such a scale being generated by a single interval. (For instance, if the C major scale is transposed to D, the adjoined pitches —C♯ and F♯—create a fourth; if it is transposed to B♭, the adjoined pitches —B♭ and E♭—create a fourth.) But this is not true for set segments in general, only for those which are inversionally symmetrical. Thus, the first hexachord of the set of the concerto when transposed by 2 adjoins C, C♯, F, A♭; when transposed by 10, it adjoins C♯, D, G, A♭. Obviously, the two collections are not intervallically identical.

I cannot believe that Perle's explanation is meant to be explanatory so much as revelatory. This is the only excuse I can find for his embracing the intentional error, which he does explicitly when he asserts that Mr. Lewin and I have succeeded in establishing no more than "an intention on Schoenberg's part." I do not know what it means to establish an intention, or to establish what is intended apart from what is presented. I assume that a composer intended what he presented, since I could never establish that he had not. Intentions allegedly established by the invocation of extra-compositional "evidence" can be neither authoritative nor relevant. What if, by such means, it were alleged that the composer intended "x" but presented what is much more simply and completely understood as "y"? Is the work, therefore, a failure? If so, it becomes one only after the "evidence" for "x" has been discovered. What if it is "discovered" that a composer intended to write a boring work and realized his intention? Does this make the work less boring, since—presumably—it "successfully" realized its intention?

Perle's observation on the neglect of the trichordal characteristics of the set as a large-scale structural determinant should not be allowed to obscure the significance of the trichordal similitudes within the set, since these still effect reference and internal redundancy by the very fact of their presence. And to term a "ritualistic obsession" the insistence on hexachordally related sets, even when "other types of segmentation" are exploited, is to disregard the fact that the establishing and maintaining of such a partition provide a norm against which other partitions can be measured and perceived in terms of degree of departure and deviation.

Since Perle is obviously dissatisfied with the concerto, even to the extent of rejecting his own "tonal" explanation as an acceptable

characterization of the work's continuity and association (since he says finally that he can find no "basis of continuity and association other than these 'surface similitudes'"), and since I suggested no evaluative conclusions (aware as I am that a descriptive or analytic statement entails no normative conclusion), I can wish only that someday Perle will see fit to write about the work, rather than to write about writings about the work.

—Milton Babbitt