# THE SCIENTIFIC IMAGE OF MUSIC THEORY

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If music theory is to be taken seriously, and we think it should, then it must clarify the nature of music and thereby guide our musical activities, whether they be performance, composition or historical research.[1] Music theory must also be a rational pursuit. By 'rational' we mean nothing arcane, merely that theory helps us illuminate, elucidate, understand or explain music. We describe these acts as rational only to contrast them with mystical and emotive acts, such as worshiping, being moved by, or becoming one with.

Now, although music theory may be rational, it is not immediately obvious how it fulfills this demand. Indeed, there is remarkably little concensus among theorists about what the goals and methods of the discipline should be. Some believe that music theory ought to model itself on the sciences; they claim that it can and should aspire to the rigorous methods and precise terminologies that have made science so successful in accounting for the world around us. They insist that it is only by applying scientific paradigms to well-defined phenomena, that music theory can be truly explanatory.

Others, however, support a quite different view; though unwilling to deny rational methods altogether, they are reluctant to conspire with the physicist and the biologist. Instead, they maintain that while scientific methods may work to explain the physical world, they cannot apply properly to music. They claim that scientific music theories cannot be tested objectively; that they cannot clarify the most significant aesthetic features of pieces; and hence that they can only be insensitive or mechanical. Such critics also believe that the ultimate purpose of analysis is not to find general laws about music or specific types of music, but rather to individuate unique masterpieces. Theorists are left, then, with an unhappy dilemma: they can be scientifically rigorous, but only at the cost of ignoring what is musically interesting about individual pieces; or they can be sensitive, but only at the price of being subjective and *ad hoc*.

The following paper will reconsider this dilemma by evaluating the claims for and against scientific music theory.[2] On the one hand, we will argue that the case for scientific music theory is far stronger than its detractors allow, and that most critics exaggerate the limitations of scientific methods because they misunderstand the nature of scientific explanation. We do not suggest that scientific methods come without a price: on the contrary, we believe not only that the costs are substantial, but that they are not fully appreciated by even the staunchest advocates of scientific paradigms. In particular, if theorists adopt scientific standards of explanation, then they must be prepared to abandon all hope of explaining what is unique about particular compositions. However, we insist that giving up such a goal does not deprive the theorist of all understanding of particular works, and more importantly, does not doom his or her analyses to being trite, mechanical, or aesthetically uninteresting.

Our paper has four parts. Since the most extensive discussion of scientific music theory appears in Benjamin Boretz's *Meta-variations*, and since this work is still widely thought to satisfy scientific standards of explanation, the first half of our paper will deal primarily with this text. In Part I, we will sketch the positivist model he attempted to endorse. We will focus on two particular points: 1) that scientific explanations require law-like generalizations; and 2) that these generalizations must be reducible to simple sense experiences.

Next, in Part II, we will describe what Boretz actually accomplished. We will show that while *Meta-variations* is certainly an important contribution to music theory, there are still basic inconsistencies between what Boretz thought he was doing and what he in fact did. In particular, we will argue that although he claimed to produce scientific theories, Boretz failed to achieve this goal because he was unable to reconcile the need for general laws with his desire both to explain the uniqueness of existing pieces and to develop maxims for composing new pieces. Furthermore, in reducing the language of music theory to a simple phenomenalist vocabulary, Boretz encountered the same problems that the positivists had in reducing the language of natural science.

In Part III we will consider some standard arguments against the fruitfulness of scientific music theory. In particular, we will focus on three challenges to the objectivity and relevance of scientific models for music. First, we will discuss the widely held view that music theory cannot be objective because it is always theory laden. Second, we will examine the claim that scientific music theory cannot work because music is by its very nature changeable and creative; hence, it is not susceptible to scientific tests. Third, we will look at the popular claim that because music theories necessarily deal with a finite body of pre-existent material they cannot be predictive in any scientific sense.

Finally, in Part IV, we round off the paper in two ways. First, we will examine the main alternative to scientific music theory, a view we call particularism. Second, we will refine the model of science presented in Part I in order to make it more attractive to music theorists. In order to support our case, we will draw on recent debates in philosophy of science.

## I

Although music theorists have appealed to scientific methods at least since the time of Aristoxenus of Tarantum, these methods are perhaps most conspicuous in the work of group of Princeton theorists from the 1960's and early 1970's[3]. To counteract the poorly argued and vague manner in which they thought earlier analysis was conducted, they tried to regiment the language of music theory along the same scientific lines that the Logical Positivists tried to regiment the language of philosophy in the 1920s and 30's. In the now famous words of Babbitt:

> . . . the notion of analysis, and . . . the requirements of linguistic formulation . . . provide the important reminder that there is but one kind of language, one kind of method for the verbal formulation of "concepts" and the verbal analysis of such formulations: "scientific" language and "scientific" method. Without even engaging oneself in disposing of that easily disposable, if persistent, dichotomy of "arts" and "sciences" . . . it only need be insisted here that our concern is not whether music has been, is, can be, will be, or should be a "science" . . . but simply that statements about music must conform to those verbal and methodological requirements which attend the possibility of meaningful discourse in any domain.[4]

Roughly speaking, there are three essential components of positivism.[5] First, positivists divided knowledge into logical and empiric truths. They thought that the only way we can know empiric truths is through direct observation or through the scientific method. Second, they presumed that the only things we can ultimately take as real are those things which can be directly perceived, phenomenal events. This view is known as Phenomenalism. Third, they assumed that to be considered meaningful, all scientific statements must be ultimately reducible to a phenomenalist basis. This sematic cornerstone of phenomenalism is the so-called verifiability principle of meaning. Let us consider each of these points in detail.

Of the various models of scientific explanation, perhaps the most well known is the Deductive-Nomological or D-N model outlined by Carl Hempel in the 1950's and 1960's.[6] Hempel ultimately rejected positivism, but D-N captures the hypothetico-deductive logic of scientific inference widely upheld by the positivists.[7] Like rival models, D-N starts from a basic distinction between descriptions and explanations. Quite simply and roughly, whereas descriptions merely list WHAT features given objects or events have, explanations also show WHY the events occur or WHY the objects behave the way they do.[8] Descriptions can apply to particular things but need not tell us anything about what to expect from other events or entities. Explanations, however, are concerned with relationships between individuals and events and can tell us what to expect about whole classes of related phenomena.

D-N tries to capture these general features of explanation for single laws and for the groups of laws we call theories. It can be schematized in the following manner:

*The Deductive-Nomological Model*

| | |
|---|---|
| C1, C2, . . . . . , Cm | |
| | Explanans |
| L1, L2, . . . . . , Ln | |
| - - - - - - - - - - - - - - - - - - - | |
| E | Explanandum |

Here, E denotes a statement about the phenomena being explained; it is called the 'explanandum.' C1, C2, . . . Cm are statements that describe particular facts or 'initial conditions,' relevant to the particular phenomena being explained. L1, L2, . . . Ln are law-like generalizations that "cover" phenomena and conditions of the kinds indicated by E and C1 . . . Cm. The two sets C and L are termed the 'explanans' and the entire schema consititutes the explanation.

The two most important features of D-N are implied by its very name. First, the term "deductive" requires that the explanans must logically entail the explanandum. Thus, in a well-formed explanation, if the statements in C and L are true, then E must necessarily be true as well. This logical connection helps to guarantee the testability of explanations: for a given set of covering laws, we can predict particular empiric consequences or explanandum events; by checking these predictions against observations, we can confirm or disconfirm our laws or theories.

We must stress, however, that while it may be possible to reconstruct scientific explanations according to D-N, such explanations need not be developed or presented in this precise way. D-N does not summarize the process of discovering explanations; it merely specifies the strictly logical relationships that make explanations work. Thus, even if music theories are not laid out or devised according to the schema listed above, they may often conform to D-N.

The second feature of D-N is conveyed by the term "nomological." The root of this term is the Greek word 'nomos' or law; because D-N requires empiric laws, it is often termed the Covering-Law Model.[9] Now, although laws are the defining characteristic of scientific explanations, they are extremely hard to pin down: not only is it difficult to establish general rules for constructing laws but it is also tricky to specify what laws will apply in any given case.[10] Very basically, however, natural laws are generalizations over a class or kind. (Some philosophers restrict them to generalizations over "natural kinds.") These generalizations are such that they govern our expectations about properties of past, present, future and possible instances of that kind, whether these instances are known or unknown.

Normally, these generalizations must satisfy four conditions: 1) they must in some sense be true; 2) they must have empiric content; 3) they must be universalizable; and 4) they must be predictive. However, these conditions need some clarification and qualification.

First, even the best explanations often contain generalizations that are only approximately true; consequently, we prefer to use the term "law-like generalization."[11] Second, since laws must have empiric content or have some bearing on the way the world is, they cannot be purely logical truths or stipulated definitions, since the truth of neither is empirically testable. Consequently, although scientific generalizations can be expressed formally, not all formalized statements constitute scientific theories.

Third, not all generalizations can be law-like because not all generalizations are universalizable. For example, some generalizations describe repeated coincidences, but they do not show why such coincidences occurred or why the relevant entities behaved the way they did. Take, the statement "All articles in *Music Theory Spectrum* have odd numbers of pages." Though the statement clearly expresses an empiric generalization, we would be foolish to infer, even if it were true, that all future articles will also have odd numbers of pages. The generalization doesn't seem to support such an inference because it offers no account of why previous articles did not have even numbers of pages. To be universalizable in a strong sense, laws cannot, strictly speaking, apply to classes that are by definition finite in size.[12] We can, however, make law-like generalizations about small, even singular classes, if these laws also cover possible but nonexistent members of the class. Thus, we can formulate laws that cover the origin of the universe, obviously a singular event, provided that they tell us how and why the universe was created, since this information helps us understand how other universes might in principle be formed.

Fourth, since laws and theories delimit classes of phenomena they must have boundaries; law-like generalizations must be able to predict or retrodict that, under certain conditions, some phenomena will or did occur and others will not or did not.[13] If a generalization does not yield these specific predictions, it cannot provide a basis for distinguishing one class of phenomena from any other. To give a simple example from music theory, a valid theory of tonality must be able to predict specific pitch relationships that occur only in tonal pieces and not in non-tonal ones.

These conditions on the law-like character of a generalization can be crudely summarized by requiring that law-like generalizations "support" relevant counter-factual conditionals. A counter-factual conditional is an "if . . . , then . . ." proposition normally expressed in the subjunctive mood. For example, when examining a piece in simple ternary form, we might note that "if this piece were a sonata form, then some material from the exposition in a key other than the tonic would be recapitulated in the tonic near the end of the piece." This counter-factual is, of course, supported by the strongly confirmed generalization that in sonata forms such a tonic recapitulation occurs. Now, the original sentence is called counter-factual because the antecedent "if this piece were a sonata form" seems to be presumed false for the work in question. The concept of support, at least in Hempels's models, means that the generalization, along with the correct initial condition statement, entails the counter-factual. It should be noted, however, that introducing counter-factuals helps to reformulate the problem of law-like generalizations, but it does not provide anything like a solution to the problems of generalizing about them.

Besides embracing a scientific method approximating to D-N, positivists also endorsed phenomenalism and the verification principle of meaning. Phenomenalism holds that reality includes nothing more than what can be "directly observed"; hence, it reduces its ontology ultimately to those things that are "given" by the senses. According to them, not only are theoretical entities, such as X-rays, neutrons, and the Pre-Cambrian Period, beyond the grasp of direct observation, but so, too, are commonplace objects of our everyday world. Phenomenalists either regard indirectly observable things as "metaphysical nonsense"—e.g., beauty or goodness—or as "theoretical constructs."[14] They argued that even our everyday accounts of the world are simply shorthand characterizations of past, present and future patterns of sense experience.

To anchor science to the certainties of phenomenalism, positivists adopted various criteria of "cognitive meaning," the most famous being the criterion of verifiability. This principle proposed that we build an entire language from primitive predicates that referred only to sense data. Sophisticated or theoretical levels of discourse were then regarded as "logical constructs" derived from the primitive sense-data by logical operators. This position is, obviously, highly reductive. Positivists tried to boil down all entities to primitive sense experiences. Thus, they could no longer take at face value theoretical claims committed to the existence of unobservables. Instead, they treated them as mere terminological abbreviations, instrumentally useful in predicting what observations one could expect to make under given test conditions.

## II

Having considered the basic tenets of positivism, we can now examine how Benjamin Boretz developed them in *Meta-variations*. The basic layout of this study is shown in Table 1.[15] Very briefly, Part 1 deals with the philosophical confusions committed by earlier music criticism. Part 1 also considers the types of entities required for music theory, the question of empiric adequacy and the relevance of D-N. Part 2, then, presents a constructional system for all musics and Part 3 describes "syntaxes" for specific repertories and pieces. Finally, in Part 4, Boretz analyzes six extracts in detail: Wagner, *Tristan* Prelude; Webern Op. 5 no. 4; Brahms, Symphony no. 4, mm. 1–18; Stravinsky, *Petrouchka,* Scene 1; Schoenberg, Op. 15, no. 1, mm. 1–7; Boretz, *Group Variations.*

Although there is much in *Meta-variations* that is obscure, it is clear that 1) to rid music theory of vague, metaphysical talk, Boretz endorses phenomenalism and the verifiability principle of meaning, and 2) to give music theory methodological rigor, he claims that it should conform to the logic of explanation demanded by D-N.

From the very beginning of Part 1, Boretz demonstrates his commitment to phenomenalism by drawing a sharp demarcation between what is cognitive or reducible to directly observable phenomena, and what is non-cognitive. Indeed, throughout *Meta-variations* he claims that metaphysical and theoretical statements must either be rejected or translated into a neutral "observation language." He shows the different types of language in an example given here as Table 2[22]. In Line A, we see the metaphysical term "chord of nature," the theoretical terms "chord," "triad" and "tonic triad," and the observational term "simultaneity." In Line G we see the metaphysical term "baroque," the theoretical terms "Sonata Form," and "Baroque" and the observation term "pattern-of-repetition-structure."

According to Boretz, most if not all of the vocabulary of traditional music theory is non-cognitive and must be replaced by an "ontologically sparser descriptive discourse."[23] With the typically aggressive rhetoric of the positivist, he denounces traditional terms as "metaphorical conceits," "non-empirical predicates," "runaway analogies," and even as "sloganizing."[24] In addition to blatantly gushy effusions, Boretz also discards seemingly innocuous expressions such as "tension and release," "similarity," "coherence," "development," "finality," "expectation," "style," plus all

TABLE 1

Boretz, Meta-variations: Studies in the Foundations of Musical Thought

| | | |
|---|---|---|
| Part 1 | 1. | Preface: Normatives and Objectives (pp. 1–8) |
| | 2. | Introduction: Varieties of Musical Thought and Confusion (pp. 8–20) |
| | 3. | Part I: Models and Metaphors in Musical Discourse |
| | | A. The Theoretical Character of Musical Entities (pp. 21–28) |
| | | B. The Designata of "Music" (pp. 29–40) |
| | | C. Explanatory Adequacy (pp. 40–51) |
| | | D. Linguistic Models as Musical Models (pp. 51–70) |
| | | E. Concluding Remarks (pp. 70–74.)[16] |
| Part 2 | 1. | Introduction (pp. 49–50) |
| | 2. | Music Theory, Epistemology, and Constructional Systems (pp. 50–52) |
| | 3. | The Notion of "Definition" (pp. 52–55) |
| | 4. | The Notions of "Structure" and "Musical Coherence" (pp. 56–58) |
| | 5–6. | The Extralogical Bases of Constructional Systems (pp. 59–61) |
| | 7. | The Role of "Sounds" in "Music" (pp. 61–63) |
| | 8. | Music-Theoretical Systems, Aesthetics, and Ear Training (pp. 63–71) |
| | 9. | Some Conceptual Consequences of a Music-Constructional System (pp. 71–72) |
| | 10. | The Beginning of the System (pp. 72–74) |
| | 11. | Outline of the Construction (pp. 74–85) |
| | 12. | Primitive Symbols and Operators (pp. 85–86) |
| | 13. | Pitches, Pitch Functions, and Pitch Relations (pp. 86–102.) |
| | 14. | Time-Order Primitives, Order Classes, and Order Relations (pp. 102–110) |
| | 15. | Conclusion of the "All-Musical" System (pp. 110–111)[17] |

Table 1 (continued)

| | | |
|---|---|---|
| Part 3/1 | 1. | The Notion of Reference (pp. 23–26) |
| | 2. | Content-Centricity and Order-Determinacy (pp.26–36)[18] |
| | 3. | Musical Systems (pp. 232–233) |
| | 4. | Structural Levels (pp. 233–242) |
| | 5. | Polyphony (pp. 243–249) |
| | 6. | Linearity and Adjacency (pp. 249–252) |
| | 7. | Structural Coherence in "Order" and "Content" music (pp. 253–256) |
| | 8. | Outline of a Tonal-Syntactic System (pp. 257–270)[19] |
| Part 4/1 | 1. | Analysis and Composition (pp. 146–152) |
| | 2. | Analytic Simplicity and Systematic Generality (pp. 152–159) |
| | 3. | Example 1: The Tristan Prelude (pp. 159–217) |
| | 4. | Example 2: Webern: Op. 5, no. 4 (pp. 217–223)[20] |
| 2 | 5. | About Comparison (pp. 156–160) |
| | 6. | Example 3: The First Eighteen Measures of Brahms's Fourth Symphony (pp. 160–166) |
| | 7. | Example 4: Petrouchka: First Scene (pp. 167–175) |
| | 8. | Example 5: Schoenberg: Op. 15, no. 1, mm. 1-7 (pp. 175–188) |
| | 9. | Concluding Remarks (pp. 188–189) |
| | 10. | Compositional Postscript: Group Variations (pp. 189–203.)[21] |

TABLE 2

| Observation Language | Theoretical Language | Metaphysical Language |
|---|---|---|
| A. "simultaneity" | "chord"-"triad"-"tonic triad" | "chord of nature" |
| B. "simultaneity succession" | "harmony," "progression" | "harmonic propulsion" |
| C. "pitch contour" | "span"-"register" "phrase"-structure | "logical form" |
| D. "pitches" | "pitch-class"-"Ab" | "musical sounds" |
| E. "pitch-dyad identity" | "intervals"-"pitch-class interval"-"scale-degree interval"-"interval of simultaneity (consonance) and of succession (dissonance) | "harmonious/ inharmonious sounds"-"dissonance/ consonance" (of "sounds") |
| F. "duration contour" | "rhythmic structure | "rhythmic music" |
| G. "pattern-of-repetition structure" | "Sonata Form"-"Baroque" | "baroque" |

Boretz, "Meta-variations," 1, p. 16

references to organicism and virtually all analogies between music and language.[25]

In Part 2 of *Meta-variations,* Boretz carries out his phenomenalist plan by presenting a constructional model following those of Rudolf Carnap and Nelson Goodman.[26] This model has two components: a basic system of primitive definitions and relationships that are applicable to all music; and a series of what he calls "syntaxes" derived from the basic system, to account for specific repertories and pieces.

The basic system stipulates a set of definitions for progressively more complex theoretical terms. Boretz proposes just three primitive predicates—"x is a pitch," "x is a time point" and "x matches y"—and, in so doing, he also commits himself to the phenomenalistic view that pitch and time-order qualities are the smallest discernible phenomenal units of musical experience.[27] This view leads him to exclude from his system considerations of timbre, dynamics and register. Having fixed his primitives, Boretz then reduces all other terms in the system to them: among others, he defines "pitch function" (or "pitch class"), "interval" and "interval class," "interval equivalence," transposition, complementation and partitioning operators, time spans and time-ordered pitch sets.[28]

Boretz states that his system has three rewards. First, it provides "an ordering of our musical concepts" that shows how they are interdependent and hierarchical in relation.[29] Second, the system allegedly minimizes ambiguity and maximizes, or so he says, the "intelligibility" of musical concepts.[30] Third, and most important, it provides a "set of guidelines to determine, for any conjunction of musical data . . . whether and how that conjunction constitutes a 'musical structure'."[31] Since the model tries to "construct all 'music'", it must account "for everything presently regarded as 'music'" as well as all possible music.[32]

Having established the basic system, Boretz then uses it to construct 'syntaxes' for particular pieces of tonal and non-tonal music.[33] A syntax divides a pitch continuum into "referential" components, such as scales or chord types, and relates it to the ordered sequence of pitches that constitutes the surface of a piece.[34] The referential component is mapped by the syntax onto the surface by different levels of pitch grouping. Taken together, these various levels constitute what Boretz calls the "global interpretation" of the piece.[35]

According to Boretz, these structures "externalize" or describe the "internal" cognitive state of listeners who understand the sequence of pitches presented in a given performance. He claims that we distinguish musical sounds from non-musical noises when we are able to apply some such "internalized system."[36] For Boretz, the value of analysis lies in its ability to guide our experience of music by revealing, for particular pieces, the greatest possible number of relations that each pitch can stand in to every other pitch.[37] The number of these relationships is a measure of the coherence of the piece.[38]

Given Boretz's concern for precise definitions, his use of logical symbols and his explicit reference to D-N, *Meta-variations* certainly has the look of scientific discourse. Indeed, it goes a long way towards providing a rigorous language for music theory. Yet, for all its advances, *Meta-variations* does not succeed in presenting a scientific model of explanation for music theory.

To begin with, phenomenalism has real problems; most philosophers doubted the truth and plausibility of phenomenalism long before the publication of *Meta-variations*.[39] First, the positivists' semantic program, essential to phenomenalist reductions, seemed self-refuting. Second, it proved extremely difficult to purge scientific language of all references to unobservables; ever more theoretical entities seemed to crop up in the reductive process. Third, so-called dispositional terms, so essential to scientific discourse (e.g., soluble or flammable), could be semantically reduced to counter-factual conditionals, but these conditionals resist further positivist reductions. Fourth, phenomenalism carries with it the threat of solipsism and the problems Wittgenstein and others have associated with so-called "private languages." Fifth, realist philosophers as well as historians of science argued that the sharp distinction between theoretical and observational languages was nothing more than an arbitrary segregation of what is at best a continuum. They insisted that the distinction is itself theory-bound; that observational terms are always theory-laden; and that the dominant theoretical paradigm determines what is and isn't observable, and not the reverse.

Similar criticisms can be levelled at Boretz's view. Boretz is far too glib in distinguishing observational from theoretical predicates. In fact, he doesn't heed one of his own quotations from the eminent logician Yehoshua Bar-Hillel:

> . . . most analytical philosophers are today aware . . . that the line of demarcation between theoretical and observational terms is blurred, elastic, and even to a certain extent arbitrary, and will therefore be rather careful with their use of the epithets 'meaningless' 'non-sensical,' or 'unintelligible.'[40]

Boretz consistently exaggerates the dichotomy between theoretical and observational terms and dismisses many sensible terms of contemporary music criticism on the grounds that they are "non-cognitive." The fact is that there is often considerable independent agreement about when supposedly "illicit" expressions do and do not apply. Style concepts, for example, offer a perfectly reasonable way of dividing pieces into aurally distinct classes. Even untrained listeners have little apparent difficulty in distinguishing Baroque style from Medieval or Impressionist styles. If stylistic distinctions can be that readily perceived, then it should be possible to explain them theoretically. After all, if we make such distinctions, there must be some way that we do it. Theory should be able to account for this process. This is not to say, of course, that style classifications will be easily explained, but it does demand that music theorists take such class distinctions seriously and not dismiss them simply as so much loose talk.[41]

Pitfalls also await those who try to reduce music theories to primitive sense-data. Consider the system presented by John Rahn in his essay "Logic, Set Theory, Music Theory." Like Boretz, Rahn tries to define a set of basic musical terms using nothing more than axiomatic set theory "with the addition of two primitive predicates, which are read 'x is a pitch' and 'x is a time' under standard interpretation."[41] But even a cursory look at his system reveals something quite different (see Table 3). In definitions I-IVH, Rahn deals only with the concepts of notes, rests, temporal adjacency, and various species of pitch adjacency.[43] Yet, in order to define these basic terms, he invokes numerous undefined primitives. In order to define pitch adjacency in IVA, he invokes the undefined primitive of interval classes of major, minor and augmented seconds. In order to define diatonic pitch adjacency in IVE, he invokes the undefined primitive of the major scale. In order to define triad adjacencies in IVH, he introduces the undefined primitive of cyclically ordered major and minor pitch-class triads. Some of these undefined primitives may be defined in a more elaborate system indebted to *Meta-variations,* but as it stands, the present system is not fully reductive.[45] On the contrary, it seems conceptually and ontologically inflationary.

Similar anomalies can be found in Boretz's definitions of intervals and pitch function. Boretz defines pitch functions as follows:

> Such entities, then, are named "pitch functions," and are defined as *classes* of pitches, such that all of the members of any one such class are assigned the same value relative to the members of any other such class in interval construction.[45]

and interval as:

> two-place relations among *functionally* qualified pitches.[46]

Those definitions seem not reductive, but circular. Reduction is no mean feat; until we have some compelling motive for seeking a reduction of the language of music analysis to a phenomenalistic basis we ought to avoid this perilous program altogether.

Even if complete reduction were possible, there are other more serious problems with Boretz's system. As mentioned earlier, he tries to construct a system applicable to all music from just three primitives. While virtually all compositions contain sounds with pitch and duration, it happens that non-musical sounds have these properties as well. Thus, the constructional system of *Meta-variations* provides a basis for describing not only all

## TABLE 3

| | | |
|---|---|---|
| I | x is a note | IFF x=<z,<T1, T2>> for some value of z, T1,T2. |
| II | x is a rest | IFF x=<s,<T1, T2>> for some value of T1 and T2. (s is a constant.) |
| III | x and y are | time-adjacent IFF x and y are notes and rests and T2 of x equals T1 of y or T2 of y equals T1 of x. (One note begins where the other leaves off.) |
| IVA | x and y are | pitch-adjacent IFF x and y are notes whose pitches are a minor, major, or augmented second apart. |
| IVB | x and y are | circle of fifths pitch-adjacent IFF x and y are notes whose pitches are a perfect fourth or fifth apart. |
| IVC | x and y are | pitch-adjacent with respect to C IFF C is a cyclic ordering of pitch-classes and x and y are notes whose pitches are less than an octave apart and belong to pitch-classes that are adjacent in C. |
| IVD | x and y are | chromatically adjacent IFF x and y are pitch-adjacent to the chromatic scale. |
| IVE | x and y are | diatonically adjacent IFF x and y are pitch-adjacent with respect to a major scale. |
| IVF | x and y are | extended diatonically adjacent IFF x and y are pitch-adjacent with respect to a major or harmonic minor or melodic minor scale. |
| IVG | x and y are | circle of fiths adjacent IFF x and y are pitch-adjacent with respect to the circle of fifths |
| IVH | x and y | are triad-adjacent IFF x and y are pitch-adjacent with respect to any (cyclic) ordering of a major or minor pitch-class triad. |

Rahn, "Logic, Set Theory, Music Theory," pp. 118–119 pieces of music, but also all sounds, from the police siren outside the window to the bird song overhead. Furthermore, it also excludes pieces whose structures do not depend on non-pitched elements, such as Varese's *Ionizations* (at least up to rehearsal number 13). Boretz's system cannot, ultimately, explain musical phenomena because it does not even delimit the class of all musical compositions from the class of all sounding phenomena. Music theories must at least be based on laws covering "musical" entities.[47]

Besides the problems with Boretz's phenomenalism, there are also significant problems with his scientific method: in Part 1 he misinterprets D-N and in Part 2 he seems to quietly ignore it altogether. Boretz introduces D-N in Part 1 with the schema given earlier.[48] He immediately illustrates it with a formalized explanation of interval equivalence. This example is given as Table 4. Roughly, C1 says that pitches a and b are simultaneous, as are pitches c and d. C2 says that some value t added to pitch a equals c and some value t added to pitch b equals pitch d. L1 says that for any four pitches, if by transposing two of the pitches defining an interval, one produces the interval defined by the remaining pair of pitches, then the two intervals are equivalent. The conclusion or explanandum asserts that the pitch set {a,b} is interval equivalent to the pitch set {c,d}.

Careful consideration of Table 4 reveals some confusions in the formalisms. 1) Premise C1 is superfluous—the conclusion can be deduced just as well without it as with; 2) likewise, the terminology of presented simultaneities, PS(x,y), which we have underlined in Table 4, may also be disregarded because it appears only in C1; 3) C2 is not, strictly speaking, well-formed both because the arithmetic operations of addition and equality are not defined over the domain of pitches and because the term 't' in C2 is completely undefined. 't' looks like what logicians call a "free variable," but if it is so construed, the deduction is rendered invalid; 4) in the statement of L1, 't' appears again, this time not free but "bound" by a symbol that logicians call a "universal quantifier" (the upside-down 'A'). Universal quantifiers allow us to symbolize, roughly, sentences having the logical form "everything in the universe of discourse is such that it has the property F," or, more idiomatically, "everything is F." Quantifiers are meaningless until they are defined over a some domain of entities, but Boretz fails to do this. Are we to construe these quantifiers as defined over a domain of all and only pitches? That will not work, because the universal quantification of 't' in either C2 or L1 entails that everything in the domain is the sort of thing that can be added to one pitch in order to get another. Pitches cannot do that; intervals can, but then not everything in the domain can be an interval either—some things must be pitches. Boretz's introduction of the interval relation I(x,y) would have been useful in this situation, but he fails to take advantage of it.

Even from this very brief discussion, it should be clear that precision,

## TABLE 4

Where a, b, c, d are *pitches,* $P(x)$ = "x is a pitch," $I(x,y)$ = "(x,y) is the *interval* determined by x and y where x and y are pitches," $IE((x,y),(z,t))$ = "(x,y) is *interval-equivalent* to (z,t)," and $PS(x,y)$ = "(x,y) is a *presented simultaneity* where x and y are pitches," then,
if

$$C_1 = (PS(a,b) \wedge PS(c,d)) \text{ and } C_2 = (a + t = c \wedge b + t = d)$$

and, if

$$L_1 = \forall x \forall y \forall z \forall w \forall t\, [P(x) \wedge P(y) \wedge P(z) \wedge P(w) \wedge (x + t = z \wedge y + t = w) \supset IE((x,y),(z,w))]$$

then it follows that

$$IE((a,b), (c,d)).$$

Boretz, "Meta-variations," 1, p. 47.

though it is a virtue of any explanatory enterprise, is not guaranteed by formalism alone. Formalisms add precision to our methods only if we formulate carefully and cogently. If we belabor the point it is only to insist that demanding precision from others comes only at the cost of having to be precise oneself.

A more profound difficulty emerges when we consider whether Boretz's alleged illustration of D-N actually meets the standards demanded by Hempel. On close inspection, it falls short of these standards because it doesn't invoke any significant law-like generalization to explain interval equivalence. Instead, L1 looks like a definition of interval equivalence in terms of the identity of intervals under transposition. If that is the case, the explanandum follows from premise C2 alone and, as Henry Martin rightly notes, Table 4 is a purely deductive inference and is not scientific in character.[49]

The explanatory nature of Table 4 might be salvaged if the relation of interval equivalence is made either a primitive in the system or is defined in some way other than L1. Either alternative would allow us to treat L1 as a law-like generalization correlating an underlying and perhaps non-obvious property of intervals (i.e., identity under transposition) with a logically independent musical state or aural phenomenon called interval equivalence. Unfortunately, Boretz neither provides any such alternative definition for interval equivalence nor does he allow it as a primitive. This effectively rules out L1 as a law-like generalization, and thus rules out the example in Table 4 as an instance of D-N.

Now the problems described so far apply more generally to the constructional system presented in Parts 2 and 3 of *Meta-variations*. Boretz certainly provides a set of general and carefully defined concepts for describing musical events. The precision of these stipulated definitions has the value of making it very clear which structures do and which do not fall under a given description. Nevertheless, for no apparent reason, Boretz ends up dropping D-N in Part 2 and along with it any notion of empiric laws as we have characterized them. He actually creates a formal system with descriptive potential, but no empiric content and no predictive consequences. Ironically, this confusion of merely formal systems with scientific theories is endemic to precisely those who are most widely perceived as endorsing the scientific ideal for music theory. Music theory becomes scientific only when empiric laws are introduced and musical phenomena are subsumed under them in ways that guarantee predictions and testability. Formalism alone does not make some thing scientific any more than a suit makes a man; no amount of formalism can ever transform a description into an explanation.[50]

Boretz, in fact, is forced to abandon the demands of D-N because he supports a view that actually denies the possibility of formulating law-like generalizations about music. To quote Boretz:

The construction of a "model of music" is best understood as the construction of a "model of an individual composition."[51]

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.[52]

We call this view "particularism."[53] Generally, particularism holds 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. Consequently, particularism also holds that a particular work of art can be understood—indeed, should be understood—in complete isolation from the classes and kinds of which it is a member. Other Princeton theorists evidently subscribe to this view:

Babbitt: But, if uniqueness is, as is more than tacitly implied here, a necessary condition for satisfactory "explanation," . . . it is surely not a sufficient condition . . . This provides another boundary (a lower bound, if you will) of [the] *ad hoc*[54].

Rahn: The apotheosis of digital explanation is a fully formalized theory that is not only capable of generating the piece it explains in all its particularity and richness of observable qualities and relationships, but is capable of generating only that piece.[55]

Rahn: Starting from the presumptuous assumption that our interest is primarily focused on particular pieces of music—after all, we never musically listen to anything else—the following statement becomes a useful if controversial characterization: an analytical music theory is a device by which someone communicates his insights about a particular piece of music.[56]

Westergaard: Theorists on the other hand—at least the theorists I know—don't seem to care much about comparing or grouping pieces. They are concerned with

1. understanding the structure of individual pieces and
2. examining the syntactic assumptions we use in understanding such structures.

Both of these concerns are sharply colored by the fact that most of the theorists I'm talking about are, like myself, primarily composers.[57]

Clearly, the particularism of these theorists is antithetical to the primary objective of scientific explanation: the formulation of powerful law-like generalizations about potentially infinite classes of phenomena.[58]

Now, in stressing that scientific explanations presuppose law-like generalization we do not mean to imply that scientists are not interested in individuals; on the contrary, whatever their field, scientists hope to understand and manipulate particular entities and events. Indeed, the measure of a theory's success is the predictions it allows us to make about particular entities and events. What separates the particularist from the scientist is that whereas the former is concerned with understanding the uniqueness of a given work without appealing to facts or laws external to it, the latter is intent on explaining a given object or event only in so far as it is an instance of some general kind. To give a crude example, Newton obviously wanted to understand why a particular detached apple hit his head as hard as it did. But in constructing a theory of gravity, he also wanted to show why, under the same conditions, any other apple detached from any other tree would bean him just as hard. There is simply no way for particularists to be scientific so long as explanations require empiric generalizations.

It may seem odd to find that Boretz endorses particularism given his announced goal of constructing a theory of "all music." How could anything be more general than that? What his system constructs is a set of concepts that have general application to music. But these concepts, far from providing the basis for empiric generalizations about music, are employed by Boretz strictly in the service of individuating particular pieces.

Why did Boretz, Babbitt, Rahn and Westergaard maintain particularism? One answer is that, as composers, they were more intent on constructing compositional maxims for particular new pieces of music than they were in creating empiric generalizations to explain classes of musical phenomena. Or, to be more precise, they conflated the explanatory as opposed to the compositional enterprise. They seemed to hold that their constructional systems not only construct descriptions of music, but that they also create pieces themselves. For example, Boretz admitted that he does "think of music, composition as well as explanation, as actually constituting an 'empirical science' in an important sense."[59] But this view confuses the properties of statements and theories about music with the properties of music itself.[60] Music is no more a language, an explanation or a science than are the moon or an electron; it is statements and generalizations about music, the moon, or electrons that may or may not be scientific and explanatory. While it may be important to devise systems for composing new pieces, we should not confuse this pursuit with the explanation of those pieces.

The phenomenalism of Boretz and his colleagues can also be explained in part by their commitment to particularism. As we have indicated, Boretz defines a set of general concepts that can be used to describe an individual piece in all its uniqueness. But, how are we to evaluate or justify one such description vis-à-vis all others? Clearly, the particularist does not want to say that each one is as correct as the next since alternative readings may be incompatible with one another and some readings may be just wierd. Nor can a reasonable particularist insist that his analytical descriptions refer to structures in the piece that are somehow inaudible; if they were inaudible, how could he justify the claim that they occur? To avoid generalization, particularists must ultimately claim that their analyses are verified by what can be heard in the piece, hence their emphasis on the direct hearability of analytical structures. Now there are two ways to secure the hearability of an analysis: either we can reduce all analytical claims to statements about uncontroversially phenomenal entities, or we can insist that all analytical structures must be directly hearable, even if perhaps only by some highly trained listeners. The former approach to particularism is the one adopted by Boretz, Babbitt, Rahn and Westergaard; the latter is the one endorsed by many critics of scientific analysis, the naive realists, whom we shall consider in the following section.

## III

In Part II we saw that Boretz attempted to take important steps towards creating scientific music theories, and, though he may be criticized for his complexity and unfamiliar formality, he certainly imposed a new rigor on the language of the discipline. Nevertheless, we have also seen that he and some other Princeton theorists underestimated the cost of scientific paradigms—they did not realize that in accepting scientific models, they must also give up the possibility of understanding pieces as essentially unique entities. Rather than give up their concern for uniqueness, they remained phenomenalistically minded particularists and, in so doing, they relinquished the possibility of having general laws of music.

While Boretz *et al.*, failed to appreciate the essential contradiction between forming general laws of music and explaining the uniqueness of individual compositions, critics of scientific music theory, such as Edward Cone, Joseph Kerman and, at times, Leonard B. Meyer, have consistently insisted that the two views are incompatible. As particularists, they invoke general concepts, such as style, harmonic and formal conventions—but they minimize the significance of general laws. They recognize that music theories cannot achieve the explanatory standards of the sciences because they cannot be tested objectively and because they cannot clarify the most important aesthetic features of particular works. Furthermore, although they repeatedly reject phenomenalism, they still maintain, like Boretz, that analytical statements must refer only to what is directly observable. Instead, they endorse a naive realism which claims that even the most complex analytical hypotheses about a piece of music are directly verified or falsified by what can be heard in that piece, albeit by properly qualified auditors.

The following list presents a cross section of particularist statements by Cone, Meyer and Kerman.

Cone: The trap that caught Cone is one that is a danger to all music theorists—one, indeed, that has caught all of us at one time or another: the snare of generalization. Every work of art is unique; its value depends on that fact. No classification, no codification, no general theory of any kind can do justice to that uniqueness.[61]

Meyer: Critical analysis seeks to understand and explain what is idiosyncratic about a particular composition: how is this piece different from all other pieces—even those in the same style and of the same genre? It is concerned with the implications of this specific motive or process, the function and structure of the specific harmonic progression, the relationship between this slow introduction and the Allegro which follows it, the reason why there is a *sforzando* on this note or why this theme is interrupted at this particular point. In short, criticism tries to discover *the secret of the singular*.[62]

Meyer: Whatever the reasons for such disagreements [between analyses], they should encourage, rather than discourage, critical analysis, not only because the task itself is challenging and fascinating, but because there is no escape. For our devotion to music ultimately stems from our delight in, and love for, particular compositions. And everything we do—all of our study and research—seeks in the end to illuminate as fully as possible the sources and basis of their power to engage and entrance us.[63]

Kerman: The most stubborn problem of all is rooted in Schenker's idealism, in his determination to seek the essence of all tonal music in an invariable abstract formula rather than in its infinite, concrete, magnificent variety.[64]

As they stand, these comments are remarkable in and of themselves; however, they seem even more striking when compared with the quotations from Boretz, Babbitt, Rahn and Westergaard cited in Part II. Indeed, it is ironic that the proponents of scientific music theory should share so crucial a view with their critics. To understand how this situation arises, it is useful to consider the various arguments against scientific music theory. From the outset, many particularists claim that since analyses are always heavily theory-laden, they provide only circular confirmation, thus no confirmation at all, of the theories that govern them. Narmour and Taruskin, for example, suggest that Schenkerian theory and Forte's set theory are both circular.[65] According to Narmour, Schenkerian theory is invalid because it allegedly shows that all tonal pieces can be reduced by a set of twelve prolongations to one of three background structures. But, to reduce a given piece, the Schenkerian must know in advance what the background structures and prolongations are—hence the circularity.

After criticizing Forte's theory as self-confirming, Taruskin proposes that, to break the circle, one needs to find external corroboration for the analysis. Along with sketch studies, this corrobration might include:

> a thorough investigation of style . . . and of the composer's theoretical environment—his training, the theory books he knew, his ways of looking at his own music . . . the music he heard, loved, hated, the books he read, etc."[66]

A second criticism offered against scientific models of music theory is that the types of phenomena musicians theorize about are quite different from those scientists theorize about. Scientists, such as physicists, deal, they say, with stable, regular, unchanging reality; after all, the physical world holds still long enough to get a handle on it. This apparently is not so for music theorists. They speculate about repertories that are anything but fixed and styles that are perpetually shifting in response to a wide variety of personal, historical and cultural forces. Kerman makes this point in the following attack on Schenkerian theory:

> Schenker was ready to strip away not only salient details of individual compositions but also distinctions between compositions composers, and periods . . . the most baffling and irritating aspect of Schenker's thinking is his view of music history as an absolutely flat plateau flanked by bottomless chasms.[67]

Other critics denounce scientific analysis on the grounds that music theories cannot yield interesting predictions. In the words of Henry Martin:

> Acoustics . . . will have to display inductive-empirical laws (because acoustics is a natural science), but such laws for the kinds of "things" dealt with in analytical discourse are not available. Thus, we cannot make inductive predictions about pieces as "confidently" as we can predict the behavior of many scientific systems.[69]

There are three main arguments against the possibility of prediction in music theory. First, critics claim that theorists are in the business of explaining music that is either unique or part of a historically distinct repertory. Since, in both cases, the relevant class is by definition finite, the properties of the members cannot be universalizable and the generalizations over that class can have no predictive value. Second, some critics insist that because human hearing is an extremely plastic faculty, there can be no predictable way in which a piece is heard from listener to listener or from hearing to hearing. As Gossett puts it:

Our understanding of a work of art is constantly in flux. Each analysis will focus on different aspects or qualities, and none can hope to 'explain' exhaustively even a relatively simple work."[69]

In short, the theorist can never step into the "main stream" of music twice. Given this plasticity, some propose that analysis must dictate and not predict how listeners hear a work. Obviously, any theory that prescribes responses cannot really be predictive. Third, other critics assert that if music theory had any real predictive power, then it should predict what particular notes will appear at any given point in a piece. Since music theory falls short of this goal, it does not seem to be truly predictive or scientific.

Up to a point, these comments are well taken; we certainly accept that existing music theories seldom meet the explanatory standards sought by science. Yet, we believe not only that Cone, Kerman, *et al.*, seriously overestimate the costs of scientific paradigms, but also that these costs are considerably less than those of particularism in any of its guises.

We certainly agree with Taruskin that most current analytical methods smack of circularity and that to break the circle, we need external corroboration. But, we reject Taruskin's claim that only considerations of style, historical background or sketches can provide such corroboration. Clearly the composer can be ignorant or confused about the underlying structure of his compositions. Why should we accept Rimsky-Korsakov's view of octatonicism for Stravinsky's music when it hinges on the idea that the properties of harmonic systems depend on scale type?[70] The idea that the properties of harmonic systems depend on those of scale type is a dubious one at best, and it is demonstrably false for tonal music. After all, we know scale membership is neither a necessary nor sufficient as a means for determining the tonality of a piece since we can establish a tonic without using all scale tones (e.g., the progressions I-V-I will do) or by reinforcing such a progression with chromatic (non-scale) tones (e.g., I-♭II6-V-I). The fact remains that theories have changed and advanced with time; what passed for an explanation in earlier ages often does not and should not pass for an explanation today. We should not flatly equate what a correct analysis might be with what previous generations thought it was.

The snag with sketch arguments is that they lead down the thorny—if not impassable—road to the intentional fallacy.[71] Unfortunately, the way in which we interpret sketches will always depend on our prior understanding of the piece. Even if we could reach some agreement about what is important in the sketches, it is doubtful whether they can provide the sorts of neutral tests that Taruskin claims. We are not, of course, saying that people should not study a composer's background or his sketches—such studies may indeed suggest important insights about the way works are put together. What we are saying is that there is no reason to suppose that they provide non-circular tests for theoretical claims.

A far better way to avoid circularities is to invoke independent laws to support the analysis in question. Let us illustrate what we mean. Narmour points out two ways in which Schenkerians counter the charge of circularity: some use the 'Chord of Nature' argument and others claim that background structures are primitives in an axiomatic system.[72]

Like Narmour, we reject the first solution; but, unlike him, we are prepared to back the second alternative with some important qualifications. In the first case, the 'Chord of Nature' argument presumes that the properties of a single note should determine those of an entire work, but we see no compelling reason to make this assumption. This is as plausible as believing that the structure of a novel should model the structure of a single sentence, word or phoneme![73] In the second case, although we endorse a model of tonal theory similar to that proposed by Michael Kassler, we believe that the empiric foundation and adequacy of this model need to be clarified more precisely.[74]

Table 5 presents our revised version of tonal theory. Here we suggest that Schenker's three background structures (Table 5a) and transformational rules (Table 5b) articulate six laws of tonal motion (Table 5c).[75] In fact, the background structures can be understood as axioms that exemplify the six informal laws in an optimally compact way; the twelve transformational rules elaborate and diversify these laws.[76] The system as a whole—the background structures and transformational rules—can thus be regarded as a precise reformulation of the intuitive generalizations expressed in Table 5c. The theory claims that any piece of music is tonal if and only if it is derivable within the system. In particular, the theory makes three main predictions: first, that all and only tonal pieces are derivable; second, that not all musical surfaces are derivable; and third, that the characteristics of a given tonal surface depend on the types of transformation used and the order in which they are generated, as specified by a series of structural levels. Of course, the theory assumes some independent and perhaps intuitive criterion of tonality. Furthermore, the theory acknowledges the probability of borderline or controversial predictions that may not be verifiable.

This latter point is important since it suggests that the theory might show why it is that certain works are controversial or ambiguous. Indeed, by studying the behavior of specific sequences of transformations, it might even be possible to measure the degree to which different pieces are tonal. Schenker himself intimates as much in the *Harmonielehre* and *Kontrapunkt 1* when he claims that modal and exotic phenomena can actually be explained by tonal operations.[77] Obviously, the implications of such provocative claims need to be studied carefully in the future.

We also agree that highly changeable cultural phenomena pose special difficulties for theorists, but we believe that such problems are more practical than theoretical. Some cultural phenomena may have so few actual

## TABLE 5a

a. Background Structures

| | | | | | | | | |
|---|---|---|---|---|---|---|---|---|
| 1. | $\hat{3}$ | | | | | | $\hat{2}$ | $\hat{1}$ |
| | I | | | | | | V | I |
| 2. | $\hat{5}$ | | | | $\hat{4}$ | $\hat{3}$ | $\hat{2}$ | $\hat{1}$ |
| | I | | | | | | V | I |
| 3. | $\hat{8}$ | $\hat{7}$ | $\hat{6}$ | $\hat{5}$ | $\hat{4}$ | $\hat{3}$ | $\hat{2}$ | $\hat{1}$ |
| | I | | | | | | V | I |

For each key, major and minor (total = 72)

TABLE 5b

b. Transformations

| Transformations | Domain | Schenker's Discussion |
|---|---|---|
| Repetition | Single State<br>Single Line | None |
| Arpeggiation<br>(*Brechung*) | Single State<br>Single Line | *DFS* par. 125–128,<br>par. 230 |
| Neighbor Motion<br>(*Nebennote*) | Multiple States<br>Single Line | *DFS* par. 106–112,<br>par. 196–202 |
| Linear Progression<br>(*Zug*) | Multiple States<br>Single Line | *DFS* par. 113–124,<br>par. 203–229 |
| Displacement | Multiple States<br>Single Line | None |
| Registral Transfer<br>(*Hohelegung,*<br>*Tieferlegung,*<br>*Koppelung*) | Multiple States<br>Single Line | *DFS* par. 147–154,<br>par. 238–241 |
| Unfolding<br>(*Ausfaltung*) | Multiple States<br>Multiple Lines | *DFS* par. 140–144,<br>par. 234 |
| Motion from an<br>(*Untergreifen*) | Multiple States<br>Multiple Lines | *DFS* par. 135–139,<br>par. 233 |
| Voice-Exchange<br>(*Stimmtausch*) | Multiple States<br>Multiple Lines | *DFS* par. 236–237 |
| Reaching over<br>(*Uebergreifen*) | Multiple States<br>Multiple Lines | *DFS* par. 129–134,<br>par. 231–232 |
| Mixture<br>(*Mischung,*<br>*Phrygische II*) | Single State<br>Multiple Lines | *DFS* par. 102–105,<br>par. 193–195<br>*Harmonielehre* par.<br>38–52 |
| Tonicization<br>(*Tonikalisierung*) | Single State<br>Multiple Lines | *Harmonielehre* par.<br>136–162 |

TABLE 5c

c. Six General Laws of Tonality

1. Tonal harmonies are fundamentally triadic.

2. These triads are related hierarchically to a principle triad, the tonic (I). The strongest hierarchi relationship is between the tonic triad (I) and the dominant (V).

3. Chromaticisms are generates by two processes—mixture and tonicization.

4. Tonal lines achieve maximum closure when they arrive on the tonic pitch. Melodic lines achieve maximum closure when they descend diatonically by step to $\hat{1}$; whereas bass lines achieve maximum closure when they leap from V to I.

5. There are no parallel octaves or fifths between successive chords.

6. There is an absolute distinction between consonance and dissonance; consonances originate in major and minor triads; dissonances arise from motion between consonances.

cases that the theorist has a tiny sample from which to generalize or against which to test hypotheses. This makes the theorist's job hard, but not impossible. Consider, for example, two different views of Schenkerian theory.[78] If, as many claim, it is a theory of value, then Schenker's original sample would seem to be too small to provide a reliable support for constructing and testing the theory. It is, for example, hard to justify including C. P. E. Bach in the canon of great composers at the expense, for example, of Palestrina, Monteverdi or Stravinsky. However, if we treat the theory as a theory of tonality, then Schenker's sample seems more appropriate. After all, the composers discussed by Schenker all wrote quintessentially tonal music whereas Palestrina, Monteverdi and Stravinsky did not. Once we accept Schenkerian theory as a theory of tonality, then we are obliged to test it for all types of tonal music (past, present and future), and not simply the masterworks of the 18th and 19th centuries.

This latter point leads us to the topic of predictive power. First, those who argue that generalizations about music cannot be universalized beyond the unique or finite historical class of compositions fail to understand the demand for universality. Suppose we notice some distinctive aesthetic qualities in the works of a dead composer. The total body of his works is terminally finite. Nonetheless, we can make universalizable generalizations about this corpus not in order to explain why he composed the way he did, but rather to explain why compositions of the kind that he alone created exhibit the properties that they do. The composer's output may be finite, but it can serve as a pretext for delimiting and generalizing about a potentially infinite class of pieces with similar musical characteristics. These generlizations could then conceivably help to authenticate works that are anonymous, wrongly attributed or newly exhumed.

Second, those who argue that music analyses prescribe rather than predict how listeners hear are partly correct. But, how are analytical prescriptions justified? Should a naive listener simply place faith in the authority of some particular analyst? If so, which one? Sooner or later, prescriptions are only as believable as they are rationally justified. If this justification is rational, then appeals to authority are unnecessary.

Third, no science is capable of predicting phenomena with the precision that some critics would demand of music theory. Many claim that music theories should predict note for note what will appear in a given piece of music. But science cannot predict such singular individuals in this way. If we collide particles at high velocity in an accelerator, we can predict that certain types of subatomic particles will be formed, but we cannot say which of billions of possible individuals it will be. Since we can only explain classes of individuals and not individual events *per se,* all we can say is that, under a given set of conditions, some type of object or event will behave in some given way. Music theory can make predictions of this very general sort and should not be required to do more.

Finally, we see no real evidence to show that scientific methods dull the sensitivity of the analyst or that they neces-sarily ignore the most important aesthetic features of a given piece. Henry Martin, among others, has suggested that "the score "incompletely represents" the music by "accounting for" a fraction of the complexity of the intended sound object."[79] Now, while few people would deny that there are qualities in art that cannot at present be explained scientifically, we see no reason, to suppose that unsystematic or metaphorical approaches have any better chance of illuminating them than systematic ones. Like the philosophers Viktor Kraft, C. I. Lewis and Monroe Beardsley, we accept the claim that aesthetic objects can in principle be studied systematically.[81]

What should be clear from our replies is that we see good reasons for seeking out and admitting general laws of music, and little hope for particularism. Take, for example, Cone's attacks on generalization. As is clear from "Analysis Today," Cone believes not only that analysis should aim to explain the uniqueness of given pieces, but also that good pieces determine their own means of analysis. Particularists might also assume that listeners can analyze the same piece in different but complementary ways. Taken to their logical conclusion, these claims imply that there will be as many methods as there are compositions and listeners. However, this conclusion is clearly problematic. Obviously, the number of valid analyses must be smaller than the number of pieces or listeners; even particularists such as Cone will want to restrict the number of possibilities to rule out empirically false or self-contradictory readings. But what criteria can they invoke? Many draw on that mystical concept "musicality." They believe that good analyses are "musical" and bad ones are not. The problem with this view is that there is no reason why we should place the same values on statements about music that we do on music itself. By this token, explanations of electro-magnetism ought to be electrifying! Music analyses are not compositions and should not be judged as such. A more reasonable way to limit the number of analyses is to invoke criteria outside the piece in question. This evidence will eventually force the particularist to admit generalization and hence the possibility of scientific analysis. We will discuss these difficulties in the following section.

## IV

As we have framed it, the issue of whether it is possible to have scientific explanations in music theory boils down to a single question: can law-like generalizations be formulated for musical phenomena? Answering "yes" to this question comes at the cost of abandoning all hopes of ever understanding musical compositions as essentially unique things. In Parts I-II, we suggested that, because of their particularism, Princeton theorists such as Boretz and Rahn failed to appreciate this cost. Critics of scientific music theory are more sensitive to the irreconcilable conflict between science and particularism; nevertheless, in Part III we also argued that their criticisms of the scientific image of music are largely without merit. We see no limits, in principle, to applying scientific methods to music in aesthetically fruitful ways. We have tried to suggest, first, that the costs of science may not be quite as severe as they might initially seem; and second, that the costs of particularism are considerably greater than its adherents suppose. In the final part of this paper we want to explore these two points in more detail: on the one hand, we will explain our criticisms of particularism in greater depth; on the other, we will propose refinements to the model of scientific explanation that should make it more palatable to music theorists.

As mentioned earlier, the particularist wants to approach individual pieces of music as unique entities and, by subjecting them to sensitive, perhaps terminologically rigorous and presumably unbiassed analyses, he or she hopes to reveal the rich, hidden structure that lies beneath the "surface" of each one. According to this view, the analyst is a sophisticated critic, a refined instrument of aesthetic judgement whose task is to communicate subtleties that may not be immediately discernible to the ordinary listener.

Yet, though particularists focus on the uniqueness of each work, they invariably employ a set of general concepts and methods in order to analyze and describe pieces. These concepts and methods are normally sufficiently general so that they might be applied to diverse pieces and repertories. Nevertheless, the the particularist must convince him or herself that only the individual piece determines which, if any, analytical system is appropriate for its analysis. In other words, the particularist claims that the choice of method is completely *ad hoc* as determined by the particular piece.

Now this position poses some interesting problems. First, why should we suppose that there will be any possibility of generalizing accurately across pieces and repertories if we are particularists? Why don't particularists find any *method* of analysis, any *conceptualization*, or any *description* employing general terms, an abuse of the individuality of a work? Would not a more thorough-going particularist recommend that we approach a particular piece with a wholly open-minded, but also, therefore, an essentially silent and inarticulate appreciation?

Second, the particularist may very plausibly argue that while analyzing a given piece the reliance on widely applicable analytical concepts does not necessarily imply the formulation of law-like generalizations. Indeed, any single piece may invoke a unique set of analytical concepts, even though each concept may be widely employed. For instance, many pieces are in the key of F major, but only one of them might begin in the dominant, modulate to the mediant, and contain hidden motivic repetitions of the gesture F-G-A-F-E-F in its final section. By invoking ever more specific systems of classification, such an approach may in fact preserve many particularist goals while deflecting the charge of inarticulate open-mindedness.[82] Furthermore, though its aims are not scientific, this approach may not be incompatible with a scientific view of music theory.

Nonetheless, despite its appeal, such a view is essentially descriptive: it simply provides us with musically interesting descriptions of individual pieces of music. We have no objections to this enterprise beyond pointing out that, no matter how particular, such analyses explain nothing—they surely do not explain some essential uniqueness of a given piece, and they fail to contrast analysis with "mere descriptions" such as strings of chord classifications.

Third, if the particularist responds to these challenges by claiming that, just coincidentally (perhaps miraculously), lots of pieces have similar underlying properties—thereby explaining the general applicablity of the methods and concepts—then he or she has taken the first step onto a slippery slope toward law-like generalizations. If our musical analyses correlate quite generally two or more distinct musical properties, then, in principle, there is no obstacle to formulating general theories which cover that correlation.

Fourth, if particularists concede all this while holding out that some characteristic of a composition are beyond generalization, then they owe us some good reason for supporting this view. Arguments to this effect have been proposed in other disciplines, but they have proved to be largely unconvincing.[83]

Fifth, and most important, particularism inevitably leads to a troubling form of connoisseurism in music theory. If a particularists want to confirm his analytical statements about a composition while refraining from external considerations, then the credibility of their reading stands or falls according to what evidence can be found within the confines of the given piece. Deprived of external corroboration, particularists come under strong pressure to justify their claims by what can be directly heard in the piece. This position seems fair enough—if an analysis alleges that some composition contains an underlying structure (process, state, function, etc.) and that structure is directly perceptible to all qualified listeners, then we have very good reason to believe that the analysis is true.

The problem is that the hypothetical structures invoked by theorists are not always directly hearable even by highly trained musicians, much less by the so-called 'musically illiterate.' In spite of this lack of perceptual corroboration, theorists often insist, quite rightly, that these analyses provide accurate descriptions of the piece in question. Now the only way we can see to justify such a conviction, within the limits of particularism, is to privilege the aural sensibility and musical judgement of the analyst over that of other listeners. The particularist must argue that the skilled analyst, with his extraordinarily refined faculties, can detect musical structures and relationships that the average listener can hear only dimly, if at all. According to this view, the skilled analyst must be understood as a connoisseur of musicality.

Such a line of reasoning leads inevitably to all the problems associated with 18th-century theories of aesthetic taste. Only the most obvious of these is the following: how can we be sure that the judgements of one self-professed connoisseur are more trustworthy than those of another? Of course, we can have faith in a connoisseur's insights and intuitions, and if we are capable of hearing some pieces the way he or she does, then we may believe our faith is well placed. But, if our intuitions diverge, or if we have a choice among incompatible but equally authoritative analytical judgements, what impartial grounds are available for deciding among them? We think that the particularist can only retreat to circular or dogmatic justifications, or to some form of pluralism.

The model of explanation that we are proposing for music theory avoids these problems with particularism. First, and most importantly, we reject the particularist expectation (whether of the naive realist or phenomenalist) that analytical statements should refer only to entities that are in some sense directly hearable. We believe that the theoretical structures, events and processes referred to in all music analyses need not be directly hearable by anyone, not even the most refined and sensitive analyst.[84] All that is required is that sooner or later, directly or indirectly, the theoretical entities postulated in analyses contribute to an explanation of musical events that are hearable. In other words, we understand analyses as attempts to explain the aural intuitions of listeners, but those listeners need not be able to hear everything postulated in the explanation. There is no problem on this view if it should turn out that some highly trained listeners can hear what the illiterate do not; our analyses can conceivably account for all of these intuitions, both refined and illiterate, without having to privelege the judgements of a few. This model also allows us to arbitrate between the analyses of experts by measuring the explanatory adequacy of each analysis against the total body of perceptual data, both expert and inexpert.

Second, though we reject positivist commitments to phenomenalism, we endorse the basic demands of Hempels's D-N model of explanation. However, in the years since Hempel first outlined D-N, many philosophers of science have maintained that it idealizes the logic of scientific inference in a way that is rarely satisfied by actual scientific research programs. Thus, our notions of law-like generalization as well as the structure of D-N must be significantly altered to accommodate these criticisms.

One aspect of D-N that needs revision is the condition of universalizability. Clearly, any generalization framed in terms of probabilities cannot, strictly speaking, satisfy this requirement. But such generalizations are unavoidable in many scientific explanations. Music theorists may also have to resort to statistical generalizations that fall short of strict universalizability.

Once we allow statistical generalizations in place of universalizable ones, then we are forced to revise the logic of D-N. Without some probability calculus, we can only deduce the explanandum from the explanans if the generalizations are universal. So D-N simply will not work as a model for statistical inference. But philosophers and logicians have proposed many models of probablistic inference, and we see no special reason to doubt that these models can be extended to music theory.[85] An obvious extension of our proposals would be an inquiry into the utility of statistical inference in music theory.

Our concept of law-like generalization should also be sensitive to current debates between realist and anti-realist philosophers of science. Realists hold that empirically adequate scientific laws and theories (i.e., those strongly confirmed by observation) must also be true or approximately true. Indeed, they often argue that the empirical adequacy of theories can only be explained by presuming the truth of those theories; after all, how else can we account for the predictive and pragmatic utility of such theories?[86] This may seem a trivial and unproblematic position until one recognizes that it has some profound implications for music theory.

To say that an acceptable theory must be approximately true, in addition to being empirically adequate, requires that the non-obvious entities, states and processes referred to by the theory must actually exist. In other words, unlike the positivists, we cannot simply dismiss references to such things as theoretical fictions that help us predict sense experiences in given test situations. Thus, if music theorists are to be scientific realists, the underlying or non-obvious structures in music must be considered "real" entities or states. The problem here is that it is not the least bit clear what sort of entity a middleground, or an implied dominant, or a nexus set might conceivably be. If we explain the so-called surface of musical compositions by appealing to such non-obvious structures, and if we confess that these are real things, then we might well ask *what* and *where* they are. If music theorists adopt a realist picture of scientific explanation, then they may feel uneasy about the prospects of responding sensibly to such questions.

Yet all is not lost. We can maintain the realist image of scientific explanation without abandoning that image of music theory. Realism puts a price on membership to the sciences—music theorists must propose a plausible ontology to serve as the referents for theoretical terms. But that is not a hopeless enterprise; we need not consider the theoretical entities of music theory as unavoidably abstract and beyond reification. Realism only requires that there is some real stuff doing the things we say, but this stuff need not be very familiar. After all, particle physicists also seem to describe real stuff, if anyone does, but its hard to imagine a field committed to a more charmingly strange ontology.

One can see quite trivially that the reality described by music theory, if anything at all, is going to be located either inside or outside the head of the listener—either in psychological or in acoustic realities, or in some combination of the two. In any event, this issue of realism in music theory will be settled, if at all, not by philosophers and musicologists but by research in psycho-acoustics and the cognition of music.

The alternative to scientific realism is some form of anti-realism. The anti-realists, a mixed group, deny the realist contention that acceptable scientific theories must be true in addition to being empirically adequate. A latter-day empiricist, such as Bas van Frassen, does not accept the positivist claim that theoretical entities are purely fictions, but he does insist that so long as a theory is empirically adequate, its truth is unimportant to its pragmatic explanatory value.[87] Thus, even if psycho-acoustics does not reveal any psychological or acoustic reality corresponding say, to a middleground or a nexus set, this would not constitute, on van Frassens' view, sufficient reason for discarding these concepts. In some ways, van Frassen's anti-realist interpretation of scientific explanation is far more congenial to music theory because it suspends the demand that music theory be supported by a plausible ontology. We, ourselves, cannot agree on the realist/anti-realist debate, but we do agree that either view leaves room for scientific explanation in music theory.

Finally, if it is taken seriously, our proposal does not only reorient music theory philosophically, but it also has practical consequences. By endorsing a scientific method for music theory, we are encouraging music theorists to be clear—perhaps clearer than they have been—about answering certain fundamental questions about their research: 1) What range of musical phenomena are being explained by some theory? 2) What generalizations or theories are being invoked to explain those phenomena? 3) What predictions does the theory make that will allow us to test the theory? 4) What guarantees the unbiased selection of test samples? 5) What theoretical states, structures, entities, or events are postulated by the theory? and is there good reason to believe that such things actually exist? We do not mean to say that analyses never answer these questions. In fact, we believe that many analyses are persuasive for the very reason that they do, at least tacitly, often satisfy these demands. Many music theorists, perhaps without even knowing it, often achieve scientifically respectable explanations.

We began our paper with a dilemma. We suggested that, on the one hand, music theory aspires to an objective understanding of musical phenomena and eschews extravangant and loose talk. On the other hand, theorists want to contemplate the so-called "masterpieces" of music, those consummate acts of "genius," by probing their infinitely rich detail and organization. These ambitions can work at cross purposes, the one encouraging and the other deploring the search for musical generalities and regularities. In our paper, we have not resolved that dilemma so much as we have heightened the contrast in order to reveal the benefits and costs of each view. Our solution, insofar as we have offered one, is simply to point out that one horn of the dilemma though not painless, is not so harmful as some music theorists might formerly have thought.

## NOTES

1. This paper was originally presented at the Tenth Annual Meeting of the Society for Music Theory, Rochester; Fall, 1987 under the title "The Nature of Explanation in Music Theory." We would like to thank the University of Rochester, Bridging Fellowship for support, Prof. Robert Morris and Jennifer Williams Brown for numerous suggestions.
2. The relevance of scientific models to other branches of the humanities has been widely discussed. Among others, see R. F. Atkinson, *Knowledge and Explanation in History: An Introduction to the Philosophy of History* (Ithaca: Cornell University Press, 1978); Monroe Beardsley, *The Possibility of Criticism* (Detroit: Wayne State University Press, 1970), "The Role of Psychological Explanation in Aesthetics," in *Perceiving Artworks*, ed., John Fisher (Philadelphia: Temple University Press, 1980), pp. 185–212; Donald Davidson, "Hempel on Explaining Action," in *Essays on Actions and Events* (Oxford: Oxford University Press, 1980), pp. 261–275; William Dray, *Laws and Explanation in History* (Oxford: Oxford University Press, 1957); Stanley Fish, *Is There a Text in This Class* (Cambridge: Harvard University Press, 1980); Nelson Goodman, *Languages of Art* (New York: Bobbs-Merrill, 1968), Nelson Goodman and Catherine Z. Elgin, *Preconceptions in Philosophy and Other Arts and Sciences* (Cambridge: Hackett Publishing, 1988); Stuart Hampshire, "Logic and Appreciation," in *Art and Philosophy*, ed., W. E. Kennick (New York: St. Martin's Press, 1979), pp. 651–657; Arnold Idenberg, "Critical Communication," *Philosophical Review* 58 (1949), pp. 330–344; Joseph Margolis, "Robust Relativism," *Journal of Aesthetics and Art Criticism* 35 (1976), pp. 37–46; Richard Miller, *Fact and Method* (Princeton: Princeton University Press, 1987); Karl Popper, *The Poverty of Historicism* (London: Routledge and Kegan Paul, 1957); Eddy Zemach, "Aesthetic Properties, Aesthetic Laws, and Aesthetic Principles," *Journal of Aesthetics and Art Criticism* 46 (1987), pp. 67–74; Paul Ziff, "Reasons in Art Criticism," in *Philosophy and Education* ed., I. Scheffler (Boston: Allyn and Bacon, 1958), pp. 219–230.
3. Aristoxenus outlines his view of an empiric music theory most clearly in the *Harmonics* Book 2, sections 43–44. See R. Da Rios ed., *Aristoxeni elementa harmonica* (Rome: Typis Publicae Polygraphicae, 1954) and Henry S. Macran ed., and trans., *The Harmonics of Aristoxenus* (Oxford: Oxford University Press, 1902.) See also Milton Babbitt, "Contemporary Music Composition and Music Theory as Contemporary Intellectual History," in Barry S. Brook *et al.*, *Perspectives in Musicology* (New York: Norton, 1972), pp. 151–184; "Past and Present Concepts of the Nature and Limits of Music," Benjamin Boretz and Edward Cone ed., *Perspectives on Contemporary Music Theory* (New York: Norton, 1972), pp. 3–9; Michael Kassler, "A Sketch of the Use of Formalized Languages for the Assertion of Music," *Perspectives of New Music* 1/2 (1963), pp. 83–94; "A Trinity of Essays," (Ph.D. dissertation., Princeton University, 1968); Benjamin Boretz, "Meta-variations: Studies in the Foundations of Musical Thought," *Perspectives of New Music* Part 1, 8/1 (1969), pp. 1–74, Part 2, 8/2 (1970), pp. 49–111; Part 3/1, 9/1 (1970), pp. 23–42; Part 3/2, 9/2 and 10/1 (1971), pp. 232–270; Part 4/1, 11/1 (1972), pp. 146–223; Part 4/2, 11/2 (1973), pp. 156–203. John Rahn, "Lines (Of and about Music)," (Ph.D. dissertation, Princeton University, 1974); "Aspects of Musical Explanation," *Perspectives of New*

*Music* 17 (1979), 204–224; "Logic, Set Theory, Music Theory," *College Music Symposium* (1979), pp. 114–127.

4. Milton Babbitt, "Past and Present Concepts," p. 3.
5. For a general account of Positivism see A. J. Ayer ed., *Logical Positivism* (New York: The Free Press, 1959.)
6. See Carl Hempel, *Aspects of Scientific Explanation and other Essay in the Philosophy of Science* (New York: The Free Press, 1965) and *Philosophy of Natural Science* (Englewood Cliffs, New Jersey: Prentice-Hall, Inc., 1966.)
7. For example, Rudolf Carnap, "Testability and Meaning," *Philosophy of Science* 3 (1936) and 4 (1937). Reprinted in *Readings in Philosophy of Science* ed., Herbert Feigl and May Brodbeck (New York: Appleton-Century-Crofts, 1953).
8. For an extensive discussion of the differences between description and explanation, see Wesley C. Salmon, *Scientific Explanation and the Causal Structure of the World* (Princeton: Princeton University Press, 1984), pp. 3–20.
9. This term is used by William Dray, *Laws and Explanation in History* (Oxford: Oxford Univiversity Press, 1957). Hempel himself does not use it, see *Aspects of Scientific Explanation*, pp. 345–346.
10. For general discussions of the problems of dealing with laws, see Hempel, *Aspects of Scientific Explanation*, pp. 354–364, 376–380 and D. M. Armstrong, *What is a Law of Nature?* (Cambridge: Cambridge University Press, 1983).
11. This term was introduced by Nelson Goodman in his essay "The Problem of Counterfactual Conditionals," in *Fact, Fiction, and Forecast* (Cambridge: Harvard University Press, 1954), p. 22.
12. Hempel notes: "A law-like sentence must not be logically limited to a finite number of instances; it must not be equivalent to a finite conjunction of singular sentences, . . . it must be of essentially generalizable form." *Aspects of Scientific Explanation*, p. 340.
13. For a discussion of the terms "retrodiction" and "post-diction" see Hempel, *Aspects of Scientific Explanation*, pp. 173–174 and 303–304.
14. See for example, A. J. Ayer, "Demonstration of the Impossibility of Metaphysics," *Mind* 63 (1934), pp. 335–345.
15. Table 1 is taken from the published version of *Meta-variations* and not from Boretz's thesis (Ph.D. dissertation, Princeton University, 1970). So far as we can tell, the main difference between the two versions is that Boretz does not publish the list of formal terms given Part II.12 and the elaborate formal appendices from Part II.13 and 14.
16. *Perspective of New Music*, 8/1 (1969.)
17. *Perspectives of New Music*, 8/2 (1970)
18. *Perspectives of New Music*, 9/1 (1970)
19. *Perspectives of New Music*, 9/2 and 10/1 (1971)
20. *Perspectives of New Music*, 11/2 (1972)
21. *Perspectives of New Music*, 11/1 (1973)
22. Boretz, "Meta-variations," I, p. 16. For a similar Table see Carnap, *The Logical Syntax of Language* (London: Kegan Paul, Trench, Trubner & Co., 1937), pp. 284–86.
23. Boretz, "Meta-variations," I, p. 39.
24. Boretz, "Meta-variations," I, pp. 51, 24, 36, 23.
25. Boretz, "Meta-variations," I, pp. 23, 25, 36, 43, 36 and pp. 51–70 respectively.
26. Rudolf Carnap, *The Logical Structure of the World*, trans., Rolf A. George (Berke-

ley: University of California Press, 1967) and Nelson Goodman, *The Structure of Appearance* 3rd Ed., (Boston: D. Reidel, 1977).

27. Boretz, "Meta-variations," II, pp. 76, 84, 104. and II, pp. 60–67.
28. Boretz, "Meta-variations," II, p. 86ff.
29. Boretz, "Meta-variations," II, p. 75.
30. Boretz, "Meta-variations," II, p. 75.
31. Boretz, "Meta-variations," II, p. 75.
32. Boretz, "Meta-variations," II, p. 74.
33. Boretz, "Meta-variations," III/1, p. 24.
34. Boretz, "Meta-variations," III/1, p. 23ff.
35. Boretz, "Meta-variations," III/1, pp. 27–28.
36. Boretz, "Meta-variations," II, pp. 66–67.
37. Boretz, "Meta-variations," II, p. 64, and III/2, 258, III/1, 24.
38. Boretz, "Meta-variations," III/1, p. 24.
39. See W. V. O. Quine, "Two dogmas of empiricism," in *From a Logical Point of View* (Cambridge: Harvard University Press, 1953, R: 1980), pp. 20–46; Thomas Kuhn, *The Structure of Scienific Revolutions* (Chicago: University of Chicago Press, 1962); John Austin, *Sense and Sensibilia* ed. G. J. Warnock (Oxford: Oxford University Press, 1962); Carl Hempel, "Empiricist Criteria of Cognitive Significance: Problems and Changes," and "A Logical Appraisal of Operationalism," both in *Aspects of Scientific Explanation*, pp. 101–133; Wilfred Sellars, *Science, Perception and Reality*, (London: Routeledge, and Kegan Paul, 1967); Norwood Russell Hanson, *Perception and Discovery* (San Francisco: Freeman Cooper & Co., 1969), pp. 59–198; Grover Maxwell, "The Ontological Status of Theoretical Entities," in *Scientific Explanation, Space and Time*, ed. H. Feigl and G. Maxwell (Minneapolis: University of Minneapolis Pr., 1963); Ludwig Wittgenstein, *Philosophical Investigations* (Oxford: Blackwell, 1953).
40. Boretz, "Meta-variations," I, p. 8.
41. Boretz is hardly alone in denouncing style as a theoretical concept. In a paper for the IMS meeting in Copenhagen, Peter Westergaard writes: "In my business, "stylistic" and "style" are dirty words. Of course we use them, but rarely with serious intent, and if in print, well insulated by quotation marks." "On the notion of Style," in *Report of the Eleventh Congress of the International Musicological Society, Copenhagen,* 1970 (Copenhagen: Hansen, 1974), p. 71.
42. Rahn, "Logic, Set Theory, Music Theory," p. 115.
43. Rahn, "Logic, Set Theory, Music Theory," p. 118–119.
44. Rahn claims that he presented a "tentative system of about fifty successively dependent fully formalized definitions and definitional schemata for predicates and operations that are basic to most theories of music using equal temperament." "Logic, Set Theory, Music Theory," p. 116. Alas, this system of definitions is unavailable.
45. Boretz, "Meta-variations," II, p. 76.
46. Boretz, "Meta-variations," II, p. 77.
47. It is worth noting that Aristoxenus came to this conclusion over 2,000 years ago. In Book 1 of his *Harmonics*, he tried to build a system starting with pitches. However, Aristoxenus ran into problems when trying to define genera because he had defined pitches in terms of stable frequencies and intervals as the fixed distance between notes, but needed movable notes and hence movable intervals to define each genus. To eliminate this problem in Book 2 Aristoxenus starts by classing music into gen-

era, then defines pitches and intervals from them. Thus, he implies that pitches and intervals are system-dependent concepts and not vice-versa. Schenker makes the same conceptual point when he takes tridic states as the basis for tonal music and not pitches or intervals.

48. Rahn also refers to D-N in "Aspects of Explanation in Music," pp. 206–207.
49. Henry Martin, "Modes of Analytical Discourse," *Perspectives of New Music* 15/2 (1977) pp. 175–176. John Rahn's illustration of D-N ("Aspects of Musical Explanation," pp.206ff) is also troubling. As an explanation of the explanandum "S is a row form used in Webern's *Concerto* Op. 24," he offers the following particular and general statements as explanans: "S is partitioned into trichordal segments that induce a partition on the set of pc {B, G, F, C♯}" and "all row-forms used in Webern's *Concerto* Op. 24 are partitioned into trichordal segments that induce a partition on the set of pc {B, G, F, C♯.}" But that inference commits the fallacy of affirming the consequent and is not a valid deduction at all. If the generalization is reversed in order to make the deduction valid, then the generalization is entirely false. At best, the generalization will be helpful in explaining why a row form S, which we know is used in Op. 24, has certain partitioning characteristics. Moreover, the generalization, as it stands, is only problematically law-like. The row forms actually used in Op. 24 are, by definition, finite; thus, the generalization is not universalizable in any straightforward sense.
50. The role of definition in scientific theorizing can be a complex one. First, an empiric generalization, as it becomes entrenched in our world view, may become elevated to a "law of nature" and ultimately can achieve the status of a semantic principle or definition. For example, dissonance in a tonal context may at first seem merely to correlate strongly with the logically independent property of interval size. But as our view of tonality becomes more regimented, we come to say that "tonal dissonance" just means an interval of a second, seventh or tritone. Second, a definition can possess some sort of empiric content. When not merely stipulated, a definition can be taken as an empiric hypothesis about what kind of semantic rule governs the intuitive application of a term by speakers of a language. When definitions are so proposed, they make predictions about and can be tested against the normal usage of the defined term. It is not at all clear that Boretz can or should be read in this way; see, for example, his definition of arpeggiation (*Meta-variations*, Part I, p.17).
51. Boretz, "Meta-variations," II, p. 50.
52. Boretz, "Meta-variations," I, p. 55.
53. We borrow this term from Frank Sibley, "General Criteria and Reasons in Aesthetics," in *Essays on Aesthetics* (Philadelphia: Temple University Press, 1983), p. 3.
54. Babbitt, "Contemporary Music Composition," p. 164.
55. Rahn, "Aspects of Musical Explanation," p. 207.
56. Rahn, "Logic, Set Theory, Music Theory," p. 114.
57. Westergaard, "On the Notion of Style," p. 71.
58. Peter Westergaard expresses this position most clearly:

    > When I compose a piece I am concerned to make those aspects of the structure that are unique to that piece as clear as possible. If the chief thing my listeners are going to get from my piece is that it somehow shares some common attributes with other pieces, why should I bother to write it? It gives them nothing they don't already have except possibly a feeling of comfort at recognizing something familiar.

    "On the Notion of Style," p. 71.

59. Boretz, "Meta-variations," I, p. 3.
60. Rahn also makes this equation in "Logic, Set Theory, Music Theory." In footnote 6 (p. 124) he considers whether true analyses should or should not be quirky or even ugly. He then says, "Run the other way, generatively, this Theory would produce only music of unsurpassed beauty; perfect music." He adds, "to attempt to make a theory generate *all and only all* beautiful music, a theory which would be capable of delivering (syntactically) *only* the most beautiful analysis, seems to me futile. One man's meat is another man's poi."
61. Cone, "*Musical Form and Musical Performance* Reconsidered," *Music Theory Spectrum* 7 (1985), p. 158.
62. Leonard B. Meyer, *Explaining Music* (Chicago: University of Chicago Press, 1973), pp. 6–7. Meyer's commitment to particularism in this essay is peculiar, especially in light of his more recent work (e.g., "Toward a Theory of Style," in *The Concept of Style*, ed., Berel Lang (Ithaca: Cornell University Press, 1987), pp. 21–71. At the very least, in *Explaining Music*, he clearly promotes critical analysis. He insists that critical analysis not only tries to discover the "secret of the singular" but also that, by its very nature, it is *ad hoc* and non-general: "But because specific musical events are the result of non-recurring concatenations of conditions and variables, no set of general laws can adequately explain the particular relationships embodied in an actual composition. In other words, no matter how refined and inclusive the laws of music theory become, their use in the explanation of particular musical events will have to depend in part on the *ad hoc* hypotheses of common sense" (*Explaining Music*, pp. 11–12). But, elsewhere in the same essay, he maintains, on the one hand, that explanations require classification, generalization and conceptualization and, on the other, that anything falling short of this standard constitutes "unintelligible mysticism" (*Explaining Music*, pp. 4–5.) The unexpected difference between the *ad hoc* hypotheses of common sense and mystical pronouncements about music makes all the difference to the intelligibility of Meyer's thesis. But what difference is there besides that the former is a more familiar brand of unjustified belief?
63. Meyer, *Explaining Music*, p. 268.
64. Kerman, *Contemplating Music*, (Cambridge: Harvard University Press, 1985), p. 84.
65. Eugene Narmour, *Beyond Schenkerism* (Chicago: University of Chicago Press, 1977), Chapter 2, pp. 12–30; Richard Taruskin, "Review of Forte, *The Harmonic Structure of the Rite of Spring*," *Current Musicology* 28 (1979), p. 119.
66. Taruskin, "Forte review," p. 119.
67. Joseph Kerman, *Contemplating Music* (Cambridge: Harvard Univ. Pr., 1985), 85.
68. Martin, "Modes of discourse," p. 177.
69. Philip Gossett, "Beethoven's Sixth Symphony: Sketches for the First Movement," *Journal of the American Musicological Society* 27 (1974), p. 261
70. Taruskin," Chernomor to Kashchei: Harmonic Sorcery; Or, Stravinsky's "Angle," *Journal of the American Musicological Society* 38 (1985), pp. 72–142.
71. See, for example, Douglas Johnson, "Beethoven Scholars and Beethoven's Sketches," *Nineteenth Century Music* 2 (1978–9), pp. 3–17.
72. Narmour, *Beyond Schenkerism*, pp 1–30.
73. For this reason we reject the claims of Keiler and others that background structures represent paradigmatic tonal progressions or cadence formulae.
74. Michael Kassler, "A Trinity of Essays," (Ph.D. dissertation, Princeton University,

1968); "Explication of a Middleground of Schenker's Theory of Tonality," *Miscellanea Musicologica* (1977), pp. 72–81.

75. This list was devised with the help of Andrew Cohen of the Society of Fellows/Department of Theoretical Particle Physics, Harvard University. To our knowledge, the only writer to offer a list like the one in Table 5a is Carl Schachter, see "A Commentary on Schenker's *Free Composition*," *Journal of Music Theory* 25 (1981), pp. 124-125. According to Schachter background structures depend "on a number of prior assumptions about tonal organization in music." They are as follows:
    1. A significant connection exists between the triad as a simultaneity and as an arpeggio, complete or incomplete.
    2. The triadic intervals (octave, fifth and third) are consonant and can function as beginnings and goals of motion.
    3. The interval of the fifth defines triadic roots and tonic notes.
    4. The tonic note is the final goal of tonal progression.
    5. Upper-voice tonal progression is basically conjunct, bass motion disjunct . . .
    6. Dissonances arise melodically and must resolve melodically—that is, by step.
76. Strictly speaking, we should not expect that these laws can be confirmed or falsified independently of one another. Predictions arise only when all six generalizations are invoked along with the transformational rules as a whole theory of tonality. Thus, the validity of the laws stands or falls as part of a larger theory. For a discussion of holism and theories see W. V. Quine, "Five Milestones of Empiricism," in *Theories and Things* (Cambridge: Harvard University Press, 1981), pp. 70–71.
77. Heinrich Schenker, *Harmonielehre* (Stuttgart and Berlin: J. Cotta, 1906), paragraphs. 38–52 and *Kontrapunkt* 1 (Stuttgart and Berlin: J. Cotta, 1910) Part 1, Chapter 1, paragraph 5.
78. For example, Julian Ruston notes: "Schenker's analyses have two distinct aims. One is the study of the unique qualities of a masterpiece, relating the minutest details to the whole; the existence of such relationships is a criterion of mastery for they show that a piece is truly organic." *The Musical Language of Berlioz* (Cambridge: Cambridge University Press, 1983), p. 172.
79. Martin, "Modes of Explanation," p. 183.
80. Viktor Kraft, *Foundations for a Scientific Analysis of Value*, ed., Henk L. Mulder and trans., Elizabeth Hughes Schneewind (Boston: D. Reidel, 1981); C. I. Lewis, *An Analysis of Knowledge and Valuation*, (La Salle, Illinois: Open Court, 1946); Monroe C. Beardsley, *Aesthetics*, (2nd ed., Cambridge: Hackett, 1981), p. 454ff.
81. Edward Cone, "Analysis Today," *The Musical Quarterly* 46 (1960), p 187.
82. We would like to thank members of Prof. Dempster's seminar in the Philosophy of Music Theory, Eastman School of Music (Spring 1988), especially David Rogers, for bringing this point to light.
83. For example, William Dray, *Laws and Explanation in History*, pp. 22–85; Stuart Hampshire, "Logic and Appreciation," *World View*, 1952 , reprinted in William Elton, ed., *Aesthetics and Language*, (Oxford: Blackwell, 1954); Mary Mothersill, *Beauty Restored* (Oxford: Oxford University Press, 1984); Eddy Zemach, "Aesthetic Properties, Aesthetic Laws, and Aesthetic Principles," *Journal of Aesthetics and Art Criticism* 46/1 (1987), pp. 67–74.
84. It is interesting to note that in a recently published lecture, Milton Babbitt endorses

a very similar view with respect to rows in twelve-tone music. When asked the question "where the hell is the row, or the series?" in *Compositions for Four Instruments*, Babbitt responds "the series is there exerting its influence constantly without being explicitly present." See "The Twelve-Tone Tradition," in *Words About Music*, ed., Stephen Dembski and Joseph N. Straus (Madison, Wisconsin: University of Wisconsin Press, 1987), pp. 26–30.

85. See Wesley Salmon, *Statistical Explanation and Statistical Relevance* (Pittsburgh: Pittsburgh University Press, 1971) and *Scientific Explanation*; Henry Kyburg, *Probablity and the Logic of Rational Thought* (Middletown, Connecticut: Wesleyan University Press, 1961).
86. See Jarrett Leplin ed., *Scientific Realism* (Berkeley: University of California Press, 1984.)
87. See Bas van Frassen, *The Scientific Image* (Oxford: Oxford University Press, 1980.)