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"The Concerned and Thoughtful Musical Citizen"
Music Analysis as Sufficient Explanation

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"The Concerned and Thoughtful Musical Citizen"
Music Analysis as Sufficient Explanation

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Thesis submitted to the
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In partial fulfillment of the requirements
For the MA in Music Theory

Department of Music
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Abstract

Some scholars have proposed the application of scientific method to the practice of music theory and analysis. This thesis examines two such frameworks: one developed by Milton Babbitt, and another by Matthew Brown and Douglas Dempster. The first part presents Babbitt's prescriptions of "scientific language and method," along with his distinction between analytical *explanation* and *description*, and an outline of Hempel and Oppenheim's Deductive-Nomological model of explanation. The second part focuses on Brown and Dempster's application of scientific method to music theory and analysis and contrasts their view with Babbitt's. Within the thesis, I make the following restrictions to my theoretical framework: music theory and analysis are explanatory activities—as opposed to descriptive—and music theories are composed of generalized empirical statements concerning individual works on an analytical level. In its consideration of large methodological questions, this thesis promotes the continued integrity of the discipline of music theory and analysis.

Introduction

Writing in 1999, Per F. Broman summarizes what he sees as a persistent methodological dichotomy remaining unresolved within the discipline of music theory:

> During the last ten years or so the American music-theory community has widely discussed the issue of what musicologists and music theorists really do—or what we should be doing—when we perform musical analysis. Are we shedding light on the music that we love (many analysts seem to agree with that interpretation) or are we performing some kind of scientific work? The two approaches are clearly different, even though they overlap.[1]

Broman is drawing on distinctions and questions that have long engaged the attention of music theorists: What are we doing when we analyze music? What do we claim to know, and how do we justify our knowledge? Broman does not attempt to answer these questions; and perhaps, no concrete resolutions are available. This should never prompt us, however, to give up thinking about them.

I focus on one half of this methodological duality, i.e. music theory and analysis as "scientific work." This thesis, therefore, examines the procedure and merits of applying scientific method to music theory and analysis. It is a consideration of an approach that has been largely neglected as of late. As a critical framework, my research finds impetus in an adjective that Milton Babbitt uses to characterize music theory and analysis: i.e. *sufficient*. For Babbitt, this term serves as a useful modifier that can encapsulate the relative merits of any given music theories or analyses. We may describe a music theory or analysis as more, or less sufficient. This terminology prompts further pertinent questions similar to the ones

[1] Per F. Broman, "Touching up the Paint or a Complete Vinyl Residing? Reflections over Meta-Scientific Problems in Music Theory," *Perspectives of New Music* 37 (1999): p. 7.

posed above: What constitutes a sufficient music theory and analysis? How do we define and measure sufficiency?

Babbitt also establishes an important restriction of the theoretical and analytical activity to which he applies the quality of sufficiency. For Babbitt, music theory and analysis are explanatory activities. In this respect, Babbitt distinguishes between *description* and *explanation*. In a lecture subsequently published as "Professional Theorists and Their Influence,"[2] Babbitt contrasts Rameau-based Roman Numeral chord labelling with Schenkerian analysis in order to illustrate the difference between analytical explanation and description. Ultimately, the distinction Babbitt invokes is this: description provides nothing more than a catalogue or identification of the surface-level features of compositions; explanation, on the other hand, provides causes for those same features.

Taking this framework into account, our earlier questions garner additional refinement. This more narrow question, then, constitutes the central motivation of this thesis: "What constitutes a sufficient analytical explanation of musical structure?" I assume for the confines of this project, therefore, that music analysis is an explanatory activity that aims to enhance our understanding of music's structural design. Patently, music theorists do not subscribe universally to this point of view. Some hold that the function of music theory and analysis is to describe a personal hearing or interpretation of music. In the words of Charles Smith, "I'm not even sure that it is helpful to postulate 'explanation' or 'clarification' as the ideal goals of analysis...What I find analytically acceptable at any

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[2] Milton Babbitt, "Professional Theorists and Their Influence," in *Milton Babbitt: Words About Music*, ed. Stephen Dembski and Joseph N. Straus, 121-162 (Wisconsin: The University of Wisconsin Press), 1987.

moment is far too dependent on my training and my musical experience, not to mention my spiritual beliefs, and probably the state of my digestion as well."[3] (I will return to this debate about the role of music theory and analysis in the main body of the thesis.) For this study, however, I consider analytical explanation from within a theoretical framework that posits it as the proper goal of music theory and analysis.

Milton Babbitt, and later, Matthew Brown and Douglas Dempster answer the question of explanatory sufficiency for music theory and analysis with scientific method—or scientific explanation. In other words, analysts may achieve a sufficient explanation of music by conforming to the methodological requirements and systematic values of scientific method. This thesis, then, divides into two large sections: the first (chapters 1 and 2) discusses Milton Babbitt and scientific method in general; the second (chapters 3 and 4), considers Matthew Brown and Douglas Dempster's application of scientific method to music theory and analysis and their connection to Babbitt.

Chapter 1 is an in depth examination of Milton Babbitt's prescriptions for music theory and analysis outlined over the course of several essays.[4] I begin with the distinction between *explanation* and *description* that Babbitt forms in "Professional Theorists and Their Influence." The rest of the chapter interprets Babbitt's answer to the problem of sufficiency through scientific method. His understanding of science is separated into two general categories: terminology and methodology. Both "scientific language" and "scientific

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[3] Charles Smith, "A Rejoinder to David Beach," *Music Theory Spectrum* 9 (1987): p. 183.
[4] Milton Babbitt, "Past and Present Concepts of the Nature and Limits of Music" (1961); "The Structure and Function of Musical Theory" (1965); "Contemporary Music Composition and Musical Theory as Contemporary Intellectual History."(1972) See the bibliography for complete references.

method" are necessary for constructing a sufficient, analytical explanation of musical structure. Furthermore, Babbitt defines scientific method with recourse to two distinct elements: empiricism, and the construction of universal laws, which he calls "primitive concepts."

Babbitt does not describe directly the characteristics of scientific explanation that he has in mind. Chapter 2, therefore, completes Babbitt's framework by presenting an outline of the standard model of scientific explanation as defined by Carl Hempel and Paul Oppenheim in their essay *Studies in the Logic of Explanation*.[5] In this chapter, I discuss their Deductive-Nomological model of scientific explanation and its most definitive features. These include: the empirical nature of scientific inquiry; the hierarchical scheme of universal laws; explanation as causal; and the equivalence of explanation and prediction. Finally, I discuss the meaning of *sufficient* and *necessary conditions* as defined by the philosophy of science and logic studies.

Chapter 3 takes a different look at the application of scientific method within music theory and analysis through the work of Matthew Brown and Douglas Dempster. In their collaborative publication *The Scientific Image of Music Theory,*[6] Brown and Dempster revive the subject in order to defend scientific music theory from its detractors and propose an unbiased reconsideration of its merits and shortcomings. Contrasting scientific inquiry

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[5] Carl G. Hempel and Paul Oppenheim, "Studies in the Logic of Explanation," *Philosophy of Science* 15 (1948): 135-175.

[6] Matthew Brown and Douglas Dempster, "The Scientific Image of Music Theory," *Journal of Music Theory* 33 (1989): 65-106.

with *particularism*,[7] Brown and Dempster uphold scientific method as the most secure way of establishing true scholarly intersubjectivity. They also adjust Hempel and Oppenheim's model of scientific explanation to make it more practical in everyday analytical activity. Sensitive to the most recent developments within the philosophy of science, Brown and Dempster replace the universal laws of the Deductive-Nomological model with statistical generalization. Finally, I address briefly Brown and Dempster's justification for considering music theory and analysis as scientific pursuits. They uphold the importance of applying empirical and systematic modes of inquiry to an aesthetic object such as music.

The passage of time that separates Milton Babbitt from Brown and Dempster does not leave the scientific framework for music theory and analysis unaltered. The fourth and final chapter of this thesis examines the differences between their respective ideas. Returning to his framework with a more critical eye, we discover that Babbitt locates primitive concepts within individual pieces of music. Discarding the notion that compositions can be understood as unique entities, Brown and Dempster universalize explanation across general classes of musical phenomena.

To summarize, this thesis follows a distinct line of thought progressing through the writings of Milton Babbitt, Matthew Brown and Douglas Dempster. It examines the application of scientific method to the practice of a music theory and analysis that intends to explain. In this way, I focus on that aspect of the discipline that Per Broman sees as accomplishing "scientific work." Beginning with Babbitt's distinction between *explanation*

[7] "*Particularism* holds that an individual artwork wholly...determines its own best method of analysis or interpretation." Brown and Dempster, "The Scientific Image of Music Theory," p. 82.

and *description*, I discuss Babbitt's answer of scientific method to the problem of achieving sufficient explanation within music theory and analysis. After a brief exposition of scientific method in general, I continue the thread to examine Matthew Brown and Douglas Dempster's treatment of music theory and science. Finally, I compare Babbitt with Brown and Dempster in order to reveal the methodological differences between their respective frameworks.

This thesis does not pretend to give a complete answer to the thorny methodological problems that motivate it. Rather, I consider one possible way among several to approach the daunting task of understanding and explaining music through the systematic practice of theory and analysis. Furthermore, my research focuses exclusively on Milton Babbitt, Matthew Brown and Douglas Dempster. Nevertheless, my bibliography includes a broad spectrum of opinions and frameworks from a variety of theorists. They have all influenced my thinking, either directly or indirectly. Ultimately, I present the picture of a scientific music theory and analysis as some have described it, and I point in the direction that further research in this area might take. In its methodological focus, this thesis represents an attempt to take the difficult, yet necessary step backwards from the practice of music theory and analysis to investigate the critical foundations of the discipline.

Chapter 1: Milton Babbitt and Scientific Explanation

1. The Professional Music Theorist

In 1983, Milton Babbitt gave a series of lectures at the School of Music of the University of Wisconsin at Madison. Subsequently, these lectures were transcribed, edited, and compiled into book form by Joseph Straus and Stephen Dembski.[9] In one of these lectures, entitled "Professional Theorists and Their Influence,"[10] Babbitt gives us his unique and privileged insight into the development of music theory and professional theorists in the American university. Of course, Babbitt himself both witnessed this development first-hand and participated as a crucial figure in it.

In his discussion of professional theorists, Babbitt establishes two fundamental considerations to bring to music theory and analysis: 1) theory and analysis can have explanatory power, as contrasted with superficial and inconsequential types of categorization and note-counting; and 2) different gradations within analytical explanation exist and we may potentially describe the relative value of explanations within this framework using terms such as *sufficient* and *satisfactory*. In order to reveal how Babbitt accomplishes this in the lecture, I examine the analytical examples he works through. I choose the following two representatives from Babbitt's discussion of Schenkerian analysis: J. S. Bach's harmonizations of, 1) Chorale #217, *Ach Gott wie manches Herzelied*, (Salzer and Schachter's analysis); and 2) Chorale #117, *Nun ruhen alle Wälder* (Schenker's analysis).

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[9]Stephen Dembski and Joseph N. Straus, ed, *Milton Babbitt: Words About Music* (Wisconsin: The University of Wisconsin Press), 1987.
[10] Ibid. p.121-162.

At the beginning of his lecture, Babbitt clearly links the emergence of the professional theorist within the university setting with the impetus to take music theory seriously. In this context, "taking theory seriously"—Babbitt's own words—implies an attempt to make significant observations about music's internal construction (how and why a piece of music comes together as it does), as opposed to superficial categorizations and note counting:

> The notion of professional theory is almost entirely new. There were virtually no professional theorists in this country, unless you count the people who took degrees at teachers' colleges by counting the number of six-four chords in the *Teutonic* Sonata of Edward McDowell (there were such theses by the way) or those people who found new labels for old chords or old labels for new chords. That's really all that one could call theory.[11]

By contrasting this chord counting and labelling type of analysis with the work of professional theorists, Babbitt is suggesting that music theory can have some sort of significant content and explanatory power beyond the superficial.[12] Taking theory seriously, then, gives the discipline a constructive and critical identity as a type of profound thinking about music and its design. Babbitt devotes this lecture entirely to a discussion of the explanatory possibilities of music theory and analysis.

A crucial consideration permeates this lecture and arises from its fundamental motivation of highlighting different possibilities within music analysis. Demonstrating different types and levels of analytical explanations, Babbitt invites us to ponder the relative values of these gradations. He makes this purpose explicit at a few points in the course of the lecture; but he mostly provokes the question rather than asking it directly. In the middle

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[11] Babbitt, "Professional Theorists and Their Influence," p. 121.

[12] In order to illustrate this dichotomy, it is hardly worth enumerating the ways in which an analysis that counts six-four chords is inconsequential and superficial. Music theory and analysis have progressed far enough to waive the necessity of this task.

of a topical assessment of Schenkerian analysis (to which we will return in more detail), Babbitt comments on the polemical language that Schenkerians often use with each other:

> It becomes violent, really violent. David Beach simply says that Felix Salzer's analysis...is a mistake. Now, what makes a mistake? One could assume that Salzer has misinterpreted Schenker and therefore it's not a good Schenker analysis—which interests me about as little as anything in the world—or that his is simply a mistaken way to view the piece, which interests me very much.[13]

This passing comment clearly demonstrates Babbitt's concern in this lecture. In the process of discussing various levels of analytical explanation, the focus does not lie merely with mechanics; rather, we must consider the possibility that musical analyses can be more or less explanatory, perhaps, better or worse. By raising the idea of an analytical "mistake", Babbitt suggests that we can establish certain ways of determining the relative value of different analyses. A mistake is only such if we can posit a theoretically "correct" answer.

In the context of this lecture, Babbitt introduces the terms *sufficient*, *satisfactory*, and *coherent* as critical concepts that can potentially articulate the process of determining the relative merits of different analyses. The final section of this lecture discusses the infamous opening measures of Wagner's *Tristan* Prelude and the different ways that theorists have attempted to explain the harmonic conundrums presented therein. Babbitt introduces this section by inviting us to ponder Salomon Jadassohn's[14] interpretation of the prelude. For Babbitt, the final evaluation of Jadassohn's analysis lies in providing answers to questions centred around the terms I mentioned above:

> He [Jadassohn] said that any fool will see how this *Tristan* Prelude goes: it simply passes through fourteen keys in the first twelve measures. This for him was an explication in terms of the received normal theory of his time. Once you can locate

[13] Babbitt, "Professional Theorists and Their Influence," p. 134.
[14] 19th Century composer and theorist at the Liepzig Conservatory. Babbitt refers to his book: *Melodik und Harmonik bei Richard Wagner*, 1899.

> things in tonalities, a piece was explicated and therefore satisfactory and therefore, I assume, a coherent piece of music. Now his explication may seem to many of you a representation of chaos rather than of coherence. The fact of the matter is that it was satisfactory for him. *This is a big theoretical issue. What constitutes a sufficient explanation? What's a satisfactory explanation? What about the scope of an explanation?*[15]

Here, Babbitt explicitly establishes the terms *sufficient, satisfactory,* and *scope* as possible key components of the question that asks about the explanatory power of an analysis.

Before moving ahead, we must remember that the discussion we are about to enter is an informal seminar with graduate students in the history of music theory.[16] As such, we cannot expect the ideas presented here to be exhaustive or representative of Babbitt's complete thoughts on the subject of analytical sufficiency. (Instead, we look to his earlier, more formal essays for a more complete account of his views about music theory and analysis.) Despite its light and informal quality, however, this lecture provides more than a simple introduction to the thorny methodological problems of the formal essays; for here, Babbitt operates primarily as an analyst. His own work as analyst serves in some way to concretize and illustrate the theoretical and methodological generalizations of his earlier writing.

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[15] Babbitt, "Professional Theorists and Their Influence," p. 146. Italics added.
[16] *Milton Babbitt: Words About Music.* Preface, viii.

2. Two Bach Chorales

As illustrations of different possible levels of analytical explanation, Babbitt examines two Chorale harmonizations by J. S. Bach. He discusses existing analyses[17] of the Chorales and offers his own insights by pointing out the merits and shortcomings of each one. This discussion falls under a general examination of Schenkerian analysis, which Babbitt credits as a key impetus for "taking theory seriously." With these examples, Babbitt demonstrates two important points: 1) Schenkerian analysis achieves a level of explanation through its ability to synthesize musical compositional features and answer the "why" of musical design; 2) Schenkerian analysis has defined limits to its explanatory power. This second point leads to a restriction upon the assessment of analytical explanation that facilitates the bridge between this lecture and Babbitt's formal essays on methodology: i.e. the necessity of an internal, systematic coherence within a theoretical or analytical framework that trumps the need to find a single method that includes every level of explanation.

Babbitt's first point is best revealed by his comments concerning Schenker's analysis of Chorale #117: his second point by his commentary on Salzer and Schachter's analysis of Chorale #217. Once again, let us remember that these discussions are not complete in themselves and they do not represent Babbitt's final thoughts concerning analytical sufficiency; rather, they adeptly illustrate the existence of gradations within analytical explanation and thereby set the stage for understanding his formal essays. And as we already know with regard to Babbitt, the first step towards defining a sufficiently

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[17] -Chorale #217, *Ach Gott wie manches Herzelied.* Felix Salzer and Carl Schachter, *Counterpoint in Composition: the Study of Voice Leading* (New York: Columbia University Press), 1989. p. 264-65.
-Chorale #117, *Nun ruhen alle Wälder.* Heinrich Schenker, *Five Graphic Analyses* (New York: Dover), 1969.

explanatory analysis is the admission that these gradations can be concretely demonstrated: qualitative terms such as *sufficient* and *satisfactory* imply the possibility of having distinct levels of analytical explanation. Let us now take up each Chorale in turn.

Nun ruhen alle Wälder: Bach, Schenker, Babbitt

Babbitt's discussion of this Chorale demonstrates two features of Schenkerian analysis that give it a certain explanatory significance: its powers of synthesis and its ability to answer the question "why" with regard to compositional design. These features, however, are not independent: the ability of Schenkerian analysis to answer the question "why" arises as a result of its power to synthesize musical elements within the analytical framework.

First of all, let us emphasize the importance of the question "why" in terms of explanation. Babbitt contrasts the investigation of the "why" of musical design with the traditional chord-based approach to analysis. He comments that chord-based analysis cannot explain specific harmonizations, such as the first melody note in measure 5 of the Chorale:

> Can you imagine any criterion in the world that could be gleaned from traditional harmony (by which I mean traditional, basically Rameau-based harmony, not figured-bass harmony) which could explain why you shouldn't have a nice root position triad to harmonize the Ab in measure 5? Is there any rule of harmony you know that forbids that as it forbids parallel fifths (nasty sounds)?[18]

In other words, traditional chord-based harmony cannot provide an explanation for the particular harmonization of that melody note: it can merely describe its surface-level design as a Tonic triad in the first inversion.

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[18] Babbitt, "Professional Theorists and Their Influence," p. 138-139.

We may, perhaps, encapsulate the point Babbitt is making in the following way: description asks "what," while explanation asks "why."[19] Within the framework of chord-based analysis, the choice of harmonization becomes assumptive: its existence is taken for granted and receives no causal explanation. Such a description moves no closer to explanation than incorrigible—or essentially irrefutable—statements that reason some fact simply "because it is so." To confuse description with explanation seems to be a clear instance of circular reasoning: "a square is a square simply because it is a square."

As we will see later, this charge of circular reasoning has direct implications within a scientifically based music theory and analysis. In one of his formal methodological essays, Babbitt links description (answering the "what" of musical design) to the inability to find *sufficient conditions* for genuine explanation:

> This inability to discover a sufficient condition is equivalent to observing that any other transposition would have been as musically "coherent" (or "satisfactory or "—") and that, again, the transpositional choice must be regarded as assumptive.[20]

An assumption, like an incorrigible statement, does not submit proof or remain open to argument. Babbitt's criticism of chord-based analysis may seem unnecessarily harsh. If we replace chordal analysis with Schenkerian, however, it becomes relevant and justified. Babbitt shows how Schenkerian analysis synthesizes musical elements to provide an explanatory context for musical design.

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[19] Hempel and Oppenheim corroborate of this simple, intuitive distinction between explanation and description: "To explain phenomena in the world of our experience, to answer the question 'why?' rather than only the question 'what?,' is one of the foremost objectives of all rational inquiry." See: "Studies in the Logic of Explanation," *Philosophy of Science* 15 (1948): p. 135.

[20] Milton Babbitt. "Contemporary Music Composition and Musical Theory as Contemporary Intellectual History," in *The Collected Essays of Milton Babbitt*, ed. Stephen Peles (with Stephen Dembski, Andrew Mead, and Joseph N. Straus), 270-308. Princeton: Princeton University Press, 2003.

As we saw in his pointed question to traditional chord-based harmonic analysis above, Babbitt's analytical point here concerns the harmonization of the first chord in m. 5 of *Nun ruhen alle Wälder*. (The chord in question is boxed in fig. 1a, and indicated with arrows in fig. 1b.) From a Schenkerian point of view, Babbitt argues that this first-inversion harmonization cannot be otherwise. In other words, the Schenkerian analysis provides a framework for answering the question "why":

> There can't be [a root position tonic triad under the melody note] because the line would end at that point. That's the kind of thing I would regard as a little more reasonable basis for teaching what is in some places called harmony. From the C before measure 1 to the C in measure 2 under the fermata, you maintain that constancy, the Ab in the bass. The Bb in measure 3 is harmonized by the dominant which recurs under the fermata in measure 4. At the Ab in measure 5 you *have* to have at least something as weak as that because otherwise your ear would take you C, Bb, Ab, and the piece from any musical point of view is over. What does it mean for a piece to be over? Not just that you repeat fourteen C-major triads and then everybody yells "bravo."[21]

Discussing this passage, Babbitt asserts the almost inevitable necessity of the harmonization in question by appealing to the Schenkerian *Ursatz*. With a root position triad at the first chord of m. 5, the *Ursatz* would be complete, its fundamental line descending ^3, ^2, ^1,[22] accompanied by the bass arpeggiation of the root-position triads I-V-I. Of course, the *Ursatz* closes only at the end of the music, not at the midpoint: when the *Ursatz* closes, the music is over. Babbitt clarifies this sense of "over": in the Schenkerian framework, the *Ursatz* encompasses, or contains, the entire motion of a piece of tonal music. The closing tonic is a real termination of the music's tonal identity; it presents a final resolution of the movement away-from and back-toward the governing tonic triad. Most obviously, the

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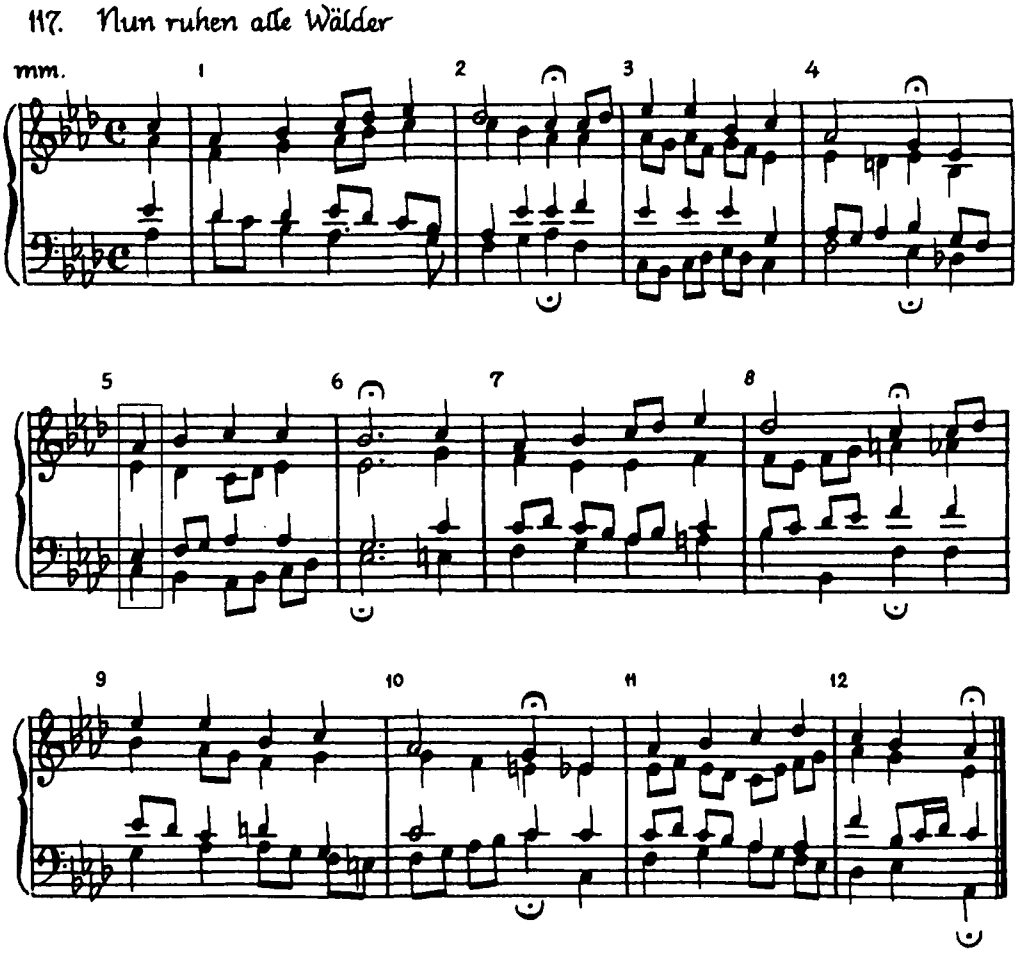
[21] Babbitt, "Professional Theorists and Their Influence," p.139.
[22] The "^" indicates "scale-degree" at this point and hereafter.

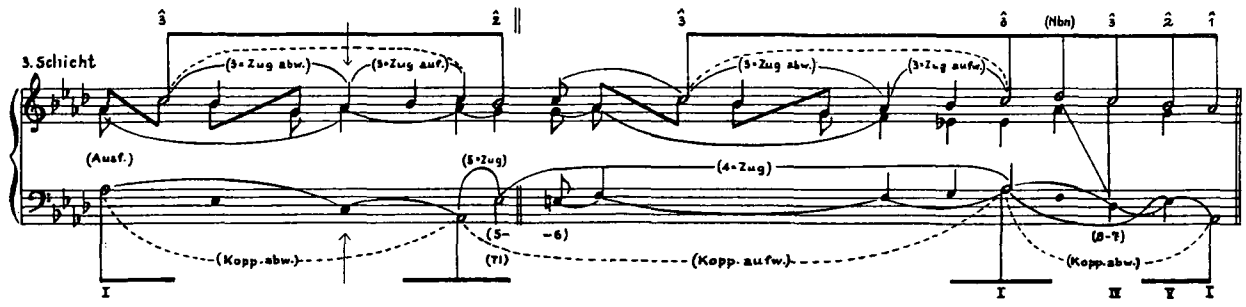
music of this chorale does not end in m. 5.[23] Therefore, a root position tonic triad at this point would be inappropriate and confusing in terms of the large-scale tonal structure of the chorale.

Figure 1. a) Chorale Harmonization



[23] Of course, Babbitt is not pretending to claim that Bach's harmonization is inevitable or necessary. If Bach had chosen to harmonize this chord with a root-position Ab triad, a Schenkerian analysis would probably interpret it as a local level *Ursatz* parallelism instead of a 3-*Zug*. In other words, Babbitt's point here is not immune to objection. Such is the nature of this lecture: Babbitt is quickly demonstrating a large concept with limited resources and we cannot expect his arguments to be complete and beyond alternative interpretation. The validity of this analysis, obviously, does not ultimately hinder his larger point concerning the synthetic powers of Schenkerian analysis.

b) Schenker Sketch



Sources:

a: Milton Babbitt, "Professional Theorists and Their Influence," in *Milton Babbitt: Words About Music*, p. 138

b: Heinrich Schenker, *Five Graphic Analyses*, p. 32

Babbitt uses the features of this piece that precede this chord in order to justify and support the Schenkerian explanation. Each time the melody note C coincides with tonic harmony in the first five measures, an Ab supports it in the bass. This pattern clearly establishes the first *Stufe* in the *Ursatz*: it is a 3-line. Furthermore, the Dominant on the third beat of measure 3 (which unfolds to the fermata of measure 4) is in root position with an Eb in the bass, and ^2 in the melody, normally features that prepare the final resolution of the *Ursatz* to ^1. Accordingly, a root position tonic triad in measure 5 would create prematurely a sense of tonal closure proper to the end of the piece.

In this way, Babbitt shows clearly how Schenker's analysis reflects the determining factors in this particular arrangement of the Chorale's harmonization. Schenkerian analysis can answer the "why" of the first chord in m. 5 by synthesizing harmonic and melodic elements of this music in order to reveal how the *Ursatz* governs its tonal motion—

interrupting movement to tonal closure and thus prolonging by composing out the interruption.

In Babbitt's comments here, we begin to see the explanatory power of the Schenkerian sketch, which provides a framework we can "regard as a little more reasonable basis for teaching what is in some places called harmony." Babbitt began his discussion by asking if traditional chord-based harmony could ever explain the first-inversion harmonization of the initial tonic triad in m. 5 of the Chorale. Therefore, we may deduce Babbitt's conclusion that Schenkerian analysis offers a level of explanation that chord labelling harmonic analysis cannot provide. Schenkerian analysis does not merely describe: it provides a reason why. Babbitt makes this clear at the beginning of this section about Schenkerian analysis:

> Things are being determined in this way that you could never get dropping out of functional harmony à la Rameau. Schenker's whole view of music immediately entails certain kinds of conclusions about musical progression—conclusions which could never be conjoined to the usual functional harmonic notion without simply adjoining a lot of new independent premises which would have no relationship to, and certainly not entailed by, any of the other premises of functional harmony.[24]

Babbitt's assessment of Schenker's sketch for *Nun ruhen alle Wälder* is particularly important because it asserts that musical analysis can have different gradations, or levels of explanation. It contrasts the descriptive power of what Babbitt calls "functional harmony à la Rameau," with the explanatory power of Schenkerian analysis. This is a crucial first step in arriving at terms such as *sufficient* and *satisfactory*, as Babbitt uses them. To distinguish *sufficient* from *insufficient*, we must have a gradation, a scale of relative explanatory value.

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[24] Babbitt, "Professional Theorists and Their Influence," p. 128.

The question as to whether Schenkerian analysis offers a sufficient or satisfactory analysis has not yet arisen: all we have seen so far is that Schenkerian analysis provides an analytical explanation, whether it is satisfactory or not. Babbitt's comments on the next Chorale open this issue by testing the limits of Schenkerian explanation.

Ach Gott, wie manches Herzelied: Bach, Salzer and Schachter, Babbitt

For Babbitt, this Chorale[25] provides two important insights: it demonstrates the explanatory power of Schenkerian analysis; and it reveals an analytical framework in which Schenkerian analysis does not normally operate. In his own words, "it suggests the things that a Schenker analysis may very well adumbrate but certainly not reveal."[26] Concerning the former, Babbitt makes the same kinds of points we have seen in the previous section: Schenkerian analysis offers a synthetic, coherent framework for answering the "why" of particular compositional features. Let us turn to the latter point, the compositional features of the Chorale that test the explanatory limits of Schenkerian analysis.

As mentioned before, Babbitt's commentary on this Chorale demonstrates the relevance of applying terms such as *sufficient* to music analysis. This leads into a larger methodological issue concerning the importance of internal consistency within any given analytical framework. (In this way, our final examination of Babbitt's lecture provides a useful transition into the formal essays.)

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[25] From his unique standpoint within the history of modern music theory, Babbitt notices that this simple Chorale harmonization is not well known: "By a strange, incredible coincidence, this chorale was never mentioned anywhere in the literature until the Salzer/Schachter book came out." "Professional Theorists and Their Influence," p.126.

[26] Ibid.

Babbitt's test of the consistency of the Schenkerian framework appears in a critique of Salzer and Schachter's analysis of this Chorale (see fig. 2a). According to Babbitt, Salzer and Schachter "miss the boat terribly."[27] Babbitt draws an interesting feature of this Chorale to our attention: the peculiar harmonization in m. 9. At this point, Bach's voice leading is awkward and it breaks no less than three established guidelines of good part-writing that every student of harmony learns. In discussing the voice-leading possibilities at this point in the music Babbitt describes Bach's choices in this way:

> How about a G# in the bass? How about a G# which does all kinds of terrible things, gives you a false relation, gives you a dissonant leap in the bass, and then, why not just cross a few voices, which you're not supposed to do, while you're at it? Three wrongs make a right.[28]

Babbitt asserts that Salzer and Schachter cannot account for this awkward voice-leading moment with their Schenker sketch. Their explanation of this motion is akin to granting Bach the compositional license afforded by genius to do whatever one wants. Of course, this answer does not truly explain the moment: it does not demonstrate a cause or reason for Bach's choices. Babbitt criticizes Salzer and Schachter in no uncertain terms. He chastises them for resting their analytical logic on undefined, assumptive concepts:

> When Salzer and Schachter get to measure 9, they say, "By using cross-relations, we make the texture less smooth. Smoothness, however, is by no means a constant requirement; and to forbid cross-relations at this stage would be sheer pedantry." Now at what stage do you stop forbidding false relations because they become sheer pedantry? They introduce the notion of smoothness, but they don't tell you why Bach doesn't use smoothness there and uses it somewhere else or in some other piece. They say, "The cross-relation results from the sudden climactic use of high register underscoring the appearance of the tonic C in the bass." What they're saying is that two wrongs make a right. The logic breaks down completely because they just don't pay any attention to this parallelism. This is exactly the kind of thing that you often miss in Schenker: explicit discussion of motivic parallelism and the way in

[27] Babbitt, "Professional Theorists and Their Influence," p.126.
[28] Ibid. p.131.

> which a parallelism will induce parallelisms in other dimensions. All I'm interested in is showing you the fact that conventional theory will simply not work.[29]

This lengthy quotation affirms strongly the inadequacy of Salzer and Schachter's explanation of Bach voice leading. They fail to recognize a real cause for this astonishing harmonization. They do not give a purely musical answer to the "why" of this harmonization; rather, they appeal to undefined, aesthetic terms such as "climactic" and "smoothness". Babbitt provides his own explanation for Bach's awkward voice leading. Measure 9 is a direct motivic parallel to measure 1:

> "The harmonies on the second and third beats of measure 9 are exactly the same with regard to E as those in measure 1 are with C, literally the same harmonies, same relation. The structures are literally identical and then go up to exactly the same place."[30]

Fig. 2b illustrates Babbitt's analytical observation: the voice leading in the bass and soprano voices of m. 9 is identical to that of m.1 (excepting the initial bass note), within their respective key areas of A-minor and C-major. Bach is highlighting the melodic parallel between these two measures by using the same voice leading and harmonies in both places.

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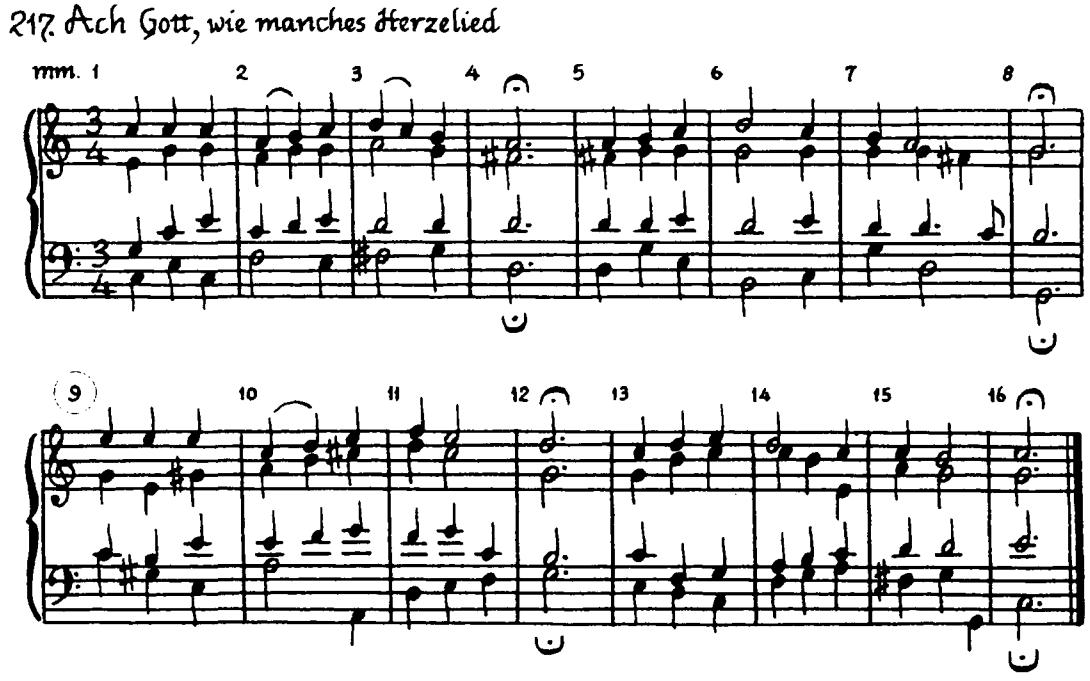
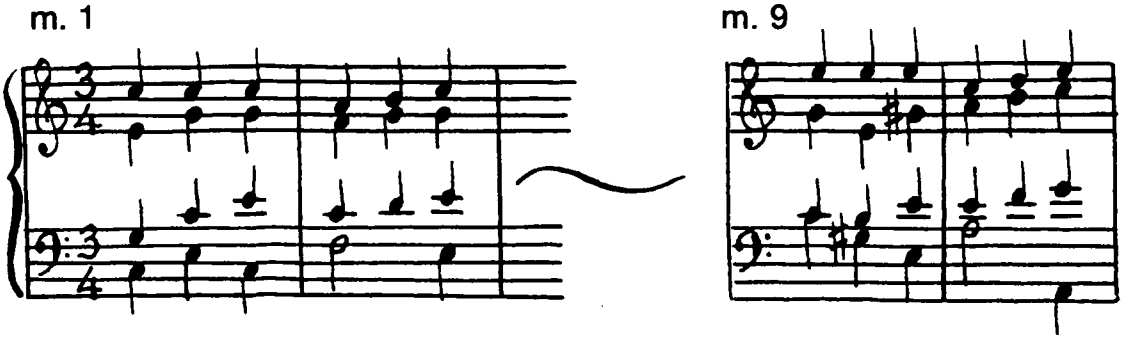
[29] Babbitt, "Professional Theorists and Their Influence," p.133.
[30] Ibid.

Figure 2.

a) Chorale Harmonization



b) Babbitt's Parallelism



Source:

Figure 2, (a) and (b): Milton Babbitt, "Professional Theorists and Their Influence," in *Milton Babbitt: Words About Music*, pp. 131 and 132.

At this point, we must be careful to establish clearly the exact terms of Babbitt's criticism of Salzer and Schachter's analysis. Babbitt does not attribute their so-called "missing of the boat" to a faulty Schenker sketch: rather, he asserts that Schenkerian analysis *per se* is simply not equipped to reveal such motivic parallelisms. As Babbitt describes it, "that's not his [Schenker's] schtick."[31] In other words, he never criticizes Salzer and Schachter's skill with Schenkerian analytical techniques. Instead, Babbitt simply points out that Schenkerian analysis does not include (to reiterate) "explicit discussion of motivic parallelism and the way in which a parallelism will induce parallelisms in other dimensions."

For all its merits, then, there is a level of explanation that Schenkerian analysis does not touch; whether this be on a higher or a lower plane is not important for the present. The interest here lies in the fact that even a perfectly crafted Schenker sketch cannot account for everything in a tonal piece of music: it focuses on certain aspects of the music and passes over others. Salzer and Schachter's attempted explanation for the awkward voice leading seems fumbling and inadequate for this reason alone: it does not fit within the framework of Schenkerian analysis. But then, explanation of motivic relations of this sort is not among the goals of a Schenkerian analysis. Let us recall Babbitt's discussion of Jadassohn's *Tristan* analysis, for it makes a similar point. The interpretation that places fourteen different keys in the first twelve measures of the music was indeed a well-formulated statement in the accepted standards of explanation at the time of Jadassohn's writing:

> This for him [Jadassohn] was an explication in terms of the received normal theory of his time. Once you can locate things in tonalities, a piece was explicated and therefore satisfactory and therefore, I assume, a coherent piece of music. Now his

[31] Babbitt, "Professional Theorists and Their Influence," p. 141.

> explication may seem to many of you a representation of chaos rather than of coherence. The fact of the matter is that it was satisfactory for him.[32]

In other words, Jadassohn's explanation is complete within its own system, even though higher levels of explanation certainly are possible. In the same way, Salzer and Schachter's analysis is satisfactory within the Schenkerian framework, despite the fact that it cannot account for Bach's strange voice leading. As I mentioned before, this important qualification serves as a connection between this lecture and Babbitt's formal essays. In essence, the explanatory powers of any analytical method must be assessed from within its own framework, its own intellectual context.

In this restricted context, we may introduce the term *sufficient* as a potentially concise and useful way to characterize different levels of analytical explanation: e.g. one type of explanation may be more sufficient than another. The term acquires particular significance because it transcends the bounds of particular analytical frameworks: it remains generalized between analytical contexts. When we apply the qualifier *sufficient* to a Schenker sketch, for example, we are examining the general character of explanation it achieves rather than a more or less correct use of the particular method, what I refer to as mechanics. Babbitt says exactly this in a statement that we have seen above that, nevertheless, bears repetition here:

> One could assume either that Salzer has misinterpreted Schenker and therefore it's not a good Schenker analysis—which interests me about as little as anything in the world—or that his is simply a mistaken way to view the piece, which interests me very much.[33]

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[32] Babbitt, "Professional Theorists and Their Influence," p. 146.
[33] Ibid. p.134.

Sufficient, in Babbitt's framework, applies to levels of explanation and not to mechanical procedure. The term encapsulates concisely the problem that Babbitt painstakingly articulates in this lecture. To frame this methodological problem as a specific question, we may say: "Is Babbitt's identification of the motivic parallelism in *Nun ruhen alle Wälder* a more sufficient explanation of this moment in the piece than Salzer and Schachter's Schenker sketch?" Indeed, this is a very different sort of question than: "Is Salzer and Schachter's analysis a sufficient use of the Schenkerian analytical method?" This latter question deals only with mechanics, the particular and functional use of Schenkerian analytical tools. The former assumes accurate and correct application of the Schenkerian method in order to address the more interesting question of its explanatory value.

Having now established that music analysis can have different levels or kinds of explanatory power, our ultimate task remains—to find criteria by which we can identify and assess gradations within analytical explanation. Babbitt places these criteria within the purview of scientific method.

3. The Scientific Music Theorist: Discourse and System

In his formal essays, Babbitt begins to describe the level and kind of explanation a music analysis must achieve in order to be qualified as *sufficient*: i.e. scientific explanation. As such, Babbitt is concerned with what we may call a universally applicable understanding of explanation: a logical paradigm for explanation that exists apart from any specific context or object of investigation that we may use to evaluate any kind of analysis that seeks to explain instead of describe. Scientific explanation denotes only that: the "logical structure

of explanatory arguments."[34] (The particular structure of scientific explanation will become clear later on.)

The independence of scientific explanation from particular contexts of investigation makes Babbitt's invocation all the more significant. Without such a format, we have to content ourselves with question such as: "sufficient for whom?" or "sufficient for what time?" Within such a relativist framework, any analytical statement reverts back onto itself and the person making it: there is no critical necessity for inter-subjective agreement or external verification. We are forced to believe that Jadassohn's superficial assignment of fourteen keys in the opening of the *Tristan* prelude can stand on equal value with another analysis that answers the "why" of this music. We must admit that the traditional chord-based analysis gives us just as much information as a Schenkerian analysis.

Contemporary music theory, however, does not hold onto these beliefs in practice. Theorists constantly evaluate analyses and make judgements between them. Even if the precise terms of the evaluation are tacit or diverse, professional music theory thrives on a critical activity that selects between kinds of analysis according to degrees of value. Of course, the entire issue depends upon how theorists define this value. Whether we locate the value of an analytical method in scope, simplicity, audibility, interpretive richness, etc..., we must make this choice. Scientific method is simply one interesting answer to this question of value.

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[34] Hempel and Oppenheim, "Studies in the Logic of Explanation," p.135.

At this point, we can reintroduce the critical limitation in the scope of the term *explanation* established at the outset of this thesis: explanation occurs within many distinct frameworks. Contemporary music theory is acutely aware of variations within explanation. Indeed, disagreements concerning the adequacy of musical explanation often verge on the violent, such as the acerbic exchange between Charles Smith and David Beach in *Music Theory Spectrum*.[35] At this point, Joseph Kerman's[36] comments will serve as an example of variation within explanatory frameworks. Kerman chastises music analysis for adhering excessively to unfounded, ideological notions of organicism, unity, and formal coherence, this while neglecting music's identity as an essentially humanistic endeavour. He clearly thinks that most structural analysis is in all likelihood no more than trivial description, falling short of uncovering the rich aesthetic content of music:

> Even the best of them [the "organic" content-based analyses] leave the reader uneasy. They come up with fascinating data and with undoubtedly relevant data; yet one always has a sinking feeling that something vital has been overlooked. For however heavily we may weight the criterion of organicism in dealing with the masterpieces of German instrumental music, we know that it is less important for other music that we value. This music may really not be "organic" in any useful sense of the word, or its organicism may be a more or less automatic and trivial characteristic. Its aesthetic value must depend on other criteria. Cannot a criticism be developed that will explain, validate, or just plain illuminate these other musical traditions?[37]

Value judgements aside, Kerman recognizes two different modes of analytical explanation: formal, systematic analysis rooted in German organicism, which privileges of unity and coherence, and aesthetic "criticism"—as Kerman calls it—that takes account of musical features beyond the scope of systematic formulation.

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[35] This exchange took place across the following articles: Charles Smith, "The Functional Extravagance of Chromatic Chords" *Music Theory Spectrum* 8 (1986): 94-139; David Beach, "On Analysis, Beethoven, and Extravagance: A Response to Charles J. Smith," *Music Theory Spectrum* 9 (1987): 173-185; Charles Smith, "A Rejoinder to David Beach," *Music Theory Spectrum* 9 (1987): 186-194.

[36] Joseph Kerman, "How We Got into Analysis, and How to Get out," *Critical Inquiry* 7 (1980): 311-331.

[37] Ibid. p. 320.

In the context of this thesis, the task of comparing and contrasting Kerman's two modes of explanation cannot enter into the picture. Such is the dual nature of music: an essentially aesthetic object, which nevertheless admits empirical and scientific kinds of investigation. In this thesis I make a particular choice: I investigate the explanatory powers of music analysis within formal systems of structural analysis. In so choosing I do not intend to discredit other, perhaps less formal, methods of analysis such as those Kerman evokes.

Matthew Brown echoes my own sentiments in the context of his investigation into Schenkerian analysis and the nature of Tonality:

> Some believe that theory building is an explanatory pursuit akin to the natural and social sciences, whereas others believe that it is a critical activity, analogous to art criticism or literary theory. As a result, some theorists deal exclusively with the internal properties of tonal music, whereas others insist that these properties cannot be studied apart from their cognitive, aesthetic, historical, and ideological context... I will proceed from the assumption that, before we can assess the cognitive, aesthetic, historical, and ideological implications of a particular theory, we must first see how that theory explains why tonal music behaves in some ways and not others. Since I believe that, at some level, we process our knowledge of music separately from our knowledge of other domains, I find it useful to treat music theory autonomously from other disciplines.[38]

In this thesis, I restrict the investigation to music theorists like Brown and Babbitt who argue that analytical sufficiency can be measured by adherence to the rigours of formal scientific methodology and language.

With these important restrictions restated, we move on to examine how Milton Babbitt answers the problem of sufficiency within analytical explanation by endorsing an analytical method that conforms to the model of scientific explanation. For Babbitt, this

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[38] Matthew Brown, *Explaining Tonality* (Rochester: University of Rochester Press), 2005. p.1-2.

conformance occurs through the systematic incorporation of two different elements: the linguistic formulation of analytical statements and the proper methodology required to produce an explanatory analysis within the framework of scientific method. I will address these points separately in the following sections of this chapter. Babbitt's scientific framework for music analysis is articulated primarily in the course of three formal essays: "Past and Present Concepts of the Nature and Limits of Music"[39] (1961); "The Structure and Function of Musical Theory"[40] (1965); and "Contemporary Music Composition and Musical Theory as Contemporary Intellectual History"[41] (1972).

This two-part interpretation of Babbitt's scientific framework finds external corroboration with Marion Guck, who speaks about Babbitt's framework according to the same divisions, i.e. linguistics and methodology. This corroboration is especially valuable since Marion Guck's own work, as she acknowledges, seems to stand in direct contrast to Babbitt's:

> My theoretical work is not objective or scientific. I didn't go to Princeton. And yet Milton Babbitt has influenced my thinking profoundly and pervasively... I will demonstrate its [Babbitt's methodological work] influence upon contemporary music theory by relating it to work, my own, that might have been thought antithetical to or even proscribed by his.[42]

Guck's analytical work shares an important kinship with Babbitt's methodological framework, but we will return to this relationship at a later point. For now, let us look at her summary of Babbitt's theoretical thought as an introduction to my own discussion:

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[39] Milton Babbitt. "Past and Present Concepts of the Nature and Limits of Music," in *The Collected Essays of Milton Babbitt*, ed. Stephen Peles (with Stephen Dembski, Andrew Mead, and Joseph N. Straus), 78-85. Princeton: Princeton University Press, 2003.

[40] Milton Babbitt. "The Structure and Function of Musical Theory," in *The Collected Essays of Milton Babbitt*, 191-202.

[41] Milton Babbitt. "Contemporary Music Composition and Musical Theory as Contemporary Intellectual History," in *The Collected Essays of Milton Babbitt*, 270-308.

[42] Marion Guck, "Rigors of Subjectivity," *Perspectives of New Music* 35 (1997): p. 53-54.

> Let me begin by outlining the ideas of Babbitt that, in my view, have been most influential. First and most fundamental is Babbitt's subject, which was how music scholars use language. Just that. So far as I know, Babbitt is the first to have noticed that it might matter what is said about music and how-that there are choices and consequences. Succeeding methodological critiques, including that currently underway in musicology, flow from his first addressing issues of the use of language in relation to music. Babbitt's methodological work famously insists that only "'scientific' language and 'scientific' method" make "meaningful discourse" possible. The invocation of "'scientific' language and 'scientific' method" responded to the state of musical discourse at the time of its writing; as the examples he provided demonstrate, writing about music could be self-indulgently impressionistic and ill-informed. Babbitt's writing advocates that a more stringent standard of intellectual discipline be met in serious writing about music. It asks that writing be precise and that writers provide evidence for the claims they make. An author should make it possible for a reader to understand and evaluate for him- or herself that author's claims.[43]

Here, Guck asserts that Babbitt's influence in the field of music theory and analysis has been two-fold: for Babbitt, scientific language and method make meaningful musical discourse possible. Babbitt himself says exactly this in an uncharacteristically clear formulation:

> ...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," whatever that may be assumed to mean, but simply that statements about music must conform to those verbal and methodological requirements which attend the possibility of meaningful discourse in any domain.[44]

Having established this conceptual division within Babbitt's writing, I will work through Babbitt's theoretical framework as adopted in the formal essays by addressing the following points: 1) the necessity and value of precise, scientific language for music analysis; 2) the consequences that such linguistic restrictions hold for analytical methodology; 3) the relationship, or interaction between music theory and scientific method

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[43] Guck, "Rigors of Subjectivity," p. 54.
[44] Babbitt, "Past and Present Concepts," p. 78.

in general. Through these points, Babbitt's notion of a sufficiently explanatory analysis takes shape.

It should be noted that Babbitt never elaborates on his conception of scientific method or scientific explanation in general. He uses the concepts a great deal but assumes that his readership is familiar with them. Following Babbitt's own framework, I leave a more explicit discussion of scientific method to the next chapter of the thesis. Here, however, I will reveal certain aspects of scientific method and explanation that are made clear in Babbitt's comments, for it is interesting to see exactly what we can take from Babbitt without outside knowledge of the logical form of scientific explanation. Therefore, in this chapter I will discuss Babbitt's framework in isolation: important clarifications will arrive in the next chapter, as well as in the chapter on Matthew Brown and Douglas Dempster. In the latter, Babbitt's understanding of scientific explanation will be solidified through a comparison of his own framework with Brown and Dempster's.

Scientific Language

The first component of Babbitt's framework for scientific method within music theory and analysis is a restriction upon language: significant observations about music can be properly formulated only in scientific language. This is a perplexing requirement. For we must ask ourselves: "what in fact is *scientific language*?" Babbitt's understanding of scientific language and its value is encapsulated in this precept: scientific language provides *meaningful* analytical discourse. We remember that Babbitt speaks of scientific language as a requirement for "meaningful discourse in any domain." In turn, scientific language

achieves this "meaningfulness" in two interrelated ways: 1) it is testable; and 2) it is communicative, or intersubjective. The intersubjective character of scientific language rests upon its testability, its ability to be justified or falsified. Characteristically, Babbitt never directly says what scientific language is. We can surmise Babbitt's answer only through his illustration of what scientific language is *not*.

In the middle of a discussion, or perhaps more appropriately a tirade about analytical language, Babbitt references an analysis by Hans David.[45] Sparing no critical edge, Babbitt condemns the undefined terms of David's analysis:

> And what of the more apparently factual scholarly statement that the C-flat of measure 53 of the second movement of the Mozart G Minor Symphony (K. 550) is "an unexpected C-flat"; overlooking for the moment the dubious status of such expressive descriptives, what can the term "unexpected" be inferred to designate when applied to the succession B-flat, C-flat, which had been stated in the movement in question at the outset in measure 2? If nothing else, such verbal phenomena would appear to be instances of the situation characterized by Quine's conservative observation: "The less a science is advanced, the more its terminology tends to rest on an uncritical assumption of mutual understanding."[46]

The offensive, un-scientific term in this passage, obviously, is "unexpected." For Babbitt, this qualifier is essentially meaningless in that it is not explicitly defined: its final significance lies tacitly within the mind of the analyst. In other words, Babbitt is complaining that nobody knows just what the term *unexpected* is intended to mean in this context besides Hans David himself. The last part of the quotation above gives the key to understanding what scientific language is—as the opposite of Quine's observation. Scientific language is an explicit terminology in which nothing is taken for granted, in which

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[45] In Babbitt's text, the author of the analysis he is criticizing remains anonymous. Subsequent investigation, or perhaps Babbitt's own private admission, has revealed that this passage comes from Hans David's "Mozartean Modulation," in *The Creative World of Mozart*, ed. Paul Henry Lang.

[46] Babbitt, "The Structure and Function of Musical Theory," p. 192.

nothing is left unsaid: i.e. it avoids "assumptions of mutual understanding." (Scientific language does not necessarily entail abstract or formulaic assertions, as we may have expected initially; our analyses do not have to read like a physics or chemistry textbook.)

Scientific language, therefore, denotes statements that are fundamentally testable. For what else is an assumption than a statement that we do not examine, that we do not put to the test? Scientific language makes all the terms of an argument freely available for third party assessment. Babbitt points out the dire consequences of an analysis that rests on assumptions:

> But the problems of our time certainly cannot be expressed in or discussed in what has passed generally for the language of musical discourse, that language in which the incorrigible personal statement is granted the grammatical form of an attributive proposition, and in which negation—therefore—does not produce contradiction; that wonderful language which permits anything to be said and virtually nothing to be communicated.[47]

Only one kind of statement separates negation from contradiction: the statement that is neither true nor false, the untestable, or incorrigible statement. Language that depends on uncritical assumptions of meaning, on un-scientific language, falls into this category.

In David's analysis, the final significance of an assumptive term rests only with the analyst, and by extension therefore, the receiver of the analysis. I may agree with David that Mozart's Cb is indeed unexpected, but this does not corroborate, or justify the analysis. Since the term is undefined, I can only assume, with no real cause, that my notion of *unexpected* is the same as David's. Conversely, if I do not agree with David, I have not necessarily falsified his analysis either: for negation is only operable under a single set of

[47] Babbitt, "The Structure and Function of Musical Theory," p. 192.

terms. Put simply, two people cannot have a true argument unless they are talking about the same thing. Again, since the term *unexpected* remains undefined, we cannot truly say anything to contradict him. David's analysis is fundamentally incorrigible.

Babbitt's comments here are in fact a more extensive restatement of a point he raises in an earlier essay, which bears citation here:

> ...it is neither surprising nor singular that, casually and noncontroversially, a hypothetical, but cautiously unexaggerated instance of "musical criticism" is cited on the first page of an elementary discussion of language as "sheer nonsense" when "interpreted literally." The content of this specific example is of no consequence, except to the extent that it shares with the majority of past and—admittedly—present "statements" about music the property of being at best an incorrigible statement of attitude grammatically disguised as a simple attributive assertion. If taken at its face value, then it creates inevitably a domain of discourse in which negation does not produce contradiction, and in which a pair of such assertions entails, in turn, any statement and its negation.[48]

Babbitt is lamenting the prevalence of unfounded analytical claims about music that pose as meaningful statements: incorrigible statements taken as attributive assertions. The passage of "musical criticism" is assessed as "sheer nonsense"—we can assume—because its very language does not carry any truly meaningful significance. Any such "incorrigible statement of attitude," by definition, cannot convey any meaning to anyone but the person who makes it.

For Babbitt then, scientific language is meaningful because it does not rely on assumptions: it is testable rather than incorrigible. We might say that incorrigible statements are so replete with meaning that they are in fact meaningless: if a statement conveys something different to everyone because of a lack of definition, then it means anything, and

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[48] Babbitt, "Past and Present Concepts," p. 79.

therefore, nothing at all. Ultimately, this criterion of testability provides for that larger conveyer of meaning we call intersubjectivity. We have already seen Babbitt speaking of this final purpose when he describes that "wonderful language which permits anything to be said and virtually nothing to be communicated." Indeed, testability goes a long way to insure the viability of mutual understanding. The same comments concerning David's analysis apply here as well. Regardless of whether I agree or disagree with David, his analysis can never be truly intersubjective since its receiver can never be sure of its exact content. As an antidote to this situation, we may speculate perhaps that David could have offered a set of precise musical conditions and situations that define and create musical *unexpectedness*.

Of course, Babbitt is not proposing some novel approach to analysis. He is merely asking that analysts be careful about what they say and how they say it. He asks us to constantly scrutinize and temper our language so that we can be sure to say significant things about music. The linguistic formulation of our statements can provide significance, or meaningfulness. Babbitt's larger concern here is that musicians should not be allowed to practice sloppy, uncritical language simply because are they are dealing with an aesthetic object. Musicians should not be exempt from responsible discourse:

> The composer who insists that he is concerned only with writing music and not talking about it may once have been, may still be, a commendable—even enviable—figure, but once, he presumes to speak or take pen in hand in order to describe, inform, evaluate, reward, or teach, he cannot presume to claim exemption—on medical or vocational grounds—from the requirements of cognitive communication. Nor can the performer, that traditionally most pristine of nonintellectuals, be permitted his easy evaluatives which determine in turn what music is permitted to be heard, on the plea of ignorance of the requirements of responsible normative discourse. Nor can the historian, in the sanctified name of scholarship, be allowed such a verbal act as the following: "There can be no question that in many of

> Mendelssohn's works there is missing that real depth that opens wide perspectives, the mysticism of the unutterable."[49]

As obvious as Babbitt's comments concerning analytical language may seem, however, their practical application is surely more difficult than expected. Can we as analysts actually and practically excise all unqualified assumptions from our work? Can we make our analytical language truly scientific in Babbitt's sense?

We can as long as we work practically within a received body of theoretical concepts. We can assume definitions of things that exist in larger theoretical concepts with which the receivers of the analysis are familiar. But more importantly, I believe that Babbitt's scientific language requirements are easier to fill if we leave out elements from our analyses that resist definition. Of course, the most likely candidates for this category are aesthetic observations about music. Think back again to David's "unexpected" Cb in the Mozart symphony: this is an essentially aesthetic comment since it describes music's expressive effect. We can make this statement scientific, i.e. testable and meaningful, if we posit that the particular note in question conforms to a set of conditions that define and create musical unexpectedness. But can we really do that? Can we, or even should we, define such elusive and aesthetically pleasing musical properties?

It is possible that these questions have no answer. I raise them to point out only that Babbitt's requirements for scientific language assume exclusively music analysis in an empirical domain. Obviously, empirical, i.e. observable, statements about music are much

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[49] Babbitt, "The Structure and Function of Musical Theory," p. 192.

more amenable to concrete demonstration and testability. In this way, then, Babbitt's prescription of scientific language has more of "science" about it than we initially may have considered. His linguistic requirements do not reduce simply to offering reasons for whatever one says: perhaps Babbitt is suggesting that the only truly significant statements about music are empirical ones. Empiricism is customarily the domain of scientific method, the subject of the next section on methodology.

Scientific Method

In the previous section, we looked at Babbitt's comments concerning the first half of his scientific framework for music theory and analysis, i.e. scientific language. With its testable, and thereby, truly intersubjective statements, scientific language provides semantic significance to music analysis; however, it goes no farther than that. Obviously, semantic meaningfulness does not automatically guarantee explanatory significance. Babbitt recognizes as much himself in this passage asserting the need for a standard or scale by which analysts can choose from all the available statements about a piece of music:

> Another facet of the same question, a facet which has made possible a great many of the analyses by which we find ourselves confronted at the moment and which perhaps underlies our dissatisfaction with a great deal of traditional theory, is that there are an infinite number of true (or false) statements that can be made about any composition, and—therefore—any collection of compositions.[50]

Without some guide in choosing between any number of potential scientific statements about a piece of music, we have no way of knowing which ones are more explanatory or significant than others. This is where scientific method steps in: it determines how we

[50] Babbitt, "The Structure and Function of Musical Theory," p. 194.

arbitrate between analytical statements. It is in essence a method, or scale for determining significance of selection among a host of possible analytical comments.

In this section, I will reveal Babbitt's criteria for the application of scientific method to music theory and analysis, the other half of his "famous duality."[51] Ultimately, his framework hinges upon two crucial methodological points: 1) pure empiricism; and 2) the choice of systematic primitives, or axioms. Babbitt's framework is delineated in the course of his two essays "The Structure and Function of Musical Theory," and "Past and Present Concepts of the Nature and Limits of Music." As a concrete example of his framework in action, I will introduce Babbitt's discussion of the twelve-tone system and Lerdahl and Jackendoff's description of their Generative Theory of Tonal Music." This section of the thesis still endeavours to expose Babbitt's scientific framework in isolation. The real significance of his statements will become more clear as we progress through the next chapters.

Before moving directly into this topic, however, we must recognize that the distinctions between theory and analysis here become tenuous. In one sense, music theory and analysis are inextricably intertwined in as much as they are mutually dependent. The principles of musical theory determine and justify analytical activity, and individual analytical statements are the building blocks for the formulation of a general theory.

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[51] Marion Guck, "Rehabilitating the Incorrigible," in *Theory, Analysis, and Meaning in Music*, ed. Anthony Pople, (Cambridge: Cambridge University Press), 1994. p. 73.

We notice this relationship in action if we ask ourselves a few simple questions about how we investigate music. For example, we may inquire concerning any given analysis what in fact it is an analysis of. We would not want to answer simply: "it is an analysis of X piece of music"; instead, we would prefer to say something like: "this is an analysis of piece X's large-scale tonal structure using Schenkerian concepts of prolongation"; or, "this is an analysis of how the permutational properties of X piece's set structures create large scale formal divisions." Music analysis gains power and significance only in relation to some received theory. Similarly, any music theory depends upon individual analytical statements, the data, empirical or otherwise, that makes up its content. Any consistent theory of music must come directly from analytical observation, as Babbitt suggests in this passage:

> ...presumably it can be agreed that questions of musical theory construction attend and include all matters of the form, the manner of formulation, and the significance of statements about individual musical compositions, and the subsumption of such statements into a higher-level theory, constructed purely logically from the empirical acts of examination of the individual compositions.[52]

If otherwise, we have constructed a theory about music that does not come from music itself, i.e. a self-confirming, even circular cognitive framework.

This understanding of the relationship between theory and analysis receives corroboration from many scholars. For example, this relationship is the essence of Eugene Narmour's harsh attack of Schenkerian analysis. In *Beyond Schenkerism*, Narmour accuses Schenker of positing the existence of the *Ursatz* as a concept apart from analysis, as opposed to deducing the *Ursatz* from analytical observation. He restates Schenker's conception of

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[52] Babbitt, "The Structure and Function of Musical Theory," p. 191.

the *Ursatz* to expose the supposed logical fallacy[53] of circular reasoning that the *Ursatz* proposition commits:

> The logical fallacy may be stated as follows: If the *Ursatz* is a structural hypothesis, then it will be successful in analysis. If it is successful, then it must *be* the structure. Simply put, there is no separation between Schenker's epistemology and his methodology.[54]

The *Ursatz* represents circular reasoning since its existence is posited before, or at the same time as the analytical process that reveals it in operation as a principle of Tonality. In Narmour's opinion, the *Ursatz* is no more than a theoretical conceit with no real basis in empirical observation.

Similarly, David Lewin expresses the need for an empirical theory to receive confirmation within the data of individual pieces of music. One cannot have a theory without some object for it to be a *theory of*:

> [Theory has manifested itself as] an empirical appeal to the practice of great composers. One would hardly be interested, as a musician, in any "theory" purporting to cover a body of literature, if one could not recognize the pertinence of the theoretical notions to the music under consideration. We would not be interested in "Tonal theory" if we did not hear triads, scales, keys, et al. functioning in the music of Bach, Mozart, et al.[55]

In Matthew Brown's words, analysis is always "theory laden"[56]: that is, we cannot presume to achieve objective or autonomous analysis, i.e. uninvolved with a certain larger theoretical conception. Within my present framework, then, I submit that anything Babbitt has to say about scientific method in terms of music theory applies in kind to music analysis. This

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[53] The validity of Narmour's argument is irrelevant for the present purposes and therefore I do no address it directly.

[54] Eugene Narmour, *Beyond Schenkerism: The Need for Alternatives in Music Analysis* (Chicago: University of Chicago Press), 1977. p. 29-30.

[55] David Lewin, "Behind the Beyond: A Response to Edward T. Cone," *Perspectives of New Music* 7 (1969): p. 62.

[56] Matthew Brown, "'Adrift on Neurath's Boat': The Case for a Naturalized Music Theory," *The Journal of Musicology* 15 (1997): p. 332.

framework is especially applicable to Babbitt's thoughts: for as we saw above, Babbitt directly states that any theory of music arises directly from empirical observations about music, i.e. scientific analysis.

A Primitively Empirical Music Theory?

Babbitt uses the terms *empirical* and *primitive* as keys elements of a scientific music theory and analysis. Indeed, he identifies precision in articulating these two parameters as essential to the success of any musical theory:

> Since a satisfactory theory is a satisfactory explanation of aspects of the empirical domain with which the theory is concerned, the contemporary dissatisfaction with the great body of pre-twentieth-century "theory" (and much twentieth-century "theory") is a fundamental one, stemming from the basic inadequacies of this theory in (1) stating its empirical domain, and (2) choosing its primitives.[57]

Let us examine separately what Babbitt means by these two characteristics of a music theory that achieves satisfactory explanation. Firstly, let us refine our understanding of empiricism and examine its applicability to music theory and analysis.

Empiricism refers to an epistemological system founded on experience and direct observation: as such it involves no *a priori*[58] reasoning, assumptions or intuitions. Let us consider briefly two simple examples to illustrate this distinction. An empirical explanation for the formation of water centres around the observation and experience of the combination of hydrogen and oxygen in a ratio of 2:1 under necessary contingent circumstances. An anti-empirical argument explains water by appealing to some *a priori* cause that is not

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[57] Babbitt, "Past and Present Concepts," p. 79.

[58] This is a common designation in the study of logic. *A priori*, literally meaning "from before" or "prior," refers to knowledge of a purely intellectual character independent from experience. See: *Stanford Encyclopedia of Philosophy*, s.v. "a priori," http://plato.stanford.edu/entries/apriori/

directly observable but a proposition of pure intellectual reasoning: for example, this water exists "because God made it so." Of course, we cannot observe God's creative act in the same way as we can experience the physical combination of hydrogen and oxygen. Empirical knowledge is *a posteriori*[59]: i.e. based in experience. Therefore, when Babbitt mentions empiricism, he is simply talking about a musical theory that is built from direct analytical observations of musical events.

Let us refine the notion of the empirical argument further. Due to its foundation in observation, empiricism necessarily excludes everything metaphysical from explanation.[60] Metaphysical arguments are necessarily *a priori*; for in essence, anything that is not physical is not properly observable. Any empirical musical theory or analysis, then, must concentrate exclusively on what we may call the "physical facts" of music, such as pitch organization and relationships. Empirical music theory and analysis must deal with demonstrable aspects of musical structure, as opposed to metaphysical theories of musical meaning or symbolism.

The consequences for music analysis are profound. Excluding everything metaphysical, empirical music theory necessarily excises from its parameters any personal engagement with music, both emotive and evaluative. Aesthetic features of the music are out as well. Most properly, empirical music theory cannot include analytical statements such as "I like this music," or "this music is beautiful," or even "this passage creates tension in the listener." Statements such as these deal with sensible aspects of music which are not

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[59] Literally, "from behind" or "succeeding." See again: *Stanford Encyclopedia of Philosophy*, s.v. "a priori," http://plato.stanford.edu/entries/apriori/

[60] By metaphysical, I do not exclusively mean religious or spiritual, in so far as the study of metaphysics popularly entails a study of the knowledge we may have of God or other spiritual beings. In this context, we merely need metaphysical to denote its literal meaning, i.e. "above", as in "not a part of" the physical world.

observable: we cannot see or demonstrate musical beauty or tension—we can only feel it. This is not to say that feelings are less real or relevant: scientific analysis must only exclude them due to its empirical restrictions.

Since this methodological point exists in tandem with Babbitt's linguistic requirements we saw above, we can make further qualification to our understanding of scientific language. As we recall, scientific language provides analysis with semantic meaningfulness by making the precise terms of a statement explicit and therefore available for intersubjective corroboration. I also suggested at the end of that section that scientific language must itself be empirical in application, restricted to observable physical facts about music; for indeed, those statements that are most testable are those that can be directly observed. Relating Babbitt's language requirements to his methodological ones, we can now say for certain that scientific language must properly be empirical within this framework. I cite again Babbitt's observation that an empirical music theory is built from analytical statements about music:

> ...presumably it can be agreed that questions of musical theory construction attend and include all matters of the form, the manner of formulation, and the significance of statements about individual musical compositions, and subsumption of such statements into a higher-level theory, constructed purely logically from the empirical acts of examination of the individual compositions.[61]

Babbitt's methodological requirement of empiricism is indeed intimately bound up with his linguistic prescriptions, so much that we can only understand them fully in conjunction.

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[61] Babbitt, "The Structure and Function of Musical Theory," p. 191.

In this next passage, Babbitt himself asserts this essential connection between language and method:

> In other words, musical theory must provide not only the examination of the structure of musical systems—familiar and unfamiliar by informal conditioning—as a connected theory derived from statements of significant properties of individual works, a formulation of the constraints of such systems in a "creative" form (in that, as language grammar does for sentences, it can supply the basis for unprecedented musical utterances which, nevertheless, are coherent and comprehensible), but—necessarily prior to these—an adequately reconstructed terminology to make possible and provide a model for determinate and testable statements about musical compositions.[62]

Here Babbitt, characteristically of his writing style, encapsulates everything we have been speaking about so far into one extended thought. An empirical music theory examines the physical facts of music (stated above as "the structure of musical systems") and derives its system from a series of "determinate and testable" (i.e. scientific) statements about compositions.

The ultimate consequence for our understanding of Babbitt's scientific language is this: scientific language does not boil down merely to offering empirical reasons for what one says. Because of the restriction upon methodology Babbitt has made, *the original statement must be empirical itself.* Therefore, scientific music analysis cannot strictly offer a statement such as: "This passage makes the listener feel tension because it uses unresolved, highly chromatic chord progressions." The original assertion of "tension" is still only of a sensible property, not of a properly observable one. The attempt at empirical justification does not redeem the metaphysical nature of the original statement.

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[62] Babbitt, "The Structure and Function of Musical Theory," p. 191.

Returning to David Lewin's essay, we realize that a compounding of physical evidence does not make a critical[63] assertion any more empirical itself. In Lewin's opinion, theory and analysis do not support an aesthetic observation, but only qualify it:

> Theory and analysis cannot be used to *support* a critical judgment. But they *can* be used to qualify it, to sharpen and develop one's own discrimination, and to communicate critical experiences to others. For instance: I don't like Brahms' Op. 76, No. 4; I do like the first movement of Bach's Sixth Brandenburg. What I don't like about the Brahms involves the lateness of the tonic arrival at m. 45, and my feeling that the 101/2 following measures of tonic prolongation are inadequate, both thematically and (more important, I think) rhythmically in the large, to resolve the tension set up by the enormous and complicated dominant elaboration up to that point. What I do like about the Bach involves the interplay between the very tight, square foreground rhythms and the very relaxed, spectacularly asymmetrical large harmonic rhythm. My invoking such analytical and theoretical terminology does nothing to prove that my critical judgments are "right"; it simply expands on the subject of *what* I don't/do like in these pieces.[64]

Understanding Babbitt's scientific music analysis with Lewin's corroboration, then, one cannot say whatever one wants: language and methodology must be restricted to the empirical domain in a closely knit bond. Once these restrictions concerning empiricism are in place, a music analysis is half way down the road to conformance with Babbitt's conception of scientific method.

Our next task in this section is to examine what Babbitt means by *primitives*. Literally, *primitives* means "first things," deriving from the Latin adjective *primus*—or "first." Things that are primitive come first, or relatively, before other things. The term *primitive*, then, always indicates a relationship of priority, whether temporal or hierarchical. For example, we can speak of primitive cultures or peoples to indicate a temporal

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[63] In the context of his essay, Lewin's understands criticism as an account of music's aesthetic features and the listener's experience of them: "CRITICISM. Here we have, at the basic level, Cone's 'I like it' or 'I don't like it,' and, for a slightly more sophisticated listener, 'this engages me' (whether or not I like it) and 'this bores me.'" See: "Behind the Beyond," p. 64.

[64] Ibid.

relationship with our own societies: the ancient Egyptian civilisation is a primitive culture.[65] Things or concepts can be primitive in a hierarchical scheme as well. For example, the property of tonality is primitive in relationship to Roman Numeral chord labelling: the tonality of a piece of music must exist prior to any analytical activity that depends upon triadic functionality. We cannot fruitfully apply Roman Numeral labelling to serial or twelve-tone music that is not constructed with chords as functional vertical sonorities. Tonality as a theoretical and structural concept must exist and be understood before the Roman Numeral analytical system. It is this hierarchical sense of *primitive* that is most important for Babbitt, and therefore for us as well.

In an explanatory system, the primitives are those things or concepts that form foundations for explaining other things or concepts. A widely known illustration of the function of explanatory primitives is Euclidian geometry. Before constructing his geometric propositions, Euclid begins with a set of definitions and postulates that determine the process and nature of his geometry. By starting from the definition of a geometric point, Euclid progressively builds the system until he is able finally to construct three-dimensional solids.[66]

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[65] The popular connotation of *primitive* culture as one "less advanced" than our own is a logical extension of the temporal relationship but is not an essential quality. For instance, we may call earlier civilisations primitive because their technology was not as advanced as our own: perhaps some early societies did not enjoy the luxury of a central plumbing system providing running water to individual homes. The ancient Roman civilisation had central plumbing systems, yet they are still primitive in a temporal relationship to our own time.

[66] Definition 1 from Euclid's Elements: "A *point* is that which has no part." See the online version of the Elements: http://aleph0.clarku.edu/~djoyce/java/elements/toc.html

Without taking recourse to scholarly sources that describe this logical system of explanation, we can determine the function of primitives from within Babbitt's articles. In this passage, Babbitt speaks of primitives as explanatory within a hierarchical system:

> The second inadequacy [i.e. choosing primitives] reflects what has long been an occupational malady of theorists of music...Thus, Mersenne—pursuing the perennial "why" of the correspondence between the interval content of the major triad and the first six divisions of the vibrating string—supplies a characteristic "justification" for the "use" of but six by citing the numerical identification with the then-known number of planets. Beyond the intimations of the cosmic scope and affinities of music, there is the implication that classes of objects hierarchically "justify" others...The unclear theoretical status of this admittedly tentative suggestion in Mersenne's theory still leads one to conjecture as to whether, in all seriousness, the discovery of a seventh planet invalidated the theory or the music founded upon the assumption of the "incorrect" number.[67]

In Mersenne's[68] system, the number of planets functions as an explanatory primitive: it is a hierarchically prior concept that purports to explain why the notes of a major triad are composed from the first six divisions of a vibrating string. As Babbitt points out, the number of planets functions with a hierarchical system of explanation for Mersenne: "there is the implication that classes of objects hierarchically 'justify' others." Regardless of the ridiculous status of the planets as an explanatory primitive for musical design, we see an illustration of the concept here.

To choose primitives, then, is to decide what concepts or objects have hierarchical priority within a formal system of explanation. For Mersenne, the number of the planets has priority. In the Roman Numeral analytical system, Tonality has the priority of a primitive concept. Most important for us is to realize the significance that Babbitt places upon the

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[67] Babbitt, "Past and Present concepts," p. 80.

[68] The editors of *The Collected Essays of Milton Babbitt* provide the following information: "Marin Mersenne (1588-1648) was a French Mathematician with an interest in music theory." "Past and Present Concepts," note 9, p. 84.

choice of primitives for an explanatory musical system. For Babbitt, the formulation of the explanatory primitives determines the significance, or sufficiency of particular analytical statements. This is what we have been searching for all along. In this passage, Babbitt speaks of this role of primitives in an explanatory system:

> Putting aside formalities, only because the result of not putting them aside is known and herein represented, any theory is a choice from an infinite number of possible theories, and *the choice is determined by what can be termed a criterion of significance in the selection, first, of primitives, whatever the linguistic form of these primitives. Whether this significance be expressed in terms of predictive power, explanatory scope, simplicity, or some other criterion, the decision is not easily made or ever surely made*, which is only to state that an empirical theory is subject always to revision and reformulation. The question of significance arises as soon as one seeks to formulate concepts for the analysis of an individual piece founded on the notion of, say, pitch. One cannot be at all certain that any concept is necessarily a fruitless or an absurd one; one simply does not know if it is or not until one has tested the results of the application of this concept for correlation with other independent concepts, for invariance under nonvacuous conditions.[69]

Here, Babbitt explicitly establishes the choice of theoretical primitives as a way to determine significance within an analytical system. The final significance Babbitt leaves open to specific formulation (i.e. "predictive power, explanatory scope, simplicity, or some other criterion"); but this open-ended framework does not absolve the theorist or analyst from either making the choice of primitives or deciding which pre-established set of primitives with which to work.

Instead of devising fictional examples for this relationship of primitives to significant analytical statements, we can turn to the parameters that Lerdahl and Jackendoff set for their "Generative Theory of Tonal Music." At the outset of their work, Lerdahl and Jackendoff define explicitly their choice for significance within the theory. The following selection of

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[69] Babbitt, "The Structure and Function of Musical Theory," p. 194. Italics added

quotations show that they privilege the description of idealized musical intuitions in structural terms and predictive power:

> We take the goal of a theory of music to be a formal description of the musical intuitions of an educated listener. By "musical intuitions" we mean the largely unconscious knowledge which a listener brings to music and which allows him to organize musical sounds into coherent patterns...Our theory is concerned not with particular instances of hearing, which are always subject to a degree of variability, but with the idealized underlying competence which the educated listener brings to bear in understanding tonal pieces.[70]

> Indeed, our way of thinking about music is patterned after the methodology of linguistics in that we demand strong motivation, formal rigor, and predictive power for every part of the theory.[71]

Furthermore, Lerdahl and Jackendoff carefully define the primitive concepts of their theory to which the analytical system must conform. The call these primitives "well-formedness rules," and "preference rules," whose own significance stems from principles in the field of psychology:

> We have found that a generative music theory must not only assign structural descriptions to pieces, but must differentiate the structural descriptions along a scale of coherence, weighting them as more or less "preferred" ways of hearing a piece. Thus, the theory is divided into two distinct parts: well-formedness conditions, which specify possible structural descriptions; and preference rules, which designate, out of the possible structural descriptions, those that correspond to the educated listener's hearing of any particular piece.
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> There are various criteria for determining which structural descriptions of a piece are "preferred." Among these, of course, are our own intuitions. Furthermore, beyond all seemingly self-evident intuitions, a "preferred" structural description will tend to relate otherwise disparate elements in a satisfying way and to reveal surprising analytic insights. In addition, we are aware of relevant research in experimental psychology and are concerned that its findings be in agreement with our theoretical constructions. Criteria within the theory itself include the internal consistency of the rules and their generalization from particular instances to the entire body of classical tonal music. We utilize within the framework of the theory such psychologically primary notions as parallelism, articulation, and stability. Finally, the nature of our structures must be psychologically plausible in that their

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[70]Fred Lerdahl and Ray Jackendoff, "Toward a Formal Theory of Tonal Music," *Journal of Music Theory* 21 (1977): p.111-112.

[71] Ibid. p.112.

> complexity must result from the interaction of a fairly small number of processes, each of which taken in isolation is relatively simple.[72]

The structure of Lerdahl and Jackendoff's system is wonderfully clear. It also illustrates the explanatory significance of theoretical primitives that Babbitt requires. Each analytical statement within their system derives its significance from the primitive rules they set up; and in turn, these rules gain significance from their predictive power and their ability to explain the coherent musical structures they take to be inferred by "educated" listeners. Lerdahl and Jackendoff have taken the difficult yet necessary step of choosing and defining systematic primitives. The ultimate propriety or success of their primitives is irrelevant for the present. Our purpose here is not to criticize their theory: we need only realize that they use primitive concepts as hierarchical bases for explanation within their own framework. In essence, Lerdahl and Jackendoff are creating a tonal musical theory from scratch. Therefore, the necessity of defining that theory's parameters and primitive concepts is paramount.

Returning to Babbitt, we find another example with what he takes to be one possible primitive concept for explanation within the twelve-tone system. Not surprisingly, this concept defines certain permutational properties of any twelve-tone set and relationships between them:

> It is well known that it can be shown easily that the rules of formation and transformation of the twelve-tone system are interpretable as a defining group element (a permutation of order or set numbers) and a group operation (composition of permutations)... The theorem states, in terms of the twelve-tone system, that two transposed set forms which are complementary with regard to a third have the same

[72] Lerdahl and Jackendoff, "Toward a Formal Theory of Tonal Music," p.113-114.

> number of order inversions (and therefore, permanences) with relation to this third set.[73]

For Babbitt, this theorem can be a primitive, explanatory concept since it generalizes a necessary characteristic of the twelve-tone system. It states a general cause for specific compositional relationships and predicts future relationships between set transformations:

> This property is explanatory in that it explains the compositional use of such related sets, and—by extension—suggests more general applications of complementation, as in the case of the operation of inversion. This property also functions as predictive in determining possible attributes of future works concerned with exposing this property.[74]

In practical application, then, an analytical statement that demonstrates this general property derives its significance from the status of that same property as a primitive concept within an explanatory system for twelve-tone composition. In this way each analytical statement made derives its explanatory power from a more general concept upon which it depends. This is the very opposite of the *ad hoc* statement.

Even though Babbitt does not commit to one form of significance for explanatory primitives (scope, prediction, etc...), we can say definitively that primitives in Babbitt's framework must be empirical in quality. Babbitt requires that primitives be formed through direct acts of empirical analysis. He says as much when, as we have seen already, he asserts that a musical theory is the result of generalizing empirical analytical observations.

Babbitt's understanding of scientific method, then, consists in two different elements: empiricism and explanatory primitives. Music theory and analysis must stay within the realm of observable and demonstrable musical events; also, music theory must contain

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[73] Babbitt, "The Structure and Function of Musical Theory," p. 199.
[74] Ibid.

primitive concepts arranged hierarchically within a formal system that lend significance and explanatory power to individual analytical statements.

As satisfying as this summary is, we cannot help but experience some disappointment. Unfortunately, Babbitt does not go further: he is not willing to offer his opinion as to the actual content of such explanatory primitives for a musical theory, and he does not even decide how their significance should be determined. The most Babbitt accomplishes is a criticism of theories that have made the perilous choice. For instance, he takes great length to debunk the status of the overtone series as a primitive concept for music and finally decides that it cannot function as such.[75]

On the other hand, Babbitt's open-ended framework is also attractive for this same aspect. If the formulation of explanatory primitives is somewhat free, then a theory or analysis can choose to give significance to any facet of music. Consider for example the difference between Babbitt's twelve-tone primitive and Lerdahl and Jackendoff's theory. The "Generative Theory of Tonal Music" gains ultimate explanatory significance from its formal description of musical hearings based in certain principles of psychology. Lerdahl and Jackendoff's theory, therefore, does not satisfy Babbitt's strict empirical requirements; however, it operates within the same explanatory system as Babbitt's twelve-tone formula. Comparing Babbitt with Lerdahl and Jackendoff reveals that scientific explanation can transcend the boundaries of individual theoretical frameworks: it serves a general logical format for explanation within which various particulars can be inserted.

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[75] Babbitt includes criticisms of the overtone series as an explanatory primitive in both "The Structure and Function of Musical Theory," and "Past and Present Concepts."

This last observation completes that aspect of Babbitt's prescriptions for music theory and analysis that I mentioned earlier in this chapter. It takes us back to Babbitt's criticism of Salzer and Schachter's Schenker sketch from his lecture on professional music theory. The final assessment of the explanatory power, the sufficiency, of any theory or analysis must conform to systematic construction of the particular framework under investigation. In other words, a comparison of Babbitt with Lerdahl and Jackendoff can only examine how or whether each one follows a scientific, explanatory scheme based on primitive concepts. We need not begin to assert whether Babbitt's choices for these primitives are better than Lerdahl and Jackendoff's. For our purposes, this question is moot. Babbitt's adherence to empiricism represents merely his personal preference: Lerdahl and Jackendoff's psychological primitives are no less explanatory.

Interactions between Music Theory and Scientific Method

To end this chapter, one final clarification remains concerning Babbitt's proposed framework. Ultimately, we must wonder: why is Babbitt proposing (or perhaps imposing) the framework of scientific explanation for music theory and analysis? How can he approach the investigation of an aesthetic object such as music in terms of scientific method? These questions echo Brown and Dempster's comments at the outset of "The Scientific Image of Music Theory":

> 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 and mechanical.[76]

How can we apply scientific investigation to an aesthetic object?

Fortunately, Babbitt directly addresses these concerns. For him, it is a simple matter: "...it may be asserted that music is not a science. This, naturally, is not the point, not even a point."[77] Quite clearly, Babbitt maintains that the characters of explanation and theory construction formulated in logic studies and science apply in general to any discipline that presumes to accomplish those ends, regardless of the particular object of study:

> ...but there is no doubt that the question as to whether musical discourse or—more precisely—the theory of music should be subject to the methodological criteria of scientific method and the attendant scientific language is a question, except that the question is really not the normative one of whether it "should be" or "must be," but the factual one that it is, not because of the nature of musical theory, but because the nature and scope of scientific method and language, whose domain of application is such that if it is not extensible to musical theory, then musical theory is not a theory in any sense in which the term has even been employed. This should sound neither contentious nor portentous, rather it should be obvious to the point of virtual tautology.[78]

In Babbitt's mind, the scope of scientific methodology transcends the particular boundaries of individual studies: the general logical form of scientific explanation applies equally to the chemistry experiment as to music analysis. The question as to whether music theory and analysis should conform to scientific methodology is irrelevant for Babbitt.

However, this is as much as we get from Babbitt concerning the interaction of music theory and analysis with scientific method; and at this time, we are not in a position to assess

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[76]Matthew Brown and Douglas Dempster, "The Scientific Image of Music Theory," *Journal of Music Theory* 33 (1989): p. 65.
[77] Babbitt, "The Structure and Function of Musical Theory," p.192.
[78] Ibid. p.193.

the validity of his broad claim. Let us move on to the next chapter which provides a general account of the nature of scientific explanation. In this section, we find that Babbitt's two criteria of empiricism and the establishment of primitives constitute integral parts of scientific explanation. Also, we will encounter the general logical format of scientific explanation that supports Babbitt's assertion of its universal applicability.

Chapter 2: Scientific Explanation—Outlining D-N

Moving through Babbitt's prescriptions for music theory and analysis, we saw that his main contention is the application of scientific language and method within music analysis. We also established that, for Babbitt, this framework hinges upon two chief elements: 1) restricting analytical statements to the empirical domain; and 2) postulating musical concepts that function as primitives within a hierarchical scheme of explanation. Unfortunately, Babbitt does not explicitly outline the model of scientific explanation, but he leaves it up to his readers to explore the subject, if they are not already familiar with it.

This section attempts to remedy this conspicuous lack of information in Babbitt's essays. Scientific explanation is not a recondite concept, as Babbitt's omission might lead us to believe. Instead, a detailed formula of scientific explanation exists and has been the subject of constant scrutiny, criticism, and scholarly debate. I will present, therefore, a brief outline of the accepted paradigm of scientific explanation as developed primarily by Carl Hempel and Paul Oppenheim in their seminal article *Studies in the Logic of Explanation*[79] published in 1948. For our purposes, the complete picture of this fascinating area of inquiry cannot come into consideration. A small glimpse from within the philosophy of science of the standard model of scientific explanation will suffice to augment our understanding of Babbitt's (and later Brown and Dempster's) proposed theoretical framework for music theory and analysis.

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[79] Carl G. Hempel and Paul Oppenheim, "Studies in the Logic of Explanation," *Philosophy of Science* 15 (1948): 135-175.

As an introduction to scientific explanation, I will address the following points: 1) the Deductive-Nomological model of scientific explanation and the hierarchical chain of universal laws; 2) scientific explanation as privileging empiricism and prediction; and 3) the standard interpretation of the terms *sufficient* and *necessary* within the philosophy of science.

1: Deduction, Laws, and Axioms

Before looking directly at the standard paradigm of scientific explanation, a few words about explanation in general are necessary. Most simply, we can posit that explanation, as opposed to description, asks the question "why," in contrast to the question "what." We encountered already this intuitive and simple distinction in the previous chapter with Babbitt's assessment of Schenkerian analysis through Salzer and Schachter's *Counterpoint in Composition*. Hempel and Oppenheim begin their own study with precisely the same point and contrast explanation with description in terms of this basic question:

> To explain the phenomena in the world of our experiences, to answer the question "why?" rather than only the question "what?", is one of the foremost objectives of all rational inquiry; and especially, scientific research in its various branches strives to go beyond a mere description of its subject matter by providing an explanation of the phenomena it investigates.[80]

Asserting that explanation is the fundamental goal of all rational inquiry, Hempel and Oppenheim place this simple formulation—*what* vs. *why*—and its potential answer in the centre of much more difficult methodological questions. In their view, any rational and systematic investigation must provide a way to answer the "why" of the object it seeks to understand.

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[80] Hempel and Oppenheim, "Studies in the Logic of Explanation," p. 135.

We must also recognize that, as difficult as the prospects of achieving explanation may be, this method of inquiry is neither novel nor arcane. One of the earliest records of an attempt to formulate a logical format for explanation is found in the philosophy of Aristotle, c. 384-322 BC. As Wesley Salmon reminds us, Aristotle first distinguished between description and explanation in his *Prior* and *Posterior Analytics*. Logicians and philosophers have been debating and refining the issue ever since:

> The idea that there are two kinds of scientific knowledge—knowledge of *what* and knowledge of *why*—is not new. In the *Posterior Analytics*, Aristotle distinguishes syllogisms that provide scientific understanding from those that do not. In *The Art of Thinking* ("Port-Royal Logic," first published in 1662), Antoine Arnauld distinguishes demonstrations that merely *convince* the mind from those that also *enlighten* it. He continues, "Enlightenment ought to be the principal fruit of true knowledge. Our minds are unsatisfied unless they know not only *that* a thing is but *why* it is."[81]

Salmon provides a useful framework for putting the nature of scientific explanation into proper perspective. He notes that not every question that seeks to answer "why" can be resolved with a scientific explanation. Instead, some questions require moral, practical, or religious justifications—none of which falls under the usual purview of scientific inquiry:

> It is crucial to recognize, however, that not all—or even most—why-questions are requests for scientific explanations. Why, we might ask, did one employee receive a larger raise than another? Because she had been paid less than a male colleague for doing the same kind of job. In this case, a *moral or legal justification* is the appropriate response. Why, someone might ask, did you go to the drugstore today? The answer, "To get some aspirin for my headache,' constitutes a *practical justification*. When Job asked God why he had been singled out for such extraordinary misfortune and suffering, he seems to have been seeking *religious consolation*. It would, of course, spawn endless philosophical mischief to confuse justification or consolation with scientific explanation.[82]

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[81] Wesley C. Salmon, *Scientific Explanation and the Causal Structure of the World* (Princeton: Princeton University Press), 1984. p. 4.
[82] Salmon, *Scientific Explanation and the Causal Structure of the World*, p. 4.

This passage highlights two important points. First of all, it prompts us to wonder about the particular aspect of scientific explanation that distinguishes it from the conceits that Salmon constructs above. Secondly, it limits the scope of scientific explanation as one possible answer among several to the question "why."

Hempel and Oppenheim illustrate the logical form of scientific explanation through the construction of what they call the "Deductive-Nomological" model (hereafter abbreviated as D-N). As its name implies, D-N requires deductive reasoning, i.e. progressing logically from universal concepts to particular occurrences. The qualifier *Nomological* derives from the Greek noun "nomos", which means "law."[83]

Hempel and Oppenheim demonstrate the operation of both these features of D-N with the following example. We may explain (and understand) the peculiar reaction when a mercury thermometer is immersed in hot water by reasoning deductively from general laws of physics concerning thermic conductivity to the particular instance that actualizes them:

> A mercury thermometer is rapidly immersed in hot water; there occurs a temporary drop of the mercury column, which is then followed by a swift rise. How is this phenomenon to be explained? The increase in temperature affect at first only the glass tube of the thermometer; it expands and thus provides a larger space for the mercury inside, whose surface therefore drops. As soon as by heat conduction the rise in temperature reaches the mercury, however, the latter expands, and as its coefficient of expansion is considerably larger than that of glass, a rise of the mercury level results.—This account consists of statements of two kinds. Those of the first kind indicate certain conditions which are realized prior to, or at the same time as, the phenomenon to be explained; we shall refer to them briefly as antecedent conditions. In our illustration, the antecedent conditions include, among others, the fact that the thermometer consists of a glass tube which is partly filled with mercury, and that it is immersed in hot water. The statements of the second kind express

[83] Brown and Dempster clarify this etymology: "The second feature of D-N is conveyed by the term 'nomological.' The root of this term is the Greek word 'nomos' or law." See: *The Scientific Image of Music Theory*, p. 69.

> certain general laws; in our case, these include the laws of the thermic expansion of mercury and of glass, and a statement about the small thermic conductivity of glass. The two sets of statements, if adequately and completely formulated, explain the phenomenon under consideration: They entail the consequence that mercury will first drop, then rise. Thus, the event under discussion is explained by subsuming it under general laws, i.e. by showing that it occurred in accordance with those laws, by virtue of the realization of certain specified antecedent conditions.[84]

We understand, therefore, the reaction of the thermometer in hot water through the invocation of general laws concerning thermic conductivity and the different physical properties of mercury and glass in relation to applied heat. Furthermore, we reason deductively, through necessary antecedent conditions, from these general laws to the particular instance of this example and we subsume the occurrence under those laws. This is to say, the particular occurrence under investigation is not unique: we may correctly assume that any mercury thermometer, under the same conditions, will behave in precisely the same fashion.

Hempel and Oppenheim encapsulate the D-N model with the illustration provided in fig. 1.[85] In this model the "Explanans"—literally from Latin, "explaining"—consists of a combination of general laws and the antecedent conditions that are required for their realization. The "Explanandum"—literally from Latin again, "some thing that ought to be explained"—is the particular phenomenon under investigation. The whole picture taken together constitutes the Explanation.

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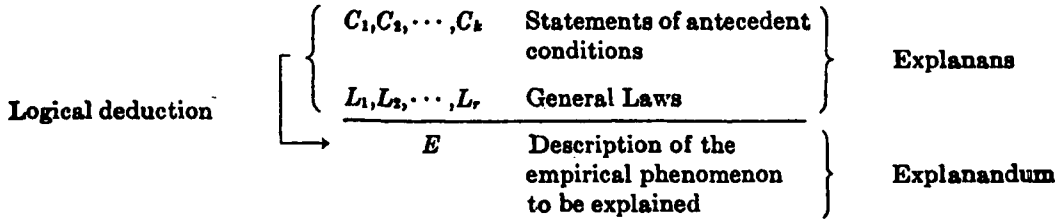
[84] Hempel and Oppenheim, "Studies in the Logic of Explanation," p. 135-136.
[85] This illustration is taken directly from "Studies in the Logic of Explanation," p. 138.

Figure 1: The D-N Model of Scientific Explanation



This model, then, synthesizes the logical form of scientific explanation into a concise arrangement of logical deduction. For the present, it is important to note once again that scientific explanation does not permit us to understand individuals as unique. Every particular occurrence is explained by an appeal to laws of a higher order that subsume them, as the model illustrates. Brown and Dempster point out that philosophers of science often refer to the D-N model as the "Covering-Law Model"[86] for this reason. The significance of this will become more clear as we work through Brown and Dempster's arguments for a scientific music theory and analysis in the next chapter.

One further point about scientific explanation will complete the framework needed for our present purposes: the hierarchical arrangement of general laws. Hempel and Oppenheim offer another thought experiment that considers the first-person perspective of an oar partially submerged in water. As we observe the oar in the water from our hypothetical rowboat, the submerged half of it appears bent upwards. We can explained this particular phenomenon "by means of general laws—mainly the law of refraction and the law

[86] Brown and Dempster, "The Scientific Image of Music Theory," p. 69.

that water is an optically denser medium than air—and by reference to certain antecedent conditions—especially the facts that part of the oar is in the water, part in the air, and that the oar is a relatively straight piece of wood."[87]

These general laws, however, are not themselves immune to questioning. We may inquire as to whether different, yet more general laws subsume the ones we have invoked to explain the phenomenon of the oar's apparent distortion:

> So far, we have considered exclusively the explanation of particular events occurring at a certain time and place. But the question "Why?" may be raised also in regard to general laws. Thus, in our last illustration, the question might be asked: Why does the propagation of light conform to the law of refraction. Classical physics answers in terms of the undulatory theory of light, i.e. by stating that the propagation of light is a wave phenomenon of a certain general type, and that all wave phenomena of that type satisfy the law of refraction. Thus, the explanation of a general regularity consists in subsuming it under another, more comprehensive regularity, under a more general law.[88]

Theoretically, then, the hierarchical chain of laws extends further and further back until we reach some supreme explanatory concept that subsumes all others.

The philosophy of science usually calls these most fundamental explanatory laws primitives, axioms, or first principles. In the hierarchical system of explanation, first principles are thought to begin a chain of causality. Scientific explanation indeed posits causal relationships as a requirement for explanation:

> The type of explanation which has been considered here so far is often referred to as causal explanation. If E describes a particular event, then the antecedent circumstances described in the sentences $C_1, C_2, \ldots, C_k$ may be said jointly to "cause" that event, in the sense that there are certain empirical regularities, expressed by the laws $L_1, L_2, \ldots, L_r$, which imply that whenever conditions of the kind indicated by $C_1, C_2, \ldots, C_k$ occur, an event of the kind described in E will take place.

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[87] Hempel and Oppenheim, "Studies in the Logic of Explanation," p. 136.
[88] Ibid.

> Statements such as L_1, L_2, ..., L_r, which assert general and unexceptional connections between specified characteristics of events, are customarily called causal, or deterministic, laws. They are distinguished from the so-called statistical laws which assert that in the long run, an explicitly stated percentage of all cases satisfying a given set of conditions are accompanied by an event of a certain specified kind.[89]

Hempel and Oppenheim refine our previous understanding of primitives. We surmised the characteristics of primitive concepts to a certain extent with our discussion of Babbitt's scientific framework. Babbitt spoke about the importance of finding primitives for achieving a truly explanatory music theory and analysis. This is the first connection between Babbitt's framework and scientific method that we notice so far.

One further point emerges in the quotation above. Hempel and Oppenheim's invocation of "deterministic" laws that "assert unexceptional connections" suggests that the laws of D-N are *universal*, i.e. unequivocally applicable in every circumstance. This fact is important to note since not every law is sufficiently general to the point of universality. Brown and Dempster point this out in their own discussion of D-N that I have been citing thus far:

> 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 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.[90]

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[89] Hempel and Oppenheim, "Studies in the Logic of Explanation," p. 139. Incidentally, Aristotle also posits a chain of causality in the *Posterior Analytics*. In his terminology, the most primitive, supreme cause is the *final cause*.

[90] Brown and Dempster, "The Scientific Image of Music Theory," p. 69.

Putting the framework together, we may assume that laws that do not have a universal character cannot provide scientific explanation in the strictest sense.

Babbitt also refers to the universal quality of explanatory laws in *The Structure and Function of Musical Theory*, which we have already examined in detail. Speaking about primitives, Babbitt proposes that we may test their adequacy according to their universal character:

> One cannot be at all certain that any concept [taken as a primitive] is necessarily a fruitless or an absurd one; one simply does not know if it is or not until one has tested the results of the application of this concept for correlation with other independent concepts, for invariance under nonvacuous conditions.[91]

Babbitt's requirement of "invariance under nonvacuous conditions" clearly and unambiguously implies that primitive concepts must be universal in Hempel and Oppenheim's sense, i.e. defining "unexceptional connections."

2: Empiricism and Prediction

Thus far, we have connected one of Babbitt's prescriptions for music theory and analysis with the standard model of scientific explanation. We see that Babbitt's invocation of primitives coincides with the universal, hierarchically conceived laws of D-N. In this next section, I discuss two other aspects of scientific explanation that are relevant to the context of this thesis: empiricism and prediction. We have already seen that Babbitt requires exclusively empirical statements for music theory and analysis: this next section completes the link, therefore, between Babbitt and the model of scientific explanation.

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[91] Babbitt, "The Structure and Function of Musical Theory," p. 194. Cross-reference this citation with that in Chapter 1: **Scientific Method**.

As they outline D-N, Hempel and Oppenheim are quick to stipulate that empirical content is necessary for scientific explanation. They make this requirement by dividing D-N into what they call logical and empirical conditions: the logical condition, of course, is deductive reasoning from universals to particulars. Concerning empiricism, Hempel and Oppenheim formulate the following rule:

> (R3) The explanans must have empirical content; i.e. it must be capable, at least in principle, of test by experiment or observation.—This condition is implicit in [the logical condition of deduction]; for since the explanandum is assumed to describe some empirical phenomenon, it follows from [logical deduction] that the explanans entails at least one consequence of empirical character, and this fact confers upon it testability and empirical content. But the point deserves special mention because, as will be seen, certain arguments which have been offered as explanations in the natural and in the social sciences violate this requirement.[92]

The connection that Hempel and Oppenheim make, then, is a simple one. In order to explain a certain phenomenon—a necessarily empirical thing—the laws that function within D-N must themselves be empirical. Otherwise, the adequacy of the laws cannot be tested or confirmed. Brown and Dempster express this requirement for scientific explanation by stating that general laws "cannot be purely logical truths or stipulated definitions, since the truth of neither is empirically testable."[93]

Looking back, this point summarizes precisely Babbitt's criticism of Hans David's analysis of Mozart's "unexpected" C-flats in the G Minor Symphony (K. 550).[94] Babbitt rejects David's statement because it is essentially private and unverifiable in its lack of empirical content. Furthermore, we surmise from Hempel and Oppenheim's simple logic that any potential, empirical evidence offered in support of this "unexpectedness" would not

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[92] Hempel and Oppenheim, "Studies in the Logic of Explanation," p. 137.
[93] Brown and Dempster, "The Scientific Image of Music Theory," p. 69.
[94] Refer back to Chapter 1: **Scientific Language**.

constitute scientific explanation. Both the Explanans—the potential evidence—and the Explanandum—the "unexpectedness" of the C-flats—need to be empirical. Recall at this point also that David Lewin corroborates the idea that empirical evidence can never strictly support an initial metaphysical (in the sense I defined in the previous chapter) statement.[95]

The final qualification we need make to the character of D-N concerns the manner in which it succeeds in explaining individual phenomena. According to Hempel and Oppenheim, scientific explanation according to D-N is equivalent to prediction. They can make this statement considering the logical process inherent within their model. Given the adequacy of a deductive explanation, the process works backwards equally as effectively. If certain laws and antecedent conditions explain a given phenomenon, those same laws and conditions also presuppose the presence of the same phenomenon:

> Let us note that the same formal analysis...applies to scientific prediction as well as to explanation. The difference between the two is of a pragmatic character. If E is given, i.e. if we know that the phenomenon described by E has occurred, and a suitable set of statements $C_1, C_2, \ldots, C_k, L_1, L_2, \ldots, L_r$ is provided afterwards, we speak of an explanation of the phenomenon in question. If the latter statements are given and E is derived prior to the occurrence of the phenomenon it describes, we speak of a prediction. It may be said that an explanation is not fully adequate unless the explanans, if taken account of in time, could have served as a basis for predicting the phenomenon under consideration.—Consequently, whatever will be said in this article concerning the logical characteristics of explanation or prediction will be applicable to either, even if only one of them should be mentioned.[96]

The same concepts that explain a phenomenon also predict its occurrence. Brown and Dempster draw on the predictive power of scientific explanation to clarify their application of it to music theory and analysis, as we will see in the next chapter.

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[95] Refer back to Chapter 1: **Scientific Method.**

[96] Hempel and Oppenheim, "Studies in the Logic of Explanation," p. 138.

Hempel and Oppenheim reaffirm the importance of prediction within scientific explanation by appealing to the process of learning. For them, we come to understand the world around us only when we can generalize adequately enough to predict future occurrences. In the following passage, Hempel and Oppenheim distinguish predictive explanations from those that lack this quality:

> It is this potential predictive force which gives scientific explanation its importance: only to the extent that we are able to explain empirical facts can we attain the major objective of scientific research, namely not merely to record the phenomena of our experience, but to learn from them, by basing upon them theoretical generalizations which enable us to anticipate new occurrences and to control, at least to some extent, the changes in our environment.
>
> Many explanations which are customarily offered, especially in pre-scientific discourse, lack this predictive character, however. Thus it may be explained that a car turned over on the road "because" one of its tires blew out while the car was travelling at high speed. Clearly, on the basis of just this information, the accident could not have been predicted, for the explanans provides no explicit general laws by means of which the prediction might be effected, nor does it state adequately the antecedent conditions which would be needed for prediction.[97]

The proposed explanation for the car accident is not strictly scientific: it does not provide a general law that unconditionally connects the car's running off the road with the blown out tire. Unequivocal causality produces prediction.[98]

3: *Sufficient* and *Necessary* Conditions

As a final refinement of our understanding of scientific method for the context of this thesis, we must realize that the terms Babbitt uses are not free from connotation. Indeed, *sufficient* is replete with meaning within the philosophy of science. The last section of this

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[97] Hempel and Oppenheim, "Studies in the Logic of Explanation," p. 138-139.

[98] Wesley Salmon reminds us that this connection between causality and prediction is a highly debated topic. See: *Scientific Explanation and the Causal Structure of the World*, chapter 5, p. 135-157. Unfortunately the scope and subject matter of this thesis does not permit for a full consideration of this aspect of scientific explanation. I only point in the direction of further research in the consideration of the interaction of scientific explanation with music theory and analysis.

chapter, therefore, gives a short résumé of the importance that the term *sufficient* carries for explanation.

To begin, it is useful to cite the standard definition of *sufficient* from Webster's English dictionary:

> Sufficient: marked by quantity, scope, power, or quality to meet with the demands, wants, or needs of a situation or of a proposed use or end.[99]

In this context, *sufficient* indicates adequacy for a specific task or end. For example, we may say that having $2.50 is a sufficient amount of money for buying an overpriced cup of coffee from the local shop. In contrast, we would not normally say that $5.00 is sufficient for the same cup of coffee: this amount of money is *more than sufficient*. In common usage, then, *sufficient* implies the minimum necessary for any given purpose—it carries the sense of "just good enough."

In the philosophy of science, however, a distinction arises between *sufficient* and *necessary*, even though the terms are related. Causal relationships are divided among necessary and sufficient conditions, the definitions of which I give below:

> A condition *A* is said to be *necessary* for a condition *B*, if (and only if) the falsity (/non-existence/ non-occurrence) [as the case may be] of *A* guarantees (or brings about) the falsity (/non-existence/non-occurrence) of *B*.
>
> A condition *A* is said to be *sufficient* for a condition *B*, if (and only if) the truth (/existence/occurrence) [as the case may be] of *A* guarantees (or brings about) the truth (/existence/occurrence) of *B*.[100]

Let us look at some standard examples that illustrate these concepts.

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[99] *Webster's Third New International Dictionary of the English Language, Unabridged*, s.v. "sufficient."
[100] *The Concepts of Necessary and Sufficient Conditions*. Copyright © Norman Swartz, 1997, Department of Philosophy, Simon Fraser University. http://www.sfu.ca/philosophy/swartz/conditions1.htm (stable URL)

Having air to breath is a necessary condition for human life. To put specific terms into the general definitions above, without air human life is impossible. This condition, however, is not sufficient. Even though humans need air, many other factors contribute to sustaining life. The truth of A (air) does not guarantee the truth of B (life). Walking is a sufficient condition for getting oneself to the shop to buy that overpriced cup of coffee; however, it is not necessary. One could just as easily get to the shop by driving in a car or riding on a bicycle.

Of course, some condition can be both necessary and sufficient at the same time. Matthew Brown gives a hypothetical example in *Explaining Tonality*:

> The concept 'passing tone' can be defined as an unaccented dissonance that moves by step between two consonances a third apart. Such a definition conveys the necessary condition that passing tones move by step and is sufficiently precise to differentiate passing tones from other dissonances, such as neighbour tones, cambiatas, and suspensions.[101]

If a dissonant note between two triads does not move by step, then it is not a passing tone: i.e. moving by step is a necessary condition for passing tones. If a dissonant note between two triads moves by step, then it is a passing tone: i.e. moving by step is also a sufficient condition for a note being a passing tone.

Despite the brevity of this account of sufficient conditions, and of scientific explanation in general, we now have the context for a more complete understanding of Babbitt's framework for music theory and analysis. Babbitt finds the means for achieving sufficient conditions for explaining musical structure within scientific method. Furthermore, empiricism and prediction form essential elements of scientific explanation for Babbitt and

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[101] Brown, *Explaining Tonality*, p. 3.

the standard D-N model. In Chapter 4, we will see that Babbitt makes direct reference to sufficient and necessary conditions in an analysis of set transformations in Webern's *Concerto for Nine Instruments*. Furthermore, the summary above provides a context for the next chapter dealing with Brown and Dempster. Unlike Babbitt, they take the time within their articles to explain D-N and the principles of scientific method.

Chapter 3: The Scientific Image of Music Theory.

With Milton Babbitt's thoughts, we established the possibility that music analysis can have explanatory value. By conforming to strict rules governing analytical language and methodology, music analysis can begin to reveal causes, both proximate and final, for individual musical events. In other words, music analysis need not be limited to superficial or even sophisticated descriptions, as Brown and Dempster assert:

> 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.[102]

We also recall that Babbitt endorses a music analysis that conforms to the model of scientific method as a paradigm for explanation. In presenting this model, however, Babbitt remains (perhaps characteristically) abstruse and prolix. He never takes the adequate time to outline explicitly the model of scientific explanation that he has in mind. In the previous chapter, we took recourse to philosophers of science in order to fill in the general details of scientific explanation that Babbitt leaves tacit.

Turning to Matthew Brown and Douglas Dempster's work, we find a significantly more lucid and direct treatment of the model of scientific explanation in terms of its potential relationship to music theory and analysis. We also notice that Brown and Dempster part ways with Babbitt on a central aspect of the character of scientific explanation: Babbitt is reluctant to endorse the primacy of universal laws within music theory, whereas Brown and Dempster's entire framework rests upon the construction of such

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[102] Brown and Dempster, "The Scientific Image of Music Theory," p. 81.

laws. I will present a detailed comparison of Babbitt to Brown and Dempster in the next chapter.

1. Following the Thread

To reiterate, one of the goals of this thesis is to follow a line of methodological thought within music theory and analysis that begins most ostensibly with Milton Babbitt's article "Past and Present Concepts of the Nature and Limits of Music" (1961), and continues in "The Structure and Function of Musical Theory" (1965). Calling for the application of scientific method and language, these articles represent Babbitt's personal investment to the continued integrity of music theory and analysis as a professional discipline. Also, we have already seen what the model of scientific explanation looks like in general, and that it is neither arcane nor trivial.

After Babbitt, the most notable published discussions of scientific method and music theory appear in papers by Benjamin Boretz and John Rahn. The voluminous *Meta-Variations: Studies in the Foundations of Musical Thought*[103] has been perhaps the most extensive treatment of the subject and—unfortunately for us—the most convoluted and notoriously difficult to understand. To summarize this work in an almost unethically concise way, Boretz's *Meta-Variations* seeks to reinvent music theory from the standpoint of Logical Positivism and thereby purge it from metaphysical terms and give it methodological rigor.[104] We will have a better opportunity to discuss Boretz later on

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[103] *Meta-Variations: Studies in the Foundations of Musical Thought*, presents parts of Boretz's PhD dissertation in composition at Princeton University. It has been published in a series between 1969 and 1973 in *Perspectives of New Music*, and subsequently in book form by Boretz's publication company "Open Spaces" in 1995. Please see the bibliography for full references.

[104] I draw this summary from that Brown and Dempster give in "The Scientific Image of Music Theory."

through Brown and Dempster's evaluation of his work.[105] Similarly, John Rahn published an influential article in 1979—to mention only one—addressing scientific explanation within music theory entitled "Aspects of Musical Explanation."[106] Both students of Milton Babbitt at Princeton, Boretz and Rahn carried on this line of inquiry undoubtedly through the influence of their teacher.

In the constant stream of professional publications, an approximate ten-year gap occurs in the literature dealing with scientific method and music theory until 1989 when Brown and Dempster published in this area. It seems as if theorists with an interest in this subject were spending most of their time trying to understand the *Meta-Variations* instead of shaping the line of study further. Of course, the literature is not entirely dry: notable publications dealing with scientific method and music theory in these years include[107]: Eugene Narmour's *Beyond Schenkerism*; Lerdahl and Jackendoff's "Toward a Formal Theory of Tonal Music" and *A Generative Theory of Tonal Music*; Allen Forte's *The Structure of Atonal Music*; and others. I do not intend to imply that theorists took no notice of the scientific framework or use it in their work; rather, I only point out that few publications address the issue exclusively and directly.

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[105] The decision to bring up *Meta-Variations* within the scope of this thesis has been a difficult one. First of all, any fair treatment of this work demands an investigation that lies far beyond the confines of the task at hand. I have also decided largely to pass over *Meta-Variations* since its own goals are slightly different from my own: I am investigating the merits of applying the rigors of scientific explanation to music theory and analysis, whereas Boretz is constructing a scientific music theory essentially from scratch. I will use Brown and Dempster's summary of *Meta-Variations* in order to illustrate certain points, but I will not address the work directly. The propriety of this approach will become more clear later on.

[106] Please see the bibliography for full references.

[107] Please see the bibliography for full references.

In 1989, Matthew Brown (from the field of music theory) and Douglas Dempster (a philosopher of art and aesthetics) published an article titled "The Scientific Image of Music Theory."[108] This article is the most direct and focused investigation and evaluation of scientific method within music theory and analysis since Babbitt, Rahn and Boretz. In it they establish a clear view of how scientific explanation works and the advantages and disadvantages of applying the model to the practice of music theory and analysis.

Not insignificantly, Brown and Dempster refer to work by several other professional theorists in order to illustrate their points: most notably they cite Babbitt, Rahn, and Boretz (particularly his *Meta-Variations*[109]). These references and the content of the article in general prompted a polemical colloquium in the same issue of the *Journal of Music Theory*, in which these theorists were given the opportunity to respond to Brown and Dempster's evaluation of their work.[110] Additionally, Brown and Dempster tabled their own response with an article in the following issue of this same journal: "Evaluating Musical Analyses and Theories: Five Perspectives."[111] Nicholas Cook and Richard Taruskin also participated in the exchange.

This chapter examines Matthew Brown and Douglas Dempster's endorsement of scientific explanation within music theory and analysis. After putting Brown and Dempster's work in its proper perspective as a neutral consideration of a neglected

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[108]Matthew Brown and Douglas Dempster, "The Scientific Image of Music Theory," Journal *of Music Theory* 33 (1989): 65-106.

[109] The first half of "The Scientific Image of Music Theory" is dedicated to a sharp criticism of Boretz.

[110] See: *Journal of Music Theory*, 33 Theory Colloquium (1989): 64-189. Babbitt did not participate in this exchange.

[111]Matthew Brown and Douglas Dempster, "Evaluating Musical Analyses and Theories: Five Perspectives," *Journal of Music Theory* 34 (1990): 247-279.

theoretical framework, I will work through their arguments in the following order: 1) the scientific image of music theory and some reasons for adopting it; 2) Brown and Dempster's reformulation of scientific explanation and the Deductive-Nomological paradigm; and 3) science and aesthetics—or, where the scientific image of music theory leads us.

2. The Scientific Image of Music Theory

Music and Science: a Defence.

At the outset of "The Scientific Image of Music Theory," Brown and Dempster take direct aim at the central question that permeates any formal investigation of music: "What is the best method for understanding and explaining music?" Of course, this question assumes, in accordance with the general confines of this thesis, that we approach music rationally (or formally), as Brown and Dempster use the term:

> 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.[112]

Brown and Dempster observe that the discipline of music theory has not decided ultimately upon the most fitting form of musical explanation. Instead, the field remains divided into two different approaches. I find it helpful to summarize this distinction with the general terms *scientific* and *aesthetic*; but allow me to quote Brown and Dempster's summary in full:

> Now, although music theory may be rational, it is not immediately obvious how it fulfills this demand. Indeed, there is remarkably little consensus among theorists about what the goals and methods of the discipline should be. Some believe that

[112] Brown and Dempster, "The Scientific Image of Music Theory," p. 65.

> 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.
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> 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*.[113]

Following their lead, I define my own designations in this way: *scientific* analysis denotes a systematic, empirically based approach to the explanation of musical construction understood in terms of broad categories or paradigms; *aesthetic* analysis directly addresses the unique properties of music as an expressive work of art, whether the approach is systematic or not. In other words, scientific analysis prioritizes an understanding of general musical features in common between individual pieces, while aesthetic analysis focuses on the unique aspects of particular works.

Brown and Dempster's summary above provides an interesting perspective on the division within music theory and its methodology. As they see it, the music theorist has the choice to be either scientific and rigorous (and therefore "truly explanatory"), or radically subjective and informal by privileging a sensitivity to the aesthetic properties of individual

[113]Brown and Dempster, "The Scientific Image of Music Theory," p. 65-66.

pieces of music. This distinction is worthy of some attention and commentary, even before we look more closely at the scientific model they propose.

At first glance, the quotation given above seems to reveal only that Brown and Dempster are indisputably biased and bigoted. They seem to ascribe "truly explanatory" value to scientific music analysis alone. Their qualifiers "subjective" and "*ad hoc*" attached to aesthetic musical analysis, furthermore, are hardly without negative connotations. They seem to imply that only a scientific method of music analysis is rigorous. And even though they are quick to modify the term, their demarcation among music analyses accomplished with the term *rational* seems to suggest disdain for those theorists who choose to incorporate an emotional or personal engagement into their work. Extending their terminology, we might say that anyone who focuses on the aesthetic properties of music is "irrational."

In order to be fair to Brown and Dempster, however, we need to consider their larger arguments and opinions in conjunction with the quotation above. Let it be clear that Brown and Dempster are not scornful or condescending toward the kind of analysis I call aesthetic; rather, they simply see no reason to privilege it. This sentiment is the key to understanding the framework that they propose in this paper and elsewhere. The quotations that follow express this and represent their ultimate opinion (all italics are my own):

> On the other hand, Cook supposes that scientifically adequate analyses must necessarily neglect the most important aesthetic features of music. For some reason, he believes that this type of analysis must confine its attention to the score and not to the much richer data of musical experience. We can see no grounds for holding this peculiar view, and Cook provides no justification for it. *Few people would deny that there are qualities in art that cannot at present be explained scientifically. However,*

> *there is no reason to suppose that unsystematic, metaphorical, or poetic approaches have any better chance of illuminating them than systematic ones.*[114]
>
> Finally, we see no real evidence to show that scientific methods dull the sensitivity of the analyst or that they necessarily ignore the most important aesthetic features of a given piece...*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 scientific ones.*[115]

From these passages, we clearly see that Brown and Dempster do not discredit an aesthetically focused music analysis. Furthermore, they are quick to acknowledge that a scientific approach to analysis cannot articulate certain aesthetic properties of music. Indeed, those who would reject aesthetic analysis altogether only reveal their own ignorance of the many skilful, significant, and illuminating music analyses that are presently available to the scholarly community.

I will reaffirm Brown and Dempster's benign motivations in a later part of this chapter. Ultimately, their framework points to proposed value of applying the scientific method of inquiry to an aesthetic object such as music. This purpose will become apparent in articles such as Brown's "'Adrift on Neurath's Boat': the Case for a Naturalized Music Theory"[116], and Dempster's "Renaturalizing Aesthetics."[117] Far from abandoning the individual aspects of music, Brown and Dempster propose that we can understand music as a compounding of the aesthetic and the scientific.

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[114]Matthew Brown and Douglas Dempster, "'A Guide to Musical Analysis', by Nicholas Cook," *Journal of Music Theory* 32 (1988): 148-158.
[115] Brown and Dempster, "The Scientific Image of Music Theory," p. 93.
[116]Matthew Brown, "'Adrift on Neurath's Boat': The Case for a Naturalized Music Theory," *The Journal of Musicology* 15 (1997): 330-342.
[117]Douglas Dempster, "Renaturalizing Aesthetics," *The Journal of Aesthetics and Art Criticism* 51 Philosophy and the Histories of the Arts (1993): 351-361.

The alternative framework that emerges in "The Scientific Image of Music Theory," however, is the picture of an analytical methodology that should not itself be discredited or abandoned. In fact, Brown and Dempster point out that criticisms of scientific, empirical musical analysis often reduce to such derogatory terms as *insensitive* and *mechanical*. As one concrete example, we certainly cannot see Gary Tomlinson's words (i.e. "limiting", "solipsistic") as anything but pejorative. In this passage, Tomlinson criticizes those who presume to understand music outside of its cultural context, a framework that cannot be included in an empirical musical theory:

> We cannot comprehend art works (or anything else) outside of a cultural context. It is only a question of whether we opt for a limited and limiting discourse, a solipsistic conversation with meanings that come to us automatically, or choose instead to try to conceive of other meanings, other assumptions, other aspirations and fears.[118]

We must recognize, therefore, that a justified defence of scientific musical inquiry is at the heart of Brown and Dempster's work, rather than a wholesale dismissal of aesthetic analysis. They believe that both methods, while completely separable, can operate together and enrich each other.

Brown and Dempster propose a reconsideration of both the merits and the shortcomings of scientific music theory. They illustrate its characteristics and consider those aspects that make a scientific music theory attractive as an analytical framework. Scientific musical explanation is not the only method, but one viable choice among many:

> Again, nowhere do we claim that D-N is the only proper model for musical explanation. We insisted that the scientific image is only one among many possible "models" of music theory. What's more, our main goal was to highlight the meta-

[118]Gary Tomlinson, "The Web of Culture: A Context for Musicology," p. 362.

> theoretical costs of that "model"—costs insufficiently appreciated by its most noted advocates.[119]

According to Brown and Dempster, the application of scientific method to music theory and analysis is, and has been misunderstood and misrepresented. As evident in the colloquium generated by "The Scientific Image of Music Theory," some who do not practice scientific analysis feel attacked whenever somebody brings up the issue. Perhaps they feel as if proponents of scientific music theory are tacitly negating the value of the aesthetic approach. Nothing is further from the truth, as Brown and Dempster are careful to articulate.

Consider the beginning of Richard Taruskin's contribution to the colloquium as an example. In perhaps characteristic fashion, Taruskin spares no violent words for Brown and Dempster:

> All of us-Catholics, Jews, Protestants, Buddhists, Muslims-are "gentiles" to the Mormons. And all of us-Babbitt, Kerman, Boretz, Meyer, Rahn, Cone, me-are "particularists" to Matthew Brown and Douglas Dempster. Levelling perspectives like these are uninformative: All Buddhists and Protestants have in common is that they are not Mormons. And all that Benjamin Boretz and I would seem to have in common is that we are neither Brown nor Dempster, who seem as little concerned with the distinguishing characteristics of the people they write about as they profess to be about individual pieces of music. Their great show of callow unobservance does not enhance one's confidence in their approach to music theory and analysis. Nor in their forensic skills: they attack with great courage positions no one to my knowledge has ever advanced. They take aim, colors flying, at barn walls from a distance of three paces. They miss. There is an unearthly aura of born-yesterday about this jeremiad.[120]

These defensive and angry words are surprising, considering Brown and Dempster's clear and objective consideration of scientific method: nowhere do they attack or belittle other theoretical frameworks.

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[119] Brown and Dempster, "Evaluating Musical Analyses and Theories," p. 268. We recall the D-N model of scientific explanation for the previous chapter.

[120] Richard Taruskin, "Reply to Brown and Dempster," *Journal of Music Theory* 33 (1989): p. 155.

For now, we have established that Brown and Dempster's framework implies no polemics (even though they invite it by citing other theorists): they are presenting the "scientific image of music theory" for what it is, what it accomplishes, its advantages and disadvantages. Not least among their goals, however, is a defence of this particular approach to music theory and analysis:

> The following paper will reconsider this dilemma by evaluating the claims for and against scientific music theory. On the one hand, we will argue that the case for a 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.[121]

Indeed, Brown and Dempster would seem to be most properly on the defensive side rather than Taruskin *et al.*

Science vs. Particularism: the Problem of Intersubjectivity

Having clarified the benign intentions of Brown and Dempster's project, we may now consider directly the scientific image of music theory that they describe. In this first section, I outline a crucial reason why Brown and Dempster propose that scientific music analysis needs reconsideration. According to their understanding, scientific method within music theory provides the most secure way to produce true intersubjectivity.

For Brown and Dempster, applying scientific method to music theory and analysis provides a basic necessity: the need for reliable, external criteria in the evaluation and understanding of music analyses. Of course, these critical activities form the foundation for the integrity and progression of the discipline by providing intersubjectivity. Brown and

[121] Brown and Dempster, "The Scientific Image of Music Theory," p. 66.

Dempster's argument culminates in this point: aesthetic music analysis rests finally within the individual and the mysterious quality of "musicality"; whereas the scientific model reduces the role of the particular analyst and favours more objective inquiry. Therefore, scientific music analysis encourages and facilitates intersubjective corroboration and communication, the cornerstone of academic inquiry. In order to understand this argument, let us examine the definition of a term that Brown and Dempster introduce in their discussion: *particularism*.

As a preamble to this discussion, I recall the previous chapter's general presentation of the logical form of scientific explanation. As we remember, scientific inquiry explains particular phenomena by subsuming, or covering them under general, higher-order laws in a hierarchical scheme. This is the logical process of deduction: reasoning from universals to particulars. As I also noted in the previous chapter, this manner of explanation is neither arcane nor unusual; indeed, this paradigm of explanation has played a significant role in Western thought since Aristotle.[122]

In their articles, Brown and Dempster identify another explanatory model operating within music analysis that they call *particularism*. As the term implies, particularism stands in direct opposition to scientific explanation and its dependence upon general laws. Particularism understands pieces of music as unique entities that define their own system of analysis:

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[122] Aristotle outlined this method of scientific explanation in the *Prior Analytics*, and further developed it in the *Posterior Analytics*.

> Clearly, ...particularism...is antithetical to the primary objective of scientific explanation: the formulation of powerful law-like generalizations about potentially infinite classes of phenomena.[123]

Brown and Dempster introduce the term *particularism* at the end of a lengthy assessment of Benjamin Boretz's epic dissertation *Meta-variations.* Unfortunately, the scope of this project cannot admit an extensive investigation of either the *Meta-variations* itself or the accuracy of Brown and Dempster's summary of it, as presented in "The Scientific Image of Music Theory". I note, however, that our understanding of particularism does not rely upon whether Brown and Dempster have accurately categorized the epistemological underpinnings of Boretz's *Meta-variations.* Nor does it matter whether Boretz is, or ever was a "particularist." The presentation and understanding of the concept behind the term is not affected by such considerations: Brown and Dempster's characterization of *particularism* is valid even if it is only a hypothetical construction.

To begin, allow me to quote the definition of particularism directly from Brown and Dempster's article:

> 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.[124]

According to this definition, particularism is completely antithetical to scientific explanation. Scientific explanation depends on general laws that subsume individuals under larger "classes" and "kinds." Scientific method is not concerned with expressing the

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[123] Brown and Dempster, "The Scientific Image of Music Theory," p. 82-83.
[124] Ibid. p. 82.

uniqueness of individuals under investigation. Brown and Dempster express the distinction again in this way:

> 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.[125]

The theorists Brown and Dempster cite as particularists are Benjamin Boretz, John Rahn, Milton Babbitt, Peter Westergaard, Joseph Kerman, E. T. Cone, and Leonard Meyer. Due to the time constraints of the present project, unfortunately, we cannot examine the validity of Brown and Dempster's charge with regard to all these distinguished scholars. One name in the list, however, commands our attention. Since we have already established that Babbitt endorses scientific music analysis, Brown and Dempster's inclusion of Babbitt among particularists is surprising to say the least. We will explore Babbitt's alleged particularism and his connection to Brown and Dempster in the last chapter of the thesis.

At first glance, particularism seems to offer some significant analytical and epistemological advantages. Without depending on general laws, a particularist analysis can focus on revealing the most salient and interesting features of individual compositions. Indeed, this is how most analysts approach their task, i.e. with the purpose of describing something interesting about *this* piece of music and showing how we can understand *this* feature. For example, is not the foreground graph of a Schenkerian analysis the most

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[125] Brown and Dempster, "The Scientific Image of Music Theory," p. 83.

interesting part? After we glance at the placement of the different notes of the *Ursatz*, we want to see the "dirty details": how the elements of the *Ursatz* are prolonged in this particular piece in it own particular way.

These sentiments are at the heart of Charles Smith's indictment of linear analysis in "The Functional Extravagance of Chromatic Chords."[126] Arguing against the relevance of Schenkerian analysis, Smith constructs a musical "counterfeit" for the *Introduzione* of Beethoven's *Waldstein* sonata: he changes some notes to keep the basic skeleton of the music intact while simultaneously destroying its coherence and line. His counterfeit version preserves (supposedly) the identical middle and background Schenker sketch as Beethoven's own score. [127] For Smith, then, the general concepts of Schenkerian analysis fail to reveal what is unique and interesting about this music:

> This failure is critical for linear theory. Analysis is predicated upon our curiosity about distinctions—between striking and less striking moment in a single piece, and between pieces that invite our repeated attentions and pieces that do not. However, we find here an analytical picture that fails to reflect an obvious distinction; it applies equally well to something truly memorable (Opus 53's Introduzione) and to something vacuous—something that could never be accepted as the work of a master.[128]

The root of the problem, this "critical failure for linear theory," is the Schenkerian practice (according to Smith) of viewing chromaticisms as incidental results of large-scale, diatonic voice leading. The general concepts of the theory subsume, and thereby negate the most

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[126]Charles Smith, "The Functional Extravagance of Chromatic Chords," *Music Theory Spectrum* 8 (1986): 94-139.

[127] In his reply to this article, David Beach finds fault with Smith's Schenkerian analytical technique: "On Analysis, Beethoven, and Extravagance: A Response to Charles J. Smith," *Music Theory Spectrum* 9 (1987): 173-185.

[128] Smith, "The Functional Extravagance of Chromatic Chords," p. 103.

important aspects of a piece of music like Beethoven's *Introduzione*: those elements that make it unique and worthy of our attention and admiration.

Brown and Dempster themselves note and summarize these arguable advantages of the particularist approach:

> 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 unbiased analyses, he or she hopes to reveal 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 discernable 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 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.[129]

Within "The Scientific Image of Music Theory," Brown and Dempster outline several counter arguments to the value of a particularist approach. One important objection concerns the apparent contradiction of invoking general concepts and methods while maintaining that individual pieces are essentially unique. Brown and Dempster introduce this idea in the quotation above. I leave this question to the side for now, however, since we will be able to address it more completely after our reassessment of Babbitt in the next chapter. For the present, I will focus on Brown and Dempster's charge that particularism, in fact, hinders true intersubjectivity. This is their most serious criticism and it carries far-reaching implications for music theory and analysis as a professional discipline.

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[129] Brown and Dempster, "The Scientific Image of Music Theory," p. 94.

Brown and Dempster suggest that particularism is a dangerous approach to music analysis for two reasons: first, it leads to what they call "connoisseurism in music theory" by privileging analyses that invoke musical elements or structures that are directly perceptible; and second, it finds justification ultimately within the individual analyst and thereby obliterates any standards we could have of judging between different analytical methods and statements. Both of these concerns suggest that particularism may in fact inhibit intersubjective exchange between theorists.

Regarding the first point above, Brown and Dempster describe the character of a particularist analysis in the following way. Without the hierarchical systems of significance that general laws create, an analysis must support its claims by appealing to the directly perceptible quality of the evidence submitted in defence of its statements:

> Fifth [in their list of arguments against particularism], and most important, particularism inevitably leads to a troubling form of connoisseurism in music theory. If a particularist wants 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.[130]

In other words, every analytical statement with such an analysis derives its force and significance from aspects of the music that are directly hearable. Only the perceivable is meaningful and intelligible.

The next step in Brown and Dempster's line of thought considers appropriately the counter claim. If the musical evidence for particularist statements is easily perceivable, then

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[130] Brown and Dempster, "The Scientific Image of Music Theory," p. 95.

we might be hard pressed to find fault with it. The analysis would be convincing and significant:

> 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.[131]

Indeed, why should discount an analysis that conforms to and enriches our own hearing of a piece of music? Furthermore, this corroboration seems only to foster intersubjectivity instead of hindering it. (More on the relationship of particularism to intersubjectivity comes below.)

We encounter a serious difficulty, however, when other "qualified" listeners do not support or share a particular analyst's hearing. For Brown and Dempster, this question finds the heart of the matter in a logical progression of methodological consequences:

> The problem is that the hypothetical structures invoked by theorists are not always 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.[132]

Here is the charge of connoisseurism in unambiguous terms. Ultimately, the justification of an analysis through its perceptibility cannot escape this problem.

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[131] Brown and Dempster, "The Scientific Image of Music Theory," p. 95.
[132] Ibid. p. 95-96.

In the event of a disagreement over the particular hearing that an analysis presumes or invokes, one wonders how to proceed. Only two solutions present themselves to resolve this impasse. The analyst could assert dismissively the superiority of his or her own listening powers to claim authority over the dissenter. Clearly, this position is untenable. The question of superiority cannot be answered without serious repercussions. Alternatively, the analyst and the challenger could simply agree to disagree in this matter; but surely, this cannot be a more acceptable solution than the first. If an analysis presumes to be explanatory in any way, and not merely an interesting but ultimately inconsequential description, an individual hearing cannot be the final word. How many fruitful discussions and exchanges that promote the advancement of understanding can we imagine would be lost if every disagreement was settled "because I hear it that way?"

Brown and Dempster articulate these concerns more formally. For them, particularism is a self-referential analytical system: it rests completely upon the individual analyst and nothing else. This solipsistic approach leads ultimately to confusion concerning the terms by which we presume to judge and compare the merits of individual analyses:

> 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.[133]

[133] Brown and Dempster, "The Scientific Image of Music Theory," p. 96.

Of course, there is no problem with this analytical approach if we have no interest in discriminating among analyses; but contemporary professional activity demonstrates such an interest constantly.

We need external criteria by which to assess analytical methods and statements; for without them, arbitration with critical integrity is nearly impossible. But what will the criteria be? General laws that provide concrete terms for significance, or ill-defined concepts such as *musicality*, or *refinement*? Brown and Dempster direct this question at E. T. Cone and his endorsement of particularism:

> As is clear from "Analysis Today," Cone believes not only that analyses 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 and 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.[134]

For Brown and Dempster, we cannot take a personal hearing as a basis for analysis if we want them to be explanatory and not only descriptive. As they point out, the logical consequences of particularism lead to some absurd notions indeed. But without some external system of verification of measurement, like scientific method, avoiding the nasty conclusions of particularism necessitates an appeal to such tenuous and uncritical concepts as "musicality" and "aural refinement."

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[134] Brown and Dempster, "The Scientific Image of Music Theory," p. 93.

As a self-referential system, particularism easy falls into circularity as well. If we take "because I hear it that way" as a justification for an analysis, we must wonder what exactly the analysis actually accomplishes. An analysis that describes a hearing cannot also receive justification from a hearing. We can image the following exchange: "Why do you analyze the structure in that way?"—"Because that is the way I hear it." And then: "Why do you hear it that way?"—"Because the structure compels me to hear it that way." As Brown and Dempster point out, we can avoid circularity by "independent laws to support the analysis in question."[135]

Brown and Dempster keep the final charge against particularism latent in the passages given above. It seems clear, however, that they intend to suggest that particularism hinders true intersubjective exchange. Brown and Dempster, as we saw above, assert that the particularist argument retreats to either "circular or dogmatic" justifications or "pluralism." These defences, especially pluralism, certainly do not promote intersubjectivity.

One could object that in the event of an agreement regarding a personal hearing, intersubjective exchange does in fact take place. In other words, if an analyst persuades me to hear a piece of music or a certain structure as he or she does (i.e. if the evidence offered in support of the hearing is convincing to me) then intersubjectivity is maintained. After all, have not the analyst and I communicated on the same terms to obtain mutual understanding?

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[135] Brown and Dempster, "The Scientific Image of Music Theory," p. 88.

Brown and Dempster do not fill in their argument completely enough in this article to provide a clear answer to this question. Following their line of thought, I would suggest that intersubjectivity, in a strict sense, still is difficult to achieve in the case proposed above. Faced with an analysis that describes an intensely personal hearing, the listener must be willing to suspend temporarily his or her own subject position in order to enter the analyst's own wavelength. Only an endeavour to perceive the music in the analyst's particular way can lend significance to the evidence offered for the hearing. If I must sacrifice my own subject to understand the analysis, however, this is surely not true intersubjectivity. Instead, I prefer to call this process "intersubjective-transformation." However willingly I follow, the analyst must lead me along his or her own path of listening. Of course, if I happen to hear the music in the same way, intersubjectivity is achieved with no effort. Furthermore, I always have the opportunity to modify my own listening, or discover a new one in response to the analysis. In these cases, the analyst and I truly share the hearing intersubjectively.

We need not be content, however, with leaving the achievement of intersubjectivity to chance. On the other hand, analyzing music according to the scientific method of explanation assures intersubjectivity by appealing to general laws as justifications. Both the analyst and the listener can maintain the subject position if they communicate in terms of a system external to them both. In other words, an external system of explanation, such as scientific method, provides subject-neutral ground for corroboration. The logical format of scientific method transcends the personal and the particular. Even in the case of disagreement, intersubjectivity remains, since the disagreement is predicated on the same general laws.

In their second article on music theory and scientific method, Brown and Dempster do address directly, albeit briefly, the way in which particularism affects intersubjectivity. Again, they bring up particularism in the context of criticizing Boretz's *Meta-Variations* and his response[136] to "The Scientific Image of Music Theory" in the colloquium presented in the same issue of the *Journal of Music Theory* that I referenced above. Brown and Dempster summarize Boretz's theoretical framework as a subjective, "psychoanalytical" process that rests ultimately within the individual:

> At the risk of oversimplification, we can summarize Boretz's image of music analysis/theory as follows. Music theory is a process of intellectual introspection that uncovers a composer's or theorist's musical cognitive tendencies, allowing him or her to be liberated from these tendencies. In this way, music theory is radically subjective in its epistemic grounding, particular in its attention to music, and purely creative in it goals. Moreover, for Boretz, music theory need be general only insofar as it is an extremely flexible conceptual resource, adaptable to the musical preferences of the listener/composer and the particularities of sounds or compositions subject to musical cognition....The most unattractive feature of this psychoanalytic image is that it asks us to abandon all hope of intersubjective corroboration for analyses/theories. According to Boretz, analyses/theories are driven only by the creative and therapeutic preferences, inclinations, needs, or prejudices—however liberated—of the individual listener or composer. Such an image leaves no room at all for the notion that analysis might be more or less true to the musical facts or that theories might be more or less fitting for particular pieces.[137]

This passage reinforces their previous suggestion that particularism relies ultimately on a mode of hearing for justification. It also expresses that such an intensely personal method of analysis does not readily admit intersubjectivity.

Later on, Brown and Dempster point out that Boretz thinks of analytical methods as cognitive "filters" through which we perceive music and select the information from it that

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[136] Benjamin Boretz, "The Logic of What?" *Journal of Music Theory* 33 (1989): 107-116.
[137] Brown and Dempster, "Evaluating Musical Analyses and Theories," p. 251.

impresses us. The problem with this view is that it does not escape the necessity of determining what criteria shape these so-called filters:

> What, then, should guide our choice of head filters? Boretz selects with an eye—or an ear—towards maximizing freedom, creativity, novelty and originality. Yet, this view quickly collapses into a form of musical solipsism in which analyses become personal expressions insulated from intersubjective confirmation or criticism. If constrained only be creative impulse or free imagination, the ways one decides to hear or analyze music cannot be corrected by anyone else.[138]

Quite clearly, then, Brown and Dempster insist that a scientific music theory, with its dependence on general laws external to the analyst, provides the surest path to true intersubjectivity. I have chosen to focus on this consideration since it carries the most impact and consequences for analytical methodology. Of course, to reiterate, the arguments that Brown an Dempster present are predicated on the desire for a musical theory and analysis that is explanatory in the strictest sense of the term, as opposed to mere description, however sophisticated and convincing, of a particular interaction with and interpretation of music. A few more advantages of a scientific music theory will arise indirectly in the context of the next discussion.

3. D-N Revised: Making Science Practical

In this section, I return to the logical form of scientific explanation that we encountered directly in the previous chapter (in contrast to Babbitt's mere suggesting of it in reference). I invoke this framework again in order to illustrate two more aspects of Brown and Dempster's scientific image of music theory: 1) what Brown and Dempster mean when

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[138] Brown and Dempster, "Evaluating Musical Analyses and Theories," p. 252.

they say "scientific"; and 2) the reformulation of the D-N model of deductive explanation to include statistical generalization.

Contrasting Babbitt's approach, Brown and Dempster do not couch their understanding of scientific music theory and analysis in convoluted and thorny prose. Instead, they clearly state the scientific nature of their framework in a simple formulation. In their terms, music theory and analysis is properly scientific when it relies upon the explanatory power of general laws:

> We are prepared to describe our view as "scientific" because we insist that music theories and the analyses governed by them presuppose law-like generalizations.[139]

For Brown and Dempster, "scientific method" is synonymous with "generalization" and "laws." The association between their understanding of music theory and science, then, is direct and literal. Their entire theoretical framework hinges upon the application of general laws.

Furthermore, Brown and Dempster enhance their image of music theory as scientific by stating what they take to be the proper objectives of theory and analysis. In addition to their broad invocations of "reason versus emotion" and "explanation versus description," Brown and Dempster also propose that music theory conform to those essential features of scientific explanation that we saw in the previous chapter, i.e. empiricism and prediction:

> On the other hand, we claim that a theory is appropriate for a given piece or repertory is it is internally consistent, if it is comprehensive and strongly confirmed, and if it predicts how similar events might occur in related music. In short, we believe that music analyses/theories should be evaluated according to their truthfulness or empiric adequacy and to their predictive or explanatory power.[140]

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[139] Brown and Dempster, "Evaluating Musical Analyses and Theories," p. 248.
[140] Ibid.

We notice here that this empirical prescription for music theory and analysis directly contrasts the particularist reliance on hearing. A hearing is not directly *observable* as such, even though it is perceptible. Brown and Dempster propose that their framework does not require the hearability of analytical statements, another advantage that scientific method provides:

> Although we insist that analyses/theories should fit with reality, we do not imply that the theoretical entities and structures themselves need be directly audible; all we claim is that, sooner or later, the theoretical entities postulated by an analysis have empiric consequences.[141]

> 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.[142]

This empirical quality of scientific analysis specifically provides the necessary external criteria that facilitate intersubjectivity. An observable and demonstrable analytical statement is more accessible for intersubjective corroboration than a private hearing for all the reasons we outlined in the previous section.

Supporting the idea of empiricism as most properly intersubjective, Brown and Dempster point out that Babbitt also prescribes that the musical structures an analysis can reveal need not be directly audible. This is not surprising given Babbitt's commitment to privileging empirical analysis, as we have already seen. In another published lecture in the collection *Words About Music*, Babbitt discusses his own twelve-tone work *Composition for Four Instruments* and asserts that the original set always exerts its structural presence even though not always directly audible:

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[141] Brown and Dempster, "Evaluating Musical Analyses and Theories," p. 248.
[142] Brown and Dempster, "The Scientific Image of Music Theory," p. 96.

> Now what happened was that this expert [left unnamed] wrote me finally one day and said, "Look, I've got a score and a tape of this piece, but where the hell is the row, or the series."...Always throughout this piece, for example, it's [the original series] there in a very decisive way. It's continuously, thoroughly, and utterly influential, but in constantly different ways and in constantly different degrees of explicitness and acting at various distances from the surface of the piece...The series is in there exerting its influence constantly without being explicitly present.[143]

For Babbitt, then, an analysis that reveals the structural influence of the original twelve-tone series is still meaningful even when we cannot perceive that influence directly. It does not matter to him whether or not we can explicitly hear the series; regardless, the series is significant as the central structural element of the music.

Regarding Brown and Dempster's notion that music theory and analysis should be predictive (thus contributing to its scientific character), we must note an important qualification concerning this attribute. Citing the role of prediction in the context of the physical sciences, Brown and Dempster assert that prediction must be understood in a general way. In other words, music theory can predict certain types of events but not always the specific features of them:

> 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 individual events *per se*, all we can say is that, under a given set of conditions, some type of object of 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.[144]

Prediction applies to classes of events but not to the particular features of those events.

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[143]Milton Babbitt, "The Twelve-Tone Tradition," in *Milton Babbitt: Words About Music*, ed. Stephen Dembski and Joseph N. Straus (Wisconsin: The University of Wisconsin Press), 1987. p. 26-30.

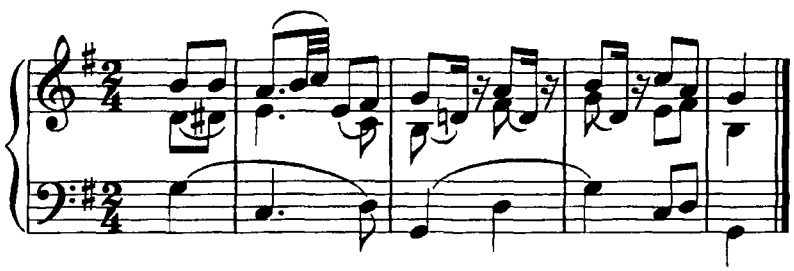
[144] Brown and Dempster, "The Scientific Image of Music Theory," p. 92.

We can illustrate this process by looking at a passage from Brown's *Explaining Tonality*. Discussing the character and function of general laws, Brown gives us an example of a law concerning leading tones: "If the leading tone appears in tonal contexts, then it normally ascends by half step onto the tonic."[145] After this, Brown points out, with the musical example below (fig. 1[146]), that this law cannot be universally true; rather, it predicts only generally how the leading tone behaves within a tonal context:

> This law explains why the melody rises from F# to G in mm. 13-14 [and] why the alto part follows suit in mm. 14-15...Having said this, it is important to note that the law governing leading tones is not true all of the time. In figure I.1a, for example, [the music given in my fig. 1] the alto F# in mm. 15-16 moves to B3 and not to G4, presumably to avoid doubling the soprano part. Since most laws of tonality are generally but not universally true, they are best classified as law-like generalizations.[147]

Figure 1: The Law of the Leading Tone

Beethoven, Six Variations, WoO 70, mm. 13–16.



The proposed law of the leading tone, then, does not specifically predict the voice leading in the alto voice at the final cadence of the example in fig. 1. In some ways, however, the law predicts the cadence generally; for, the leading tone in the alto voice rises virtually with the

[145] Brown, *Explaining Tonality*, p. 4. This definition is not seriously made and defended. Brown constructs it merely hypothetically in order to illustrate the character of general laws about music.
[146] This musical example is taken directly from *Explaining Tonality*, pg. 3.
[147] Brown, *Explaining Tonality*, p. 4.

replacement of its own tonic by the soprano's. This is a normal voice leading pattern at perfect cadences.

We might go further to explain this cadence by joining Brown's law of the leading tone to anther hypothetical law concerning triadic part-writing in tonal contexts. We may propose something like this: "When writing triadic tonal counterpoint, a minimum of two notes of any chord, most preferably the tonic and the third, must be present at all times." Brown's own comment about doubling the soprano voice in the quotation above presumes a law like the one I have just made. Of course, my own law about triadic voice leading must also rely on other laws as to why the tonic and third of any chord are to be preferred, as opposed to the tonic and fifth, and so on. Regardless of the lack of completeness of the examples I have invoked, the main point comes through: prediction within scientific explanation, in Brown and Dempster's view, can only be expected to function generally—in terms of classes rather than individuals.

In several of the quotations I have given above, Brown and Dempster use a term that deserves particular attention: *viz.* "law-like" generalization. This contrasts with Babbitt's invocation of "general laws." Indeed, Brown and Dempster make the distinction themselves when they assert that laws of tonality are "best classified as law-like generalizations" because they are not "universally true." Far from a mere difference of terminology, the distinction between law-like generalizations and general laws has profound methodological consequences for the scientific image of music theory. Brown and Dempster are implying that "law-like" generalizations are not *universals*, which the D-N model requires for

explanation. The designation "law-like" arises from an important revisions Brown and Dempster make to Hempel's D-N model of scientific explanation, to which I turn now.

As we recall, the Deductive-Nomological model works with universal laws that "cover" or subsume particulars. In this way, it uses deductive reasoning: particular occurrences are explained by appealing to the universal concepts that govern them. Brown and Dempster question one of the key features of this model, i.e. the *universals* that govern the hierarchical scheme of explanation. They note that the practicality of the strict form of the D-N model has encountered criticism from the philosophical community:

> However, in the years since Hempel first outline 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.[148]

D-N has been evaluated as an idealized logical paradigm that can rarely be attained in practice. With a sensitivity to the progressions and changes within the philosophy of science,[149] then, Brown and Dempster endorse a revised, less idealistic version of D-N.

Specifically, Brown and Dempster alter the universal character of the laws that work within D-N. They alleviate the methodological burden (and practical impossibility) of finding universal laws by replacing them with statistical generalizations, concepts that are valid for the majority, but not the totality of individual cases[150]:

> 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

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[148] Brown and Dempster, "The Scientific Image of Music Theory," p. 96.

[149] Brown and Dempster cite the influence of several prominent logicians including: W. V. Quine, Thomas Kuhn, Rudolph Carnap, Nelson Goodman, Bas van Frassen, and Monroe Beardsley. My bibliography references the pertinent writings of some of these authors.

[150] Recall that Hempel and Oppenheim explicitly contrasted statistical generalizations with the universal laws necessary for D-N. See chapter 2: **Deduction, Laws, and Axioms**.

> 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.[151]

In this way, scientific method is more easily applicable to music theory, which deals with such a diverse and varied aesthetic object. For example, returning to our example of the leading tone, we can say: "within a tonal context, the leading tone rises to the tonic at a perfect cadence *most of the time*." The last qualifier is key: this law-like generalization can allow for the anomalies that certainly exist.

It is important to note in passing that the presence of such anomalies does not confirm the law-like generalization. This is not a fallacious instance of "the exception that proves the rule." Rather, as more and more instances of non-conformance are uncovered through analysis, the law-like generalization is subject to revision given the new empirical data available. (More on this issue in the next and final section of this chapter.) Brown and Dempster, also, are not watering down the understanding and application of scientific method: we cannot charge their framework as "un-scientific." As I mentioned above, Brown and Dempster are simply taking into account the most recent developments in the debate over scientific explanation.[152]

This important revision of D-N also lies at the heart of Brown and Dempster's proposal for a reconsideration of a scientific music theory and analysis. With statistical generalizations in place of universals, the scientific image of music theory becomes more feasible, practical, and inviting. We may have been sceptical to adopt scientific explanation

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[151] Brown and Dempster, "The Scientific Image of Music Theory," p. 96-97.
[152] A fascinatingly complex debate that we, unfortunately, cannot enter into at this time.

within music theory since essentially musical universals are difficult to determine (as difficult to pin down in any other fields of inquiry as well). Babbitt himself spent a good deal of effort in his methodological essays warning us to be wary of choosing musical primitives too lightly: "the decision is not easily made or ever surely made."[153] Brown and Dempster's acceptance of the revised D-N model sidesteps this difficulty that Babbitt is unable to resolve. The next chapter will illustrate Babbitt's inherent scepticism of finding musical universals. Perhaps if Babbitt had also accepted the revision of the D-N model, he would have found the reconciliation with scientific method that he was looking for.

Renaturalizing Music Theory: Science and Aesthetics

In this section, I briefly introduce Brown and Dempster's ultimate justification for the propriety of applying scientific method to music theory and analysis. Their framework incorporates to a great degree various methodological debates within the philosophy of science. The progressions within this field (unfortunately for us) constitute another line of inquiry not immediately pertinent to the scope of my present project.[154] I will address this topic, however, in order to point out the direction that further research beyond this thesis could take. Besides an investigation into how scientific method can operate within music theory and analysis, we must ask ultimately whether and how such a combination of theoretical frameworks is appropriate. This section introduces the answer that Brown and Dempster give.

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[153] Babbitt, "The Structure and Function of Musical Theory," p. 194.

[154] Additionally, the framework that Brown and Dempster put forward to justify their scientific approach to music theory has not been reconsidered (at least within the field of music) since they published in their ideas. They themselves only point toward the door that needs to be opened. Matthew Brown confirmed this point in a personal communication to me.

As we recall, Babbitt also attempts to justify the evaluation of music theory and analysis in terms of scientific method. He makes the pithy observance that if music theory is to be a theory in any sense of the word, then it must conform to scientific method. For Babbitt, the point is "obvious to the point of virtual tautology."[155] The relationship between music theory and science, however, is not as clear as Babbitt makes it out to be. Fortunately, Brown and Dempster give a little more justification for their approach than Babbitt does.

Brown and Dempster affirm the epistemological value of applying scientific methods of inquiry to an essentially aesthetic object such as music. This position counters some claims that they address at the beginning of "The Scientific Image of Music Theory": e.g. that scientific method only works for explaining the physical world, and not for artistic productions. I will focus the discussion in this section of Matthew Brown's article "Adrift on Neurath's Boat: The Case for a Naturalized Music Theory."[156] In it—as the title implies—Brown appeals for a "naturalized music theory," or, a music theory that seeks law-like connections between the aesthetic and non-aesthetic properties of music:

> For my part, I prefer to naturalize music theory. Such a view emphasizes the need to find law-like relationships not only among aesthetic properties, but also between aesthetic and non-aesthetic properties. It blurs the line between music theory and cognitive psychology and it erodes the distinction between theory, analysis, and criticism.[157]

As inspiration for his own independent investigation of the subject, Matthew Brown draws on work by Douglas Dempster, with whom he worked so closely to publish the articles we

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[155] Babbitt, "The Structure and Function of Musical Theory," p. 193.
[156] Brown, "'Adrift on Neurath's Boat': The Case for a Naturalized Music Theory," *The Journal of Musicology* 15 (1997): 330-342.
[157] Ibid. p. 341.

have studied so far. It is appropriate, therefore, to offer a small glimpse into Dempster's article that influenced Brown: "Renaturalizing Aesthetics."[158]

Addressing his topic, Dempster includes a short discussion of music. In this passage, Dempster identifies a possible connection between how we respond aesthetically to music and certain aspects of its structural design. In effect, Dempster claims that musical structure and aesthetic response share a close bond. We cannot fully understand one without the other:

> Nonetheless, those mental states [i.e. aesthetic response] may still not accurately represent the structural properties possessed by some passage of music that constantly correlate with musical judgements of "sprightliness." It is those correlations, presumably, that we want to understand when we want to understand the expressive character of music.
>
> Here's an analogy to illustrate the point. Musical compositions, like other works of art, are described and evaluated as being either more or less unified. And many people would say, and quite correctly I think, that there is a kind of unity or coherence to tonal music that distinguishes it from other large stylistic classes of music, say Indian music, or free atonal, or totally serial music. Do these people know what it means to say that a piece of tonal music is "unified?" In some trivial sense, of course, they do. Their judgements of "musical unity" constitute a linguistic and aesthetic norm for what pieces are musically unified. They *may* even share some common thought or sensation which occurs to them upon encountering a piece of music they regard as musically unified. But in another sense most people haven't the slightest idea what it means to say that a piece of tonal music is unified because that have no idea what features of the score, or performances, or sound-waves account for a piece having a characteristically unified appearance. The most widely accepted account of unity in tonal music claims that tonal unity is dependent upon a peculiar sort of hierarchical structuring of relatively simple, embedded harmonic and voice-leading patterns. But there is no way that one could know all that, assuming it's true, simply by reflecting on one's concept or feeling of musical unity.[159]

Both the aesthetic response and empirical investigation—obviously, Dempster's appeal is to Schenkerian analysis—contribute to the understanding of a piece of music as "unified."

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[158] Douglas Dempster, "Renaturalizing Aesthetics," *The Journal of Aesthetics and Art Criticism* 51, Philosophy and the Histories of the Arts (1993): 351-361.
[159] Ibid. p. 355-356.

Dempster's way of "naturalizing" music theory is to point out the possible correlation between aesthetic judgement and structure: both necessarily contribute to a complete understanding of music. This framework immediately asserts the relevance of applying empirical, scientific investigation to an aesthetic object.[160] For Dempster, the feeling of unity, an aesthetic response, is confirmed and complemented by an empirical analysis that reveals those aspects of the music that a higher-level theory designates as significant for the achievement of this unity.

Following Dempster's line, Brown also accepts the possibility of a connection between the structural and aesthetic properties of music. As we saw in the quotation above, he regards the role of a "naturalized" music theory as an exploration of these potential connections. Brown, however, adds his own important insight. Considering that structural properties can be intimately bound with the aesthetic ones, Brown adopts a relaxed view of empiricism—one that takes the downfall of Positivism[161] into consideration. In this new framework, empirical statements are no longer presumed to carry the force of autonomous and objective truth. Rather, they are characterized as "adequate"—instead of "true"—according to whether they fit accurately the musical facts or not:

> It [naturalized music theory] puts a premium on the idea that music theories and analyses should be judged according to their empiric adequacy—that is, whether

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[160] Music theorists, of course, have explored the connection between musical structure and aesthetic response in interesting ways. As an example see: David Epstein, "A Curious Moment in Schumann's Fourth Symphony: Structure as the Fusion of Affect and Intuition," in *The Practice of Performance: Studies in Musical Interpretation,* ed. John Rink, 126-149 (Cambridge: Cambridge University Press), 1995.

[161] Positivism refers to an epistemological paradigm that privileges phenomenal experience as the basis of all knowledge. As such, Positivism holds that empirical observations can hold the status of objective and autonomous truths. As Brown points out in "The Scientific Image of Music Theory," and "Adrift on Neurath's Boat," Logical Positivism is no longer viewed as a credible epistemology. W. V. Quine, whom Brown relies on, was a prominent critic of Positivism. Also, see "The Scientific Image of Music Theory," p. 67-71, for a summary of the tenants of Positivism.

> they fit the physical facts—and their predictive power—that is, whether they predict how particular musical relationships will occur in future instances.[162]

We refined the notion of prediction in an earlier part of this chapter: a naturalized music theory predicts in the same general sense that Brown and Dempster require for a scientific music theory and analysis.

With empirical statements liberated from the unrealistic Positivist burden of autonomy and objective truth, Brown ties the framework together by citing the metaphor of Neurath's Boat.[163] In this illustrative analogy, empiricism lies within a continuous process of reformulation according to new material that arises:

> According to the story, empiricists resemble sailors at sea on a leaking boat. Instead of rebuilding their boat from the keel up in a dry dock, they fix the leaks while adrift on the open water. As each plank is replaced, the remaining timbers keep the craft afloat. But once one leak is patched another appears; bit by bit the boat become transformed, being carried along by nothing but the evolving conceptual scheme itself.[164]

Instead of positing autonomous universals, then, music theorists approach their task *in media res* building on previous empirical research and constantly reformulating the theoretical principles as new evidence becomes available. Brown summarizes the process of empirical research in this way:

> In other words, empirical research is always open ended. Researchers do not begin with a blank slate, they do not have fool-proof methods, and they do not reach definitive solutions. Instead, they plunge *in media res*. They must tentatively believe all of their inherited world view, but they must also realize that some unidentified potions are wrong. They must improve, clarify, and understand by trading off evidence with system: too much evidence creates a mere record of observations; too much system creates a myth without foundations. By focusing on

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[162] Brown, "'Adrift on Neurath's Boat'" p. 338.
[163] As Brown points out, the Viennese logician Otto Neurath first referred to his proverbial boat in 1932, whereupon Quine popularized the metaphor. The quotation in Brown's title is from a paper by Quine of the same name.
[164] Brown, "'Adrift on Neurath's Boat,'" p. 337.

> the way in which the empiricist justifies his beliefs, epistemology grades off with psychology, thereby naturalizing the former.[165]

In other words, empirical research must most properly be viewed as a theory of evidence instead of a theory of objective truth.

The revised empiricism that Brown endorses in this article provides a useful concluding framework for our understanding of the scientific image music theory that he creates with Dempster. If the empirical primitives of a scientific music theory need not be universal concepts—we may ask—how they might we say that it can be truly explanatory? Statistical generalizations are indeed practical and convenient, but in the end, how do they accomplish a complete understanding?

Neurath's Boat begins to answer these questions for us. As a theory of evidence, empirical theory does not become a matter of absolute truth; rather, theorists must content themselves with the body evidence currently available and accept it as the foundations of the theory. In other words, an empirical theory, if accurately portraying the musical facts, is true *for now*; that is to say, it is constantly subject to revision. This framework fits well with Brown and Dempster's statistical generalizations since it alleviates the methodological need for universal truths. We can hold onto the temporary completeness of our statistical generalizations *because* we now accept that the search for empirical universals is futile and unnecessary. Therefore, statistical generalizations take the place of universal primitive concepts in a hierarchical system of explanation.

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[165] Brown, "'Adrift on Neurath's Boat,'" p. 337.

Brown and Dempster, then, have done their work to make the scientific image of music theory more palatable and accessible. They contrast the outmoded rigidity of Babbitt's scientific prescriptions for theory and analysis. As we will see in the next chapter, however, Babbitt's framework is not as inaccessible as it may seem; for in the end, he does not accept the possibility of finding universal empirical concepts for music.

Chapter 4: Babbitt and Brown

1. Bridging the Gap

In this section of the thesis, I consider Milton Babbitt again in light of Brown and Dempster's characterization of his scientific framework for music theory and analysis. In "The Scientific Image of Music Theory," Brown and Dempster contrast Babbitt's approach with their own by labelling him a particularist, which suggests that Babbitt's theoretical and analytical prescriptions are not scientific in the strictest sense. Unfortunately, Brown and Dempster do not discuss explicitly the details of this characterization: one brief citation—which we will see below—suffices to close Babbitt's case. An understanding of this charge of particularism, therefore, must rely upon a close reading of the context surrounding the passage from Babbitt to which Brown and Dempster refer.

The first part of this section considers Babbitt's constant insistence upon the use of scientific language for music analysis. Endorsing a governing scientific method for analytical explanation, it would seem as if Babbitt requires scientific language within the methodology as a whole. This exaggerated dwelling upon scientific language seems unnecessary. We may resolve this dilemma by positing that Babbitt is ultimately reluctant to endorse the supreme explanatory power of general laws, or law-like statements. (As we recall, the explanatory power of general laws forms the core of Brown and Dempster's scientific music theory.)

In the second part, I examine Babbitt's commentary on an analysis of Webern's *Concerto for Nine Instruments*. Specifically, I will focus on the mathematical formula

Babbitt constructs to counter an *ad hoc* analytical statement that some analysts have proposed. Whereas we would expect a formula to possess more explanatory value, Babbitt locates it ultimately within a certain hierarchy of genuinely *ad hoc* analytical statements. In this account, the formula is not explanatory, but merely a sophisticated description.

In the third section, I outline what I call the particularist problem: the reasons why Babbitt is forced to endorse particularism, according to Brown and Dempster's definition of the term. Ultimately, Babbitt cannot foresee the probability (despite the theoretical possibility) of discovering inherently musical principles capable of functioning as primitives, or axioms within an explanatory system. (At this point, I reintroduce Carl Hempel and Paul Oppenheim's description of the chain within general laws that leads back to a final explanatory cause.) Adopting particularism, Babbitt cautions us about forming general explanatory laws about music. He does this by distinguishing between an *object language* and a *metalanguage*; respectively, the music itself and theoretical formulations that we use to talk about it.

The fourth section illustrates Babbitt's particularism with recourse to his own analytical writing; specifically, the analysis of Schoenberg's *Fourth Quartet* that Babbitt develops in "Set Structure as a Compositional Determinant." I propose here that Babbitt resolves this methodological dilemma (object language vs. metalanguage) by defining explanatory primitives from within individual works. With this method, each piece of music defines its own unique explanatory laws. This approach is not scientific in the strict sense that Brown and Dempster endorse; scientific explanation relies upon general laws that

transcend the unique features of individual compositions. The scientific music analyst must abandon the prospect of understanding individual pieces of music as unique entities. In essence, however, Babbitt's approach is a logical resolution: without the ability, or even the desire to form general laws about music, the critical integrity of scientific language and method can be retained only by choosing explanatory primitives on a piece-by-piece basis.

2. A Linguistic Dilemma

As I have established already, Milton Babbitt and Matthew Brown[166] form the two conceptual pillars of this thesis. Both endorse the application of scientific method within music theory and analysis, and both address the issue directly. Looking at their work in detail, however, we realize that Babbitt and Brown are not in agreement: the passage of time between their respective articles has not left the scientific image of music theory unaltered. We will now investigate the difference between the scientific frameworks that Babbitt and Brown describe.

Before charting the territory between Babbitt and Brown, let us take a moment to point out the remarkable similarities in their motivations.[167] Significantly, Brown and Babbitt express their concern for the discipline of music theory in the same language. As we have already seen, Babbitt speaks about "taking theory seriously." Brown and Dempster

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[166] Throughout this section, I sometimes refer to Matthew Brown alone even though I discuss his work with Douglas Dempster for the most part. I do not mean to belittle Dr. Dempster's contributions to the framework: the reference to one author is a little easier to incorporate into the prose smoothly, and Matthew Brown is the music theorist of the two.

[167] Indeed, Brown directly acknowledges his association with Milton Babbitt and the respect he holds toward him: "Finally, I owe a special thank you to Milton Babbitt for encouraging me to publish my ideas in book form." *Explaining Tonality*, Preface: xix.

begin their first collaboration on the subject of a scientific music theory with precisely the sentiment:

> If music 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. 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.[168]

Babbitt associates "taking theory seriously" with an attempt to achieve an explanation of musical events with analysis. He establishes this association, in part, by contrasting "serious", or professional music analysis with note counting and Roman Numeral labelling. Brown and Dempster use the phrase in the same way: for them, as in the quotation above, "taking theory seriously" indicates an explanatory activity. (I interpret the phrase—"clarify the nature of music"—to be equivalent to explanation.)

Furthermore, Brown and Dempster's qualification of *rational* aligns with Babbitt's comments concerning analytical language. Babbitt endorses scientific linguistic formulations in order to establish the critical distance between the analyst and the music that demands accountability and integrity. For Babbitt, scientific language purges the assumptive and essentially irrefutable personal statement from music analysis:

> But the problems of our time certainly cannot be expressed in or discussed in what has passed generally for the language of musical discourse, that language in which the incorrigible personal statement is granted the grammatical form of an attributive proposition, and in which negation—therefore—does not produce contradiction; that wonderful language which permits anything to be said and virtually nothing to be communicated.[169]

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[168] Brown and Dempster, "The Scientific Image of Music Theory," p. 65. Italics added.
[169] Babbitt, "The Structure and Function of Musical Theory," p. 192.

Brown and Dempster's *rational* music theory achieves the same goals as Babbitt's scientific language: it rids theory and analysis of fundamentally personal attributions, or "emotive acts." Although Brown and Dempster are not referring specifically to analytical language in the passage above, it seems as if restrictions on analytical language follow naturally from an adoption of scientific method. The extension from methodology to the language that enacts it seems logical and necessary. For how can scientific methodology exist without scientific language, and *vice versa*?

As obvious as this connection between language and method may seem, we have come across a paradox that cannot be ignored. This question arises: if scientific method and language are inextricably linked, then why does Babbitt emphasize the importance of scientific language? Would not a complete exposition of scientific method within music analysis have been enough? After all, Brown and Dempster have very little, if anything, to say about language in isolation. Interestingly, the answer to this question is found in the same place as the difference between Babbitt and Brown's respective paradigms for the operation of scientific method within music theory and analysis. In fact, noticing this surface-level difference between Babbitt and Brown is the first step in sorting out the distance between them.

In the chapter dedicated to Babbitt's framework, I noted the influence that he has had across a wide spectrum of musicians; in particular, I provided a quotation from Marion Guck to this effect.[170] In another essay titled *Rehabilitating the Incorrigible*, Guck re-evaluates

[170] See Chapter 1: **3. The Scientific Music Theorist Discourse and System.** I quoted from: Guck, "Rigors of Subjectivity."

Babbitt's prescription of scientific method and scientific language. For her, this framework can dispose ultimately of scientific language requirements in favour of scientific method alone, which she interprets merely as an offering of concrete, demonstrable evidence for personal assertions about music:

> The really serious issue is akin to that of providing empirical support for scientific statements. The grounds for making a statement must be made available for scrutiny and evaluation by the community to whom it is made. Babbitt finds it indefensible if no reasons are—or can be—given for an attribution. This fear is warranted, and must be addressed if analytical and critical discourse is not to risk slipping back into the morass of verbal carelessness and self-indulgence that Babbitt's papers first brought to our attention.[171]

Guck claims that we can rehabilitate, or acknowledge the value of personal statements as long as the analyst who makes them is careful to include empirical reasons for any such assertion, and thereby maintain a professional intersubjectivity and integrity.

In order to illustrate her point, Guck offers an analysis of a familiar moment in Mozart's G-minor Symphony, K. 550, and certain notes she describes as "portentous."[172] Guck offers empirical evidence for this personal and incorrigible statement, thereby "rehabilitating" it. She expresses her point concisely in the conclusion of the essay:

> This paper bears a complicated relation to Milton Babbitt's methodological work. It adopts his analytical goals, but, to achieve them, demotes the claims about '"scientific" language', by which he is best known, in favour of the other half of that famous duality, '"scientific" method'.[173]

Admittedly, Guck applies a liberal interpretation to Babbitt's framework. Thus, the kinship between them is in spirit rather than letter. She makes a powerful point, however: Babbitt's linguistic and methodological prescriptions can be successfully separated. Her analysis

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[171] Guck, "Rehabilitating the Incorrigible," p. 60.
[172] The object of her analysis is the same moment for which, as we have seen, Hans David received such harsh criticism from Babbitt in "The Structure and Function of Musical Theory."
[173] Guck, "Rehabilitating the Incorrigible," p. 73.

clearly supports our discomfort with Babbitt's framework, as expressed above. Why does Babbitt make two claims? Why does he spend so much time talking about appropriate analytical language when it should follow logically from scientific method?

Our unease with Babbitt leads us to the single most important aspect of scientific method that Brown and Dempster uphold: the formulation of general laws, or law-like statements, that explain individual musical occurrences. For Brown and Dempster, this is the crux of the scientific framework:

> We are prepared to describe our view as "scientific" because we insist that music theories and the analyses governed by them presuppose law-like generalizations.
>
> ...Music theorists widely endorse the notion that a music analysis should be "something more" that a mere descriptive catalogue of the elements and relationships evident in a piece or class of pieces... [Our scientific image of music] accounts for the "something more" by insisting that explanations are grounded in law-like generalizations.[174]

The primary objective of scientific explanation, from this perspective, is the "formulation of powerful law-like generalizations about potentially infinite classes of phenomena."[175] Brown and Dempster insist unrelentingly on general laws. Also, they modify the scope of such laws to make scientific music theory more practical and palatable to music analysts who may overestimate the price of scientific method.

Herein lies the difference between Brown and Babbitt: Babbitt never accepts the fundamental and ultimate explanatory power of general laws, or law-like statements, within

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[174]Brown and Dempster, "Evaluating Musical Analyses and Theories: Five Perspectives," *Journal of Music Theory* 34 (1990): pp. 248 and 249.

[175] Brown and Dempster, "The Scientific Image of Music Theory," p. 82-83.

music analysis.[176] For Babbitt, generalized laws at best provide the upper bound of *ad hoc* statements: only context-specific interpretation reveals explanatory causality. In the absence of powerful explanatory laws, Babbitt's framework can easily boil down merely to offering empirical reasons for analytical insights: any reason will do as long as it is verifiable—and therefore also falsifiable—by the general community of music theorists. Without the governing superstructure of law-like generalizations, scientific method reduces to amassing empirical evidence for one's personal analytical assertions. In order to demonstrate Babbitt's rejection of general laws in the next section, I will introduce Babbitt's assessment of an analysis that presumes to explain one of Webern's set transformations in the *Concerto for Nine Instruments*.

Thus, Marion Guck can properly assert and uphold her kinship with Babbitt's framework:

> In telling my tale, I will be on dangerous ground, because I conceive of the immigrant Cb as animated and motivated—but I intend to stay here. My underlying contention is that *musical structure can be modelled in terms of patterns in other domains of experience*, included motivated action, with useful and significant analytical results.[177]

For Guck, her "incorrigible" statement about the expressive character of a single note can be rehabilitated sufficiently to pass as Babbitt-like as long she ties empirical observations concerning musical structure to "other domains of experience." (I do not believe that Babbitt would ultimately endorse this application of his analytical framework: I merely suggest that Guck's argument is an entirely justified extrapolation.)

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[176] In a personal communication to me during a visit to the Eastman School of Music, Professor Matthew Brown articulated this as the crucial difference between Babbitt's scientific framework and his own. He suggested that the insistence on law-like generalizations is perhaps the only element that separates their work.
[177] Guck, "Rehabilitating the Incorrigible," p. 68. Italics added.

3. Webern Revisited: Explanation or Sophisticated Description?

Let us begin with Brown and Dempster's characterization of Babbitt in "The Scientific Image of Music Theory." Citing a passage from "Contemporary Music Composition and Music Theory as Contemporary Intellectual History," Brown and Dempster label Babbitt as a particularist:

> 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 [i.e. other than Boretz] 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.* [178]

(Recall the definition of particularism from the previous chapter.)

We can understand Brown and Dempster's charge of particularism only by exposing the specific context of this quotation from Babbitt. Initiating an extended discussion of analytical explanation, Babbitt challenges an analysis of the third permutation of the primary twelve-tone set in Webern's *Concerto for Nine Instruments*.[179] As he points out, a popular explanation of this specific permutation highlights the pitch-class invariants between discrete, contiguous trichords of the derived set:

> Why this transposition rather than any one of the ten others available? It would be and has been regarded as acceptable to offer the "explanation" that this particular transposition has the result of holding at least one note in common between each of the trichords in this set form and of the preceding set form (and necessarily of the initial set form).[180]

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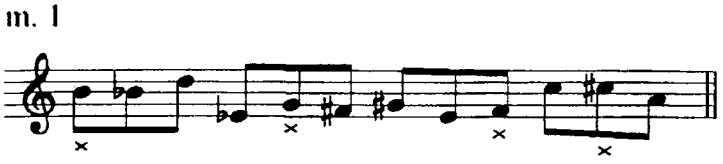
[178] Brown and Dempster, "The Scientific Image of Music Theory," p. 82. I also note at this point that Brown and Dempster only cite or discuss Milton Babbitt once in this article. It seems as if their assessment of Babbitt is made unwillingly, due to the strong conceptual and critical kinship between them.

[179] See Figure 1. The musical examples are copied directly from: Babbitt, "Contemporary Music Composition and Music Theory as Contemporary Intellectual History."

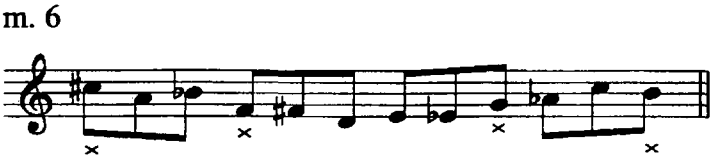
[180] Babbitt, "Contemporary Music Composition," p. 278.

Figure 1: ***Concerto for Nine Instruments***

a: Webern's original twelve-tone set



b: The third permutation (transposition) of the original set



After noting this analysis, Babbitt constructs a formal, mathematical equation that generalizes this exact property of Webern's original set:

> If a collection of $n = pq$ elements is partitioned into p classes of q elements each in any two ways, then there is at least one collection of p elements (termed a common system of representatives) such that in both partitions each of the p classes contains one of the p elements.[181]

The final result, consequently, is that any transposition of Webern's set possesses the property of retaining pitch-class invariants among distinct trichordal partitions under the operation of transposition:

> But if we interpret "elements" as "pitch classes," and "partitioned" as "partitioned instrumentally or timbrally" (as any twelve-tone set or aggregate necessarily is in a compositional presentation), then it follows that with $n = 12$, and $p = 4$, and, therefore, $q = 3$, any such instrumental presentation of *any* transposition of the set (and, of course, any form of the set) would have the property of maintaining at least one pitch-class in common between trichords.[182]

[181] Babbitt, "Contemporary Music Composition," p. 278.
[182] Ibid. p. 278-279.

This is an essentially *formal* theory since the descriptive elements are musically uninterpreted.

Babbitt goes on, therefore, to deny the initial analytical statement any substantial explanatory significance:

> Could one be said to have explained a particular occurrence, a particular choice, by citing a property necessarily associated with a class of occurrences, of choices?...It is known immediately that pitch-class identity between trichords cannot be offered to "explain" uniquely even the fact that any form of the set would have provided the property, for the formal theorem assures this property for any assignment of three notes to each of the four partitioning instruments.[183]

It merely points out one instance of a universal, unalterable property of Webern's original set. In this sense, it does not move beyond description: the analysis is genuinely *ad hoc*.

What, then, can we say of Babbitt's mathematical formula that generalizes and abstracts this particular property of the original set? Contrary to what we may initially suppose, Babbitt does not see the formula as a better, more significant or explanatory analytical statement! Here, we find a precedent for Brown and Dempster's own sentiment:

> 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.[184]

Looking back at Brown and Dempster's quotation from Babbitt, we realize that Babbitt regards the general formula as the "upper bound" of a potentially infinite number of truly *ad hoc* analytical statements that merely describe the musical surface, even though it

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[183] Babbitt, "Contemporary Music Composition," p. 279.
[184] Brown and Dempster, "The Scientific Image of Music Theory," p. 81.

may be a particularly sophisticated description. Brown and Dempster do not fill in the rest of the passage that makes this point clear, as I provide below:[185]

> So, for all the justified warnings that the formally valid is not necessarily empirically true, or even empirically meaningful, the pertinent formal theorem obliges one to change drastically the scope of what is generally taken to be a germane statement of "explanation," and the basis for the change could not imaginable have been arrived at by any but formal means, since it involved showing that any trichordal partition would possess that attribute which had been assumed to be uniquely possessed by the set form presented. But if uniqueness is, as is more than tacitly implied here, a necessary condition for satisfactory "explanation," it is surely not a sufficient condition. For if it were, any characteristic of the set whose transpositional level was to be "explained" which differentiates it from the preceding set would function as such a condition. This provides another boundary (a lower bound, if you will), ad hoc, to oppose the upper bound provided by the theorem in this application.[186]

For Babbitt, then, the analysis in question mistakenly asserts the pitch-class invariance between distinct trichords as an explanatory property of the set form in question. As the formula shows, this property of pitch-class invariance is not unique to this set form, but it is a formal characteristic of Webern's derived set under transposition. The mathematical theorem is the upper bound (or highest in a hierarchical system) of a set of such *ad hoc* statements in that it generalizes this structural property and provides a reasonable basis for uniqueness. It does not truly explain, however, in the sense that it relies ultimately on the normative assumption of this particular transposition over any other: it

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[185] Please refer back to the quotation that opens this section in order to compare it with the more complete on given here.

[186] Babbitt, "Contemporary Music Composition," p. 279. It is worth noting that Brown and Dempster's quotation from Babbitt is curiously sparse. We must assume, however, that Brown and Dempster intend to speak to an audience already familiar with Babbitt's essay; for, no reader unfamiliar with the essay could possibly understand Brown and Dempster's charge of particularism from such a reduced quotation as they give. There is a minor yet noticeable grammatical difference between Brown and Dempster's quotation from Babbitt and the one I give. Brown and Dempster cite a different edition of this article than the one I am using. Theirs comes from this collection of essays: *Perspectives in Musicology*: *The Inaugural Lectures of the PhD program in Music at the City University of New York*, ed. Barry S Brooks *et al.* (New York: Norton, 1972). The most recent edition of Babbitt's essay from which I quote was not yet published when Brown and Dempster wrote "the Scientific Image of Music Theory." I have meticulously compared the two editions at this point in the essay and have found no significant differences in the content of the argument.

does not provide the compositional *cause* of the choice, Babbitt's "sufficient condition" of explanation:

> This inability to discover a sufficient condition is equivalent to observing that any other transposition would have been as musically "coherent" (or "satisfactory" or "—") and that, again, the transpositional choice must be regarded as assumptive.[187]

John Rahn describes the situation in his own synopsis of Babbitt's commentary on Webern. The formal theorem does not explain because it predicts with equal precision many distinct row forms:

> For one thing, this explanation—containing the "law-like" statements that all row forms used have the same "system of common representatives," namely that set {B, G, F, C#}—does not satisfy the criterion of uniqueness: each n-th row-form could be any in the category of those sharing that system of common representatives. Thus this explanation would predict impartially many distinct row-form successions, of which one could be an arbitrary number of repetitions of the first row-form. Obviously, not satisfactory.[188]

In a close and careful reading of the passage that Brown and Dempster cite, then, Babbitt's ultimate endorsement of particularism begins to emerge.

Brown and Dempster's characterization of Babbitt is solidified if we proceed a little further into the analysis of the Webern Concerto to the point where Babbitt considers possible explanations for particular set transformations. As we see in the next section, Babbitt ultimately suggests that analysts must look to the specific compositional importance of set transformations in order to achieve a sufficient explanation of the music.\

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[187] Babbitt, "Contemporary Music Composition," p. 280.
[188] John Rahn, "Aspects of Musical Explanation," *Perspectives of New Music* 17 (1979): p. 207.

4. Babbitt's Particularist Problem: Finding the Primitives

Exploring the implications of the analytical problem highlighted in the example of the Webern Concerto, Babbitt ponders the development of a more satisfactory explanation. Babbitt looks for explanation beyond generalized laws as expressed in terms of uninterpreted formulae. Ultimately, he believes that the particular compositional context of music more closely approaches sufficient explanation than generalized laws. His approach, therefore, is not scientific as understood in the strict sense that Brown and Dempster endorse.

In all fairness to the essays we are studying, however, we must recognize that Babbitt remains largely undecided on the issue. He leaves the issue hanging with no resolution intentionally:

> These and a multitude of related issues are left dangling and unresolved, for I wished merely to indicate that apparently conventional and local questions about music cannot even be clearly formulated, and—therefore—certainly cannot be satisfactorily answered, and that the still larger question as to whether questions about musical compositions are susceptible to resolution in the same form as questions in other fields, without involving genuinely theoretical issues whose scope is decidedly not confined to music.[189]

Nevertheless, Babbitt's reluctance to endorse the supreme explanatory power of general laws lies in the fear of analytical misrepresentation. His final point, I think, is one of extreme caution toward the formulation of general laws, rather than of wholesale dismissal. Around his discussion of the Webern analysis, Babbitt invokes 1) the problem of finding primitives within an explanatory system for music; and 2) a critical distinction between object language and metalanguage, a possible source of analytical misrepresentation. Let us

[189] Babbitt, "Contemporary Music Composition," p. 281.

bring these issues together in order to finalize our interpretation of Babbitt and bridge the gap between his approach and Brown's.

As Babbitt ponders the explanatory value of general laws, such as the mathematical formula he has just given, he comments on the necessary universal character these laws must have in order to be sufficiently explanatory. In other words, we must consider how it is possible to achieve axiomatic primitives in an explanatory system for music. How far back in the chain of causality must we go in terms of music?

At this point, let us bring back an illustration from the previous chapter on the method of scientific explanation, Hempel and Oppenheim's discussion of the distorted oar. As we recall, Hempel and Oppenheim discuss the chain of causality that leads backwards from one general law to another in a hierarchical scheme of explanation:

> Consider another illustration. To an observer in a row boat, that part of an oar which is under water appears to be bent upwards. The phenomenon is explained by means of general laws—mainly the law of refraction and the law that that water is an optically denser medium than air—and by reference to certain antecedent conditions—especially the facts that part of the oar is in the water, part in the air, and that the oar is practically a straight piece of wood.—Thus, here again, the "*Why* does the phenomenon happen?" is construed as meaning "according to what general laws, and by virtue of what antecedent conditions does the phenomenon occur?"
>
> So far, we have considered exclusively the explanation of particular events occurring at a certain time and place. *But the question "Why?" may be raised also in regard to general laws.* Thus, in our last illustration, the question might be asked: Why does the propagation of light conform to the law of refraction?...Thus, the explanation of a general regularity consist in subsuming it under another, more comprehensive regularity, under a more general law.[190]

Theoretically, then, the chain of explanation in terms of generalized laws normally reaches far back to some final cause, the primitive source of any particular event under investigation.

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[190] Hempel and Oppenheim, "Studies in the Logic of Explanation," p. 136. Italics added.

The explanation of the oar's apparent distortion under water depends upon, or reverts to the law that describes how light conforms to the general laws of refraction, a higher-level explanation that merely accepts this fundamental property of light. General laws, then, continue to be subsumed under higher-level generalities until we reach the theoretical end of the chain of causality.

Of course, the possibility of actually reaching such a primitive point, and no less of knowing when we have arrived at it, is intensely problematic, as Hempel and Oppenheim address in their article. This philosophical conundrum is the crux of Babbitt's warning considering general laws applied to musical explanation. As he sees it, the prospect of finding such primitives for music is an immensely daunting task, perhaps just as impossible as it is necessary for true explanation. Read in this light,[191] the next step in the Webern analysis expresses exactly this concern:

> What has made musical explanation unsatisfactory is not the vagueness of the explanation, but the vagueness of the notion of the nature of a *musical* explanation. The formal theorem does patly satisfy the formal expression of the explanatory schematum, but it must now finally be said that—as in corresponding explanations in simple cases in the physical sciences—what the theorem explains is why the two different set forms possess a system of common representatives under the partitions in question (as an instance of a more general set of conditions subsuming this one), and our other attempts at explanation were simply not explanations in this sense, *for whether we can expect to provide satisfactory musical explanations depends upon whether we can produce, or discover, "general laws" of a different theoretical category, at a different theoretical level*, which poses questions closely and complexly related to those moot and tantalizing ones which arise in association with what is now so modestly termed "the derivation of 'ought' from 'is'".[192]

Here, Babbitt is highlighting the ultimate necessity of finding truly primitive principles for explaining music: general laws of "a different theoretical category" and at "a different

[191] And we may safely undertake such a reading: in the paragraph preceding the quotation, Babbitt directly refers to the logical form of scientific explanation that Hempel and Oppenheim outline.
[192] Babbitt, "Contemporary Music Composition," p. 280. Italics added.

theoretical level" than the mathematical formula. In Hempel and Oppenheim's framework, Babbitt's formula can be subsumed by other, more general statements until we reach the true primitives of a musical system, those that provide a completely satisfactory explanation.

This significance of Babbitt's comments here requires further elaboration. Babbitt merely introduces the need for finding such general laws of a different, i.e. higher, theoretical category. Also, his language is a somewhat vague (especially with his invocation of the "moot and tantalizing [questions] which arise in association with what is now so modestly termed 'the derivation of "ought" from "is"')[193] and he does not offer any clearer guidance within the scope of this article. We remember from our work with his other essays, however, that the possibility of finding musical primitives is a central preoccupation of Babbitt's methodological thinking. Furthermore, Babbitt demonstrates a deep scepticism concerning the possibility of ever finding those primitives. In "The Structure and Function of Musical Theory," Babbitt unambiguously affirms that the choice of system-defining primitives is never easy to make:

> Putting aside formalities...any theory is a choice from an infinite number of possible theories, and the choice is determined by what can be termed a criterion of significance in the selection, first, of primitives, whatever the linguistic form of these primitives. Whether this significance be expressed in terms of predictive power, explanatory scope, simplicity, or some other criterion, the decision is not easily made or ever surely made.[194]

In the rest of the essay, as we remember, Babbitt criticizes, and finally rejects, musical theories that have chosen systematic primitives: he attacks musical theories founded in the

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[193] Concerning this phrase, the editors of this collection of essays have only this to say: "A distinction first drawn by Hume (in *Treatise of Human Nature*), and still a matter of some controversy in ethical theory." *The Collected Essays of Milton Babbitt*, p. 306, note 17.

[194] Babbitt, "The Structure and Function of Musical Theory," p. 194.

concepts of pitch, interval, and the overtone series. His goal is to illustrate how difficult the prospect of choosing explanatory primitives really is.

As a familiar example, Babbitt considers how one might form a general law prohibiting the motion of parallel fifths in eighteenth and nineteenth-century tonal counterpoint:

> But from this body of works [i.e. eighteenth and nineteenth-century tonal music] one probably would formulate, for example, a law regarding the "prohibition" of motion in parallel fifths... The formulation surely would not take the form of: "Parallel intervals of the unison, octave, and perfect fifth have been systematically avoided by composers of the eighteenth and nineteenth centuries, whenever it has been their intention to write basic four-part texture," for all that this is the most popular of formulations. Nor would it or could it be explained by some statement such as: "Fifths are too closely related." What does it mean to be too closely related? To be fifths. It is difficult to see how the law can be derived from other, nontautologically related, more general laws of "tonality"...[195]

This passage reveals Babbitt's inherent scepticism: how can we possibly find a general law of Tonality *per se* that prohibits motion in parallel fifths? Babbitt warns against tautologies that lead us erroneously into believing we have found a primitive explanatory law. The idea of tautology—or a tautological argument—relates quite clearly to Babbitt's "moot and tantalizing" questions concerning "the derivation of 'ought' from 'is.'"

In "Past and Present Concepts of the Nature and Limits of Music," we recall that Babbitt identifies the inadequacy of much twentieth-century theory with a failure to choose and define proper primitives, "the occupational malady of theorists of music."[196] He specifically questions the ability of mathematics and uninterpreted mathematical formulae to form explanatory primitive for musical theory:

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[195] Babbitt, "The Structure and Function of Musical Theory," p. 198-199.
[196] Babbitt, "Past and Present Concepts," p. 80.

> What, then, determines the choice of the mathematical expression? Usually, the formal theory is chosen or constructed in the light of an intended interpretation; what properties are possessed by an arithmetical progression, for example, which make it appropriate for interpretation as a metrical or ordinal pitch, or durational, or dynamic, or timbral, or density determinant? What data in the field of musical perception determine the rules of correlation? And what of the possible overpredictability and the assumed separability of the components of even the atomic musical event? All of these questions and others must be probed thoroughly before a decision can be even tentatively reached as to the extent to which such procedures enlarge the domain of music.[197]

Babbitt is taking the assumption of the relationship between mathematical and musical representation to task at a fundamental level. In order to legitimately hold mathematics as an explanatory primitive for music, we must answer the difficult, perhaps even unanswerable questions Babbitt poses in the passage above: what laws assure and define the relationship between music and mathematics?

Reading the Webern analysis in conjunction with these other essays confirms Babbitt's deep reluctance to make definitive choices concerning primitives in an explanatory system for music. He simply does not know how we can arrive at them: music is too problematic. Babbitt never offers finally an explanation for the transformational choice in the Webern Concerto: he is content within the scope of his article to pose the problem without answering it. In this same essay, Babbitt remarks discouragingly that even the most simple analytical questions about music are fraught with deep methodological problems:

> ...I am far more concerned to demonstrate that, while beginning with the simple musical instance, and while remaining deliberately and even selfishly insistent upon musical matters, one finds oneself proceeding directly from the most habitual and seemingly innocent of musical inquiries to unavoidable confrontation by intricate and puzzling methodological issues.[198]

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[197] Babbitt, "Past and Present Concepts," p. 81-82.
[198] Babbitt, "Contemporary Music Composition," p. 277.

Realizing that the comments about the Webern analysis ultimately involve larger questions concerning the search for musical primitives, Babbitt's next point falls into place beautifully. He tips his hand completely by invoking a distinction between *object language* and *metalanguage*. With these terms, he warns that how we talk about music, the metalanguage, may not always correspond exactly to the reality of the music itself, the object language. We must mind the discursive gap between the two referential systems:

> The formal-interpreted theory issue leads immediately to that of the distinction between metalanguage and object language, which arises as soon as a composition (object language) is referred to by a natural or formalized language (metalanguage), or when such a collection of references are themselves discussed in a meta-metalanguage. If this distinction between object language and metalanguage is maintained most vividly by the very difference between the act of composing and the act of talking about composing, the most vulgar and—therefore—the most frequently encountered violation of this distinction is that in which a musical composition and—by faulty extension—a body of music are labelled "mathematical" because an expression containing mathematical terms (such as our previous theorem), under suitable interpretation, accurately characterizes come aspect of the musical composition.[199]

As soon as we begin to talk about music, then, we run the risk of missing the music altogether: we must not fall into the trap of confusing the music with the formal theory. Babbitt gives his own apt, albeit intentionally extreme illustration of the consequences of such confusion:

> This is precisely equivalent to labeling a musical work "mathematical" because it employs two violins, or is 369 measures long, or to describing German word order as being in "English" because it is accurately describable in English or—most absurdly and relevantly—to labeling someone a mathematician because he can be accurately characterized as being twenty-five years old or as having two children.[200]

Putting the framework together, then, Babbitt warns us to approach general laws with great caution since they ultimately require us, at some time or another, to look for the

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[199] Babbitt, "Contemporary Music Composition," p. 281-282.
[200] Ibid. p. 282.

primitives in the explanatory system. As soon as we begin to move backwards in the causal chain, however, we move deeper and deeper into the metalanguage, and consequently, farther and farther away from the music we are talking about. The risk of confusing object and meta-languages is surely heightened as we approach explanatory primitives far removed from local musical design. This is the source of Babbitt's particularist problem: even if we could settle upon musical primitives, the process of taking theory and analysis back that far leads down a dangerous path toward misunderstanding and misrepresenting the music we set out to explain in the first place.

5. Solving the Particularist Problem

Babbitt's particularism is a result of his scepticism concerning the discovery of explanatory musical primitives. It also emerges clearly in his personal solution to this methodological problem. In order to circumvent the methodological conundrum of finding universal explanatory concepts, Babbitt locates musical primitives (on an analytical level) within individual pieces themselves. In other words, particular pieces of music define their own explanatory scheme. As we recall, this process is exactly the one Brown and Dempster describe as *particularism*. To demonstrate the way in which Babbitt operates, I will discuss his analysis of Schoenberg's *Fourth Quartet*, found in "Set Structure as a Compositional Determinant."[201] As the title of this essay suggests, Babbitt demonstrates how certain properties of the work's twelve-tone row, taken as a primitive concept for the compositional design, explain Schoenberg's creation of form through the ordered presentation of set

[201] Babbitt, Milton. "Set Structure as a Compositional Determinant," in *The Collected Essays of Milton Babbitt*, p. 86-108.

transformations. The set structure itself can be said to cause, or determine the compositional choices that Schoenberg makes.[202]

Before delving into this analysis, however, we notice that Babbitt's particularism surfaced already in the analyses of the Bach chorales in chapter one. With Babbitt's assessment of Salzer and Schachter's analysis of *Ach Gott, wie manches Herzelied*, we established at the time that the explanatory value of analytical models must be evaluated from within their own system of operations, according to their own frameworks. We cannot compare the explanatory value of analytical systems that do not work with the same explanatory principles. Babbitt's answer to Salzer and Schachter's fumbling treatment of Bach's awkward voice leading consists in pointing out a motivic parallelism within the work itself that Schenkerian analysis does not, and cannot be expected to reveal ("that's not his [Schenker's] schtick.")

In the context of that discussion, we notice that Babbitt's answer is entirely particular in its formulation: the recognition of the motivic parallelism works for this piece alone.[203] We cannot expect such a motivic parallelism between the first and ninth measures to explain other voice-leading situations in Bach chorale harmonizations. In Babbitt's mind, this parallelism becomes a defining feature of the chorale, as is latent in his accusation that Salzer and Schachter "miss the boat terribly." Marion Guck notices this same point, and confirms Babbitt's particularism in general:

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[202] A certain *caveat* is necessary here. In the course of discussing this article, I do not address Babbitt's lengthy exposition of the inherent properties of Schoenberg's set. The point to be made is entirely analytical, and to make it efficiently within the scope of this chapter, I must assume the set properties that Babbitt reveals.
[203] Refer back to chapter 1: *Ach Gott, wie manches Herzelied*, fig. 2.

> More recently, in *Words About Music*, his consistent concern for "purely contextual contingencies and dependencies" has been persuasively demonstrated in analyses that trace networks of subtle parallelisms between events that contribute to the individual character of a work. It should not be surprising that an individual so acutely aware that the subtle details of musical events create the contextual identity which *is* the musical work abhors language that serves only as a blanket and a sedative—the musically soporific language of pat explanations and thoughtless labels.[204]
>
> The analyses in Babbitt's papers exemplify a further point, for each is intended to show what is unique to the musical work examined. As I understand his view, uniqueness results from the profound contextuality of each work: a work gains its identity through the "meanings" created from its particular web of internal relations.[205]

Finally, let us look at Babbitt's particularism in action with his analysis of Schoenberg's *Fourth Quartet*. First of all, we must note that "Set Structure as a Compositional Determinant" is concerned primarily with exposing the properties of Schoenberg's set with regard to different possibilities of hexachordal combinatoriality, the "H-combinatorial property."[206] Even without examining all the interesting details of this property, we can notice the ways in which Babbitt applies them to his analysis. Indeed, we do not necessarily need to understand the ins-and-outs of hexachordal combinatoriality in order to see the significance of Babbitt's analytical application of it.

Consider this passage analyzing the presentation of the original set form and its first transformation at the very beginning of the quartet:

> ...The opening—completely linear—statement of the S [set] delineates the importance of its hexachordal structure by articulating, through a rest, this set

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[204] Guck, "Rehabilitating the Incorrigible," p. 61.
[205] Guck, "Rigors of Subjectivity," p. 54.
[206] The "partitioning of an aggregate into two parts of six elements each." Expressed in its prime form: "$H_0+T_tH_0=A$." (H=hexachord; T=transposition by some operator number; A=aggregate) See: Babbitt, "Set Structure as a Compositional Determinant."

> division...The end of the second H [hexachord], and of the S [set], is articulated by a change of instrumental texture from a four-instrument unison to a single instrument statement, by a change of register from that available to all four instruments to that uniquely available to the cello, and by an extreme dynamic change...[207]
>
> (The score is given in fig. 2a)

Another analysis of a later moment in the quartet continues in this way (The score is given in fig. 2b):

> Example 2 demonstrates Schoenberg's use of combinatorially determined criteria as determinants of external structure: "form," in the sense of the deployment of areas of thematic, textural, rhythmic, and comparably manifest similarities. Example 2 is a return, the only return, to the unison statement, the tempo, the rhythmic structure (with one tiny exception), and the metric of the movement's opening. The set form is T_5I, the one form of H-combinatorially related to S omitted, as observed above, from the opening succession of set statements. This "functional transposition" of S assures that (F, Gb) now occupies the position held by (C, B) in S, and (B, C) those of (Gb, F). Now, repeated notes of the set statement in this "isorhythmic" repetition are (B, C), and those of the following RI-related form (R) are (Gb, F). The terminal and initial notes of these so-related forms are the familiar (Ab, A), et cetera, et cetera. All of these features are the results of the principles discussed above. This successive statement of what, in simultaneous statement, would produce a note-against-note combinatorial presentation of two, pitch dyad—identical, successive aggregates is time-congruent form of the most definitive sort.[208]

These analytical comments are full of potential for interesting commentary. Most importantly, however, they show Babbitt's particularism quite clearly. In their remarkable clarity, these comments speak for themselves; nevertheless, I offer an interpretation of them below.

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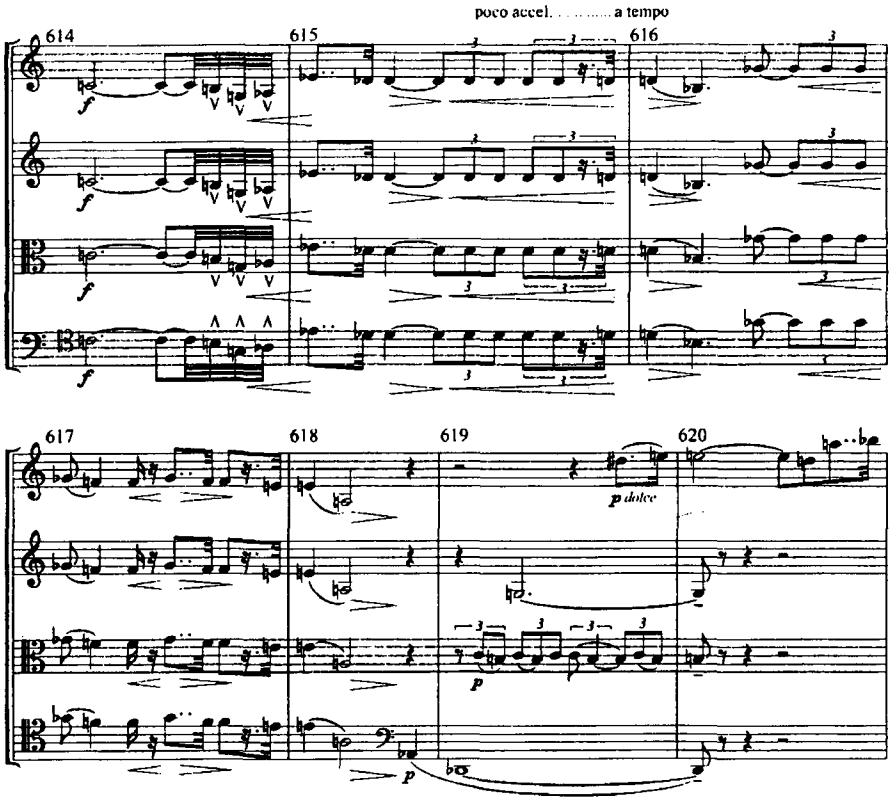
[207] Babbitt, "Set Structure as a Compositional Determinant," p. 97.
[208] Ibid. p. 104.

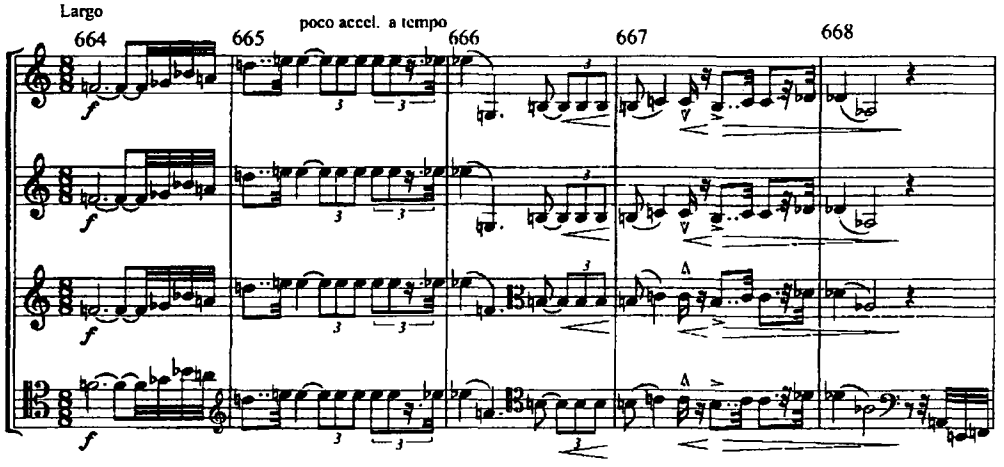
Figure 2: Schoenberg's *Fourth Quartet*

a: mm. 614-620



b: mm. 664-668

EXAMPLE 2.



Source:

Milton Babbitt, "Set Structure as a Compositional Determinant," in *The Collected Essays of Milton Babbitt*, pp. 87 (2a) 104 (2b)

In the first passage, Babbitt shows how the H-combinatorial property of Schoenberg's twelve-tone row explains a seemingly minor and unimportant compositional detail: the dotted sixteenth-rest in m. 615. This rest separates the two distinct hexachords of the twelve-tone aggregate. In other words, the H-combinatoriality of the set determines the significance of this compositional choice: we can explain the presence of this rest by invoking the H-combinatorial property that Schoenberg exploits in his set construction. Babbitt's analysis is explanatory only if we regard the original set form as a primitive concept for this music; otherwise, the analysis is genuinely *ad hoc*. Babbitt is very clearly trying to use the set structure "as a compositional determinant," in the fullest sense of the phrase: this is about as far from the *ad hoc* as we can go.

Babbitt also explains Schoenberg's choices of register and dynamics with reference to the original twelve-tone set. Some of these choices appear to be trivial. For example, Schoenberg could have articulated the end of the original after its first presentation by moving to a register "uniquely available" to the violin, instead of the cello. In other words, the choice of register may be underdetermined by the properties of the set.

The second analysis is more robust analytically and convincing. Here, Babbitt makes a grand statement about the essential role that H-combinatorial property of the twelve-tone set plays in determining large-scale form within the quartet ("combinatorially determined criteria as determinants of external structure"). We may summarize the point of Babbitt's analysis in this way: the H-combinatorial properties of Schoenberg's original set prescribe and limit the available options for coherent formal construction. For Babbitt, the

formal constraints of this work result directly from the H-combinatorial property. Of course any analytical statement about formal properties approaches the most fundamental aspects of music's internal design and coherence. In this way, the H-combinatorial twelve-tone set functions as a primitive concept for this piece. It is the general law that explains and determines Schoenberg's compositional choices from formal construction to the placement of individual rests. Every analysis Babbitt makes in this piece draws upon the property of H-combinatoriality for it ultimate significance.

6. Babbitt and Brown: the Final Assessment

This has been only a brief glance at Babbitt's particularism as manifest in his analytical practice. One may object that the analysis presented above in not an instance of particularism at all. After all, Babbitt is dealing with aspects of set structure that are presumably universal, acting as general laws that transcend their individual manifestations in particular works of music. H-combinatoriality is not a property unique to the twelve-tone set of Schoenberg's Fourth Quartet. This pertinent and well-placed objection implies the results of our final assessment of the differences between Babbitt and Brown.

As I suggested at the beginning of this section, this difference rests upon a fine shade of distinction and may not be too divisive. Babbitt and Brown both work with general laws and explanatory primitives in their own respective ways. The difference lies herein: whereas Brown prefers to locate musical primitives outside of individual works, Babbitt places them *within* individual works. I have already outlined the reasons for this choice. In some sense, Babbitt's approach is not too far of from Brown and Dempster's strict scientific method. Again, we recall Babbitt's ambivalence with regard to general explanatory laws: he

desperately wants to use them (and the analysis above demonstrate this use), but he cannot resolve his inherent scepticism and fears of analytical misrepresentation. Babbitt never wants to go too far from the musical object language itself: he is afraid that the formulation of general laws ultimately entails that a theorist or analyst search for the final cause in the chain. The methodological distinction between Brown and Babbitt, then, is not sharply defined: it oscillates between convergence and disparity as a result of Babbitt's own unwillingness to make a firm commitment one way or the other.

The crux of the problem lies in another issue to which I alluded in the previous chapter.[209] In their criticism of particularism, Brown and Dempster discuss those analysts who invoke to general laws of a certain while retaining the notion that individual pieces of music determine their own best method of investigation. Here, Brown and Dempster propose that this approach, while not strictly scientific in intention, may not be incompatible with scientific method:

> 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, and 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 as approach may in fact preserve many particularist goals while deflecting the charge of inarticulate open-mindedness. Furthermore, though its aims are not scientific, this approach may not be incompatible with a scientific view of music theory.[210]

I believe that Babbitt's analyses fall into this category. As in the analysis of the Schoenberg Quartet, he retains analytical concepts unique to the piece (i.e. the articulation of the formal

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[209] Refer to the discussion of particularism and intersubjectivity in the previous chapter.
[210] Brown and Dempster, "The Scientific Image of Music Theory," p. 94-95

structure through the set H-combinatorial property) while at the same time invoking generalizable properties of the original set structure. Babbitt cannot make the final move to strict scientific method, but he is close to it. He stops perhaps halfway down the "slippery slope" that Brown and Dempster invoke in analogy:

> 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 applicability of the methods and concepts—then he or she had 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.[211]

[211] Brown and Dempster, "The Scientific Image of Music Theory," p. 95

Conclusion

This thesis examines the application of scientific method to music theory and analysis in the writings of Milton Babbitt, Matthew Brown and Douglas Dempster. The statement it makes, then, is a methodological one. Using Milton Babbitt's terminology as a guiding framework, I present a line of thought that promotes the use of scientific method for achieving *sufficient* explanation within music theory and analysis. These activities (following Babbitt again) are assumed to be *explanatory* pursuits, as opposed to merely *descriptive.*

As such, this thesis gives one possible answer among many for the important vocational and methodological questions that remain unresolved within the professional practice of music theory:

> What makes a good analysis of a musical composition? What makes a music theory or analytical method appropriate for some pieces, but not for others? Although these questions are surely fundamental to the study of music theory, there is little consensus about what answers theorists can or should give.[212]

For Brown and Dempster, scientific method answers these questions by ascribing systematic values to music theory and analysis: "empiric adequacy and predictive power."[213] Naturally, defining such values is the most important step in establishing possible criteria that make theories and analyses sufficient explanations. This is to say, sufficiency will always be defined by those values given the greatest significance within a theoretical framework.

To summarize, the first half of the thesis (chapters 1-2) develops in detail Milton Babbitt's prescriptions for a scientific music theory and analysis, and it outlines the logical

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[212] Brown and Dempster, "Evaluating Musical Analyses and Theories," p. 247.
[213] Ibid.

format of scientific explanation to which Babbitt refers. Babbitt incorporates scientific method into music theory by imposing both terminological and methodological requirements—"scientific language" and "scientific method." Furthermore, Babbitt understands scientific method as consisting of two fundamental elements: empiricism (*a posteriori* knowledge as opposed to *a priori* knowledge), and the construction of general laws in a hierarchical system of explanation. Babbitt calls the laws at the top of this hierarchy "primitive concepts."

A discussion of Hempel and Oppenheim's D-N model of scientific explanation completes our understanding of Babbitt's framework. Privileging empiricism and the explanatory value of universal laws, the D-N model corresponds with and confirms Babbitt's presentation of scientific method. This section also outlines the definitions that the philosophy of science has given to *sufficient* and *necessary conditions*.

The second half of the thesis (chapters 3-4) picks up the line of thought with Matthew Brown and Douglas Dempster who address the issue of music and science after a period of virtual silence within the scholarly literature. Brown and Dempster's intentions are primarily defensive. They propose that the scientific image of music theory has been unduly neglected and deserves reconsideration. Most importantly for them, the logical form of scientific explanation provides the necessary external criteria of verification that are the surest means of establishing true intersubjectivity. Taking the most recent developments in the philosophy of science into account, Brown and Dempster also revise the strict D-N model to incorporate statistical generalizations in place of universal laws. In this way, they

make scientific music theory more viable in everyday practice. Finally, I complete Brown and Dempster's framework with a brief discussion of their justification for subjecting music—an aesthetic object—to scientific inquiry. They maintain that any artwork can be properly understood as an inextricable union of the aesthetic and the empirical.

Finally, this thesis offers a comparison of Milton Babbitt with Brown and Dempster. Prompted by Brown and Dempster's curiously underdeveloped charge of *particularism*, I revisit Babbitt's framework with a more critical eye. Babbitt cautions us in our analytical activity by setting up a distinction between the *object language* (the musical work) and the *metalanguage* (a general music theory). With this context in mind, we realize that Babbitt prefers to locate explanatory primitives within individual pieces of music, as opposed to generalizing explanation across classes of musical phenomena, as Brown and Dempster do. Babbitt's framework for music theory and analysis, therefore, is not scientific in Brown and Dempster's strict sense, but it is not far off. Babbitt still endorses the explanatory power of laws; however, his laws do not transcend the boundaries of individual compositions.

The title of this thesis bears a direct quotation from Milton Babbitt. At the end of *The Structure and Function of Musical Theory*, Babbitt asserts that the verbal and methodological concerns he raises "must be central to the instruction of the student of music theory...if he is to attain that rarest of all states: that of the concerned and thoughtful musical citizen."[214] I wish to echo Babbitt's sentiments. Those of us who love music enough to analyze it should take a moment, every now and then, to reflect deeply and critically on our practice. No matter how endless—or even fruitless—the search for answers

[214] Babbitt, "The Structure and Function of Musical Theory," p. 200.

to the methodological problems of our field may seem, we must endeavour to resolve them, if only for ourselves.

Ultimately, this thesis raises and considers deep methodological questions about music theory and analysis: what are we doing when we analyze music? What do we say we know about music through theory and analysis? How do we justify to the scholarly community the knowledge we claim to have about music? What is the proper function and purpose of music theory and analysis? What makes an adequate or appropriate analysis of a musical composition?[215] As in any honest pursuit, those who practice music theory and analysis must, every now and then, take a step back from their daily activity to reflect upon the foundations of the discipline. This difficult—yet necessary—and macroscopic perspective is surely central to the continued integrity and development of music theory and analysis however we conceive of them. Indeed, the love of music demands such meta-theoretical thinking from those who would otherwise indulge in its pleasures without the burden of intellectual responsibility. Hopefully, this thesis has represented an attempt at least to ask these difficult methodological questions with due concern and thought.

[215] As we have seen, Babbitt, Brown and Dempster explicitly pose questions such as these in order to support their unpopular advocacy of scientific method for music theory and analysis.

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