Music theory has long aligned itself with the sciences – particularly with physics, mathematics, and experimental psychology – seeking to cloak itself in the mantle of their epistemological legitimacy. This affinity, which was foreshadowed in music’s inclusion in the medieval quadrivium alongside geometry, astronomy, and arithmetic, is evident throughout the history of music theory from the scientific revolution onward. Yet, as eager as music theorists have been to claim the epistemological privilege accorded the sciences, they have also been wary of limitations attending such an alliance.

This collection of essays will provide a historical as well as a philosophical and aesthetic context regarding music theory’s relationship to science. The thirteen authors work in a range of fields and traditions including music theory, musicology, and philosophy.
What Kind of Theory Is Music Theory?
What Kind of Theory Is Music Theory?

Epistemological Exercises in Music Theory and Analysis

Edited by
Per F Broman and Nora A Engebretsen
To the Memory of Bo Alphonce (1931–2000), our dear friend and colleague who was involved in the initial stages of this project.
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The subtitle of this collection, “epistemological exercises in music theory and music analysis,” is at once precise and ambiguous. It is precise if it is understood in a wide sense, ambiguous if one interprets it in a more restricted way.

In the context of philosophy, “epistemological” would be the adjective of “epistemology,” and the common problem would be the actual conditions of knowledge in music theory and music analysis. The discussions would be about the character of knowledge, the sources of knowledge, the conditions for separating truth from falsehood in music theory and music analysis, and the like.

In the context of intellectual history (history of ideas, history of science), “epistemological” would probably rather be related to something like the Foucauldian term “episteme.” The problem would be to pose music theory and music analysis in historical contexts mainly concerning, or consisting of, the discourses of different sectors of science.

In fact, the anthology comprises texts of both these characters. But that doesn’t give the whole picture.

As in other “aesthetic disciplines,” one can’t ignore the ways criteria other than those based in truth or rationality have developed for the evaluation and legitimization of music theories—ethical and aesthetic criteria. The interplay between claims of truth, claims of goodness, and claims of beauty tends to be delicate, to say the least: To discuss epistemology or epistemes in relation to music theory and music analysis can be rather different from discussing the epistemology of mathematics or science, the relation to epistemes of natural history or political economy. Several contributions address these issues.

A further complicating factor is that music might be considered a form of thought, and aesthetic experience of music a form of acquiring a specific kind of knowledge. The former is in line with modes of expression of Hanslick and Boretz, the latter with the ideas of Baumgarten and Adorno, among others: Music theory might be a theory about an object with epistemological problems of its own. The field is open to ambiguities. Though none of the anthology’s authors defends views like these, they are reflected in the discussions, and in the concluding contribution the relation between mental representations of music and conceptual structures concerning these is considered.
Finally, the implications and relevance of the actual identity of the theor-etcian or the analyst for the theoretical or analytical work is on the agenda in every typical historical account, but the identity has tended to be reduced to an intellectual context, where possible conflicts are between different world conceptions. But identities might also be about class or gender, and these might be considered both in an objective and a subjective sense. In at least one of the articles, the somewhat “pure” atmosphere of theoretical discussion, seemingly lacking the involvement of the author as a social subject, is transgressed—on this occasion in terms of gender.

In short: In subtitling this collection of articles “epistemological exercises in music theory and music analysis,” the editors Per F. Broman and Nora Engebretsen have implied that the term “epistemological” in this case must be understood in a wide sense, relating to a rich variety of questions concerning music theory and music analysis: Questions of the status of music theory and music analysis are posed in the terms of epistemology as traditionally understood, but also in a historical and critical fashion in terms of the relations of these disciplines to science, ideology, aesthetics, psychology, gender, rhetorics, etc.

Most of the texts are published for the first time, one is a revised version of an earlier article, and one is textually identical with its first edition. The authors work and live in the United States and Sweden. This reflects the backgrounds of the editors: Engebretsen is active in the United States, Bro-etcman is a native Swede, though he has worked in the US since many years. It also reflects the common ground and common scene of modern musicology, especially the relative lack of isolationism. We are a far way from the nationalistic tendencies that once were a trademark of Musicology (as of Humanities in general).

We have chosen to publish this book as the first one in a new series at Stockholm University, Stockholm Studies in Musicology, devoted to advanced studies in Musicology, to be presented in major languages like English, German, or French. The title is in line with the interest in Music Theory at the Department of Musicology and Performance Studies—in the European tradition, Music Theory is not an independent discipline at the university level in Sweden. We hope it will mark an era of intensified exchange between Anglosaxon and Swedish Music Theory.

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Introduction

Music theory has long aligned itself with the sciences—particularly with physics, mathematics, and experimental psychology—seeking to cloak itself in the mantle of their epistemological legitimacy. This affinity, which was foreshadowed in music’s inclusion in the medieval quadrivium alongside geometry, astronomy and arithmetic, is evident throughout the history of music theory from the scientific revolution onward, perhaps most strikingly in the highly formalistic strain of music theory that emerged in the United States just after the middle of the twentieth century, at a time when scientific positivism was very much in vogue.¹ The epistemological claims of this particular strain of music theory are encapsulated in Milton Babbitt’s 1961 assertion that “there is but one kind of language, one kind of method for the verbal formulation of ‘concept,’ whether in music theory or in anything else: ‘scientific’ language and ‘scientific’ method.”²

Yet, as eager as music theorists have been to claim the epistemological privilege accorded the sciences, they have also been wary of limitations attending such an alliance. While music theorists commonly measure their models against scientific standards of logical rigor and terminological precision, they have long struggled to reconcile their desire for systemic coherence and economy with the need to account for the empirical data at hand. Moreover, they have grappled with the notorious problem of reconciling the normative tendencies of scientific theory with the individuating goals of music analytic practice, and they have confronted questions about the nature of “truth” and “correctness” in music theory and analysis, mapping analytic

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statements onto a continuum defined at its extremes by notions of objective, verifiable truth uncovered through a scientific enterprise and ad hoc subjectivity. The uneasy state of music theory’s alliance with the sciences has led even the most committed positivists to temper their claims with appeals to pragmatism. Even Babbitt softened his stance, to a degree, in noting that “the relation between a formal theory and its empirical interpretation is not merely that of the relation of validity to truth (in some sense of verifiability), but of the whole area of the criteria of useful, useable, relevant, or significant characterization.”

Much as music theorists covet the apparent legitimacy of the sciences, the extent to which music theories are or should be theories in a scientific sense remains a subject of debate. The rise of Postmodern thought and challenges raised by the New Musicology certainly contributed, though in varying degrees, to a series of essays that appeared during the late 1980s and into the 1990s addressing these matters and related methodological questions. Matthew Brown’s and Douglas Dempster’s favorable comparison of music-theoretic models with scientific models met with considerable criticism, including John Rahn’s objection that analytically oriented music-theoretic systems lie within the aesthetic domain and, as such, should be judged on the basis of aesthetic, rather than scientific, criteria. Marion Guck has sought to bridge the divide between the objective/scientific and the subjective/aesthetic, both by drawing a distinction between scientific method and scientific language and by substituting the looser notion of intersubjective corroborations for the problematic demand of verifiable truth. More recently, Nicholas Cook has also mapped out a middle ground, suggesting that contemporary music theory be understood to embrace an empirical methodology regulated by multiple, shifting epistemologies—including both natural-law explanations and metaphorical, performatively, subjective constructs—which carry multiple notions of truth.


The essays in the present collection build upon recent scholarship and are grouped in three sections. The first section consists of five essays directly engaging questions about the relationship between music theory and the sciences. Per F. Broman’s essay “Music Theory: Art, Science, or What?” explores what, precisely, scientific theories might be said to entail, given that philosophers of science have not provided definitive answers to this question. Broman demonstrates that matters widely regarded as shortcomings of music theory vis-à-vis a “scientific” perspective—matters such as the questionable applicability of the notion of objective, verifiable truth—are in fact contested within the scientific community as well. Like Broman, Gregory Proctor and Elizabeth Sayrs depart from recent research in the philosophy of science, but come to a different, albeit interrelated, conclusion. They argue “that the current proclivity for ‘empirically testing’ a music theory by importing concepts, facts, and techniques from other fields cannot accomplish what it claims, because an empirical test of a theory is the evaluation by that theory of facts intended for that theory” and that the reason for the incorporation of scientific methods is political. Through discussions of recent music theories, the gap between the claims of scientifically oriented theories and the concerns of most theorists is analyzed. In “‘Initial Conditions’: Problems of Scope and Cause in Music-Analytic Claims,” Stephen Peles considers whether music-analytic claims constitute explanations, let alone scientific explanations. Peles questions music theorists’ reliance on Hempel’s deductive-nomological (D–N) model of scientific explanation, given that it no longer carries much weight among philosophers of science, and ultimately suggests both that the distinction between explanations and “scientific” explanations is one of degree of incompleteness, rather than one of kind, and that the distinction between description and explanation is not as clear-cut as is widely assumed. In “Simplicity, Truth and Beauty in Music Theory,” Nora Engebretsen presents accounts of the concept of simplicity from the natural sciences, and compares and contrasts the role simplicity plays in the building of music-analytical systems. It turns out that simplicity is a complex, problematic notion in music analysis, particularly with regard to explanatory adequacy versus the notion that that analytically oriented theoretical systems lie within the aesthetic domain and as such should be judged solely on the basis of aesthetic criteria. Finally, in “The Unity of a Musical Composition,” James Manns, like Engebretsen, approaches another widely used concept from a philosophical perspective, posing and exploring fundamental questions about the assumed aesthetic value of unity and about the link between human emotion and musical form. Manns’s invocation of Aristotelian rhetorical models suggests a link between his ideas and the epistemological framework presented by Elisabeth Kotzakidou Pace in section two of this volume, and his discussion of difficulties involved in describing emotional experience ties in with Marion A. Guck’s discussion of related matters in section three.
The four essays in the second section consider the nature of music theory from several different historically informed perspectives. In “A Neo-Aristotelian Paradigm for Music Theory: Music-as-Techne and the Epistemic Domain of Rhetoric,” Elisabeth Kotzakidou Pace departs from Aristotle’s inquiry into the epistemic domains of the various Arts (technai) to propose a conception of music-as-art (as opposed to music-as-science, music-as-mathematics, or music-as-language). Insofar as music theory, under this account, addresses the “artistic” employment of sound, Kotzakidou Pace argues that it is a discipline akin to rhetoric, and she explores the epistemological status of musical rhetoric as an alternative to musical positivism—one that admits the results and systematization of scientific or philosophical inquiry without appealing to “extraneous” epistemological domains. In “Countless Western Art Music Recordings: Towards a Theory of What to Do With Them,” Jonathan Dunsby presents a method supporting a credible study of historical recordings. He argues, “If you are analyzing the recording of the music, rather than the music itself in some more virtual form, then presumably you are paying constant attention, by definition, to the music in a context.” Departing from a number of examples he concludes that such analyses must adhere to certain criteria: the recordings have to have some kind of canonicity so that appropriate comparisons can be made; that instances of performance are more important than the general practices; and that there has to be some kind of intersubjective truth in the evidence. He concludes that music theory “needs to develop a different view of itself in the world of technology.” In “When the Treatises are Silent: The Search for a Historically Consistent Theory,” Ruth Tatlow asks questions about the mathematical nature of Johann Sebastian Bach’s works departing from the historical treatises: How should Mattheson’s call for precompositional organization be understood in the absence of detailed directions? Were there clearly defined numerical methods underlying these guidelines? Did Bach use proportions to organize his works, and if so, would it be possible to find them in his scores? She then discusses the limitations of conventional analytical methods and the need for an interaction between theoretical and historical work. In “Mathematics and Ideology in Modernist Music Theory,” Jacob Derkert makes a critical assessment of the role of mathematics in the modernist music theory of Ernst Krenek and Milton Babbitt. As a background he sketches a major shift in the way mathematics is and has been used in music theory, contrasting Pythagorean and acoustic theory on the one hand, and “modern” music theories on the other. He shows that the growing emphasis on phenomenal issues in music theory of the seventeenth century correlates with the development of a new object for mathematization in music theory. This mathematization implies new ways to ideologize music theory, including the use of mathematical theories of structure, rather than traditional aesthetic ideals, as means for realizing a modern approach.
The final section comprises three essays, each of which looks beyond the bounds of “scientific” practice to address aspects of the roles language and metaphor play or might play in music theory and analysis. In the first essay in this section, “Reason and Reality: The Concept and Matter of Music Theory,” Sten Dahlstedt explores the impact that changing understandings of the nature of scientific inquiry had on music theories of the nineteenth and twentieth centuries. Dahlstedt concludes, in particular, that as a strict empirical view of science came to dominate American music theory from the mid-twentieth century onward, scholars increasingly ascribed objective meaning to music-theoretical terminology and accordingly identified the musical work as a real object, as a real entity in an ontological sense. The problem of music theory, he suggests, is thus both one of a theory of knowledge and a philosophy of language. Marion A. Guck’s “A Woman’s (Theoretical) Work” takes as its focus the problem of accounting for the nature of experienced qualities of time. Guck observes that the means required to address this issue fall outside the bounds of standard music theoretical practice—in their privileging of the particular and contrastive over the general and generalizable, for instance—and proposes methodological approaches incorporating figurative descriptions into hybrid analytical accounts blending varying technical focuses. Finally, Mark DeBellis’s piece, “Conceptual and Nonconceptual Modes of Music Perception,” examines the relationship between music theory and language within the context of a broader examination of ways in which the philosophical study of perception, language, concepts, and intentionality might refine and enrich music theorists’ conceptions of hearing and of the ways in which music theories both model and inform our the way we hear.

Per F. Broman      Nora A. Engebretsen
Music Theory
Art, Science, or What?

Per F. Broman

… if you want to understand what a science is, you should look in the first instance not at its theories or its findings, and certainly not at what its apologists say about it; you should look at what the practitioners of it do.

Clifford Geertz (1973, 5)

We are like sailors who have to rebuild their ship on the open sea, without ever being able to dismantle it in dry dock and reconstruct it from the best components.

Otto Neurath

Given that music is an art, it seems unlikely that music theory and music analysis would have anything to do with science and scientific method. To analyze a piece of music, the analyst must have an understanding of the aesthetic qualities of the work or the repertoire. The music-analytical world is artistic in its foundation and, in a way, the analyst has to become an artist. Through this view, even if an analyst utilizes mathematical methods the outcome is bound to be unscientific. However, there are similarities between music theory and other sciences in terms of method; many philosophers of science are actually indirectly describing the working process of a music analyst. In this essay I will discuss science—the natural sciences, in particular—and the philosophy of science and make connections with music theory.

This paper was read in earlier versions at the music-theory colloquium at Indiana University, Bloomington, October 15, 2003, and the graduate colloquium at McGill University, November 4, 2005. I am grateful for the comments I received during these occasions. I am also most grateful to Nora Engebretsen for comments and suggestions on this paper and to my former Butler University philosophy colleague Stuart Glennan for providing me with some challenging comments and sources of references.

Quoted in David Edmonds and John Eidinow 2001, 163. Neurath’s boat was the departure for a thought-provoking piece on the scientific nature of music theory and its relationship to New Musicology by Matthew Brown (1996).
The natural sciences relate to relatively recent discussions within the music-theoretical community regarding the status of the discipline. Although music theory might appear unscientific occasionally, so do the natural sciences. In many instances, Science is not an elevated, unreachable entity in philosophical terms. It turns out that for many philosophers of science, the notion of scientific theory comes close to the daily-speech use of the term “theory” as in, “I have a theory,” meaning “I have a hypothesis” or “I’m guessing that...” Far earlier than Thomas Kuhn, philosophers have been discussing the sociological aspects of science and its sometimes-surprising lack of logic. I will show that music theory not only resembles scientific theory with regard to methodology, but also that the kinds of problems and methodological hazards philosophers of science have encountered in the natural sciences relate to music theory. I will also discuss the concept of the “Model” to illustrate how theories of music can be used to illustrate real music and to enhance our understanding of it. Schenkerian theory will be used in the discussion quite a bit. The reason is simple: it is a well-known and well-developed system that fits the scientific discussion on many levels.

I will depart from the work of philosopher of science Karl Popper, whose work is concerned with the fundamental working process of a scientist, and continue with Stephen Jay Gould and Richard C. Lewontin, Paul Feyerabend, and Margaret Morrison.

* * *

I disagree with Hume’s opinion (the opinion incidentally of almost all philosophers) that induction is a fact and in any case needed. I hold that neither animals nor men use any procedure like induction, or any argument based on the repetition of instances. The belief that we use induction is simply a mistake. It is a kind of optical illusion.

Karl Popper

Karl Popper (1902–1994) became famous as an adamant opponent of inductive reasoning in science, meaning the method of drawing conclusions from a limited number of observations. He departed from David Hume’s famous anti-inductionist example: It is impossible to prove that all swans are white using induction, since there is always a possibility that we might find one

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2 Thomas Kuhn was the author of the influential volume *The Structure of Scientific Revolutions* (1970). According to Kuhn, scientific knowledge is achieved through revolutions in the scientific community—paradigm shifts—during which one idea takes over from what has previously dominated the field.

3 Popper 1985, 103–04, originally published as “The Problem of Induction.”
black swan that then would demolish our assumption. It does not matter how many white swans we find—even if we find all living swans—there is always a prospect that one black swan might appear. Philosopher Bryan Magee, one of Popper’s strongest and most initiated supporters, provides another example that illustrates this problem. We believe that water boils at 100° Celsius. It is no problem finding innumerable of instances where this is the case. Time after time we can repeat this experiment, but our suspicion has not been proven. It is not until we find an instance where water boils at another temperature, when we falsify our initial statement, that we are getting somewhere. Then we can begin to speculate under which circumstances water boils at 100°C (Magee 1985, 21–2). Science presuppose that nature is regular and that predictability regarding the future will follow; that is the danger with the experiment above. For Popper, however, this kind of regularity cannot be taken for granted (Magee 1985, 16).

Instead a scientist has to be guided by fundamental questions. Popper once told the story about a guest lecture he gave to physics students in Vienna. He began by giving the following instructions: “Take pencil and paper; carefully observe, and write down what you have observed!” (1963, 46). On one level, this is an innocent but very effective example of why induction does not work. The students were baffled, of course. We cannot just “observe”—we must have a focus, questions, and theories before we start observing something. This anecdote puts the finger exactly on the induction problem, and illustrates how Popper claims that inductive conclusions are impossible. Before fact finding, there has to be a theory. Popper is not concerned with the origin of this theory. Experiments and empirical observations are important parts of Popper’s epistemology, but only as tests of a hypothesis or a theory. For Popper, experiments and empirical observations are only tools to test the theory—and to try to falsify it. The initial theory, or the question, or the problem, is the spark that ignites the scientific pursuit.

Popper came up with a scheme that explains how he saw the difference between traditional methodology and his own (Magee 1985, 55).

Traditional method (Induction)
1. Observation and experiment
2. Inductive generalization
3. Hypothesis
4. Attempted verification of hypothesis
5. Proof or disproof
6. Knowledge
Popper (Deduction)

1. Problem
2. Proposed solution (a new theory)
3. Deduction of testable propositions from the new theory
4. Tests (attempted refutations), observation and experiment
5. Preference established between competing theories

Deduction reverses the process of induction, the process of reasoning, of building theories through the use of logic. In Popper’s view, deduction is the only scientific method of reasoning. One may ask, from where does the research problem, that is, the topic to be investigated come? One quick answer is from point 5 of the circular and limitless Popperian scientific process. It is true that theories have been preceded by observations. But these observations require a framework to create any scientific knowledge. This is not a chicken or egg exercise; rather, these are two fundamentally, philosophically different approaches for conceptualizing the scientific process. There is another reason for Popper’s insistence on this conceptualization: A researcher should deal with what he or she believes to be actual problems. Popper was persistently critical of Wittgenstein who claimed that there are no philosophical problems, just linguistic puzzles.

From Popper’s discussion it follows that scientific laws are not provable. They are testable and they are falsifiable, a key concept for Popper. Falsifiability constitutes the very demarcation of science versus non-science. This is not a value judgment; Popper does not believe that philosophers should abstain from discussing value or meaning. Also non-scientific ideas or theories have proven to be significant to human life. For Popper, it is not a problem to present a theory that at some later point is falsified. “Water always boils at 100°C!” is a false statement, although definitely useful in daily life at sea level on earth, but it becomes even more powerful with the modification that water boils at 100°C at Normal Pressure and Temperature. The initial theory was indeed functional as an important step on the way to a fuller comprehension.

There were two fashionable theories that Popper wanted to discard as non-scientific: psychoanalysis and Marxism. The problem with both is that they are impossible to falsify. To take an easy example, any state of mind can be read by a Freudian analyst as either fitting the theory or a repression of the state of mind that likewise fits the theory. For a Marxist, the Soviet Union’s collapse does not falsify the Marxist idea of an inevitable development of history resulting in the state of proletariat as an end result. For a believing Marxist, the Soviet Union was not the correct end of capitalism and Marx will be proven right at some point in the future. Popper warned about theories that were so all-encompassing that they could explain virtually everything. With such theories, the world seems full of verifications.
One reason why Popper rejects empiricist epistemology, and insists that all observation must be founded on theory, is that our sensory organs are not completely accurate and that theories could be counterintuitive (Magee 1985, 57). Just take Newton’s physics versus Einstein’s—a favorite example for Popper and so many other philosophers of science. Newton’s linear laws of attraction work very well on earth in an environment that can be perceived with senses. They do very well explain how fast an item falls to the ground from a given height. However, Einstein showed that the link is not linear when dealing with objects moving at, or close to, light’s speed. Thus, the relation does not really follow our senses and is the reason why Einstein’s theories are so difficult to comprehend, and why, despite Einstein, we are still dependent on Newton’s physics.

Among philosophers of science today, Popper is not particularly popular. Among many scientists, on the other hand, Popper is still considered a brilliant mind. Physicist Sheldon Glashow’s fierce critique of string theory, for example, reverberates Popper: it has not been possible to verify the theory through experiments, thus far: “there ain’t no experiment that could be done nor is there any observation that could be made that would say, ‘You guys are wrong.’ The theory is safe, permanently safe. I ask you, is that a theory of physics or a philosophy?” The scientists recognize Popper’s falsification arguments and description of the often trial-and-error-like process of performing research, where a theory precedes experiment, while philosophers find problems in the way certain key concepts are poorly defined, or not defined at all. What does it really mean to falsify a theory? For a philosopher of science, this is a highly complex matter. What happens if a minor part of a theory is falsified; does the entire system then fall apart? For Popper, the philosophical aspects of falsification were quite unproblematic: “Newton’s theory is a system. If we falsify it, we falsify the whole system,” a reasonable account, at first glance. But has Newton’s classical mechanics really been falsified by quantum mechanics? If so, why is it still in use? The simple answer is that it works under certain conditions. The same question applies to Einstein: Has his theory been falsified by the theories of quantum mechanics, since his theory does not work on a sub-atomic level? Magee calls Popper “a naïve falsificationist at the level of logic” and “a highly critical falsificationist at the level of methodology” (Magee 1985, 19). That is, the most important aspects of a theory are the set up of the experiment and the logic of the reasoning, not its philosophical underpinnings, which may seem an unusual stance for a philosopher of science. But in Popper’s view, it is a completely logical one since theories continuously change. Each scientific

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4 See Stove 1982 and Elizabeth Sayrs’s and Gregory Proctor’s text in this collection.
5 In an interview for PBS Nova (Glashow 2003). For a book-length critique of string theory, see Woit 2006.
6 Popper 1985, 124, originally published as “The Problem of Demarcation.”
discovery is only a step on an infinite ladder of understanding. Popper’s scientific world is a shaky, unstable one. As philosopher Stephen Toulmin argued, “Popper’s own philosophy of science had this element of paranoia in it. Because what he used to teach us is that the nearest thing to a true theory is one that hasn’t betrayed you yet. Any proposition is bound to let you down finally, but we cling on to the ones that haven’t let us down yet” (Edmonds and Eidinow 2001, 289).

* * *

Recognizing failure is a useful part of the scientific strategy. Only when failure is recognized can dead ends be abandoned and useable pieces of failed programs be recycled. Aside from possible utility, there is a responsibility to recognize failure. Recognizing failure is an essential part of the scientific ethos. Complete scientific failure must be recognized eventually.

Daniel Friedan (quoted in Woit 2006, 256)

Let us turn to music theory, then. Music theorists are normally not interested in absolute quantitative certainty à la Hume. With the exception of research projects using David Huron’s “Humdrum Toolkit,” a free piece of software that can be used in quantitative musical research, theorists typically do not care to find the answers to questions such as “Do all of Corelli’s movements in a minor key contain a Neapolitan sixth chord?” if not, “what relative number of works would include at least one?” Or to borrow some research problems from Humdrum researchers: “In the music of Stravinsky, are dissonances more common in strong metric positions than in weak metric positions? In Urdu folk songs, how common is the so-called ‘melodic arch’—where phrases tend to ascend and then descend in pitch? What are the most common fret-board patterns in guitar riffs by Jimi Hendrix? Which of the Brandenburg Concertos contains the B–A–C–H motif? … In what harmonic contexts does Händel double the leading-tone?” Thus theorists seem to go free from Hume’s critique. Music theorists are more interested in showing convincing interpretations—resulting in the competition of ideas in a process so well described by Kuhn. For example, in North America the structural-listening mode of hearing music has gained an immense influence during the last century, outmaneuvering other approaches. In Schenkerian analysis, it is probably impossible to “prove” the dependence of tonal works on species counterpoint; however, the theory does work for a great number of composi-

7 The Humdrum manual states, “Although Humdrum facilitates exploratory investigations, it is best used when the user has a clear problem or question in mind.”
<http://www.musiccog.ohio-state.edu/Humdrum/guide01.html>
— at least until there is a better theory. However, musical scholarship is more constrained by tradition. There are rarely any large, global paradigm shifts, as in the natural sciences, in which one mode of description nullifies earlier modes. Not even the powerful Schenkerian analytical method came into widespread use outside of North America. The reason is probably a certain conservatism: the older methods do explain what they are expected to explain. The repertoire has not changed, only expanded, so why should the analytical techniques? We have theories for historical music that work well, which would be the equivalent of Newton’s theory that works so well in daily situations.

Even if music theorists seldom do pay interest to falsification from a quantitative perspective, the notion has implications for music. On one level, the philosophical critique of Popper applies very well to musical scholarship: one cannot discard Schenkerian analysis because the method in its traditional form is not applicable to Webern’s twelve-tone music, or because it is difficult to determine fünf oder drei in the Ursatz of a piece such as the theme of Mozart’s Piano Sonata in A-Major, KV 331. To find one instance of parallel fifths in Bach does not falsify the advice, or rule, to avoid parallel fifths in the music from the High Baroque. But on another level, most music theory appears unscientific in the most fundamental way: it is difficult, if not impossible, to falsify any claim of a Schenkerian analysis, or even to describe what a falsification might be: “I don’t hear it that way,” is of course not enough, assuming that the analysis is technically correct. Schenkerian theory, for example, departs from a presumption that species counterpoint was inherent in the compositional practice. The literature is filled with verifications of this claim. Indeed it seems as if all swans are white, that is, they fit into a Schenkerian scheme. Or even more dangerously, through Hempel’s Pardox, which also departs from Hume: If we consider all ravens to be black, not only are raven-like birds not ravens, all non-black things are not ravens. The continuation of this paradox is even more challenging. Every perception of a non-black or even a non-raven thing, verifies the statement that all ravens are black (Edmonds and Eidinow 2001, 164). For an analyst searching for verifications of voice-leading rules, there is nothing but in the music, and analysts are often quick to dismiss any musical instance of a broken rule.

Within the natural sciences, these kinds of problems have recently begun to be addressed. Two online journals, *Journal of Articles in Support of the Null Hypothesis* and *Journal of Negative Results in Biomedicine*, acknowledge this fundamentally important feature of science: experiments do not always lead to the expected result, or not even to a useful result. Even erroneous hypotheses and failed experiments can be useful to the scientific community. *Journal of Articles in Support of the Null Hypothesis* states on their webpage:
In the past other journals and reviewers have exhibited a bias against articles that did not reject the null hypothesis. We seek to change that by offering an outlet for experiments that do not reach the traditional significance levels (p < .05). Thus, reducing the file drawer problem, and reducing the bias in psychological literature. Without such a resource researchers could be wasting their time examining empirical questions that have already been examined. We collect these articles and provide them to the scientific community free of cost.

An article that has musical implications is “Is There an Effect of Subliminal Messages in Music on Choice Behavior?” (Egermann, Kopiez, and Reuter 2007). Articles from 2006 in Journal of Negative Results in Biomedicine include the following fascinating titles, “Sporadic ALS is not associated with VAPB gene mutations in Southern Italy” (2006, 5/7, 29 May 2006) and “Aortic distensibility measured by pulse-wave velocity is not modified in patients with Chagas’ disease” (2006, 5/9, 12 June 2006).

These journals would have made Popper very happy: As he argued, even if they were wrong in many respects, Aristarchus of Samos’s and Copernicus’s astronomical theories were “far from meaningless; and in proposing a new bold view of the universe [they] made a tremendous contribution to the advent of the new science.” The scientific process is one of trial and error, and it is also a process where the scientific community collaborates. Any insignificant finding might help other researchers avoid performing the same experiment. Although there are articles dealing with problematic readings of a work or repertoire, it is hard to imagine articles in music theory entirely addressing one’s own failed analysis. But the notion of acknowledging and discussing methodological problems would be beneficial even in subjective analyses.

Music analysis thus seems to have a major Popperian flaw: it is unfalsifiable, which places it on par with psychoanalysis and Marxism (and according to some, also with string theory). But on the other hand, music analysis conforms to what appears to be Popper’s fundamental view of science, a theory is introduced and the analyst tries to apply it to the repertoire; or put in a more elementary way, when we approach a piece of music we have a preconceived notion of what music is, even if not a full-fledged formalized theory. Fux’s contrapuntal method, for example, may have been developed through hypotheses and then tested on existing works. Schenkerian analysis has proven to be very useful in analysis and in theory pedagogy in explaining hierarchies of a composition. Both methods have been successful, have undergone constant refinement, and are still in use. Theorists try to promote their interpretations and theoretical methods among their colleagues, although the paradigm shift rarely occurs. Falsification is thus a philosophical reality in music theory, as it was for Popper’s critics, creating a fascinating

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8 Popper 1985, 120, originally published as “The Problem of Demarcation.”
dichotomy. On the one hand, a musical analysis cannot be falsified, but the analytical process itself often works through a process resembling falsification and deduction.

Much of music theory is concerned with regularities, just like the natural sciences, which could be a danger. We do not want music to be disorganized; instead we want to discover its regularities of different kinds—in the piece or across the repertoire. We also want to find regularities that fit into an analytical narrative. We want to behold of the artwork in all its glory and create a comprehensive story incorporating logical compositional features. But this is a result of the fundamental difference between theory and analysis. In the creation of musical theories, regularity is the norm, but in analysis, uniqueness is sought within some regularity (see John Rahn 1989, for example).

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Ultimately, the surface–depth metaphor feels increasingly irrelevant to the new, exotic topographies characteristic of the postmodern work of art. When we recognize new kinds of space in a late twentieth-century piece of music—and we will—shouldn’t our analytical models (and the subsequent value-judgments) “mutate” right along with them? Robert Fink (1999, 123)

The aim of creating logical analyses is a problem dealt with by philosophers. It is an important meta-scientific problem that is relevant for the study of music. An influential article in evolutionary biology by Stephen Jay Gould and Richard C. Lewontin (1994), entitled “The Spandrels of San Marco and the Panglossian Paradigm: A Critique of the Adaptationist Programme” takes the initial argument from the realm of architecture: how spandrels interact with mural paintings. A spandrel in this article is the triangular space that is formed from the intersection of two rounded arches. The purpose of using an architectural example in biology is a pedagogical one: it is easier to discover problematic conclusions outside one’s own field. The chain of cause and effect is obviously inaccurate: the spandrels were not applied to provide a beautiful space for art works, they are an unavoidable result of constructing a dome from rounded arches. Their critique within evolutionary biology is founded upon the common notion of the purposefulness of each aspect of a living creature. There must be a reason for the Tyrannosaurus to have short front legs and a reason why the moles are blind, etc. They provide

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9 Gould has previously inspired a music theorist. See Patrick McCreless 1996.
a telling example: By the full-scale fiberglass Tyrannosaurus at Boston’s Museum of Science, the note reads “Front legs a puzzle: how Tyrannosaurus used its tiny front legs is a scientific puzzle; they were too short even to reach the mount. They have been used to help the animal rise from a lying position” (78–9).

The questions are valid, but the answers may not be related to a chain of cause and effect resulting from an logical adaptationist development. Gould’s and Lewontin’s critical description of this research tradition consists of two parts: First, an organism is divided up into different parts, or traits, each of which can be explained as a result of adaptation. Second, it is recognized that the different traits cannot be optimized without limiting other traits, and thus the notion of “trade-offs” is introduced: “organisms are interpreted as best compromises among competing demands. … Any suboptimality of a part is explained as its contribution to the best possible design for the whole” (77). This sounds like Candide, and the authors mockingly quote Dr. Pangloss: “Things cannot be other than they are. … Everything is made for the best purpose. Our noses were made to carry spectacles, so we have spectacles. Legs were clearly intended for breeches, and we wear them” (75).

Later Pangloss comments on the earthquake in Lisbon in 1755: “All this is a manifestation of the rightness of things, since if there is a volcano at Lisbon it could not be anywhere else. For it is impossible for things not to be where they are, because everything is for the best” (79).

Even if Gould and Lewontin were somewhat unfair in their critique of adaptationalism, and their work has been criticized, their arguments illustrate dangerous traps in scholarship in general and loudly resonate with musical scholarship as well. “Everything is for the best”—indeed, that resembles many analytical descriptions. A work of art by a master is absolutely perfect and would be of considerably less value if one line/note/word were to be changed. Every aspect of a work has to be explained and fit into the larger scheme. The Popperian notion of not trusting one’s senses is applicable here: a piece has to be so deeply analyzed and connections shown on so many levels, showing so many relationships involving the same feature (a motif, a set class, a transformation, etc.) that the end result may seem incomprehensible and very remote from a reasonable hearing of a work. One example is the Schenkerian notion of large-scale motivic coherence, in which background motives are mirrored in the musical foreground. First of all, such relationships can hardly be perceived, but that is not necessarily the major problem; rather, second, if the motif is simple enough, it will inevitably be found on more than one level. Another example is the frequent practice in Bartók scholarship to search for one analytical element—the Fibonacci se-

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10 See, for example, Daniel C. Dennett 1996. In short, he questions the notion that the spandrels and paintings are unrelated: maybe the architectural design was chosen to accommodate the paintings.
quence, pitch symmetry, or tetrachords—as the Rosetta Stone that would explain a work or virtually all of Bartók’s works. In musicology, attempts are made to map personal lives of composers with their works in ways that try to provide a tightly-knit web of cause of effect. Alan Forte expressed the belief underlying this problem: “Every detail, no matter how minute, is an integral part of the musical conception” (1972, 48). Indeed, this resonates with the physicists’ claim that superstring theory being a “unified theory,” a theory that explains everything.

* * *

… there is a legitimate place for dogmatism, though a very limited place. He who gives up his theory too easily in the face of apparent refutations will never discover the possibilities inherent in his theory.

Karl Popper

Instead of order and control, there are contrary opinions among philosophers of science regarding the process of scientific progress. Paul Feyerabend (1924–1994), the highly controversial philosopher of science, or perhaps an anti-philosopher, and a critic of Popper, argues against the notion of science being an organized logical field. Feyerabend’s major book, Against Method, deals with a few cases from the history of science, particularly astronomy with Copernicus and Galileo in the center of the debate. Scientific discovery, he argues, has been filled with mistakes and successful discoveries have happened by pure luck. It does not follow from the fact that scientific theories are, for the most part, logical and neatly organized that the way the conclusions were reached was as logical.

On closer analysis we even find that science knows no “bare facts” at all but that the “facts” that enter our knowledge are already viewed in a certain way and are, therefore, essentially ideational. This being the case, the history of science will be as complex, chaotic, full of mistakes, and entertaining as the ideas it contains, and these ideas in turn will be as complex, chaotic, full of mistakes, and entertaining as are the minds of those who invented them (Feyerabend 1993, 11).

He also criticizes the compartmentalization of science:

Scientific education as we know it today has precisely this aim. It simplifies “science” by simplifying its participants: first, a domain of research is de-
Feyerabend argues that real science instead is anarchistic and that good scientists are breaking the rules within their disciplines. His infamous statement “anything goes” refers to his belief that there is not one scientific method; a scientist has to work with a number of methods. His description resonates with quite a few traditions of music theory. Music theory has been divided up into small subareas that deal primarily with narrow aspects of music, particularly pitch. Even the study of pitch has been divided up into different discourses dealing with different time periods or different methods of analysis. This focus on methodology and technique excludes a wider inquiry into the nature of music. To paraphrase Imre Lakatos (and Kant), theory of music without philosophy of music is empty; philosophy of music without theory of music is blind.

A Popperian scientific project begins with a problem or a question: “what is the conductivity of a certain metal in different temperatures?” Schenkerians’ inquiries are often limited to the Schenkerian methodology, and often with a methodological orthodoxy: “What can Schenkerian analysis show about this piece?” And in other cases, the problem relates to the development of the method itself: “How can Schenkerian theory be used to understand large-scale register?” The method generates the questions instead of the other way around.

But many recent successful music-theoretical achievements reached beyond the fixed set of disciplinary behavior. Fred Lerdahl and Ray Jackendoff (1983), for example, incorporated linguistic theory in their harmonic-structure analysis. Their questions might have been more basic, “How does harmonic syntax work in tonal music?” and “Could linguistics provide an answer?” Another example is Neo-Riemannian theory, which has incorporated nineteenth-century harmonic theory with twentieth-century transformational theory. The questions here might have been “Can Riemann’s theory be of any use today to better understand the music at the time of the theory’s conception?” and “Are there any similarities with later methods?” In Schenkerian studies, cases have been made to show that some works may have an ascending Urlinie. For these scholars, it was unreasonable to graph the piece according to traditional Schenkerian techniques since it went against the way they perceived the piece.12 Other disciplines have also in-

12 David Neumeyer discusses a number of works in his “The Ascending Urlinie” (1987); Robert Fink provides the “inverted” analysis of the Credo movement from Missa Solemnis in “Going Flat: Post-Hierarchical Music Theory and the Musical Surface” (1999). Fink is “appealing not to hierarchy, but to the presence or absence of untransformed sounds on the musical surface.” The section he discusses “does not depend on a transcendental ear for tonal voice-leading on the largest scale” (112).
spired questions: “Has the gender of the composer has anything to do with the musical outcome?”

* * *

If some day it should no longer be possible for scientific observers to reach agreement about basic statements this would amount to a failure of language as a means of universal communication. It would amount to a new “Babel of Tongues”: scientific discovery would be reduced to absurdity. In this new Babel, the soaring edifice of science would soon lie in ruins.

Karl Popper

In natural sciences there has been a long tradition of de-emphasizing “Theories” in favor of “Models” (Morgan and Morrison 1999). In natural sciences, a Model is often built from theory, such as $F=ma$, but a simple equation is not enough to make a model to mirror the real world. In this epistemology, a theory is the smaller entity—a building block of a model. The equation needs modifiers, such as surrounding conditions that take into account forces of friction (5). The laws of physics are not really real systems but entities, parts of the model building, which can be turned into real systems (5). Theories are thus parts that help the scientist to make a model—underlying structures to explain the real world.

Models have a dual function not only can they explain complicated processes and make them easier to overview but the actual construction of a model will enhance the understanding of the system in question. Margaret Morrison shows how immensely complicated a model is needed to model the movement of as simple an entity as a pendulum. The more exact we want the model to work, the more complicated will the equations be. The model builder has to consider a number of forces that will effect the movement: the mass of the string, the buoyancy of the air, etc. The effect of each force is described through calculus, since in natural sciences, a model is often a non-linguistic entity (Morgan and Morrison 1999, 3). But the use of models has been favored in other fields as well. The perhaps most well-known examples from daily life come from econometrics. Real monetary movements are too complicated to grasp on a large-scale level. Agents in the economy are represented by different representational subjects to create models of different monetary movements. Complex issues of interest rate and supply of currency can be better understood.

13 Popper 1985, 160; originally published as “The Empirical Basis.”
What would a music-theoretical model look like? It seems to me that virtually everything we do is founded upon the notion of models—some more related to real music than others. A Schenkerian model represents the fundamental voice leading; the Fuxian model of counterpoint comprises a set of rules that captures contrapuntal relations in a Renaissance composition. David Cope has developed software to generate compositions in different styles, for example, piano pieces in the style of Beethoven or Bach chorales. By constructing the software, he was able to gain an immense understanding of the styles in question, and he received direct feedback. The underlying rules, or theories, in these examples are simple, but the model becomes extraordinarily complicated when the theories are combined. There are syntactic rules for chord connections and voice leading.

Many contemporary composers work through models as well. They build their own compositional systems through a set of rules that are then assembled into a model—the model being a draft of the composition. Music analyses may as well be labeled musical models. More recent approaches related to the pendulum example within music theory, although not widely discussed, include the large project, “Computer-Based Music Research: Artificial Intelligence Models of Musical Expression” in Austria. They fine-tune the analytical tools to develop quantitative models for understanding expressive dimensions of musical performance, that is, aspects of a performance not necessarily present in the score. The verification process is still problematic: How does one verify which performance is the most convincing one? The Austrian team is using both qualitative and quantitative means. The computer performance is compared with a human performance and evaluated in terms of musicality as well as how much the performances deviates in technical terms, and importantly, where the limits are for a comprehensive performance.

So what kind of models do contemporary analysts construct? If Fuxian counterpoint is a model to compose a piece, analytical models deal with theories aimed at highlighting some feature of a composition or repertoire. The Fuxian model builds works, while many contemporary analytical theories take them apart. Transformational theory, perhaps the most complicated analytical system due to its heavy dependence on mathematics and its wide range of applications, models relationships based on motion from one entity to another. David Lewin argues that the transformational interval could be used in a pragmatic way, in music making: “like a singer, player, or composer, thinking: ‘I am at s; what characteristic transformation do I perform in order to arrive at t?’” (1987, xiii), or to “hear” certain transformational progressions (164). For Lewin analysis involves a listening process to ask questions about “the ways in which I am satisfied and dissatisfied when focusing

15 <http://arts.ucsc.edu/faculty/cope/experiments.htm>
16 <http://www.ofai.at/research/impml/projects/startproject.html>
my aural attention in that manner. It is important to ask those questions about any systematic analysis of any musical composition” (1993, 53). In addition, an analysis (as well as the piece) should challenge the listener and reader. Lewin feels “dissatisfaction at an analysis that does not make me extend my ways of listening, and I feel it all the more when the analysis tells me pointedly that it is setting out to describe ‘what the music does to us’” (1993, 62). Lewin’s method is a scientific one, indeed. He presents a reading through arguments, and although the end result might not be falsifiable in terms of the way we hear a work, it certainly resembles the scientific deductive process.

Lewin’s model making departs from the perception of the work. It is an education process for the analyst: it requires an enhanced listening in combination with intense studies of the score. The model must reach beyond the obvious observations. But how far should the model proceed? Hardly anyone would disagree with the notion that an analysis must go beyond the obvious. But can a pendulum model exactly describe the motion of the real pendulum? No, only to a certain degree, to a finite number of decimals. The scientist have limits regarding the input data, the buoyancy, for example, as well as how exactly the model has to represent the real world. How deep should a musical analysis go to show relationships hidden in the score as hidden in a “normal” listening process? This is one of the key problems for music theory: many non-theorists think we have gone too far already.

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I believe that the function of a scientist or of a philosopher is to solve scientific or philosophical problems, rather than to talk about what he or other philosophers are doing or might do.

Karl Popper (1962, 66)

Finally, I do not think that music theory has to worry too much about being unscientific. Even if the falsifiability criteria are hard to fulfill in most analysis there is much more to scientific method. Falsifiability is a problem in other sciences too, particularly philosophically. Science is more than collecting facts and connecting the dots. Scientific theories and models, like analyses and methods, are like commodities on an open market where the best may win in the long run—although this is perhaps true to a lesser extent in music theory. Music analysis belongs to the overarching family of sciences in the sense Wittgenstein suggests when he talks about family resemblance: How can we make a condensed and precise definition of a “game”? It is
impossible since there are so many different kinds of games—card games, football, etc.—but they have something in common: they share a family resemblance. Music-analytical endeavors are to physics what black jack is to polo, or something like that.

The situation for music theory is similar to several decade-long discussions within the discipline of history. Historian Richard Evans (2000, 39) has argued that the discussion of whether history is a science departs from “an eccentricity of the English language.” The problem came about when the German term Wissenschaft was translated to English, science. Wissenschaft has a slightly different connotation and can easily be combined with areas within the humanities and social sciences, as in Sozialwissenschaft and Literaturwissenschaft, for example. Social Science works fine, whereas Literary Science sounds problematic, not to speak of Musical Science. But one thing is for sure though, whether or not we decide to call music analysis a science, the problems that philosophers of science have outlined do apply to music. Indeed, music theory is like science: it is flawed, random, and personal, and, like science, it does try to convey an interpretation through arguments.

The main lesson from this comparison has to do with the questions we ask. To be even more science-like, at least according to Popper (and I agree) music theory must aim at raising more central questions—fundamental questions about the nature of music, about how to compose music, about trying to understand what makes a piece appealing. The fundamental questions hardly are raised within the firm grip of one single modeling system, as David Lewin recognizes (1993, 55–6). These kinds of questions are probably not answered exclusively with a graph, as useful as that could be, it will only be part of the problem solving. There are real questions out there, not only methodological puzzles. I end with a quote by Bryan Magee:

The scientist and the artist, far from being engaged in opposed or incompatible activities, are both trying to extend our understanding of experience by the use of creative imagination subjected to critical control, and so both are using irrational as well as rational faculties. Both are exploring the unknown and trying to articulate the search and its findings. Both are seekers after truth who make indispensable use of intuition (1985, 69).
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Playing the “Science Card”
Science as Metaphor in the Practice of Music Theory

Elizabeth Sayrs
Gregory Proctor

Introduction
It has been forty-five years since Milton Babbitt’s famous assertion that the “scientific method” and “scientific language” were the only appropriate means “for the verbal formulation of ‘concepts’,” whether in music theory or in any domain (Babbitt 1972 [1961], 3). While Babbitt was arguing simply for musical discourse to be responsible and meaningful, the “scientific” aura of his injunction has come to have as much influence on the field as has the impulse behind that injunction, permeating the contemporary practice of music theory. In the fifteen years since Brown and Dempster presented “The Scientific Image of Music Theory,” music theory finds itself squeezed from at least two sides. While weathering the cannon fire of charges of positivism lobbed by New Musicology (and having achieved a mutual transformation of fields in the best cases), music theory at the same time has been taken to task for not being scientific enough. Thus, simultaneous to the development of interaction between music theory and New Musicology, in a concurrent stream the mantle of “scientific practice” has come to cloak much of the contemporary practice of music theory. Now we consider the testability of Schenkerian and set theory using (extrinsic) music cognition techniques; we have mathematical models of musical space; we have computational analysis; we see the fields that constitute “scientific music research” such as “psychomusicology, ethnomusicology, and other systematic disciplines” pitted against the “methodological mediocrity” that is alleged to flourish in music theory and analysis (Huron 1995, 476, 479).
In considering the status of “the scientific” as the model for music theoretic research, as music theorists we restrict most of our examples to “mainstream” music theory. One impetus behind our chapter is to bring the many streams of science and music research into communication with each other; this implies that there has been lack of communication heretofore. As Sharon Traweek notes (here in relation to the social sciences), “We are misunderstanding each other’s work because of our dissimilar discursive practices” (Traweek 1992, 433). (Examples of this lack of communication will be cited shortly.) Because the appeal to “the scientific” has become so widespread, we cannot possibly examine how each individual author applies (or misapplies) such models (although occasionally we necessarily must explore such details). Rather, we wish to focus on the purposes for which “the scientific” is invoked in music-theoretic discourse. Over the years since Babbitt’s dictum, the field of the philosophy of science has been visited upon the field of music theory. Once we commit ourselves to such an intrusion, we have the responsibility to get it right, to follow the field as it develops, and to avoid using it for political purposes.

Similar issues arise in other interdisciplinary exercises, whether the field being drawn upon is philosophy of science, literary theory, or feminist theory, among others. As Ian Quinn has noted, “[i]t is difficult enough to be an expert in a single field: it is next to impossible to be fluent in several” (Quinn 2000, 244). We choose to examine the use of scientific discourse because, in society at large as well as within the university, this discourse is credited with the highest degree of rigor, objectivity, utility and truth-value. It is important to note that the invocation of “the scientific” is a problem not just in our own field; nearly all fields have been affected by the “science wars.” Traweek notes,

I know that all of this rigor and propriety and theoretical testimony ought to include as many numbers, mathematical symbols, and charts as possible; this sort of work is called ‘quantitative,’ and the people who do it call everything else ‘qualitative,’ when they are being nice. In descending order of politeness, research that does not meet their own rhetorical criteria for science is

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1 But we also point out that the status of “research” and “science” as an authority to be respected is not universal, even within what many would consider “our culture.” Because the atrocities committed by colonial researchers still resonate, for example, Smith (2002) notes that “from the vantage point of the colonized … ‘research’ is one of the dirtiest words in the indigenous world’s vocabulary” (1). Thus for those doing what is called “aboriginal” research in Canada and Australia, the term “research”—let alone “scientific” research—has a quite different meaning. Smith also argues that “[r]esearch is one of the ways in which the underlying code of imperialism and colonialism is both regulated and realized,” reinscribing the binary between the West and the Other (8). Traces of logical positivism’s imperialism linger in our language about music and science today: one researcher writes, for example, that music theory as a “discipline would not be safe from the zealous probing of systematic musicologists” (Huron 1995, 479).

Playing the “Science Card”


Theories and “Common Sense”

Despite the respect typically accorded scientific discourse, the general public has no idea what a theory of any sort is, let alone what music theory in particular might mean.

“So you’re a theorist. What’s your theory? Ha, Ha!”

How facts and theories relate to each other is a complete muddle in the popular media and therefore in the public mind (see Figure 1).

![Figure 1. The popular perception of the relation between theory, philosophy, and fact.](image)

This is best exemplified by the controversy surrounding the teaching of evolution in the schools. It is sad enough that the popular mind imagines that Darwin articulated a theory of how life arises; they also assume that this elaborate apparatus (confirmed beyond the dreams of any music theorist) is somehow a lower order of entity than unarticulated “facts.” Consider the following, one of the stultifyingly similar variations on the familiar theme that is repeated across the United States as part of the so-called debate over evolution:

Michael Cochran, a board member from Blacklick in suburban Columbus who earlier had pushed for intelligent design to be included in the standards, said he looks at the final draft of the standards as a compromise that presents evolution as a theory and not a fact.³

Then also, we find people who say “I have a theory that…” This is intended to convey that “I have an assertion of cause of one phenomenon by

another, but I have no mechanism for determining how to confirm it or its contrary.” This is to say that in popular discourse, the phrase “I have a theory…” is a signal that no theoretical structure will be revealed; it is used to lend legitimacy to under-considered opinion, while at the same time distancing the speaker from full responsibility—after all, it is “just a theory.” At the “lowest” level, moving down from “fact” to “theory,” is the popular notion of “philosophy”: “It is my philosophy that…” suggests that you can expect me habitually to make such assertions, while “It is my philosophy to…” suggests that I behave according to habit. In the world at large, therefore, philosophy not only does not contain theories, it is opposed to them. Thus the “common sense” view of science categorically inverts the hierarchy to which philosophers of science ascribe.

All of this would seem quaint, amusing, and perhaps even expected, when we consider the state of both science and philosophy in the public schools—if it weren’t that music theory generates the near equivalent as a matter of course. There is a danger in believing that the only way to avoid bias is to somehow operate outside theories; of believing that we only deal with, or should only aspire to deal with, immutable, self-evident facts. As Babbitt noted, we should not equate freedom from constraints with ignorance of the constraints under which we work (Babbitt 1972 [1965], 10).

Theory and Philosophy of Science

After half a century of upheaval arising out of the doubt that philosophy of science has heaped on the once widely-accepted positivist school—the aforementioned “science wars”—there is broad consensus by that field about what components a theory might have. A good representation comes from Frederick Suppe:

In general, a scientific theory has the task of describing, predicting, and (possibly) explaining a class of phenomena. It does so by selecting and abstracting certain idealized parameters from the phenomena, then characterizing a class of abstract replicas of the phenomena … The theory thus provides a comprehensive characterization of the behavior of physical systems over time. When coupled with an appropriate experimental methodology, the theory can also predict or explain phenomena which do not meet these idealized conditions by displaying how these phenomena would have behaved had the idealized conditions been met (Suppe 1989, 67).

It is important to note that, despite the conflation of theory and methodology in popular conceptions of science (e.g., the “scientific method”), according to Suppe, experimental methodology “is not part of the theory; rather, it is based on various extra-theoretical regularities, other theories, laws, known regularities about the kind of phenomena involved, and so on” (Suppe 1989, 103).
Although Suppe’s particular version of scientific theory is what he calls the “semantic conception”—and is elaborately developed—this general description of theories is a reasonable generalization for the field as a whole.\(^4\) Despite real conflicts about other aspects of theories, there is general agreement on these baseline claims.

**Theory, Justification, and Testing**

Contrary to the common conception of “facts” as something *proved* through a theory, philosophy of science considers facts to lie either within the theory they feed or immediately at its boundary. If within, they are *defined* by the theory itself. If at the boundary, they are defined by another mechanism, perhaps amounting to nothing more than intersubjective agreement, but which is more often another theory that either contains the object theory or abuts it conceptually (see Figure 2).

![Figure 2. The relation of facts to theories.](image)

Thus facts are defined and agreed upon: not real-world entities in any demonstrable way, but abstractions dependent on theoretical frameworks for their contextual meanings. In the (normal) case where they are prior to the theory, they are axiomatic, in the sense that their nature and utility are visited upon the theory from without. Attempts at justification of assumptions are equivalent to attempts to justify facts; any justification resides elsewhere. That “elsewhere” is itself theoretical.

Rather, theories address, depending on one’s philosophical orientation, the truth, validity, value, and/or utility of hypotheses. Hypotheses are simply the articulation of problems, the asking of questions, or the making of predictions that can then be evaluated with respect to the theory and its back-
What Kind of Theory Is Music Theory?

ground information, including facts. Thus it is possible that the narrative of explanation engendered by a theory may not be supported when a hypothesis is evaluated. This may lead to an alteration in the hypothesis or theory, or it may lead to the reconsideration of the utility of the assumed background information, including facts. One may then alter the assumed background information, but this does not equate to “disproving” the background information or the facts.

The urge to make music theories “scientific” seems to some a call for demonstrating objective, true foundations for theoretical assumptions. The greatest problem with music theories over the years has been precisely this attempt to justify the assumptions of the theory. It is impossible. In the course of brilliantly creating the concept of pitch class, for example, Rameau variously had recourse to the stretched string and its integral low-number divisions; to the 2:1 ratio as indicating separate elements, but also as a marker of “identity”; to the harmonic series; and finally to the undertone series. Similar excursions were regularly picked up by subsequent major figures in the field, including Riemann and Schenker. Hindemith added to them the force of gravity as a source for the sense of rootedness of intervals. Despite this hope for confirmation of foundations, a theory cannot reach outside itself to dispose of its assumptions as though they were part of the theory.

One may—as we often do—happily believe in the external reality of the phenomena our facts point to. Following Carnap, we expect that if we send a realist and a solipsist out to measure a mountain, they will come back with

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5 Here we agree with Suppe, Shapere, and others that predictions and “why” questions are not the only valid aims of a scientific theory. See, for example, Suppe 1989, 179–81.

6 When this paper was presented as part of a lecture series, we were asked indignantly by an audience member—a composer—whether we therefore were rejecting the harmonic series as the “cause” of Western tonal music. Yes, we do.

7 The implication in the foregoing, as many post-positivists have argued, is that “truth” is slippery. As indicated earlier, people in general equate facts with truth, so if facts are flexible and theory-dependent, then truth is likewise non-absolute. Truth, of course, continues to reside within symbolic logic in the sense of “truth-values.” But symbolic logic is as close as we shall ever get to uninterpreted theory, and we shall still have the problem, taking even a positivist position, of connecting our observations with our logical mechanism. Beyond that, it ought to be borne in mind that the predictive value of physicalist theories—at least—hangs on counterfactual conditions of a certain type. Unfortunately, symbolic logic cannot differentiate among the kinds of counterfactuals. There is a strain of philosophy, exemplified by Kuhn and Feyerabend, which maintains that your truth and my truth may not survive the correlation equally. Feyerabend goes so far as to argue that the terms only have meaning with respect to their theoretical system; thus each theory has its own unique experience. (See the discussion in Suppe 1977, 636–49 and Suppe 1989, 301–37.) We of course have beliefs and knowledge that we trust in the consideration of one another’s statements, so we are more likely to be realists or quasi-realists. If not, we could not sanely be in a scholarly field. Yet, even Suppe, who rejects the extreme relativism of Kuhn and Feyerabend, cautions that “the issue of scientific realism is not whether theoretical knowledge is possible, but rather what its limits are” (Suppe 1989, 346). Van Fraassen (1980, 10) maintains that “theories need not be true to be good.”
the same information, whatever the ontological status they attribute to the mountain. And as music theorists, we adopt the stance that we are trying to figure out how “music works,” while acknowledging that it “works” in different ways in different domains—compositionally, performatively, analytically, conceptually, perceptually, and so on, each in a multitude of cultural contexts. But to be rigorous is not to be unbiased in the sense of being free from theory; to be aware of the metaphorical basis of theories is not to be irrational. Rigor and lack of bias reside in the precision of use of one’s tools, not in scientists’ belief in some connection to ultimate reality to which they are privy by virtue of the machinery they use. This is important to include, since claims to the “scientific” approach as a corrective not only entail the notion that those being “corrected” are unscientific, but also that they indulge in something other than literal truth—call it metaphor.

For example, Marion Guck proposes that Babbitt’s “methodological precepts apply to a wider range of language than most of his readers might have imagined,” and advocates “an explicit connection of interpretive attributions with observational language” (Guck 1997, 55). (This is reminiscent of philosophy of science’s post-positivist position that there is no clear distinction between observational and theoretical language.) For Guck, this occurs in the context of using language with precision, and articulating reasons for assertions. The goal of such activity is to “codify and enhance hearing,” and to “address the contextualities of the musical work in order to give an account of its individual identity” (Guck 1997, 55). In this case, the precise language used with rigor is metonymic language.

Babbitt’s response to Guck’s article is interesting: he wonders why Guck “approaches the danger zone of that vulnerable yet invulnerable private language,” and with it the risk that one might veer forth from “the properties of the object” into “unintelligibility.”

In this respect, Babbitt’s response is in accord with what Lakoff and Johnson have called the “first generation” of cognitive science, which has considered the mind to be computational (analogous to a computer); thus human thought was construed to be best represented by the manipulation of content-free symbols abstracted from specific knowledge, manipulated

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8 In this respect we disagree somewhat with Cook (1990), but we are less convinced that there is a necessary distinction between Cook’s “imaginative knowledge” and the “facts of the matter” (Huron 1995, 477).

9 This reflects the view of scientific realists that theories are either true or false, and that such truth is literal, not metaphorical (Brown 2001, 96). This view is reflected in music theory as well. In his review of Cook 1990, Huron (1995, 478) defends the uncrossable divide between literal truth and metaphor, arguing that by “removing theorizing from any discipline of verisimilitude, Cook, in effect, sanctions any kind of gobbledygook that can gain some currency in music circles as valid theorizing.” See also Dempster and Brown 1990, 257–8.

10 Babbitt 1997, 135. This hinges on Goodman’s assertion, quoted by Babbitt, that “what is translated in the world of words is what is denoted.”
analogously to symbolic logic. Recent studies in the philosophy of science and cognitive science have gone on to consider and substantially alter this view, proposing that the mind is not well-represented by content-neutral symbolic logic (the “mind as machine” metaphor). Rather, there is a neurally- and phenomenologically-instantiated logic of perception and motor movements; there are conceptual metaphors based on bodily (sensory) experience. These form the basis for linguistic metaphors. Embodied conceptual metaphors are used to project inference patterns from a source domain to a target domain. In short, reasoning is fundamentally largely metaphoric. Abstract concepts may have a literal non-metaphoric skeleton, but they must be fleshed out metaphorically in order to be useful and available for reasoning. As Lakoff and Johnson put it, “metaphorical thought is what makes abstract scientific theorizing possible” (Lakoff and Johnson 1999, 128). Even so abstract and “pure” a field such as mathematics relies on metaphoric entailments (see Lakoff and Núñez 2000). (Brower (2000), Larson and VanHandel (2005), Johnson and Larson (2003), Saslaw (1996), Sayrs (2003), and Zbikowski (2002), among others, have explored the relationship between conceptual metaphor and music.) More globally, if our “scientific” language is inherently metaphorical, the argument that “other” language—specifically what is commonly called “metaphoric” language—is necessarily less precise loses its basis. Some have argued that studying metaphor is “more scientific” because this stance relies on a theory of the mind that is “more cognitively and neurally realistic” (Lakoff and Johnson 1999, 79).

Having completed this brief linguistic tour, we now turn to the issue of empirical testing in music theory within the context of contemporary philosophy of science. David Huron has rightly pointed out that empirical testing does not only apply only to perceptual phenomena, but to analytical, formal, and historical hypotheses, and so on, as well (Huron 1995, 478).

Some of the problems with the empirical testing of music theory arise from framing errors. These are often minor and perhaps arise from different practices both within the field and compared to other fields. Consider the recent article by Bret Aarden and Paul von Hippel that attempts to discover empirically which “rules” of chord doubling are “necessary” by creating a computer model that, when presented with two chords, is able to predict which of the chords is “real,” i.e. taken from the literature of Bach, Haydn, and Mozart; and which is “incorrect,” i.e. generated “randomly” using a set of constraints (Aarden and von Hippel 2004). This is an interesting task, and

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11 Lakoff and Johnson 1999, 75 ff. This view also entails several other “philosophical commitments,” including functionalism (that the mind is disembodied); symbol manipulation (thought is formal operations regardless of the content of symbols); representational theory of meaning; classical categories; and literal meaning (that no meaning is fundamentally metaphorical or imagistic) (78–9).

12 See, for example Lakoff and Johnson 1999.
includes an interactive program that allows one to try to beat the computer model’s success.\textsuperscript{13}

We simply wish to point out that the original framing of Aarden and von Hippel’s research is slightly misleading; it conflates pedagogical rules for chordal doubling (as articulated by composers and theorists) with how theorists think music “goes.” Aarden and von Hippel identify two trends in the teaching of chord doubling rules: the “double a particular chord member” school vs. the “(don’t) double a particular scale degree” school. Not surprisingly, they found that both schools were equally “correct” at describing common-practice chord doubling, noting that “scale-degree and triad-member rules are different ways of describing the same musical practices” (Aarden and von Hippel 2004, [16]). They hasten to point out, however, that “This is not to say that the results merely tell us what theorists already knew. It is not entirely clear what theorists knew, since theorists disagreed on a number of points” (Aarden and von Hippel 2004, [14]). But it is not clear that music theorists disagree about what constitutes musical practice as much as they disagree about the best pedagogical methods to lead the student to that musical practice, particularly to the underlying voice-leading that drives surface part-writing in common practice music. As a test of “theoretical rules,” there is no difference in accuracy between the triad-member rules and the scale-degree rules. But as a test of “pedagogical rules,” this research explores only half of the issue. The empirical test of a pedagogical rule must include an evaluation of the outcome of those rules, or in this case, how well these pedagogical rules lead a class of students in a variety of settings closer to common practice music (and it is in this “laboratory” that many of these pedagogical rules originally arose).\textsuperscript{14}

\textsuperscript{13}One of the authors has regularly “beaten the computer” with ease; one simply has to recognize that the real task is not to discriminate between “correct” and “incorrect” chord doublings, but to choose which doubling is more likely when confronted with two plausible doublings. When presented with simple four-part chords drawn from Bach, this requires a two-step process: 1) reject any unequivocally incorrect doublings (e.g., doubled leading tones); 2) among plausible doublings, simply imagine the context(s) in which each doubling could occur, and choose the chord that fits the context that occurs more frequently in Bach. Most of the time, this exercise is really a contextual probability task, not an isolated discrimination task. It should also be noted that when trying to beat the computer, you are asked for some personal data, including your level of education. This author routinely lies in this respect (and other similar internet situations), but especially in this case, because of an initial intuition that this information would then be used to “prove” that the computer does better than Ph.D. theorists, or those without training do better than those without, etc., without informing that participant about the purpose of such data. And indeed, in their reply to Wibberley (2004), the authors assert that the computer model’s success rate of “70% is certainly higher than the average accuracy of expert readers” (defined as those with a Ph.D. in some area of music), despite the broad consensus that self-reported internet data is inherently useless because of its unreliability (von Hippel and Aarden 2004, [16]).

\textsuperscript{14}We wonder if this conflation of pedagogical with musical rules is due in part to the popular understanding of Kuhn’s description of the accepted body of knowledge in a field as “textbook knowledge”; thus the materials in a theory textbook would constitute the accepted knowledge in music theory. But music theory is a bit different from most other fields, because
More crucial is when the questions asked by what is termed “empirical
evaluation” are substantively different from those asked by music theory.
Often these differences are acknowledged, whether these are called aesthetic
vs. theoretical claims, or competency vs. performance, and so on.\textsuperscript{15} But
sometimes, what Traweek has described as “dissimilar discursive practices”
 obscure these differences. We first consider a case in which empirical
 evolution does not reach outside the methodologies of a single area; then we move
to an example that reaches across areas.

Considering Melodic Peaks

Let us take as a case study Zohar Eitan’s 1997 book \textit{Highpoints: A Study of
Melodic Peaks}, as well as two reviews of Eitan’s book by Ian Quinn in \textit{Music
Eitan studies selections from Haydn, Chopin, and Berg in order to test hy-
 potheses about the role of melodic peaks across stylistic boundaries. Eitan
articulates two cross-stylistic findings about melodic peaks in this repertoire:
“First, in each style peaks tend to be approached (and to a certain degree left)
by relatively large intervals. … Second, all three styles have a strong inclin-
a to present the peak pitch only once in a segment” (Eitan 1997, 145).

Both Huron and Quinn are suspicious of these findings for statistical rea-
sons. Considering Eitan’s first claim—that peaks are approached and left by
large intervals—both Quinn and Huron assert that this is a “statistical arti-
fact” related to pitch distribution (identified by Quinn as a result of regres-
sion to the mean). Both Huron and Quinn use the example of lining up a
random sampling of people as an example of regression to the mean (al-
though Huron does not use this terminology). The tallest person in such a
lineup is likely to have a greater height differential with the people on either
side than with the average height of the group, because the natural distribu-
tion of heights in a given population tends to cluster around the average
height.\textsuperscript{16} Along these lines, Huron argues:

\begin{quote}
[A]lthough peak points are approached and followed by larger intervals than
is the case for other notes, peak points are not approached and followed by
intervals that are larger than would be expected in a random ordering of the
notes of a melody. The larger melodic intervals surrounding peak pitches
seems to be little more than an artifact of the fact that peak pitches are simply
further away from the majority of other pitches in the melody (Huron 1998,
261).
\end{quote}

\textsuperscript{15} See, for example, Mailman 1996 and Cook 2002.
\textsuperscript{16} For a more rigorous discussion of regression to the mean, see Campbell and Kenny 1999.
Also considered statistically problematic is Eitan’s choice of control pitches—he uses random pitches as controls, rather than low pitches, as Huron suggests. Huron writes:

Consider … the hypothesis that peak pitches tend to occur uniquely (only once) in a musical segment. Because peak pitches lie at the end of the pitch distribution, one would expect them to be rare for the same reason that a person with the biggest feet in a crowded room will tend to be the only person wearing that particular shoe size (Huron 1998, 259–60).

In other words, Eitan can not show that there are fewer peak pitches in relation to middle pitches because there are fewer peak pitches in relation to middle pitches.\textsuperscript{17}

In both cases, consider what we are being asked to view as comparable situations. One (big feet, tall height) is a natural distribution of a feature across a given population, influenced by the vagaries of genetics, nutrition, social conventions of marriage and procreation, and so on. The other (melodies from Haydn, Chopin, and Berg) is a deliberate and immutable distribution of pitches in musical space and time; there is no element of chance in this data set.\textsuperscript{18} On the basis of this “natural” analogy to population genetics, both Huron and Quinn declare these two findings of Eitan’s to be “suspicious” (Huron 1998, 259) and “fallacious” (Quinn 2000, 242) respectively. Quinn elaborates that “[t]he regression fallacy is the error of assuming that some unrelated factor causes the regression, even though the regression is a purely stochastic phenomenon” (Quinn 2000, 241).

But claiming that peak melodic notes exist, that they are perceptible, and that they help form how we conceptualize melodies is quite different from claiming that they are caused by “artistic tendencies,” or stylistic constraints, or the “gap-fill” psychology of the Penn school.\textsuperscript{19} Besides, we know what caused these melodic peaks: a composer put them there. Even if we accept the regression to the mean and random ordering as appropriate tools in this case, it does not change the “truth,” as Huron points out, that melodic peaks are approached by larger leaps than other notes in melodies (Huron 1998, 262). Here, the relationship to population genetics, which is unequivocally related to stochastic phenomena, is telling: in context, the really tall person

\textsuperscript{17} While there are certainly constraints based on tessitura (as pointed out in von Hippel and Huron 2000) and comfort, one can also imagine a repertoire in which the pitch distribution is the opposite of this supposedly “natural” distribution, where most pitches lie at the extreme, and very few lie in the middle; consider an extended compound melody, or the second oboe part in a Classical symphony.

\textsuperscript{18} It should be clear that we are not rejecting the concept of regression to the mean—a well-known statistical concept—but are questioning its application to this set of questions as asked by music theorists.

\textsuperscript{19} While Eitan raises the question of whether his findings “represent some image of ‘good’ melodic gestalt” or “are related in some way to cross-linguistic traits of speech intonation,” he is clear that these questions cannot be answered by his study (Eitan 1997, 146).
still is taller compared to his or her neighbors rather than compared to the average height; that one pair of really big feet still stands out as rare, regardless of whether the tallness or big feet are statistical artifacts. As Godfrey Winham once asked, “Do you mean to say that because something is necessary, it is trivial?”

How does the “mainstream” music theorist fit into such a discussion? The first question from a music theorist is likely to be, “Do you mean melodic peaks, or just the highest note in a segment?” (Eitan takes the highest note, but is aware of the issues entailed by that choice.) For a Schenkerian, for example, the random ordering of melodic notes in Schenker-tonal music will always result in the melodic peak being left by step; otherwise it is not a melodic peak, but a cover tone, octave transfer, or some other such transformation. These differences arise in part because Huron’s and Quinn’s discussions seem to claim that at some level they are dealing with unmediated “natural” phenomena; theorists deal with human artifacts, consonant with Guck’s assertion that “it is only when events are interpreted that they come into existence as part of a musical work.”

This is not to say that we believe that investigating the extent to which innate human capacities interact with music-making is not interesting or useful; quite the contrary. But these are two distinct theoretical stances, with different background information, and the results from one can not prove or disprove the results of another. Whether one chooses to believe that Eitan’s observations about melodic peaks are relevant or are rendered “fallacious” because of their status as statistical artifacts or by regression to the mean depends on the observer’s overarching theoretical stance, not on statistical calculations or musical phenomena.

Schenker and the Empirical, Part I: Putting Popper to Bed

One hears calls for the empirical testing of Schenker’s theories, by which is often meant determining if one can “hear” Schenkerian structure, whatever that might mean. In fairness, we should recognize that Schenkerians have tyrannized students and other Schenkerians for generations by appealing to their own superior ear, which is to be understood to guide their graphing and confound any Philistine in disagreement with them. Even so, a great theory ought not to be punished for the intemperate imperialism of some Schenkerians.

An attempt was made by Nicholas Cook (1987, see also Dibben 1994) to put the Ursatz to the test. The issue lay in how the subjects responded to transposed endings of pieces. Since the subjects were not gifted at differenti-

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20 Comment in tonal theory seminar, Princeton, 1968.
21 Guck 1997, 60–1 (based on Boretz 1972, 149–50). In this respect we agree with some of the comments of Taruskin (1989).
ating sufficiently well among the endings, it was proposed that a fundamental prop of Schenkerian theory, tonal closure and containment within a single harmony, was unsupported. These findings were considered by some in the cognition-perception community as an empirical demonstration of how truth could and should intrude correctly upon music theorizing. That is, they were used politically. On the other hand, Robert Gjerdingen has pointed out that Cook’s experiment was methodologically so seriously flawed as to render the results unusable.\textsuperscript{22}

To have an empirical perceptual/cognitive test of Schenker’s theory, if we were Popperians, we could do two things: falsify it or confirm it in Popper’s terms, and by doing so, reinforce that it is actually a theory. In fact, Popper has emerged as scientific authority of choice in music theory, especially if one wants to take a music theorist to task for not being “scientific enough,” particularly based on Popper’s criterion of falsifiability.\textsuperscript{23}

The standard Popperian ideal, that a theory must be falsifiable to be confirmable and make predictions, is normally conceived of in simple taxonomy-level terms. Consider:

Person A: All sheep are white.

Person B: Here is a black sheep. And over here is a brown sheep, but they call it “red.”

Person A: Oh, sorry. (By the way, you just needed one counterexample.)

Where communication is sought, the situation is always more complicated. Person A may adjust the statement in response to an accumulation of facts:

Person A: All sheep are white.

Person B: Did you mean all white? Actually, Suffolk sheep, for instance have black faces.

Person A: Suffolk black-faced sheep are white.

Person B: Here are Suffolk lambs. Notice that all are black…

Person A: Adult Suffolk black-faced sheep are white.

Person B: …and many stay black when they grow up.

Person A: A lot of adult Suffolk black-faced sheep are white.

Person B: O.K.

\textsuperscript{22} Gjerdingen 1999. Yet this has not stopped the citation of Cook’s experiment as having “proven” that tonal closure does not exist.

\textsuperscript{23} See, for example, the two citations of Popper within fifteen pages of each other in two different reviews in the Spring 2004 issue of \textit{Music Theory Spectrum}. In both cases, Popper is wielded as a de facto authority in order to dismiss the author of the book being reviewed as less scientific than he ought to be (Hermann 2004, 150, fn. 4, and Agmon 2004, 135, fn. 4). Honing (2006, 3) also notes “the growing role of formalization and the notions of testability and falsification” following Popper in music scholarship. See Brackett 2003 for other branches of philosophy of science that have influenced music theory.
Note the progression from a law with a universal quantifier, to a loss of the explicit statement of the universal quantifier together with a differentiation among breeds, to a differentiation between mutton and lamb, to a statistical assertion. In fact, in the process of challenge and revision the theoretical assertions become more informative, especially when we know the history of the objections.

If Person A had instead asserted that “All sheep are white and all responses to my assertions are the work of Satan,” then Person A is shut off from the tool of falsifiability, which saves the statements but prevents them from confirmation and communication of knowledge. When statements are accumulated to resemble a quasi-theory, the prime indicator that communication is not intended can also be reflected in the lack of a continuous chain of logic. Satan (or a Schenkerian’s “I hear it that way”) intervenes to ward off challenge to the coherence of the argument.

An apparently similar ploy, but one that can eventually prove fruitful is to deny the facts.

Person A: All sheep are white.
Person B: Here is a black sheep.
Person A: That’s not a sheep.

This seems as incorrigible as the Satan statement, but plays a role in the real world. To reject a fact may reveal that the fact is not relevant to the theory, or that the theory explicitly forbids naming the fact in the way it is named. The English sparrow is taxonomically not a sparrow, but a weaver finch. It just looks like a sparrow. Or further investigation in the face of the contradicting fact may provide the opportunity to adjust the theory, as in the case of black sheep.

As science becomes more complex, falsifiability becomes fuzzier; and the trend in the philosophy of science in recent years, across a broad spectrum of viewpoints, is that falsifiability does not demarcate science from pseudo-science in the way that Popper had proposed. Empirical testing in the practice of science is normally neither in the form nor for the purpose of disconfirming anything, despite Popper’s protest. Since predictions are derived from theories by the addition of auxiliary statements,24 the test of predictions tests the complex or even just the auxiliary statements, but not the theory directly. Leonard Susskind gives numerous examples of theories that are accepted today but were originally dismissed by opponents as not falsifiable in Popperian terms, including quark theory, Alan Guth’s inflationary theory, and Darwinian evolution.25 This is not to dismiss Popper’s contributions, but rather to assert that the appeal to Popper as the last word in defining the “sci-

24 Setting boundary conditions, for example.
25 Susskind 2006, 193–4. Lamarckian theory, on the other hand, passes the “falsifiability test.” See also Cook 1989, 128.
"Scientific" does not reflect the current state of philosophy of science, but is usually played as a “science card”; the effect is to close down rather than foster communication. But to be free from Popperian falsification is not to be free from concepts of testability or confirmation; exactly what that means in the context of music theory remains contested.

Schenker and the Empirical, Part II: Modeling Schenkerian Theory

In “The Scientific Image of Music Theory,” Brown and Dempster—to their credit—offered a version of Schenkerian theory that laid claims to be at least an outline of orderly discourse. Among other questions, they asked: “What predictions does the theory make that will allow us to test the theory?” (Brown and Dempster 1989, 98). In a subsequent article, joined by David Headlam, they proceed to make such a test (Brown, Dempster, and Headlam 1997). Their procedure follows a plausible model of empirical testing.

We summarize their argument and critique their test, both in their own terms and in light of a competing theory, the theory known as “19th-century chromatic tonality.”

Brown, Dempster, and Headlam state what they assume Schenkerian theory to be: the model outlined in Brown and Dempster 1989. With respect to the harmonic content admissible under the theory, they observe that, through mixture, their theory allows the possibility of a major and minor triad on each of the ten versions of seven scale degrees of combined major and minor diatonic steps of a single tonic, to which is added the lowered second scale degree, or “Phrygian II.”

There are thus major and minor triads on each of eleven of the twelve chromatic pitch classes that can be said to be directly related to the tonic. This set, built on F, would be: F G♭ G D A♭ A D♭ E♭ E. (This is without the intervention of tonicization, which can produce some of the same harmonies as mixture but which necessitates the intervention of another harmony in the derivation scheme.) The only unfilled gap in this set is that between the fourth and fifth scale degrees. Their first auxiliary statement, therefore, is that filling this gap—representable as either IIV or °V—requires tonicization. They then hypothesize that, as theirs is “an empirical theory of tonality,” they can use their first auxiliary statement to test for the limits of tonality (Brown, Dempster, and Headlam 1997, 157). As a model for their analytic preferences, they provide a series of examples of Schenker’s own analyses to show the categories to which IIV’s and °V’s

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27 These are the three forms of mixture—simple, secondary, and double—as defined concisely in Aldwell and Schachter 1989, 504–10. The derivation of a major or minor triad on the seventh degree of major as well as the Phrygian II is ambiguous in both Brown 2005, 43–5 and in Schenker’s writings. See the discussion in Proctor (forthcoming).
might be assigned as mediated relationships. They then methodically analyze a series of compositional segments ranging historically from Beethoven to Webern in which there are examples of potentially direct relations between $IV$ (or $V$) chords and the tonic in force.

They find support for their hypothesis throughout, with greater difficulty—to be sure and as intended—as the styles of music proceed to the limits of tonality. They find this support even in the pieces that lie outside their version of Schenker-tonality. That is to say, the $IV$ or $V$ represents tonal disjunction both within tonality (where it is necessarily mediated) and in music outside their tonal system (where it is overtly affirmed).

This, then, is an example of empirical testing of a theory.

Is their program open to critique? Of course. Much of their investigation and subsequent categorization, for example, seems to rest partly on a too-vague appreciation of Schenker’s style of Roman numeration. Much is made of Schenker’s use of “$IV$” instead of secondary dominant notation—a notation which he did not have. He would not bother to indicate a *Hilfsakkord* as a modulation, except in an exemplary way, as in the chart from *Harmonielehre* cited by Brown et al (Brown, Dempster, and Headlam 1997, 165). As a rule, Schenker notates secondary harmonies as alterations of diatonic harmonies ($II$, for instance, rather than $V/V$), so that the difference between an alteration by tonicization and an alteration by primary or secondary mixture cannot be ascertained from the Roman numeral alone. The authors’ treatment of Schenker’s analytic notation in one place also occasions a distortion, when they assert, with respect to Schenker’s graph of the third movement of Beethoven’s Piano Sonata Opus 26/3, that a VII of $V$ is allowed “to support $\bar{3}$ of the *Urlinie.*”

The middleground graph at issue has the diagonal line connecting the first structural bass tone $A^{\flat}$ (I) to the C5 (the $\bar{3}$), always used to show support at a prior level. Schenker is indicating that this C5 is supported by $A^{\flat}$, but that the initial arpeggiation at a subsequent level has moved the C into association with a subsidiary harmony. These are notational quibbles whose bearing on the authors’ theory is simply to proliferate facts where such proliferation is superfluous.

Let us instead turn to a few items confined to systematics, as presented in their “Scientific Image.”

**Laws of tonality.** The “scientific” nature of the entire enterprise is to apply “law-like” statements to the core (Schenkerian) theory as purportedly elucidated by Schenkerian transformations (Brown and Dempster 1989, 90, Table 5b), but their “Six General Laws of Tonality” leave scientific precision to be supplied by the reader (Brown and Dempster 1989, 91, Table 5c). These laws are unusable as originally articulated, and Brown has recently revised, expanded, and elaborated on them under the guidance of Schenker’s understanding of strict counterpoint (Brown 2005, 25–65).

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28 Brown, Dempster, and Headlam 1997, 166; see also Example 1, 167.
**Phrygian II.** Brown and Dempster are careful to include the Phrygian II under the “mixture” transformation, following Schenker (Brown and Dempster 1989, 90, Table 5b). But the extension of mixture from two modes—major and minor; to two and a half modes—major, minor, and one aspect of Phrygian—is proliferative. Allen Forte long ago made a convincing case, adumbrated in Schenker’s Harmonielehre and evident in Schoenberg’s chart of the regions, that the Phrygian II world can be derived from the subdominant side of minor. Reducing the Phrygian II to a kind of microtonicization would confine mixture to only major and minor. (At least one of their analyses seems to need the $\beta$II as a direct relation, so they might be resistant to this adjustment, but it is not obvious to us that anything in the test of their theory actually depends on $\beta$II as Phrygian mixture. While opening another gap in direct relation to the tonic, that between scale degrees one and two in both major and minor, this subordination of the Neapolitan to the subdominant function is exactly as distant from the tonic triad as is the dominant of V, as displayed most efficiently in Hauptmann’s system of key relations.)

**Enharmonics.** Their theory makes no mention of enharmonic relations, and neither distinguishes between enharmonic re-notation (equality) and enharmonic shift of diatonic reference (equivalence), nor the relation between enharmonicism in diatonic space and in saturated chromatic or 12-gamut space. Yet enharmonicism is invoked in most of their analyses as *explanatory*. It one case—an analysis of Chopin’s Etude in F Major, op. 25, no. 3, reproduced here as our Examples 1a and 1b—an analysis by Salzer that displays a direct $\text{♭IV}$ relation (and therefore places this piece outside their Schenker-tonality) is rejected in favor of one that subordinates one of Salzer’s structural boundaries to a larger cycle of “minor thirds.”29 But there is an augmented second, not a minor third, between A♭ and B♭, if we are to accept that we will return to F and not G♭.30

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29 This is originally Example 3a (from Salzer 1973, 285) and Example 3b (Brown, Dempster, and Headlam 1997, 171).
30 For a discussion of issues related to “symmetrical” divisions in diatonic space, see Proctor 1978 and Cohn 1996 and 2004.
What Kind of Theory Is Music Theory?

Example 1a. Salzer’s analysis of Chopin, Etude in F major, op. 25, no. 3, mm. 17–48.

Example 1b. Brown, Demster, and Headlam’s analysis of the same segment.

In a classical diatonic tonal world, with mixture and tonicization, there are necessary distinctions between chromatic equivalences, to the degree that there are infinitely many chromatic notes. But if we are to accept unbridled enharmonicism, then we are forced to consider a chromatic pitch-class system for tonality. We might then wonder why we care so much that chords are spelled a certain way; why we prefer to see $\flat\text{VI}$ as mixture but $\natural\text{V}$ as tonicization, since there can be no intrinsic difference between them if $A\flat$ to $B\natural$ is no different from a minor third.

Finally, Brown and Dempster’s theory makes no overt provision for chromatic passing tones, but several of the test analyses absolutely depend on the concept.
Nineteenth-century chromatic tonality. The theory of nineteenth-century chromatic tonality makes the distinctions called for in our discussion. It reflects a 12-gamut transformation of the type both Salzer and Brown *et al* call for within the overall umbrella of Schenker tonality without recourse to undefined enharmonicism, as depicted in Figure 3.

![Diagram of 12-gamut late middleground structure](image)

Figure 3. Model of nineteenth-century chromatic tonality.

Nineteenth-century chromatic tonality accepts diatonic material as infinitely extensible through ramification. If the space is diatonic, then equal temperament is a convenience that has no effect on the conceptualization of that space as the nesting of diatonic steps. Mixture (except double mixture, which, along with Cohn and others, we reject as explanation) and tonicization participate in this ramification and in (sometimes alternatively) defining the nesting; that is, which diatonic pattern controls which other patterns. It agrees with (or is allowed to agree with) the Schenkerian project, therefore.

There are two differences: 1) the invocation of an alternate space based on 12-to-the-octave pitch-class universe (the “12-gamut”); and 2) the expansion of operations to include transposition. As an example of (1), there is allowance for the choice of harmonic generator (usually the root of a triad, sixth, or seventh chord) through invocation of the 12-gamut directly; that is, without recourse to diatonic mixture, tonicization, or whatever form might be construed to eventuate from diatonic complexes. Thus, the place between E and F might possibly be neither E nor F but simply the diatonically undefined 12-gamut point equidistant between the diatonically determined E and F. With respect to (2), it is admitted that cases arise in which voice leading in either Rameau’s abstract idealized sense or in any of the more modern senses (down to part-writing), does not illuminate the phenomena in the face of wholesale shifts of chords from one place to another (in whatever space is invoked) without alteration of voicing. This leads to the addition of *transposition* as an operation, added to the essential four of passing, neighbor, suspension, and anticipation. It should further be noted that either the first or
second of these special conditions, 12-gamut space or transposition, can and do occur independently of one another.

Under this theory, both the Salzer and the Brown et al analyses are equally legitimate alternatives (if the invocation of diatonic origins is ignored), but Salzer’s directly invokes chromatic tonality. Several other of the test analyses by Brown et al fit easily into chromatic tonality, including those that call for chromatic passing tones and the reinterpretation of a dominant-seventh chord type as an augmented sixth chord. This makes a clear distinction between diatonic and chromatic tonality, but minimizes a feature prominent in the Brown et al discussion: that tonality can be traced on a continuum from clear to opaque; that there is such a concept as “the fringes of tonality.”

It is worth noting that to accommodate their model, Brown et al reduce out different elements from those reduced out by Salzer (as shown previously in Example 1).31 An obvious instance is the B chord in bar 29 of their graph, which is absent from Salzer’s graph. Similarly, Salzer includes a G major chord in bar 24, which is nowhere to be found in the Brown et al graph (compare Examples 1a and 1b). We again approach a central issue to science as it is really practiced as opposed to how it is discussed: one’s theory draws out relationships and conceals others, based not only on the theory, but on the background information that accompanies the theory. Brown et al wish to assume Chopin’s Etude as diatonically tonal in their terms, so they elevate data that fits that assumption; Salzer is perfectly willing to find 12-gamut matter and so discovers a symmetrical division of the octave and parallel voice leading in root-position triads.

Thus Brown, Dempster, and Headlam have taken an articulated theory, produced a hypothesis, and tested it. We have provided sample criticisms using terms that we take to be in common with them, and proposed that some of their analyses would be more simply addressed through a different articulated theory. Does one theory have an advantage over the other? It seems to depend on what you wish to demonstrate, explain, or predict in the first place.

That’s science.

In arguing that a “scientific” program for tonal music is possible, and then in arguing that a test is possible and successful, Brown and Dempster suggest, if not outright assert, that what music theorists have been doing is not scientific.32 This may be as much to intimidate as to enlighten, a style more

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31 This is not to label Brown et al as “reductionists”; we are simply labeling the direction of motion from foreground to background.

32 Brown comments that part of the impetus behind their work was “[t]o quell fears that Schenkerian theory is unfalsifiable” (Brown 1996, [15]). Later, Brown (2006, 17) concedes that “Popper’s claims that falsifiability provides us with a definitive means of discriminating science from non-science seems … too strong.” In general, Popperian falsifiability, which addresses the structure of a theory directly, is hard—if not impossible—to visit upon real-world
natural to British parliamentary debate and perhaps philosophy itself, where arguing over form can seem to be the entire enterprise; perhaps it reflects Popper’s comments about the need for a little dogmatism in order to establish a theory (Stokes 1998). Nevertheless, music is rare among the world of intellectual creations in having explanatory theories addressing an astounding range of issues, theories which are predominantly successful, which have been so for thousands of years, which are and have been as scientific as any other explanatory systems, and which thus did not need the appearance of twentieth-century German philosophers of science to become adequate to the needs of musicians.

In seriously complex circumstances, to be sure, where parts of a theory are not well-defined, it becomes hard to resist the suspicion of “pseudo-science” when that theory is unsatisfactory. The theory may have turned into pseudo-science, or we may want different information, or we cannot see how to clarify the theory. In all these cases, we move on to other theories. When we do move on, we may forever assert the superiority of our new theory for the questions it answers for us, but that does not legitimate our intruding it upon the other theory, which we have already found intractable. It also does not provide disproof of the other. One theory can not disprove another theory; it can displace it.

Conclusions

We have recounted some of the issues involved when importing “the scientific” into music theory, especially when “the scientific” is held to be something inherently different from music theory as commonly practiced. As can happen with any interdisciplinary transfer, there are not only fundamental misunderstandings about what “scientific” theories are considered to be in the field of philosophy of science, but also how elements of these theories might transfer to music theory; how empirical testing might be carried out; and perhaps most fundamentally, what types of questions are being asked and answered. We have also appealed to contemporary philosophy of science to break down the distinction between what is construed to be scientific language and what is construed to be metaphoric language. Scientific language is inherently metaphoric; music analytic language is inherently metaphoric. As Guck has proposed, we agree that when done well, there is no difference of rigor between these two stances; both can meet the criteria of replication and communicability. Cook goes as far as to assert that scientific theories. When error is detected in scientific practice, it is far more likely to be found in the ways in which the facts used by the theory are related to the phenomena it hopes to explain or predict.
[t]he discursive structure of a natural-law explanation [for example] and of Guck’s narrative fiction is essentially the same: music is assimilated to generalized model within some kind of regulatory framework … Their performative effect, their impact on perception or belief, remains the same (Cook 2002, 98).

(This is not to say that the information yielded is necessarily the same; as noted previously, theoretical frameworks in many respects determine observation.) This is often explicitly acknowledged in some fields. For example, Shapere points out that “scientific” theories have been involved in narrative explanations in connection with Big Bang cosmology, thus closing the gap between physical and historical modes of explanation (Shapere 1988, 208).

While we agree with Cook that the precision and rigor is potentially the same, we are less optimistic about the equality of their impact on perception or belief, especially when we move beyond the individual to the discipline as a whole, and eventually beyond the discipline. We return to our opening point: whether done well or done poorly (and many people, including “scientists,” have difficulties discerning this distinction), scientific discourse is commonly assumed to have a higher truth-value than “non-scientific” discourse. Thus, insisting only on “scientific” language or “scientific” methodology to the exclusion of what is then termed to be “merely” metaphoric language (or methodological mediocrity, or lack of empirical verification, and so on) is a political act, with consequences that reach far beyond mere methodological quibbling.

Here we insert a few cautionary tales.

Beginning in 2000, the QuesTec Umpire Information System has been installed at eleven major-league baseball parks. This system consists of software that merges the data from two cameras high in the stands which record the horizontal position of the ball when it crosses home plate, with the data from two cameras at ground level which record the height of the pitch as measured against the stance of the batter. A technician at the game sets the batter’s strike zone. QuesTec was originally sold to major league ballparks as a training tool for umpires. Umpires received a CD of each game, with which they could review their calls compared to QuesTec’s calls. It was simply “more information” from a different source in order to augment precision.

At least, it was originally intended to be simply a source of convergent evidence, until major league baseball started using the QuesTec Umpire Information System to evaluate umpire’s performance, and thus to determine who was assigned to postseason play.33 Now the umpires are objecting to its use. This could all be sour grapes, except that Robert Adair, a physicist who is consulting for the umpires (and the author of The Physics of Baseball)
Playing the “Science Card”

contends that the science is not that simple; the umpires are claiming that the QuesTec system is wrong on 80% of the disputed pitches.34

Why, given the uncertain status of the technology and the difficulty of the science involved with the physics of baseball, would the major leagues shift from treating the QuesTec system as a “training” tool to using it as a punitive device in only two seasons? Major league baseball’s executive vice president for baseball operations, Sandy Alderson, says that it is fueled by television broadcasters who have overhead cameras and slow motion replays; using this as “evidence,” they second-guess the umpires on the air. This, says Alderson, “can be a source of embarrassment that leads to a loss of credibility” (Starr 2002, 62). In other words, even if the evidence used by the broadcasters to make on-air judgments is suspect (after all, seeing is believing), and the QuesTec system is itself suspect, it is more important to create a public perception of accountability and accuracy based on the status of “scientific” methods than to be embarrassed by the faulty methodology of broadcasters. “Science” is used for political purposes, here for profit, both on the part of QuesTec and major league baseball.

This is an amusing example; and after all, one could argue that baseball is entertainment for profit, and should not be held to the same standards as is scholarship. (As baseball fans, we would disagree.) As in baseball, in music theory it is easy to think that science/discovery/theory drives technology. But as Judith Lochhead (1998) has pointed out, the reverse is true: technology usually drives theory. In some music circles this may mean that a theory is suspect if it is not fully realizable (operationalizable) via computer modeling. (This disregards that computers might be a bit behind the human mind when it comes to conceptualization and musical meaning. In many cases, it also disregards that the mind does not use propositional logic.)

Or consider the article published in a reputable biology journal, which used genome analysis to try to understand the relationship between Macedonians and the other peoples of the Mediterranean (Arnaiz-Villena et al 2001). Nathaniel Knight points out:

It was striking … that no other Slavic peoples were included in the study … for example, the relationship between Macedonians and Bulgarians was not addressed at all. Instead, the article focused on Macedonians in relation to Greeks. The authors … implied, with absolutely no supporting evidence or discussion that the Macedonians of today are the direct descendants of Macedonians from the era of Alexander the Great. The implication appeared to be that this study of contemporary populations would shed light on the question

34 Umpires are not the only one unhappy with QuesTec; in May 2002, Arizona Diamondbacks pitcher Curt Schilling was fined about $15,000 for smashing a QuesTec camera with a bat. Other pitchers argue that anxiety over QuesTec has caused umpires to call more balls, leading to more pitches per game and especially damaging “finesse” pitchers. The use of QuesTec has also fueled numerous conspiracy theories on the web that allege fixed data, thrown games by umpires, and the like.
of whether Alexander was himself in any way Greek. Naturally, it turned out that the Macedonians are genetically light years apart from the Greeks (as was, apparently, Alexander as well). In fact, while the Macedonians were closely related to the indigenous [sic!] peoples of Europe such as the Italians, French, Spanish, and Basques, the Greeks were in a completely different category. The only people who came close to matching their genetic profile were certain tribes of Sub-Saharan Africa.

I could only conclude that the tool of population genetics was being usurped for the purpose of striking another blow in the ongoing cycle of ethnic hatred between Macedonians and Greeks. Sure enough, when I did some poking around the Internet, I found that the study was prominently featured on Macedonian extremist websites (including one that began with a long list of “traitors” who deserved to die) (Knight 2002).

Lest we think that only fields like biology could be usurped, Cook points out that music is not exempt: “One might say that there was a fifty-year hiatus in systematic musicology as a result of the manner in which the comparative project was hijacked by the totalitarian regimes of 1930s–40s Europe, with the idea of the ‘essential attributes of humanity’ being glossed in racial terms,” especially to serve the Third Reich’s racial ideology (Cook 2006, 7).

Given the serious potholes encountered, does this mean that we should not reach into disciplines traditionally construed as “scientific” for inspiration? Hardly. Diverse methods yield rich results. Cook has gone so far as to assert that epistemological pluralism is one of music theory’s defining characteristics (Cook 2002, 102). The language of science and of the philosophy of science has fertilized our field at various times across its history. This is appropriate insofar as what musicologists do is indeed science, and every now and then we need to be reminded by someone such as Babbitt that we therefore need to be scientifically responsible. Babbitt was directly, especially, and overtly concerned with the negative effect on some members of the music community resulting from wrongly-conceived language. Forty years ago, he was concerned with the politics of musicographical language—language which, at the time, was being used to prevent Babbitt’s compositions from being performed. Now, we are looking at apparent greater precision being offered on one side together with what seems to be its complement on the other side, both directed at those in the middle. That is to say, we are again facing the politics of musicographical language.

But we are in fact all using language, our own languages, with precision. In the light of the philosophy of science, we find nothing more or less scientific in all the linguistic conventions of all the scientists engaged in the field of music theory. To argue for a certain kind of language (or methodology, or epistemology) at the exclusion of all others—based on the received view that one is more “scientific”—is campaigning for one approach as superior to another. Would philosophers of science recognize that cognition researchers are doing science? Yes. Would they recognize that ethnomusicologists and systematic musicologists are doing science? Yes, and yes. Are theorists and
New Musicologists? Again, yes. To suggest that some theorists are doing (scientifically) legitimate work and others are not is a political act.

What are the costs of choosing a metaphoric path, for example, over an allegedly more scientific path? One writer has conjectured that the only alternative is for music analysts and theorists to be considered “really fiction writers in drag” (Huron 1995, 480). We do not personally object to that label for ourselves—as if that were a bad thing!—but given the many stereotypes of effeminate musicians, the erroneous but common equation of transvestitism with homosexuality, and the low percentage of women in the field of music theory, this is quite a charge. And the benefits of explicitly adopting a consciously “scientific” apparatus are readily apparent, especially to those who work at universities that reside in the “second tier” of any national ranking system, whether it be the NRC or *US News and World Reports* or *MacLeans*. Overtly scientific scholarship is something administrators understand, and can point to. It can be represented quantitatively. It is clear to all, especially those outside the field, including donors and granting agencies, that it is “real” research.

Each choice of language, methodology, epistemology, will yield different information because of its different theoretical underpinnings; but they all *have* theoretical underpinnings. If the choice between theoretical underpinnings is based on personal preference, by consensus of a community of scholars, by historical precedence, by utility of representation, so much the better; convergent evidence is a good thing. But when it ceases to be simply more information that enters into dialogue with information gathered by using other methodologies, and becomes the only information deemed valid or valuable, and especially when it becomes used as the only possible yardstick of “real” knowledge, it becomes problematic. When the choice of language, methodology, or epistemological stance is cast as a choice between “rigorous,” “real” science on the one hand, and the “methodological mediocrity” of “pseudo-science” on the other hand; or if the pretense of “scientific rigor” is used to destroy the straw man of a misunderstood theory; then we are indeed playing the science card. In this case, an appeal to “science” then becomes *not* an appeal to a particular methodology, theoretical construction, rules of replication, and so on, but simply becomes a transparent cloak for what is really an *argumentum ab auctoritate*. 
References


Playing the “Science Card”


“Initial Conditions”
Problems of Scope and Cause
in Music-Analytical Claims

Stephen Peles

Apparently there is more to worry about than I had thought.
I had thought that if what I claim is both true and novel then I have, however modestly—solely in virtue of the truth and novelty of my claim—advanced human knowledge and honorably discharged my responsibilities as a theorist. It isn’t, after all, as if either truth or novelty are just there for the picking. One needn’t spend much time perusing the professional literature of music theory to figure out that managing either is pretty hard work.

There are some, however, who insist that truth and novelty are not enough, and that there is a more exalted realm to which we should all aspire if we wish to be taken seriously: the realm of scientific truth. Mind, I am yet to encounter a scientist making this demand of music theory. The position in question, rather, is more typically adopted by one of our own or by a member of a sister discipline such as musicology. While I suppose the question of the relation between music theory and science has been around as long as music theory and science themselves, the particular form in which it is apt to be cast these days, in the English-speaking world at least, seems to date from the early sixties, when Arthur Mendel (Mendel 1961) introduced many in the musical community to Carl G. Hempel’s “deductive-nomological” model of scientific explanation. Since then, whenever the question arises it is almost invariably limited to a consideration of whether music-theoretical “explanations” fit the model proposed by Hempel. My colleagues in the philosophy department would no doubt be deeply amused to learn this, since I doubt that there is a philosopher alive today who thinks Hempel’s work of more than half a century ago is representative of the current state of the art.

Nonetheless it is true that Hempel was an immensely important figure in the history of his discipline, so in this respect it is not surprising that if a music theorist has heard of any philosopher of science they will have heard of Hempel, whose work in one respect or another has set the agenda for philosophical accounts of the scientific enterprise for over fifty years. Indeed, Hempel’s work is widely held by historians to mark the beginning of modern
philosophy of explanation; Wesley Salmon, whose book *Four Decades of Scientific Explanation* (Salmon 1990) documents the recent history of that field, rightly regards all philosophical work on scientific explanation preceding that of Hempel as belonging to the “prehistory” of the discipline. What Hempel did was make explicit a long-running consensus view on how explanations in the sciences are constructed, an explication first presented in his 1948 essay, co-authored with Paul Oppenheim, entitled “Aspects of Scientific Explanation.” In doing so he performed the immeasurably valuable service of providing a sufficiently well-defined portrait of what came to be known as “the received view” of scientific explanation that we could begin to see precisely why that view was flawed. And a consensus has emerged in the decades since that it was flawed. What perhaps has not emerged has been a similar consensus on the nature of its flaws. This too counts as progress in my book: one of the most important parts of Hempel’s legacy has been the laying bare of previously unacknowledged differences in our views of what counts as “science” and “explanation.” I also happen to think that Hempel got quite a lot right, though to appreciate that one needs to know what it was Hempel set out to do. So a brief review is in order.

Hempel was a German physicist and latterly philosopher who came of professional age at a time when challenges to classical physics from relativity and quantum theory had forced the issues of theory choice and the conditions under which a theory could properly be said to compel belief (or at least acceptance) to the front of the line of philosophical questions that urgently needed answers. Closely associated with the work of the Vienna circle and the Berlin group, Hempel’s position has clear ancestors in Hume and Mill as well, though it is set forth in vastly greater detail and with immense sophistication.

In the spirit of the Vienna circle and the Berlin group, what Hempel wanted to do was explicate the notion of “scientific explanation” in the form of a “model” of what he took to be its defining logic. More than one model, in fact, depending upon whether the logic in question was deductive or inductive, and whether the fact to be explained was singular or not.¹ But all shared the same general assumptions and structure: an explanation is an argument whose conclusion follows from its premises (viz., has to be true given the premises) and is the thing being explained. Hempel’s model was intended to characterize what became known as “the received view” of sci-

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¹ Hempel acknowledged other sorts of useful scientific discourse that nonetheless falls short of achieving the status of an explanation such as “explanation sketches” and various forms of “analysis.” Thus, for example, Hempel did not speak of functional explanation but only of functional “analysis,” the latter being a sort of stage of theory construction appropriate when not enough is yet known to construct an explanation proper.
tentific explanation. In this respect it was successful: it accurately portrayed what a lot of people believed to be the way science works.

Example 1 shows Hempel’s original model of deductive-nomological explanation in schematic form (Hempel and Oppenheim 1948). The premises are of two sorts: a set of initial conditions and a set of natural laws. In addition Hempel imposed four conditions which must be met if this schema is to model a valid scientific explanation: (1) it must represent a valid deductive argument; (2) it must contain at least one “general law of nature” (so the set $L$ cannot be empty); (3) it must have empirical content (that is, it must be possible in principle for an observation-sentence to contradict it); and (4) the premises in the explanans must all be true. So, if you want to know why a particular ice cube melted in a particular case, example 2 shows a possible model D–N explanation.

More compactly, the model may take the form of a conditional: “if $C$ and $L$ then $E$,” and there can be no doubt that some scientific explanations can take this form. But it has become clear over the years that (1) not all can, and (2) not everything that conforms to the model is an explanation, scientific or otherwise. The literature of the last half-century or so which uncovered these well-known defects in the D–N model is immense. I’ll confine myself to a few canonical counter-examples, which are most relevant to this essay.2

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2 Those in the market for a summary of the debate through 1989 should consult Salmon 1990.
Perhaps most celebrated is the flagpole example.\(^3\) Let \(C\) be a collection of empirically true statements specifying the physical dimensions of a particular flagpole, its position in physical space and time, and the position of the sun at the same point in time; let \(L\) be a true statement of the laws governing the propagation of light; let \(E\) be the location and dimensions of a particular shadow in space and time. Given \(C\) and \(L\) we can compute \(E\). So we have an explanation it, would seem, of why there is a shadow of particular dimensions at a particular place and time: the conjunction of \(C\) and \(L\) explains the shadow. The problem is we can swap elements of \(E\) and \(C\) and still get a valid deductive argument with a true conclusion. That is, given the position and dimensions of the shadow (now part of \(C\)) and the laws governing the propagation of light (still \(L\)) we can compute the height of the flagpole (now \(E\)). But it is surely incorrect to say that the shadow explains the height of the flagpole in the same sense that the height of the flagpole explains the shadow.

Or consider the relation between the behavior of a barometer and the subsequent occurrence of a storm (Salmon 1971). There is an inductively derived and highly reliable relation between falling barometer readings and the onset of storms. While it makes sense (more strongly, it’s true) to say that falling barometer readings have predictive value (viz., they are reliably correlated with the onset of storms) it makes no sense (more strongly, it’s false) to say that falling barometer readings explain storms.

Then there’s the problem of the failure of men who take birth control pills to become pregnant (ibid.). Let \(C\) contain the true statement that Bill regularly takes his wife’s birth control pills. Let \(L\) be the law (or at least the exceptionless truth) that no man who takes birth control pills becomes pregnant. Let \(E\) be the empirically true observation-sentence asserting that Bill did not become pregnant. So we have nothing but true premises and we have a valid deductive argument leading to a true conclusion. But it is patently not the case that the argument explains why Bill didn’t get pregnant.

What these examples show is that the original D–N model fails to distinguish explanations from non-explanations in that it fails to capture essential asymmetries between explanans and explanandum and because it is indifferent to the relevance or otherwise of premises so long as they are true. It has been noticed that one particular form of explanation isn’t vulnerable to the asymmetry and relevance problems: causal explanation. Thus, the reason for the asymmetry in the flagpole problem is that it makes sense to say that the flagpole is a cause of the shadow (flagpoles are the kind of thing that can cause shadows) but not that the shadow is the cause of the flagpole (they’re not the kind of thing that can). Similarly, neither a falling barometer nor an approaching storm can be said to cause the other (though a different sort of

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\(^3\) The example is due to Sylvain Bromberger, though never published by him in this form. See, however, Bromberger 1966.
causal explanation would presumably explain the correlation between the two). And both the fact that Bill took birth control pills and the law that men who take birth control pills never get pregnant are causally irrelevant to the state of affairs wanting explanation (Bill’s state of non-pregnancy).

Without doubt some explanations in the sciences are causal: the explanation-seeking “why”-question “why X?” is, at least some of the time, a request for the cause of X. Hempel acknowledged this, though he denied that all scientific explanation is causal. The issue is still a matter of dispute. In what follows I shall focus on causal explanation, leaving open the question of whether there are explanations of other sorts.

Apart from all this, is there anything in the model that distinguishes valid scientific explanations from valid explanations of other sorts? If so, it can only be the requirement that the explanation include an appeal to one or more natural laws; that requirement is so important in distinguishing the D–N model from certain of its competitors that these days it’s more often known as the “covering law model,” a coinage owed to the philosopher of history William Dray. This certainly comports well with one common notion of “science”: the purpose of science is (in more extreme form, is only) the discovery of natural laws, with the main virtue of discovering such laws residing in the role they play in reliable prediction. And to be sure, Hempel was committed to the view (widely disputed it must be added) that any valid scientific explanation can serve as a prediction. It’s worth noting that this definition of science, perhaps the majority view of Hempel’s generation of physicists, has not always been the dominant one even among scientists, but rather reflected the historical state of the sciences at the time: since the eighteenth century the most celebrated examples of scientific progress were, arguably, the discovery of laws, and by the late nineteenth century the view that physics was almost complete (viz., that there were few laws left to discover) was by no means eccentric, even among professional physicists. The principle competition for science-as-law-discovery is the broader view that the task of science is to increase our knowledge of the natural world (which may include the discovery of laws but is not limited to it). We will revisit the troubled recent history of the concept of natural law later.

Next we need to get clearer on what Hempel’s D–N model was a model of. We start by noticing some things it wasn’t. It wasn’t an account of confirmation (and thus differs essentially from Popper’s otherwise formally similar hypothetical-deductive model). It wasn’t offered as an account of the pragmatics of science: it’s not a story about how scientists go about their business. And it emphatically wasn’t a discovery procedure: the truth of both the premises and the conclusion are presumed to be known antecedently. Rather, a Hempelian explanation is what you end up with when you’re done: it shows how propositions antecedently known to be true relate to one another through the relation of entailment. In this respect it can be understood as a model of what a hypothetically complete science would look like.
What Kind of Theory Is Music Theory?

That this was close to how Hempel himself understood the model became increasingly clear in his publications subsequent to the original 1948 article, emerges particularly clearly in his discussions of the knotty problem of \textit{ceteris paribus} clauses and “provisos,” and was reflected in his growing sense that there might not be any strict laws (\textit{viz.}, laws that are so absolutely exceptionless that they require no hedging by \textit{ceteris paribus} clauses and assume no provisos). For Hempel this meant that it’s possible that no explanation is complete. Here is Hempel commenting in 1988 on the nature of provisos (roughly, unstated assumptions that are nonetheless necessary for an inference to be valid).

Thus, if a theory is to be thought of as a calculating instrument that generates new … sentences from given ones, then it must be conceived as supplemented by an instruction booklet that says: “This instrument should be used only in cases in which certain provisos are satisfied, namely, the assumption that no disturbing factors of certain kinds are present.” … Provisos might rather be viewed as \textit{assumptions of completeness} (Hempel 2001 [1988], 243–4, emphasis in original).

And here he is in 1963, commenting on what he meant by “model.”

The term “model” can serve as a useful reminder that the two types of explanation as characterized [i.e., deductive-nomological and probabilistic-statistical] above constitute ideal types or theoretical idealizations and are not intended to reflect the manner in which working scientists actually formulate their explanatory accounts … the models exhibit the rationale and logical structure of the explanations they are intended to represent (Hempel 2001 [1963], 282).

I’m not sure that the specter of irremediable incompleteness made Hempel happy, but I suspect that something like this is true. That is, I suspect that explanation isn’t something you either have or you don’t but rather is something you can always have either more or less of. Thus a “scientific” explanation differs from other (presumably true) explanations only in virtue of its offering a larger than average serving of relevant explanatory information, where the “average” is always somewhat ill-defined since it varies depending upon context, interest, and the like. I further suspect that this is why no one agrees on how properly to distinguish the two: there’s no natural joint at which to carve since the difference between the two is one of degree, not one of kind. And if that’s the case then any plausible definition of what it means to be “scientific” will need to be formulated at least partly in terms of the history and sociology of science, not cast in solely epistemic terms.\footnote{Which is simply to say that there is no principled way of disuniting the set of true propositions into a set of those which are “simply” true and a set of those which are, in addition, \textit{scientifically} true. That doesn’t mean that there might not be something worth calling, say, the “scientific attitude,” which consists in following the evidence where it leads, independently of}
Hempel’s talk of “ideal types” might have alerted his American colleagues to the possibility that he wasn’t necessarily talking about exactly the same thing they were talking about, or at least not in the same way. But Hempel’s reception was hampered, I suspect, by something of a culture clash: the *Gedankenexperiment* of imagining the likely nature of a hypothetically complete science was a kind reasoning with a long history in German philosophy and was far more familiar to Hempel’s European colleagues as a genre of discourse than to native English-speaking philosophers.\(^5\) Hempel’s teacher, Hans Reichenbach (who suggested that a distinction between the “context of discovery” and the “context of justification” was the best way to make sense of Carnap’s notion of a “rational reconstruction”) would not have had much difficulty understanding what was being proposed.

II

A hypothetically complete science, then, might be understood as a network of infinitely many true propositions (some of which may or may not be “laws of nature”) along with the infinitely many relations between them; at any point in time the actual state of a science consists of some subset of this, and the actual state differs from the ideal state in virtue of its incompleteness. Hempel’s model, then, may be understood as providing one particular slice of this network, the one where the relation is that of entailment.\(^6\) If we count causal explanation as a subset (whether proper or improper depends of course on whether you think there are other kinds) of deductive explanation then it constitutes a slice of Hempel’s slice.

It’s still a pretty big slice, though. David Lewis was on target when he remarked that “[w]e might imagine a world where causal histories are short and simple; but in the world as we know it, the only question is whether they are infinite or merely enormous” (Lewis 1986, 214). Lewis’s notion of “explanation” is simple: “To explain an event is to provide some information about its causal history” (Lewin 1986, 217). Since that history is either infin-

the pursuit of other perhaps equally attractive goals. We adopt the attitude, for example, when we pursue truth even at the expense of personal happiness.

\(^5\) While much work remains to be done on the European historical contexts within which logical positivism and logical empiricism arose, there are some encouraging early signs. Friedman 1999 provides much useful documentation regarding the relevant political and cultural milieu, and begins to reconnect these philosophers to the peculiarly European debates in which they took part. On the difficulties American and European philosophers had to overcome to understand one another, the Carnap/Quine correspondence is a telling case study (Creath 1990). On Hempel in particular, Fetzer 2000 provides much useful information and includes an interview with Hempel.

\(^6\) If anything remotely like this is accurate, it should give pause to anyone who thinks that some things are *essentially* descriptions and some other things are *essentially* explanations, since this makes the distinction relational. Put differently, a single proposition may relate to some other propositions as an entailment (viz., as *explanans*) and to some other propositions as entailer (viz., as *explanandum*). Put as a slogan: it’s *all* description.
nite or enormous, any individual act of explaining will of necessity provide only a portion. This is why, asked to explain why a particular drunk driver crashed his car into a particular tree while driving on a particular wet road, a physiologist and a physicist might give different answers but each be valid; each is concerned with that portion of the complete causal history relevant to their discipline and describable in that discipline’s proprietary language. But both (necessarily incomplete but arguably correct) explanations could, Lewis notes, be offered in deductive form.

In short, if explanatory information is information about causal histories, as I say it is, then one way to provide it is by means of D–N arguments. Moreover, under the hypothesis just advanced, there is no explanatory information that could not in principle be provided in that way. To that extent the covering-law model is dead right. But even when we acknowledge the need to distinguish explanatory D–N arguments from others, perhaps by means of explicitly causal constraints, there is something else wrong. It is this. The D–N argument—correct, explanatory, and fully explicit—is represented as the ideal serving of explanatory information. It is the right shape and the right size. It is enough, anything less is not enough, and anything more is more than enough. Nobody thinks that real-life explainers commonly serve up full D–N arguments, which they hope are correct. We very seldom do. And we seldom could—it’s not just that we save our breath by leaving out the obvious parts. We don’t know enough. Just try it (Lewis 1986, 236).

Now, it happens that the aspect of the Hempelian tradition that deals with ideal types has, I think, a contemporary descendent in the form of the “unification theory” of explanation. On this account scientific explanation contributes to the growth of science by unifying the body of scientific knowledge through, roughly, decreasing the number of independent assumptions needed to explain events, typically through the discovery of laws that allow us to understand individual events as members of the kind to which the law applies (Newton’s laws being the most popular example); this as opposed to thinking of explanation as prototypically involved in the causal/mechanical explanation of individual events (see, e.g., Kitcher 1985 and 1989 [1985], and Salmon 1985). Thinking of the result of successful explanation as being an increase in knowledge leads to the happy possibility that both views (what Kitcher calls the “top-down” view of the unificationists and the “bottom-up” view of the causal/mechanical set) might be right, viz., each might be capturing a portion of the total picture (see, e.g., Salmon 1990, especially the final chapter, “Conclusion: Peaceful Coexistence?”), which is all one can ever hope for anyway.

Something like this has been suggested by Peter Railton (a student, as it happens, of David Lewis). Railton proposes that we understand the scientific enterprise in terms of what he calls the “ideal explanatory text,” some portion of which would contain all the information relevant to the explanation of a given event.
The full-blown causal account would extend, via various relations of reduction and supervenience, to all levels of analysis, i.e., the ideal text would be closed under relations of causal dependence, reduction, and supervenience. It would be the whole story concerning why the explanandum occurred, relative to a correct theory of the lawful dependencies of the world. … But it is clear that a whole range of less-than-ideal proffered explanations could more or less successfully convey information about such an ideal text and so be more or less successful explanations, even if not in D–N form. … the actual ideal is not to produce such texts, but to have the ability (in principle) to produce arbitrary parts of them. It is thus irrelevant whether individual scientists ever set out to fill in ideal texts as wholes … (Railton 1981, 247).

To summarize. I’m trying to sell a story in which the task of science is to fill in portions of an ideal explanatory text relevant to chosen explananda, and I want to focus on that portion of the text containing causal information (it’s irrelevant for our purposes whether this is the whole text or not, that is, whether or not there are non-causal explanations). I trust it’s clear that even a tiny bit of this portion of the text will be multidimensional in virtue of its containing the entire causal history of the event. That is, given a pair of causally related events, say $F \rightarrow G$ (we’ll assume for ease of exposition that the relation of causal dependence can be at least roughly modeled by the relation of entailment), the two are not only related to one another by the causal relation, they are related to some underlying mechanism which implements the causal relation and on which they supervene; causality is not, after all, just temporal precedence. And that mechanism will itself typically supervene on even lower-level mechanisms. So, for instance, if you ask a Boy Scout why two sticks are burning he might answer, truly, that he caused them to burn by vigorously rubbing them together. There’s more that could be told, of course, and how much more you want depends on why you want to know. For some purposes you might want to know something, say, about the relevant chemistry, such as the chemical principles involved (including, e.g., information regarding the susceptibility of wood to oxidation, the chemical makeup of these particular pieces of wood, and so forth), for other purposes you might need even more. And so it goes, all the way down to the atomic level at which point we would have to stop. How far “down,” as it were, you need to go is relative to your interest, and who you go to depends on that as well: chemistry and physics are not social categories, they differ because the entities with which a particular discipline concerns itself, the relations between them, or both, are proprietary to that discipline and are typically described by that discipline in its proprietary language.

III

We’re now in a position to ask the question that concerns me. Granting that some explanations are causal, that individual causal explanations are always
incomplete, and that the events involved are related both to one another by a precedence relation and to some implementing mechanism through supervenience, is it ever the case that music theorists construct causal explanations?

My answer is “yes,” though it’s not the only thing we do, nor do I think it should be. If you think of music theory as quantifying only over sounds, then the answer would mostly be “no.” It’s not that sounds don’t have causes, it’s that their causes aren’t music-theoretical causes. So, if you want to know what caused there to be a B-natural in the upper voice starting at the midpoint of the second measure of example 3 there’s a perfectly good and perfectly true causal explanation, it’s just not music-theoretical in any conceivable sense: the B is where it is because someone put it there thereby causing it to be there. Full stop. In most cases of western art music we can get more particular and name the composer. To be sure there’s more to say, but there’s always more to say. We could ask why someone put the note there, and go on to trace the causal history of the disposition toward that behavior. Assuming we could get an answer to each new question we could trace the etiology of that particular note back to the big bang and beyond, but if we had a time machine and followed along each step of the way we would encounter precious few music theorists en route.

The reason being that music theorists are not in general concerned with the causes of notes in pieces despite the fact that they’re often concerned with notes. If notes could cause other notes, things might be otherwise but they don’t. The reason notes don’t cause other notes is because they can’t. And that is because there is no possible world in which there is an implementing mechanism such that one ink blot could cause another: there’s nothing on which the causal relation could supervene. And the same is true if by “note” you’re imagining the sounds of a particular performance rather than the ink blots. So it would be false to the point of silliness to say, for example, that the cause of the B is the dissonant suspension that precedes it.

\[
\begin{align*}
\text{Example 3.}
\end{align*}
\]

So, if you think the domain of music theory is limited to acoustical events (and their representation, manipulation, properties, and so on) then you’d

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7. The point applies, of course, to any acoustical event conceived as a musical event, not just to those we conventionally refer to as notes; the latter are simply standing in for the former in the present case to ease the exposition.

8. It would be nice if this particular bit of silliness had been invented for the occasion, but I’m afraid the example is real. See Brown 2005, 10 where “causal connections between the suspension and the resolution” are claimed.
probably be right to say that you don’t make many causal claims in the course of your work. But you’d then have to face the problem of distinguishing music theory from acoustics.

But music analysis quantifies over two domains: acoustical events and mental events. And mental events can have causal histories that invite description in music-theoretical terms. An example is the sort of mental event reported by the informal utterance “the B sounds like a resolution.” Acoustical events may well be implicated in the etiology of the mental state reported, but the report is not itself a report of an acoustical event. There is no property “being a resolution” that would show up, say, in a sonogram of a performance of the passage anymore than the property of tonicity would. The Chomskyian “poverty of the stimulus” argument applies with full force here: there’s information in the response that isn’t present in the stimulus.

If we take the explanadum as the mental state reported by the claim that “the B sounds like a resolution,” it’s hard to imagine what kind of explanation other than a causal explanation might be required. Let \( E \) be the mental state that Jane is describing when she asserts that she hears the B as a resolution. From what sorts of premises would that follow deductively; viz., what are the initial conditions such that \( C \rightarrow E \)? The set \( C \) would have to contain true and relevant statements of propositions concerning both the acoustical signal and Jane herself, for reasons given a paragraph back. That is, the etiology of the mental state \( E \) includes both external stimuli as well as other mental events. All of this will sound familiar to some, no doubt. What we have here is a classic case of an intentional psychology (intentional because the mental states in question typically involve “hearing as”).

This is not uniformly good news, I must add. Intentional psychology is up to its neck in questions whose answers lie outside the known boundaries of the very small portion of the ideal explanatory text we’ve managed to construct to date. We can consider some of the most typical problems in the context of the present example. We know that \( E \) (though we don’t know why). Partition \( C \) into two sets. The first, \( C_m \), contains all and only those statements of true propositions concerning Jane’s other mental states which are part of the causal history of the mental state \( E \).\(^9\) The second, \( C_p \), contains a true description of the acoustical stimulus—to a first approximation, the information represented by the score in example 3. It’s pretty obvious that we are on much firmer ground when it comes to \( C_p \), since while we might not know precisely which of the acoustical phenomena are implicated in the

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\(^9\) Natural law fans should feel free to add the set \( L \) if they like. Laws are just more premises for present purposes, and it doesn’t matter to the current argument whether they’re included or not.

\(^{10}\) While I’m admittedly playing fast and loose with the undefined notion of a “mental state” it doesn’t matter at present since all we’re doing is isolating the organismic contribution to the causal history in question.
etiology of $E$, we know almost exactly what the possibilities are. We know comparatively nothing about what’s going on in Jane’s head.

But we wouldn’t know much at all without music theory, one task of which has been to specify which of the available variables are likely candidates for inclusion in $C$ in virtue of their playing plausible causal roles in the etiology of $E$. Let’s assume Jane has command of some of the relevant technical terminology. If asked to explicate the meaning of “sounds like a resolution” she might reply “it sounds superordinate to the C.” Further investigation would reveal that there’s a theoretically useful 2-partition that we might induce on the set of intervals such that some are strongly though not invariably correlated with the effect of subordination (call them “dissonances”) and the others are strongly though not invariably correlated with the effect of superordination (call them “consonances”) and that $ceteris paribus$ Jane tends to hear dissonant notes like the C as subordinate to step-related consonant notes that immediately follow them—she hears the B as a resolution $because$ it occurs in a context of the sort just described. We cash the $ceteris paribus$ clause precisely to the extent that we are able subsequently to add additional causally relevant details about C. At a certain point in tracing the causal history of $E$, the proprietary theoretical language of music theory will no longer apply and the task of describing the next level of implementing mechanism will appropriately fall to some other discipline (say, neurophysiology).

Now if Jane were the only person on the planet, life would be simpler (though we’d still face another complication that I’ll get to in a moment). But she’s not. And even with as simple an effect as the one involving the B heard as resolution, anyone who has ever taught a rank beginner knows that not everyone will experience the same effect as Jane. But knowing something about the causal history of the effect enables us to induce it in those who wouldn’t otherwise have the experience; it allows us, in short, to change the initial conditions. We start at the earliest point in the causal history to which the subject responds, and move incrementally up the chain from there, effectively reading the causal history forward $toward$ the effect, rather than backward $from$ it as we did when we were constructing an explanation. In this respect music theory has two tasks, one explanatory and the other didactic; mirror images of each other, they differ in the direction in which they read the relevant portion of the explanatory text. Schenker’s theory, which introduces low-level pitch and time relations through the study of counterpoint and low-level pitch-class $Stufen$ relations through the study of harmony and then teaches how they interact with each other and with other factors such as repetition when combined in free composition, and then reverses the direction when employed analytically, is exemplary in this respect. Indeed, Schenker’s theory taken in its entirety might best be described as specifying in considerable detail a hypothetically ideal listener (ideal from Schenker’s point of view, which is to say a listener interested in precisely and only what
interested Schenker, and with initial conditions adequate to the realization of the effects he cared about) along with a method for constructing it.

Now, the plasticity of our cognitive apparatus is such that to varying extents we can try on novel hearings for size, which is to say we can to some extent alter our own initial conditions. It is only this, of course, which makes the didactic task possible. But by the same token it poses a challenge to those in search of broad exceptionless generalizations. Generalizations must have a well-defined scope: they quantify over sets. In the simplest of cases (if indeed there are any) those sets are natural kinds and the generalizations are laws. The individuals that belong to the set share projectible properties that warrant inductive inferences from observed to unobserved members of the set. Given the lack of canonical effects, and the plasticity of C (Jane’s C might be different tomorrow than her C today, making her for purposes of generalization a different subject, so even if she was the only person on the planet we’d still have an epistemically moving target), it’s hard to see exactly what music-theoretically relevant predicate might serve to uniquely define such reference classes. Like most intentional psychologies, we haven’t yet figured out how to properly individuate mental states, so it should be no surprise that we’re not sure how to partition them into classes that will reliably support induction. So I’m not optimistic that significant progress in the discipline in the near future will take the form of the discovering of new natural laws.\footnote{It should be clear by now that I don’t see the abandonment of the quest for laws (temporarily or not) as entailing the abandonment of explanation or science. Cf. Schiffer (1991, 16): “When I read biology, I have a hard time finding anything that looks like a law-invoking explanation, and I think I know why. Suppose you just invented the spring-activated mouse-trap and had to explain how it worked. You would explain that, when the device works, it’s because a mouse nibbles at cheese placed on a release mechanism; the movement caused by the nibbling releases a bar attached to a stretched spring; etc. But you wouldn’t mention any laws. Maybe if you went on in an explanatory chain long enough you’d get to laws; but they’d be laws of physics, not laws of mousetrap theory. In the same way, much of biology is concerned to explain how various mechanisms work—think of the explanation of photosynthesis—and such explanations seem not to invoke any biological laws, strict or ceteris paribus.”}

Still, all is not that gloomy. We actually have a lot of acquired folk wisdom that gives us a reasonable place to start if we want to learn more about the causal etiology of individual cases. Getting more specific in our specification of the sorts of initial conditions we assume in our analyses would help, too; our unacknowledged “hypothetical listeners” aren’t respectable Hempelian provisos, they’re simply a bit of sleight of hand that allows us to pretend we’re not engaged in psychology. That change alone would provide information relevant to answering what I take to be a much more interesting and thorny question than the one—“Why is it that people often hear things similarly?”—most beloved of generalization junkies, namely, the question “Why does everyone hear a bit differently, sometimes quite a bit differ-
ently?” The answer to both, of course, lies somewhere in those initial conditions.

IV

It is, I suppose, one of those good-news/bad-news things. I’ve tried to suggest that if your claims are true and novel then you’re adding to the explanatory text and any additional worry about whether your claims are also scientific is unwarranted and quite possibly a bit neurotic. That’s the good news. The bad news is that I’ve also tried to suggest that true claims are harder to formulate than we like to admit. Being honest about the fact that we’re making psychological claims will help, as will making explicit the hypothetical listeners implicated in the truth conditions for those claims. And since we don’t know the limits of human cognition (musical or otherwise) it’s fine with me if the listeners we construct are idealizations beyond the known capacity of a single listener at a single time, because in the final analysis they, like our theories themselves, represent not only our accomplishments but also our aspirations.

12 Kitcher (1993) provides an extended account of “the growth of science as a process in which cognitively limited biological entities combine their efforts in a social context” (9), in effect providing a detailed picture of how humans go about individually and collectively constructing the explanatory text that might be thought of as amplifying aspects of Railton’s account.
References


Simplicity, Truth and Beauty in Music Theory

Nora A. Engebretsen

Our general empirical criteria for success of the theory are how adequately it describes musical intuition, what it enables us to say of interest about particular pieces of music, what it enables us to say about the nature of tonal music and of music in general, and how well it dovetails with broader issues of cognitive theory. In addition, we impose formal criteria common to any theoretical enterprise, requiring internal coherence and simplicity of the formal model relative to the complexity of the phenomena it accounts for. In short, we conceive of our theory as being in principle testable by usual scientific standards.

Lerdahl and Jackendoff (1983, 4–5)

[C]riteria of elegance or economy of formulation, useful for theories in non-aesthetic domains, such as physics, are also beside the musical point. Music interests us because it is (beautiful, interesting, etc.) music. Therefore any theory of music should aim at least to capture most poignantly that aspect of the music’s beauty or essence that most appeals to the theorizing musician.

Rahn (1989, 148)

Simplicity is commonly assumed to be a desirable feature of theoretical systems. Explicitly and implicitly, philosophers, scientists, statisticians and music theorists alike wield Occam’s Razor, the well-known dictum that “entities should not be multiplied beyond necessity,” in the construction and evaluation of their theories. This preference for simplicity, the notion that “all other things being equal, the simpler theory is the better,” has definite aesthetic, psychological, and pragmatic appeal. A simple theory brings order to complexity, and the simpler a theory is, the easier it may be to apply and the less likely it is to be contradicted. Yet the assumption that simplicity is a virtue is by no means universal. Certainly, the pitfalls of oversimplification and an attendant loss of explanatory adequacy loom large here, but so do the questions of how or even whether a preference for simplicity can be justified on rational grounds.

The following essay considers the nature of music theory in relation to other kinds of theories by examining the roles simplicity plays in the construction, evaluation, and application of theoretical systems. Two issues
raised in the epigraphs will receive particular scrutiny: the notion that music theory should conform to “scientific standards,” and the notion that simplicity, as a systemic value, is irrelevant to, if not incompatible with, the discovery of truths about musical phenomena and their aesthetic value. The discussion draws upon a range of music-theoretical texts and an existing body of literature in which philosophers have grappled with the notion of theoretical simplicity, primarily in the context of the natural sciences, and is loosely organized around four questions at the center of these philosophical considerations of simplicity: How is simplicity defined? What role does simplicity play in specific areas of inquiry? How is simplicity to beweighted relative to other theoretical values? And how can a preference for simplicity be justified on rational grounds? These questions overlap somewhat—definition, weighting and justification go hand-in-hand in many cases—but, in general, issues raised in defining simplicity will provide a background for discussion of matters of usage, weighting and justification in music-theoretical and other contexts.

Defining simplicity

The notion of simplicity is fraught with complexities, complexities deriving in part from the various senses in which the term simplicity is invoked. These range from familiar but largely undefined notions of both qualitative and quantitative simplicity to relatively specific notions of simplicity, such as ontological simplicity. These complexities are compounded by the fact that the various understandings of simplicity do not necessarily correlate with one another (Hesse 1967). An intuitively simple theory or concept may be represented by a highly complex mathematical expression, for example, or a complex musical surface may best be modeled by or interpreted through reference to a relatively simple theory. The crucial first step in any discussion of simplicity thus involves the specification of the kind of simplicity under consideration.

The primary concern here is with the simplicity of music-theoretical systems themselves—that is, with notions of simplicity that refer to the logical structure of a given theoretical language or of statements in that language, as opposed, for instance, to the more subjective standards by which we judge the relative simplicity or complexity of a given piece of music (aesthetic and psychological notions of simplicity) or the ease with which a given analytic theory can be applied to a musical repertoire (pragmatic simplicity), although these other kinds of simplicity will be engaged. Systemic simplicity, which is also referred to as formal or objective-logical simplicity, itself as-

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1 The complexity of simplicity is a recurring theme in the philosophical literature. The discussion here follows most directly from Bunge 1962 and 1963 (see especially the discussion of different notions of simplicity in chapters 4 and 5) and Derkse 1993, 163–74.
sumes two distinct forms: syntactic simplicity, also known as epistemological simplicity, and semantic simplicity, also known as ontological simplicity (Baker 2004, Gauch 2003, Sober 2002). Sober (2002, 14) distinguishes between the two as follows:

To strive for simplicity in one’s theories means that one aims to minimize something. The minimization might involve a semantic feature of what a set of sentences says or a syntactic feature of the sentences themselves. An example of the first, semantic, understanding of simplicity would be the idea that simpler theories postulate fewer causes, or fewer changes in the characteristics of the objects in a domain of inquiry. An example of the second, syntactic, understanding of simplicity would be the idea that simpler theories take fewer symbols to express, or are expressible in terms of a smaller vocabulary.

In other words, as Baker (2004) summarizes, semantic simplicity’s concern is parsimony, “roughly, the number and complexity of things postulated,” whereas syntactic simplicity’s concern is elegance, “roughly, the number and complexity of hypotheses.”

Even having narrowed our focus to syntactic and semantic simplicity, the formulation of a precise, operational definition of simplicity remains problematic. Most attempts at clarification have focused on the measurement of simplicity, with the goal of producing a metric for ranking theories on the basis of their relative simplicity. Questions immediately arise about what should be measured and how it should be measured, for conclusions about theories’ relative simplicity can vary when these criteria change. These questions in turn raise others about the justification of simplicity and its weight relative to other theoretical desiderata.

Analyses of formal simplicity have pursued two general courses, related to the two facets of formal simplicity identified above. The more common approach, which focuses on semantic simplicity and is exemplified in the works of Harold Jeffreys, Karl Popper, John Kemeny, Elliott Sober, and Kenneth Friedmann, among others, seeks a measure of the simplicity of a theory or postulate set in a given language, usually representing simplicity as a function of logical strength. The other approach, which focuses on syntactic simplicity and has been advocated by Nelson Goodman, seeks to measure the simplicity of the language basis of a given theory, rather than the simplicity of the theory itself.

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2 Baker’s equation of parsimony and elegance with semantic and syntactic simplicity respectively is by no means universally recognized. Derske (1993), for example, characterizes elegance as a concomitant of simplicity rather than as one of its facets, and, as Baker himself acknowledges, the terms simplicity and parsimony are “used virtually interchangeably in much of the philosophical literature.”

3 Sober 2002 provides several clear illustrations of this “measurement problem,” including one based on Nelson Goodman’s well-known Grue Paradox.
Those engaged in the analysis of semantic simplicity have generally construed the problem to be one of induction and have focused in particular on what is known as the “curve-fitting problem” (Ackermann 1961). Points on a plane are taken to represent observed instances of some phenomenon, while the competing hypotheses invoked to account for these data are represented by curves drawn through the plane. A satisfactory hypothesis is represented by a curve that either passes through or in close proximity to each of the points (and will presumably continue to do so as more points are plotted in accordance with future observations). There may exist many curves—and therefore many hypotheses—that fulfill this requirement for any given data set, but science (so the argument goes) seeks not merely a satisfactory hypothesis, but rather the most satisfactory hypothesis. The strategy adopted by advocates of “inductive simplicity” is to choose the simplest of the competing curves as representative of the best available hypothesis. Two questions immediately arise: how does one determine which curve is the simplest, and on what grounds can the assertion be made that the simplest hypothesis is also the most satisfactory? An acceptable solution to the curve-fitting problem must therefore incorporate both a definition and a justification of simplicity.

Jeffreys (1957), Popper (1959), and Kemeny (1955) are representative of attempts to analyze formal simplicity from this perspective. The approaches proposed by these authors differ significantly in their details, but share a similar underlying strategy: all three propose a simplicity ordering of all possible scientific hypotheses (or, in Kemeny’s case, of some circumscribed number of such hypotheses), and all offer justifications of the preference for the simplest satisfactory hypothesis based on the probability of either the confirmation or falsifiability of such a hypothesis as determined by its logical strength.

Jeffreys suggests that the privileging of the simplest hypothesis compatible with a given data set derives from an association of simplicity with high probability and develops a confirmation theory supporting this assertion. He begins by defining a simplicity ordering of all denumerable laws, which he claims are equivalent for practical purposes to the set of all differential equations of finite order and degree. This simplicity ordering hinges upon his measurement of the relative complexity of a given equation in terms of the number of its parameters, taken along with the sum of the absolute value of its integers (both degrees and derivative orders).

Several serious objections have been raised with regard to Jeffreys’s simplicity ordering, focusing primarily on the requirements that the set of hypotheses under consideration be denumerable and that these hypotheses must take the form of differential equations. Ackermann (1961, 155) has noted that Jeffreys’s ordering will not account for the many scientific laws represented by equations in which parameters range over the real numbers as the set of such equations is non-denumerable, and also that Jeffreys’s emphasis
on polynomials leads to the exclusion of other kinds of functions significant to science. Jeffreys's simplicity ordering fails, for example, to account for simple transcendental functions such as \(y = \sin(x)\), \(y = \log(x)\), and \(y = e^x\). The approximation of such functions via power series renders them highly complex, which seems counterintuitive, while measurements taken in terms of their differential equations result in an impracticably large number of laws with very low complexity. Furthermore, examples yielding counterintuitive classifications under Jeffreys's simplicity ordering can be constructed: \(d^2y/dx^2 + 2.345y = 0\) and \(y = ax^2\) have the same complexity value, for example, but the latter expression appears to be simpler (Hesse 1967, 446).

Jeffreys's association of simplicity with high probability is also problematic, as under this view any hypothesis can be simplified merely by adding disjuncts. While the addition of disjuncts will undeniably produce a weaker hypothesis, it does not necessarily follow that the resulting hypothesis will be simpler or preferable to the original (Friedmann 1990, 57). As Goodman (1961, 334–5) has demonstrated, simplicity cannot be defined as a product of logical weakness:

Suppose we have examined many and widely distributed specimens of maple trees and found them all to be deciduous, and suppose this constitutes our entire evidence. Since we still will not have examined specimens from every small locality, our evidence may then leave us with a choice between the following two hypotheses: (1) All maples, except perhaps those in Eagleville, are deciduous. (2) All maples are deciduous. The second is clearly both the stronger and the simpler. We would incorporate (2) in our theory, and retreat to (1) only if further evidence indicated that (2) is false. Insertion of the ad hoc exceptive clause both weakens and complicates the hypothesis.

As Goodman further notes, “the principle of maximum safety [that is, logical weakness] quickly reduces to absurdity; for it always dictates the choice of a hypothesis that does not go beyond the evidence at all.”

While denying a correlation between simplicity and logical weakness, Goodman's example seems to suggest a possible connection between simplicity and logical strength—precisely the avenue of inquiry pursued by Popper. Popper presents a simplicity ordering similar to that of Jeffreys, but rather than equating simplicity with confirmation and high probability, Popper argues that simplicity is correlated with falsifiability and thus that the strongest hypothesis—that is, the hypothesis with the greatest empirical content—not falsified by the collected data should be preferred. Popper presents two independent criteria by which to compare the falsifiability, and thus ostensibly the simplicity, of competing hypotheses.

The first of these, Popper’s “inclusion” criterion, applies to cases in which one hypothesis is included in another more general hypothesis. The basic argument can be summarized as follows:

If \( H_1 \) implies \( H_2 \) but \( H_2 \) does not imply \( H_1 \), then any set of observations that could conceivably suffice to falsify \( H_2 \) would be included among those that could conceivably suffice to falsify \( H_1 \), but not conversely; so here \( H_1 \) must be regarded as more falsifiable than \( H_2 \) (Barker 1961, 246).

As the more general hypothesis, \( H_1 \) presumably involves fewer qualifications, and would therefore be regarded as the simpler hypothesis. Popper’s second criterion, the “dimension” criterion, is more directly applicable to the curve-fitting problem. Though somewhat vaguely formulated in Popper’s writings, the dimension criterion seems to reduce to his assertion that “the number of freely determinable parameters of a set of curves by which a theory is represented is characteristic for the degree of falsifiability of that theory” (Popper 1959, 131). Popper then asserts a correlation between this falsifiability ordering and his simplicity ordering, for “an equation of low degree and order with a small number of parameters requires fewer data points to falsify it if it is false and therefore is more restrictive on the possibilities in the world and is more falsifiable” (Hesse 1967, 447).

While Popper claims that the preference for simpler hypotheses is justified on the grounds of falsifiability, he does not prove that the most falsifiable hypothesis should always be preferred or that the most falsifiable hypothesis is necessarily the simplest. Ackermann’s criticisms of Jeffreys’s simplicity ordering apply equally to Popper’s, intuitive notions of the simplicity of curves do not always correlate with Popper’s definition of simplicity in terms of his “dimension” criterion (Hesse 1967, 447), and counterexamples can be produced to refute Popper’s assertion that the most falsifiable—that is, the logically strongest—hypothesis is the simplest. Under this account of simplicity, it should always be possible to simplify a given hypothesis merely by adding non-trivial conjuncts, but in practice these added conjuncts often make the hypothesis more intuitively complex (Friedmann 1990, 57). Returning to Goodman’s maple tree example, the addition of a conjunct to the original hypothesis, “All maples are deciduous,” does produce a stronger hypothesis, “All maples whatsoever, and all sassafras trees in Eagleville, are deciduous,” but the new hypothesis is clearly less simple than the original—suggesting that the correlation of simplicity with logical strength inaccurately represents the nature of formal simplicity.

Kemeny (1955) presents a third approach to the curve-fitting problem. Kemeny focuses on complexity rather than simplicity, but his complexity can be understood as the inverse of the type of simplicity under discussion.

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5 Goodman 1961, 335; Friedmann (1990) also cites this example in his discussion.
He begins by proposing a complexity-measure for individual predicates—a measure determined by counting the number of basic relations expressible by a given predicate—and then proceeds to suggest a similar measure for sets of predicates. (A theory or hypothesis is, of course, a statement or set of statements prescribing relationships among predicates such that the total number of viable outcomes is somehow circumscribed.) Kemeny asserts that the complexity measure of a given predicate set is a function of the number of possible relationships that obtain for that set within a given finite universe, and offers a justification of the preference for simplicity on the basis of a correlation between simplicity and logical strength (that is, falsifiability). In this respect, Kemeny’s account of simplicity can be viewed as comparable to Popper’s and, as such, is open to many of the same criticisms.

Kemeny, like Popper, ultimately does not demonstrate that the most falsifiable hypothesis is also the simplest. Again, as Goodman has shown, the addition of non-trivial conjuncts will increase the logical strength of a given hypothesis, but will not necessarily result in a simpler claim. Furthermore, despite the different mechanics involved, Kemeny’s approach to the measurement of complexity displays shortcomings similar to those noted with Jeffreys’s and Popper’s approaches to simplicity. Specifically, since Kemeny’s complexity measure depends upon knowledge of the number of elements in the universe of discourse—and ultimately upon the enumerability of the various ways in which the given theory could be realized within this universe—it runs up against many of the same limitations associated with the restriction of Jeffreys’s and Popper’s accounts to finite predicate sets. Moreover, as Kemeny himself admits, the size of the universe under consideration is often not known, and when it is known, if the cardinality of the universe is even moderately large, the complexity measure quickly grows unwieldy.

Kemeny proposes to remedy this situation through an alternative version of his second complexity measure: where the exact size of the universe (N) is not known, but is known to be very large, he suggests that the complexity value may be thought of as a function of N, rather than as a specific number, and that such complexity values may then be compared in terms of their asymptotic behavior (that is, for all “sufficiently large” values of N, if \( f_1(N) > f_2(N) \), then \( f_1 > f_2 \)). This approach has been soundly criticized by Goodman (1959, 309–10), who argues not only that it behaves badly when N is known to be very large, but also that it “leaves open the question what we do when N is indeterminate but not guaranteed to be very large; and such cases are common, since many systems are so designated to make minimal existential commitments.” Ultimately, Kemeny, like Jeffreys and Popper, falls short of presenting a convincing account of the nature of simplicity or justifying the preference for simpler hypotheses.

While marked by a shift in focus away from the curve-fitting problem in particular and toward issues related to the matter of induction in general, the
two more recent accounts of the nature of formal simplicity presented in Sober 1975 and Friedmann 1990 build upon the works of Jeffreys, Popper, and Kemeny in their attempts to define a measure of the simplicity of theories. Both Sober and Friedmann break with the methodological strategy of the earlier accounts, however, in that they abandon the aim of producing a universal simplicity ordering and focus instead on the determination of the simplicity of competing hypotheses relative to some specific question under consideration.

Sober’s account hinges on the assumed correlation of simplicity and “informativeness.” Sober asserts that the simplest hypothesis with regard to a given question is that which goes the furthest toward answering the question—that the simplest hypothesis is the one requiring the minimum amount of extra information to arrive at an answer. To compare the simplicity of two or more hypotheses, Sober requires a single question to be selected as the standard against which to judge relative informativeness. To guarantee consistent and generally intuitively supportable results, he requires that this question be formed from members of a previously delimited class of “natural” predicates, predicates reflecting “law-like” features of the world. Sober then provides a set of rules for choosing between competing hypotheses on the basis of informativeness (that is, simplicity—which he defines as a product of logical strength) and a twin goal of evidential support (a concept he apologetically leaves unformalized). Briefly summarized, Sober requires that all acceptable hypotheses reflect some minimal degree of evidentiary support—an arbitrary threshold that may vary from case to case and from person to person. Thus, if some hypothesis A meets this support criterion, while a competing hypothesis B does not, A is preferred. If both A and B receive greater than minimal support, however, the simpler of the two is chosen, or, if A and B are determined to be equally simple, the most strongly supported hypothesis prevails.

A central feature—and the basic flaw—of Sober’s approach is his representation of simplicity as a monotonically increasing function of logical strength (Friedmann 1979, 77 and 1990, 68). In this respect, Sober’s account is fundamentally not far removed from Popper’s and is subject to many of the same criticisms. As noted above, for example, the addition of conjuncts will always produce a logically stronger hypothesis (if only in some trivial sense), but the resulting hypothesis will not always be intuitively simpler than the original. Furthermore, as Friedmann (1979, 77) has noted, this account of simplicity conflicts with the seemingly reasonable methodological decision to favor the simplest hypothesis supported by the available data, for there can be no such thing—any given hypothesis can be “simplified” merely by adding conjuncts and these conjuncts can always be chosen such that the resulting hypothesis will also be supported by the data at hand. Friedmann (1979, 78) goes on to find fault with Sober’s assertion that the
acceptability of a given hypothesis is based on a combination of simplicity and evidential support:

Simplicity is a monotonically increasing function of logical strength. But since acceptability is not an increasing function of logical strength it is desirable that evidential support counteract the effect of simplicity by being a decreasing function of logical strength. Yet this assumption entails that evidence provides at least as much support for a tautology as for any theory.

While the notion of determining simplicity relative to a specific question is appealing, insofar as it provides for the most pragmatically interesting sort of comparison of hypotheses and at the same time avoids difficulties encountered in attempts to formulate universal simplicity orderings, Sober’s account—like Popper’s and Kemeny’s—founders on the association of simplicity with logical strength.

Friedmann’s own account of formal simplicity differs significantly from those discussed thus far, in that he abandons the notion of a correlation between simplicity and logical strength, and instead characterizes simplicity in terms of testability, his basic premise being that “other things equal, the more systematic a theory the simpler it is; also, the more systematic a theory, the more testable it is” (Friedmann 1990, 56). Testability did figure in the earlier accounts of simplicity, construed either as falsifiability and associated with a preference for stronger hypotheses (in Popper, Kemeny, and Sober) or as verifiability and associated with a privileging of weaker hypotheses (in Jeffreys). Friedmann, however, advocates a notion of testability associated neither exclusively with falsifiability nor exclusively with verifiability, and eschews any reference to logical strength in conjunction with the determination of simplicity.

Friedmann’s (1990, 58–9) basic approach can be illustrated by considering the relationships suggested by the possible truthvalues of three hypotheses: $H_1 = a$; $H_2 = (a \& b)$; $H_3 = (a \cup b)$.

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$H_3$ is more verifiable than $H_1$, which is more verifiable than $H_2$, whereas $H_2$ is more falsifiable than $H_1$, which is more falsifiable than $H_3$. Or, setting aside the distinction between verifiable and falsifiable and simply counting the number of tests for each hypothesis, all three might be viewed as equally testable. Friedmann, however, exploits the fact that the tests for $H_1$ rely solely on knowledge of the truth-value of $a$, while the set of tests for $H_2$ and
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$H_3$ require knowledge of the truth-values of both $a$ and $b$. Thus, if knowledge of the truth or falsity of $b$ is set aside and we instead rely on tests depending solely upon the truth-value of $a$, there will be more tests for $H_1$ than for $H_2$ or $H_3$. Under Friedmann’s account, $H_1$ is the most testable and therefore the simplest of the three hypotheses.

Friedmann (1990, 59) argues that the exclusion of knowledge as to whether $b$ is true or false is justified in that “$a$ is a kernel common to all the hypotheses around which an unlimited number of hypotheses can be generated by conjoining and/or disjoining it with arbitrary $b$’s,” and he goes on to present a more formalized explanation as to what sort of information is to be excluded from consideration when testing hypotheses. On the basis of this explanation, Friedmann then asserts more precisely his notion of the relationship between simplicity and testability: a hypothesis $h$ is simpler than another hypothesis $h'$ if the set of tests for $h'$ (independent of the information to be excluded) form a proper subset of the set of tests for $h$ (again independent of the information to be excluded).

The characterization of simplicity as a function of increasing testability both explains the methodological significance of simplicity and yields results that conform to our intuitive notions of simplicity, avoiding many of the pitfalls associated with accounts of simplicity as a function of logical strength. This definition, however, fails to account for certain kinds of simplicity relations. Specifically, it lacks some general means of ordering predicates or hypotheses formed from these predicates according to their relative simplicity: even if $P_1$ is simpler than $P_2$, and $P_2$ is simpler than $P_3$, it does not necessarily follow from the proper subset definition that $P_1$ will be simpler than $P_3$, as the sets of tests underlying the evaluation of $P_1$ and $P_2$ and of $P_2$ and $P_3$ may involve disparate elements.

Friedmann therefore sets out to refine his definition of simplicity to ensure transitivity—or more accurately, to isolate those cases in which transitivity does not hold—thus paving the way for the ordering of comparable hypotheses. He begins by drawing a distinction between relatively simple and relatively complex predicates, which he terms “atomic” and “molecular” predicates, respectively. A molecular predicate is “one which is logically equivalent to some concatenation of other predicates such that according to the proper subset account of simplicity some of these other predicates are simpler than the molecular one” (Friedmann 1990, 70). Atomic predicates are defined negatively as those that are not molecular.

Friedmann next turns his attention to simplicity and similarity relations between predicates. The relative simplicity of any two given predicates is determined according to the original definition. The new similarity relation obtains between two atomic predicates, $P$ and $P'$, if and only if they are mutually incompatible and $P$ ($P'$) is not simpler than $P'$ ($P$). A similarity relation obtains between two molecular predicates if and only if when they are both expressed in their simplest terms—that is, when they are both expressed
solely in terms of atomic predicates linked by the fewest possible binary
connectives—a one-to-one mapping exists from each atomic predicate of
one onto a corresponding atomic predicate of the other.

On the basis of these simplicity and similarity relations, Friedmann (1990,
71) defines two additional relationships among predicates:

A descending chain from $P_1$ to $P_j$ is defined as an ordered sequence of predi-
cates starting with $P_1$ and ending with $P_j$ such that (1) at least one predicate is
simpler than its successor, and (2) each predicate is either simpler than or
similar to its successor … A chain from $P_i$ to $P_j$ is defined as a sequence of
predicates starting with $P_i$ and ending with $P_j$, each predicate similar to its
successor.

These new “descending chain” and “chain” relations in turn underlie Fried-
mann’s revised definition of simplicity:

$P_i$ is simpler than $P_j$ if there is a descending chain from $P_i$ to $P_j$. If $P_i$ is sim-
pler than $P_j$, then $P_j$ is less simple and more complex than $P_i$. If $P_i$ is as simple as
$P_j$ if there is a chain from $P_i$ to $P_j$. $P_i$ and $P_j$ are mutually incomparable if
there is neither chain nor descending chain between them (Friedmann 1990,
71).

The relations “simpler than” and “more complex than” are irreflexive, transi-
tive, and non-symmetric; “as simple as” is reflexive, transitive, and symme-
tric; and “incomparable with” is irreflexive, non-transitive, and symmetric.

Therefore, given three predicates, $P_i$, $P_j$, and $P_k$, we know, for example, that
if $P_i$ is simpler than $P_j$, and $P_j$ is simpler than or at least as simple as $P_k$, then
$P_i$ will be simpler than $P_k$, or if $P_i$ is simpler than $P_j$ while $P_j$ and $P_k$ are mu-
tually incomparable, then $P_i$ and $P_k$ will be mutually incomparable as well, and
so on.

Friedmann goes on to demonstrate that this account of the simplicity of
predicates easily extends to universally or existentially qualified hypotheses,
but as he is quick to note, even this revised definition of formal simplicity is
not entirely satisfactory. His account fails, for example, to address intuitions
about the relative simplicity of “incomparable” predicates or hypotheses, and
it is too weak to allow for the comparison of full-fledged scientific theories.
Friedmann (1990, 83) does assert, however, that his definition offers a
means by which to approach the simplification of “principles operating
within theories.” As such, while Friedmann’s account does not support
the privileging of formal simplicity as a criterion for choosing between compet-
ing theories, it does contribute to an understanding of simplicity in the con-
text of theory building.

As noted above, Goodman has advocated an approach to the analysis of
formal simplicity quite different from those heretofore considered. Rather
than attempting to define a measure of the simplicity of a given theory,
Goodman (1972 [1963], 338) marks off a smaller part of the problem for consideration, suggesting that “since systematization increases as the set of undefined ideas and unproven statements is reduced, what should concern us is the simplicity of the *basis* of such a system.” For Goodman, it is this notion of systematization—rather than that of some form of testability—that underlies a justification of the privileging of formal simplicity in scientific theories. As he explains:

Simplification is the soul of science. Science consists not of collecting particular truths but of relating, defining, demonstrating, organizing—in short, of systematizing. And to systematize is to simplify; an integrated system is achieved just to the extent that everything can be reduced to a minimal apparatus of underived terms and statements—that is, of undefined or primitive terms, and postulates or axioms (Goodman 1972 [1967], 351).

This assessment leads Goodman to develop a “calculus of simplicity” by which to measure the simplicity of extralogical predicate bases.6

Goodman begins by considering three possible approaches to the problem of measuring the simplicity of a language predicate basis: as a direct inverse of the defining power of the basis; as the sum of the number of predicates in the basis; and as the sum of the number of places in all of the predicates in the basis. The first of these proposals—the characterization of simplicity in terms of defining power—seems reasonable: if a basis *A* is definable from a second basis *B*, but *B* is not similarly definable from *A*, then *A* would seem to be simpler than *B*. This criterion does not allow, however, for the comparison of two bases unless one is definable in terms of the other. Furthermore, its assumption as a general principle would lead to the misrepresentation of maximum complexity as maximum simplicity, for under this definition the simplest basis for any system results from taking all of its extralogical terms as primitives. The notion of some correlation between defining power and simplicity is significant, though. The second proposed measure—the counting of constituent predicates—also fails as a test for simplicity, again leading to a trivial notion of maximal simplicity as any number of predicates can always be combined to form a single predicate of many places. One of the major shortcomings of this approach is its failure to register any differences of complexity among predicates, which leads to Goodman’s third proposal—that the simplicity of a basis be measured by the total number of places in all of its predicates. This measure solves the problem encountered with the second, but it still does not present a satisfactory account of the simplicity of a basis, for as Goodman (1972 [1958], 286) notes:

6 The most rigorous formulation of this calculus of simplicity appears in the third chapter (sections 3–10) of Goodman 1966. The relevant passages contain material not found in the first edition of this text. Somewhat less formalized discussions of the simplicity of predicate bases are also found in many of the articles collected in Goodman 1972.
Replacement of a set by a single predicate is always possible, but not replacement of single predicate by set. The complexity of bases seems to vary not only with the number of places but also in some manner with the way this number is distributed among predicates in the basis.

This notion of replaceability figures prominently in the development of Goodman’s calculus of simplicity.

Goodman ultimately presents a highly systematic and detailed explication of the simplicity of formal systems. Briefly summarized, Goodman takes as his basic premise the notion that “if every basis of a given kind can be replaced by some basis of a second kind, then no basis of the first kind is more complex than every basis of the second kind,” further stipulating that every extra-logical predicate must have a positive complexity-value and that the complexity-value of a basis can be determined by taking the sum of the values of its constituent predicates (Goodman 1966, 70 and 73). By qualifying the notion of replaceability to account for the varying degrees of complexity engendered by predicates of different logical types—differences among symmetrical, reflexive, and self-complete predicates (where self-completeness is a strong case of transitivity)—Goodman avoids the pitfalls associated with the three approaches to the definition of simplicity he initially proposed.

Goodman’s calculus is generally held to constitute a fairly precise account of simplicity, but he himself stresses that this definition addresses only the structural simplicity of the basic extralogical components of a given theory and not the simplicity of postulate sets constructed from these components. As such, Goodman never purports to provide a measure of the overall simplicity of a theory. Furthermore, since a single language basis could conceivably underlie a number of alternative theories, the status of the simplicity of bases as a criterion for choosing between competing theories is tenuous at best.7

On the function and value of simplicity in music theory

Having a clearer account of what simplicity entails, we are now in a position to begin to examine the role simplicity plays in the construction, evaluation, and application of music-theoretical systems. Is simplicity a formal or logical desideratum, or rather strictly an extra-systemic aesthetic or pragmatic consideration? What is the status of simplicity as a criterion for choosing

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7 Hesse 1967, 448. Just as a single language basis could underlie a number of alternative theories, a single theory could conceivably be expressed in terms of a number of alternative language bases. As Sober (2002, 16) observes, “Syntactic measures of simplicity inevitably encounter this sort of problem. Since a proposition can be encoded in many different ways, depending on the language one adopts, measuring simplicity in terms of code features will fail to be linguistically invariant.”
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between competing theories and analyses? Do we value simplicity as a test of completed theories, or rather for the ways in which it informs the process of theory building? With regard to analytically oriented theories in particular, does the notion of simplicity intersect with that of explanatory adequacy, and if so, how? Does there appear to be any correlation between the relative simplicity or complexity of musical data with that of the theoretical system(s) invoked to model it? And how does the relative simplicity or complexity of a given model affect its analytical application?

On the basis of the above survey of attempted analyses of formal simplicity, it seems clear, given the failure of any of the accounts to produce a logically sound explication or practically viable measure of the relative simplicity of complete theories, that formal simplicity as presently understood cannot function as a test for choosing between competing theories. Rather, as suggested by Goodman’s account and, to a lesser extent, by Friedmann’s, formal simplicity may well be a desideratum of scientific and music-theoretical systems alike, insofar as it informs the process of theory building.

Before proceeding under the assumption that simplification should underlie all theory building, however, we might do well to revisit the two epigraphs to this essay. In the first passage quoted, Lerdahl and Jackendoff explicitly embrace simplicity as a characteristic of successful theoretical systems and position themselves as part of a broadly defined scientific community committed to a set of shared “theoretical values” (Kuhn 1977). Notably, their list of the characteristics of a good theory corresponds closely to the five theoretical values favored by Kuhn: accuracy, consistency, scope, simplicity, and fruitfulness. Rahn, in contrast, deems simplicity—specified as elegance and economy of formulation (parsimony)—to be “beside the musical point,” and draws a sharp distinction between values in aesthetic and “non-aesthetic” domains such as music and science.

Indeed, not all music theorists support the privileging of simplicity as a systemic property, particularly when this status is founded upon a presumed analogy between music theory and the natural sciences or formal logic. In addition to Rahn’s adamant assertion that analytically oriented music-theoretical systems lie within the aesthetic domain and as such should be judged solely on the basis of aesthetic criteria, Benjamin Boretz has suggested on at least one occasion that simplicity is, if not altogether irrelevant to music-theoretical constructs, at least of secondary importance relative to perceptual and aesthetic considerations. A more detailed consideration of such objections will reveal that neither author’s position is fully at odds with

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8 Kuhn’s “fruitfulness” is the only value not identified in Lerdahl and Jackendoff’s list. Kuhn’s “consistency” encompasses both Lerdahl and Jackendoff’s “internal coherence” and the notion of “external coherence” inherent in their discussion of the need for their theory to “dovetail[] with broader issues of cognitive theory.” Brown (2005, 18) likewise divides Kuhn’s “consistency,” though using different terminology, in his discussion of scientific values in music-theoretic contexts.
the notion of simplicity being advanced here, and it will afford the opportunity to examine the grounds upon which the privileging of formal simplicity might be justified in music-theoretical contexts (and how this relates to the justifications of simplicity given in the accounts above), the ways in which simplicity informs theory building, and the ways in which this influence might be constrained in practice.

In his polemic response to Brown and Dempster 1989, Rahn (1989) argues against the presumption of equivalence between music-theoretic systems and the models of the natural sciences or of formal logic, claiming that the criteria according to which one evaluates the fit of these models necessarily differ between different (in this case, aesthetic and non-aesthetic) domains. Rahn suggests, for example, that criteria such as truth, comprehensiveness, and predictive power, while highly valued in scientific theories, are either inapplicable (in the case of truth) or irrelevant to music theory. As we have seen, Rahn likewise rejects the notion that simplicity, due to its value in the sciences and in formal logic, must also be a significant feature of analytically oriented music-theoretical systems. To invoke simplicity in music theory is to conflate a feature of the modeling theory, whether formal logic or scientific explanation, with the theory being modeled:

In logic, it is logical to generate each theorem in the most efficient way, using the fewest and simplest steps in the derivation sequence. It is only too easy ... to carry this over fallaciously into the derivation of a piece of music, where the point is the beauty of the structure revealed in the music by the entire derivation sequence. The most beautiful structure is seldom shown in the most efficient sequence. The same fallacy can seduce music theorists into aiming for theories that are themselves of maximal formal beauty, rather than theories through which the music as modeled is most beautiful (Rahn 1989, 150).

Ultimately, Rahn suggests that the only criteria relevant to the evaluation of musical analyses or the theories that support them are aesthetic criteria—that “the description that results should be the most musically satisfying description among the alternatives” (Rahn 1989, 150).

Setting aside the unsupported assertion that “the most beautiful structure is seldom shown in the most efficient sequence,” whether or not one agrees with the conclusion that systemic simplicity is irrelevant in aesthetic domains, Rahn’s objections do raise serious questions with regard to the defensibility of privileging simplicity as a criterion in the evaluation of music theories solely on the basis of existing justifications of simplicity in terms of scientific methodology, as analytically oriented music theory and the natural sciences have very different aims. Science notably values regularity and generalization, examining the range of outcomes for some recurring behavior and formulating hypotheses that not only explain observed instances of such behavior but also, it is hoped, accurately predict the range of outcomes for
future instances. Music analysis, on the other hand, is most often not interested in some range of possibilities, but rather in the details of the specific piece at hand and in what makes this piece interesting or special—generalization is useful only insofar as it helps one to understand the individual work. That is, whereas scientific inquiry has as its primary objective the explanation of universal properties, music analysis tends to focus on the description of unique features.

Rahn’s central argument is that theoretical systems must be evaluated according to criteria relevant to the desired output. Consider the criterion of formal simplicity, characterized as a correlate of testability as in Friedman’s account. Intuitively, it seems quite plausible that testability, and therefore simplicity, should be of significance in the evaluation of predictive (scientific) theories. It is much less obvious, however, that testability and simplicity should have any bearing on the evaluation of a music-analytical theory, the goal of which (or at least one goal of which) is to produce the most musically satisfying (that is, aesthetically satisfying) description of a given piece. But does this observation really preclude the possibility that structural simplicity is of relevance to music-theoretical systems? Might it not simply indicate that this particular representation of formal simplicity is not applicable in a music-theoretical context or, more probably, that while some notion of formal simplicity may exert an influence on the theory’s construction, simplicity is subservient to criteria relating to the power of the theory to capture musically relevant relationships—that simplicity, in and of itself, is not an ultimate, universal aim?

As noted above, Boretz has also suggested that formal simplicity, if at all relevant to music-theoretical models, is of secondary importance to perceptual matters and musical intuitions. In his response to Brown and Dempster 1989, Boretz touches upon the role simplicity played—or, more accurately, did not play—in the formulation of the basic theoretical system underlying his *Meta-Variations*:

benevolent friends of mine, earnest and engaged fellow music-thinkers, strenuously urged me to reconsider my construction of the definition of “intervals” out of that of “pitches” in Part II of *Meta-Variations,* on the plausible grounds that the logical formalizations could be made much simpler, more economical, and elegant if the ascension were reversed: that is, if “intervals” were taken as primitive and “pitches” defined by extract from them. But my use of logical forms and structures was motivated entirely by epistemic interests: I needed to explicate and examine my intuitions about the relative depths and essential character of musical phenomena; the logico-definitional sequence had to follow, delineate, particulate and test—not dictate the structure—the logic of my primal musical intuitions. Each step was a means to help me see the path ahead, in strenuous communion with my “mind’s ear,” and in strenuous disregard of any issues of logical concision (spelling everything out was my need; what use could I possibly have for elegant shortcuts?) (Boretz 1989, 110–11).
In certain respects, Boretz’s stance may be viewed as a less extreme version of that assumed by Rahn. In evaluating music theories, both value criteria relating to the ability of the model to capture musically and perceptually relevant relationships ahead of any sort of formal simplicity, rejecting outright any notion of formal simplicity as an absolute or universal goal—a position in keeping with the rejection of simplicity as a test for choosing between competing theories. Boretz, however, certainly appears to be more willing than Rahn to acknowledge that formal simplicity may yet serve some systemic function in analytically oriented music theories. In fact, Boretz’s *Meta-Variations* includes one of the most explicit engagements of issues related to simplicity and the construction of music-theoretical systems available and, as such, provides valuable insight into the ways in which simplicity can and does operate in music theory.

In Part I of *Meta-Variations*, Boretz suggests that when evaluating an analytical model,

> We want to see how fully it explicates the relation of the *discernible particular* to the *conceptual general*, and thus to what degree it gives answers to questions that musicians regard as crucial ones regarding musical structures. Thus we would hope that, for every “musical concept,” the theory under consideration would either *interpret* it as an essential aspect of the scheme of relations, or *eliminate* it as redundant (show how it could be replace in full operational extensionality by a term or conjunction of terms already in the theory) (Boretz 1969, 64).

Boretz demands, first and foremost, that the theory be able to account for the perceived particulars and to organize them within some network of musically relevant relationships, presumably yielding insights with regard to the features that make the piece at hand special or interesting. To this end, rather than as a goal unto itself, Boretz introduces the notion of what he describes as “an ‘Occam’s Razor’-like parsimony” in the construction of the organizing conceptual classes, suggesting that a theory should eliminate from consideration any “musical concept” that affords no cognitive-explanatory distinction not already otherwise accounted for within the theory. The elimination of such redundancies, he goes on to note, is methodologically desirable in that it avoids the pitfalls of a “top-heavy” theory—“one that gives us plenty of names for the same things but enables us to discriminate too few distinct things,” making it difficult to draw connections between various events or concepts.⁹

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⁹ Boretz 1969, 65. Boretz immediately hedges his position, explaining in a footnote that “obviously, a theory that had maximum cognitive scope, included all the relevant concepts, but also multiplied ‘conceptual terms’ to excess could not be actually faulted except on grounds of terminological gluttony.” While the elimination of redundant conceptual classes may not be an absolute necessity, it is desirable not only from the standpoint of practicality, for “termino-
Boretz’s call for “an ‘Occam’s Razor’-type parsimony” in the construction of classes and the pragmatic justification he provides are, of course, quite resonant with Goodman’s account of simplicity and with his correlation of simplicity with systematization. Indeed, of the various approaches to simplicity described above, it is Goodman’s notion of the simplicity of language bases that most clearly informs the construction of music-theoretical systems. Many presentations of rigorously formalized music theories do, in fact, presume the desirability of the simplicity of language bases, whether or not the theoretical foundation established by Goodman’s works is explicitly acknowledged. Consider, as one of many possible examples, the pitch/interval model set forth by Jay Rahn (1994). Rahn’s interest in simplicity in the Goodmanian sense is evident both in his stated aim (“sought here are ways in which pitch and pitch-interval predicates might be interconnected logically”) and, to an even greater degree, in his attendant methodology (“As far as possible, pitch- and interval-predicates are defined in terms of a single, maximally economical, basic, primitive predicate…”).

By far the most explicit reference to Goodman’s theory per se occurs in Boretz’s *Meta-Variations*, particularly in sections 5 and 6 of Boretz 1970 (the sections entitled “The Extralogical Bases of Constructional Systems” and “The Extralogical Basis of the System To Be Sketched”). Boretz first introduces the notion of extralogical bases by quoting relevant passages from Goodman 1966, emphasizing that the primitives chosen must form not only an adequate basis for the definitions required within the system, but the simplest adequate basis. He then outlines the basis underlying his own theoretical system—a basis which consist of only two primitive predicates (P(x), which means “x is a pitch quale,” and T(x), which means “the time of x”) plus one basic relation to be taken as a primitive (P_m(x,y), which he translates as “x and y are matching pitch qualia”)—citing further correspondences between his work and Goodman’s as he proceeds.

In a passage quoted above, Boretz recalled the suggestion of friends that, in the interest of logical concision, he reconsider his choice of “pitch” rather than “interval” as a primitive. While his rejection of this idea derived first and foremost from his particularistic insistence that the modeling theory accurately reflect his perceptual processes and musical intuitions at every level, his apparent disregard for the simplicity of the overall theory (or even for some portion of the theory extending beyond the extralogical basis) does not contradict in any way his advocacy of a Goodmanian notion of simplicity, which addresses only the simplicity of the extralogical basis and not that of postulate sets constructed from the components contained therein. Indeed, logical gluttony does tend to produce unwieldy theories, leading to the sorts of difficulties cited above, but also—and perhaps even more significantly—in terms of the correlation between theoretical simplicity and our perceptions of music-structural coherence, to be discussed below.
looking at the proposed substitution of an “interval” primitive for the “pitch”
primitive, the resulting basis would not necessarily be simpler than the one
Boretz proposes, so there would be no clear advantage to be gained from the
perspective of Goodmanian simplicity.

As the foregoing suggests, while the simplicity of extralogical bases is
clearly a desirable feature of and is readily adaptable to music-theoretical
systems, it is much less obvious, given our present understanding of formal
simplicity, how or even if some type of systemic simplicity continues to
inform the theory building process as one moves beyond the language basis.
While we have already rejected the notion that formal simplicity is, in and of
itself, the ultimate criterion by which music theoretical systems should be
evaluated—for as both John Rahn and Benjamin Boretz have noted, it is the
ability of the theory to model our musical experiences or to capture the
special features of a given piece that we truly value—it still seems reasonable,
from a pragmatic or aesthetic point of view, if not from a strictly logical one,
to suggest that given two theories that capture precisely the same musical
relationships, we would prefer the “simpler,” more concisely formulated
theory. Of course, as noted above, none of the accounts of formal simplicity
produced thus far have included a viable measure of the simplicity of com-
plete theories; however, Friedmann suggests that his definition offers a
means by which to approach the simplification of principles operating within
a theory. As such, his account might be instructive in terms of further clari-
fying what role simplicity can or should play in the construction and evalua-
tion of music-theoretic systems.

Certainly, aspects of Friedmann’s account are appealing in this regard.
His theory, for instance, supports the notion that an advantage is obtained by
explaining new phenomena in terms of existing laws rather than by inventing
new laws to cover every case, which reflects an extension of the “‘Occam’s
Razor’-like parsimony” advocated by Boretz. Ultimately, however, Fried-
mann’s correlation of simplicity with testability problematizes the applica-
tion of his approach in music-theoretical contexts, as there is no clear benefit
to be gained by privileging testability in a situation where descriptive power
rather than predictive power is the goal. In fact, under Friedmann’s account,
the choice of the most testable hypothesis often leads to the least precise
description. Moreover, even if we were to grant the applicability of Fried-
mann’s account to music-theoretical systems, simplification of a theory re-
quires that we determine “not merely which of two hypotheses is the simpler
but which one makes for the simpler total theory” (Goodman 1961, 336)—
something Friedmann’s account is not equipped to do.

In lieu of a formalized account of simplicity capable of supporting an ex-
amination of the role simplicity plays in the constructions of music-theo-

10 See, in particular, Friedmann 1990, Example 8.8.3.
11 See Friedmann 1990, Examples 7.6.1 and 7.6.5.
retical systems in their entireties, Boretz’s call for “an ‘Occam’s Razor’-like parsimony” in the construction of classes may seem intuitively sound as a methodological default. Certainly the notion is as applicable to terms defined within the system as it is to those taken as primitives, and it is easily extended to the predicates governing relationships among these terms (as suggested in conjunction with Friedmann’s account). Concision of formulation, however, while certainly desirable, will not guarantee the “simplest” realization of a given theory—a concise formulation of a particular theory may not necessarily be the most concise formulation possible. Insofar as the construction of music-theoretic systems is concerned, then, we would do well to heed Goodman’s dictum that “to systematize is to simplify” and that “an integrated system is achieved just to the extent that everything can be reduced to a minimal apparatus of underived terms and statements” (Goodman 1972 [1967], 351). Once the language basis is in place, however, we seem to have little choice but to take recourse in intuitive notions of simplicity, perhaps aiming for concision and hoping that simplicity will follow.

Simplicity, of course, does not function independently of other systemic properties or goals. In light of the logicians’ failure thus far to produce an account of formal simplicity sustainable beyond the language basis of a theory, a more productive approach to the problem of understanding simplicity in the context of music-theoretical systems might be to consider the ways in which simplicity interacts with other forces at work within a theoretical model.

Perhaps most obviously—as has been intimated several times above—a tension exists between theoretical simplicity and explanatory power. Theoretical simplicity demands the most concise explanation or description of the data at hand, while explanatory power favors the theory that accounts for the largest part of the available data—in terms of musical analysis, the theory that best captures the musical relationships or features we value. Certainly this tension is not unique to music-theoretic contexts, as Bunge (1963, 115) makes clear in the colorful warning:

Occam’s Razor, like all razors, should be handled with care to prevent beheading science in the attempt to shave off some of its pilosities. In science, as in the barbershop, better alive and bearded than cleanly shaven but dead.

As noted above, however, whereas predictive theories are valued for their ability to approximate the data—to circumscribe some general range of possibilities—descriptive theories are expected to account for specific individuating features. Accordingly, the tension between simplicity and explanatory power may be somewhat more pronounced in descriptive music-theoretic contexts.
Explanatory power is likely to be sacrificed both when theoretical simplicity is over-emphasized and when it is under-emphasized. As Boretz (1969, 65) has observed, in evaluating theoretical models:

We will want to be as critical of a “top-light” theory [too simple a theory] which fails to account for concepts that we do believe “make a difference” as of a “top-heavy” one [a theory formulated without regard for concision] that gives us plenty of names for the same things but enables us to discriminate too few distinct things.

In practice, however, it is more often the case to find analytically oriented theories in which simplicity has been sought at the expense of power.

Consider, for example, John Rothgeb’s (1996) criticism of the extensively revised version of Riemann’s *Funktionstheorie* presented by Eytan Agmon (1995). As Rothgeb readily admits, “Agmon’s theory is admirable in its simplicity.” Indeed, Agmon presents a symmetrically balanced system founded entirely on a definition of the “degree of triadic similarity” for the diatonic triads, three principles “on the basis of which the primary triads I, IV, and V may be selected as prototypes,” and two further propositions that “determine the additional members of each category and their respective prototypicalities” (Agmon 1995, 199–201). Unfortunately, Rothgeb points out, for all its systemic beauty, Agmon’s theory is woefully inadequate as a model of tonal function and “cannot be reconciled with certain palpable musical effects.”

Specifically, Rothgeb’s criticism focuses primarily on Agmon’s fundamental assertion that harmonic function can be determined without reference to chord progression (and apparently without knowledge of any other contextual information, such a voice leading or motivic considerations). Rothgeb suggests that this supposition forces Agmon to adopt a trivialized conception of the scale degree as “devalued to identity with vertical note-content” which in turn creates a “theoretical void”—the theory cannot, for example, distinguish a functional triad from a triadic configuration arising within some sort of linear prolongation and therefore demands that such a non-functional harmony represent one of the three primary categories, often leading to bizarre characterizations. As such, Agmon’s theory fails to provide an accurate theoretical model for many palpable musical events.

One can also find instances in which a theorist has consciously chosen to sacrifice theoretical simplicity so that his or her theory will better reflect musical realities. David Lewin (1982), for example, clearly engages issues of simplicity and power. Taking little more than a tonic pitch-class $T$, a dominant interval $d$, and a mediant interval $m$ as given, Lewin initially presents a generalized definition of Riemann Systems, along with a set of four basic operations—identity, tonic-dominant inversion, retrograde, and a conjugate relation—which form a serial group on the Riemann Systems.
Lewin’s theory thus begins with what is intuitively a relatively simple model. As he begins to examine the system’s ability to model real musical situations and to accommodate pre-existing theoretical constructs, however, he finds need for several additional transformations. The relation between dual F# minor and dual A major at the opening of Brahms’s *Intermezzo* Op. 119, no. 1 (Lewin’s Example 3), for example, suggests the necessity of the “shift” operation (the introduction of which then leads to the development of the notion of a “shift group” and to the inclusion of a number of specialized shift functions), while the need for a transformation that will capture the notion of moving to the “relative key” leads to Lewin to introduce “tonic-mediant inversion” and, in turn, “mediant-dominant inversion” (both conceptually related to the earlier “tonic-dominant inversion” operator). The various transformations can then be combined in numerous ways, leading “to more remote transformations and to larger, more complex, groups of operations”—and to a much more intuitively complex though more powerful system (Lewin 1982, 55).

The balance between simplicity and power sought in the above models is often referred to as “economy.” Goodman has explained the notion of economy in relation to his account of simplicity as follows:

> The most economical basis, like the most economical engine, is the one that accomplishes most with least. Simplicity—or low fuel consumption—is a different factor from power and has to be taken equally into consideration. And power, far from being inversely proportionate to economy, is directly proportionate to it where simplicity is constant; the stronger of two equally simple ideas is the more economical. Moreover, where we are concerned with comparing interdefinable and thus equally powerful alternative bases for a system, as is often the case, simplicity is the sole determinant of economy (Goodman 1966, 69).

Simplicity, in and of itself, is therefore of interest only in those cases where the explanatory power of the competing bases (or hypotheses or theories) is equal—that is, when simplicity is, for all intents, equivalent to economy.

**Concluding remarks on simplicity’s status**

Philosophers’ failure to arrive upon a global, rational justification of a preference for simplicity, in general, and formal simplicity, in particular, has left simplicity’s status as a theoretical value undetermined, and has prompted some to consider why this much sought-after justification remains elusive. Two noteworthy explanations offer insights into connections often posited between simplicity, truth and beauty, and open a potential point of redress of Rahn’s and Boretz’s rejection of simplicity as a music-theoretic value.
Simplicity, Truth and Beauty in Music Theory

Sober (1994, 139–40), departing from a critique of correlations of parsimony with plausibility, has concluded that a global justification for parsimony has not been found because “parsimony is not a scientific end in itself”—that it is valued only as a reflection of some more fundamental theoretical property. Simplicity may serve as a “symptom” of truth but it never serves as a “test” of truth (Bunge 1963, 97). Sober (1994, 140) explains his position as follows:

Philosophers have hypostatized parsimony. When a scientist uses the idea, it has meaning only because it is embedded in a very specific context of inquiry. Only because of a set of background assumptions does parsimony connect with plausibility in a particular research problem. What makes parsimony reasonable in one context therefore may have nothing in common with why it matters in another. The philosopher’s mistake is to think that there is a single global principle that spans diverse scientific subject matters.

In other words, simplicity for Sober is a local rather than global matter, and if valued its justification “must be local and subject matter specific” (Sober 1994, 154).

Other philosophers have taken a different tack, positing a distinction between rational epistemic simplicity, which includes formal simplicity and is associated with the “context of justification,” and an arational or extralogical aesthetic simplicity, which Derkse (1993, 172) describes as being “connected with the striving for intellectual and theoretical beauty” and associated with the “context of discovery.” This recasting of simplicity as an aesthetic impulse is not without precedent and recalls Popper’s remark that simplicity “has little interest from the point of view of the theory of knowledge: it does not fall within the province of logic, but merely indicates a preference of an aesthetic and pragmatic character” (Popper 1959, 137).

That aesthetic simplicity is not quantifiable and does not lend itself to comparisons does not necessarily preclude it from playing a role in the evaluation of theories. McAllister (1989) has proposed a category of “aesthetic criteria of theory-choice” or “indicators of beauty”—comprising simplicity, symmetry, analogical interpretability, and consistency with metaphysical presuppositions—to function alongside but independent of the familiar theoretical values—internal consistency, consistency with pre-existing and well-corroborated theories, predictive accuracy, predictive scope, and fruitfulness—which he terms “indicators of truth.”

The realignment of simplicity with beauty, rather than with truth, and its association with the discovery of intellectual and theoretical beauty, suggests one potentially illuminating line of inquiry might be to examine the relationship between simplicity and closely held notions of structural coherence in Western art music. Cohn and Dempster (1992, 156) note as a point of departure for their argument:
The principle and most persistent canon governing our Western aesthetic is that successful works of art, including the “masterpieces” of Western art music, exhibit unity, coherence, or “organic” integrity. Music theory upholds this canon in its seminal commitment to the presupposition that musical unity is to be found not “exposed on” the complex, sometimes bewildering phenomenal “surface” of a composition, but rather “hidden in” some “underlying” structural simplicity.

Cohn and Dempster ultimately reject the specifically hierarchical organization of this canon of underlying structure, but we might nevertheless safely assert that most music-theoretic systems do aim to measure or represent coherence in terms of the number of basic concepts invoked along with the degree to which these fundamental notions can be projected across structural levels in accordance with some hierarchical scheme. A piece, as modeled by some given theory, is said to display a high degree of coherence if a simple structure can be shown to underlie the complexities of the musical surface—that is, if the modeling theory represents the piece as a ramified, multi-level structure founded upon a relatively simple theoretical basis.\(^\text{12}\) Conversely, a low degree of coherence is associated with pieces for which existing theoretical models lack structural depth, relying instead on complex basic vocabularies and on predominantly taxonomic analytic applications.\(^\text{13}\) In this context, the search for simplicity in the complex in effect involves the unification of disparate surface event to reveal the beauty of the work, and the simplicity of the theory reflects the intuited simplicity of underlying structure within the aesthetic domain.

Consider, for example, the differences between theoretical models typically applied to highly coherent tonal repertoires and those developed for the more problematic “freely atonal” or “motivic” works. A functional progression model for tonal music, such as that presented in Smith 1981, involves a relatively simple prolongational vocabulary (the three traditional functional categories plus an allowance for non-functional chords, taken along with a notion of the seven diatonic bass scale-steps) plus a basic progression paradigm (T–DP–D–T) that may be transformed in certain specified ways. The analytical applications of this model become quite elaborate, however, requiring a differentiation between chord tones and non-chord tones and then between functional and non-functional harmonies, and also involving simultaneous hierarchized re-interpretations of prolongational accounts. On the other hand, the set-theoretic approach most often applied to post-tonal or motivic repertoires features a markedly more complex basic vocabulary (352 set-classes or 224 set-class types, along with all the possible abstract similar-

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\(^\text{12}\) For a similar discussion approached more from the standpoint of musical/aesthetic simplicity rather than that of theoretical simplicity, see Meyer 1976.
\(^\text{13}\) For a somewhat more formalized discussion of music-structural coherence and the role of simplicity, see Boretz 1970, 56–8 and also the related discussion in Boretz 1972, 152–9.
Simplicity, Truth and Beauty in Music Theory

ity, inclusion, and complementation relationships) and lacks any standar-
dized progression paradigm, the succession of elements being determined on a
purely contextual basis. Analytic applications of set theory tend to be rather
simple, in that the process of analysis essentially amounts to the labeling of a
segmented musical surface without asserting any sort of deeper level (non-
abstract) structure.\textsuperscript{14}

To pursue the connection between formal simplicity and music-structural
coherence in further detail would require presenting a more rigorous account
of coherence, which is beyond the scope of the present study; however, sev-
eral observations can be made on the basis of the above discussion. Cer-
tainly, the relationship between the relative simplicity or complexity of a
theoretical syntax and that of its analytic application seems to reflect our
perceptions regarding the structural coherence of the repertoire at hand. In
particular, the simplicity of theoretical bases seems somehow to contribute to
or to be correlated with a high degree of structural coherence. Given the
significance of coherence to Western musical aesthetics, if formal simplicity
were to be shown to be a necessary condition for music-structural coherence,
this would constitute at least a partial justification—on aesthetic, rather than
logical or “scientific” grounds—of formal simplicity in local music-theoretic
contexts. (Since the perception of structural coherence is relative to some
filtering theory, it follows that, if a correlation does indeed exist between
formal simplicity and coherence, formal simplicity would be a necessary
condition for music-structural coherence, rather than vice versa.)

Near the end of his study \textit{Unplayed Melodies: Javanese Gamelan and the
Genesis of Music Theory}, Marc Perlman remarks that “all of the theorists
discussed in this chapter [whether European or Javanese] tried to find simple
explanations of their music’s complex patterns; all constructed an ideal
realm of regularity and used analogical thinking to populate it with abstract
entities,” but, he goes on to note, “the European writers explicitly thematize
the value of simplicity and regularity. They affirm it as an explanatory virtue
and use it to proclaim the merits of their own accounts” (Perlman 2004, 191–
2). Though the Javanese musicians were equally engaged in the discovery of
underlying simplicity and theoretical beauty, these notions went unmen-
tioned and were in a sense left in the aesthetic realm. Perlman attributes this
difference in attitude—in essence the shift from the context of discovery to
that of justification—to the Europeans’ learned preference for the epistemic
over the aesthetic:

\textsuperscript{14} This is not to say that the analytical process is simple, however. Pople (2004, 131) charac-
terizes set theory somewhat differently, describing the theory itself as relatively simple while
observing that “the associated methods of analysis are varied, and even the most fundamental
process—that of segmentation—depends largely on personal habits of working at an analy-
sis.”
The fact that the European writers (Lippius and Rameau in particular) did praise the simplicity and regularity of their theories is probably due to the prestige of European models of intellectual inquiry (logic, mathematics, philosophy, science) in which these values were consciously affirmed (Perlman 2004, 192).

Yet, in music-theoretic contexts, we find that formal simplicity does not function as a universal aim or as a test for choosing between competing theories. Ultimately, analytically oriented music theories must be evaluated on the basis of their explanatory adequacy—their ability to capture the unique features and significant relationships that distinguish the piece at hand. To this end, we wish not merely for concision or parsimony in construction, but for a balance between theoretical simplicity and explanatory power—for economy of formulation—and value simplicity both for the pragmatic benefits of the systematicity it favors and for the aesthetic significance of the attendant implications of this systematicity with regard to music-structural coherence. Clearly, however, much work remains to be done before we can fully understand the role formal simplicity plays in the construction and evaluation of music-theoretic systems.
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The Concept of Unity in Music

James W. Manns

Introduction and preliminary observations

This paper grew out of an intuition that imposed itself on me, one which insisted that unity was a value-able property for any composition to possess. That may sound like an odd sort of an intuition to be visited by, but if I may enlarge a bit on the context within which it emerged, perhaps I can paint a reasonable face on it.

Unity has of course been a concept central to aesthetic analysis dating from a time long before such analysis was ever referred to as aesthetic. Aristotle, in his Poetics, assigned to unity a place of primacy in the proper structuring of a tragedy; but even at that, Plato was well aware of its artistic importance; and for that matter, the Parmenidean One contains strong aesthetic overtones.

Now when I attempted to see how this age-old concept could be related to the art of musical composition—when I started looking, that is, for some objective criterion or criteria that would allow us to deem a musical composition unified—my reflections on the various accounts of such criteria that had been forwarded seemed always to lead me down one blind alley or another. In the pages that follow, Sections II through V trace out the salient points of these reflections. At one time I took these negative results to be a sufficient refutation of the very notion that unity was important, or that it was even intelligible as a concept. I would have said that to believe that unity was important was to commit the Charlie-the-Tuna fallacy: it is Charlie, recall, who is forever confusing “tunas with good taste” with “tunas that taste good.” And I was convinced that it was inappropriate to look at a work as “a satisfying whole,” but instead, that the best we should say was that “the whole work was satisfying.”

My epiphany came to me over a time when, having the good fortune to spend several months in Paris, I would attend, each Saturday night, the free organ recitals that were held at the Eglise St. Séverin. Normally the organist-du-jour would follow a roughly chronological sequence, starting from the very old and working his way, ultimately, to the very modern. I had very little idea who the earlier works belonged to (they often provided me with an
What Kind of Theory Is Music Theory?

What a pity it was to examine the centuries of different architectural styles laid alongside one another in the church, but eventually we would arrive at something by Bach. At that point I knew where the organist was, chronologically, and lost track of where I was, geographically, being drawn entirely into the Realm of Music. After Bach, things began to dissolve again, and I found myself once more, both literally and figuratively, in the dark. Eventually I began to ask myself “What is it that Bach accomplishes that all these other composers fail in varying degrees to accomplish?” The answer I arrived at ultimately was that with the Bach works, from the first note to the last, all the musical forces were united in making a single, whole, self-sufficient utterance. Since I had already refuted to my satisfaction what I had taken to be the major accounts of unity, and yet so strong was my intuition as to the grounds of the success of these works, I could not resist concluding that somewhere out there the adequate account was waiting to be found. Ultimately, the key to its discovery lay in shifting the focus of inquiry away from objective criteria and instead toward subjective criteria. Before pursuing the various lines of thought suggested above, however, let me make explicit the primary presumptions and intuitions that underlie this analysis.

A. There is a real, although vague, hierarchy of values in the musical realm—that is, some music is better than other music. A variety of countervailing facts (yes, facts) speaks loudly against the usual protestations of “You can’t judge …” or “It’s all relative. …” Here are a few of them.

1. Some works are performed often in concert halls throughout the world, some are performed occasionally, some never. This is not to recommend making a simple compilation from a stack of symphonic programs over a given period of time and declaring a winner—if only the matter were that simple! But it does indicate the existence of certain general lines of preference detectable across boundaries of time, place, and culture, and value and preference are hopelessly—though problematically—connected.

2. Some works are chosen again and again for study in classrooms everywhere—the Eroica is a fine example (and a fine symphony)—while others are unlikely ever to make it through the door; thousands and thousands of others.

3. It may never be sorted out whether Bach is a greater composer than Mozart, but neither of these musical giants faces any real competition from Liszt or Delius or Froberger. Anyone who places these latter individuals at

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1 Is there any other realm in all of art where one individual stands apart from every other artist in a fashion like this, where we can say “Well, we have two kinds of artist here: X, and all the others”? Rather like “Michael Jordan and all the other guys on the court,” or “Greg Louganis and all the other divers.”

2 I believe there actually is much to be learned from studying some massively mediocre works alongside a great one, but mine seems to be the minority view in this regard.
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the top of their list is thoroughly suspect (or perhaps a distant descendant). The same point could be made concerning individual compositions.

(4) While there may not be any “right” way to perform certain (most) works, there are certainly “wrong” ways; it is possible, and sometimes rather easy to distinguish between an adequate, spirited, or inspired performance and an inept one. John Williams’s rendition of the Asturias by Albeniz may not obviate all further performances of that work, but it certainly provides any future performer with a paradigm to be lived up to, or not. Singers have long been well-advised to approach with caution songs sung by the Beatles, or Elvis, or Barbara Streisand. I could go on.

B. Music theory, properly so called, emerges from, and is thoroughly animated by our endeavors to understand or at least make some sense of these valuations that permeate the musical realm: music theory is normative to the core.

It may be claimed that there are non-normative studies of special relevance to music theory, studies in the physics of sound production or the physiology of sound reception, that bear the look of non-normative objectivity. Such studies, however, only contribute to music theory when they are applied to the question of why we appreciate some music more than other music and thereby place a higher value on it, or from the standpoint of creation, why certain composers or performers chose to actualize this set of sounds rather than another.

Consider, as a potential example of a non-normative fact directly pertinent to musical analysis, the simple determination that the pitch called A will be produced by a string vibrating 440 times per second. What could be more objective than that—the point where mathematics, physics, and music collide? Well, one relevant point would be that the pitch known as A in the mid-eighteenth century would have been produced by a string vibrating not at 440 times per second, but at 408 times per second. What does this say of that three-way collision? What happened between the mid-eighteenth century and the present day to produce such a change as this? The answer to this question leads straightaway into questions of value: certain instruments, wind instruments in particular, exhibited a more brilliant and pleasing sound when pitch levels were notched up a bit, then they notched them up a bit more, and a bit more, and so on. Pitch just didn’t float upward of its own accord—it was taken upward by people—composers and performers, in collaboration with instrument makers—who found a generally higher pitch to be more satisfying to the ear, or more suitable to purposes of dramatic expression.

An analogy from the realm of visual art may be helpful here. Let’s say we describe a painting such as van Gogh’s Starry Night as being roughly 78

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3 Or latter day townsfolk, as the good people of Büttelstedt have seen fit to resurrect Johann Friedrich Fasch from Total Oblivion and seat him upon the throne of Mediocrity.
centimeters in height and 92 centimeters wide (a close to correct description). Now this seems to say something totally bland and objective about the painting. After all, this consideration taken entirely by itself would place van Gogh and thousands of other artists on precisely the same footing—hasn’t every artist you know produced at one time or another a painting that was roughly 78 x 92 cm? It looks to be the kind of determination that would be of relevance only to UPS or the postal service: one can see a postal worker mechanically measuring the case in which the work was being shipped and coming up with a figure as to what it would cost to overnight it to the Met. His eyes would only light up when he got to the point of asking whether you’d like to insure it, and for how much!

When abstract expressionism was at its height in New York, however, arguments were made by noted critics to the effect that works of the sort that De Koonig or Pollack were producing had to be carried out on a grand scale—that the very style itself required a certain visual expanse in order to make its effect. Size seems to have some normative relevance, after all. In fact, however, those particular arguments were mistaken, but to determine as much, one must be willing and able to cast a gaze beyond the asphalt confines of New York and take a look at the French abstract expressionists—Bissière, Manessier, Estève, Soulages, to name a few. These artists worked extremely effectively in much smaller formats. The mere fact that I offer such a counterargument, though, further underscores the normative considerations embedded in the question of (seemingly) brute dimension. Now let us suppose we are walking through a museum of modern art, and we come across a work that spreads itself across an entire wall, yet which offer little visual appeal, and may even seem offensive, or (more likely) positively silly. And suppose then that we pass straightaway into a room in which Starry Night stands in exhibition: we will find ourselves overwhelmed by the power of this work—and it’s only 78 x 92 cm!

I have experienced somewhat the same sensation listening to Mozart, who could at times achieve the largest effects through the smallest of gestures. The E♭ that falls from the heavens and lands in measure 14 of his Ave verum corpus, for example, has been making listeners’ hearts flutter and heads shake for centuries, while many a latter day composer has summoned massive orchestral forces in dogged pursuit, ultimately, of the musically trivial. Chopin’s B-minor prelude lasts barely a minute and a half, but for all its brevity—perhaps because of its very brevity—it contains more poignant moments than entire Dvorák symphonies (and there are plenty of less significant composers than Dvorák).

These considerations are introduced here to demonstrate how factors which bear the look of “pure objectivity” can and do have normative relevance, and indeed to suggest that the closer we look into such matters, the harder it becomes even to draw any line between the objective and the nor-
Let us now give consideration to some of the more venerable objective accounts of unity.

The programmatic account

It is claimed at times that an extramusical program of some sort is capable of uniting what otherwise would be heard as a disparate or disjointed series of sounds. Such a program may be supplied by the composer, as with Berlioz’s *Symphonie Fantastique*, in which we are told that the music represents different phases in a man’s imaginary infatuation with a certain woman. How else are we to bring together a pastoral setting, a ball, a walk to the gallows, other than to be told ahead of time how these situations relate to one another? Mussorgsky’s *Pictures at an Exhibition* provides a similar program, in which each movement represents, well, a picture at a particular exhibition of paintings. The work offers musical depictions of various pictures in said exhibition, and even provides a representation of the composer himself meandering from one canvas to the next.

Even where a program is not supplied by the composer, however, we may be prompted by the music itself to supply our own description of the succession of states it could well be taken to depict. There is no program, for example to the second movement of Beethoven’s op. 111 piano sonata, but I could supply one on demand; it would go something like this: from the insouciance of childhood we are led through the jauntiness of youth then through the enthusiasm and energy of the mature man, only to arrive at certain self-doubts, which grow into cosmic doubts, which lead, after great inner turmoil, to a joyous transcendence of any and all worldly preoccupations. I don’t insist on this reading, but just the same I am prepared to supply references to the sections that lead me to it—even to the very Depth of Despair measure itself.

Again, Leonard Bernstein once “explained” the interplay between piano and orchestra in the second movement of Beethoven’s fourth piano concerto as suggestive of Orpheus taming a wild beast with his lyre, and indeed the

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4 Along these lines R.G. Collingwood argued against the emotive-descriptive dualism that the Logical Positivists erected to hold scientific discourse separate from poetic language, showing how emotionality pervaded science (see his discussion on “The Logical Analysis of Language,” in Collingwood 1958, 259–68). John Dewey likewise strove continually to break down similar dualisms, as, for example, in the following passage: “Only the psychology that has separated things which in reality belong together holds that scientists and philosophers think while poets and painters follow their feelings. In both, and to the same extent in the degree in which they are of comparable rank, there is emotionalized thinking, and there are feelings whose substance consists of appreciated meanings or ideas” (1958, 73). The fact-value dualism appears equally susceptible to considerations of the sort raised by these two thinkers.

5 A touch of realism may be added when we learn that Berlioz himself underwent such an infatuation, but the work stands on its program, it would be contended, regardless of such a consideration.
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orchestra does storm about at the outset only to become increasingly re
strained after each gentle declaration by the piano until in the end both play in piano. This observation was offered at one of his Children’s Concerts, so he was obviously looking for some means of getting a very young audience to attend to the music at something more than the note-by-note level, but still it seems quite in order.

Now the question is: can any such program, whether actual (that is, sup
plied along with the work itself) or hypothetical, actually impart a sense of unity to a composition, which it would otherwise lack? For several reasons I believe the answer must be negative.

A. Let us look first at those programs, which could actually be thought to come with their own certification of unity. Romeo and Juliet, for example, can be and has been set to music, and since we already know the story and are aware of how Shakespeare wove it together, our minds may be disposed to read Shakespeare’s own unifying elements into the music we hear. But we are hardly obliged to do so. It may well be that the “program” offered by Shakespeare did act as a stimulant to the composer to impart a sense of unity to the musical version, but not all composers are of equal merit, and some may just not be able to follow the tensions and resolutions presented by the play and impart them to the music. If Prokofiev succeeds, it is because of his skill as a composer, not Shakespeare’s skill as a dramatist.

Bach’s St. Matthew Passion comes with a script—the gospel—which is undeniably serious, coherent, and dramatic: indeed it possesses all the elements of a finely wrought tragedy. But does the gospel itself bring a sense of unity to this work? Again, it seems not. In fact I suggest that of the myriad of musical and textual elements that combine with one another in the St. Matthew Passion, the words taken from the gospel are among the least responsible for imparting any such sense of unity.

From the first few measures of the opening chorus we derive a sense that something of immense importance is about to unfold, a sense that only aug
ments with the actual entry of the chorus itself, then the second chorus, and continues unabated through the entire movement. A beginning of this magni
tude requires serious development and closure to “bring the work home,” and it receives just that in the numerous arias and chorales that lead us through the work and to the final, crushing chorus. These are the elements principally responsible for carrying the dramatic flow of the Passion. They do follow a pace set by the recitative (which sings portions of the gospel), yet if the “highlights” of this work were to be excerpted and collected, as they sometimes are, precious few of them would consist in recitative; and if the recitative were presented in isolation, the work would surely lose its

6 Of course this explanation has ruined the movement for me forever, by rendering it obvious, transparent or predictable, instead of suggestive, obscure and rich with surprises, as the best music ought to be.
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It is clear that our emotional experience of the Passion, and the sense of satisfying wholeness it produces, results from the manner in which Bach has harnessed a rich variety of musical forces, and not from a “simple” story line.

A song by the French singer Francis Cabrel was working its way across the airwaves in France back in 1994. It was entitled La Corrida, and was, in effect an account of a bullfight through the eyes of the bull. Now that sounds like a single thought—one which could form the base of a unified work. And yet the song was two distinct songs: the first section was done in an ambling, Dylanesque, too-many-words-per-phrase-but-who’s-counting style; this abruptly gave way to the hoarse, strained incantation that one associates with the flamenco vocalists. In fact, Cabrel stepped aside at this point and let one of the Gypsy Kings finish the song. Now only because one heard the two in frequent juxtaposition could it be judged that they were somehow meant to be associated in the same song. Try as they might, the lyrics, however coherently they might have read, could not, when heard, hold these two radically disparate elements together. Unity was forced to surrender to duality.

B. Not all actual programs, however, even seem to contain any strongly unifying tendency. In the case of Berlioz one can easily wonder, why go straight from the ball to the pastorale? Why from the pastorale to the gallows? Why have a pastorale in the first place? And why finish with that dream of a Sabbath night? After all, not many folks do much dreaming of any sort after a trip to the gallows. The program, quite evidently, consists of one loose end after another. If it is held together at all, or to the extent that it does hold together in any satisfying fashion, it is the music that deserves the credit, not the program.

The same could be said in the case of Mussorgsky’s Pictures. What unifying element could the program itself offer except that all the pictures depicted are being viewed by the composer, who at least is self-identical? More than two millennia ago, Aristotle offered the sage pronouncement that of all the unifying devices at the disposal of a dramatist, the episodic was the worst—a harsh judgment, perhaps, but not a groundless one (Aristotle, Poetics, 34). And surely Mussorgsky’s program is just that: a single character taken through a series of events, in this case viewings, that in and of themselves suggest nothing of the beginning-middle-end structure which a truly unified work possesses. Why view and “comment” musically on just these pictures rather than others? Why present them in this order—why not finish with the old castle or the chickens hatching? Why not place a promenade theme between each viewing, rather than just a few, or, for that matter, why not end with a long promenade theme, as the viewer/composer collects his hat and coat and heads off into the night? These are questions that the program poses but is powerless to answer. Yet Mussorgsky’s music satisfies us, leaves us content with the journey he has led us through, seems to hang together as a coherent whole. It does so on its own, it appears, and not with the drama, and probably even its coherence.
help of any program. In fact the succession of pictures owes what intelligibility it has to the music; it is only during the listening process that it becomes obvious why we finish with the Great Gate and not the Hatchlings or the Ox Cart, and why they occupy the places they do.

Likewise, Mozart’s *Magic Flute* can provide us with as satisfying a sense of wholeness and completeness as any work ever written—but I would advise listeners to keep their librettos folded in their lap, and maybe even keep their eyes shut! Such grand music and such frivolous themes! We have a hero, Tamino, who is menaced by some dragon-like creature, and would surely perish were he not rescued by three ladies who decide, despite his having demonstrated no prowess at anything but fainting in the face of danger, that he is just what their queen needs to rescue her daughter from some sort of bondage. Alongside this action, a silly birdcatcher, Papageno, tries to take the credit for saving Tamino and gets a lock put on his mouth by the three ladies for his prevarication. The queen sings a magnificent entreating aria, all about a mother’s love, to induce Tamino to take up the challenge. It turns out, though, that the queen is really looking for her daughter in order to convince her to kill her father, Sarastro (high priest of Isis and Osiris!), who happens also to be the queen’s husband, hence her beautiful aria turns out to be a study in hypocrisy, when we juxtapose it to her equally famous second aria, in which she is wishing all manner of evil on Sarastro. Sarastro takes to Tamino, too, enough so that he is willing to have him marry his daughter and ultimately inherit the kingdom over which he presides. But first, Tamino must prove his worthiness by enduring three tests of courage and character, two of which involve keeping quiet, while the third involves passing through some perilous, fiery catacomb—but by that time he has come into possession of the magic flute, so what does he have to worry about? He just plays his way through the catacombs, and all is well. Papageno’s nuptial adventures help the music phase down its intensity toward the end; the evil queen is throttled one last time and everybody who matters lives happily ever after.

If ever a story has been taken beyond itself by the music that accompanies it, it is this one. Bach may well have needed the gospel in order to bring his genius to focus, but Mozart’s genius appears to soar indifferently over its feeble text (or perhaps it hauls it along behind, like one of those airplanes that fly up and down along the seashore, dragging banners proclaiming the virtues of this or that suntan lotion).

C. As to hypothetical programs, such as the one I supplied for the Beethoven sonata, I must point out that this program in no way preceded or conditioned my appreciation for the piece; for years I had been strongly moved by it, years during which neither that nor any other program ever suggested itself to me. If anything, it could actually interfere with my response, in the way in which, as I observed (in note 6), Bernstein’s comments have impeded my appreciation of Beethoven’s fourth piano concerto. Should someone else offer an alternate interpretation of the sonata, I can easily see myself accept-
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ing it, but I can’t see my appreciation of the piece being augmented or diminished by any such alternative. In the listening process, the succession of tones overwhelms imagery; the music itself has its tendencies, directions, and that is where our attention is directed. Consequently, I judge that even where a program fuses with a work in the imagination of the listener it is not this program which supplies any sense of wholeness or unity to that work.\(^7\)

The conclusion to which these reflections lead me is this: for any musical composition, if there is in any respect a program accompanying it—or perhaps a libretto, or a title, or just a scenario imagined by the listener—it is the music itself which will (or will not, of course, depending on how successful it is) impart its own unity to the program, and not vice versa.

The tonal center

Much has been made of the power possessed by a tonal center to hold a work together, thereby endowing it with unity. Even if a composition should, in the course of its unfolding, appear to stray from such a center, its return “home” could be counted on, indeed for the period stretching from the very early eighteenth century to the very late nineteenth century, the orientation of a piece around a fundamental tone was a virtual inevitability. The renowned German theoretician Heinrich Schenker based his method of analyzing the ebb and flow of tones within a given piece precisely on that desideratum.

We needn’t tarry too long, however, in refuting any claims to the effect that the (underlying) tonal structure of a work contains the secret to its unity. For one thing, the mere fact that virtually all works within the aforementioned period of western music do exhibit the feature of orienting themselves around a tonal center shows that to do so brings with it no guarantee of unity, since it is likewise a fact than an enormous number of works from this very period fail to impart any overpowering sense of unity. Most popular songs that are born, die a natural death, and proceed straight to oblivion never stray from the key in which they were conceived and executed. But if they had impressed on listeners a sense of unity, they would likely have enjoyed considerably greater longevity. We can provisionally conclude, at best, that tonal orientation may be a necessary condition for unity, but it certainly is not a sufficient one.

But is it even that? There appears to be no overpowering reason why a skilled composer could not provide most of us listeners with a sense of that

\(^7\) Indeed, it has long appeared to me to be the case that those individuals who seek out programs, who supply them themselves or who endeavor to discover by various means what any composer might have “meant” by this work or that—or this melody or that—are not all that sensitive musically to begin with, and would be better advised to remain within the domain of the written word (though even there, specifying with any precision what a poet meant is hazardous business).
“return home,” while nevertheless depositing us in a quite different tonal neighborhood. Only those unfortunate few who suffer from perfect pitch would be any the wiser. And surely many a successful piece has been written in the past century which, while remaining faithful to the principle of relating individual tones around a center, adopts no particular key signature, shifts centers from time to time during a single work, and ends, well, where it ends. Thus it appears that tonality, and all that it entails, provides neither a sufficient nor a necessary condition for the unity of a composition.

Still, it is undeniable that much music does exhibit this tonal orientation. Why is this the case? I would suggest that it is primarily because a tonal center offers a stable framework within which a composer’s ideas can unfold: a place to start and a goal to reach, with a limited range of conventional points of interest to reach along the way. Within such a framework, the array of choices a composer faces is reduced to a manageable number. How incredibly more complex composition becomes when the composer must trouble not only over how to reach a certain goal, but what goal, exactly, is to be reached, with each possibility bearing a different complex of choices, any and all of which would now be enveloped within the larger question “Why?”

If the initial choice of a tonal framework automatically prescribes, say, a return to D major, then the mental and emotional energy of the composer can be directed, almost unconsciously, toward arriving at D major in an interesting and dramatic fashion. If no particular tonality is prescribed, then questions immediately arise as to where the work ought to go, and these questions provoke further questions, the answers to which would involve justifying in some manner the choice of this one key over twenty-three others—or perhaps over no tonal resolution whatever.\(^8\)

Artists need freedom, but absolute freedom can be every bit as burdensome as enslavement to externally prescribed norms: fixed points of orientation are vital to measuring the flight of artistic freedom—even to the artist. Too precise and detailed a structural map, specifying just what points are to be reached, and when, will lead to predictable, boring art. The absence of any map at all, however, conduces only to silliness. Imagine how much Dostoevsky was capable of expressing within the strictures of czarist Russia! Compare such accomplishments with the expressive content of a Howard Stern show, which indulges in the luxury of near complete freedom, and weep.

Just as tonality offers creative benefits to the composer, it may offer certain auditory benefits to the listener, as well. Perhaps a distinct tonal center does provide a certain comfort to us, and facilitates the forming of expecta-

\(^8\) It is interesting to note that the development of atonal music involved the development of the atonal system of composition. Schoenberg realized that one couldn’t just dissolve all reference to a tonal center and leave it at that. The critical failing of atonality thus became not its lack of a system of order but the utter arbitrariness of this system.
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tions and the (subtle psychological) evaluation of their successive confirmations and disconfirmations. It undeniably enters into the listening process at an important level. And yet it hardly can be seen to bear the burden of lending unity to a piece. In shorter works, such as the dances within a suite, we may well retain a sense of “home base” throughout an entire dance, even where there may be a modulation to the dominant or relative major toward the end of the first section, and unresolved disruption to this sense of ours may well lead us to judge ill of the piece. In longer works, however, it is much easier to lose sight of home, hence the obligation set up by the initial statement of key can easily be effaced as the work progresses. In sum, then, tonality offers great benefits to the composer, and definite but limited benefits to the listener, but it carries with it no guarantee of the unity of any composition.

The thematic approach

That a theme or motif may bear the responsibility for unifying a composition is both a modern and an ancient notion. In recent times, the most detailed articulation and defense of the thesis was offered by Rudolph Réti, in his book *The Thematic Process in Music* (1951). Réti focuses on this tendency, one that seemed to intensify during the Romantic era, but which was plainly evident even in Baroque times. In truth, though, the theme and variations, as a mode of musical development, may well be as old as music itself, and the persistence of a given theme through a variety of transformations is precisely the sort of unifying device that Réti is concerned to highlight.

The idea that a melodic theme or a rhythmic motif can impart, through its recurrence, a certain sense of unity to a composition is a tempting one. And yet I contend that it is vulnerable to criticism from a variety of directions. Actually, the criticisms I have in mind follow the lead taken by Aristotle (in a somewhat different context), when he declared that “a plot is not unified, as some people think, simply because it has to do with a single person. A large, indeed an indefinite number of things can happen to a given individual, some of which go to constitute no unified event; and in the same way there can be many acts of a given individual from which no single action emerges” (Aristotle, *Poetics*, 31). And similarly, we may judge that a large, indeed an indefinite number of uses can be made of a single musical theme, without in any way producing a unified work. It is not enough just to say so, however (something that Aristotle occasionally overlooked), so let us present some more specific considerations.

Much more recently than Aristotle, Leonard Meyer, in his book *Explaining Music* (1973), laid bare many of the problems that attach themselves to
any use of the thematic process as an analytic tool. He argues “methodologically, if one can pick and choose—selecting those voices or pitches which support one’s hypothesis, and disregarding those which do not … then almost any melody can be related to any other whether within or between works” (62). Such relationships, which he terms “conformant relationships,” are only, in his judgment, “of secondary importance for creating unity in the repertory … of Western tonal music from 1700–1918” (66). Rather, he claims, “all the kinds of relationships present in a composition—processive, tectonic, ethetic, … as well as conformant ones—contribute to the impression of unity” (ibid.). While endorsing Meyer’s critique of Réti’s methodology, I nevertheless cannot accept his revised account. However broader and more encompassing it may seem, it still aims, in my estimate, in the wrong direction. But first, let us consider some of the primary reasons why the mere presence, or even the omnipresence of a particular theme cannot be assigned responsibility for unifying a work.

1. Thematic conformance can occur without occasioning any thought of compositional unity. We need only turn to Television Theme Music for examples of this: Dallas, say, or Leave it to Beaver. In all likelihood, the average viewer is quite unaware of the ubiquity of the single theme that undergirds the background music in these shows, and if made aware, couldn’t care less. (I employ the present tense in speaking of these shows because, well, what with cable and all, they are always with us.) But whether jaunty and insouciant, excited, sentimental (Beaver); or urgent, ominous, triumphant (Dallas); somehow the same theme finds itself recast to fit each of these emotional situations. Should an episode of either ever make any pretense to dramatic unity, such a pretense would surely not be grounded in the recurrence, however frequent, of the primary theme music.

2. Thematic conformance may actually repel a sense of unity. To show that this is not as shocking a contention as it may sound on first hearing, let us hypothesize, instead of the theme and variations mode of composition, one that would be similar, yet drastically different: the theme and repetitions. Suppose a work were to state a theme then restate it; then re-restate it; and re-re-restate it, ad nauseum. In no time, we could well find the listeners to such a work (a) laughing at themselves for being duped (the volume of laughter varying inversely with the amount they paid to be in attendance); (b) angry at the composer or the musicians for inflicting such a joke on them; or (c) just bored to tears, thinking to themselves “how long can this go on?!?” Certainly no one would be gripped by any sense of unity emerging

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9 See, in particular, Chapter Three, on “Conformant Relationships,” 44–79. Réti is treated from 59–79.
10 I swear—though I don’t know how it could be—that I once heard the theme to Hart to Hart somehow twisted into a perfect (and eerily beautiful) twelve-tone row!
from the “composition.” The simple moral here is: unity does not lie in uniformity, at least no artistically relevant sense of unity.

3. Relationships of thematic conformance can often go unheard, even to listeners of reasonable sensitivity. Can that which is unheard play any active role one way or another in an auditory art?

How many of us have been surprised to learn that that most rhapsodic variation among Rachmaninov’s series of variations known as *Rhapsody on a Theme by Paganini* is really little other than the principal theme stood on its head? Of course the tempo is altered, the ethos transformed, but the notes and the sequence they follow reveal their connectedness. Were we all “missing the point” until such time as we were informed of this connectedness? Will we even hear the variation differently, once we are apprised of its mode of origination? My answer to both questions would be “doubtful.”

Can the average listener—even the deeply appreciative listener—put a finger on precisely where any number of the variations that comprise Bach’s famous *Chaconne* correspond to the theme that spawned them? The richer the theme and variations, it appears safe to say, the greater the difficulty anyone would have in keeping such an account ledger in order.

4. Radically nonthematic elements can be integrated (by the right composer, of course) into works that are heard as successful wholes. Or at the very least, works perceived as whole and unified may still disclose no particularly strong effort to strive for thematic conformance.

Consider, along the first of these lines, the third and last movement of Mozart’s violin concerto in A major, K. 219. Composed in rondo fashion, the A theme is delicately and delightfully “Mozartean,” and on first hearing it, one is ill-prepared for the “outrages” that are to follow. Yet before the movement is over, a “Turkish March” imposes itself into the structure (borrowed from an earlier work of Mozart’s, the opera *Lucio Silla*). It arrives in the D position of the rondo and storms about for a considerable time, until the initial theme is allowed to return. After that, Mozart even treats us to the B theme once more then returns to the A for closure. One would be hard pressed to find any ground of conformance between the D section and the principal one, but in the end, the general verdict seems to be that its intrusion is delightfully disturbing, regardless how thematically irrelevant it may be—indeed, perhaps because of how thematically (and melodically, and ethetically) irrelevant it is. It does appear that, of the cluster of violin concerti that Mozart produced in 1775, this is the most popular; at least it is the one that is most frequently performed. Either the unity of the work is of little importance, or unity is an attainment that is not grounded in thematic conformance. Mozart would surely opt for the second alternative. One can notice how he “paves the way” for the intrusion of this march in the C section of the rondo, which does have some dark overtones to it; how he eases us into it then eases us out of it, then reaches a closure of the sort we had been expecting all along, but deprived of for a time. There are many musical scenarios in
which a section of this sort would leave us with an unsatisfactory dichotomy of styles (see my example of the Cabrel song *La Corrida*, mentioned above), but Mozart does not allow himself to fall victim to any of them.

Consider, again, Bach’s organ fugue in C minor, BWV 537. A work of this type is sometimes referred to as a double fugue, since midway through it, a second theme barges into the development section and takes the work to an entirely different dramatic plane. The second theme, consisting only of a series of chromatically ascending notes:

![chromatic ascending notes]

can hardly be wrung out of the principal theme, no matter how strong one’s theoretical forearms might be:

![principal theme]

And yet it ratchets up the dramatic impact of the work, leaving us with a whole entity that is much the greater for its intrusion.

As for works that make no strong effort even to achieve thematic conformance, Chopin’s Ballade, Op. 23 is hardly tight-knit, thematically, yet very effective dramatically. A suite of dances by anyone from Bach to Tansman can satisfy us with its wholeness, without belaboring any particular thematic material. And certainly the larger the work, the greater the need for a variety of thematic sources, not just a single one.

5. It seems perfectly reasonable to find unity embedded in a single theme, which of course would completely be at odds with any thematic account of unity. Certain of Chopin’s preludes involve hardly more than the elaboration of a single theme, yet they seem surprisingly complete. I would cite those in A major, C-sharp minor, and B major and minor as just a few examples of this phenomenon. Baroque themes are often broad and elaborate enough to leave us with a sense of completeness even before being subjected to any development by the composer. The opening themes of Bach’s violin concerti in A minor (BWV 1041) and E major (BWV 1042) come readily to mind in this regard, as does the initial theme of the Fifth Brandenburg Concerto, as well as the themes that open innumerable Vivaldi concerti (which apparently proved to be a great source of inspiration to Bach himself).

To put my contentions here to the test, I propose that we examine a work that has every chance of making a sound case in favor of a thematic account

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11 Bach’s dances may, and often do, start out with a pair of notes before opening into the dance proper. This hardly seems to qualify as thematic conformance, though I have seen people argue that it does.
of unity, for it is (a) unquestionably unified, and (b) deeply thematic in construction. The work is Bach’s G-sharp Minor fugue from the Well-Tempered Clavier, Book I. While not one of his most famous works, I find it nonetheless to possess considerable charm, emotional interest, drama—all the qualities so often embedded in Bach’s compositions. As for its thematic cohesion, quite simply, the counter-subject(s) and each and every episode throughout the work have been carved out of the original fifteen-note subject. Here is that subject in its initial statement (I assign a number to each note, below it, for ease of reference):

![Main Subject](image)

Then follows the counter-subject, again, as it first appears (in measures 3–5):

![Counter-Subject](image)

Clearly this counter-subject has been generated from notes 3–5 of the main subject, repeated sequentially, then with a small cadential formula at the end, reminiscent of the cadential formula in the main subject itself, though not identical to it.

The first episode to make its appearance arrives at the end of the exposition (measures 9–10), playfully prolonging that exposition, rather than offering any contrast to it:

![First Episode](image)

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12 I may be underselling the importance of this work to Bach himself. It happens to be the only one of his 48 fugues in the Well-Tempered Clavier to contain exactly 41 measures, and the number 41, to Bach—numeromaniac that he was—held great importance: it is the reverse (right-left, left-right) of 14, which is the sum one derives from B–A–C–H, where the letters B=2, A=1, C=3, and H=8 (their relative positions in the alphabet; in German notation, B=B♭ and H=B♭). I do not believe there is another of his fugues in which every bit of the subsidiary thematic material can be so readily drawn out of the original theme. So it could well be that this is the game that is afoot in this particular fugue, and it is not a game that Bach took lightly.
This episode unambiguously derives from notes 10–15 of the initial subject, arranged in an ascending sequence. It recurs again soon (measures 13–14), this time arranged in descending sequence, and under a more sparse treble, with one of the voices silent. Its final reappearance comes much later (measures 30–31), by which time it no longer has anything of the sprightly feel it had at the outset.

Soon after the second appearance of episode 1 (measures 15–16), interlaced with the main subject, arrives what may best be thought of as a second counter-subject:

![Counter-subject](image)

Clearly the rhythmic impulse for it derives from notes 3–5 of the subject (as does the first counter-subject), and yet it can easily be contended that the first six notes are in fact notes 2–7 of the subject, in retrograde. This counter-subject only occurs one other time—in the thick of the climactic gestures of measures 32–33.

Episode 2 occurs in measures 17–19:

![Episode 2](image)

Unlike the first episode, which attaches itself to the main subject and carries it forward, this episode offers something of a contrast with preceding material. It draws from notes 7–10 of the principal subject, though while note 11 moves upward (from F♯ to G♯) in the subject, here the fifth note falls downward (in the manner of notes 6–7, actually, but there is no need to force conformity). In measure 17, though, the upward pairing is turned upside down, and now descends. Still, the contribution of notes 8–9 is plainly felt.

An even more disruptive episode (the third) follows soon hereafter, from measures 21–23:

![Episode 3](image)
Here a fragment is tossed back and forth between the bass and the treble, and it appears to derive from notes 1–5 of the main subject, except it consists of six notes itself. But it is easy to see that one note has been interposed between notes 3 and 4—one which falls off a third (major or minor, depending on the context)—after which the normal progression is resumed. And of course the rhythmic values have been altered. This episode recurs in measures 28–30, with the episodal figure this time being passed back and forth between two upper voices.

Therefore, if we do the math, we can see that every bit of the main subject is put to some use, either in counter-subjects or in episodes—not a single note is left out! (It is as if this was the little intellectual task that Bach set for himself to complement the dramatic flow of the work). And of course throughout the 41 measures of the piece, this all runs alongside the twelve appearances and reappearances of the principal theme itself. Consequently, only ten measures can be found that have no original thematic material, and naturally they are dominated by one or another of the derivative sub-themes. Our question, then, becomes this: does this thoroughgoing permeation of the main subject through the entire work constitute the source of the unity which this work exudes? My contention is that it does not.

To demonstrate this contention, let us suppose that the various interludes that occur in the fugue were in fact not derived from the original theme (a point raised in ¶3, above). Would the unity of the work be at all compromised by such a possibility? I suspect not. For one thing, in the case of my own response, I was not really aware that the interludes were one and all derivable from the main theme until I sat down with the music, away from the keyboard, and made some comparisons. That is to say, my ears did not immediately relate to me the connections between theme and interlude—my eyes did. And yet long before I took the trouble to seek out any such relationships, I was appreciative of the work, and felt that it held together as a whole. Where did such a sentiment come from? Let us look a bit more closely at what happens in the work.

The exposition unfolds in four voices over the first eight measures of the work. (Of course an exposition, by its very nature, is an opening up, a complexifying, as one layer of melody then the next asserts itself.) In measures 9–10 the first episode extends the thematic material in a somewhat jaunty fashion, as can well be expected at this stage of the work. The episode of measures 15–16 begins to intensify the feeling of the work, while measures 17–18, bringing yet another episode involving some perilous melodic leaps and falls, lead us into more serious dramatic territory. This is underscored by the abrupt entry of the four notes, $D\#–E–F–G\#$, in the bass line in measure 19. If we weren’t paying attention to this point, we are now. In terms of time elapsed, we are now at the half-way point of the piece, but time-on-a-clock and time-in-a-musical-work are simply not the same time (time-counted-out
as opposed to time-felt). The harmonic-contrapuntal complexity that is initiated here exerts a considerable stretch on time-heard.

The episode that follows is more disruptive yet, as a figure (see episode 3, above) is passed back and forth between the bass and the treble for three measures. Clearly, the harmonic relations in this section have become much more dense; dramatic tension is building. It bursts, in a manner of speaking, in measure 25, with the cascade of notes that washes over us, and washes away much of the intensity of the earlier measures. At once the notes high up in the register have lost their stressful feeling, and now suggest joyfulness or triumph. With a genius like Bach, however, triumph never comes without a struggle. We are led to an episode of the same sort that we found so unsettling in measures 21–23, only now, in measures 28–30, its character has changed: the episodic figure is passed back and forth between the upper voices, while the bass rolls out a pattern that communicates resoluteness—the end is nigh. This episode terminates in another episode similar to the one we confronted “long ago,” just after the exposition—the sparse cadential formula—only it is less sprightly now, more ardent about bringing things to a close.

As it turns out, we are to be thrown into the dark one more time, with the measures that follow—measures 32–33 (with one final surprise in the middle of measure 34)—being undoubtedly the most dense and agitated of the entire work. Immediately thereafter, however, the harmonic landscape brightens, and we are led to completion and closure. At the end, the deep bass notes, unlike their earlier appearance, offer not shock and disruption, but a sense of firmness and stability.

Now what I am suggesting here is that it is the dramatic flow of the work—as I attempted to outline it—that is responsible for our feeling this to be a unified artistic creation, and not the occurrence and recurrence of bits and pieces of thematic material here and there, as the work unfolds. Bach has taken these bits and pieces and employed them in the fabrication of a dramatic entity, and it is the sense of drama—the gradual building of tension; the successive gestures toward resolving it, etc.—that leave us with the feeling that we have dealt with a work that can rightly proclaim its artistic unity.

Why do composers, as Bach often did, spin subsidiary material from one original theme? There are various reasons; some have little or no bearing on the dramatic unity of a work, some have a definite bearing, but not for their thematic conformance simpliciter. Let us view the matter first from the composer’s perspective, then the listener’s.

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13 Compare the emotional significance of the two prolonged trills in Beethoven’s op. 111 piano sonata, the first struggling, searching, reaching out, leading up to the heavy climactic moment in which bass and treble find themselves several octaves apart; the second, at the end, rejoicing, strewing flower petals in the way of the spirit that has triumphed over its demons.
A composer’s ideas must come from somewhere, and if a given theme yields a plethora of ideas, so much the better for the theme. But from this vantage point, we are viewing the theme not as a source of musical unity, but as a source of musical ideas, which then must themselves be bound together into a dramatic unity (or not—success cannot be presumed to follow naturally). It would be excessively burdensome to devise a new theme for each step in the progression of a particular piece. And once a particular theme has consolidated itself in a composer’s mind, it is only natural to expect succeeding development to derive from the initial thematic impulse. The themes from Leave it to Beaver or Dallas that ring through each entire show enable the individuals in charge of scoring each segment to get their job done without devising a new theme for each plot twist, thereby freeing themselves to attend to other segments or other shows (or to how the ocean is lapping against the shore on a given afternoon).

From the point of view of the audience, the phenomenon of expectation is a powerful force in drawing out their emotions, and emotions are utterly crucial to the apprehension of the drama of a work. This point will be developed more fully in the next section; for the moment, however, let us consider the role that a particular theme can play in provoking an emotional response. All the parameters of music create expectations: once a rhythmic figure sets your toe to tapping, you expect it to keep your toe tapping. Harmonic relationships are practically defined in terms of expectations: lean heavily on the dominant, and we expect the tonic to follow, and the like. In similar fashion, a particular theme can dictate the flow of our expectations once it has etched itself in our mind—we need only hear the first few notes of it, and straightaway we find ourselves expecting the rest of it to follow. Sometimes it does, sometimes it doesn’t; the skilled composer knows when to deviate from our expectations thereby agitating our emotions, and when to gratify our urge to hear the theme in its entirety and set our emotions to rest. But make no mistake—many a successful coda brings our emotions to rest without drawing on any thematically conformant material.

Organicism

If unity is not to be imparted by any of the foregoing means (extra-musical program, thematic patterns, tonal structure), where else might we look in the hope of finding its ultimate ground? One last attempt deserving of our attention:

14 Meyer judges, “the study of such motivic modification may be vitally important in helping us understand the way Beethoven went about the act of composing” (1973, 78). Undoubtedly such a study would require us to “get into Beethoven’s head” much deeper than we could ever hope to. But surely, if we could, our understanding would be greatly enriched.

15 So much excellent analysis of this issue has been done by Leonard Meyer in Emotion and Meaning in Music (1956), and Music, the Arts, and Ideas (1967). I refer the reader to these works for discussions far more detailed than I can provide here.
What Kind of Theory Is Music Theory?

... tion is the view that holds that the (successful) work of art (hence, composition) possesses what is termed *organic unity*. Like the living organism—all of whose component parts make some necessary contribution to the survival of the whole being, none of whose parts are superfluous to this (monumental) task—the individual composition consists of innumerable elements, each of which makes, or should make, a necessary contribution to the unique being of the artistic whole of which they are parts: all for one; one for all.

I have elsewhere offered criticisms of the organicist position, which I believe to be no less decisive now than before (1994, 85–7). That critique, however, was offered in a context in which it was the rules of musical composition that were under consideration. Here I should like to offer a criticism of a different sort: namely, that organicism illegitimately objectifies its criteria of unity. That is to say, it places these criteria (however endearingly loose, as criteria, they may appear to be) wholly in the work, as if they could be discerned and pointed out in the same manner in which a physiology instructor would point out to a class where the pancreas is to be found, and what it contributes to human digestive functions. My contention—the contention toward which I have been aiming throughout—is that unity is a quality that can only be known when it is felt, and in order to feel it, one must, so to speak, enter into a work: one must engage in the activity of formulating, evaluating, and reformulating expectations; one must feel the tendencies embedded in the flow of sounds, be befuddled by periods of harmonic density, relieved to arrive at moments of clarity, surprised by the intrusion of a novel theme; one must long for resolution and take delight in its realization. In short, it is only in our emotionally charged experience of a work that we apprehend its unity, and the importance of this unity. To point out the singular importance of any harmonic sequence or melodic turn to those who, for any of a thousand reasons, may not be psychologically or emotionally disposed to experience the tensions and resolutions a composition gives rise to, will no more bring understanding to them than pointing to your slippers will prompt your cat to fetch them for you.

Although I have been speaking of the *elements* in Bach’s G-sharp Minor fugue that are responsible for lending it a sense of dramatic unity, I had no intention of suggesting that these elements were such that, when collocated, they would produce unity in a composition in the manner that the physico-chemical elements of quartz produce a crystal. In fact, the dramatic unity only emerges in the interaction between a work and its audience. And it was intentional, not through any descriptive carelessness that my characterizations of the moments and sections of that work rely heavily on terminology rich in emotional content. When Aristotle observed, ages ago, that a plot “must be so structured, even without benefit of any visual effect” (Hollywood take note!), “that the one who is hearing the events unroll shudders with fear and feels pity at what happens” (*Poetics*, 40), he was giving ex-
pression to the vital role played by the emotional response of an audience in locating dramatic structure.

Aristotle, with the special emphasis he placed on the necessity of unity to the successful work of art, may well be looked upon as the grandfather of organismism. (He did work out the constituents of such unity in some detail in the case of tragedy, but where other art forms were concerned he let slip little more than a suggestive remark here and there.) Nevertheless, unlike certain of his progeny, he never lost sight of the importance of the emotional engagement of the audience in appraising such unity.

In a recent article that seems to share much in common with the general spirit of the present one, Fred Everett Maus (1999) offers an account of musical unity that is grounded, not simply in the objective structure of a given composition, but in our experience of any such composition. He examines at some length, and strongly approves of the distinction, drawn by John Dewey in Art as Experience (1958), between experience-in-general and an experience. It is the notion of an experience that, for Dewey, possesses aesthetic relevance; experience-in-general refers to the intelligible but unordered sequence of events characterizing most lives most of the time. An experience occurs when a variety of elements contributes to and defines a particular event in a manner that makes it stand apart from those other events that constitute the quotidian flow, thereby asserting itself and its uniqueness.

Though I only mention it now, any reader of Dewey can see clearly how and how much his thinking resonates through my own outlook. But what is astonishing in Maus’s treatment of Dewey (all seven pages of it, and throughout Maus’s entire article, for that matter) is that there is not a single reference made to the emotional factor in experience. This is so striking that it must be intentional, and not just a casual oversight, since for Dewey emotion is not just an occasional or incidental element in an experience—it is an abiding and imbedded aspect of any such experience. In his own words:

I have spoken of the esthetic quality that rounds out an experience into completeness and unity. … In fact emotions are qualities, when they are significant, of a complex experience that moves and changes. … All emotions are qualifications of a drama, and they change as the drama develops. … Experience is emotional but there are no separate things called emotions in it (Dewey 1958, 41–2).

And a bit further on:

Physical things from far ends of the earth are physically transported and physically caused to act and react upon one another in the construction of a new object. The miracle of mind is that something similar takes place in ex-

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Dewey minus emotion is but a shell of Dewey. In “purifying” Dewey of any emotional element, however, Maus appears merely to be subscribing to a recent and widespread trend in aesthetics.

There was a time—and this time was hardly brief in duration—when any aesthetic analysis, regardless of its particular focus, made ample if not excessive use of the concept of emotion. Tolstoy’s *What is Art?*, for example, characterizes the artistic process as one that (a) begins with an emotion strongly felt, which then (b) is recaptured within the artist, who, (c) strives to provoke a similar emotion in an audience, thereby, when successful, (d) giving legitimate artistic expression to the said emotion. His account is only a bit over the line when compared to other aesthetic theories from neighboring periods.

It wasn’t until several decades later, though, when Suzanne Langer would proclaim that “music is not the cause or cure of feeling but its logical expression” (Langer, 1948, 176), thus urging that we purge our analyses of emotion-felt, and focus instead solely on emotion-represented. By 1970, the expression theory of art was all but dead. Certain thinkers (Alan Tormey, Peter Kivy, to name but a couple) sought to find, embedded in the language (being deferent, after the fashion of the time, to “the dictates of ordinary language”), some sense in which “emotion” could be said to be “expressed,” which did not require anyone, at any point along the line, actually to feel anything. If in fact they succeeded, it would have been only the willows that wept. What I am suggesting, however, is that it is in—and only in—an emotionally charged interaction with a composition that a sense of the one-ness, the wholeness, the completeness of a work is capable of emerging.

Conclusions

Viewing all of the above considerations in light of the overarching issue of music theory, a sense of hopelessness may seem to emerge. In place of what we had hoped would be science stands, instead, an approach to analysis grounded in normativity, subjectivity, emotion. I believe, however, that there exist—to borrow a term in use among French meteorologists (a generally gloomy lot)—certain “belles éclaircies” to quicken our spirits. In brief, it is my contention that normativity, subjectivity, and emotionality are all indis-

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18 See, for example, Alan Tormey, *The Concept of Expression: A Study in Philosophical Psychology and Aesthetics* (1971); or Peter Kivy, *The Corded Shell* (1980). The passage from Langer to this next generation of “expression” theorists roughly parallels the evolution of Ludwig Wittgenstein’s thinking, as Langer professed allegiance to the early Wittgenstein, while Tormey and Kivy were evidently deeply impressed by the later Wittgenstein.
pensable elements of music theory, properly conducted, although undeniably
the mutual interdependence of these three, in effect, cubes the difficulty of
the project. Let us give one last look at each of these issues.

A. Normativity. True, the normative character of any legitimate form of
analysis does bear with it all the eccentricities and uncertainties of any nor-
mative account of anything. To be sure, there are realms of discourse where
valuations are made with almost mathematical precision: a great, that is,
rock solid, legal case against someone, in which all possible meaningful
questions are cogently answered, all objections readily dismissible. There
can be a beautiful proof of a theorem: one that relies on a minimum of pre-
sumptions while sustaining a rich variety of implications. If only the realm
of art were this precise! And yet the considerations I voiced at the outset
here (Section I, A, 1–4) still stand, and there are many, many more of the
same sort I could have offered. Societies and traditions are value-laden—
indeed in some cases they are value-defined: when we talk of the Victorian
Era, what is it that comes to mind but the social, ethical, aesthetic values
of a particular slice of time in a particular place? And limitation within time and
place is hardly the downfall of analysis: rather, it is an essential element of
the human condition! To understand better why we value what we value is to
understand better who we are.

B. Subjectivity. Wherever subjectivity becomes enmeshed in any analysis,
opacity appears to increase; yet opacity is hardly equivalent to irrationality—
we may only need guidance as to where and how to look. Indeed, there is no
shortage of accounts of rationality that actually locate it within subjectivity:
no human subjects, no “reason” to speak of. The pejorative, rhetorical “But
isn’t it all just subjective?” is often met with “Of course it is—that’s the only
way we can make any sense out of it!” Human subjectivity is perpetually
under scrutiny by not just philosophers but psychologists and more recently
neurologists. It in no way constitutes an analytical dead end.

C. Emotion. Emotions, to be sure, are volatile elements within any study
of anything. We all have them, we all recognize them and make liberal refer-
ence to them; but somehow when it comes to integrating them into an ade-
quate theoretical account of anything, we balk.

On the brighter side, however, let it be noted that emotion has been made
the object of innumerable empirical studies. To cite just one example
within the domain of music, Leonard Meyer’s work on the relations among
emotion, meaning and value in our experience of music has been extremely

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19 A couple forestry students who appeared in my aesthetics class many years ago had under-
taken a study of the relation between beauty and emotion based on observable epidermal
reactions to sights and images: goose bumps, hair standing on end, chills accompanied by a
flushed appearance, and the like. I am not advocating this particular approach, but I do admit
to having myself undergone all these very reactions in the presence of beautiful things.
enlightening, even if it does not constitute the last word on the subject.\textsuperscript{20} Indeed, in\textit{Music, the Arts, and Ideas} he set out to apply information theory to musical analysis, although aside from some broad, general insights such an application could bring, the main lesson learned from it was that music is just too rich and complex a phenomenon to submit to rigid mathematical formulation. But by then he seemed to be following the general trend in aesthetic analysis and leaving emotion behind. It is time we saw that trend reversed.

Perhaps what is required of us is to obey Aristotle’s sage advice: from the\textit{ Nicomachean Ethics}:

\begin{quote}
Our discussion will be adequate if it achieves clarity within the limits of the subject matter. For precision cannot be expected in the treatment of all subjects alike, any more than it can be expected in all manufactured articles (Aristotle, \textit{Nicomachean Ethics}, 5).
\end{quote}

Within these guidelines, music theory can well continue to expect and enjoy a long healthy life.

\textsuperscript{20} This is obviously the central focus of \textit{Emotion and Meaning in Music}. In \textit{Music, the Arts, and Ideas} chapters one through three pick up and develop some of the same themes.
The Concept of Unity in Music

References


The Techne of Music Theory
and the Epistemic Domain of the (Neo-) Aristotelian Arts of Logos

Elisabeth Kotzakidou Pace

He who attempts to construe Rhetoric or Dialectic, not into the powers/potentialities/faculties/abilities that they are in themselves, but into [object oriented] sciences, dissolves their nature and makes them disappear in the process of crossing over [into the domain of those sciences]; [by so doing] he refurbishes them into sciences dealing with the underlying [nature] of specific things [namely, their substratum], rather than exclusively [dealing] with [the knowledge of] meanings and rationales [logoi].

Aristotle, The Art of Rhetoric

1. Introduction

A survey of contemporary music theory reveals a number of intellectual trends. Prominent among them: music-as-mathematics (Babbitt 1972 and Lewin 1987), music-as-physics (Brown and Dempster 1989), music-as-biology (Epstein 1995), music-as-natural-language (Lerdahl & Jackendoff 1983), and music-as-general-aptitude (a considerable portion of studies in music perception and cognition). As expected, they all come equipped with their own distinct methodologies, depending on how each construes the object of its study.

I would like to propose instead a broader conception of the knowledge domain of Western “art music”1 as a collection of historically conditioned competences, culture-laden techniques, and laboriously acquired skills: music-as-techne (Art). This is a sensible and pragmatic outlook that welcomes valuable contributions from a variety of other disciplines. Nonetheless, it does not allow the internal logic of any of those fields to structure the do-

1 For obvious methodological reasons I restrict this historically motivated discussion to its native milieu, namely the European musical tradition that is colloquially referred to as “classical,” “serious,” or “art music.” By analogy, and assuming that the question itself is still meaningful outside the occidental Weltanschauung, one may ponder the epistemic status of the body of knowledge that supports other highly skilled modes of music making (for example, the classical traditions practiced in India, China, or Japan).
main of music in terms of borrowed and entirely inappropriate epistemic criteria. Inspired by Aristotle’s enduring inquiry into what constitutes the domain of the various Arts or techne, it yields an epistemologically sound and historically supported account of what kind of “theory” is music theory.

For my initial dialectic step in this direction, I have chosen to elucidate the first seminal moments in the epistemological life of the field, whose identity as the self-conscious branch of knowledge that we would recognize today was in fact solidified during the sixteenth century. This genealogical excursion rewards us with the revelation that, as the discipline entrusted with formulating the precepts of one of the most vital forms of cultural expression, music theory has a deep-rooted affiliation with the sciences of Man—the humanities. More specifically, insofar as it occupies itself with the intentional and principled, namely, the artistic, artful, and artificial employment of sound, music theory emerges as a discipline akin to poetry (which includes drama), rhetoric, dialectic, and grammar. Evidently, the Renaissance founders were keenly aware of the fact that music constitutes an exceptionally rich conceptual domain, which is just as representative and revealing of human cognitive capacities as the study of language. They proceeded to properly classify music theory together with the Aristotelian Arts of Logos, namely, the “logico-communicative” Arts most famously represented by

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2 I capitalize the term Art to remind the reader that I am alluding to the Greek term techne in its broad Aristotelian sense. For the remainder of this essay and unless otherwise indicated, the concept of Art/art (capitalized or not) should always be taken in this sense. It will shortly become evident that this meaning survived Greek and Roman Antiquity and persisted throughout the Renaissance. By contrast, the largely anti-intellectual notion of the (marginalized just as mythologized) “beaux arts” is the product of the Enlightenment and the ensuing nineteenth-century Romanticism.

3 Singular: techne; plural: techai. This is the Greek word for art whose original meaning surfaces in the cognates technical, technique, technology and technological, more so than in Latinate words like artisan, or the English term craft.

4 The term “cognitive” is used here synonymously with a broadly conceived “logical” and occasionally “rationalizing” faculty of Man employed to conceptualize situations by all available means (images, sounds, and sensations emanating from both the exterior and the interior of the body, as well as high-level knowledge structures, such as categories, propositions, models, metaphors, and myths). This faculty is consequently capable of drawing inferences, setting goals, making plans and setting them into action as needed.

5 A felicitous translation for the Greek “Τέχναι του Λογού” has eluded generations of English-speaking classicists. I settle here for the bilingual expression “Arts of Logos” glossed as “Science-Arts of Rational Communication.” I do so with the caveat that the term “rational,” rather than merely “rule-following” or “rationalistic,” stands for “thoughtful,” “product of reflection,” “planned,” “intentional,” and “in control of the expressive medium.” The awkward compound “science-art” is a rendering of “techne.” In accordance with both Aristotle’s classical diction and Modern Greek vernacular use, the Greek term for “science” (ἐπιστήμη—episteme) is employed in its general sense as “domain of systematic knowledge.” In other words, it denotes the genus under which “art” (τεχνη—techne) is classified as a species. Thus, the compound “science-art” underscores the fact that the “art” in question has precious little in common with the now prevalent notion of beaux arts, and is instead a species of “systematic knowledge” and therefore a type of “science.” Finally, the meaning of the pivotal term logos translated here as “rational communication” is established in greater detail below.
dialectic and rhetoric. These belong to the genus of the “productive” or poietic Arts, whose other branches include fields like architecture and sculpture.

A living testament to the wisdom of classical thought, the issues first formulated by the Aristotelian Arts of Logos are still featured prominently among the central concerns of modern cognitive science. For example, the study of single terms and the structure of their categories, the semantics of sentences and the construction of large scale discourse, the pragmatics of syllogism and inference, the role of metaphor and analogy in our conceptualization of the world and in problem solving, the semantic plasticity of poetic constructs, and lately a keen interest in the conceptualization of emotions. Translated into a modern idiom, it is only natural that music theory belongs nowadays amongst the fields contributing to a humanistically conceived cognitive science.

Having supplied the answer to the question “what kind of theory is music theory?” it is also imperative that we remove any potential confusion by disambiguating the meaning of the term “theory” in its name. The techne of music theory is indeed “theoretical” insofar as it opposes “craft-like practice.” At the same time, any identification with the “theoretical” sciences (of mathematics, physics, biology, and according to some, metaphysics and theology too), amounts to a serious category error analogous to confusing the bank of the river with the financial institution by the same name.

Since the discipline of music theory does enjoy the prestigious epistemic status of an Aristotelian techne, as a corollary I would also like to propose that it cannot possibly want in academic “legitimacy” (McCreless 1995). Music theory is rightfully entitled to a proper place in Academe, both in the teaching curriculum and as an area of systematic research. With its epistemic position secured, it should not have to resort to forced, distorting, and ultimately demeaning methodological practices purporting to “elevate” the knowledge-domain of art music into a de facto branch of mathematics, a naturalistic discipline akin to physics, or even the prerogative of a biologically oriented neuroscience.

6 The rather misleading term “productive” is the most common English translation of “poietic.” While the creation of a “product” is certainly the likely outcome of a “poietic” activity, what is at issue here is the “made” or “made-up” aspect of its meaning. The expression “a world of make-believe” seems at first to solely characterize the Art of Poetry, but the Arts of Rhetoric and Dialectic pertain to equally “made,” “shaped,” and “artificial” forms of reasoning and expression, speech acts and communicative gestures, whose efficacy is heavily vested in the technical apparatus delineated by the respective Arts.

7 See footnote 19 below.

8 I consider cognitive science to be “humanistic” in its quest to understand the workings of natural cognition through the development of “psychologically real” models. This is a crucial point in view of the early history of the field, when efforts to develop a viable science of the artificial (computer-based artificial intelligence and robotics) were frequently mistaken for a realistic cognitive science of Man.
2. The Aristotelian Arts of Logos

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If five hundred years from now historians of music-theoretical ideas were to explain to their contemporaries what Lerdahl & Jackendoff’s acclaimed 1983 book *A Generative Theory of Tonal Music* was all about, they would at a minimum have to offer a detailed exposition of what the Chomskian approach to language was, and how it almost single-handedly defined the field of linguistics in the third quarter of the twentieth century. Similarly, an adequate exposition of Baroque musical thought requires a nuanced appreciation of the methodological choices of early modern science (Christensen 1993 and 1987).

It should then come as no surprise that to fully immerse ourselves into the episteme of sixteenth-century theory and practice we need unimpeded access to the grand Aristotelian paradigm that preceded the rise of modern Science. Indeed, neo-Aristotelian precepts saturated all levels of the educational system of humanist-dominated Europe, creating a unique intellectual climate that left an indelible mark on the music production of the era. All the while, the intricacies of the departing modal paradigm were documented in a plethora of theoretical treatises both North and South of the Alps.

It has, of course, long been recognized that during the Renaissance the domain of music (re)connected with that of rhetoric. Far less acknowledged is the fact that this link to rhetoric was part and parcel of a vastly larger picture: Music’s epistemic realignment with the Aristotelian Arts of Logos. Or in most cases, with their pedagogical reflection in the Renaissance school curriculum: the linguistic Arts of the Trivium—Grammar, Dialectic, and Rhetoric.

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9 The capitalized “Science” stands for the large scale Weltanschauung (or “episteme” in Michel Foucault’s terminology) of the modern era—driven by the mathematicized natural sciences, whose first paragon was physics and more recently biology.

10 An extensive bibliography on the relation between music and rhetoric (mostly during the Renaissance) can be found in Kotzakidou Pace 2006, esp. Chapter IV.

11 The relation of Music to Dialectic is broached by Howard (1985) and investigated at length by Kotzakidou Pace (2006).

12 With the advent of the Christian era the Aristotelian Arts of Logos became all but synonymous with their anthropomorphic rendition in Martianus Capella’s *The Marriage of Philology and Mercury*. In keeping with tradition I henceforth capitalize the names of these logos-loving maidens. Martianus Minnei Felix Capella was a Carthage-born Roman who composed his allegorical textbook sometime between 410 and 439—probably towards the beginning of the terminus (Stahl 1991 [1971]). The work maintained a steady popularity as an elementary schoolbook throughout the course of the ensuing millennium, but the Arts of Logos were not fully restored as an all-consuming intellectual preoccupation until the Humanist movement started to gain momentum.
The most prominent constituents of the Aristotelian Arts of Logos are undoubtedly Dialectic and Rhetoric. Perhaps slightly peripheral to the genus, but just as legitimate amongst her sisters, stands Aristotle’s Poetics, also known as Poietic Art (ποιητική τεχνη—poietike techne). Though historically prior, owing to a pedigree so old that it is lost in the primordial mists of what Ong (1982) terms “primary” orality, conceptually and normatively the Art of Poetry leans heavily on the other two, and is thus deemed epistemically posterior to them.13 The author of a relatively short treatise on the subject, Aristotle did not lavish as much attention to this Art, nor did its transmission over the next two millennia have the same profound “structural” consequences for the intellectual life of the West as her sisters. Finally, Grammar, though casually mentioned by Aristotle on many occasions, is actually a latecomer to the genus. Its struggle to delineate its own intellectual sphere and assert the independence of its methods began during the Alexandrian times and was accomplished during the Roman era, when it finally earned full intellectual legitimacy and was awarded the coveted epistemic status of an Art (γραμματικη τεχνη—grammatike techne).

But to properly introduce the Arts of Logos, we first need to elucidate the key concepts that govern their common genus: Logos and Art (techne). Unfortunately, English translations have frequently betrayed the semantics of these characteristically Greek terms, whose closely intertwined meanings encapsulate a complex of rather unique and mutually supportive cultural values.

Throughout the history of the Greek language the enduring concept of λόγος (logos) is a multivalent one, seamlessly encompassing the notion of human reason (as in the cognate “logic”) capable of producing a well-ordered diction (as in the expression “to give a speech”). For example, in the following passage Aristotle uses the expression “has logos” rather informally to indicate both a reasoned view and something reasonable in general:

A [dialectical] thesis is a conception contrary to general opinion but pronounced by someone famous as a philosopher … or a thesis may concern matters about which we hold a reasoned view [περὶ όν λόγον εχομεν] contrary to received opinions; for example, the view of the sophists that … This view, even if it is not acceptable [as true] to some people [δοκειν], [it] might be [entertained] on the grounds that it is reasonable [δοξειεν αν δια τον λόγον εχειν] (Topica, I, xi).14

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13 The concept of epistemic priority is explored below.
14 Translation by E.S. Forster. Throughout this essay, translations from the Greek original are mine unless otherwise indicated, as in this particular quote. Typographical emphasis and glosses inserted in square brackets [ ] are also mine.
Elsewhere Aristotle presses the term into narrow technical uses. For example, \( \lambda\omicron\gammao\varsigma \) signifies a semantically conceived grammatical entity, a *sentence*, while the compound \( \lambda\omicron\gammao\varsigma \alpha\pi\omicron\phi\omicron\alpha\omicron\nu\iota\kappa\omicron\varsigma \) (logos apofantikos) indicates a logical *proposition* that can be either true or false (*On Interpretation*, IV). Along the same vein, *logos* also means *syllogism* (*Topica*, X), dialectic *argument* (*Topica*, XI), or logical *argument* in general—whether deductive or inductive (*Posterior Analytics*, I). Another all important technical use of the term is one which we will have many occasions to revisit: *logos* is the last “why” of a term, its linguistic “essence,” namely the most informative explanation of its meaning that helps define its species and form a proper definition. By extension it can also indicate the original or source meaning of, say, a compound term considered as the simple sum of its parts before it takes on its holistic meaning (*Topica*, II, vi). For example, in English we distinguish “blue grass” from “Bluegrass”; in Greek “\( \epsilon\upsilon\tau\omicron\gamma\eta\varsigma \)” literally means “good-fortuned,” but when considered as a whole it simply means “happy.”

Consistent with the traditional Greek reverence for a dialectico-rhetorical form of life (namely, the conviction that “articulate reason” yields the highest mode of human existence), the passage to Christianity was poignantly marked by St. John’s opening gambit: “Εν αρχη ο \( \Lambda\omicron\gammao\varsigma \)”—“In the beginning was Reason capable of articulation through well ordered Language.”\(^{15}\) It is as if John with one stroke put the audience of his fellow Greeks at ease by informing them that not only God is Wisdom, but His Wisdom is neither capricious, nor unfathomable. This God is knowable, the ultimate manifestation of well-ordered Reason and therefore, for all His venerated Jewish roots, is peculiarly “Greek” as well. Christ-in-the-flesh is also referred to by the Greek Church Fathers as the \( \Lambda\omicron\gammao\varsigma \), the human articulator of a graspable Divine Logic. In the same vein, the legendary cathedral of Orthodox Christianity in Constantinople was dedicated to Agia Sofia—not some female Saint named “Sofia,” but God’s Holy Wisdom.

An important semantic extension of the sphere of *logos* is the notion that certain human activities can in fact be discovered to harbor *logos*. In Greek, both classical and modern, for an activity to have *logos* is to be governed by “reason.” In English we also casually say that “there is a logic to it,” meaning that the activity embodies a degree of conceptual order and predictability. A domain which was initially guided by mere habit (of body or mind) and depended upon the casual memorization of haphazardly laid facts or pattern-fragments, can upon reflection be found to embody powerful principles capable of pulling the facts and procedures together into an organized

\(^{15}\) Here the need for poetic flow clearly forces us to settle on the usual translation “In the beginning was the Word”—even though, in my opinion, the English rendering “In the beginning was Reason,” which errs on the side of emphasizing the dialectic nature of the Divine rather than the rhetorical means of evangelizing, would have been far more felicitous.
whole. Our most immediate experience of the presence of logos in human affairs is, of course, the way our rational thought processes are effortlessly reflected in well-ordered language and become a communicative diction that possesses logical flow and coherence. The fundamental belief in the possibility of expanding the sphere of logos beyond the narrowly conceived domain of everyday language to encompass political and judicial action (πράξις—praxis), lies at the heart of the Athenian way of life and is the spark that ignites the remarkable philosophical tradition associated with that city-state.

D

Regarding the human capacity for logos, in his Nicomachean Ethics (Book VI, I, 5–6) Aristotle states specifically that the soul has two parts,\(^\text{16}\) one rational and the other irrational—literally, the one that has logos and the one that does not. He further subdivides the rational part into two, based on the kind of knowledge-seeking activity each engages in: one is the scientific (ἐπιστημονική) part, also identified as deductive, and the other the calculating (λογιστική, as in the English cognate logistics) or deliberating one—the latter two notions deemed by him synonymous in this context.

To be sure, the term science (ἐπιστήμη—episteme) is also used by Aristotle to indicate any sort of “carefully and systematically derived knowledge” in general—a usage akin to the German term Wissenschaft. For example, in the following quote he observes that when science is used as a genus it is always relative to something; this is not so when the term is used as a species. As examples of specific sciences he then lists both Grammar and Music:

Science [ἐπιστήμη], taken as a genus, we define by a reference to something else (for it is said to be the science of something). Regarding the particular branches of science, however, they are not said to be about something else. For example, grammar is not called the grammar of something, nor music the music of something. Nonetheless, in these cases too, when employed with respect to their genus, we call grammar the science of something, not the grammar of something, and music we call the science of something, not the music of something (Categories, VIII).

Similarly in the following statement from his Topica (VIII, i) he employs the term “science” as a genus:

\(^{16}\) Aristotle points out that for the sake of his discussion, it is immaterial whether these “parts” can be separated from each other as the limbs from a body or they have the same relation as the convex to the concave “parts” of a single object.
Among the sciences [επιστήμαι—epistemai] some are theoretical, others practical [pertaining to praxis, action, performance\(^1\)], and others poietic [ποιητικαι—poietic; making/creative/productive].

When the practical and the poietic sciences are considered in tandem they are known as technai (Arts); in which case, the name of the genus episteme (science) is recycled at the level of its species to indicate the theoretical sciences alone.

This brings us back to the technical exposition of the Nicomachean Ethics. There the scientific faculty is understood to be the one that contemplates things, which exist of necessity and admit no variation (such as mathematical objects and natural phenomena governed by immutable and presumed to be deterministic laws). By contrast, the “calculating” faculty applies itself to all things variable, about which deliberation and judgment is necessary; for example, all those Arts which produce man-made objects, words and ideas, such as the Arts of Architecture, Sculpting, and Poetry, as well as all those dealing with issues pertaining to human action, such as the Art of Politics. The action-oriented type of Art he names Prudence (φρονησις) is a type of practical wisdom aiming at a beneficiary end that comes with hands-on experience, so that the poietic Arts now stand alone under the following definition (Nicomachean Ethics, VI, iv, 3–4):

> Art is the same as a [rational] habit [of mind] concerned with making in accordance with true reason.

He proceeds to generalize:

> All Art deals with bringing something into existence; and to pursue an Art means to study how to bring into existence a thing which may either exist or not, and the efficient cause\(^1\) of which lies in the maker and not in the thing made; for Art does not deal with things that exist or come into existence of necessity, or according to nature, since these have their efficient cause in themselves (translation based on H. Rackham's).

He then recapitulates:

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17 The “practical” Arts Aristotle has in mind here are domains of civic action like Politics and Economics. Their outcome is not a permanent “product” documenting either a purely contemplative or a goal-oriented activity. Rather, it is their performative aspect, their praxis, the act in itself, which constitutes the “essence” of these Arts.

18 Aristotle’s classification of causes found in his Metaphysics (I, iii) is four-fold: formal, material, efficient, and final. The formal cause or “principle” of each thing is its “essence”—best translated into English as its meaning-for-us, its verbal definition, or the ultimate reason (also termed its logos) in a chain of why’s? The material cause is its substratum, the stuff it is made out of, the material that cradles its form. The efficient cause is the source of its motion (where “motion,” besides movement in space, includes the birth and destruction of a thing, the means of manufacture or its biological origin). The final cause is the teleological purpose of a thing’s motion, birth, or creation.
Art \[\text{τεχνη}\] … is a \[\text{rational}\] habit \[\text{εξις}\] concerned with making \[\text{ποιητικη—poietike; poetic}\] in accordance to true reason \[\text{µετα λογου αληθους}\]. On the contrary, lack of Art \[\text{ατεχνια}\] is a rational habit concerned with making in accordance to false reason \[\text{µετα λογου ψευδους ποιητικη εξις}\]. Both deal with that which is contingent \[\text{το ενδεχομενον αλλως εχειν—that which could be otherwise}\].

In Aristotle’s use the term “poetic” or “poietic”\(^{19}\) literally means something one makes or puts together—hopefully under the guidance of a true Art. This aspect is showcased in the title of Aristotle’s own Poetic Art (the Art of made-up or “created” speech about a make-believe world) and resurfaces in Renaissance music treatises on musica poeitica, such as those by Heinrich Faber (manuscript, ca. 1548), Gallus Dressler (2001 [1563]) and Joachim Burmeister (1955 [1606]).

Running parallel to the distinction between science\(^{20}\) and Art (and occasionally substituted for it) is the partition of knowledge-seeking activity into theoretical and practical. As we will see later,\(^{21}\) the theoretical–practical opposition is recycled at different levels of Aristotle’s dialectic division of knowledge, appropriately adapting its sense to each context. In the case at hand, we can say that when compared to Art, deductive science is “theoretical” insofar as it occupies itself with things that are worth contemplating in their own right with no particular application in mind and without aiming at utility of any kind (Metaphysics, I, i, 14–16). By contrast, the domains of “practical” knowledge governed by the various Arts, whether “productive” (poietic) or “performative” (action oriented), are always in the service of improving what Husserl termed the Lebenswelt of our tangible human experience. Admittedly, the obvious example of the exceedingly practical applications of Geometry, the paragon of all theoretical science, seems to contradict this straightforward definition; but this is only a hasty conclusion. While the practical work of a land surveyor involves complex material objects with frequently intractable shapes and heuristically determined measurements, the theoretical science of Geometry deals with conceptually constructed abstract objects with zero or single dimensions, the likes of which are never to be encountered in the multidimensional world of nature. Nonetheless, the study of these mental constructs, their properties and relations, is its own reward.

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\(^{19}\) Classicists frequently employ this spelling (pronounced “pee-eh-tic”), a direct transliteration of the Greek alphabet, precisely because they wish to capture the literal “making/creating” aspect of its original meaning. Besides the narrowly conceived Art of Poetry, this meaning encompasses all the Arts of Logos, in addition to Architecture and many others capable of giving rise to tangible products or creations of the mind that would not have existed were it not for human intervention.

\(^{20}\) Per our previous discussion, the term “science” is used here as a species of the genus “science.” As a genus it meant “systematically gathered knowledge of any kind,” while as a species it is restricted to deductive domains like mathematics and natural science.

\(^{21}\) See section 6B.
If our criterion is the level of certainty to which we can hold the resulting knowledge, the sciences are understandably judged to be epistemologically superior to the Arts. In the Aristotelian conception, science deals with deductively derived knowledge pertaining to immutable and eternal objects (mathematical shapes and numbers, the immutable laws of Nature, God, and the motions of the heavens). This type of knowledge is considered “better” (μᾶλλον) or, as we would say, “more secure” compared to the knowledge that can possibly be obtained about contingent constructs that “could have been otherwise.” In this latter category we find all language, poetry, music, and every idea about issues, which admit contrary opinions (namely, all those important life-changing matters that can become the topic of a well-formed “dialectic” debate).

One may reasonably object that should Aristotle have been aware of the fact that the seventeenth-century “scientific revolution” largely consisted in the adoption of the “scientific method,” he may have revised his position and classified the natural sciences together with the Arts—leaving only mathematics (and theology) under the category of “science.” After all, this is a method that (ideally) involves proposing multiple models and theories as possible explanations of a group of well-delineated natural phenomena. Following a process of rigorous debate and empirical falsification, it (ideally) culminates into the (temporary) verification of one of the proposals. It would seem then that the so-called scientific method is actually a paradigmatic case of the truth-seeking dialectic procedures that the Stagerite\(^{22}\) so lucidly describes in his Organon. Nonetheless, short of attempting to revise the standard occidental view of the epistemic status of natural science in the direction of the Arts,\(^{23}\) Aristotle’s fundamental distinction between the contingencies of human poietic activities and the immutability of the Laws of Nature still holds. Thus, the ensuing epistemic distinction between the humanities (the “Arts”) and the natural sciences (lumped here together with mathematics) would have to suffice for the purpose of the present discussion.

Finally, regarding the intimate relation of logos to wisdom, we can now say that the dialectic question of whether logos constitutes wisdom was directly tackled by Aristotle, and a positive conclusion was reached. In the narrow sense of the term, Aristotle tells us that the term “wise” is employed in the Arts to denote those men “who are the most perfect masters of their

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\(^{22}\) Known simply as “the Philosopher,” Aristotle is also referred to as “the Stagerite”; for he was born in 384 B.C. at Stagira, a town located in the Macedonia region of Greece. His father Nicomachus (to whom he later dedicated his *Nicomachean Ethics*) was the court physician to King Philip. As a youth he was sent to Athens to study in Plato’s Academy, where he also taught for twenty years during the period 367–347 B.C. After serving as tutor to Alexander the Great, he eventually returned to Athens where in 336 B.C. he established his own school, the Lyceum—also known as the school of the Peripatetics. In 323 B.C., due to rising anti-Macedonian feeling in Athens, he found it prudent to retire to his estate on Euboia, the elongated island mass adjacent to Attica, where he died the next year.

\(^{23}\) Such an ambitious goal is beyond the scope of the present analysis.
Art ... In this use, wisdom signifies artistic excellence [in a well delineated
domain] (Nicomachean Ethics, VI, vii). Concerning the case of people who
are “wise in general,” wisdom is a combination of the general power of intel-
lect (νοῦς) and scientific knowledge (ἐπιστήμη) of the most exalted subjects
(such as the Deity, the fixed stars, the fundamental principles of being, and
the immutable laws of mathematics). Since both theoretical scientific
knowledge and knowledge of the practical Arts are species of the rational
part of the soul, the presence of logos is fundamental to both. Then the ded-
ication of the great Cathedral of Constantinople to God’s Wisdom is an in-
dication that the classical Greek concept of wisdom as “excellence in all things
rational” survived into the Christian era and continued to be celebrated as a
central cultural value.

E

Per our earlier statement, among the Aristotelian Arts of Logos there are two
that stand out as paragons of all poietic activities driven by true reason: the
sister Arts of Dialectic and Rhetoric. Having examined their common genus,
let us now focus on the species themselves.

i

The Art of rational thinking in general, both deductive and deliberative, is
gathered in the treatises known as the Organon (the instrument for proper
thinking). This is comprised of six books entitled Categories (Κατηγορίαι),
On Interpretation (Περί ερμηνείας), Prior Analytics (Αναλυτικά προτερα),
Posterior Analytics (Αναλυτικά υστερα), Topica (Τοπικα), and On Sophistical
Refutations (Περι σοφιστικων ελέγχων). Taken as a whole the Organon
strives to characterize the nature of conscious, reflective, and critical dis-
course, and provides techniques which can aid in the discovery of all true
factual matters—including mathematics, natural science, and every other
systematic field of knowledge.

Of these, the last two books (generally considered to comprise a single
grand treatise) are explicitly dedicated to the Art of dialectic discourse,
namely the formation of specifically “dialectic” problems and questions about
matters of interest to a community of experts and laymen alike. This
Art of Dialectic (διαλεκτικη τεχνη—dialectike techne) teaches how to delve
into issues about which an obvious consensus is lacking among people holding
what otherwise seems like perfectly reasonable, yet contradictory posi-
tions. For the purposes of its demonstrations Dialectic possesses two modes
of argument, syllogism and induction (Rhetoric, I, ii, 8). In general, one rea-

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24 See also the introduction to the Loeb Classical Library edition of the Nicomachean Ethics
by H. Rackham, p. xv.
25 For further discussion of the peculiarities of “dialectic” propositions please refer to Kotza-
kidou Pace 2006 (Chapter I).
sons syllogistically when the conclusion flows from its given premises by necessity; but a syllogism only amounts to a proof when it flows either from true premises or self-evident principles. Similarly, a dialectic syllogism produces a proof when it proceeds from generally accepted opinions (Topica, I, i). Induction, on the other hand, is a form of reasoning that proceeds from particulars to universals; it tends to be more convincing and clear, more easily grasped by sense perception, and better shared by the majority of people. Nonetheless, syllogistic reasoning (deduction) is more cogent and more efficacious against argumentative opponents (Topica, I, xii). The goal of the dialectic process is to uncover the truth by equitably bringing to the fore all conceivable arguments, so that it can shine the light of reason on every possible aspect of a given problem. Only then can a group of interlocutors weigh the issues dispassionately, and by the application of good deliberative judgment turn their disagreement into a thoughtful consensus.

If the dialectic process begins to sound like a sort of democracy for the intellect, surely this is not an accident, as this is precisely the sort of reasoned exchange that ideally sustains the function of a democratic society. Furthermore, notice the relevance to Dialectic of a truly profound and poignant comment Aristotle actually makes about Rhetoric. He emphasizes that, unlike commonplace or even sophistical definitions of Rhetoric as “the Art of Persuasion,” the ultimate goal of Rhetoric is not persuasion at any cost and by any means. What really counts is the discovery of arguments in favor of all sides of an issue, presented with logical integrity in an honest pursuit of the truth. Everyone wants to wield power over the minds and actions of others, but that should never be the ultimate goal of either the orator or the dialectician. Specifically,

It is … evident that Rhetoric … is useful; and further, that its function is not so much to persuade, as to find out in each case the existing means of persuasion. The same holds in respect to all the other Arts [including Dialectic]. For instance, it is not the function of medicine to restore a patient to health, but to promote this end as far as possible; for even those whose recovery is impossible may be properly treated. It is further evident that it belongs to Rhetoric to discover the real and apparent means of persuasion, just as it belongs to Dialectic to discover [the deductive and inductive syllogisms] (Rhetoric, I, i, 14; translation by J.H. Freese).

Aristotle leaves no doubt that Dialectic similarly seeks to elucidate all sides of a contested issue. The newfound logical clarity will hopefully lead to some consensus, but never a forced consensus or a consensus for its own sake. Clearly, the most interesting dialectic questions are the ones that are left inconclusive for future generations to continue pondering.
The Stagerite expounds on the Art of Rhetoric (ῥητορική τεχνή—rhetorike technē) in the voluminous treatise bearing the same title. Unlike Dialectic, which deals with known actualities and seeks to forge the truth out of mere facts or conflicting opinions, the Art of Rhetoric is defined as “the power to examine what is conceivably possible about each contingent matter” (Εστω δε ρητορικη δυναμις περι εκαστον του θεωρησαι το ενδεχομενον πιθανον. Rhetoric, I, ii, 1). Its purely “logical” means of persuasion (for it also makes available psychological, performative, and means derived from the power of the discursive order itself) are analogous to those used by Dialectic—adjusted for the fact that we are now dealing with contingent matters and hypothetical states of being. These are the enthymeme, a rhetorical version of the dialectical syllogism, and the example, corresponding to dialectical induction (Rhetoric, I, ii, 1). Rhetoric’s paradigmatic application is official civic discourse—both legal and deliberative—for it offers the best way to deal with its inherent contingencies. Having established its precepts in these two prominent domains of civic action, Aristotle proceeds to apply his technical apparatus to epideictic rhetoric as well, namely celebratory oratory showcasing the orator’s skill. The use of oratory in pedagogical settings is also broached.

Curiously, the above definition and the ensuing elaborations do not appear until later in Aristotle’s text, as his book on the Art of Rhetoric actually opens with a striking acknowledgement of the practical and conceptual inseparability of this Art from its twin sister, the Art of Dialectic—a point which apparently Aristotle deemed to be important enough to lead his exposition: “Rhetoric is the counterpart (αντιστροφος) of Dialectic.” Aristotle’s translator J.H. Freese annotates the term αντιστροφος as “not exactly a copy, but making a kind of pair with it, and corresponding to it as antistrophe to the strophe in a choral ode.” As his allusion to strophe-antistrophe may render the concept unnecessarily obscure, it is worth noting that to a native speaker of Modern Greek αντιστροφος (opposite-turn) is a very common word, with a myriad of concrete and metaphorical usages; and I have no reason to believe that in classical Greek the term was a rarity restricted in specific technical contexts. What seems to motivate its broad utility is the basic schema of a physical or abstractly construed motion of “turning over to the other side,” which in the vertical dimension conjures schematic images of flipping coins and inverting numerical fractions, and in the horizontal plane yields “turning around towards the other direction.” It is probably by metaphorically extending this basic meaning of “strophe” that a Greek chorus (in its totality or divided in sub choruses) was said to intone “strophes” and “anti-strophes.” These were pairs of poetic text uttered either from spatially opposing sides of the theatrical stage set “against” each other or, most likely, while engaging in fluid dance-like gyrations that took them across the
stage and back. Conceivably the dancers punctuated the completion of each arc in their rotation with the intonation of the corresponding text.

Indeed, the sister Arts are considered to be complementary and interdependent ways of exploring the various aspects of the act of rational communication, for

both are about matters which in some way are common to all things one knows and are not confined to any special science. Hence all people in one way or other participate in both; for all, up to a certain point, endeavor to examine [the witnesses and the arguments of the opposition] and to take the stand, and to plea and to sue [in court] (Rhetoric, I, i, 1).

The first point regarding the epistemic interdependence and practical inseparability of the two Arts will acquire even greater significance in the course of their historical evolution as organized fields of knowledge. Nonetheless, it is fair to say that before the onset of the Renaissance confusedion and the ensuing dismemberments and reapportionments of the materials taught by each field, the traditional view has been that the teachings of the Organon, including the last two books on Dialectic, are epistemically prior to the teachings of Rhetoric. In turn, both Dialectic and Rhetoric are prior to Poetics.

iii

In his various writings Aristotle enumerates several different senses of the term prior (προτερον). While the primary sense is certainly temporal, a related sense is one indicating the relation of two things, when one precedes the other in irreversible order. For example, the number one is prior to the number two since, among other things, each number along the series of (positive) integers is generated by the previous number plus one. Of all the additional senses, the one pertaining to matters of definition (where the genus is considered prior to the species, and is therefore expected to provide the what is? of the species) is of particular interest: The genera are always prior to the species, as in the example of an aquatic animal. If the notion of

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26 A modern musical analogy seems appropriate here. Rhetoric is to Dialectic what Harmony is to Counterpoint: two inseparable sides of the same musical edifice, even though for pedagogical reasons we may choose to teach their precepts separately. Additionally, they are both the type of knowledge that is not specific to any style; knowledge of their principles is a prerequisite for dealing with any type of composition in the Western art music tradition (at least until recently). Our musical analogy can be pursued even further by noticing that, just like the cyclic debate(s) over the epistemic “priority” of counterpoint over harmony (and vice versa), the assumption of dialectic’s priority over rhetoric is not as secure as one may think—precisely because their inherent interdependence makes any air-tight compartmentalization problematic.

27 Martianus Capella’s presentation of the Seven Liberal Arts reflects this view in their order of appearance: first come the fundamental teachings of Grammar, followed by Dialectic, and then Rhetoric.
an “aquatic” species exists, then so does the genus of “animal.” The opposite is not true, as we can maintain the notion of a genus of “animals” without necessarily requiring the existence of the species “aquatic” (Categories, XII). Equally important is the sense, which pertains to the relation of the order of definition to the order of teaching (the logically prior should naturally precede in definition, and in pedagogical presentation as well—except when it is not practically advisable). A definition, he strongly advises, should be made by means of prior and more intelligible terms:

The purpose of assigning a definition is to make known the meaning of the subject, and we make things known by using, not any [arbitrary] terms, but those which are prior and more intelligible, as we do in demonstration (for this is true of every kind of teaching and learning); it is, therefore, obvious that the man who does not define by means of such terms has not defined at all. [Otherwise], there will be more than one definition of the same thing; for clearly he who has used terms which are prior and more intelligible has given another and better definition, so that both would be definitions of the same thing … The prior is simply more intelligible than the posterior; for example, a point is more intelligible than a line, a line than a plane, and a plane than a solid; just as the number “one” compared to any other number, since it is prior and the [principle and] starting point \( \alpha \rho \chi \eta \) of every number. Similarly, a letter is more intelligible than a syllable. To us, however, the converse sometimes happens; for a solid falls most under our perception, and a plane more than a line, and a line more than a point. For most people recognize such things as solids and planes before they recognize lines and points; for the former can be grasped by an ordinary understanding, the latter only by one which has been learned through the application of precise and superior thinking (Topica, VI, iv; translation based on E.S. Forster’s).

Having just introduced a thoroughly modern sounding distinction between what we may call a normative/structural versus a cognitive/procedural definition, Aristotle elaborates further on the significance of these newly introduced kinds of “priority”:

[In an absolute sense] it is simply better to aim at knowledge of the posterior by means of that which is prior; since this is more scientific. Nevertheless, for the benefit of those who are incapable of acquiring knowledge by such means, it is perhaps necessary to frame the description by means of terms which are intelligible to them. Among definitions of this kind are those of the point, the line and the plane; for all these demonstrate [\( \delta \rho \lambda \alpha \sigma \sigma \nu \) — declare] the prior by means of the posterior—the point being called the limit of the line, the line that of the plane, and the plane that of the solid (ibid.).
To avoid a proliferation of definitions,\textsuperscript{28} we are ultimately advised to define in terms of the \textit{what is}? as much as possible—namely keep defining the species in terms of its genus.

Thus, the Art of Dialectic is typically considered prior to Rhetoric (and Poetry too) for the simple reason that the precepts of the first are required for an understanding of the latter, but not necessarily the reverse. What’s more, Dialectic is also “prior” along the axis of epistemic certainty—a condition that is better described as an epistemic “superiority” of sorts. Indeed, there is an important asymmetry to be observed regarding the domain of application of each of the prototypical Arts of Logos. The books of the Organon (including those on Dialectic) are normatively considered to be the source of all broadly-construed scientific argumentation, both deductive and inductive. In this general sense which, as we have seen, goes beyond the narrow enclave of mathematics, “science” is understood to be a systematic knowledge gathering, the sort of rational activity that strives to produce factual statements, preferably of general and even universal validity. Rhetoric on the other hand, especially as used in the political arena for the deliberation of future action, typically relies on “memory-based” arguments (enthymemes), whose premises are derived from the historical experience of human affairs—a non-deterministic sphere par excellence. Her conclusions, therefore, tend to be contingent and frequently merely hypothetical.

Regarding the issue of inseparability in practice (as distinct from their epistemic interdependence discussed above), Aristotle freely acknowledges that in reality dialectic modes of argumentation are omnipresent in every systematic domain of thought and action, and rhetorical settings are no exception. For example, in legal rhetoric deductive or inductive dialectical thinking often plays an important role in determining whether the defendant is in fact the perpetrator of the crime, or if the court is instead faced with a case of mistaken identity. But even in cases where the identity of the perpetrator is established with certainty, the ultimate goal of legal proceedings is to determine the moral status of the defendant, his or her guilt or innocence. And this in the Greek world view necessarily involves a judgment regarding the intentional mental state of the defendant at the time of the crime—not just the physical fact that (s)he caused harm to another. This, of course, is a contingent matter with no universal reality or applicability that naturally falls within the domain of rhetoric.

More importantly, the reverse is also true. For all the strict requirements to avoid passionate means of persuasion, scientific exchange is still a form of discourse.\textsuperscript{29} It employs various rhetorical devices, and even abstains from the

\textsuperscript{28} Aristotle observes that different things are more intelligible to different people, and not the same things are intelligible to all. To make things worse, even to the same person a given thing may be more intelligible at different times.

\textsuperscript{29} Though obvious, it is worth pointing out that Aristotle’s philosophical discourse was taking place in an era before the discovery that human memory and everyday cognition are guided
dialectical order in favor of a rhetorical mode of disposition in those cases where a more pragmatic approach is needed in order to reach a certain kind of audience.

Similarly, in his Poetic Art Aristotle acknowledges that, even in the world of make-believe, the carefully constructed arguments and powerful speeches of imaginary characters rely on the same logico-linguistic techniques, and fundamentally aim at similar communicative goals as all other endeavors in the domain of Logos.

Nonetheless, the presence of strong affinities and use of overlapping techniques should not prevent us from keeping all these species of discourse separate according to the main epistemic goal of each. For example, in his Poetics Aristotle is quick to point out that, just because a scientific theory may be delivered in verse, its expressed purpose to stand as philosophical or reality-describing discourse falls short of the aesthetic goals of poetry (a type of discourse that evidently embodies the principle of mimesis; namely reality-imitation of the poietic, creatively constructed, make-believe sort):

Of course, people attach the verbal idea of “poetry-[making]” to the name of the meter, and call some “elegiac poets,” others “epic poets.” But this is not to classify them as poets because of mimesis, but because of the meter they share; hence, if writers express something medical or scientific in meter, people still usually apply these terms. But Homer and Empedocles [who composed two philosophical/scientific works in hexameters] have nothing in common except their meter; so one should call the former a poet, the other a natural scientist (Poetics, I; translation by Stephen Halliwell).

Overall, we could say that by tending to factors like truth status together with various pragmatic criteria governing speech-acts, we should be able to

by powerful mental constructs, which can now be found in the pertinent literature under such names as schemata, scripts, plans, goals, mental models, idealized cognitive models, and structural (conceptual) metaphors—just to name a few. Sadly the importance of cognitive structures in shaping scientific thought has yet to be fully explored by corresponding work in the area of epistemology, even though the first important steps in this direction were already taken in the first part of the twentieth century by the French philosopher of science Gaston Bachelard in his groundbreaking works The Psychoanalysis of Fire and The Poetics of Space. It is my personal position that the distinction between the rhetoric and dialectic of science has been (or should have been) seriously undermined by the “second wave” in cognitive science (the twin frameworks of Cognitive Linguistics and Parallel Distributed Processing). Even the boundaries between literal and figurative language have been shown to be porous (by Lakoff and Johnson in their 1980 book Metaphors We Live By, and a host of other authors who followed their lead). Both as daily practices and as systematic knowledge domains, the natural sciences crucially depend on “experience-filtering” and “experience-structuring” cognitive mechanisms—including those of “analogy” and “metaphor” first documented by Aristotle in his Art of Rhetoric and his Poetics—so that so-called “rhetorical” figures can no longer be dismissed as innocuous to the structure of the scientific dialectic.

Scientific discourse in the pre-classical era was often set in verse. Similar practices can be found in medieval school materials, which merely record in writing what was clearly intended for memorization. It would seem that, in cultures that favored orality in their daily practices, the requirements of memory gave rise to such mnemonic strategies.
provide a principled (albeit, neither context-free nor absolute) discrimination between “scientific,” “deliberative,” “poetic,” and other types of discourse.

Two additional points are raised by the opening gambit of *The Art of Rhetoric* quoted earlier. The first is that Rhetoric and Dialectic are “meta-Arts.” The second pertains to how an Art arises in the first place. These are discussed next.

iv

As meta-Arts, Rhetoric and Dialectic are the distillment of all rational activity. They pertain to the process of thinking and communicating *per se*—rather than the specific content of what is thought and conveyed in each systematic field of knowledge.

Aristotle distinguishes between, on the one hand, the general applicability of his Arts of Rhetoric and Dialectic and, on the other hand, the specific sciences. For him these Arts of Logos represent δυναµείς, namely “powers” or “potentialities” of the rational mind to achieve desirable results in all of its endeavors. They deal with the invention, order, and proper presentation of “reasons and justifications.” To use a musical example, a violinist needs to practice and conquer the technical demands of her instrument through appropriately designed finger exercises before she can tackle specific repertoire and achieve musical expression within the guidelines of particular styles. Similarly, a dialectician and a rhetorician both practice to achieve an effortless function of their logical apparatus, the workings of their mind, apart from any specific problem or issue they may be called upon to tackle with their practiced powers of thinking, arguing, and persuasion.

By contrast the various sciences are conceived as content-specific, object-oriented activities of the human mind that generate topic-specific knowledge with narrow applicability. Therefore, looking for “reasons” is not an “object”-oriented epistemic activity concerned with the underlying nature of the objects of reference “in-themselves,” but rather an activity concerned with the nature of the “speech act of referring” as such (and all other “logical/communicative” acts as well):

He who attempts to construe λογική, construct] rhetoric or dialectic, not into the powers/potentialities/faculties/abilities δυναµείς that they are in themselves, but into [object oriented] sciences έπιστηµα, dissolves their nature and makes them disappear in the process of crossing over into the domain of “deterministic” science; [by so doing] he refurbishes έπισκευαζείν them into sciences dealing with the underlying nature of specific things [namely, their substratum], rather than exclusively [µονον] [dealing] with [the knowledge of] meanings and rationales λογίων (The Art of Rhetoric, I, iv, 4–7).

31 Singular: λόγος—logos; plural: λόγοι—logoi.
As we have already discussed, in this context “science” means systematic intellectual activity seeking to understand all those aspects of our universe that could not have been otherwise—for example, the eternal laws of Nature. The use of the term “deterministic” in the gloss is an attempt to capture the non-contingent aspect of the objects of “science,” rather than restrict them to mechanistically construed linear phenomena. Therefore, this conception of science effortlessly accommodates modern uncertainty principles and chaotic behaviors exhibited by otherwise “non-contingent” physical systems. What it contrasts with are those sciences (termed here “technai” or “Arts”) which in fact seek to understand the contingent aspects of our reality: historical, linguistic, artistic, ethical, political, and so forth—in short, all matters that are contingent upon free will, intentionality, and the tribulations of human existence.

Thus, Aristotle forcefully maintains that the Arts of Logos are exclusively concerned with discovering and applying the principles of meaning creation and rational justification, something that is accomplished only at the level of our conceptually construed linguistic “reality.” Searching for the underlying nature and material causes of tangible things-in-the-world is the business of natural science. Any misguided attempt to “refurbish” these Arts into (presumed to be deductive) sciences dealing with the eternal and deterministic laws of nature will pervert their epistemic nature and divert them from their true mission!

ν

The third important point made in the opening of Aristotle’s Art of Rhetoric quoted earlier pertains to how one comes to know these Arts in the first place, namely the “genetic” process that gives rise to an Art:

Most of the people [already] do this [argue in court, etc.]; some do it by chance, others through force of habit [συνηθεια δια εξως; “practice sanctified by tradition” arising through what is thought to be mindless and repetitive habit]. But now that both of these are possible, it should be clear [how these acts are accomplished] and made into a method [δηλον οτι ειη αυτα και οδοποιειν; literally, made into a road or path]; for it is possible to examine [θεωρειν; to look intensely; to examine carefully; to theorize] the reason or cause [αιτια] why some attain their end by familiarity of habit and others “automatically” by chance; and such an examination [i.e., theorizing] all would at once admit to be the function of art [τεχνης εργον; namely, the proper work to be accomplished by an art, its job, its domain of expertise] (translation roughly based on J.H. Freese’s).

There is a rather peculiar sense of the concept of “artificial” that is introduced here. In our current use of the term, we say that a garment is made out of artificial man-made fibers, as opposed to the (typically more valuable and

32 See section 2D.
What Kind of Theory Is Music Theory?

highly desirable) natural materials. Thus, “artificial” usually carries negative overtones and in some cases becomes synonymous with “forgery” or “fake.” However, this is not so in this Aristotelian context. For him, “artificial” is a positive term of approbation, which opposes a negative “natural” state of confusion and chance where, in the absence of deliberation and proper planning, human actions may or may not be effective. In this sense “artificial” has very positive overtones and is synonymous with “artful”—that is, according to the precepts of the Art. Reflection and careful theorizing can bring intentionality and predictability where once reigned confusion and chance. Then by definition, Art is capable of inducing Aristotelian form (μορφή—morphe) on naturally given amorphous matter, thus elevating any “technically” organized field to the status of meaningful knowledge. In the case of Rhetoric, the unreliable habits of a naturally gifted but untrained orator may occasionally produce some good results, but it is only through the knowledge of the Art’s precepts that fully intentional effects can be achieved. Before leaving this point, it may be tempting to add the dichotomy of “order from chaos” to our toolbox of metaphors describing the effects of Art. But this is not the spirit of Aristotle’s texts. Nonetheless, this metaphor was popularly employed to evoke the effects of Method in the political, cultural, and religious climate of the late Renaissance, and especially that of the Enlightenment.

In view of the popularity of certain anti-Aristotelian (or more accurately, pseudo-Aristotelian) dialectic developments during the Renaissance (Rationalism in particular) we should also consider a negative use of the concept of “artificial” lurking in Aristotelian thought. An activity being dragged against its true nature, “artificially” as it were, into the realm of false logos was deemed by the Athenians to be the work of a sophist rather than that of a true philosopher. The important distinction between the proper employment of the precepts of the Art and their intentional abuse is made plain on many occasions in the classical literature; arguably the most entertainingly instructive among them being Plato’s dialogue “The Sophist.” This negative meaning of “artificial” is also addressed with exhaustive systematicity in the last

33 Aristotle’s method of division into species is inextricably linked to the conferral of linguistic meaning, an approach that has significant consequences for his system of thought.
34 The Masonic message of “light from darkness” found in Mozart’s opera Magic Flute comes to mind. A product of the Enlightenment, the Free Masons were a brotherhood of highly trained professional practitioners of an important field of technical knowledge: the Art of Stone Masonry. A similar message is, of course, conveyed by the Book of Genesis, where the Art-giving God bestows form upon shapeless matter. Poignantly, the Divine Reason made manifest in Genesis appears in the poetic imagination of Greek Christendom as God, the Master Builder (Πρωτοστορας).
35 Herbert A. Simon (1969), one of the celebrated fathers of the field of Artificial Intelligence, penned a famous apologia of the field he helped to develop entitled The Sciences of the Artificial. In hindsight it is fair to say that, given the state of AI at the time, its practitioners were far more interested in the possibility of creating new forms of robotic intelligence than in imitating natural human intelligence with any verisimilitude. Their paradigm fundamentally
book of Aristotle’s logico-dialectic Organon, his On Sophistical Refutations. This is an important work, whose Greek title Περὶ σοφιστικῶν ἐλεγχῶν promises to guide the budding thinker into the Art of conducting a deliberate and detailed “elenchos” (a critical investigation, a check, an audit) of sophistical fallacies for the purpose of exposing their technical flaws. Thus there are specific contexts in which we would be justified in our use of the term “artificially,” not in its usual positive sense “according to the precepts of the Art,”

36 but in a derogative sense of “forcefully construed against its true logical nature, in terms of a pseudo-logos.”

A true philosopher then, whether in the Platonic or Aristotelian tradition, is solely interested in discovering the actual logos or reason behind the “true” and “natural” order of things, not in providing some artificial pseudo-logical construct. Of course, where exactly this “true,” “actual,” “unforced,” and “natural” order of things should be sought was the subject of perennial debate. Plato sought to locate logos in the presumed to be permanent, albeit abstract and immaterial world of Ideas, while Aristotle sought logos in the ephemeral, but tangible amalgam of Nature with the time-honored habits of the Athenian linguistic community.

A collection of well-articulated true reasons that imbue a domain with logical sense is metaphorically construed by Aristotle spatially as the path laid down by the expert practitioner—the method [μεταθέωδον > μεθ'θοδον = with way/road/path]. These logically organized precepts constitute a precious body of knowledge and account for the skill of the expert. Most importantly, they function as a distilled teaching method, a set of principled instructions that can be transmitted efficiently and successfully to the novice student. Indeed, for Aristotle the ultimate measure of a techne’s worth is its teachability. Vague claims of ability are unconvincing evidence of the possession of knowledge. For one to have knowledge one must be aware of his procedures and the reasons behind them—all of which can be explicitly articulated. Thus, regarding the fact that Dialectic’s central task is teaching, the Stagerite states the following in his Metaphysics (I, i, 12):

In general the sign of those who have knowledge versus those who do not is the ability to teach, and for this reason we hold that Art more so than mere experience is science; for the first can teach, but the others cannot.

Such are the Arts of Dialectic, Rhetoric, and even the Art of Poetry, taught by Aristotle. Additional Arts, like Grammar and Medicine, though

36 depended on a notion of “functionality” that was construed in abstract symbolic terms and was inspired by design principles of serial computer software, rather than the complexities of natural biological “wetware.” The logic embodied in that software was presumed to be “functionally” equivalent to human thinking in some “relevant” aspect—an idea that did not take into account that human brains and the forms of intelligence they “support” are biology dependent, not merely biology based.

36 Something that I occasionally signal with the hyphenated “art-ful.”
traceable to humble beginnings of mere mental habit, were similarly elevated over time to the status of techne through the discovery of logos, enabling both efficient and “reason-able” codification of their respective time honored practices.

3. The struggle of grammar to become a techne

A
A latecomer to the Arts of Logos, Grammar joined the paradigmatic Aristotelian duo of Dialectic and Rhetoric only in late antiquity. The newly formed trio became known as the Trivium of the logico-linguistic curriculum of medieval and renaissance schools. However, this epistemic triumph of Grammar did not come easy, and was the result of considerable effort and methodological debate. To the degree that Music Theory today still struggles to find its epistemological footing, the history of Grammar’s rise to a higher epistemic status is very instructive.

R.H. Robins (1973) traces Grammar’s struggle for epistemic respectability to a historical controversy revolving around the question of whether a successful grammatical theory ought to be a purely descriptive collection of isolated word-forms and phrases catalogued and learned individually, or a “pattern-driven” domain of knowledge. In their extreme versions, the first option considers language to be an ad hoc collection of unique and idiosyncratic forms. The second option construes language into a rule-governed domain—an attractive notion that, nonetheless, makes it susceptible to excesses of prescriptive behavior. The specifics aside, what was essentially debated was the question of whether the newly independent field of Grammar deserved to be considered a bona fide principled Art or a mere craft.

B
We have already discussed the epistemic reasons that account for the distinction between (poietic and performative) Art and (deductive) science, and have concluded that the Arts of Logos, due to the contingent nature of their subject matter, cannot be absorbed into the sphere of the mathematical or natural sciences. The case of Grammar forces us to step on the other side of the hierarchy of knowledge (shown below), and try to understand what motivates the epistemological move from “practical” and “empirical” craft (empiria) to “theoretical” Art (techne).

In his effort to delineate and properly classify all knowledge, Aristotle starts his Metaphysics (I, i) by summing up and elaborating what he had said in his Nicomachean Ethics (VI, iii, 2–4). But now he has more to say on the distinction between the various levels of practical experience (empeiria—a cognate of “empirical”) and art & science (techne & episteme)—the latter two considered here together in their opposi-
tion to practical experience. His discussion yields the following dialectic division:

```
το ειδεναι
"the knowing"
```

εμπειρία
experience
tεχνή & επιστήμη
Art & science

This conception (where “knowledge” is not static, but is considered procedurally as an active “knowing,” a set of intentional actions capable of producing desired effects) unfolds into the following detailed hierarchy of knowing:

εχων αισθήσιν

− εμπειρος
− τεχνητος (χειροτεχνης < αρχιτεκτον)  
− επιστήμων (ποιητικαι τεχναι [τεχνικος] < θεωρητικαι επιστημαι)

one with (casual) sensory exposure
− one with practical experience
− artisan or craftsman [technitis] (where craftsman < master-craftsman)
− scientist (where the poietic Arts [technikos] < the theoretical sciences)

Starting at the lesser end of the hierarchy of knowing, the one who has mere sensory contact with a skilled domain (i.e., has been exposed to an activity and therefore has some casual experience of it) is inferior to the one who is “self-taught” as a result of hands-on experience (occasionally referred to as πρακτικος—praktikos\(^{37}\)). Such person is in turn inferior to the artisan or craftsman with very long practical experience resulting in some level of restricted and context-specific knowledge. Here the knowledge of a mere craftsman\(^{38}\) who works with his hands is considered lesser than the knowledge of the same domain held by the master-craftsman, who presumably has a global view of the project and is in better command of the *logos* guiding the activity. However, even this type of knowing is limited, as it is context-bound and ultimately depends on unexamined habit for its success. As such it is inferior to the knowing of a scientist, who has at her/his disposal systematically formulated reasons, principles and methodologies (*logoi*) which not only account for the success of the artisan’s habits, but have general applicability. Among the sciences (the term employed here as a genus to mean

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\(^{37}\) Akin to the English term “practitioner,” but restricted to this much narrower definition.

\(^{38}\) Notice the subtle difference between τεχνητος (technitis) and τεχνικος (technikos). The first is a mere craftsman or artisan. The second is the practitioner of an Art, properly called an Artist.
“domains of systematic knowledge”), the poietic technai of the contingent are considered inferior to the theoretic sciences of the immutable.

These last two domains taken together form a common genus of “science” insofar as they contrast with the practical domains of mere empeiria and even craft. Since the specific logoi discovered by craft are presumed to be context-bound, they lack the level of generality and broad ability to cope with new situations—the sort of flexibility that is, by contrast, fully expected of the Arts and sciences. This then is how Aristotle frames the issue of knowledge-acquisition along an axis of specificity, whose two poles can be thought of as token versus kind, or context-boundedness versus generality of applicability for its precepts:

It is from memory that men acquire experience [empeiria], because the numerous memories of the same thing eventually produce the effect of a single experience. Experience seems very similar to science and art, but actually it is through experience that men acquire science and art; for as Polus rightly says, “experience produces art, but inexperience chance.”

Art is produced when from many notions of experience [οταν εκ των πολλων της εμπειρίας εννοήματον] a single universal judgment is formed with regard to like objects [μια καθολον γενήται περὶ των ομοίων υπόλημας]. To have a judgment that when Callias was suffering from this or that disease this or that benefited him, and similarly with Socrates and various other individuals, is a matter of experience; but to judge that it benefits all persons of a certain type, considered as a class, who suffer from this or that disease … is a matter of art [techne] (Metaphysics, I, 1, 4–6; translation by Hugh Tredennick).

Aristotle is very quick to point out the pitfalls of his scheme, which should not be taken as a judgment of the general “worth” of one domain of knowledge over another, but only as intended—namely an epistemological evaluation along an axis of specificity versus generality:

It would seem that for practical purposes [in the domain of praxis; regarding the “doing”] experience is in no way inferior to art; indeed we see men of experience succeeding more than those who have theory without experience. The reason is that experience is knowledge of particulars, but art [is knowledge] of universals; and actions and the effects produced are all concerned with the particulars … for it is the particular that must be treated.

What follows brings us even closer to the crux of the issue:

Nonetheless we consider that knowledge and proficiency belong to art rather than to experience, and we assume that artists are wiser than men of experience (which implies that in all cases wisdom depends rather upon knowledge); and this is because the former know the cause [αιτια], whereas the latter do not. For the experienced know the [specific] fact [the “that”], but not the wherefore [lit., “the cause and the ‘because’”]. For the same reason we consider that the master craftsmen in every profession are more estimable and
know more and are wiser than the [practical] artisans [who work with their hands], because they know the reasons [logoi] of the things which are done … Thus the master craftsmen are superior in wisdom, not [in the practical aspect], but because they possess a theory [logos] and know the causes (Metaphysics, I, i, 7–12; translation by Hugh Tredennick).

Let us now consider the exact nature of the “causes” and “reasons” that pertain to each type of Art or science. As we have seen earlier, any activity which at first sight was perhaps construed as a mere habit (of body or mind), upon careful reflection can be found to be governed by logos—principled explanatory patterns. What a domain needs in order to escape the sphere of the “practical” and qualify as a “theoretical” Art or science is a higher level of predictability based on “a reason.” The desideratum is an explanation of sorts marked by the presence of order and logical flow that affords us better control of the artistic medium.

This requirement by itself does not specify whether the logical “explanation” in question should be a formal, material, efficient, or final cause. It does not even specify whether it should operate in the sphere of the natural, man-made, or magical. Though, admittedly, the latter is ruled out by the very nature of the Greek philosophical enterprise, which sought to rise above the otherwise wonderful universe of its own mythical and religious traditions, and looked for the kinds of answers found through critical reflection and reasoning. Logical coherence, a sense of “down-to-earth” plausibility, and an unbroken chain of (physical) causality became paramount—the very antithesis of any magical pseudo-explanation.

It soon becomes evident that Aristotle allows for a variety of species of explanation, each in accordance with its own domain. The sphere of mathematics demands a different form of explanation (that relies on deductive reasoning flowing directly from generally accepted a priori first principles) than the kind required in matters of biology (inductive and analogical reasoning based on empirical data). Yet another kind of explanation is appropriate to all things linguistic; man-made and contingent, they frequently seem to require “formal” explanations. Even within the narrow province of the Arts of Logos, Dialectic requires a different level of demonstration (reasoning based on historical and other generally accepted facts) than what is appropriate for Rhetoric (use of examples and hypothetical reasoning based on mere plausibility of scenarios that are used to weigh possible future actions and their likely consequences).

39 See section 2C.
40 This sense of “formal” pertains to the distinctly Aristotelian concept of Form that shapes the material Substratum. As explained at length in Kotzakidou Pace 2006 and 2002 [1999], this classical notion is the very antithesis of the modern sense of “form” we encounter in such expressions as “mathematical formalism” or “formal semantics.”
Then, what about the case of Grammar? What is the type of explanation that would be appropriate to its peculiar subject matter and, therefore, adequate for the purpose of elevating it to the “theoretical” status of an Art?

While in other cases the logos in question can be an “external” explanation in terms of material or efficient causes, it seems that in Grammar’s case the act of gathering into an orderly arrangement constitutes in itself, not merely signifies, the existence of a principle. In this case logos can be thought of as operating “internally”—a formal cause par excellence. Rather than being produced by a material cause or mechanism (as is the case with physical phenomena), Greek and Latin are found to be “governed” by elaborate analogical patterns, also known as “paradigms” of grammatical declension and conjugation. These are the Aristotelian formal causes of their respective languages. Once discovered and shown to adequately account for the data, the internal organization of these paradigms constitutes a uniquely “grammatical” logos. In other words, the grammatical data themselves are shaped into a system, whose epistemic justification is the fact of its existence. Should someone become interested in investigating the biological or other material causes of such grammatical behavior, they are, of course, encouraged to pursue their mission. But then they need to be advised that their engagement with fields such as Biology, Physics, and Chemistry will be subject to the methodological strictures of those fields—and would have little or no bearing on the epistemically independent and self-sufficient Art of Grammar.

If we venture into the domain of Music Theory, which, as we will see shortly, enthusiastically joined the Renaissance Arts of Logos, we are immediately reminded of Burmeister’s (1955 [1606]) notion of musical syntax (συνταξις—syntaxis). This grammatically inspired notion ingeniously invokes the sort of information he is able to present in neatly arranged charts (Figure 1). Not a mere matter of convenience, or even clarity of presentation, this is a matter of deep epistemological significance and a source of great pride for the author. He evocatively names his charts of consonances after the grammatical paradigms known as συζυγιαι (syzygiai). These are patterns of grammatical “analogies” (verb conjugations and noun declensions) arranged in neat vertical columns familiar from Greek and Latin grammar books (Figure 2). The arrangement itself is thought of as the embodiment of the “logos” (rationale, precept, law) that elevates Grammar (and Burmeister hopes the grammatical component of his Musica Poetica as well!) to the coveted epistemically independent and self-sufficient Art of Grammar.
### Figure 1. Facsimile of “first schema of consonances”; Joachim Burmeister, *Musica Poetica* (1955 [1606]).

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### Figure 2. Facsimile of verb conjugation paradigm; Philipp Melanchthon, *Grammatica Graeca* 1969 [1527].

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D

Let us now briefly recount the historical steps of the field of Grammar.\footnote{This section is largely based on the following: Robins 1973 and 1997 [1967]; Johnson and Burge’s commentary in Stahl 1991 [1971], 99–104; Fries 1927; Colson 1919. See also: Koerner and Asher 1995 (Section V).} The tradition of Greek grammatical thought between the fifth and second centuries B.C. is linked with the names of Protagoras and Prodicus, Democritus and Aristotle, the Stoics, and finally the Alexandrian critics of the second century B.C. It is well documented that in the second century B.C. two competing schools of Greek grammarians, the “Anomalists” and the “Analogsists,” were engaged in a heated debate. Not surprisingly, it is often pointed out that, rather than an explicit, well-delineated controversy, the two labels stand for extreme expressions of concurrent trends in Hellenistic grammatical theory. Their positions are often softened and even reconciled in the work of the most thoughtful grammarians of that era.

Originally associated with the general philosophical program of the Stoics of Pergamon (namely Stoic natural philosophy and their ethical mandate of living in accordance with nature), the Anomalists maintained that language is a natural process in which there may be “resemblances” but no strict “rules,” so that variety, coinages, and anomalies are to be expected. Though his overall credentials as a stoic are uncertain, the prolific philologist (or “critic,” as he preferred to be called) Crates of Mallos was an outstanding adherent of the anomalist view of language. He is one of the best-known commentators on Homer and flourished in the city of Pergamon, where he is thought to have been the head of its famous library. Crates is also the author of possibly the first Greek grammar book—a collection of linguistic facts detailing the differences between the language of Homer, the classic Attic writers, and that of their own Hellenistic times.

The Alexandrian Analogist school of thought is associated most closely with the rival school of Homeric philologists-critics of Alexandria, and it is represented by Crates’s contemporary Aristarchus. Stated in its extreme form, the Analogists adhered to the dual view that there is a strict law of “analogy” between the word and the idea for which it stands, and that there can be no exceptions to the grammatical rules they laid down. These rules were thought to represented the logos, the underlying principle, that explained the “formal” analogies (both phonological and morphological) found in linguistic data.

Aristarchus’ ideas were carried on by his follower Dionysius Thrax, who flourished in the Hellenistic period circa 100 B.C. during a time of intense Roman interest in the Greek language. He authored a highly influential Greek grammar book, which became the model for the first Latin grammars in their dual effort to apply the analogists’ principles and to translate Greek grammatical terminology in their own language. In the centuries that fol-
lowed, “analogy” triumphed as the principle believed to underlie natural language—even though over time the original strictness of the doctrine was relaxed and the rules were acknowledged to have some exceptions.

The Latin grammars modeled upon that of Dionysius Thrax formed a tradition that leads to the fourth-century A.D. Latin grammar of Donatus and that of Priscian (ca. 500). In turn Donatus and Priscian are the two main sources informing both the Greek and Latin grammars of the Middle Ages and on to the early Renaissance Humanists. Thus, the first vernacular grammars already appearing in sixteenth century Western Europe are inevitably influenced by the analogists’ point of view.

The very title of Dionysius’s seminal book *Techne Grammatike* (Grammatical Art) clearly evoked the Aristotelian classics *Rhetorike Techne* (The Rhetorical Art) and *Peri Poietikes [Technes]* (About [the Art of] Poetry). Despite this prominent appearance of the term techne—controversy did arise among his later commentators regarding the status of Grammar among the Arts. Dionysius was accused of actually diminishing the status of the field when he opened his short treatise with a definition of Grammar as “the practical knowledge [empeiria] of the general expressions of poets and prose writers.” This understandably raised a red flag for some, since in Dionysius’ time the accepted hierarchy of knowledge was the same as the one discussed earlier, restated by Robins (1973, 13) under somewhat different English terms: “experience [peira] advances to practical knowledge [empeiria], practical knowledge to science [techne], and science to understanding [episteme].”

By placing it in the context of the polemical Analogist-Anomalist debate, Robins rebuts the charge that Dionysius’ aforementioned definition downgraded the nascent field of Grammar to the level of a merely empirical subject. In fairness, Dionysius Thrax was immersed in the Alexandrian scholarly tradition of Homeric literary studies and was actually associated with analogist principles. Even so, grammatical description for the Alexandrines was first and foremost textual, so that the “working out of regularities” was always based on a body of actual linguistic data. In such a data-driven theorizing, idiosyncrasies that present exceptions to the analogically formed paradigms were fully expected and accepted as natural. By contrast, it was only radical analogists who attempted to eradicate irregularities from natural language by substituting “regularizing” neologisms for obstinate forms which did not align with their stated paradigms. These normalizing pressures which were present in some professional circles are also acknowledged by Householder, who in the introduction to his translation of Apollonius Dyscolus’ book on Syntax states that

some grammarians, in their writings, specialized in one or other branch of the art [Grammar]. Those who specialized in linguistic topics, like Apollonius or
Dionysus, were known as *technikoi*.

Another function of the grammarian, which might also be part of the specialty of *technikos*, was improving the students’ Greek. The attempt to put brakes on linguistic change for which school-teachers have long been condemned by some of their critics had already begun in antiquity, certainly before Apollonius’ day, perhaps already in Plato’s …

In any case, regarding the partisan one-sided charges against Dionysus, Robins concludes:

> in terms of the *techne-empeiria* distinction Thrax, probably following Alexandrian practice, was anxious not to claim more for a descriptive grammar (strictly phonology and morphology) of Greek than could be implied by *empeiria*, though in a more general sense he made use of the term *techne*, thus placing grammar among other principled theoretical and practical activities recognized in the contemporary culture of ancient Greece (15).

E

In conclusion, it is worth reflecting on the possible reasons behind Robins’s peculiar, yet very instructive choice of English terms in his translation of the Aristotelian hierarchy of knowing. While raising some much needed sympathy for the translator’s predicament, it will also help us understand why some very capable commentators have at times opted for convoluted translations of otherwise perfectly lucid Greek.

In the aforementioned quote R.H. Robins, writing for his 1973 audience, translated the term *techne* as “science” and the term *episteme* as the needlessly nebulous “understanding.” While (at the level of species) I personally favor the more transparent translation of *techne* as “Art” and *episteme* as “science,” I fully sympathize with Robins’s communicative intent to impress upon his readers that in the ancient Greek mind *techne* occupied the same place of intellectual respectability as *science* does for us today—namely, a systematically gathered and well organized body of knowledge whose precepts are capable both of satisfactorily elucidating the subject matter and of being imparted to the next generation of learners in an efficient and effective manner.

In the space of a short article with no room for extended glosses, Robins followed what could be termed the translator’s *default* directive: unless compelled to do otherwise, try to express foreign culture-laden concepts in terms of nearly-analogous local and contemporary vocabulary. In this case, try to express ancient Greek concepts in terms a 1973 English speaking audience will understand. Thus, he opted for the term “science” as a translation of the Greek *techne* in an effort to convey the import of that concept by a single term, and at the same time to avoid any unwanted association with

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42 See the hierarchy of knowing we examined in section 3B.
the currently degraded term “art.” In choosing the more direct translation “Art,” and occasionally even retaining the original term techne, I hope to invite my reader to see the Greek Weltanschauung from an internal vantage point and (contrary to prevailing academic winds) to open the door to the view that epistemically the humanities are technai—and need not pretend to be anything else.

Figure 3. Dialectic Division (“Partition”) of knowledge according to Gregor Reisch (Margarita Philosophica, 1503). Source: Heikki Mikkeli 1999.

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43 See fn. 2.
Figure 4. Dialectic division of “habit” according to Thomas Blundeville (1667 [1599]).
4. The Revitalization of Music’s Affiliation with the Arts of the Linguistic Trivium

A Gregor Reisch’s early division of knowledge (Margarita Philosophica, 1503) is a nearly faithful Aristotelian scheme, which under the category of “theoretical” knowledge correctly classifies the (metaphysical, mathematical, and physical) sciences together with the “rational” Arts of the Trivium (Figure 3). Under the “practical” fields he correctly lists the action-oriented domains of Ethics, Politics, Economics, and “Monastics” (presumably a form of institutional administration) but also a new category of practical fields concerned with “making”—distinct from the Aristotelian poietic Arts, which for Reisch are still a form of theoretical knowledge. Though under the lofty super-genus of “Philosophia” (literally, “Love of Wisdom”) this new category sounds more like “applied crafts.” It groups Weaving, Armature, Agriculture and Hunting together with the fields of Medicine (held in lower esteem in in Reisch’s time than in the ancient or modern world) and Theatre (which in this context conjures up the “craft” of street performers more than the poietic aspect of Poetry and Drama). Notice however how Reisch’s dialectic division, when read from top to bottom, proceeds hierarchically along the (combined) axis of certainty and loftiness of subject matter—thus reproducing Aristotle’s basic scheme of the Arts.

No such order is discernable in Blundeville’s (1967 [1599]) division of “habit” (a direct translation of the Aristotelian term εξις), which departs considerably from the classical scheme (Figure 4). Nonetheless, under the genus “science” Blundeville similarly groups mathematics and the natural sciences together with the aptly named “rational sciences” of grammar, logic, and rhetoric. Clearly, the source of the genus of “certain” knowledge (whose slightly erroneous name is intended to set systematically obtained scientific knowledge apart from mere opinions and hearsay) is still Aristotelian’s traditional division (Nicomachean Ethics, VI, iii, 1):

Let us say that those through which the mind/soul [psyche] achieves truth in affirmation or negation are five in number; these then are: art [techne], scientific knowledge [episteme], prudence [fronesis], wisdom [sofia], intellect [nous]; as notion/impression [hypolepsis] and opinion [doksa] can turn out to be wrong.

Though this does not affect us here directly, it is worth noting that (compared with what Aristotle would have paired together under a common ge-

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44 It is quite common by now to identify Dialectic with (elementary) Logic alone, so that in both of these divisions the constituents of the Trivium are listed as Grammar, Logic, and Rhetoric.

45 See section 2D.
Blundeville’s fourth category labeled “Art” is divided into Liberal (Architecture and Husbandry) and Mechanical (the crafts of tailor, shoemaker, and carpenter). This new category is the result of some interesting “cut and paste,” and is the sort of phenomenon that holds great interest for students of semantic and social change. Blundeville is an English author writing at the dawn of the seventeenth century. Whether by intention or ignorance, he is compelled to alter the traditional scheme, presumably in the direction of his contemporaries’ changing linguistic habits. He casually inserts new but quite “believable” categories under the otherwise intact labels of the classical scheme—a linguistic process that is licensed by garden variety ambiguity and polysemy; the result of non-technical use of words by a culture that is (linguistically, historically, socio-economically, politically, and philosophically) worlds apart from the semantic origin of these terms in the intellectual and material universe of classical Greece.

Of still greater interest is the fact that, under the guise of “Harmony,” a field called “Music” maintained a steady membership in the mathematical Quadrivium of Geometry, Arithmetic, Astronomy and Harmony throughout the Middle Ages and as late as the mid-seventeenth century. This is seen in all traditional divisions of knowledge (such as those reproduced above) and, of course, in Martianus Capella’s elementary exposition of the seven liberal “Arts.”

Indeed, owing to partial semantic overlap of the terms music and harmony, the idea that Music was a type of knowledge that essentially pertained to numerical ratios of string segments persisted for far longer than the paradigm that supported it. To us today this classification is highly misleading in at least two ways: (1) what we now mean by the term “music” is not what the term signified when it functioned in the old classification, and (2) past the first decades of the sixteenth century this categorization was already a relic and a gross anachronism, though still of service in certain limited quarters.

To begin with, the Quadrivium “Art” of Harmony had precious little to do with our modern concept of the domain of music, as it was neither “music” nor, strictly speaking, an “Art” (on a par with the linguistic Trivium of Grammar, Dialectic and Rhetoric). Rather, the classification of all seven liberal fields of study as “Arts” shows that in the medieval school setting the term was over-generalized in the direction of the deductive sciences. At a time when the remarkable intellectual achievements of the ancients in the domains of Geometry, Arithmetic, Astronomy, and the physical sciences had faded from memory, the subtle distinctions between the “technical” contingencies of the linguistic fields and the “scientific” certainty of mathematical knowledge observed by Aristotle were deemed unnecessary. As the reader may recall, the feature that had differentiated Art from science was their
relative position along the axis of epistemic certainty—driven in turn by the nature of their respective objects of study. But this criterion was only reinstalled to the center of philosophical discourse after the scientific revolution was well under way, and René Descartes (1596–1650) was motivated to zealously embrace it.

The medieval Weltanschauung was an odd mixture of Christian theocratic views and remnants of the holistic Greek view of the natural universe. It unified the realm of the heavenly stars with the earthly kingdom, both natural and human, through the rather astute observation that they were all manifestations of various modes of vibration. As such, they should be open to the possibility of attunement via sympathetic motion. The heavenly motions were put in correspondence with the movements of human “humors,” the fluids presumed to be responsible for our bodily health and psychological moods. Then, the physical vibrations of musical strings became the mediator that connected Man, Nature, the Heavens, and God Himself into an unbroken chain of Being, the magnificent cosmic “Symphony” immortalized by medieval mystics from Hildegard von Bingen to Ramón Llull.

The rising historical interest in the aesthetic and technical aspects of ancient Greek music-making was accompanied by a nearly concurrent decline of Harmony’s epistemic independence among the mathematical fields of the Quadrivium—eventually to be absorbed into the brand new science of Physics and the vibrant new field of Dynamics, in particular. No longer the spiritual touchstone at the heart of the Quadrivium, what was left of the old domain shifted its focus and reinvented itself into a discipline dealing exclusively with the “earthly” sights and sounds of musical composition and performance—namely, all the activities we recognize today as the prototypical occupation of professional musical studies. But given the affinity of their verbal labels, it will perhaps come as a surprise that this new concept of Music—what we today understand by this term—attached itself not to the pre-existing quadrivial domain of Harmony, but to the linguistic Trivium.

Naturally, an interest in tuning and the production of instrumental sound continued to be cultivated by musically inclined thinkers, but it was now increasingly understood that such preoccupations were the task of mathematicians, physicists, and instrument makers—not the vocation of practicing musicians. To put it differently, the sages who the ancients had called “musicians” (a cross between an astronomer, astrologist, mathematician, physicist, psychologist, physiologist, medical healer, poet, amateur lyre player, non-professional singer and dancer) were now primarily “naturalists,” “natural philosophers” or “scientists.” Just as rapidly, the practical sphere of activity of the performer/composer (previously held in mild to severe scorn) was emancipated, rehabilitated, and quickly upgraded to a worthy technical profession suitable for the rising middle class.

The humanist revival of classical learning made it clear that the traditional distinction between theoretical knowledge (of interval ratios) and the practi-
cal/technical knowledge needed for actual music-making fell neatly along the Aristotelian lines of (demonstrative and eternal) science and (example-driven and contingent) Art. The latter, considered in both its performative and productive species, could be further set in correspondence, on the one hand with the activities of musical praxis (performance, including improvised polyphony), and on the other with the compositional activities of poietic music (increasingly known by the name musica poetica). Thus, given the apt choices provided by the classical scheme (and its various Renaissance reincarnations), it was only natural for the rising “new” genus of (composed and performed) Music to align itself with the Aristotelian Technai. As the name musica poetica indicates, the art of musical composition in particular became an honorary member of the most prestigious of them all: the poietic Arts of Logos. Any remaining issues still calling for the sort of deductive proofs and scientific demonstrations best suited to the mathematical and natural sciences were quietly delegated to them.

Old paradigms die hard, and as late as the seventeenth century the theoretical preoccupation with interval ratios was still operative in the somewhat “old-fashioned” notions of Universal Harmony, which guided the otherwise cutting-edge work of astronomer Johannes Kepler (1571–1630) and the truly marvelous treatises (just as “avant-garde” in their own way) of polymath Athanasius Kircher (1601–1680). In a surprising turn of events, this latter-day engagement with the old holistic view of the cosmos did eventually yield the first bona fide fruits of modernity, particularly through the mathematical work of Kircher’s devoted follower G.W. Leibniz (1646–1716).

C

As the impressive array of musical treatises produced in the German lands during the early decades of the sixteenth century show, this was the historical moment during which the meaning of the term “music” began favoring the sphere of practical music-making and the experiencing of music in sight and sound. But like all great semantic shifts, it had started gradually at a far earlier time and, once under way, was not accomplished overnight.

Indeed, nearly two centuries before its wide circulation in German treatises we find a remarkably balanced view of Music embodied in the groundbreaking early Renaissance manifesto with which the Northern Italian theorist and composer Johannes Ciconia (ca. 1370–1412) prefaced his otherwise mostly speculative treatise Nova Musica (1993 [ca. 1411]). Ciconia readily recognized the multifaceted epistemic complexity of the domain with these words:

The ancient music, produced by the will of the ancients, which they themselves were unable to expand into a complete doctrine, we wish to revive in a

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46 See Kotzakidou Pace 2006 (Chapter IV).
new style, to leave out those things that were not appropriate, to perfect those that were inadequate, and to add those of which they were unaware. Who among the authors, in imitation of the art of grammar, has discovered the declensions of music that are in songs? Or who before has heard these? Who would have believed it to have accidents and declensions like grammar, genera and species like dialectic, and numbers and proportions like arithmetic? For its declensions, then, it is paired with grammar. For genera and species, it is likened to dialectic. For numbers and propositions, it is equated with arithmetic. It is necessary, therefore, in this work to branch the mind out in many directions and yet maintain the continuous sequence of speech, and to put forth many sayings of the authors so that the new music may grow by adding to the many sayings of the authors and may maintain their semblance in both the spoken style and doctrine of antiquity, and because that which is revived will be better arranged, it may be called new. In short, we wish to state openly—in the views of the authors—some of their sayings, to expound then upon some, and to add some of our own. We wish also to compose this book in four books and in eight plenitudes. The first book will be on consonances, the second on species and songs, the third on propositions, the fourth on declensions. The plenitudes, indeed, are pitches, consonances, species, modes of the tones, songs, proportions, accidents, and declensions. All of which with full justification will produce music and will be arranged in an entirely reasonable manner (1993 [ca. 1411], 53; Ellsworth’s translation).

Ciconia’s lucid and succinct presentation chronicles the dawn of the new musical era in Northern Italy. Despite this early start, the redrawing of the disciplinary lines for Music in Italy also came to maturity during the late Renaissance, roughly the period 1500–1650. This is how historian Ann Moyer (1997) summarizes her impressions of that era:

The study of music as understood by Doni and his generation in the seventeenth century differed from that of the age of Gaffurio in the late fifteenth century. The center of the field had shifted from proportion and pitch to musical compositions, and the relationships to other disciplines had changed accordingly. Mathematics remained important in studying the science of sound and in the descriptions of pitch and tuning systems, but not in aesthetics; astronomy and cosmology no longer received more than cursory mention. Mathematical explanations were also restricted to analyses of sound production. The study of sound perception now joined other attempts to explain perception, emotion, and reason as part of medical scholarship … Within the studies that continued to be known as the discipline of music, the most important single transition was the introduction of analytical tools from the humanist tradition (Moyer 1997, 198–9).

Despite the aforementioned survival of certain elements of the old paradigm in the astronomical theories of Kepler, the enthusiastic embrace of the Arts of Logos by the field of Music throughout the sixteenth century makes the newly restructured and “methodized” discipline look decidedly “progressive” and in keeping with the fast paced intellectual revolution of its day: the
reinstatement of the ancient concern for the rationality of Man as the object of systematic inquiry.

In Italy the shift was made apparent in Zarlino’s two-fold partition of the discipline of Music: one methodical, dealing with the timeless and absolute mathematical principles of proportion and other such axiomatic principles, and the other “historical,” what epistemologists call “contingent,” handling the particulars of musical practice and ever-changing custom. In Moyer’s (1997) view, the introduction of this distinction signals the onset of the study of musical style as a cultural phenomenon open to the humanists’ methods and procedures. This shift was solidified further when Vincenzo Galilei distinguished a science of sounding bodies from an art of music. The decline of the Boethian tradition, which had placed Music at the center of the numerical sciences, is further documented in Italy around the turn of the century. Circa 1578 Giovanni dei Bardi defined music as a composition of words, harmony, and rhythm, and mentioned neither science nor mathematics. In 1592 Lodovico Zacconi distinguished between “music-as-science” involving number and proportion, used by arithmeticians and architects, and “music-as-art,” which relates to sounding music, choosing the latter as his subject of inquiry. Finally, circa 1628 Vincenzo Guistiniani noted that “music ranks high among the arts called liberal, approaching but not reaching the ranks of science” (Moyer 1997, 188).

Of course, a secular engagement in systematic studies of history, philology, aesthetics, psychology, ethics, and politics, as well as scholarly investigations of the Christian Scriptures, is already synonymous with the revival of classical Greco-Roman culture spearheaded by the pan-European humanist movement. But what historians of ideas and musicologists alike have hitherto handled solely as a cultural and historical phenomenon, does in fact include a very strong systematic component that demands the attention of the modern researcher. The logico-communicative focus of the sixteenth-century Trivium (which adopted Music as its honorary member, when the latter finally embraced its cognitive, emotional, communicative, and aesthetic aspects in earnest) is what sets these classic Arts in direct correspondence with the modern preoccupation with human cognition under the banner of a broadly construed Cognitive Science.

Though the particulars may differ outside Italy, during this same period of 1500–1650 the decisive alignment of the discipline of Music with the linguistic arts of the Renaissance Trivium found exceedingly fertile ground beyond the Alps, notably in the mostly Lutheran North. Like their Italian counterparts, German theorists turned towards the Aristotelian linguistic technai to gather the concepts necessary to structure the new domain of musical practice and sounding music. Unlike the Italians, however, they plunged into the Aristotelian systematic Arts with such gusto that the fruits of their industrious labors in Rhetoric and Dialectic established the characteristically German tradition of musica poetica (the poietic Art of “making,”
“crafting,” “creating,” or “composing” music). True to the spirit of the Aristotelian enterprise, the Northern emphasis on the “conceptual” aspects of the domain (admittedly at the expense of its epistemologically inferior performative aspects) is inextricably mingled with the twin communicative doctrines of Imitation and the Affections and marks the beginning of the German Compositionslehre. These principles saturated both the Renaissance and Baroque aesthetic with a coherent and fully integrated theory of compositional technique and musical expressivity. Its patrimony is to be traced directly to Aristotle’s monumental investigation of the duality of Logos (organized thought articulated through orderly speech), as recorded in his treatises on the Art of Rhetoric, the Poetic Art, and the logico-dialectic Organon that sustains them all.

5. Music in the neo-Aristotelian Arts of Logos: Evidence from the German sixteenth century

A

In a language that unmistakably echoes the humanist concern for the restoration of the Arts of the linguistic Trivium to their ancient glory, Sebald Heyden (1499–1561) is actually talking about Music when, in the introduction to the third edition of his textbook De Arte Canendi, he vows to free “the [liberal] arts from abuse and barbarisms” and restore them “to their natural condition and true practice” (Heyden 1969 [1540], in Miller’s 1972 translation). His classification of music together with the linguistic Trivium is further established in his next paragraph:

Moreover, I also wonder sometimes who has taken on the greater labor, those who first created these arts or those who are now restoring them. Grammar and Dialectic can serve as examples (ibid.).

And to justify the continuing revision and expansion of his treatise that takes place with each new edition, he is compelled to reveal his cultural and scholarly heroes:

How many books about them [Grammar and Dialectic], in what quantities of editions, revisions and amplifications have now seen the light of publication? Yet you can hardly find any authors who do not want to change or enlarge such editions of their own works in some place or other, even in the latest edition. Erasmus of Rotterdam, a most distinguished luminary of scholarship, has left his own testimony: one would no more stop reworking his enriched and more substantial lucubrations than he would stop living (ibid.).

It seems then that since the primary concern of sixteenth-century German humanism (both Catholic and Lutheran) is the “restoration” and active culti-
vocation of the linguistic Trivium, the domain of Music is simply eager to oblige and adapt to the new intellectual climate. The perennially neglected mathematical Quadrivium remained on the back burner of the northern epistememe until well past the mid-seventeenth century—when the new geometrical paradigm that Descartes popularized will establish its dominance, and through the demagogic promise of a generalized mathematics-like “certainty” will eventually insinuate itself even into the Arts of Logos.

B

Published a little more than a decade after Heyden’s treatise, Adrianus Petit Coclico’s (ca. 1500–1562) textbook Compendium musices (1954 [1552]) contains explicitly stated evidence of a self-conscious turn away from the mathematical fields of the Quadrivium. According to Coclico’s linguistic construal of the domain, music theory should henceforth focus on the (liberal) Art of musical composition.

Coclico prefaces his textbook by disparaging the (mathematically oriented) “theorists” of music with feigned praise, promoting instead the “method” actually used by “practical musicians”—those “princes of music” Josquin des Prés (whom he claims as his teacher), Pierre de la Rue and others:

They [namely, the mathematically oriented “theorists”] have treated of the doctrine of the scale and the tones, certainly of great necessity, in a thoroughly dull way, relying mainly on a zeal for this to sketch diligently the proportions and other things on which it is useless to delay. Moreover, they teach nothing or teach obscurely on the manner of singing elegantly, on counterpoint or on composition. As a result, no youth could prepare for himself a solid comprehension of this art (of music) (Coclico 1954 [1552], in Albert Seay’s 1973 translation, 1).

He goes on to explain what the positive contribution of his approach is:

Since all those outstanding artists whom lower Germany has produced never directed their thoughts to the writing down of precepts concerned with the art of music, which is seen more in their practice than in rules, ... I have written this little volume and epitome concerning this art, which ... I have confirmed by practice, by comparison with that of others by discussion, and in it I pass on in a simple manner those things which are pertinent to the task (ibid.).

Coclico’s book, in exemplary humanist form, advertises itself as a brief and clear exposition of those precepts which need to be memorized in order to learn the art of singing rapidly and correctly. That the “art” in question was in fact an Aristotelian liberal Art, and not a “fine” or “beau” art (as Seay anachronistically misconstrues the term elsewhere), Coclico makes clear a few pages later:
Music has not been placed outside the number of liberal arts, for it is taught in the same way as either Rhetoric or any other art as an art, certainly, by practice and by imitation (ibid., i).

To this he adds that, even without the mnemonic aid of a book, he learned not only the musical elements but how to “form all of his models after his [master’s] example” (ibid., 7).

Coclico’s exposition leaves absolutely no doubt regarding his self-conscious devotion to the paradigm of rhetorical learning applied in the domain of music. He tells us that this form of direct learning by the example of his master (rather than a series of endless scholastic rules and irrelevant mathematical precepts) was how he himself was initiated into the Art of music. Naturally, he recommends that from now on this is how young people can best accomplish their practical learning.

But he does not stop there. In the process of fulfilling the dialectic requirement of providing a definition of music together with its divisions, he offers his unvarnished opinion and uncompromising value judgment on what he thinks is the worth of the four types of musicians he enumerates:

1

The first type are men like Boethius and Guido d’Arezzo, who discover useful things about music; but these, we are told, are “only theorists.” Rather surprisingly in this category are also included the Netherlanders Ockeghem, Jacob Obrecht, and Alexander Agricola, all of whom are known to us today exclusively for their compositional output. As far as we know, these men did not produce theoretical writings.

A clue to the meaning of Coclico’s puzzling classification is given by Leeman Perkins’s article “Ockeghem” in the Grove Music Online. There he writes:

As Ockeghem’s music disappeared from the practical sources in daily use, knowledge of his existence, and of his extraordinary contrapuntal skills, came to be transmitted solely by the theorists of the 16th century [my emphasis]. Writers from Aaron to Zacconi, and in particular German schoolmasters such as Heyden, Ornithoparcus and Wilflingseder, commented on the exceptional achievements of the Missa cuiusvis toni, the Missa prolationum and the canonic chanson, Prenez sur moi.

By this account, Coclico must have encountered Ockeghem only on the pages of sixteenth-century treatises and simply mistook him for a theorist. Yet, the accidents of Ockeghem’s transmission does not completely explain the fate of his musical output. The music of Binchois, DuFay, Busnoys, and Josquin—all men with whom he served side by side in the French court—was in fact judged very differently by Coclico, though these composers are also mentioned in the very same treatises. For example, Heyden (1969
[1540]), exhibiting the same respect and admiration for each, uses examples by all of the following, listed here alphabetically: Alexander Agricola, Antoine Brumel, Johannes Ghiselin, Heinrich Isaac, Josquin des Prez, Jacob Obrecht, Johannes Ockeghem, Marbriano de Orto, Nicolaus (?) Piltz, Pierre de la Rue, Ludwig Senfl, Gaspaar van Weerbecke, in addition to some anonymous sources.

We may then be justified in our conjecture that there was something about Ockeghem’s compositional style as such, that Coclico did not value. He may have considered Ockeghem’s formal layouts and cantus-firmus procedures as merely embodying “cut and dried” musical principles, which only entitled him to the status of a “theorist.” In fact, this sort of reception of Ockeghem as a stilted composer, whose music merely exemplifies theoretical principles, became apparent in the ensuing centuries. Whatever the real reason, Perkins also informs us that Ockeghem’s music was in fact

... viewed rather negatively by 18th-century scholars such as Charles Burney and Nicolaus Forkel. Although they appreciated Ockeghem’s contrapuntal genius, they were clearly put off by what seemed to be an excessive emphasis on contrapuntal artifice; Burney opined that “learning and labour seem to have preceded taste and invention” and Forkel characterized Prenez sur moi as “unsingable” (Perkins, “Ockeghem,” Grove Music Online).

Similar considerations to those of Okeghem were possibly at play in the cases of Obrecht and Alexander Agricola causing Coclico to classify all three under the same “lesser-musical” category of “theorist.”

Whatever his opinion of Ockeghem and company, Coclico’s opinion of the next category of musician is all too explicit. To this second class belong those who are in fact “mathematicians”! Though widely known as composers and frequently honored for their compositions by many generations, in Coclico’s view these men do not pursue the goal of music:

They do not honor the smoothness and sweetness of the song... In teaching precepts and speculation they have specialized excessively and, in accumulating a multitude of symbols and other things, they have introduced many difficulties. Disputing much a long time, they never arrived at the true rationality of singing [namely, its logos or ratio]. Of these are Johannes Ghiselin, Johannes Tinctoris, Franchinus Gafrius, Dufay, Busnois, Buchoi, Caron and many others (Coclico 1954 [1552]).

Coclico’s description of these composing “mathematicians” (whom he criticizes in the same language humanists were typically using against the scholastics) is tantalizingly close to the modern disparaging ascription of “academic composer.” We cannot possibly ignore the bitter humor in the circularity, not to mention futility, of the human epistemic condition. The
same dichotomies persist and are periodically resuscitated, as old arguments keep reincarnating amongst us dressed in new garb.

iii

The third type of musician is what Coclico considers the “most outstanding” and “almost as kings of the others.” These men do not specialize in teaching, but rather join together theory and practice into the creation of admirable melodies, elegant embellishments, and the expression of all kinds of emotions through them. These “skilled musicians and artful symphonists” are men like his teacher Josquin des Prés, Pierre de la Rue, Brumel, Heinrich Isaac, Ludwig Senfl, Adrian Willaert, Le Brun, Consilium, Morales, Laffage, Lheritier, Nicolas Combert, Crecquillon, Champion and Jacquet, Pipelare, Nicolas Payen, Courtois, Maistre Jan, Lupi, Lupus, Clemens non Papa, Petrus Massenus, Jacob Buus, and innumerable others (ibid., 8). Finally, we have a sizeable list of “true” composers which Coclico admired for the expressivity of their melodies and their polyphonic47 skill.

iv

The fourth and last type of musician, are creative poets “who have originated from training with the third type of musician.” These are renown singers, who “not only know the precepts of the art, but they also compose well and extemporaneously add their own counterpoint to any chant melody. They also employ all the precepts and all their skill in singing for this, so that they sing smoothly, ornately and artfully for the delight of men.” These singers who “have pursued the true goal of the art” are held up by Coclico as paragons worthy of imitation by young aspiring musicians: “the Belgians, Picards and French, for whom this ability is almost a natural one…” (ibid., 9).

Coclico’s exaltation of the third and fourth type of musician reads like a page ripped out of Quintilian’s or Aristotle’s descriptions of what molds a good orator: knowledge of the precepts of the art resulting in the appearance of an effortless and “natural” grace. Coclico tells us that arid mathematic or mathematic-like “rules” may guarantee the perfection of theoretical knowledge, but do very little for music. Instead, knowledge of the precepts of a linguistically construed Art of music is what is required. Here then a distinction is clearly drawn between two kinds of guiding principles or “methods”: on one hand we have “mathematical rules” and on the other “linguistic precepts.” The first, valuable though they are in certain other domains, do not serve the (aesthetic) goals of music. Evidently for Coclico, music is a form of language and its rules are of a different nature than those of mathematics.

47 Coclico’s term “counterpoint” refers to the sul libro improvisations of the singers, better known as sortisatio. When he intends to refer to composers of written polyphonic compositions of the sort we today call “counterpoint,” he employs the expression “symphonists.” The term “symphonia,” which corresponds roughly to musica figuralis and belongs to the genus musica harmonica, must have been widespread for it is also found in Beurhaus 1961 [1580].
This musico-linguistic category of precepts is an epistemologically significant category, whose almost casual appearance in Coclico’s text leads us to believe that by the mid sixteenth century it must have been widespread and was not particularly controversial.

C

It is worth noting that the association of the domain of music with the linguistic Arts of the Trivium, though typically associated with the Lutheran north, in fact predates Lutheranism and was already evident at the dawn of the sixteenth-century in Bavaria—a region that never veered from the traditions of its Catholic faith. Celebrated humanist Johannes Turmair, known as Aventinus (1477–1534), published his *Musicae Rudimenta* (1516) just after his *Rudimentæ Gramaticæ* of the same year. They were both for the instruction of the two younger dukes of his native Bavaria. Under the duke’s patronage the rudiments of grammar were promptly adopted as the official text for the University of Ingolstadt. On the heels of that success, his rudiments of music were commissioned in imitation of the first textbook, and they were purposefully designed to “resemble” it in every way (Figure 5). Michael Bernhard in the commentary to his 1980 translation of Aventinus’ musical text singles out the ubiquitous use of brackets and conspicuous presentation of all the key terms as the constant that dominates both textbooks. Conceivably inspired by the work of his older contemporary Rhineland dialectician Rudolph Agricola, these devices were adopted in order to promote ease of learning and prompt memorization.48

As we would expect, given the early date of its composition, the book seems at first glance to be taking its leave from the musica theoretica interest in the mathematical proportions of intervals. But things are not always as they seem. Aventinus chose to focus his attention on an actual physical monochord, rather than the disembodied mathematical discussions of older treatises. He instructs us where to place the bridge, and exactly how to subdivide the string in order to produce the sound of the various intervals—presumably all for the sake of practical and very audible vocal instruction. He then proceeds to other staples of practical instruction, such as solmization and the church modes.

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48 For a detailed discussion of various methods of dialectic divisions in Renaissance music treatises see Kotzakidou Pace 2006, especially chapters II and III.
VIII.

Aus Ioannis Aventini (Turnmair von Abensperg)
Grammatica omnium vitilissima et breuissima
vom Jahre 1512 1).

[St. alj*:] De Nomine.

Octo sunt partes orationis/ Es sind
achterlap wörtter die man in aller
red pracht

Dictio ein wort 2)
Ilia dictio est nomen cui in nostra lingua potest addi ein vt homo
ein mensch. Equus. ein pferdt.

Nomini accidunt sex

<table>
<thead>
<tr>
<th>Qualitas</th>
<th>Comparatio</th>
</tr>
</thead>
<tbody>
<tr>
<td>Genus</td>
<td>Numerus</td>
</tr>
<tr>
<td>Figura</td>
<td>Casus</td>
</tr>
</tbody>
</table>

1) Röhere über die Grammatica omnium vitilissima und über Aventini Rudimenta
grammaticae stehen in der Lit. Notizc! Dort auch über die Grammatica nova fun-
damentalis, deren Text in der vor mir eingeglichenen 1. Ausgabe mit dem vorliegenden der
Grammatica omn. vitilissima übereinstimmt, abgelesen von der Begründung der Klammern,
von kleinen Dialekt-Verschiedenheiten und von der allgemeine Ausgabe interessierenden
Druckfehler.

2) Die Rudimenta grammaticae (von mir benutzt in der Edition princeps: der Augs-
burger Ausgabe vom 8. Januar 1517, sowie in der Ingolstädter von 1542) lassen diese
Worte weg, belegen aber schon durch zahlreiche Ausfälle, daß sie erweiternde Ausarbeiten,
Aventini „große Grammatik“, find. So lagen die Rudim. von 1517 (St. Blatt*) bei:
Müller, Quellenschriften.

Figure 5. Aventinus’ Rudimentae Grammaticae (1512). Source: Johannes Müller’s
Quellenschriften (1882), which evidently respects the layout of the original.

Aventinus’ own list of the “three kinds of musicians” (Figure 6) decidedly
moves away from the “canonici,” namely the mathematicians who followed
Pythagoras in all matters musical. By taking music away from the realm of
mathematics and making it a matter to be judged by human ears, Aventinus
opens the door wide for a younger generation of theorists like Cociico to
usher in an interest in the “psycho-logical” matters of music. Aventinus’
music is a matter of human psychology rather than a matter of the mathe-
matical perfection that reflects the heavenly spheres. As before, the Latin
text given here is from a modern transcription that preserves his characteristic brackets:

<table>
<thead>
<tr>
<th>Musici</th>
<th>Triplices</th>
</tr>
</thead>
<tbody>
<tr>
<td>Aristoxeni sectatores/ qui &amp; harmoniaci duntaxat aurium iudicium sequuntur in examinando cantu</td>
<td>Pythagorici repudiato penitus sensuum iudicio/ vt quorum iudicium fallax sit ac incertum omnia ad rationem numerorum reuocant hi &amp; canonici.</td>
</tr>
<tr>
<td>Ptolemy mediam elegit viam/ haec coniunct vtriusque &amp; senus &amp; numerorum rationem habuit</td>
<td></td>
</tr>
</tbody>
</table>

Figure 6. Aventinus’ *Musicae Rudimenta* (1980 [1516]) list of “three kinds of musicians.”

In English translation the text reads:

There are three kinds of musicians:

The followers of Aristoxenus (who are also known as the “harmonici”) only follow the judgment of the ears in examining music.

The Pythagoreans, rejecting entirely the judgment of the senses as being false and untrustworthy, reduce everything to mathematics; this group is called the “canonici.”

Ptolemy took the middle of the road, combining the opinions of both, and taking into account both senses and numbers (Aventinus 1980 [1516], in Keahey’s 1971 translation, 6).

Aventinus later rejects both the enharmonic and chromatic genera in favor of the diatonic—a decision he justifies on the “practical” grounds that it is easier to sing and sounds better (*ibid.*, 11). And when the time comes to decide which among the various divisions of a tone are to be adopted, he dutifully presents the opinions of both the “canonici” mathematicians and the followers of Aristoxenus (who, he reminds us, “spurn the explanation by numbers” in favor of the judgment of the senses). Nonetheless, he concludes with an exhortation that leaves no doubt as to whose side he is on: “The careful reader will easily perceive [by ear] all these things on the monochord if he gives it attention and consideration” (*ibid.*, 16).

Having now acknowledged Aventinus’ transitory epistemic shift away from mathematical thinking towards a human-centered “psycho-logical” judgment, we can better appreciate the completeness of music’s transition into the Trivium and grasp the full extent of the “technical” or “artistic” commitment exhibited by Coclico only a few decades later in the Lutheran north.

The reasoned precepts of the Art of Dialectic (as taught by a variety of scholarly traditions) continued to determine the logical organization of treatises and elementary instruction manuals in *musica practica* throughout the sixteenth century. However, a new level of conceptual closeness between
Music and the Arts of Logos was undoubtedly reached with the inauguration of the German tradition of *musica poetica* (or *poëtica*), which brought about a lasting appreciation of the musical import of the Art of Rhetoric. Already evident in the Heinrich Faber’s (1500–1552) manuscript on *musica poetica* entitled *De Fingendis Musicis* (ca. 1548), and most notably Gallus Dressler’s (1533–1580/89) *Praecepta musicae poeticae* (1563), it reached its first great milestone in the work of Joachim Burmeister’s (1564–1629) *Musica Poetica* (1955 [1606]). I undertake the exploration of this enormously interesting topic elsewhere.  

6. Postscript: What Kind of “Theory” is Music Theory?  

A  

The example of Grammar’s ascent to the status of *techne* holds some lessons for Music Theory in its modern struggle for intellectual respectability. Under the intellectual leadership of figures like Milton Babbitt, Allen Forte, and David Lewin, during the second half of the twentieth century Music Theory in America fought to escape the “empirical” domain of the conservatory and enter the hallowed halls of the presumed to be “theoretical” Academe. Evidently the exact nature of the move was such a foregone conclusion that nobody was willing to play Devil’s advocate and raise the obvious question: “Under what conditions?” Or more to the point: “What is the price of admission?” With the benefit of hindsight plus a heightened philosophical and historical awareness, it now appears that in the heat of the battle for modernism, a sizeable portion of the field took a wrong epistemological turn towards scientism.

To get a feeling for the power that such an outlook has wielded in America, the reader may consider the following anecdotal evidence. In the mid 1960s, when the prevailing epistemic climate of positivism was undergoing one of its cyclical paroxysms, the Juilliard School50 started issuing “Master of Science” degrees to its music performance majors. An opera singer who had been awarded this degree explained to me that the official justification for this dubious practice was the inclusion in the traditional conservatory curriculum of a single course in “Acoustics.” While the exact content of the course remains shrouded in mystery, we do know that electronic experimentation was by then ubiquitous in art-music circles in New York City and neighboring Princeton. This points to the possibility that the well-meaning intention behind this short lived “scientific” sounding pseudo-degree was the

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50 The Juilliard School is an American bastion of professional musical education in the tradition of the great European conservatories.
What Kind of Theory Is Music Theory?

Based on the historical and epistemological genealogy presented in this study, the “theory” in Music Theory opposes the craft (empeiria—experience) of the conservatory-like training in Music Performance. It is, therefore, neither a member of the “theoretical” deductive mathematical sciences, nor a “theoretical” science akin to the natural sciences entrusted with the discovery of the eternal laws governing the immutable cosmos.

This distinction between “theoretical” Art and “practical” craft duplicates at this level of the epistemic hierarchy what we have already seen happening one level higher. There Aristotle had used the same “theoretical” versus “practical” differentia to separate “theoretical” science from “practical” Art. As the reader will recall, science pertains to deterministically existing objects or natural events that could not be otherwise, as their efficient cause lies in themselves. They are the eternal manifestation of an immutable Nature, whose Laws are not the sort of topic that, once discovered and properly demonstrated, is subject to dialectic deliberation or ethical and aesthetic choice. Ultimately, it is not the sort of thing about which reasonable people can disagree. This kind of science was sharply contrasted with Art which, whether poietic or action (praxis) oriented, is the sort of knowledge that creates objects and guides acts whose efficient cause is the will and imagination.

Setting aside the dismal state of elementary “theory” courses offered in many lofty university settings today, this lopsided view (which I obviously do not share) does not take into account the specialized training conservatory students receive in their advanced studies is composition, theory and analysis. After all, the totality of the canon forming the basis of any serious training in the tradition of western art music was composed not by university trained musicologists, but by “practical” musicians. Like Beethoven, Mahler, and Debussy, they were all beneficiaries of the long pedagogical tradition still propagated by elite professional conservatories around the world. It follows, therefore, that what is really at stake is not some alleged inadequacy of a conservatory-based musical education, but the epistemic goals of such an education—something that may or may not be universally shared by every student population.

Differentia is a technical term which signifies those characteristic differences that are informative enough to adequately sort out the species of a genus. Subsequently, the enumeration of these differentia is considered the most efficacious way to verbally define each species.

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51 Occasionally this epistemological judgment (unfairly) spills over from the area of performance practice to the kind of music “theory” taught at conservatories. It is certainly a truism that the typical conservatory training in “theory” is epistemologically inferior to the kind of music theory that Babbitt and his contemporaries had in mind. But this is only because such a statement is typically uttered in reference to the elementary instruction which practicing musicians receive as part of their professional training in instrumental or vocal performance: notes, scales, intervals, chords, sight-singing, formulaic four-part harmonic writing, and even some species counterpoint. Any generalization stemming from this fact is surely the result of semantic confusion, as the aforementioned topics are the traditional province of musica practica—nowadays only erroneously called “music theory.”

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of humanity. It gives rise to things and situations that, were it not for the
efforts of an Artist, can be otherwise. As will become even clearer below,
the meaning of the term “practical” in this context is correlated with the
concept of “praxis” (occasionally rendered as “action”), and differs markedly
from the meaning of “practical” when used to describe the epistemological
status of craft.

The pitfall just waiting to happen was, of course, the assignment of the
discipline of Music Theory to an inappropriate epistemic level, and the ensu-
ing misallocation of the term “theoretical” to oppose the wrong kind of
“practical.” Eager to differentiate their brand of mathematically savvy Music Theory from what was hitherto taught in conservatories, Babbitt, Forte,
Lewin (and now a younger generation of “neo-Riemannians”) over-extended
the application of the term “theory” and laid claim to a level of epistemic
certainty that is simply not available to an Art. This amounts to a serious
category-error; namely, the misallocation of the contents of one category to
the conceptual space of another.

C
Due to its paradigmatic standing among the various poietic Arts, for the pur-
pose of this discussion let Rhetoric stand metonymically for all her sisters,
including Music Theory. Rhetoric is defined by Aristotle as a method for
doing systematically what others may have come upon by accident. It in-
volves not only a meticulous gathering of successful approaches, but also a
thoughtful analysis of what it was about them that made them successful—so
that the desirable effect can be duplicated on demand. It relies on a charac-
teristic mode of learning that employs the study of paragons and eschews
strict rule-based systems.

Aristotle and modern neo-Aristotelians (Wittgenstein and Ryle, among
them) draw a distinction between the epistemic domains of theory and praxis (action). The corresponding modes of knowing are “knowing-that” for phil-
osophy and science, versus “knowing-how” for the various Arts (medicine,
engineering, jurisprudence, and of course, the most important for the func-
tioning of a democratic state, rhetoric). Rhetoric (understood here as a path
to effective communication, whose modes of expression can rely on lan-
guage, music, painting, cinema, and a myriad of other media) clearly belongs
in the domain of praxis.

Nonetheless, its special epistemic status does not preclude Rhetoric from
availing itself of results obtained through the scientific or other methods of
inquiry. As an alternative to Babbitt’s (1972) musical positivism and

53 The mathematical techniques introduced at that time were adaptations from Linear Algebra
and Set Theory and were intended to facilitate the analysis of various early twentieth-century
post-tonal styles. This was accompanied by an equally enthusiastic adherence to the tenets of
American Schenkerianism. Both of these topics they were eventually hired to teach in univer-
sity settings.
Lewin’s (1987) efforts to conjure up an axiomatic mathematical image for Music Theory, our conception of music-as-Art accomplishes an appropriate level of systematicity and control over its materials without aspiring to extraneous epistemological domains. For example, Babbitt’s “common tone theorem” is welcome knowledge, and so are the latest scientific discoveries produced by the scientific fields of acoustics or neurobiology. But neither mathematics nor any natural science can structure the domain of music, any more than jurisprudence should be reclassified as a natural science just because DNA tests are used in legal proceedings.

This is because Music does not simply reproduce what is given in nature. Rather, it avails itself of the properties of sound and the ability of the human mind to cognitively manipulate and construe objects of thought, all for the purpose of producing something artful, something that goes beyond (and sometimes against) nature in the service of historically fluid aesthetic goals.

D

An interesting case of bringing scientific evidence to bear on aesthetic issues in performance practice is undoubtedly David Epstein’s 1995 book *Shaping Time: Music, Brain, and Performance*. The less than enthusiastic critical reaction to this voluminous study seems to pivot on the premise that the (aesthetic) superiority of certain large-scale rhythmic organizations is dictated by the outcome of scientific investigations into people’s “natural” abilities and inclinations. To his critics Epstein seems to be arguing that since scientific data shows that people naturally tend to organize their rhythmic behavior into certain simple patterns, these same metric proportions should govern the aesthetic realm of music performance for “best results” to be attained.

What is missing from this discourse is the realization that Epstein’s neuro-scientific approach to performance practice may indeed be valid in the case of Beethoven and his contemporaries, for whom “naturalness” in their Art was an aesthetic goal of the highest order. But, this being the domain of Art (also a cognate of artificial and even artifice), aesthetic goals change as quickly as social and material circumstances. In the blink of an eye, composers may start seeking “artifice” as an aesthetic desideratum, just as easily as they have sought “naturalness.” Thus, even if we were to accept that neuroscience has in fact discovered the “definitive” criteria for naturalness in the domain of human rhythmic behavior, with equal certainty we can also forecast the rise of a “protest” art movement, whose music is constructed in violation of these strictures for the sake of creating a desirable artistic effect.

E

In its quest to understand the musical mind, research in music psychology has largely adopted the cognitive paradigm originally launched by psycholinguistics nearly fifty years ago during the heady days of the Chomskian revolution against behaviorism. Though it has crucially relied on the concep-
tion of music-as-language, this research program has reached an overwhelm-
ing, but oft unacknowledged consensus: with respect to any substantive mu-
sical task studied experimentally so far, a clear division of the population
into groups of “novices” and “experts” is consistently revealed in the struc-
ture of the data (Krumhansl 1990).

This is a stunning outcome because, according to the standard Chomskian
conception, language-qua-language (to be distinguished from language-qua-
art, as in poetry) is not a skill per se, and it is not subject to the novice/expert
distinction. Indeed in modern times, linguistics has focused on native lan-
guage ability, assumed to be homogeneously perfected among the speakers
of any given community. Its object of study is the “universal” language fac-
ulty that purportedly underlies each and every speaker’s basic command of
his or her native tongue. Linguistics has not shown much interest in the con-
tingent and multifaceted language skills necessary to produce “artificial”
forms of diction (such as literature or technical language)—something that
the members of the general population exercise to varying degrees.

It is undeniable that the human animal is “equipped” for music, just as it
cannot be denied that we are “born” to produce language and to walk up-
right. But to walk is natural and innate. To walk on a tightrope is a skill that
takes years to perfect, and it is therefore considered an Art. Metaphorically
speaking, Music Theory focuses on what it takes to walk on a tightrope in
aesthetically interesting ways. Rather than a monolithic, possibly innate,
generic “one-size-fits-all” musical competence, the epistemic domain of
Music Theory, like all other technai, is best understood as a varied collection
of acquired cognitive skills that capitalize on natural endowments, but go
well beyond them.

F

There is no doubt that institutional and individual professional interests have
been served by a scientistic construal of the domain of music—whether as
mathematics or as a natural science. But considering that Academe is the
birthright and natural abode of rhetoric, medicine, jurisprudence, engineering
and other such “practical” Arts, it follows that, just like them, the techne of
Music Theory can successfully coexist with the axiomatically organized
field of mathematics, the deterministic natural sciences, and even the rarified
disciplines of philosophy and theology. It is also true that none of these Arts
or sciences was ever forced to sacrifice its special methods or to be dis-
lodged from its individual epistemic sphere for the sake of academic cohabi-
tation with “loftier” subjects. Music Theory should be able to do the same.
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What Kind of Theory Is Music Theory?


Countless Western Art Music Recordings
Towards a Theory of What to Do With Them

Jonathan Dunsby

To call musical recordings “countless” may not be literally accurate, but it expresses the overwhelming abundance of the legacy of recorded sound, even in the limited field of Western Art Music. Of course, one of the seductions of theorizing recorded music is that seemingly endless, musically concrete illustrations are to hand, all of them offering to WAMistas the solace of music as an escape from words and concepts. Nevertheless, here there will be discussion rather than illustration or detailed case-studies, in a conceptual environment free of empirical detail—free of issues about transfer fidelity, click-ware accuracy, covert splicing, and the many other admittedly fascinating variables involved in hands-on analysis of musical recordings. Initially, and rather informally, we will explore the relationships between philosophy, psychology, and sociology in the context of the general question: what are we to make of the legacy of more than a century of WAM recordings, and indeed of any of them individually. That leads to consideration, rather precisely I hope, of how a theory of recording might be grounded, despite the seemingly random course so far of our development of an idea of what to make of recordings: a consideration, even, of how a theory of recording ought to be grounded.

It is instructive for music theorists to contemplate some of the wonderings and assertings of philosophers of the mind. Such philosophers constitute a breed that has existed, it seems, since the dawn of what is recognized as philosophy, and a breed that was particularly conspicuous around the middle of the last century. Philosophy of the mind was conspicuous not least because of the confidence and apparent relevance to everyday life with which it encountered psychology, a different kind of study, often a would-be “science” of a kind.

I am grateful in particular to the Korean Society for Music Theory for offering me the opportunity to air and discuss some of the ideas and specifics of this research on a visit to Seoul in May 2006, and also to the Birmingham Conservatoire and University College Dublin. I owe a significant debt to John Rink, who does not share all of my views, for his penetrating advice on this work in its nearly final form, all faults in which are my own responsibility.
The thing about psychology, I take it, and to the extent that one cannot but generalize about the history of ideas, is that people think they understand it and why it matters. Certainly they did in the middle of the last century, notably during the decades of recovery after the Second World War. Psychology at its simplest was concerned with how people think, how people behave, how people think they behave, and so on, and it was not concerned with how philosophers of the mind think, behave, or think they behave. Equally obviously, philosophers, to ponder them more generally for a moment, have often—or it may even be said that they have typically—studied “people,” although usually these are people facing death, gods, pain, moral turpitude, or perhaps more abstractly people facing phenomena, signification and the like. Even that most humanitarian of more modern, general-philosophy luminaries, Jean-Paul Sartre, expects us to take all simple actions on behalf of the whole of humanity, which is not going to be achieved in a moment of casual reflection. Contrast the impact of the enthusiasms of much twentieth-century psychology, Gauguinesque in the compulsive gaudiness of their outlines. Everyone can understand and feel the experience of “unity” of perception for instance: we all know we can normally remember 7±2 roughly similar entities:1 we all know that when we make a slip of the tongue it probably is anything but a fluke;2 and anyone can add, accurately, to these enthusiasms for what still drives our conceptual frames of reference, and not just still drives them but in some areas is life-shapingly embedded in them—think for instance of the grip that psychometric so-called “testing” has on employment practices.

It does not matter precisely how wrong or right the interaction is, as portrayed here, between philosophy and psychology. The fact itself is what counts. The period that is the direct intellectual inheritance of current generations was one when questions about ordinary behavior seemed as urgent as questions about the meaning of meaning, although maybe not to most philosophers of the mind; and maybe that period endures around us in the new century, a speculation that I happily leave to contemporary philosophers and psychologists.

Those observations provide a background in the history of ideas for up-to-date thinking by music professionals, as found, for one good example, in Susan McClary writing about Richard Taruskin, two modern musicologists of high repute, which includes their well-known originality of perspective. McClary informs us, in her overview of Taruskin’s majestic narrative The Oxford History of Western Music, that he rightly claims this: “no historical event or change can be meaningfully asserted unless its agents can be speci-

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1 This phenomenon was reported definitively by George Miller (1956).
2 For one of a myriad explanations of ideas about unconscious control, beginning with Freud’s identification of “parapraxis” (the famous “Freudian slip”), see Fuery 1995.
fied; and *agents can only be people*” (McClary 2006, 409, emphasis original). She comments as follows:

This position (sometimes disparaged as “sociology” by those who apparently have no idea what sociologists—or historians, for that matter—actually do) commits Taruskin to paying constant attention to the particular context within which various practices arise, develop, and fall into obsolescence—always according to the preferences or antagonisms of the human subjects who created, listened to, disparaged, or paid for the music. … As his method shows over and over again, analysts have impoverished their enterprise by cutting themselves off from the very data they need in order to produce plausible explanations (410).

One may rightly ask whether it is worth unpicking such a quotation from an informative, lively, generous piece of writing about writings, but the interest and the authority of both the plaintiff Taruskin and his counsel McClary outweigh any potential lack of decorum. After all, there are a number of propositions here that can provoke justified response, and the most curious proposition is that there is information (or, there are data) needed by music analysts, but analysts have cut themselves off from it, while, oddly, it was the very information that analysts would have needed in order to produce plausibility.

Agreeing or disagreeing, one may wonder whether an example of such essential information or data is that to be gained from the analysis of recorded music. The requirements of McClary and Taruskin seem to be well met. Recorded music does appear to have the plausibility of being produced by *agents* if it was originally produced in real time—possibly in lots of little bits of real time—by actual people. If you are analyzing the recording of the music, rather than the music itself in some more virtual form, then presumably you are paying constant attention, by definition, to the music in a context. What is more, to return to our opening reflections, you will be asking questions about ordinary behavior. Musicologists will be making points about an object that is there for you yourself to hear (X’s recorded performance of Y). You will know about speed from everyday experience, so the basic idea of tempo is covered. With a little experience of physical motion you will be entirely familiar with acceleration and deceleration. You know about soft and loud, clearly. Even if not understanding much at all about Western music notation, you will certainly have an idea of fidelity, brilliantly discussed in Jean-Jacques Nattiez’s recent study of musical time.3 If all that is to labor the point, the reason is to articulate the kinship between psychology, as outlined briefly above (and as it is generally understood), and the fashion for studying recorded musical performances. This position is

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3 “Fidelity” might well be thought of as the substrate of authenticity and of what Taruskin famously called “authenticism”: see Jean-Jacques Nattiez 2004.
strongly confirmed by the comprehensive statement of Simon Frith, coming from the perspective of popular music, that “the musical experience has been individualized,” which he goes on to gloss as follows:

Music is no longer a necessarily social or collective affair materially (though it may be in the imagination). … we can now possess music as obsessively, as madly, as music once possessed us. … This means … a new dialectic in music: absence of performer/presence of performer (absence of audience/presence of audience) … (1998, 237).

Point number 1 which needs to stand out in this argument is that studying WAM recordings is really about psychology, not about sociology, or it needs to be about psychology if Frith’s diagnosis of the contemporary transcendence of the individual over the social is valid and paradigmatic. McClary is probably justified in supposing that some people do not know what sociologists actually do, but it is also possible to sense that she is seeking to protect the wrong species. Her accusation is frankly counterintuitive. At least as far as music theorists are concerned, it may well be that sociologists are not the problem she thinks they are—of which more below. To see music theory as a sub-discipline of sociology should hardly raise any hackles. It is not the study of agency in itself that bothers theorists, and Taruskin’s own partit pris is no threat to the core values of those researching the actual characteristics of WAM, but a welcome, broad-spectrum addition to the music theory perspective—history-telling that could never have been achieved without stepping well beyond the concerns of music theory specialists, and this is exactly what big science is telling us, that knowledge is all about collaboration. What may legitimately trouble the music theorist, though, is how weak in this modern history of ideas the idea of agency really is. We have on the one hand sociology that (pace McClary) everybody knows is about populations or at the least about groups. We have on the other hand psychology that is about, say, the individual response, and in the wrong hands can let loose all the distortions that emanate from failing to distinguish between the individual response within a controlled environment (pace Heisenberg) and the individual response as supposed raw data. Cutting across this divide between the general and the particular, neither sociology nor psychology has seen fit to take onboard music as a fundamental human behavior, and if mu-

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4 From its earliest years twentieth-century psychology was of course always hunting for commonalities and principles, including music psychology, which, as Eric Clarke notes, only (my inflection) “in recent years … has aimed at specifying the psychological principles that govern expressive performance in music” (2002, 64, my emphasis). With a much wider purview the mind theorist and clinician Gerald Edelman argued that psychology had “only lately come to grips with the key issues of biology itself” (1992, 41). His general discussion of that topic suggests that he believed psychology, well into the second half of the last century, had not come of age scientifically, despite the best efforts of psychologists at inference from behavior.
sic is indeed a fundamental human behavior, as is coming to be thought in
important anthropological circles represented for instance in recent research
by Steven Mithen (2005), then the big disciplines in the human sciences are
in for a serious re-think. McClary seems to want us to believe, from what she
says, that sociology is some kind of given. Yet a different view is that soci-
ology is inherently problematic to music theorists as a discipline for very
good reason, if it is true that sociology has been unable to incorporate human
music into its hypotheses about human behavior.6

As a corollary to point number 1 that studying WAM recordings is really
about psychology, it needs to be observed, without giving chapter and verse
which would require a major literature review in itself, that there is a mas-
sive psychologistic hubris at large in performance studies. Readers of jour-
nals and books in this area either will know what is meant by that comment,
or are invited to follow up one example mentioned here, by Mark Tanner
(2000). The example is selected not to interrogate a writer whose intentions
and integrity are no doubt honorable, but because it is emblematic of en-
demic psychologistic hubris. As a starting point, imagine what kind of theory
can support the following description of “intensity” as offered to subjects of
a listening experiment: “the combined performance elements, principally
tempo, dynamic, articulation, and pedaling, which may increase or decrease
in response to the performer’s concept of the music’s ebb and flow” (187).
One can only wonder how many listeners, even “musically literate” listeners
(186), can rate on a scale of 0 to 10 (188) the “ebb and flow” in the “per-
former’s concept” in respect of “pedaling” (for example). If you were one of
Tanner’s subjects, apparently you had to listen out accurately for the pedal
technique, which for anyone is an extraordinarily challenging task; and relate
that to the performer’s concept as evidenced in a recording, always assuming
you can somehow identify some “concept” from listening to music; then
assess it in relation to something called “ebb and flow.” Apparently, then,
you would have needed to be highly expert, deeply oracular and fundamen-
tally empathetic all at once. In reporting on an extract from the Liszt B-
minor sonata, no fewer than one quarter of (“some” 65) subjects disagreed
with the majority about where the “intensity” peaked in an expressively neu-
tral MIDI version of the music, which leads Tanner to say that something
“remarkable emerges from these results” (188). Actually, the results may
tend to show, if anything at all, pretty much what a music theorist studying
the score would expect, in that there are certainly different, credible ways of

5 Not all experts warm to the line of specula-
tion in The Singing Neanderthals: The Origins of
Music, Language, Mind and Body as is abundantly clear from the discussion, in fact a series
of confrontations, in “Review Feature” (Mithen et al., 2006).
6 The human sciences in general have been slow to embrace the evolutionary role of music in
human behavior. For a modern conspectus on this issue, see Nicholas Bannan (forthcoming),
“Language out of Music: The Four Dimensions of Vocal Learning.”
What Kind of Theory Is Music Theory?

hearing this passage of music. What comes over in this research, published in a prestigious journal, is the extent of belief in psychologistic validation, which is why the word hubris has been used here.

Looking at the current state of music theory, I defer to nobody in acknowledging the importance that needs to be ascribed to electronic recording. Indeed, I wrote the following, rather journalistically perhaps, in 1995, a year worth bearing in mind if only in that it preceded the wholesale spread of the Internet throughout the affluent world:

It is interesting to ask whether technology, in its sound recording, then vision and sound, holography, virtual reality, and who knows what may come next, is generating a fundamental shift … We may be witnesses, the only direct witnesses there will ever be, to the beginning of the music of the future. Is it not easy to imagine that two thousand years or five thousand from now people will say that Western music really only got going properly during the twentieth century from which distant time there date the earliest proper sonic and visual records, following that strange “mute” early period of music history that spanned the Greeks … to, say, Mahler? (1996, 15–6).

There are tendencies in the recent flourishing of the history and analysis of recorded music to recycle debates of much longer pedigree than the computer or even the first phonographs. In a context where Nicholas Cook (2001), a leading exponent and promoter of studying WAM recordings, is typically enthusiastic and informative about what he calls playfully “music and/as performance,” he is also typically frank about the downsides of that kind of enquiry:

You can work with large numbers of recordings … but an essentially inductive approach of this kind does not easily provide the kind of insight into the specific qualities of specific interpretations that score-based analysis characteristically offers. (It suffers, in short, from the traditional problems of style analysis.) ([22])

The study of the human body in performance is also fraught with problems, he finds, as in fact a “site of resistance to text”—an opportunity missed there for Cook to use rather than “text” his favored term “script” through which he seems to have caught the attention of some other recent writers: it is hardly a secret that the analogy between “script” and “score” or “music,” perpetually intriguing, has been recycled periodically for centuries; it made a strong music-theory showing some two decades ago when David Lewin (1986) wrote tellingly on the theme of, as it were, song is to text as actor is to script.

Tanner is commendably careful to note the reasons why some listeners may have selected an alternative “expressive goal,” although ironically he offers not psychological hubris, but music-analytical “data” to explain it: “presumably because of the thickly textured development from m. 363 and the sense of harmonic resolution following the short cadenza at m. 375” (189).
Even that idea of music as a script, however, appears to frustrate Cook—a state to which he himself refers—at least when he is discussing a dialogic approach in theatre and literary studies; and so do current trends in ethnomusicology. Research in these areas is, he indicates, seemingly always dogged by “its lack of engagement with the specifics of music.”

Cook’s valiant, programmatic attempt to rescue the situation by reference to considerations of “social interaction” may hold out the promise of what he calls a “culturally-oriented musicology,” if it can indeed be shown as he wants that musical works are “scripts in response to which social relationships are enacted,” and that consequently “the object of analysis is now present and self-evident in the interactions between performers, and in the acoustic trace that they leave” (2001, [22]–[31]). Nevertheless, let us review some actual music-analytical benefits to interpreters that might be culled from a theory-oriented book, *Making Words Sing* (Dunsby 2005). These benefits would probably include at least the following sample (*Case 1, Comment*, etc.), and in each case there is a music-recording angle worth considering to provide some perspective on the music-theory premises:

**Case 1**
In the Schubert Lied “Erster Verlust” (First Loss), a new understanding is proposed of the difference between the three time-dimensions of the text and the two-dimensional tonal space of the musical composition, complementing the way that the piano after the voice provides musico-semantic completion to the musical structure, though the two come together in that third entity called “vocality.”

**Comment:** In this case, issues of interpretation have been discussed by Lawrence Kramer, but arguably on false grounds in that he fails to take into account contemporaneous performance practice. The grounds for Kramer’s interpretation are also suspect because of the simple and astonishing fact that he builds his argument about personification in this song on the assumption that the singer is male, for which there is zero evidence, and even though in his closing remarks he admits that the whole argument might need to be turned on its head if the singer were female (1998, 24). For once, statistical information, about, say, how many males as opposed to females have made commercial recordings of this song, might be germane to worthwhile theoretical enquiry; and it would be interesting, to say the least, to be offered concrete evidence, from authoritative recordings, of perceived gender awareness or even, conceivably, of perceived non-awareness.

**Case 2**
In one of the most famous Brahms Lieder, “Von ewiger Liebe” (Eternal Love), an understanding is proposed of the vocal pacing and shape of this

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8 The standard source on this remains Heinrich Schwab 1965.
proto-cinematic scenario and of the climactic vocal/instrumental interactions of the song, such as a Schenkerian approach reveals decisively.

Comment: This is a case where the kind of retrospective musicology mentioned above would be missing the point entirely. “Whether or not Brahms ‘heard’ in his inner ear a particular singer singing ‘Von ewiger Liebe’ or an ideal singer or the sum or (as it may better be expressed) the essence of his experiences of vocality, we just cannot know” (Dunsby 2005, 142). With respect to the music-analytical findings here, any such specifics emerging from the legacy of recordings of this song would be equally beside the point. Cook’s “interactions between performers, and … the acoustic trace that they leave” could provide only color and perspective, and marginally at that, to “understanding” this music. Those who may object that the description stated in Case 2 above is too prescriptive might argue that studying a broad range of interpretations could suggest that the description is wrong; this would not go to the possibility, however, that all the interpretations in a sample happen to be “wrong.” or indeed, in principle, all known interpretations. See my comments on “instances, rather than practices” below.

Case 3
In Copland’s song “Going to Heaven,” an understanding is proposed of the musical (pitch/rhythm) invariants that the composer re-contextualizes tonally as well as poetically, supporting the sense of continuities and discontinuities in Emily Dickinson’s modernist text.

Comment: Since this take on “Going to Heaven” is very much poetically oriented, issues of interpretation are at some remove. It would be valuable to assess, however, whether we find specifically American qualities in significant recordings, given that the American qualities of Dickinson’s language and imagery do clearly form part of the data that make up this composition, and, by the way, given the clearly American approach, linguistically at least, in Dawn Upshaw’s landmark recording.9

Case 4
In Cathy Berberian’s music-theatre solo Stripsody, an understanding is proposed of the interplay of semantically loose images and vaguely specified vocal sounds that in fact, in performance, provides the simulacrum of a most precise, virtuosic musical form-building.

Comment: This is a striking example of music, in the sense of music theater, using extended vocal techniques through iconic notation, that is barely interpretable with any confidence in some kind of fidelity except by reference to the composer’s own recorded performance.10 Only relatively re-

10 Berberian’s recording has been re-issued on CD, for example in “Magnificathy: The Many Voices of Cathy Berberian,” WER 60054-50, 1993.
cently, in a conspectus on this period, when at the time Pierre Boulez was objecting to musicians being asked to play cartoons, did one historian note how “the deciphering process became more difficult since no practical guide was available for interpreting many of the new notational gestures found in the scores” and, most pertinently, that “only a few singers, such as Jan DeGaetani and Cathy Berberian, contributed fine recorded examples of the music, generally limiting themselves to major works” (Mabry 2002, 2). On the one hand, in other words, the performer has to work especially hard to interpret a score the meaning of which was relatively obscure in the first place, many aspects of that “meaning” being nowadays probably lost for all time. Yet on the other hand, although this case seems to foreground the importance of a contemporaneous recording, merely copying an original interpretation is never likely to lead to a committed, convincing modern interpretation: the singer has no option but to revert to the notated “interplay” and try to figure out a way to bring it to life, as the extremely entertaining, memorable and thought-provoking musical experience that it undoubtedly is or can be.

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What those offerings from analysis to actual interpretations have in common is that even regardless of the glosses applied to them in this context they are in fact already true for performers and even despite performers. They would be true even if in some weirdly abstract world those works had never actually been staged or heard, and even in such an extreme case as *Stripsody*—where after all what would therefore have been lost in terms of authenticity might have been richly compensated for in terms of the necessarily creative re-invention that performers would have no choice but to bring to such an under-determined composition.

Note that no reservations are being registered here about Cook’s vision of a kind of socially anchored, retrospective musicology. What does need to be emphasized, however, and this is point number 2, is that interpretation draws on potential in musical works, a composed potential, largely unforeseen by the composer, that may never have been realized before in the substantive world, potential that is perhaps yet to be released in some future, revelatory interpretation. Cook’s enthusiastic view of an interpretation-based musicology, thus a retrospective musicology, may well prove to have some grip on reality as a broadening of our experience of music. Yet his picture serves to underline the importance of figuring out the underlying questions in the first

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11 For further discussion of this issue, but concerning Berberian’s imprint on Berio’s *Sequenza III*, see Joke Dame 1998 and Janet Halfyard 2006, in particular 106–7.
place, of not giving up on the potential that is waiting to burst out of the actualities of musical compositions, with the help of worthwhile music analysis, in the sense expressed by Boulez.

Again, it would be difficult to argue that Cook’s urge, and the urge characteristic of theorists involved in the history and analysis of recorded music, is really to do with “social interaction,” which is much more apparent in the rightful preoccupations of recording-based research where historical contingencies are often usefully referenced, for example in the pioneering work of Robert Philip (2004) and, a little earlier, Tim Day (2000). What the theoretical urge is to do with is, the reader will hardly need to be reminded, psychology, said earlier to be about “how people think, how people behave, how people think they behave, and so on,” if obviously professional psychologists would likely object to the caricature and point in evidence to the long and complex ingredients-list of their discipline. A sociology of music in performance (or “and/as” performance if you prefer Cook’s formulation) would be about markets, preferences, economic and political contingencies.

Yet recorded performances are probably in a different epistemological category. Is it not in fact in the nature of recorded music to be “bracketed” in this way? Even if we had a film of Mahler conducting one of his symphonies, still it would be a recording. That is undoubtedly what Glenn Gould perceived, with an artistic vision that was at least as “modern” as any other in the twentieth century—modern meaning, in Baudelaire’s sense, genuinely new rather than merely recent. In order to have this vision of a new kind of WAM—created in the studio, assembled from the best possible parts of interpretations, producing a whole that could never be expected to arise in one “live” performance whether public or private—Gould had to be both one of the best practitioners of his age and also one of the deepest thinkers about the philosophy of interpretation. At some point in WAM, there was a category shift brought about by technology and bearing down on the philosophy of interpretation around the time that Gould came of age as a “great” pianist. Signs of this shift were all over the place in the twentieth century. Thinking of the accidents of history, let me repeat one last time that, spectacularly, nobody ever did record Mahler conducting on film despite nearly two decades when it could have been achieved.12 Schoenberg offers another, teasing and not entirely dissimilar case, for not only did he deplore the crude sound of early radio transmission,13 but then he failed to take the technological advantage of perpetuating his interpretations of his own music through relatively “high fidelity” audio tape in the 1940s (even though, incidentally, he

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12 As recently as 2006 I was assured by a leading authority on Mahler that persistent rumors of the existence of one Mahler film clip are in fact unsubstantiated, and insiders are not optimistic that such material will ever come to light. It is well known that various sound recordings of Mahler as conductor and pianist do survive.

13 See his 1933 essay “Modern Music on the Radio” (1975, 151–2), in which Schoenberg also expresses sociological concerns about the subject.
owned one of the first high-fidelity, reel-to-reel tape recorders in Los Angeles.\textsuperscript{14} Stravinsky, in one opposite perspective, did “make” recordings of his extraordinarily successful works, which ought to amount to a wonderful legacy, and yet the extent of his personal input into those recordings as conductor is often doubtful, and the issue of fidelity means that they merit a permanent hazard warning.\textsuperscript{15} Boulez, an acute and on the face of it positive example of this opposite perspective, is represented—as a virtuoso of the podium and one of the most influential composers of his age—by very many hours of “authentic” recorded audio and audio-visual performance, all of it “live” or edited with credible fidelity; and yet it is probably fair to say that nobody and no institution has determined what to make of such a rich resource. In general, what recording practice has done is to catch up randomly, and painfully slowly in some cases seen in retrospect, with cultural values, rather in the way that in music theory notions of pitch structure developed whimsically in the twentieth century, as described by Jonathan Bernard (1997). Even a “classic” essay on music recording, Jose Bowen’s (1993) “The History of Remembered Innovation,” draws on jazz and a sprinkling of philosophical hand-holds rather than setting a substantial, grounded course for future research, and is basically absorbed with notions of canonicity that were being formulating with more appropriate philosophical expertise and conviction around the same time by, in particular, Lydia Goehr. This phenomenon of the random is not restricted, obviously, to matters of modernist art or of theory, but criss-crosses popular taste and imagery just as much. For example, completely by accident, it seems, we have fabulous cine film of Giacometti sculpting while he’s talking, and many publics worldwide recognize the extreme cultural validation of Giacometti’s shapes and sizes, even if most people would not be able to put, say, a name or approximate date to them; yet on the other hand the Hungarian pianist György Cziffra, besides whom that approximate contemporary and iconic virtuoso Vladimir Horowitz was surely no more than an equal at his very best, barely exists in the twenty-first century as a matter of record or as a cultural resonance.

No wonder, then, that where we are now is early days. No wonder that we have to ask questions which are in danger of seeming to be simple-minded. No wonder that there are accusations of widespread hubris and confusion, and observations of stark heterogeneity as well as missed opportunity.

\textsuperscript{14} In the interests of chapter and verse note that Schoenberg did make a recording of Pierrot Lunaire as conductor in Los Angeles. It has indeed been argued recently that he laid down a tradition of composer-interpretation: see Avior Byron 2006.

\textsuperscript{15} It is worth noting that it is not only recording that can be tainted in this way, as is evidenced by for example the famous Volkov memoirs (Testimony) that led to prolonged, high-temperature debate among Shostakovich aficionados and Russian music specialists in general in the 1980s, even though it seemed evident as early as 1980 that they were in essence fakes. In “The Prospects of Recording” (Page 1990, 331–53) Gould synopsizes the history of the Vermeer forger Hans van Meegeren to remind us about the delinquency of applying information to aesthetic appraisal (341).
It is a good time, then, to outline in closing what kind of theory a theory of WAM recording ought to be.

First, to be credible it would have to demand a sense of canonicity so that like can be compared with like. We used to distinguish between “live” and “recorded” but that distinction has seemed to break down as there is now such empirical fluidity, with studio production that cannot be produced literally “live,” and “live” production that is so technologically supported as to be what would have formerly be called engineered. There is nothing wrong with engineering sound (I, personally, would say), but the precise poietic status of any recording needs to be part of its identity under theoretical scrutiny. What this means is that research in this area is even harder, if that can be imagined, than in traditional musicology where you can often say, to a reasonable person, that the score is the score. We need to know whether we are comparing a good performance with a good performance, or in some other way specify good comparison. There is no room in this area for aesthetic vacuum, and a pseudo-scientific manner is completely inappropriate.

Secondly, in general we need to ask questions about instances, not “practices,” practices in the sense that McClary has indicated (and doubtless not expecting to be quoted in a context such as the present one). Analysis of recorded music needs to be evidence-based, surely, yet the literature is rippling with misconceptions where instances have been read as practices. The 1990s fashion for examining tempo-change in WAM interpretations is telling in this respect. Tempo-change became fashionable in musicological research because it became measurable, or so it might be conjectured. Study of this type was seemingly always comparative. In a context where the discussion is of “what kind of theory” theory is, one may be permitted regardless of any embarrassing and prize-worthy exception to mention that research on instances of tempo-change and why they may or may not matter was not the normal output, only research on practices, on comparisons, as if that mattered by definition. Yet without doubt what psychologists call “timing” has been the mainstream of music-psychological research, often using musical recordings as the database. In Alf Gabrielsson’s magisterial literature review (2003) we are told that “measurement of performance has dominated performance research” and “timing, in particular, has been the primary subject for investigation, which is to be expected as timing is used in performance on all instruments as well as singing. … The term ‘timing’,” Gabrielsson reminds us,

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16 If “timing” is a particularly arcane area of recording-derived research, not least because it seems so very unclear what any theorist or practitioner would be supposed to do even with convincing research outcomes, this is not to say that an original mind can never come up with intriguing findings about practices based on recorded instances: see for example Peter John-son 2004.
usually refers to how the duration of single notes or other entities “deviates” from a norm, such as a “mechanical” or “dead-pan” performance with absolutely constant tempos and strict adherence to the ratios between note values in the score. Such deviations were reported from the very beginning of performance research, and numerous examples [that is, publications] … demonstrated different types of timing, at different levels … and in different kinds of music (225–6).

Thirdly, evidence needs to be intersubjectively true. It’s that old (in music theory) Benjamin Boretz test, well grounded in philosophy: that as opposed to the objective, where something is true in the best of all possible worlds, and the subjective, where something seems to you to be, intersubjectively something can be true for anyone. Tempo-change does well under this head, admittedly, as a phenomenon that in any particular case is highly likely to be true for anyone; yet that hardly matters after its abject failure in the first two tests. The best music criticism, even if it be frowned upon by music theorists asking about canonicity and technical specifics, can also pass this test of intersubjectivity.

I note in closing that it is probably that third category of intersubjective evidence in which two universes can come together somehow: music theory, with its legacy of institutional approval and some degree of acceptance in the history of ideas overall; and recorded music, as a kind of music-theoretical utopia, simply bigger than we have been able to figure so far. One answer to the question “what kind of theory is music theory?” is that it is destabilized, for in some aspects it needs to develop a different view of itself in the world of technology.

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17 Boretz’s writings on “meta-variations” appeared in Perspectives of New Music from 1969, and his original doctoral dissertation was published by University Microfilms, Ann Arbor, in 1971. The most recent published version is Meta-variations: Studies in the Foundations of Musical Thought (1995).
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Literature


Recordings
When the Theorists Are Silent
Mattheson, Bach, and the Development of Historically Informed Analytical Techniques

Ruth Tatlow

Over the past thirty years musicology has moved in a very specific direction, a direction that could easily have been different. The trend for developing interpretative and cognitive models where music is studied as a sounding rather than a written phenomenon has reduced musicology’s traditional emphasis on research into the historical primary source. In this paper I will describe an exercise in experimental music theory based on primary sources, which resulted in the formulation of a new, historically informed theory. The methodology is old-fashioned; the actualization possible only in the twenty-first century. While acknowledging the problems of developing historically informed analytical techniques and suggesting some solutions, I propose that by pursuing similar experiments, musicology could be greatly enriched.

An exercise in experimental music theory

It was while trying to understand mathematical processes in Bach’s works that I first came face to face with the effects on musicology of the general lack of historically consistent analytical techniques. Friedrich Smend’s number alphabet theory purported to be historically based. After a closer study of his sources, however, it was clear not only that his interpretation was faulty, but that he had no historical or documentary evidence for his fundamental premise, that Bach used numbers when he composed (Tatlow 1991). In 1999 I formulated the problem like this:

The lack of evidence from Bach’s manuscripts and contemporary treatises for number symbolism should have prevented [number symbolism] gaining so much popularity … The priority is to establish historical plausibility that Bach used numbers as a tool when he composed. Only then will it be clear which forms of enumeration, operation and translation he used, and only then will analysts deciphering his compositional process be able to make valid interpretations (Tatlow 1999, 321).
Idealistically, I expected to be able to solve the problem in three simple stages: 1) to establish that Bach used numbers when he composed, 2) to isolate which techniques he used, and then 3) to interpret the discoveries. The reality was to prove more complex.

A wide survey of published treatises from the late seventeenth and early eighteenth centuries showed me that several theorists praised the virtues of the well-balanced composition. Johann Mattheson was among these, but he went further than most by actively encouraging the composer to create proportions in his work. The composer

should outline his complete project on a sheet, sketch it roughly and arrange it in an orderly manner before he proceeds to the elaboration. In my humble opinion this is the best way of all to ensure that each part will demonstrate a specific proportion (*Verhältniß*), uniformity and agreement: for nothing in the world is more pleasing to the ear.\(^1\)

By stating that “each part [of a complete project should] demonstrate a specific proportion” Mattheson was describing a compositional pattern which implied the use of numbers.\(^2\) This was a significant find. I now needed to know if Bach used this compositional pattern, and if so, which numbers he used to form the proportions. Published claims about the supposed numerical bases of Bach’s compositions date back to the 1740s. Mattheson, however, did not specify the nature of his “specific proportions” and, as the descriptions of Bach’s use of numbers were equally vague, the quotation was not immediately useful. What kind of proportions was Mattheson referring to? What did he mean by “each part” and “specific proportion”? Was he describing a well-tried and tested numerical method? Would it be possible to find out from the scores whether Bach used proportions to organize his works? Mattheson was not exactly silent, but his message was distinctly unclear.

This was experimental musicology: a dangerous and risky pursuit with no guarantee of a successful outcome or results worthy of publication. Ideally the numerical patterns should to be historically plausible, or even better, taken directly from a historical description and every stage of the experiment should have sound methodological basis with clear ground-rules. I was only too aware of the many well-intentioned but spurious experiments in numer-

\(^1\) Mattheson 1739, 240. The original German of the complete text reads: p. 240 §30 “Wer sich also, seiner Fertigkeit im Setzen ungeachtet, der oberwehnten Methode, auf gewisse ungezwungene Art bedienen will, der entwerffe etwa auf einem Bogen sein völliges Vorhaben, reisse es auf das gröbste ab, und richte es ordentlich ein, ehe und bevor er zur Ausarbeitung schreitet. Meines wenigen Erachtens ist diese die allerbeste Weise, dadurch ein Werck sein rechtes Geschicke bekömmt, und ieder Theil so abgemessen werden kan, daß er mit dem andern eine gewisse Verhältniß, Gleichförmigkeit und Uebereinstimmung darlege: maassen dem Gehör nichts auf der Welt lieber ist, denn das. §31 Zeit und Gedult wollen dazu gehö- ren.” In *Musurgia Universalis* (Rome, 1650), 193 f. “Plectrologia Musarum.” Athanasius Kircher described a similar method of composing “Plectrologia Musarum.”

\(^2\) See the discussion of *Verhältniß* on p. 214 below.
ology. Arithmetical correlations frequently appear when notes, bars, pulses and other rhythmic units are counted, leading many gullible and naïve number hunters to believe they have unlocked the composer’s secret methods, when in fact similar results could be found by counting any random feature in a newspaper article. The results frequently show little more than the strong human desire for pattern, and are a demonstration of the author’s own views. The path of Zahlensymbolik is strewn with such writings: an analyst sees numerical patterns in scores; each interpretation is more bizarre than the last, telling the reader more about the analyst’s personal philosophy than about the composer. And yet I was aware that in pursuing the experiment I had to go through some of the same procedures as those number hunters of whom I was most critical. There was a high probability that I would end up with strings of random numbers and no serious conclusion. Misunderstanding and ridicule lay ahead, but the slim possibility that the investigation might result in a historically consistent analytical technique gave me courage. If eighteenth-century theorists could be released to speak of hitherto unknown compositional techniques and shed new light on Bach’s music, it was worth the risk.

To begin the investigation I needed good sources and clear avenues of inquiry. It was necessary first to discover with what Bach would have formed Mattheson’s “proportions in every part.” Some feature of the score had to be counted in order to form proportions. Would the proportions be formed by the number of bars, the rhythmic divisions, the number of movements, or something else? Counting the wrong unit would obviously be a complete waste of time. The best historical statements I could find were by Praetorius and by Mizler, both of whom described a method to estimate the duration of sacred works, and both of whom used the number of bars as the unit of measurement.

Praetorius reckoned that in average metre, 80 tempora (bars) would last half of quarter of an hour, 160 tempora fifteen minutes, 320 tempora thirty minutes and so on.3 In 1754, Lorenz Mizler published a similar, updated version of Praetorius’s rule of thumb, writing:

From experience one can determine the length of a cantata, so that 350 bars, of any metre, will last approximately 25 minutes, which is long enough in

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winter, although in summer it could be 8 to 10 minutes longer and be roughly 400 bars long. But it’s my opinion that a composer should think more about the music or movement, bringing it into beautiful order, rather than about the time. It should not depend on specific minutes.4

Mizler’s equation shows that the bar and minute were used as units of measurement, although he considered that creating beautiful order in a composition was more important than exact timing. Coming immediately after the announcement of Bach’s contribution of the canon BWV 1076, the positioning of this quotation in Mizler’s *Musikalischer Bibliothek* suggests that these guidelines also came from Bach.5 And if they did, I had struck gold. Although these quotations did not prove that Bach or Mattheson formed proportions with the number of bars or minutes, Mizler’s description was sufficiently clear to justify the start of some numerical experiments. Before beginning, however, I had to decide whether the composer would have been interested in the bars or the minutes.

The fact that the number of bars in a work does not change, and that there are many examples of cumulative bar totals written by composers and copyists in early eighteenth-century copies, including Bach’s own manuscripts, persuaded me that a composer seeking to create beautiful structural proportions in a composition might prefer the unchanging medium of the bar, rather than duration, which changes with every performance. Proportions formed by the number of bars were what I would investigate. The choice may be wrong, but it was a risk I had to take. It was, after all, experimental musicology.

As many of Bach’s compositional choices were vital to the experiment, it soon became obvious that the autograph score, rather than a modern edition, was an essential source.6 In time reams of figures, contradictions, and patterns were generated and, as predicted, very few were convincing. I was on the point of closing the books on the project when there was an unexpected breakthrough, which eventually led to the formulation of a historically informed technique. I have named the technique proportional parallelism,
which is itself a historically plausible formulation.\textsuperscript{7} In order to illustrate the procedure, and before discussing the implications for music theory and analysis, I will give a brief description of one set of results.\textsuperscript{8}

**Formulating the historically informed technique of proportional parallelism**

At least five levels of proportion can be identified in the collections Bach published or transcribed as fair copy. The proportions are formed by the number of bars and “each part … demonstrates a specific proportion” (Mattheson 1739, 240), see Example 1.

<table>
<thead>
<tr>
<th>Nature of the proportion</th>
<th>Level</th>
</tr>
</thead>
<tbody>
<tr>
<td>Proportion formed by two collections</td>
<td>5</td>
</tr>
<tr>
<td>Proportion formed in the collection as a whole</td>
<td>4</td>
</tr>
<tr>
<td>Proportion formed between two works in a collection</td>
<td>3</td>
</tr>
<tr>
<td>Proportion formed between movements of a work</td>
<td>2</td>
</tr>
<tr>
<td>Proportion between sections of a movement</td>
<td>1</td>
</tr>
</tbody>
</table>

Example 1. The five different levels of proportions found in Bach’s revised works.

The collection of sonatas and partitas for solo violin, *Six solos* (BWV 1001–1006, SBB P967), has at least four different levels of proportion in its structure formed between the number of bars, movements and works, as shown in Example 2 (Tatlow 2007):

<table>
<thead>
<tr>
<th>Proportion</th>
<th>Number of bars</th>
<th>Level</th>
</tr>
</thead>
<tbody>
<tr>
<td>1:1</td>
<td>2400:2400</td>
<td>5</td>
</tr>
<tr>
<td>2:1</td>
<td>1600:800 4:2</td>
<td>4</td>
</tr>
<tr>
<td>1:2 and 2:3</td>
<td>408:816 and 272:408</td>
<td>3</td>
</tr>
<tr>
<td>1:1 and 2:1</td>
<td>136:136 and 272:136</td>
<td>2</td>
</tr>
</tbody>
</table>

Example 2. Four levels of proportion found in the *Six solos* for violin.

The smallest level of proportion discussed in this paper is level 2. It is formed between the movements of a work, and can be seen in the G-minor

\textsuperscript{7} Zedler 1732–54. See the entries on “parallelistica methodus,” “loca parallela,” and “parallelismus.”

\textsuperscript{8} Tatlow 2007, 37–58. In preparation is also a monograph *Bach’s Numbers Explained* (working title), which will discuss many other examples of the technique in Bach’s works.
Sonata (BWV 1001), the B-minor Partita (BWV 1002) and the E-major Partita (BWV 1006). In BWV 1001 a 1:1 proportion is formed between the first three movements, which have 136 bars, and the fourth movement, which also has 136 bars (see Example 3).

<table>
<thead>
<tr>
<th>Sonata 1</th>
<th>Number of Bars (no repeats)</th>
<th>Total</th>
<th>Ratio</th>
</tr>
</thead>
<tbody>
<tr>
<td>Adagio</td>
<td>22</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Fuga</td>
<td>94</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Siciliana</td>
<td>20</td>
<td>136</td>
<td>1:1</td>
</tr>
<tr>
<td>Presto</td>
<td>136</td>
<td>136</td>
<td>1:1</td>
</tr>
</tbody>
</table>


In the second solo, the B-minor Partita (BWV 1002), the proportion is formed by the first six movements, which have 272 bars, and the seventh and eighth movements, which have 136 bars, forming a 2:1 proportion (see Example 4).

<table>
<thead>
<tr>
<th>Partita 1</th>
<th>Bars (no repeats)</th>
<th>Total</th>
<th>Ratio</th>
</tr>
</thead>
<tbody>
<tr>
<td>Allemande</td>
<td>24</td>
<td>272</td>
<td>2:1</td>
</tr>
<tr>
<td>Double</td>
<td>24</td>
<td>272</td>
<td>2:1</td>
</tr>
<tr>
<td>Corrente</td>
<td>80</td>
<td>272</td>
<td>2:1</td>
</tr>
<tr>
<td>Double</td>
<td>80</td>
<td>272</td>
<td>2:1</td>
</tr>
<tr>
<td>Sarabande</td>
<td>32</td>
<td>32</td>
<td></td>
</tr>
<tr>
<td>Double</td>
<td></td>
<td>32</td>
<td>2:1</td>
</tr>
<tr>
<td>T di Bore</td>
<td>68</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Double</td>
<td>68</td>
<td>136</td>
<td>2:1</td>
</tr>
</tbody>
</table>

Example 4. Proportion 2:1 in the B-minor Partita (BWV 1002).

The next level of proportion, level 3, is formed between two solos within the collection. In this collection the proportion can be seen between the first and second solos (see Example 5). Examples 2 and 3 show that the first two solos have a common denominator of 136, and therefore we find a 2:3 proportion between the 272 bars of the G-minor Sonata, and the 408 bars of the B-minor Partita. When all the repeats are performed in these two works, there is a 1:2 proportion, with 408:816 bars. Bach must have planned these proportions for them to be so exact.
When the Theorists Are Silent

<table>
<thead>
<tr>
<th>Work</th>
<th>Bars (no repeats)</th>
<th>Total</th>
</tr>
</thead>
<tbody>
<tr>
<td>S1 (Sonata in G minor)</td>
<td>272</td>
<td></td>
</tr>
<tr>
<td>P1 (Partita in B minor)</td>
<td>408</td>
<td></td>
</tr>
<tr>
<td>S2 (Sonata in A minor)</td>
<td>396</td>
<td></td>
</tr>
<tr>
<td>S3 (Sonata in C major)</td>
<td>524</td>
<td>1600</td>
</tr>
<tr>
<td>P2 (Partita in D minor)</td>
<td>412</td>
<td></td>
</tr>
<tr>
<td>P3 (Partita in E major)</td>
<td>388</td>
<td>800</td>
</tr>
</tbody>
</table>


This cannot have happened by chance. In order to achieve these specific proportions, Bach must have planned and sketched the numerical order for each part, just as Mattheson had described. An earlier version of the collection in Kellner’s hand (SBB P 804) lacks the second solo entirely and has shorter versions of several other movements. When Bach revised his early version, he added the B-minor Partita, making it a perfect numerical match for the first sonata, and constructed the two large fugues (BWV 1003/2 and BWV 1005/2) so that the whole collection created a perfect 2:1 proportion.

The final proportion, level 5, is perhaps the most bizarre of all the proportions to be found in Bach’s collections. It is formed between two complementary works, which Bach designed as a pair. Several of Bach’s revised collections have exactly the same, or exactly half, the number of bars as another collection, which I term a level 5 proportion. The Six solos for violin is numerically parallel to the Six sonatas (BWV 1012–1019) for harpsichord and violin, as both have 2400 bars. Furthermore, they both have an identical level 4 proportion with 1600:800 bars. There are three versions of the Six sonatas, and it is the latest version, in a copy (SBB P 229) by Bach’s son-in-law Altnickol, which has this extraordinary structural parallel to the violin solos. There is no evidence to suggest that Bach planned this at an early stage. It was as Bach revised the Six sonatas that he decided to make the two collections numerically related.

Having stumbled across such an extraordinary series of proportions I was keen to test the principles against Bach’s other works. Did they all have
similar levels of perfectly planned proportions? Did Bach form the proportions as he planned the collection of six, or as he composed each individual work, or would proportions be found in sketches and early versions? The score of the *Six solos* is a fair copy, or Bach’s equivalent of a published work. It quickly became clear that parallel proportions are a feature of all the collections that Bach published or wrote in fair copy. Perfect proportions were a feature of Bach’s finally revised versions, and not of his early sketches.

Against all odds, the experiment had succeeded. By working from a pattern alluded to, but not described in any detail, in a treatise contemporary with the composition, proportions that had been intuitively sensed by generations of musicians could now be demonstrated.

Mattheson’s treatise had been the subject of several studies, including a critical translation, before I began scouring it for clues about Bach’s compositional procedure. Many readers had missed the significance of Mattheson’s references to proportion, partly because he did not say how the proportions should be formed in all parts, and partly because of a misleading translation of the word *Verhältniß* and *Verhalt*. Re-reading Mattheson after discovering the proportions in Bach’s works, it almost seemed that he was describing proportional parallelism. For example, in a summary of how to compose a melody, Mattheson lists thirty-three guidelines, among which are the following:

- observe well the proportion of all parts, sections and divisions;
- you should have the geometrical proportion of similar movements consciously before you, specifically the *numerum musicum*, i.e. retain exactly the measurement of the melody by numbers;
- the number of bars should be proportioned.10

Since the experimental method has resulted in the formulation of an “authentic” analytical technique it can be pronounced successful. But what has it really achieved? In what sense is proportional parallelism “authentic”? In naming the technique “proportional parallelism,” I have consciously gone beyond Mattheson’s description: as an academic working in 2007, I felt I needed a label for the phenomenon. Even though I invented the term, it would not have been strange to a Leipzig or Hamburg citizen in 1723 or 1739. Nonetheless, it is unashamedly a result of twenty-first century musicalological inquiry.

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9 Harriss (1967) and Wolff (1998) (whose mother tongue is German) both use the words relation, relation and proportion to translate “Verhältniß,” see section 3 below.

The problematic of historically informed theory

You may ask what is so historical about historically informed theory if it is a product of our time. Surely anyone could take Bach’s scores today and find parallel proportions without recourse to Mattheson’s texts? Did I really need to struggle with Mattheson to discover this new technique? The answer is that I did need to go through the procedure, and that the formulation of the theory was a direct result of the relationship between the historical research, the sources and the scores. But many musicologists today would wholeheartedly disagree with me. I will therefore explain my motivation by examining four essential ingredients in the procedure: trends in musicology, the nature of analytical procedure, the nature of historical inquiry, and our view of music today.

A “crisis of historicism” in musicology

Musicologists today are increasingly engaging with models of interpretation and criticism, viewing music as a sounding, rather than written, phenomenon. It is no accident that these new methods are being pursued at a time when there is a discipline-wide loss of confidence in historical method.

We cannot simply throw ourselves into an encounter with the past, or so the reasoning goes, for we have no idea how base our hidden motives may be, and how badly we may need to expiate them. That is why, over the last decade or so, musicologists have become engaged in a desperate search for legitimation—a predicament summed up by the question, is it still possible? (Wegman 2003, 142).

This, combined with a tendency to “depreciate historical musicology because we cannot attain perfection in it” (144) is producing research shot through with “scholarly angst, narcissistic self-torment and defensive theorizing” (145). This is a desperately depressing analysis, more so because it rings true; but there is a way forward. Wegman continues:

The only world that is real is the one we live in today. History adds a rich dimension to that world … It is only the paralyzing fear to take human risks that might render historical musicology impossible … or the fear that we may not be forgiven for our failings (145).

If Wegman is correct, it should once again be possible to enjoy using primary research to enrich musicology with new insights, while still recognizing that we cannot escape our own worldview. It is now a matter of being prepared to take risks, deciding which questions we should ask of history, and choosing a methodology to help us answer those questions.

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11 I would like to thank Rob Wegman for his courageous and inspiring article, which helped me through an intellectual trough at an opportune time.
The nature of analytical procedure

The symbiotic relationship between analyst, analytical theory, and composer lies at the heart of the problem of developing analytical techniques. At its most basic, this problem is caused by the combination of at least three, potentially contradictory, philosophies: 1) the philosophy of the analyst, 2) the philosophy lying behind the technique used by the analyst, and 3) the philosophy of the composer who wrote the score.

The analyst’s worldview is colored by his personal philosophy; this personal philosophy affects his understanding of music, and his search for patterns in that music. The composer creates the patterns, but not all the many different patterns, which can be made from any score, were put there consciously by the composer. The analyst recognizes these patterns, and then asks and answers questions about them, using a technique written by a theorist from a particular time and culture; the technique used affects which patterns can be found. Therefore a technique can either help or hinder the analyst in discovering the composer’s conscious patterns. For example, if a technique described by a late nineteenth-century theorist is used to study a 1723 score by Bach, the analyst will be able to recognize nineteenth-century patterns, but will not be able to claim that Bach deliberately put them there. A nineteenth-century theorist is not a reliable guide when exploring an eighteenth-century score. The analyst is limited to the questions: “Will the 1723 score function according to the technique described by the nineteenth-century theory? And does the score behave as the theorist would have expected?” The analyst, however, may still be separated from the patterns the eighteenth-century composer consciously put in the score.

I am claiming that, by taking principles from an eighteenth-century theory written by a theorist with the same educational and cultural background as the composer, the analyst has a better chance of finding the composer’s original patterns. Johann Mattheson published many theoretical writings. He was a composer and performer who knew Bach’s compositions and whose thinking was formed under the same Lutheran educational system. Since the worldviews of composer and theorist are similar, as are their views and experiences of music, the analyst is cutting out one level of complexity from the procedure. For example, the analyst can look in the 1723 score for patterns described by Mattheson in 1739, and be in a position to ask not only “How does this score function?” but “How did the composer make this score function?” and even “Why did the composer make this choice or this change?” As a result of the shared cultural, linguistic, and philosophical basis of the composer and theorist, I consider this technique to be “authentic,” “historically informed,” and the methodological approach HIT—Historically Informed Theories/Techniques. (The acronym HIP [historically informed performance] is frequently used today. Thus HIT seems an appropriate acronym for its theoretical counterpart.)
We will never know if an interview with Bach and Mattheson would provide a more “authentic” explanation of a Bach score than an illuminating twenty-first century analysis, because Bach and Mattheson are both dead. But we do have their treatises and scores. If we create circumstances in which both composer and treatise speak the same language, literally and musically, then surely we will be closer to the hypothetical interview?

The wide availability of published and electronic sources, together with the detailed source studies, facsimiles, and critical reports published since 1950 enabled me to discover Bach’s parallel proportions. Without lexicographical studies, Mattheson studies, the on-line publication of Zedler’s dictionary and much more I could not have begun to understand Mattheson’s words. The development of historically informed theories need not be limited to compositions and treatises from the eighteenth-century. Since ancient times, music theorists have described compositional method, making available to the modern analyst a wealth of descriptive patterns used in compositions from every age. The raw material awaits our attention, but we need first to overcome the “crisis of historicism,” accept that our view of the world cannot be neutral, and explore the treatises for new historically informed techniques. Even then, the theorists are still prevented from speaking by a variety of practical obstacles.

The nature of historical inquiry

A historian, or in our case analyst, approaches the sources in order to answer questions. The manner in which these questions are asked or answered can be problematic. Frequently the analyst asks a question that post-dates the treatise. The question may be formulated in terms which the theorists of the period would not have recognized. Similarly, the question, although not alien to the theorists, might not have been of interest to them, and so they would not have covered it. The treatises may be written in the language of another culture or historical period, language which is unfamiliar to the analyst. Their titles can be the first obstacle. The content pages and indexes are better guides, and yet these too are frequently limited. There is no alternative but to leaf through the volume, skim-reading every page to look for a key word or concept, before deciding whether the treatise is valuable or not. Obviously this key word or concept must be one the author of the treatise would have used.

Even when the printed words can be read and an interesting passage has been found, the analyst’s comprehension will remain limited until the cultural codes hidden in the words are understood. Gulfs of time, language, society, and culture stand between writer and reader: they remain separated until bridges of understanding can be built. A modern dictionary is useless; a contemporary encyclopedia might help.

The social history of the period in which the author was writing is also as important. The pace of life in 2007 in a densely-populated city, or even vil-
lage, with modern media and communications, would have been beyond the conception of the most sophisticated theorist in 1739. The analyst in 2007 must allow his or her understanding of the parameters of music and musical concepts to shrink accordingly.

The analyst also needs to develop a detailed knowledge of the language in which the treatise is written. The treatise might not only be in a foreign language, but in a 1739 version of that language. Vocabulary learnt in a modern language course will have changed its meaning over nearly 300 years. Great care has to be taken before presuming to understand a word, as the precise shade of meaning may make a significant difference. The predominance of English as the principle musicological language causes many problems, but there are difficulties even when the treatise is in the analyst’s mother tongue. There is no difficulty when a well-known word is used in a strange expression, as the strangeness alerts the reader to the fact that the phrase makes no sense, although every single individual word is familiar. The real problem arises when a well-known word is used in a context, which appears easy to understand. The reader naturally assumes that he or she understands the full meaning of the word, without realizing that its meaning has evolved over the years. In my own research it was the key word Verhältniß that had changed its shade of meaning. It has been translated as relation and relationship, which is the modern, general meaning of the word. However, it is clear from the thirty-four entries for definitions of Verhältniß in Zedler’s Lexicon (1732–1754) that it was used exclusively as a mathematical term at that time and meant a literal 1:1, 1:2, etc. proportion. It still has a mathematical meaning in modern German, but as it is frequently used in a transferred sense to mean “relationship,” the strength of Mattheson’s usage went unnoticed. When Mattheson used the word Verhältniß he meant a mathematical proportion. His readers in 1739 understood this and the analyst today must also understand this.

**Changed view of music**

The discovery of Bach’s perfect proportions is a reminder of an older tradition in classical music, when music was regarded primarily as a written rather than a sounding art form. In 1788, Johann Nikolaus Forkel, Bach’s first biographer, stated that “musical perfection is dependent upon notational perfection” (Forkel 1967 [1788]; Tomlinson 2003, 36). At this time “the notated music came to be viewed less as a preliminary script for performance than as the locus of the truest revelation of the composer’s expressive spirit” (Tomlinson 2003, 39).

Did Forkel intend us to understand that the musical perfection of Bach’s work was dependent upon notational perfection? And if so, was he advocating a measure of perfection that could be achieved in notation in the score, which would thus make the music more perfect? If such an influential composer as Bach deliberately manipulated the number of bars in his works
to create perfect proportions that cannot be heard, it would surely impoverish our understanding of Bach’s music if we forgot to see his scores as he did, undervaluing the perfection of the notation? Furthermore, if it transpires that composers before and after Bach viewed the score similarly, the significance to our understanding of western music as a written art form becomes even greater.

Will the theorists remain silent?

The search for historically consistent techniques belongs to the late twentieth and the early twenty-first centuries (Buelow 1973–4; Dreyfus 1996). Proportional parallelism is just one technique. There are many more formulations in Mattheson’s publications waiting to be understood, all of which may enrich our understanding of music as both a written and sounding art form. Mattheson is just one theorist. Treatises by theorists of every culture, philosophy and language line the shelves of our libraries, while musicology continues uncertainly along a path lined with “scholarly angst and defensive theorizing” (Wegman 2003, 145). The fact that we cannot escape our own worldview need not prevent us from reviving the old-fashioned joy of engagement with primary sources. The fact that we understand music primarily as a sounding art form need not prevent us from appreciating the aspirations of notational perfection of earlier times. It is my hope that this exercise in experimental musicology will encourage other musicologists to search for new historically informed analytical techniques and release more theorists to tell us about the compositions they understand best.
References


<http://www.bachnetwork.co.uk/ub2_contents>


Mathematics and Ideology in Modernist Music Theory

Jacob Derkert

Introduction

As is well known, mathematics has a venerable tradition in music theory. Pythagorean theory as mediated by Plato and others exerted a strong influence on Western Music Theory, and was the exclusive model for mathematics in music theory until the late sixteenth century. The physical theory\(^1\) that had its first manifestations during this time did share the majority of its issues with Pythagorean theory, but at once focusing on, and limiting its interest to, the propagation and reception of sound.\(^2\) In short, these theories had in common: 1) a conceptual framework including interval and consonance-dissonance; 2) a mathematical framework in terms of ratios between magnitudes; 3) a conception of consonances as associated with ratios composed by relatively small whole numbers, and 4) an observational framework concerning or including physical bodies. The differences are nevertheless significant. In Platonic-Pythagorean theory, on the one hand, the numbers involved

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1 In the words of H.F. Cohen, it happened around 1600 that “the phenomenon of consonance was placed on a new, physical basis” (Cohen 1984, 75). Cohen extensively treats this period of change from an older theory, in which mathematics was thought to unveil the actual numerical (or sometimes geometrical) properties of reality, to a physical theory, in which the role of mathematics was to give an account of the (physical) relations between physical entities.

2 Theories of sound relating it to bodies and to excitation of a medium did not originate with the physical theory, of course. But in general earlier theory lacked a clear conception of a role for mathematics in the very description of physical processes. Not entering this rather complex subject matter here, let us note that according to Palisca 1961, 104 ff., and Cohen 1984, 77 ff. one could tentatively date the first connection of antique conceptions of the nature of sound with a quantitative approach to consonance to Giovanni Battista Benedetti in the 1560s: “What is really new in Benedetti’s account is his linking up, in a quantitative fashion, [the] ancient wave analogy with the problem of consonance. Before, the musical intervals had always been primarily associated with string lengths” (Cohen 1984, 77).
per se explains the phenomenon of consonance, on the other hand, music, via harmony as the locus of correspondences, is related to other dimensions of reality (the soul of man, the world etc.). Physical theory in contrast tends towards a modern conception: The fact that observable physical relations (for example between portions of a string) in the case of being “simple ratios” tends towards suavity of resulting sound is something to be explained on the same level, i.e. physically, and numbers are thereby “reduced” to a role of measurement. At the same time processes in sound media acquires a main role in explaining the experience of consonance-dissonance, etc. In short, an analogistic world-conception gives way to a conception for which efficient causation is the sine qua non of understanding the world.³

The physical theory is not only the disloyal heir of its Pythagorean progenitor, it is also the ancestor of acoustic theory. But just after the inception of modern physical thinking about musical sounds, the first application of mathematics in a music theory that is not primarily a theory of intervals in abstracto and is not to be considered an acoustic theory in spe, came to light in Marin Mersenne’s writings.⁴ Mersenne’s problem is a typically combinatorial one, to determine the possible sequences of tones that could be produced with a collection of tones, repetition not allowed—in other words to determine the possible total orderings of a set of tones. In the process he also mapped the actual combinations.

After Mersenne, applications of mathematics in respects other than proto-acoustical or acoustical came to the fore during the latter half of the nineteenth century in Austria and France.⁵ These theories mostly unite a combinatorial aim with an attitude that presages twentieth-century modernism. In these theories one finds for the first time: 1) translations (bijections) of scales and intervals to finite sequences of non-negative or natural numbers; 2) definitions of pitch-class sequences as equivalent under transposition, given identical internal interval successions; and 3) systematic catalogues of all possible pitch-class combinations given a well-tempered chromatic scale. The writings of Loquin and others were without much impact. They didn’t initiate traditions, nor have they been resurrected by later generations as anything more than historical documents.⁶

Despite this limited impact, the three theoretical acquisitions mentioned became important aspects in the application of mathematical theories and methods in music theory after World War II. But this music theory has gone

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³ An overview of the Pythagorean tradition, built on primary sources from the pre-Socratics to Kepler, is found in van Wymeresch 1999, 13–57. For a penetrating discussion of the late instance of Pythagorism that manifest itself in Zarlino’s work, see Moreno 2004, 25–49. Both van Wymeresch and Moreno apply Michel Foucault (Les Mots et les Choses, L’archeologie de savoir, in particular).
⁴ Mersenne 1624, Livre Troisieme and 1636, Livre Second, Propositions VII, VIII–XXI.
⁵ For an overview, also including some later authors in the same vein, see Nolan 2003.
beyond the primarily combinatorial problems posed and solved by Mersenne and his followers. Though difficult to relate in a short general formulation the mathematics used, central concerns have been combinatorics (still), group theory,\(^7\) non-axiomatic set theory (primarily in terms of Boolean algebra),\(^8\) axiomatic set theory,\(^9\) diverse forms of probability theories,\(^10\) and lately graph theory (as an aspect of what in music theory is called transformational theory), among others.\(^11\) The sophistication in the application of mathematics in general is much higher than it was before. Still, one should not be too sure to date the emergence of a “modern” mathematization of music theory to the post-WWII era.

One can note a radical shift in the mathematized subject manifesting itself at a far earlier date than 1946. Mathematization in modern music theory often takes place in the form of a dual articulation, with an initial articulation resulting from some verbal assumptions concerning the physical properties of a basic material, implying an acoustical mathematical formulation. The elements of the basic material and the assumptions concerning their physical properties then are translated into a finite or infinite sequence consisting of non-negative integers. In a second articulation, one relates the basic elements in diverse ways, forming sets (unordered or ordered) or sequences, determining relations between sets or between sequences, etc. Both the primary and secondary articulations have a strong potential to be interpreted in relation to sounds as phenomenally given to a listener. This constitutes a clear difference from the Pythagorean or acoustic-physical theory, where the actual mathematical formulation concerns measurements of lengths and weights of sound-producing instruments, measurements of frequencies, etc., all things

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\(^7\) First applied by Babbitt (1946 and 1960) and in later writings of his. As will be discussed later, this application is subservient to an aesthetic, ideological project. More neutral applications can be found in Mazzola 1985 and 1990. And as has been shown in Gollin 2000 and Engbrotsen 2002, group theory was in some sense latent in music theory from at least around 1700, and especially so in late-nineteenth-century harmonic treatises in Germany—but then one should not make too much out of this, the fact is that group structures are central in music on different levels, and that certain aspects of these group structures have been adequately, though partially, described in music theory long before the application of mathematical group theory in music theory, or even long before the existence of mathematical group theory.

\(^8\) Boolean algebra is central in the work of Forte, of course, see Forte 1970, for example.

\(^9\) For a succinct example of the application of axiomatic set theory in music theory, see Rahn 2001 [1979]. A critical question is what role the introduction of a system of axioms for set theory, for example, the Zermelo–Fraenkel-axioms used by Rahn in this article, actually implies for the application of sets in music theory.

\(^10\) There are many different applications of these, for example, Iannis Xenakis’s 1992, 1–109 prescriptive ones.

\(^11\) See Lewin 1987, 193 ff. The overview in Nolan 2002 emphasizes the American scene, to the point of overemphasizing it, not mentioning the works of, for example, Xenakis or Guerino Mazzola. Given its format, it is also bound to make some simplifications. Andreatta (2003) relates extensively, though without pretensions to cover the whole field, the use of algebraic methods in music-theory and music-analysis.
that are not accessible for the sensual subject in a direct and pure relation with the sound.

One could date the appearance of an unequivocally modern music theory, in a sense strongly connected to the modern mathematization I have tried to characterize here, to Fétis, above all in his *Traité complet de la théorie et de la pratique de l’harmonie* from 1844, and its extended “quatrième édition” of 1849. Mainly in the fourth part of this work, a revised version of the author’s *Esquisse de l’histoire de l’harmonie* from 1840, Fétis makes extensive critical comments on writings that are trying to base harmonic theory on mathematic, physicalistic, or mathematic-physicalistic thinking of an acoustic or proto-acoustic stance.

In the *Préface* of 1849, after relating how in the flash of a moment he found what he considered to be “the principle of harmony,” he succinctly gives his opinion of different standpoints on this matter:

> The principle of harmony has been searched for in acoustic phenomena, in numerical progressions of diverse systems, in more or less ingenious aggregations of intervals of sounds, and in arbitrary classifications of chords; but as an examination of the monuments of music history makes clear, it is not of things like these that art is formed … A more active and immediate cause has had to act on them [the musicians] in the forming of chords and in the successions given to these. Both as art and as science, this cause, or in other words this harmony, could be no other than the one which regulates the relations of tones and the order in which they follow each other in the two scale modes.13

Fétis’s name for this principle is tonality (*tonalité*). He further characterizes this as a “metaphysical principle”:

> If it is recognized that these foundations of the system [in acoustical phenomena and/or the laws of mathematics] are deceptive … it is evident that there remains no other principle for the construction of the scale and of tonality than the metaphysical principle, a principle, both subjective and objective, the necessary result of the sensitivity that perceives the relationship of sounds, and the intelligence that measures them and deduces the results. After so many centuries of studies carried out in absolutely opposite directions, one manages to recognize that the Pythagoreans were mistaken in attributing a basis for tonal construction to numbers that does not belong to them. The

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12 For an English translation of this work with commentary, see Fétis 1994.
13 “On a cherché … le principe de l’harmonie dans des phénomènes acoustiques, dans des progressions numériques de divers systèmes, dans des procédés plus ou moins ingénieux d’agrégations d’intervalles des sons, et dans des classifications arbitraires d’accords; mais il est évident, par l’examen des monuments de l’histoire de la musique, que ce n’est pas par ces choses que l’art s’est formé. … Une cause plus active, plus immédiate, a du agir sur eux [les musiciens] dans la formation des accords et dans l’enchaînement qu’ils leur ont donné. Cette cause, ou, en d’autres termes, ce principe de l’harmonie, et comme art et comme science, n’a pu être que ce qui règle les rapports des sons et l’ordre où ils se suivent dans la gamme des deux modes” (Fétis 1849, vii; Note that the pagination is faulty, it ought to have been p. ix).
Aristoxenians were no less mistaken in attributing a faculty to the ear that it
does not have. The ear perceives the sounds; the mind compares their relations-
ships, measures them, and determines the melodic and harmonic condi-
tions of a tonality (Fétis 1994, 158 f.).

The last sentence could seem deceptively close to a philosophical pro-
gram wholly compatible with modern mathematized music theory. But Fé-
tis’s own theory is certainly not mathematical. It concerns the characters the
mind (l’esprit) distinguishes in the chords built from the tones of a given
scale type. Fétis distinguishes among these characters of repose and conclu-
sion, of attraction calling for a continuation (that is, a resolution), etc. In all
this, he not only prefigures authors on harmony like Ernst Kurth—rightly or
wrongly characterized as “phenomenological”—but even takes up the thread
from certain passages in earlier theory, for example Rameau in 1722: “A
perfect cadence is the name one gives to a way to end a tune that satisfies in
such a manner that after that cadence nothing more is to be desired.”¹⁴ The
conceptual scheme is here obviously desire/satisfaction, the former a state
calling for change, the latter a static state.¹⁵

Schellhous (1991) considers Fétis as representing a Kantian solution to
the problem of founding a theory of harmony, in that he is thought to have
reused the conception of the synthetic a priori. The intuitive feeling for ton-
nality he ascribes to human beings should be an inborn human faculty, on
the same footing as the conception of time and space according to Kant,
which is at once made possible and limited by an inborn human capacity.¹⁶
Accepting Schellhous’s interpretation, and in line with Moreno’s character-
ization of Kant as a “major philosophical articulation[] of Western subjectiv-
ity” (Moreno 2004, 9), which he terms “modern,” our choice of Fétis as an
instigator of “an unequivocally modern music theory” would seem justi-
fied.¹⁷

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¹⁴ “L’On appelle Cadence parfaite, une certaine conclusion de chant, qui satisfait de façon,
que l’on n’a plus rien à désirer après une telle Cadence” (Rameau 1967 [1722], 54).
¹⁵ The conceptual couple desire-satisfaction does not originate with Rameau, but can be found
considerably earlier, for example, in Descartes Compendium Musicae from 1618, see Descar-
tes 1987 [1618], 125 and 127. Here Descartes discusses intervals, not cadences seen as
standardized successions of chords like Rameau. Later on page 127, one finds a passage
extremely close to the one cited in Rameau: “At the end of the tune, the ear must be satisfied
so that it does not want anything more, and considers the tune to be perfect. This is best effec-
tuated by certain orders of tones, always ending in perfect consonances, that the practitioners
call cadences.” “Vt in fine cantilenae ut auribus satisfiat, ut nihil amplius expectent, & per-
fectam esse cantionem animadvertant. Quod fiet optime per quosdam tonorum ordines, sem-
per in perfectissimam consonantiam desinentes, quos practici cadentias vocant.”
¹⁶ Schellhous should perhaps have made some comments on Fétis’s conception of a history of
tonalities, i.e., that the pretended Kantian subject in this case changes room or space accord-
ing to the period she is habiting.
¹⁷ Though Moreno discusses Kantianism in relation to Gottfried Weber, see Moreno 2004,
18 ff. and 247–54.
It is tempting to suggest that modern mathematized music theory is a post-Fétis theory in which mathematical structures supplant the energetically biased conceptual system of Fétis. In both cases, what is theorized is a musical landscape given phenomenally—with or without an explicit translation relation to a physical reality—the structures of which are investigated, and are described or interpreted, in either “energetic” metaphors or in mathematical terms and in accordance with mathematical theories of structure.

I will resist this temptation in this context; sticking to the formulation that modern mathematized music theory concerns aspects of music that could in principle be reduced to what is phenomenally given in a direct relation between subject and music as a “pure” sounding object. Thereby, one won’t need to distinguish between theories in which the phenomenal aspect of music is the actual outspoken or implied “object of investigation,” and theories in which it is not, but in principle could have been. Consequently, a modern mathematization of music theory would be at hand in the early seventeenth century: In the part of Mersenne’s work where he is concerned with combinatorics, the acoustical substratum (which he discusses elsewhere in this work) and the notions related to that (consonance–dissonance, etc.) are not really relevant for the questions his combinatorial exercises answers.\(^\text{18}\)

**The survival of a weak physicalism**

A modern mathematized music theory doesn’t preclude a rather strong presence of assumptions concerning the physical background. Theoreticians of the modern vein, for example, might take the equal-tempered chromatic scale as a basis for their own endeavor. It was true for Carl Friedrich Weitzmann and Heinrich Vincent in the nineteenth century (Nolan 2003, 208 f.), as well as for Milton Babbitt (1946, 1; 1960, 56), among others. But this is just a prerequisite for the mathematical problems actually posed, and as such it is not even logically necessary; from a mathematical viewpoint, the twelve-tone system or a twelve-tone system, for example, works perfectly well without an assumption of equal temperament.

One could speculate on the assumption of equal temperament underlying much mathematized music-theory as an analogue to the conception of geometry as “the quantitative science of extension,” the conception that was

\(^\text{18}\) Relating to the more general question of a “modern” music theory, one has to think through the full meaning of the revival of Aristoxenianism from the late sixteenth century onwards, and the way changing epistemologies affected the manner in which Descartes and others discussed music in the seventeenth century. With the point of departure mainly in van Wymeersch 1999 and Moreno 2004, one could argue whether Fétis really represents a critical turning point for “modern” music theory. This is not the place to develop a critical discussion on this matter, but if one were to do so, one should have to hold tongue in cheek, keeping in mind the difference between music aesthetics and music theory, between music psychology and music theory, etc., i.e., being more exact in the explicit delimitation of music theory than I am in this article.
the first to be dethroned in the radical development of geometry during the nineteenth century. Nagel (1939) relates this history, first describing how the development of projective geometry as a systematic study begun in the seventeenth century, especially in the work of Desargues, was delayed (or seems to have been delayed) by the invention of coordinate geometry by Descartes.\footnote{See also Kline 1972, 285–301, 834–60.} The revival of projective geometry in the early nineteenth century soon led to an awareness of a crucial difference between “two distinct classes” among the different theories, which constitute the science of extension … There are, in the first place, certain theorems which depend essentially on metrical relations found to exist between the different parts of extension which one studies, and which, consequently, can be proved only by the aid of the principles of algebra. On the other hand, there are others which are in fact independent of these relations, and follow simply from the positions which the geometric entities upon which we reason have to one another.\footnote{Originally appearing in an article by Gergonne, Annales de Mathématiques 16 (1826), 209, quoted in Nagel 1939, 148.}

The role of von Staudt is noted in Nagel 1939, but even clearer characterized by Kline (1972, 850–1):

By 1850, the general concepts and goals of projective geometry as distinguished from Euclidean geometry were clear; nevertheless the logical relationship of the two geometries was not clarified. The concept of length was used in projective geometry from Desargues to Chasles. … Yet length is not a projective concept because it is not invariant under projective transformation. … von Staudt … decided to free projective geometry from dependence of length and congruence. The essence of his plan, presented in his Geometrie der Lage (Geometry of Position, 1847), was to introduce an analogue of length on a projective basis. … The principal contribution of von Staudt in his Geometrie der Lage was to show that projective geometry is indeed more fundamental than Euclidean. Its concepts are logically antecedent.

The projective relations between the different forms of the twelve-tone set are logically independent from the assumption of equal temperament, that is, the former does not imply the latter. Physically, the twelve-tone space might just as well be defined in terms of unequal frequency relations between adjacent elements in the basic scale, or, more radically, just as an ordering of separate points in the frequency spectrum, without any measuring of the distances between these points separate from the one defined by the actual ordering.

It is not transparent (to me) why Babbitt and others assumes equal temperament. But the consequence is nonetheless to restrict music theory to studying “spatial transformations” under the restrictions implied by a meas-
asurement of distance between the elements on the one hand separate from—at least in principle—and supplementing the one defined by the scalar order, but on the other hand calculated on, if not identical with, traditional physical conceptions of intervals. The exploration of other kinds of transformations has just become a potential central concern, through the work of David Lewin, for example.\footnote{In Lewin 1987 the notion of interval includes cases that could be interpreted as concerning distances in terms of order relations only. Lewin’s transformational theory clears the ground for the introduction of transformations that, in contrast to, say, the transformations of classical twelve-tone theory, do not imply an absolute commitment to the notion of interval. Note also the posthumously published Lewin 2004, explicitly inspired by projective geometry. For a clear conception of the principle of separability of two fundamentally different ways of reasoning in much music theory, see Cohn 1997, 59: “The responsiveness of the Tonnetz, designed to model acoustic relationships, to a group-theoretic model potentially furnishes a lever for prying apart the acoustic from the group-theoretic aspects of triadic progressions, and for exploring the cohabitation of a nascent and tacit group-theoretic perspective with an explicitly acoustic one in nineteenth-century harmony.” What is at stake here is on the one hand a separation of acoustics and structure which can take place even in non-mathematized music theory, and on the other hand a near illustration of the autonomy of mathematics inside modern music theory (in this case the autonomy of group theory) from mathematics inside acoustic theory.}

Modern mathematized music theory, science, and aesthetics

The shift from a Pythagorean to a physical theory of consonance–dissonance, etc., is part of the shift one can find in science ca. 1600. If one talks just about mathematics and mathematized science, the Pythagorean–Platonic conception has three implications: that each number has a quality of its own, an inborn essence, that relations between numbers \textit{per se} answer questions of balance or harmony, and that the relations between different parts of the Universe essentially are of the form of likeness.\footnote{See van Wybmeres 1999, 17–31. For a discussion of numbers as bearers of essences in a critical comparison with mathematical structuralism, see Shapiro 1997, 72 f.} All these aspects are still found in Zarlino’s \textit{Le istitutioni harmoniche} (1965 [1558]), the first two respectively in the notion of the \textit{senario}, which “contains” the harmonic numbers, and in the conception that the diverse ratios between harmonic numbers determine the “most” consonant intervals. The physical theory instead tends to explain the consonance or dissonance of an interval in terms of the effect of this interval on the body and on the mind. The model is one of simplicity versus complexity, or regularity versus irregularity.

The relations of the modern mathematization of music theory to science and ideals of scientificity are not so easy to disentangle and characterize. Authors like Milton Babbitt (1961a and 1965) and Benjamin Boretz (1995 [1969–] and 2003 [1970]) in the 1960s and early 1970s did indeed invoke, and were quite well-versed in positivistic, and to some extent post-positivistic, philosophy. They certainly also exposed momentarily an ideali-
zation of the scientist as a model for the composer (in this respect, see also Babbitt 1958) or even the listener.

Still, these clear statements are not representative even of the works of these two authors. To confirm this impression of marginality, the quite heated debate that took place in the Journal of Music Theory 1989–90 certainly indicated that music theory in general is very far from subscribing to a scientific standpoint or scientific identity, at least in an ordinary sense. Unlike some critics of an anti-positivistic or anti-scientistic stance, Matthew Brown and Douglas J. Dempster (1989 and 1990) did not accept the description of authors like Boretz circa 1970 or John Rahn in the earlier part of his career as really representing a scientificity inside music theory. Challenged by the somewhat narrow image Brown and Dempster gave of a possible scientificity in music theory, neither Boretz nor Rahn defended their own oeuvre as representing scientism after all; instead they invoked “musical sense-making” (Boretz 1989, 113), or “propagation of musical culture and the enrichment of musical taste” as the “raison d’etre” of “analytically oriented theories” (Rahn 1989, 151).

This debate didn’t specifically concern mathematized music theory. The ideological implications of early music theory, and its development in later ages, were to a large extent bound up with the mathematics used. In the application of diverse modern theories and techniques of mathematics, often modelled on applications in scientific fields, the modern mathematization of music theory has a potential for a high level of “descriptivity,” which if it were realized would make it very much like a science. The crux of the matter is that this potential so often is left unrealized. The modern mathematization, possible to relate to the “pure” experience of music as phenomenally given sound, is for the most part not coupled to a scientistic world conception or a scientific aim of study. And this seems to sharply distinguish it from physicalistic, acoustic theory.

In the remainder of this article, I will consider certain aspects of this modern application of mathematics in relation to modernist theorizing about music during the twentieth century. In this context, I will take it for granted that mathematized music theory, as it has developed during the twentieth century, has to a great extent, though not exclusively, been an asset of musical modernism. Many of the aspects of this particular mathematical music theory have their ultimate ground, not in mathematics per se, nor in adequate

23 Davis (1993) has to my knowledge given the most extensive critical assessment of “positivism” in music theory. His in-depth discussion is concerned with Allen Forte’s theorizing, rather than Babbitt’s or Boretz’s. This is a bit idiosyncratic, given the lack of outspoken epistemological or ontological standpoints on the part of Forte. And its philosophical references in the role of positivists are mainly to Frege, Russell, and Wittgenstein, rather than to Carnap, Nelson Goodman, Tarski, or Hempel, the philosophers who, superficially at least, seem most relevant for a discussion of Babbitt and especially Boretz. The short comments in Nattiez 1990, 166 f. concerning Boretz 1995 [1969–] and Babbitt 1965 should be considered anti-positivist but not anti-scientist.
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positivistic or, more generally, scientistic paradigms of mathematical science, but in ideological aesthetic standpoints and doctrines—in standpoints and evaluations concerning the (modernist) artist and her position in the world, in standpoints and evaluations concerning what makes for “good art,” and other questions of this kind. It perhaps should be of no surprise that modern mathematized music theory, so close to the phenomenal and thereby phenomenalistic conceptions, should also be close to aesthetics.

In articulating these standpoints and doctrines mathematics has in fact had different roles. In this article, I will just discuss some of the earlier manifestations of such “ideological mathematics.” One role has been as an instance or an example giving legitimacy to a modernist project through perceived analogy (Krenek, for example). Another role has been as a model for a concrete articulation of an aesthetic standpoint that per se is not related to or sprung from mathematics (Babbitt, for example).

Concluding note on physicalism

The foregoing attempt at tracing a modern mathematization of music theory might seem counterintuitive. As many readers of this text know, an extensive theoretical literature has appeared since WWII in which one names the musical events directly in terms derived from physical theory, like Herz, decibel, and seconds. This is true of much electronic music,24 of much theorizing emanating from Iannis Xenakis, and even among the Princeton writers one finds such strategies, most notably perhaps in Kassler 1967. Often this mirrors a compositional practice, of course, like the handling of frequency generators. But the naming of the basic elements in terms of frequencies instead of tone names doesn’t mean anything when it comes to the actual practice and the implications of “modern mathematization.” Take Gottfried Michael Koenig’s discussion of serial music, for example, and the habit of seeing the series as a sequence of numbers. In the concept of the series,

\[ \text{[m]aterial and means are interlocked. As a sequence of numbers, the series functions as substitute, and as such it is blind for its meaning: e.g., the series } 3 2 5 1 6 4 \text{ can denote pitches or durations, etc. As it has to be varied constantly, and in exactly defined ways, even a series conceived as a sequence of pitches or durations must be substituted for by sequences of numbers … such a series, conceived as a sequence of numbers, says nothing about the actual acoustical processes, that they are meant to regulate … If, e.g., the chromatic scale is represented by the numbers 1 to 12, then these will match the former in the sense of an approximate comparison of size, but not as a mathematical expression of the semitones; that would have had to be } 1, \sqrt[12]{2}, \sqrt[12]{2^2}, \sqrt[12]{2}, \text{ etc., until } (\sqrt[12]{2})^{11}; \text{ or in decimal fractions: } 1, 1.059, 1.122, 1.189, \text{ etc., until } 1.888. \ldots \text{ Consequently the series will in most cases not be} \]

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24 See, for example, Stockhausen 1964 [1954] and Koenig 1991 [1958].
used in a mathematical sense, i.e., as a denomination of proportions, but as a
denomination of the sequence.²⁵

Koenig’s statement in the last sentence need to be qualified: If the “series” is
used as a sequence to be permuted, this game of permutations is mathemati-
cal on the same footing, but with a different aim and method, as the measur-
ing of intervals, etc.

Mathematics as a role model for modernism:
Krenek and axiomatics

In a series of lectures held in Vienna in 1936, and published in German the
following year, the Austrian composer Ernst Krenek presented what might
be considered a manifesto of modernistic composition. As Krenek notes in
the foreword, the lectures emanate from a long series of conversations with
Theodor W. Adorno (Krenek 1937, 3). Many themes known from Adorno
appear in Krenek’s lectures, and to a large extent Krenek shares Adorno’s
standpoints, with small but telling differences. Krenek perceives a relation
between society and music mediated through musical Form, and founded on
Truth, rather than Beauty, as a qualitative criterion:

In this the proper and real relation between music and sociology is enshrined:
Taken by radical expressionism to its most extreme constructive conse-
quence, in the abolishment of old forms, the new music expresses in com-
plete truth the condition of society. Alienation is the distinguishing charac-
teristic of this state: Alienation as the fate of music.²⁶

Krenek immediately distances himself from a Marxist interpretation of these
sentences, and thereby from Adorno. For the subject we are discussing here,
there is a more immediately important difference in that Krenek has no ap-

die Reihe Substitut und gegen ihre Bedeutung blind: die Reihe 3 2 5 1 6 4 etwa kann Ton-
höhen bezeichnen oder Dauern usw. Da sie wiederholt und nach genauen Definitionen variiert
werden soll, müssen selbst als Ton- oder Dauerfolgen konzipierte Reihen durch Zahlenfolgen
ersetzt werden. … Solche Reihen, wenn sie als Zahlenfolgen konzipiert werden, besagen von
sich aus nichts über die tatsächlichen akustischen Ereignisse, die mit ihnen geregelt werden
sollen. … Wird zum Beispiel die chromatische Tonleiter mit den Ziffern 1 bis 12 versehen,
entspreche sie ihr zwar im Sinn eines ungeführten Größenvergleichs, nicht aber als mathemati-
sche Ausdrücke der Halbtöne; es müßte heißen 1, 1·2, 6·2, 4·2 usw. bis (1·2)¹·; oder in
Dezimalbrüchen: 1, 1,059, 1,122, 1,189 usw. bis 1,888. … In den meisten Fällen wird die
Reihe indes nicht im mathematischen Sinn, d.h. als Bezeichnung von Verhältnisgrößen ver-
wendet, sondern zur Bezeichnung der Reihenfolge” (Koenig 1991 [1958], 48 f.).
²⁶“Hier liegt die eigentliche und reale Verbindung von Musik und Soziologie beschlossen: Die neue Musik, durch ihren radikalen Expressionismus zur äußersten konstruktiven Konse-
quenz getrieben, spricht in der Aufhebung der alten Form mit voller Wahrhaftigkeit den Zu-
stand der Gesellschaft aus. Die Entfremdung ist das charakteristische Kennzeichen dieses Zustandes; die Entfremdung als Schicksal der Musik” (Krenek 1937, 92).
prehensions about twelve-tone music, but sees it as a direct continuation of early expressionism.\footnote{For that part of the discussion between Adorno and Krenek, which ensued during the period 1929–1932, see Paddison 1993, 81–97.} His lectures are above all concerned with twelve-tone music.

In direct relation to twelve-tone music there is a discussion of a practical application of mathematics in music theory. Krenek discusses the problem of determining how many all-interval, twelve-tone rows are possible, and notes that it is a mathematical problem concerning permutations (1937, 73). He then relates European studies dealing with the all-interval rows, focusing on the question of the total number of all-interval rows, but without reaching a conclusion.\footnote{A few years later, a solution is presented in Babbitt 1946 chapter two, 47 ff., though the term discussed in that book is the all-interval set.} This is a problem in the tradition of Mersenne’s old question of the sum total of possible melodies, given a collection of tones. Krenek duly notes the difference between this type of mathematical reasoning and a physicalistic one. He then twists this distinction towards a distinction between a natural—or physical—and a mental use of mathematics, in a deeper sense, in relation to music.

Underlying Krenek’s distinction lies a conception of Nature as deterministic and \textit{Geist} as free, that is, a conception close to Kant’s \textit{Critiques}. He pretends that studies of the physical properties of the tone aim at domesticating it, while the “free creation of the mind uses mathematical means to \textit{arrange} regularities in its chosen material.”\footnote{“die Thesis, die freie Schöpfung des souveränen Gedankens, bedient sich der mathematischen Mittel, um Gesetzlichkeiten in dem von ihr gewählten Material anzurichten” (Krenek 1937, 80, my italics).} And he further specifies that

\begin{quote}
[our] discussion of the relation between mathematics and music has as its point of departure that the physical properties of the tones … at least in the actual advanced state of development of Western Music, and for the artistic aims of this state of development, are of less importance; that the tone doesn’t interest us as a product of nature, but as a symbol for the spiritual reality, meaning that what has to happen in relation to it will not be a result of its given properties, but rather how the mind disposes of it.\footnote{“Unsere Erhebungen über den Zusammenhang von Mathematik und Musik gehen … davon aus, daß die physikalische Beschaffenheit der Töne … zumindest in dem heutigen fortgeschrittenen Stand der Entwicklung der abendländischen Musik, für die künstlerischen Aufgaben, die dieses Entwicklungsstadium stellt, von untergeordneter Bedeutung ist, daß der Ton uns nicht als Naturprodukt, sondern als Symbol der geistigen Realität interessiert, so daß, was mit ihm zu geschehen hat, nicht aus seiner wie immer gegebenen Beschaffenheit, sondern aus dem resultiert, was der Geist über ihn verfügt” (ibid.).}
\end{quote}

This opens up a rapprochement with mathematics, not as a technique, but as an epistemology and an ontology, a Way of Thinking and a Way of Being. The mathematician becomes a model for the musician, or more precisely the
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composer—and the modern mathematician becomes a model for the modern composer.

The role model of the (modern) mathematician for the (modern) composer, distinguished by Krenek, is centered on what in Mathematics is called Axiomatics. Krenek initially talks about Arithmetics as well as Geometry, but in the development of his thought he relates to just Geometry. It is the development of Geometry during the nineteenth century that has created the mathematical thinking and the mathematical thinker that could serve as an ideal for the modern composer.

In very general terms, the development of geometry discussed implies that the mathematical structure has become freestanding, that it is studied not as a description of the structure of Real Space but as a totally abstract relational system. This affects the status and interpretation of the Axioms. In the traditional interpretation axioms were self-evident truths about real space. In Kritik der reinen Vernunft Kant made what he considered to be a radical reduction of this ambition, declaring that Axioms are self-evident truths, not about space in itself, but about the way—the one and only way—human beings are thinking (about) space. Still, axioms were bound to a single, as true considered conception of Space. That other geometries, founded on alternative systems of axioms, were conceivable was an insight of the nineteenth century. As one did not yet question the validity of Euclidean Geometry as a description of the structure of actual space, this meant a cleavage between pure and applied mathematics. As a corollary to this cleavage, one finds a rethinking of the rationality of the freestanding mathematical structure. At the turn of 1900 Poincaré in France and Hilbert in Germany made coherent expositions of this new perspective. Nagel (1939, 211) characterizes Poincaré as embracing an explicit conventionalism. This ranges over conceptions relating both to the mathematical language and the sensory perception. First, the latter, or more precisely “the materials of sensory intuition,” “do not present themselves as logically ordered and as sharply demarcated” (212). Secondly, the terms of the axioms of pure geometry have no meaning beyond the theoretical system in which they are used, “A ‘point,’ for example, is anything which satisfies the conditions required of points by the axioms, so that nothing which fails to satisfy these conditions can be a point, and nothing can fail to be a point if it does satisfy them” (213). That is, neither intuition, nor the empirically given, are arbiters of

31 For the expression “freestanding structure” and its interpretation, see Shapiro 1997, where it can be found on many occasions. Given our present historical interest, see especially pp. 157–63.
32 A thorough critical discussion of Kant’s conception of the synthetic a priori in geometry, giving three possible interpretations of its meaning, can be found in Coffa 1991, 43–7.
33 In Grassmann’s Ausdehnungslehre (1844), the author distinguishes formal from real science, he distinguishes “between pure mathematics as the science of forms in general, and applied mathematics such as geometry conceived as the science of space” (quoted in Nagel 1939, 169).
truth. Different geometrical systems, Euclidean or Non-Euclidean, might be correlated to more or less the same physical configurations. (This doesn’t make Poincaré a relativist in a strong sense, but the criteria for choosing one theory over another are “simplicity” and “convenience,” rather than “truth” (215).)

Hilbert is the sole author to whom Krenek refers in relation to axiomatics, and then he refers to just one title, *Grundlagen der Geometrie*, first published in 1899. This book is a formulation of classical Euclidean geometry in terms answering to a new kind of rigor concerning the axioms. And this rigor is delimited by, among other things, the conception of axioms as implicit definitions immanent to a theoretical system, rather than intuitive truths about physical space or the human capacity to apprehend space.\(^{34}\) An extended citation from Krenek’s text is motivated in this case. He cites from the first paragraph in Hilbert’s book, and then comments:

Says Hilbert: “We do imagine three different systems of objects: the objects of the first system we do name points…; the objects of the second system we do name straight lines…; the objects of the third system we do name surfaces … We think of points, straight lines, and surfaces as having internal relations in terms of ‘lying,’ ‘between,’ ‘parallel,’ ‘congruent,’ ‘continuous’; the precise, and for mathematical aims complete, description of these relations follows from the axioms of geometry.” The importance of these unobtrusive sentences is that the axioms are not seen as expressions of unprovable, but eternal, naturally given truths, truths that cannot be proved for the reason that the human intelligence can’t penetrate the depths of olden wisdom—on the other hand it does not even need to do so, as it has the same origin [as the axioms], and so has the capability to see the axioms as evident; but that the axioms [are seen] are free postulates of the human mind, created with the aim to make geometry possible.\(^{35}\)

The rather long detour that ends with the evocation of evident insight is most certainly directed against a Kantian conception of (geometrical) axioms. And

\(^{34}\) See Shapiro 1997, 130 n. 15 for a discussion of two different meanings of “implicit definition.” Coffa (1991, 128–37) and Shapiro (1997, 152–70) both relate a pair of early debates on the question of how to interpret axioms, with Poincaré and Hilbert in one corner, and Russell and Frege in the other.

for his own aims the most essential quality Krenek distinguishes in Hilbert’s
text is that it implies freedom of mind (Geist):36

Those who have followed our way of thinking are in possession of the key to
the practical application of this mathematical notion. We do not demand any-
thing else of the organizational development of a field of music, other than
what has for a long time been true for geometrical systems: their dependence
on certain, not natural, but mind-dependent conditions, definitions and axi-
oms of a musical sort. These are not eternal, but, however they are stipulated,
they have defined consequences, dependent on the actual conditions. That
this conception is useful in relation to music is a consequence of our doctrine
of musical thought, which in a crucial way influences all our contemplations
and conclusions.37

The simple point is that there are no transhistorically valid criteria con-
cerning the optimal organization of music, or, more literally and more pre-
cisely, that Nature doesn’t furnish criteria for “the organization” of music,
and that Mind is free to construct diverse musics as long as these construc-
tions are systematic consequences of certain freely chosen presuppositions
(“axioms”).

One can note that the use of mathematics in this case is wholly ideolo-

gical—Krenek does not pretend that it is through the actual use of mathematics
that the composer acquires freedom, only that the activity of composition is
like mathematical activity in its freedom. And he gives a hint of where to
start to seek if one wants to deepen the picture of this likeness—it is in the
“doctrine of music as thought.”

Krenek points at Schoenberg and his students as the source for his own
use of the term “musical thought” (musikalischer Gedanke), but he also
clarifies that the conceptual development is his own. In the tradition of

36 The theme of the relation between Geist and Nature reoccurs in later writings of Krenek, as
well as in the thinking around serial music in Germany, cf. Grant’s study of serialism, concen-
trated on Die Reihe: “Behind Stuckenschmidt’s essay ... there lies a model of history which
recurs in the essays written by Eimert and Krenek for the same volume [die Reihe 1, 1955].
This is the dialectic between nature and Geist, a progressive model of the latter’s domination
by the former which suffuses the whole tradition of Hegelian and consequently Marxist
thought. Both Stuckenschmidt and Krenek express music’s evolution towards the realm of the
pure Geist—which is, in effect, freedom from natural boundaries—in terms of the evolution
of its means of production” (Grant 2001, 81).

37 “Wer unserem Gedankengang gefolgt ist, hat den Schlüssel zur Nutzanwendung dieser
mathematischen Auffassung bereits in der Hand. Was wir für die organisatorische Durchbil-
dung eines tonsprachlichen Bereichs fordern, ist nichts anderes, als was für die geometrischen
Systeme längst erkannt ist: ihre Abhängigkeit von gewissen, nicht naturbestimmten, sondern
geistbestimmten Voraussetzungen, von Definitionen und Axiomen musikalischer Art, die
keineswegs ein für allemal feststehen, sondern, wie immer sie festgesetzt werden, bestimmte,
je doch von diesen jeweiligen Festsetzung abhängige Konsequenzen haben. Daß diese Auffas-
sung auf die Musik anwendbar ist, folgt aus unserer Lehre vom musikalischen Gedanken, die
alle unsere Betrachtungen und Folgerungen entscheidend beeinflußt” (Krenek 1937, 82).
Hanslick and formalism—but without referring to Hanslick—he denotes by a musical thought such a one, that exclusively belongs to the domain of music, that can only be presented by musical means, that is identical with its presentation in the material of music, with its musical figure, that can’t be separated from it nor be found, denoted, or defined outside the musical domain, not even be denoted by the means of verbal language.38

Krenek makes further distinctions that are not of importance for the present discussion; let us just note that, differently from Hanslick for which Idee and Gedanke seems to be synonymous, Krenek sees the Idea as an overarching concept, “possible to identify with the total conception of the work,”39 the top layer of musical creation communicating with the outer world, a layer with undefined (unclear) and general musical representations. But then, Krenek, like Hanslick, asserts that the actual object for investigation is the musical thought as it presents itself for us in audition.

In a central part of his text, Krenek describes the relation between the thinking subject and “the acoustical material” (das akustische Material). The presumed structure is one of a primary autonomous thinking for which the material just is an organ for making itself manifest, for embodying itself.

to us the most important moment in the consideration of the language of music is the notion that this isn’t a given unambiguous system, founded on the acoustical material and possible to explain in terms of its natural dispositions, but rather an organ and even the original creation of free, sovereign thought. According to this conception, the elements constituting a musical language have no special constructive power of their own.40

It ought to be in this radical constructivism allocated to Mind that Krenek wanted to see a likeness with mathematics. In another part of his book, he came to discuss the relation between different “activities of the Mind” (Geistestätigkeiten). The actual problem is the relation between theoretical investigation and real music, but his reasoning aims at generality:

38 “einen solchen, der ausschließlich dem Bereich der Musik angehört, nur mit musikalischen Mitteln dargestellt werden kann, der mit seiner Darstellung im musikalischen Material, mit seiner tonsprachlichen Figur identisch ist, von ihr nicht abgetrennt werden und außerhalb des musikalischen Bereichs nicht angetroffen, bezeichnet, definiert, ja mit den Mitteln der Wortsprache nicht einmal benannt werden kann” (Krenek 1937, 26).
39 “Man kann sie etwa mit der Gezamtkonzeption des Werkes identifizieren” (Krenek 1937, 29 f.).
40 “Das für uns wichtigste Moment bei der Betrachtung der Tonsprache ist die Auffassung, daß diese kein an sich gegebenes, im akustischen Material gelegenes und aus dessen Naturbe- schaffenheit heraus erklärtbares, eindeutiges System, sondern daß sie ein Organ und zugleich die erste Schöpfung des freien, souveränen Gedankens ist. Die Elemente, die eine Tonsprache konstituieren, sind nach dieser Auffassung noch keine solchen von besonderer konstruktiver Eigenkraft” (Krenek 1937, 29 f.).
consequently, what I do understand by the unity of human activities of the mind is not exhausted in symbolic character or expressive relations, but signifies an agreement in inner essence, close to identity. Thereby, the possibility to think about music depends on the fact that music itself is a form of thought, implying that the analytical process of thought used when we investigate music is wholly identical with the intuitive process of thought that emanates in the creation of [an] artwork.41

Given a conception of the near identity of different activities of mind, and given a belief in the creational potential of (musical) mind and its power to structure (musical) matter, Krenek seems to have missed an open opportunity to make mathematics an ally, not only in proclaiming musical structures free in relation to a pretended natural acoustic space, but also in addressing the even more fundamental question of the ontological status of musical objects. Krenek comes very close to a conception of music as a free creation of mind, a conception that had close analogues in mathematics, which were strongly propagated during the first half of the twentieth century. Of the three schools of philosophical thinking about mathematics that were influential during this period—logicism, formalism, and intuitionism—Krenek came to invoke formalism in the shape of Hilbert’s Grundlagen der Geometrie. This partnership is not without its problems. In mathematical formalism, mathematics is seen ideally as a game functioning autonomously in terms of its own rules. These games certainly have autonomy in relation to physical reality (nature), but this autonomy is not located in a free-thinking Mind, but in the clearly delineated and ramified potential of humans to handle symbols in a rule-bound fashion considered primarily in terms of consistency. This did not prevent Hilbert from seeing the mathematician as having great freedom in handling these signs, thereby having a great freedom in the creation of axiomatic systems. And so, formalism certainly could function well as an ideological model for modernist compositional activity, at least in the case of twelve-tone music. But true formalists would not, in the way Krenek did, have invoked the notion of Mind, which goes hand in hand with the notion of intuition. And the severing of mathematical reasoning from intuition was a central concern of formalism, as it also had been of much mathematics earlier in the nineteenth century.42

41 “Was ich unter der Einheitlichkeit menschlicher Geistestätigkeiten verstehe, ist also nicht in Symbolhaftigkeit oder Ausdrucksrelation erschöpft, sondern es bedeutet eine das innerste Wesen betreffende Entsprechung, ja geradezu Identität. Die Möglichkeit des Denkens über Musik beruht demnach darauf, daß die Musik selbst eine Form des Denkens ist, so daß der analytische Denkvorgang, in welchem wir das Musikalische untersuchen, völlig identisch ist mit dem intuitiven Denkvorgang, der zur Herstellung des Kunstwerkes führt” (Krenek 1937, 19).
42 See for example Detlefsen 2005. Jacquette (2002, 3) characterizes formalism together with realism as “in full agreement that we must try to make sense of mathematics as a mind-independent study of real things and their properties … A formalist … can be every bit as […] much a realist as the Platonist.”
To combine formalist axiomatics with the sphere of Ideas, the existence of a free thinking Mind and other notions in the same vein was perhaps just an eclectic inconsistency on Krenek’s part. Even so, in a sense it would not have been surprising had Krenek’s reference inside mathematics and mathematical philosophy been intuitionism, rather than formalism. It is not so much the “technical” aspects of intuitionism, or its differences from formalism in this respect, as the ideological clothing that seems to be very close to his position:43

The intuitionist mathematician proposes to do mathematics as a natural function of his intellect, as a free, vital activity of thought. For him, mathematics is a production of the human mind. He uses language, both natural and formalized, only for communicating thoughts, i.e., to get others or himself to follow his own mathematical ideas. Such a linguistic accompaniment is not a representation of mathematics; still less is it mathematics itself (Heyting 1983 [1931], 52 f.).

As it goes, Krenek’s lectures seems to be uneasily stretched between an ideological invocation of axiomatics in a formalist variant, with the rather plain function to legitimate a non-tonal practice of composition, and an individualistic ideology of creative freedom, closely related to those espoused by Busoni (1916),44 by Schoenberg (1984 [1911]), elusively already by Debussy (1987 [1901]), and by other radical modernist tracts in the same vein—none of them mathematical in aim, and Schoenberg outspokenly anti-mathematical.

The axiomatics and related notions, have, in a similar fashion and without invoking the extremely individualistic version of modernism figuring in Krenek’s text, been used to legitimate a certain modernistic activity through invocations of an affinity with mathematics and/or science. Even earlier than in Krenek, one finds this strategy in an article by Henry Cowell (2002 [1929], 249), where the references are to “non-Euclidean geometry and Einstein’s physically demonstrable theories,” which serve as rhetoric models for a plea for the possibility or the legitimacy of “a non-Bachian counterpoint, a non-Beethovenian harmony, or even a non-Debussian atmosphere, and a non-Schoenbergenian atonality.” But there are also telling differences: Cowell seems to think of the history of musical systems as (potentially at least) parallel to an evolutionary conception of science. This is in line with his argumentation in Cowell 1930, a plea for a radical music based on the overtone series.

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43 Some thirty years later, Boretz (1995 [1969–]), likewise interpreting music as thought, came pretty close to a standpoint blueprinted on mathematical intuitionism in his Metavariations.
44 Cf. Grant 2001, 81 in close connection with the passage earlier cited from that book: “Freedom had been, since and perhaps because of Busoni, the accepted goal of electronic music.”
References to axiomatics and related conceptions also are found in Boulez’s writings. As is true for his generation of serial European composers in general, Boulez has been rather reticent in invoking mathematics and science in the discussion of music, his own music as well as that of others. The majority of references are in music or the fine arts. Still, in Boulez 1995 [1954] and 1963, there are some isolated discussions of possible parallels between music and philosophy, logic, mathematics, or science. As shown by François Nicolas, the references in the latter text do not manifest any deeper knowledge of or interest in the subjects on the part of Boulez,\(^{45}\) still the latter manages, with the help of his “superficial” readings, to invoke some of the themes found in Krenek: a conception of a certain freedom of mind in relation to nature, the need for a systemacticity in the conception of music (musical systems) founded in something like a musical axiomatics. As with Cowell, there is a strong evolutionist element in Boulez’s thinking, manifest in Boulez 1995 [1954], but the backdrop here might be Adorno’s conception of the “tendency of the musical material,” rather than a notion of the way science develops.\(^{46}\)

In the meantime, between the appearance of Krenek’s lectures and Boulez’s flirtations with axiomatics and the like, a much more consequent application of mathematics, and in a form that one in principle could liken to an axiomatic one, had begun to take form in the United States.

**Quasi-Aristotelian and unitary conceptions in Babbitt’s early theorizing**

The theoretical work of the American composer Milton Babbitt seems to live up to his own characterization of the string quartets of Béla Bartók, that they display a “homogeneity and consistent single-mindedness” (Babbitt 1949, 1). His theoretical and analytical texts seemingly have an enduring style, and also enduring aims—the latter not least in the persistent loyalty to, or even identification with, the twelve-tone-system, the most crucial characteristics of which already were identified in the middle of the forties. At the same time, there are changes in perspective, uncommented on by the author himself, but nonetheless obvious and probably significant.

So, the earlier writings are more programmatic in character, they are evaluative and prescriptive. The latter writings in general take the form of music analysis, describing existing twelve-tone music, with a quite elusive strain of evaluation in selected instances—as for example when, at the end of

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\(^{45}\) See Nicolas 2005 for an extended discussion of what he calls “Boulez’ musical intellectualitiy.” An interesting finding of his is that the references to actual mathematicians, scientists, etc., in Boulez 1963 are mainly related to two isolated issues of a semi-popular journal, both of which contained articles on music by Boulez, i.e., that their access on the part of Boulez seems to be rather fortuitous, see Nicolas 2005, 141 f. and 144 f.

\(^{46}\) See, for example, Adorno 1997, 38 ff. as well as Paddison 1993, 65–107.
an article on Stravinsky, Babbitt mentions the two alternatives for “the young” of using a very early Stravinsky or a very late Stravinsky as an example or inspiration for one’s own compositional work, clearly manifesting a preference for the latter alternative, representing a specific way of handling twelve tone music (Babbitt 1987, 423 ff.). But in a more critical reading one can find another change in focus. From about 1960, Babbitt’s “rhetorics” turn more decidedly towards scientism, not only in relation to the ways one talks about music in theory and analysis, but also in relation to actual compositional practice. This doesn’t prevent the possible survival throughout Babbitt’s career of a certain basic conception of the way theorizing is ideally done, but it certainly marks a shift in emphasis—Babbitt does begin talking about hypothetico-deductive strategies in relation to music theory early on, but the focus on the deductive end of the spectrum is much more pronounced in the beginning. And this goes hand in hand with the manifestation of certain aesthetic valuations and preferences in his critique, that shows up interplaying with the formal, more specifically mathematical, procedures in his theoretical work. This aspect becomes heavily subdued, though not extinct, under the latter part of Babbitt’s career.

Babbitt and scientism
In Babbitt 1961a, 1965, and 1972 one finds an epistemological program for music theory closely related to positivistic, post-positivistic, not to say analytic philosophy. In these writings, Babbitt is constantly alluding to a background of scientificity, and “non-nonsense,” stating on one occasion that “there is but one kind of language, one kind of method for verbal formulations of ‘concepts’ and the verbal analysis of such formulations: ‘scientific’ language and ‘scientific’ method” (Babbitt 1961a, 78). But in the last instance, Babbitt is not entering a detailed discussion of the characteristics of this language and this method. In the notes, however, there are scattered references to Carnap, Hempel, and Nelson Goodman, among others.47

The actual realization of a music theory, trying to show that it could live up the standards alluded to by Babbitt in his articles of the 1960s and 1970s, came in Boretz 1995 [1969–]. But one can also note a change in Babbitt’s own writings from about 1960. As noted, analysis of actual works now take pride of place, where earlier presentations of theoretical systems had been dominating. At the same time, Babbitt came to describe the role of the composer and the act of composing with the scientist as model, using key words as “experiment,” “hypothesis,” “research,” and the like (see Babbitt 1962, 109, for example). The value criterion seems to be the not further explicated

47 Rahn (2001 [1979]) positions the articles of Babbitt discussed here in the wider context of other music theoreticians of the period, also entering a discussion of Hempel’s “deductive-nomological” model of scientific explanation—which also was in focus in Brown and Dempster’s critical articles mentioned earlier.
“coherence,” and the means of evaluation is “listening.” The exact role of mathematics in this is not discussed in detail, but then there existed already in those days a vast literature on the subject of mathematics in science, a literature that had made clear that the question isn’t exactly easy to handle. In Babbitt 1965, one finds a catalogue of different conceptions of the character of theory (read, scientific theory), ending with the non-conclusive statement that “[s]urely there is no more crucial and critical issue in music today, no more central determinant of the climate of music today, than that of the admittedly complex and intricate problems associated with assertions about music” (191). The point here is that Babbitt exclusively refers to, and shows his orientation in, a tradition of discussing scientific theory and the role of mathematics in relation to that theory, in which logical positivism had been one phase during the second quarter of the twentieth century, and in which the notion of model had its heyday around 1960.48

The early phase
Of the more systematic theoretical writings on twelve-tone music by Babbitt, three come before the manifestation of “positivism” as a program in Babbitt 1961a. These three, together with the somewhat later Babbitt 1962, aim primarily at describing what Babbitt calls a “system” or aspects of a system, in abstraction from actual compositions. Starting with Babbitt 1961b, and except Babbitt 1962, the later of Babbitt’s theoretical writings are analytical, or at least have the outward shape of music analysis.

Reading the main theoretical articles actually published up to 1960 (that is, Babbitt 1955 and 1960), it would be hard to say what rationality they imply. But then, in a much earlier review from 1950, Babbitt talks about twelve-tone music, or possibly its system, as “the most influential hypothetico-deductive system in the history of music” (1950a, 12). This ought to indicate some kind of continuity in Babbit’s conception, after all, the hypothetico-deductive method being a way of apprehending scientific method that is very close to the positivistic movement.

How is one to interpret the statement that the twelve-tone system would be hypothetico-deductive? In a passage in Babbitt 1955, 41, the author describes his own achievement as having

its specific origins in the investigation of the implications of techniques of the “classics” of twelve-tone music. Indeed, it is in a principle that underlies the bulk of Schoenberg’s work (namely combinatoriality), and another, superficially unrelated, principle occupying a similar position in the music of Webern (derivation), that have each been generalized and extended far beyond their immediate functions, finally to the point where, in their most generalized form, they are found to be profoundly interrelated, and in these interrela-
tionships new properties and potentialities of the individual principles are revealed.

This is in all likelihood a description of part of what Babbitt would like to call a hypothetico-deductive work. But he gives no clue about the crucial question of how “new properties and potentialities of the individual principles” are to be tested empirically. The deductive end of the chain is not un-critical, either. If it is relevant to talk about a hypothetico-deductive method at work in Babbitt’s writings, his main focus is on this aspect. And the clearest example would be his earliest and most extended writing on the “twelve-tone system” (Babbitt 1946).\(^{49}\)

The rationality of the thesis of 1946

The general aim of Babbitt 1946 is stated as

> a tentative effort to set forth the compositional assumptions of the twelve-tone system in a rigorous statement, and to examine the implications of these assumptions as the basis for the construction of a deductive, yet applicable, theory of the system (i).

He makes the precision that it is not “an attempted axiomatization of the system” (Babbitt 1946, i). Differently from Krenek, for whom axiomatics has nothing but a relationship of analogy with composition and music theory, Babbitt sees a literal axiomatic treatment of twelve-tone theory as possible and foreseeable in the future.\(^{50}\)

In further contradistinction to Krenek, this time made explicit, Babbitt insists that dodecaphony should be seen as a system, rather than as a technique.\(^{51}\) Babbitt observes Krenek’s invocation of Hilbert’s foundations of geometry, and comes to the conclusion that Krenek has refuted himself. Hilbert was constructing a mathematical system, which merely means that he was defining elements, relations between them, and operations upon them. This is certainly what has been done in constructing the twelve-tone system, and thus, in any strict sense of the word, it is truly a sys-

\(^{49}\) The passage in Babbitt 1965, 191, in which he writes that “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 signification 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,” is an index of a continuity in Babbitt’s thinking on these matters, but also subtly shifts the focus from the system-building to the analysis of individual works.

\(^{50}\) Babbitt 1946, i. The rigorous demands of an axiomatic treatment had become part and parcel of mathematics during the first decades of the twentieth century. Closely following Babbitt himself (1946, i), these are about the definition of the primitives, the independence or completeness of the assumptions, and the strict application of a predetermined logical model. See also, for example, Kline 1972, 1026f.

tem. A technique implies something far narrower and superficial in the sense that it indicates an operational mode within a system (Babbitt 1946, viii).

Despite the confident tone, this passage is not wholly clear. In talking about “constructing the twelve-tone system,” and posing this construction on the same footing as the construction of a mathematical (which ought to mean a mathematically-theoretical) system, one wonders whether Babbitt aims at the construction of a systematic theory of twelve-tone music, or at the construction of some kind of musical reality embodying or exemplifying a system.

If one assumes, not unreasonably, that Babbitt was a constructivist in matters of mathematics, he might have taken for granted that mathematical systems are constructions of the mind. He might further, in line with Krenek before him and Boretz after him, have considered music to be a constructive enterprise closely related to, or even identical with, thought. Music being related to a system, interpreted as a product of thought, would then just be a natural outcome of, or a corollary to, music’s “essence,” likewise as thought. And given this, Babbitt might have been less impressed by the difference between the theory of the twelve-tone system and of twelve-tone music on the one hand, and the possible reality of a twelve-tone system “inside” musical practice on the other.

Closer to what is actually written, and deeming by a later article, Babbitt didn’t, in the actual case of twelve-tone music, see the distinction between music theory and musical reality as essential, given that the manifestation of the system in music, and a more or less adequate thinking (that is, a verbalization) about it, were realized more or less simultaneously:

the twelve-tone system, like any formal system whose abstract model is satisfactorily formulable, can be characterized completely by stating its elements, the stipulated relation or relations among these elements, and the defined operations upon the so-related elements. Such a characterization, though explicitly presented in verbal forms at the earliest stage of the twelve-tone development, is likewise easily and explicitly inferable as the maximum procedural intersection among the “classical” twelve-tone works of Schoenberg, Webern, and Berg (Babbitt 1960, 55 f., my italics).

That the system was realized through the emergence of twelve-tone music would be a non-controversial conclusion, as it ascribed a descriptive role in relation to a collection of systematicities and regularities forthcoming in a certain repertoire. But it is on this point that Babbitt, at least initially, lets

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52 Cf. the citation from Krenek 1937, 19, discussed earlier, and Boretz 1995 [1969–], 26: “music is among those domains in which the ‘objects’ of consideration are objects … only by special virtue of a singular disposition and observation of ‘real’ events by, respectively, an author and a perceiver. And thus the further question … arises concerning the respects in which these ‘objects of thought’ are also instances of thought. In other words, we are confronted with an experiential domain that is not only thought about but also, apparently, thought in.”
Aristotelism in on the scene. The formal-teleological conception concerns on the one hand the construction of the twelve-tone system, and on the other hand the relation between the twelve-tone system and actual composition. Concomitantly, Babbitt came to speak about the system as “pre-compositional.” The prefix “pre-” seems to have disappeared from Babbitt’s vocabulary rather early, but as late as in Babbitt 1961b, 86 one finds a an expression like “discovering … compositional consequences of set structure.”

The twelve-tone system
The twelve-tone system, as presented in Babbitt 1946, 4, has what the author calls “a pre-compositional principle,” the twelve-tone set. If the twelve-tone set consists of an ordering of the twelve pitch-classes of the equal-tempered chromatic scale, in Babbitt’s notation the result has the form a collection of twelve ordered number pairs, the first number in each pair representing order number in the set, the second the order number of a pitch-class in an equal-tempered chromatic scale (which in itself is a twelve-tone set, as Babbitt notes). Babbitt also distinguishes the succession of intervals between adjacent pitch-classes, and represents this by a succession of integers. An interval is treated as the difference between two numbers. Given this kind of mathematization, an interpretation of the interval concept as bearing upon relations of ordering, rather than physical distance, lies pretty close at hand.

The twelve-tone set is presented as the constructional outcome of “arranging all the integers representing the elements of the system according to the requirements of a simple linear ordering.” A set complex consists a collection of sets “generate[d] from any given set” such that the character of the complex “is uniquely determined by this set, and can be derived from no other set” (Babbitt 1946, 46). The generation in question is a mathematical way of representing the classical twelve-tone techniques of transposition, inversion, retrogression, and combinations of these, in the process referring

53 The literature on Aristotle and teleology is vast, and differences in interpretation abound. In our context, the essential point is that a teleological process has a goal or telos that is considered as being part of the essence of the object undergoing change. Thereby, the teleological process has an intrinsic and “necessary” character, though in fact the potentiality immanent in the object in its initial state isn’t necessarily actualized. The pretended necessity as well as the actual enchainment of a teleological process is possibly of markedly unclear character—though teleology is a crucial notion in modern biology, and as such has arrived at higher degrees of precision. In the actual case discussed here, I will use the word to denote the character of a process that is quite understandable in terms of its goal, as the “realization of an ideal,” but lacks a description of an unbroken chain of effective causation or deduction from what is pretended to be the initial principle. That is, a positivist of the old school would talk about “metaphysics.” To emphasize that a given teleological process is impersonal, that the telos is founded in the essence of the object rather than in an intention originating with the agent, I will occasionally use the term “formal-teleological process.”

54 Babbitt (1946, 2). One common way of expressing these requirements is that if \( a \leq b \) and \( b \leq a \), then \( a = b \) (antisymmetry); if \( a \leq b \) and \( b \leq c \), then \( a \leq c \) (transitivity); \( a \leq b \) or \( b \leq a \) (totality or completeness). Note that the requirements in Babbitt are somewhat differently formulated.
to mathematical concepts as operation, generator, translation, and transformation.

In fact, one could alternatively express the set complex as consisting of all the permutations of the pitch-classes of the source set that keep either the sequence or the inverted sequence (i.e., the retrograde) of the differences of two adjacent pitch-classes in the source set invariant, modulo-12, given multiplication of the resulting sequence by either 1 or -1.55Expressed in that way, the likeness with rotations and reflections in geometry is obvious. The set complex consists of all the permutations of the pitch-classes of the chromatic scale that keeps a given “shape” constant, through “transposition” (≈ rotation in geometry), through “inversion” (≈ reflection around the horizontal axis), or through “retrogression” (≈ reflection around the vertical axis), or through combinations of these.

The set complex doesn’t make up the whole system. Babbitt introduces two more (internally closely related) concepts, combinatoriality and derived set. Both of them describe relations between set segments. Combinatoriality, related to the compositional practice of Schoenberg, is used as a basis in formulating rather strict criteria for the simultaneous use of different sets of the set complex, with the intention not to violate the identity of the set complex, or more precisely the uniqueness of the set complex actually associated with a given composition (see Babbitt 1946, chapter III). The derived set, on the other hand, related to Webern’s typical way of constructing a row, is not in this way uniquely associated with a given set-complex. As will be evident later on, both these points are of central relevance for the subject matter of this article. However, as Babbitt’s references to combinatoriality are not altering my interpretation of his aesthetics, but rather corroborates it, I will restrain myself to a short comment on the rather problematical status of derived sets.56

In Babbitt 1946, many questions just hinted at, or even not posed, in earlier twelve-tone theory are discussed in a systematic way. These are partly questions arising because of the mathematical framework, as when it is shown in a mathematical fashion that every twelve-tone set “has associated with itself eleven non-equivalent transposed sets” (1946, 10)—equivalence meaning that the sequence of pitch-classes is exactly the same in both cases. In the same way, the somewhat more complex question of “the number of

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55 An example:

PCs P: 9 10 3 11 4 6 0 1 7 8 2 5 SIA: 1 -7 8 -7 2 -6 1 6 1 -6 3 = SIA (P)
PCs I: 2 1 8 0 7 5 11 10 4 3 9 6 SIA: -1 7 -8 7 -2 6 -1 -6 -1 -6 -3 = SIA (P) * (-1)
PCs R: 5 2 8 7 1 0 6 4 1 3 10 9 SIA: -3 6 -1 -6 -1 6 -2 7 8 7 -1 = SIA (P) * (-1) backwards
PCs RI: 6 9 3 4 1 0 1 5 7 0 8 1 2 SIA: 3 -6 1 6 1 -6 2 7 8 7 -1 = SIA (P) backwards
56 I will not enter a discussion of the references to and use of group theory in Babbitt, as this particular aspect is not vital to the discussion. For a succinct overview of the different group structures identified by Babbitt in twelve-tone music, see Fritts 1997.
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non-equivalent sets within a complex” is posed and it is shown “to be either 24 or 48” (1946, 45).

These questions concerning structure could be considered factual. But intermingled with them, there are questions of an altogether different character. Concerned with the twelve-tone set and the set complex Babbitt (1946, 46) writes in the concluding chapter:

We have thus progressed from the identity of set structure to that of set complex structure, and it is herein that the operational assumptions of the system justify themselves most conclusively, for the defined transformations and translations generate from any given set a complex whose character is uniquely determined by this set, and can be derived from no other set which is not operationally related to the initial set.

The question of justification implied here is not posed earlier in the thesis. It could be read as if one had solved a given mathematical problem. If so, why was this mathematical problem posed in the first place? Inside mathematics, and inside a thesis, it would probably have had an autonomous aim, to solve a hitherto unsolved question in mathematics, for example. In the context of Babbitt’s thesis, what is discussed is not pure, but applied mathematics, i.e., the aim ought not to be purely mathematical. Returning back to the generally stated aims of Babbitt 1946, i, these are “to set forth the compositional assumptions of the twelve-tone system in a rigorous statement, and to examine the implications of these assumptions as the basis for the construction of a deductive, yet applicational, theory of the system.”

Though perhaps not quite clearly stated, the assumptions related to it are on the one hand those presented as requirements on the ordering of the set, and on the other the actual choice of operations on a set. It might be the sum total of these that “justify themselves … conclusively” in generating “a complex whose character is uniquely determined by this set, and can be derived from no other set which is not operationally related to the initial set” (1946, 46). What Babbitt here hints at is that the union of all set complexes is partitioned by these, that is, no set complex overlaps with any other. In that sense every set complex has a unique identity, and this identity can be represented by any of the sets in the set complex, that is, any set can serve as the generator, the application of the operations of the system on which will create the whole complex.57

What is at stake under the name of identity seems to be what one otherwise in a musical context often speaks about as unity, a unity coming by

57 The derived sets are shared between set complexes, and in that sense disturbs the characteristics of the system. In defending their use, Babbitt came to refer to their pretended organic character: “We have justified the concept of the derived set on the basis that it derives its material completely from the set complex, and thus is organically related to the original set, and actually is nothing more than a presentation of the set itself in a segmented form” (Babbitt 1946, 149).
through a reflection of a part, an Origin, in every other part, and thereby in the Whole. On this matter, Babbitt represents a certain relativity, for no set has an ontological claim to be the prime set, every set being possibly a source set thereby representing the set complex.

If the assumptions of the system include the requirement of linear ordering on one hand, the choice of operations on this linear ordering on the other, the question arises why these particular operations were chosen. Babbitt gives no direct answer to this question, but his theory has a clear tendency, and the passage already cited twice gives evidence that Unitarianism, rather than being an effect of the system, really is a leading strategy in working it out. One could perhaps even say that it is an implicit axiom.

Babbitt and the quest for Unity

Babbitt’s quest for unity is manifest from his earliest writings, and it even takes different forms. In the article on the string quartets of Béla Bartók cited earlier (Babbitt 1949), Babbitt tries to show that an associative motivic relationship pervades each one of the six string quartets. The procedure is very close to the one that was to be systematically exposed in Rudolph Réti’s two major books some years later. Spoken philosophically, it is a kind of “motivism” or “thematicism” that from likeness in shape (between non-identical elements) wants to infer unity in Origin and unity in Purpose. Ruth Solie states that Réti “concentrates less upon the synchronic view of an individual organism, preferring metaphors of growth, development, and evolution” (Solie 1980, 152). Rereading Réti, I am not really convinced that this is to the point—rather one finds surprisingly little evolutionism and teleology there. But be that as it may, in Babbitt’s text there are, against a dominating background of simple associationism, some single spurs of an evolutionist or stage-oriented conception, as when he speaks of “continuous phases of association” (Babbitt 1949, 2) or suggests “Bartók’s formal conception emerges as the ultimate statement of relationships embodied in successive phases of musical growth” (Babbitt 1949, 7).

Schenker and Schenkerism open up another conception of unity, in that the Ursatz is reflected on different “structural” levels (for this choice of expression, see Babbitt 1952, 24), from a whole movement down to the simple phrase, all this possible to visualize as a treelike structure, with the characteristic that every node reflects all other nodes (all being Ursätze), that is, it is an overall design which in its details at the same time is realized as a unity of appearance, this appearance mirroring the overall design (a work as an

58 A few examples of conceptions of Unity and related notions, in temporal, ideological, and in one case even personal closeness to Babbitt: Unity and Complexity are among the main subjects for discussion in Beardsley 1958; a developed discussion of Coherence and Complexity in music can be found in Boretz 1995 [1969–], 94–7.
Ursatz from which in a treelike way springs forth Ursätze, and Ursätze from Ursätze, or a work as a treelike “hierarchy” of Ursätze).\(^5^9\)

But the writings here used as evidence both postdate the thesis of 1946. Returning to a passage earlier discussed, and expanding the citation, we will find a clear statement of a Schenkerian-like conception of unity:

> We will assume that the general desideratum of formal organization is that of the complete exploitation of the stated material in the construction of a whole, which is logically resultant from the basic material, in its characteristic compositional form. Form in triadic music is an expansion of triadic structure ... the formalisms of triadic music are but different modes of affirming the component elements of the tonic triad at varied levels of autonomy. The tremendous resources of triadic structure are a result of the number of progressive levels possible between the statement of microcosm and macrocosm, all of which reflect and develop their character; this makes possible the control of a large scale organization by a comparatively slight, delineated unit (Babbitt 1946, 151 f.).

From this passage, another central tenet in Babbitt’s early theorizing makes itself manifest, the tendency to see the musical system as a Source in a teleological process.

In a much more abstract reading of Babbitt (and Boretz) than the one presented here, Nattiez has in a very general way noted the inconsistencies in Babbitt’s theorizing, that in defining

> a musical theory as a hypothetical-deductive system, one might think that he [Babbitt] is operating in strict conformity with the epistemological exigencies of logical empiricism. But if we look closely at what he says, we quickly realize that the theory also seeks to legitimize a music yet to come; that is, that it is also normative (Nattiez 1990, 167).

Nattiez builds this characteristic on a single, rather late title of Babbitt.

> Being loyal to our focus on early Babbitt, one can note that what Babbitt tried in those days was to present composition as a teleological consequence of a “pre-compositional” system (either Schenkerian or twelve-tone, depending on repertoire). Characterizing this state of affairs in twelve-tone music, there are some deficiencies in the reasoning. In using terms like “implication,” “logic,” relatives and derivatives of these, Babbitt makes stronger demands on the argumentation than it can stand up to in a test. In such a strict sense as implied by logic, neither the system, nor composition follows from the stated initial assumptions concerning set construction. In Babbitt’s way of exposing things, the system results from the actual choices of operations

\(^5^9\) The question of unity and organicism in relation to the arts is often discussed inside and outside musicology. Organicism in Schenker is particularly focused on in Solie 1980, though many aspects of Schenkerian thought, its teleological character included, are more effectively pinpointed in Narmour 1977.
on the prime set. That there are criteria guiding these choices, and what these are, emerges only after the fact, and then only half spoken. In making these explicit, one finds, or recognizes, some aesthetical principles, concerning identity and uniqueness. And given these, one can possibly accept his compositional system as a way of realizing a certain ideal of unity in music under certain restrictions. But even so, to characterize this as a way to draw logical consequences from a few premises can’t be anything but a metaphorical way of speaking—which Babbitt himself seems to have been more or less conscious of, given that he admitted that his system was not yet an axiomatic system “and resulting makes very little effort to define the primitives, examine the independence or completeness of the assumptions, or to develop according to the procedures of any predetermined logical model” (Babbitt 1946, i). One can also note that the actual way of realizing unity through a play of likeness between the different elements of a universe—Babbitt even speaks of microcosm and macrocosm—associates Babbitt’s theorizing with the premodern, Pythagorean, Platonic, (and Aristotelian) world of a Zarlino.

At some distance, one can note that what Babbitt values in the twelve-tone system—which he both pretends to have found in a rudimentary form in history and claims to have developed—are certain aesthetic principles. And in the conjunction of these principles and the shape the system got in Babbitt’s hands one certainly finds a consequence. Speaking of “logic,” Babbitt could have started his argumentation one step earlier, with the criteria of value steering his whole enterprise. But then, it would have been clear that the “demands” and “implications” of the pre-compositional principle (the series), and the twelve-tone system really depends on the kind of attributive thinking typical of ideology. It is not a question of implications in a strictly logical sense—nor, for that matter, of mechanical causation—but of “realizations” of an Aim, with the efficient cause—the composer—and the final

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60 “The tremendous resources of triadic structure are a result of the number of progressive levels possible between the statement of microcosm and macrocosm, all of which reflect and develop their character” (Babbitt 1946, 152).

61 No comment has been made here on the relation between system and actual composition. Without entering a full discussion, let me note that there are some objections to be made against Babbitt’s procedures here. For example, by interpreting the ordering of the series as a temporal one (1946, 3), Babbitt, to allow for simultaneities, has to introduce what he calls a “compositional modification” (1946, 66). Had he made the system more abstract, without any temporalities involved, he could instead have chosen to see all compositional procedures related to the system—“melodic” and “harmonic” alike—as interpretations of the system, rather than, as what seems to be implied here, as “pure” and “distorted” consequences of the system. And through this formulation, I have hinted at the main objection: that he sees the relation between system and composition as one of logic, rather than as one of defining rules of translation between them. Had he introduced the latter interpretation, his position would have been more tenable, as well as more flexible, opening up diverse ways of compositional realizations of a system, even in terms of the general regularities between them. But then he would have been farther away from the teleological way of thinking in the Schenkerian tradition.
cause—the striving for Unity in Unicity—hidden behind a pretended essence or formal cause—the pre-compositional (serial) principle.

Schenkerism as a role model
It is possible to see Schenkerism as a model for Babbitt’s early theorizing about twelve-tone music in many respects:62 1) The distinction between a pre-compositional system and actual composition has a very close analogue in Felix Salzer’s distinction between a pre-creative and a creative stage of composition (1962:I, 230 ff.; II, 263), and more generally in the distinction between the given basic structures (“the structural progression”) and their prolongations in actual composition; 2) As in Schenkerism, the notion of goal-directed process emanating from something like a formal cause or a transcendental Idea has a dominating role in the theoretical play; 3) The basic structure in Schenkerism and the twelve-tone set both, though in slightly different ways, permeate the whole fabric of a composition, thereby assuring the relatedness of the parts, the “unity” of the whole.63

It is quite plausible to see Babbitt’s twelve-tone theory, focusing on the twelve-tone system, as initially an essay in creating an analogue to the Schenkerian way of thinking Process and Unity in tonal music. In the early phase, assumptions regarding these are essential for the argumentation.64

Conclusion
In this article, I have presented some results of a study of the way ideology and mathematics has interplayed in musical modernism. Though acts of faith in positivism, at least on a rhetorical level, are not to be denied, in the cases presented here there are other aspects of ideology, more marked and of more consequence, for example, individualism, creationist idealism, evolutionism, and other tenets of modernism, as well as use of teleological rationalities, and ideals of unity in art.

The role of mathematics in the examples has been shifting: Certainly, both in Krenek and Babbitt there are moments in which mathematics is a vehicle for treating problems of structural properties inside a given “technique” or “system,” for example, problems relating to the all-interval series or set. On the other hand, mathematics, in the form of axiomatics, has been found as a role model, the activities of which have been seen as having strong analogies with musical ones, but without the two really sharing fields.

62 See Babbitt 1999 for biographical details concerning his early contacts with Schenkerism.
63 I will take these statements as known facts, as far as Schenkerism goes.
64 Seen from our vantage point of today, one could perhaps be surprised that Schoenberg didn’t figure as a central reference for Babbitt when it came to ideals of Unity. Under the threat of inflating the list of references, Neff 1994 is a chosen reference about ideals of coherence and organicism in Schoenberg; for a discussion of a telling passage in prose from Webern, see also Street 1989.
On a middle ground between these, actual mathematical ways of approaching and expressing problems has manifested themselves, though subservient to aims of constructing an ideal (new) reality, rather than “just” describing an existing or a possible one. In the latter case, in Babbitt’s early writings, most notably the thesis from 1946, there were certain possible confusions concerning the actual presuppositions of the so-called system, in that the series or the system, rather than being the primary and intrinsic cause in a more or less in an Aristotelian fashion conceived process, seems to be tailor-made to harbor certain aesthetic-ideological values, which are the “true” forces behind the process that seemingly emanates from a formal cause or essence. In the last analysis, this process is more rightly to be seen as an outcome of a program for action, implying a more pronounced role for the efficient cause, that is, the composer, than just being an agent in the realization of an Idea.
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Mathematics and Ideology in Modernist Music Theory


From one point of view, music theory should educate. This we know from the oldest history of the Greek word *theoria*, which can be derived from the classical Greek word for “behold.” Thus, one kind of educational view is present in Boethius’s threefold concept of music in *De institutione musica*. It actualizes the old mystic idea that phenomena in one world could be explained in another, postulated world, whereby music theory becomes an instrument for learning to understand the fabric of cosmos. Quite another sort of educational view can be found in Johann Joseph Fux’s *Gradus ad Parnassum*. His five steps to heaven are methodically organized levels of practical training, aiming at perfection of multipart writing.

From another point of view, music theory is considered a philosophical and scientific study of the fundamentals of music making, a viewpoint developed during the last three-odd centuries. This is true at least as long as we put some stress on the expressions “scientific” and “fundamentals of music making.” Even though Jean-Philippe Rameau’s *Traité de l’harmonie* no doubt bears the marks of a rational and in a certain respect scientific attitude, it does not meet the scientific criteria of our time any more than does Descartes’s psychological treatise *Les passions de l’âme*. The point is that the concept of science and the idea that something could be considered to be scientific are of relatively late origin. The belief in the powers of science is also an outcome of modernity. As we know, it still attracts criticism and discussions.

This view of music theory and its history is based on the study of Western intellectual history, not on music history. Such a perspective implies some methodological attitudes specific to studying the history of ideas. The main interest lies not in the question of whether a scientific theory, a political ideology, or some other form of complex of ideas is true or not. Instead such a
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view is focused on understanding the theory, the ideology or the complex of ideas from its own assumptions and in its proper historical context (Skinner 2002 and Burke 2002). As these things change over time, so do theories, ideologies, and complexes of ideas. This brings the history of music theory close to the history of science, to the history of political thinking, and to the history of aesthetic ideas generally. Such an approach does not primarily teach us what is right or wrong, but it may inform us as to why a certain theory is not any longer appropriate for understanding the matter it set out to explain.

The nearness of the history of music theory to the history of science should not, however, overshadow the fact that there is a fundamental difference between the two. Certainly music theory takes music’s physical, physiological, and other conditions into consideration, but that still does not make music theory a science. While science relies on empirical information and on a mind-dependent theoretical apparatus with high general acceptability, music theory is the theory of an art form.

The aesthetic field is—at least in the long run—governed to a high degree by changeable human conventions, which means that music theory takes a lot of scientifically debatable assumptions for granted. One example of the dependence of music theory upon aesthetic theory touches on the classic epistemological question regarding the extent to which and the ways in which knowledge and—in this special case—musical experience is mind-dependent. As we know from Heisenberg’s uncertainty principle in theoretical physics, such dependence must be illuminated and its consequences must be coped with.¹ When theorizing about music we must know what is important to the listener’s experience and also why it is so. The question is not only important in discussions of music theory, but also in art education and in aesthetics generally.

Another prominent example of the dependence of music theory upon aesthetic theory is connected to the work concept (Pudelek 2000–05) It is hardly too much of an exaggeration to maintain that work became something of a key concept in nineteenth-century aesthetics. In several ways it also became a metaphysical assumption in music theory, and maybe it still is. Could a work of music be analyzed without first being delimited, so that the ontological character of the work seems clear? Does traditional music theory presuppose a kind of fundamental holism, so that the work—the whole—must be understood before we can appropriately handle the separate parts? In the following I will try to point out and discuss some situations where those two problems concerning music theory and its dependence on aesthetic principles mattered during the heyday of European music theory writing, which

¹ As a result of the process of measurement, according to Heisenberg, the exact position and the precise velocity of an electron could not be determined at the same time. See further Pauli 1955.
means the end of the nineteenth and the beginning of the twentieth centuries. Though I will handle historical material, I do not think that what I say should be of mere historical interest. Do we not still often rely upon theories from this period? And do we always realize what the assumptions are?

II

It is hardly controversial to point to the eighteenth century, with its row of followers of Rameau, as the century when modern music theory was born. It was not only the century of Johann Philipp Kirnberger and Heinrich Christoph Koch. It was a time that saw the birth of the market economy, of the public sphere and of modernity in general. This was the fundament of the modern, institutional position of science in society and of the modern concept of art.

During the following century—after the Romantic Movement and a breakthrough for ideas supporting a broad, liberal and moral education—the standards of higher music education were revised. According to new general outlooks, music theory needed to involve more than just paradigms for practical training. Instead there was a belief that the fundamentals of art must contain things that allowed the initiated to bring about an experience of the ineffable. Artistic theories got a philosophical aura they had not had since antiquity. The nineteenth century was also an era of success for science in general and especially for natural science. Much as the theory of painting relied on optics, music theory was to a certain extent determined by acoustics. But like the theory of painting, music theory was also defined by the discoveries in physiology.

The success of physical sciences and the fast growth of physiology during the second part of the nineteenth century fueled the development of music theory, while the fire came from the musicians’ need to handle new and still more complicated problems. The German physicist and physiologist Hermann von Helmholtz played an important role in this development. He had earned his historical importance already in 1847, when he published his findings on force and energy that for the following fifty-odd years represented the perfection of Newtonian physics. Between 1856 and 1867, he released three volumes of Handbuch der physiologischen Optik (Helmholtz’s Treatise on Physiological Optics). As it combined physical and physiological perspectives it became the source of new professional attitudes among painters at the end of the century. While still working on the optics, he released On the Sensations of Tone as a Physiological Basis for the Theory of Music (Lehre von den Tonempfindungen) in 1863. Here, as in his work on optics, he combined findings in physiology with physical results to create a rather convincing theory about the fundamentals of music. His work was for a long time impossible to ignore for anyone who had ambitions to do something in music theory.
During the second part of the nineteenth century Helmholtz held a prestigious professorship at the legendary Berlin University and was one of the brightest stars of German natural science. He has often been alleged to having pioneered a strict empiricist view of science. However, his debate with the physiologist Ewald Hering about the theory of perception and his late writings on epistemology tell us otherwise (Turner 1994 and Helmholtz 1977). Here he took a modified Kantian position, as he rejected the idea that judgments about psychological matters should be reducible to physiological judgments. He maintained that sense experiences are constructed from perceptions in every single person that is exposed to the same outer stimulation. From this we can conclude that he, like so many of his contemporaries, had an idea of a mind that actively contributed to the formation of knowledge. In his research on how we perceive phenomena of light and sound he evidently imagined three levels of experience. He saw first a physical level, then a physiological level, and finally a psychological level. The final level, at least, could not be reduced to the other two.

Kant most convincingly expressed the idea that the human mind is active in the formation of knowledge and for instance contributes in ordering sense experiences spatially and temporarily. After his Critique of Pure Reason this idea became something of a commonplace among people of rather different opinions and was used toward rather different ends. It was not only scholars and scientists who had reasons to think about the experiences and the behavior of human beings. In one form or another Kant’s ideas also influenced artists, teachers, and people writing books on the theory of music. Helmholtz had several empiricist traits, but many of the music theorists he inspired would depend considerably more on the perception of musical structures than he did.

The idea that the mind is actively engaged in music perception seems to be a main assumption for many a theory of music from the nineteenth and early twentieth centuries. This is true not only in connection with obvious idealists such as Adolf Bernhard Marx, Moritz Hauptmann, and Siegfrid Karg-Elert. It would also be hard to motivate Hugo Riemann’s harmonic functions and even more his rhythmic theory without help of the working mind. One of the most notable assumptions in his theory of harmony is the substitution of his own “Tonvorstellung” (apprehension of tone) for Helmholtz’s concept “Tonempfindung” (sensation of tone). Even Heinrich Schenker’s representations of musical developments as hierarchies of musically more or less important tones and his conception of Fernhören (distance hearing) seem to presuppose the idea of the mind’s activities. Ernst Kurth does not just use a metaphorical language that allows him to formulate a theory of melody as a theory of force. In the Grundlagen des linearen Kontrapunkts (The Fundamental Laws of Linear Counterpoint) he bluntly takes over a good deal of Riemann’s conceptual apparatus, which among other things
enables him to write about *Tonvorstellungen* rather than about *Tonempfindungen*.

This view of the active mind was not controversial by any means, nor should it seem so today to people with a philosophical education. A principally Kantian view of perception was generally accepted during the nineteenth and the early twentieth centuries, and not only in Germany. The extensive discussions over “psychologism,” for example, were in essence a debate about the internal relations between philosophy and the new science of psychology. Opposing the tendency within experimental psychology to identify mental phenomena with physiological processes, some philosophers advocated what they called “mentalism.” They would not make use of ontological reduction of mental phenomena. The main question was whether human knowledge presupposed organizing principles validated *a priori*.²

When Riemann, Schenker, and Kurth formulated their theories, the manifold of positions within psychology and philosophy made it necessary to choose a point of view and a conceptual apparatus, which gave them certain possibilities while it denied them others. They had to decide whether they wanted to treat musical experience as primarily a process governed by psychological laws and describable in physiological and anthropological terms or whether they preferred to treat it as an irreducible mental process, governed in particular by the experiencing individual’s own mental activities. Riemann, Schenker, and Kurth all chose the second alternative. For them music was not a play with tones and rhythms governed by cultural conventions. They rather saw music as an expression of objectively true cultural values—real and existing on their own—and as a demonstration of the powers of the human mind.

Riemann meant to expose an exclusively “musical logic” with roots in the musical material as well as in a deep knowledge of the human mind. Schenker’s idea of the “Urlinien” as the expression of a certain kind of logical structure in all music hints in the same direction. As a child of his age Kurth dressed his thinking in conceptions of power and emotion. That allowed him to present aspects of musical perception that usually were considered to be irrational, or at least subjective, as being rationally determined and musically objective. As our own history tells us, this was not in vain. The theories of these three are still part of the music curricula in different parts of the world. But does that mean that they are truly understood?

### III

In *fin-de-siècle* Europe there was a call for a new philosophy, a form of thinking that could do justice to the natural sciences, but that did not reduce

² For a general overview of the discussion, see Smith 1997, especially chapters 14 and 17. See also Sluga 1980, chapter 1.
knowledge to only the facts revealed by the physical sciences. The old world of culture and human values could not just be emptied of everything but material elements and hard facts. The task of the philosopher and scholar was to unite the scientific and the cultural perspectives. A very important aspect of this, which we nowadays often overlook, was the need to find a way to handle value judgments. This was also an important element of the work of Riemann, Schenker, and Kurth, who all in different ways tried to legitimate what they understood as cultural values.

“Function” is a rather common concept not only in music theory but also in mathematics. A special case of the concept of function has had great importance in several scientific areas during the last two centuries, and it has also been the object of many a dispute. As a fundamental concept of mathematics function is used when one quantity is regarded as depending on other quantities. It assigns to every single value in one specified set a unique value in another set. The concept originates in the correspondence between Leibniz and Jean Bernoulli in the end of the seventeenth century. About hundred years later Kant used the term “function” in two other related meanings in his Critique of Pure Reason. One of them concerns the formation of concepts. He described function as the unity of the act organizing different ideas under a common idea. As will be obvious later on, this view of function became important for the use of function in the theory of harmony. Beyond that, the concept of function has been of great importance to various sciences, especially in biology and social sciences. It even has had a rather great impact in musicology, but that is beyond our focus here.

The introduction of the concept of function into the theory of harmony was perhaps Riemann’s greatest effort in music theory. In his original presentation of the functional theory of harmony (Riemann 1882 and 1916), Riemann saw the possibility of tracing the harmonic development back to a fundamental principle. Here he stressed acoustic conditions, such as the structure of the overtone series, but also his own rather headstrong theory of musical perception, where apprehensions of tone (Tonvorstellungen) played an important role. Riemann’s theory of functional harmony is not unambiguous, however. On the one hand it can be understood as a theory in which the internal relations among the chords in a cadence define their harmonic functions. On the other hand, the chords’ attributes, that is their tonal content, should determine their functions. Whatever the case, the main thought was that all chords in the end should be traceable to the three harmonies that Rameau already had seen as fundamental in every cadence: tonique, dominante, and sousdominante. Here is the influence from Kant’s idea of a function as the unity of the act organizing different ideas under a common idea evident. The principle behind this was the chords’ relations to the tonic, to

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3 Kant, Kritik der reinen Vernunft, A 68/B 93: “…die Einheit der Handlung, verschiedene Vorstellungen unter einer gemeinschaftlichen zu ordnen.”
the tonal center of the cadence. Riemann himself put it the following way: “The functional representations of the harmonies are a suggestion of the disparate significance (function) that the chords have for the logic of the tonal structure due to their position to the present tonic.”\(^4\)

According to Carl Dahlhaus, “position” in this quote had the relation within the circle of fifths in view. As he regarded the matter Riemann could hardly mean the chord’s position in the cadence. However, Dahlhaus’s supposition is not fully convincing. There are two reasons to be skeptical of Dahlhaus’s interpretation. The first reason is that Riemann evidently considered “the logic of the tonal structure,” Logik des Tonsatzes, to be conclusive. The second reason is that Riemann so closely connected “significance” (Bedeutung)\(^5\) with “function” is this passage. His ideas of a musical logic mirrored the peculiar tension between demands for a metaphysical fundament of knowledge in a changeable world and the new possibilities for abstraction and secure reasoning that were furnished by the development of mathematics during the nineteenth century. The philosopher Hermann Lotze had particularly expressed this tension, when in his Metaphysik he commented upon the presentation of non-Euclidean geometries, that is geometries constructed on axioms other than the classical Euclidean (Lotze 1912, 234). For Riemann there existed a special kind of logical relation, which could not be reduced to a general and formal logic, in the musical material (a notion inherited from Hegel via Moritz Hauptmann). This was opposed by the obvious difficulties of expressing general law-like connections based on empirical studies in the theory of music. These were not least concerned with the problems applying quantitative and probabilistic reasoning in an area so highly determined by individual aesthetic thinking and cultural relativism.

It is tempting to look for parallels between Riemann’s endeavor to develop a theory of harmony and Gottlob Frege’s attempts to find means to analyze thought and language. They were not only contemporaries; during the 1860s and 1870s they had also both attended Lotze’s philosophy lectures in Göttingen. An important part of Frege’s theory of language consisted of a description of the relation between individual and general concepts as a kind of functional relation. This idea had its origin with Kant. Generally it also was a part of the theory of concepts, which Lotze had formulated in his Logik (Lotze 1912 vol. 2, 4 et passim). To him the individual concept’s as-

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\(^4\) “Funktionsbezeichnungen der Harmonien ist die Andeutung der verschiedenartigen Bedeutung (Funktion), welche die Akkorde nach ihrer Stellung zur jeweiligen Tonika für die Logik des Tonsatzes haben.” Riemann, Musik-Lexikon 7/1909, 441), citation Dahlhaus (1989, 112) (my translation).

\(^5\) In the translation of the citation and in the following I use the English word “significance” to translate the German Bedeutung. This is the dictionary translation of the word, even if it is contrary to praxis in British and American philosophy of language where “reference” is the most common translation. But as “significance” has a value-ascripting secondary sense (though it is weaker than its German counterpart) that fits in with my argument I prefer to use that term.
What Kind of Theory is Music Theory?

assignment to the general concept had the same ontological status as a mathematical function. Departing from that view the mathematician Frege described conceptuality so that when the independent variable in the function “capital of” was given the value of “Sweden,” the dependent variable took the value “Stockholm.” This construction permitted Frege to determine the meaning of the sentence with respect to the criteria for accepting it as true.

Riemann had another problem, probably to a higher degree than he himself realized. His way of connecting function and significance (Bedeutung) suggests that he was thinking in a mode similar to Frege. The fact that he discussed the problems in the area of logic, which according to Lotze and Frege was its proper place, further suggests that this was the case. Such a perspective on Riemann’s functional theory of harmony makes the picture much more diverse and complicated than the rather meager explanation relying on the relations within the circle of fifths. For Riemann the problem was not simple relations within the circle of fifths or the chords’ position in the cadence. Of course both those things mattered, but he had an urge to put the argument on such a high and lofty theoretical level that contradictions in details did not disturb the overall theory. He could use help from Lotze’s functional representation of the relation between individual and general concepts. Following this model he could make a scientific theory out of Rameau’s opinion that all chords could be related to one of three main categories. Naturally Frege could have directly inspired Riemann, as he published his functional theory of concepts in the beginning of the 1890s. However, this does not seem very likely, as Riemann seemed to have had an embryonic version of his theory already in the beginning of the 1880s.

In the extensive discussion about Frege’s importance for the philosophy of the twentieth century, the German philosophers Ernst Tugendhat and Gottfried Gabriel have discussed his use of the German word Bedeutung (significance) (Tugendhat 1969–70 and Gabriel 1986). Both have noticed that he initially gave the word a broader denotation than he later did in his article “Über Sinn und Bedeutung” (“On Sense and Reference”) from 1892 (though they have drawn different consequences out of this). As they both have pointed out the German word Bedeutung has a subordinate sense, which means that it both refers to something and tells that something is important. Thus it ascribes a positive value to the thing signified. From that point of view Gabriel actualizes the strong connection between truth and value we know from the neo-Kantians of the Baden School. One of their most prominent representatives, Wilhelm Windelband, was also one of Lotze’s students. Gabriel points out that this connection not only was important for the young Frege, but that it also had its roots in Lotze. Against such a background it is easier for understanding Riemann’s slightly unclear talk of “the disparate significance … that the chords have for the logic of the texture.” In his philosophy of language Frege pointed out the sentence as the obvious context to understand what a single word is signifying. In the same
way the harmonies in Riemann’s theory were defined with respect to at least as extensive musical unities as cadences. But at the same time he assumed that the guiding principle when defining the harmonies must be their roles in constituting the aesthetic value of the whole composition.

An absolute assumption for Riemann’s functional theory of harmony was the idea that music was a play with apprehensions of tones (Tonvorstellungen) and not with sensations of tones (Tonempfindungen). This could be understood as sheer idealism, but it could also be seen as a view that called upon training and acquired cultural competence. Whatever the actual position of Riemann, his theory presupposes the idea that the mind is contributing to experiencing music. But the key to the theory lies in the connection between significance, understood so that it also is related to the aesthetic value, and function. Here the power of function was not that it should guarantee a transport of truth, but rather a transport of aesthetic value. With the use of function Riemann modernized the idea of the organism as an interactive determination of the parts and the whole. In this Riemann managed to join the classic concept of work with what appeared to be a scientific attitude.

IV

After the modern pioneering generation of Riemann, Schenker, and Kurth, music theory adapted a new ideal of a unity of science, empiricism, phenomenology and the dream of a science that should be absolutely free of value judgments. Social sciences, like functionalist sociology and empirical psychology, promised to explain everything from suicide to aesthetic experiences. Psychology aimed at formulating law-like propositions that could substitute for older ideas about the mind’s contribution to experience and the formation of knowledge. This also happened in music theory. Scientific realism separated acoustics from psychology, and logical empiricism preferred laws founded on probabilities to rational deduction and introspection. Real music appeared to be identical with either the acoustical or the notational forms of existence. The idea of a work with a content that consisted of something more than the mere sum of the physical parts was as unpopular as the idea that a work of art needed a context to be understood.

While the premises of music theory drastically changed, theory and education piously held on to old theoretical patterns and to a well-known conceptual apparatus. A problem was that many a theoretician gave the terminology of music theory objective references corresponding to what they imagined to be real correlates that should be possible to define physically. Concepts of music theory such as “theme,” “tonality,” “melody,” “rhythm,” etc., are not primarily definable in physical terms, but are definable in psychological terms.
The problem could be illuminated by an example that originates with Aristotle (“Physics,” I, 193b32–194b15). If physically definable frequencies, time intervals and variations in air pressure are important parts of the material of music, this will only hold for their properties as natural kinds. As material for a cultural activity they have to be considered as preformed by the same mental process that converts mere sensations to apprehensions. Frequencies will become tones in relation to a tonal system, time intervals will be durations in relation to some kind of rhythmic organization and so on. What is presented to our perception when we are listening to music is something other than physical attributes. It is not uncommon for people to believe that human feeling and thinking could be brought back to the physical conception of the universe. That idea is an expression of a rather simple and reductive materialism that has been too common during the twentieth century.

Someone who still wants to push a materialistic point of view can well accept all that has been said here. She could then add that she actually does not know the whole process between the changes in air pressure and the experience of music. The formation of musical entities could take place already within the act of perception. Someone brought up in the Western music tradition has learned to make a lot of fundamental distinctions by ear. Generally this means that the musical material is actually formed on two levels. Like most other things it is primarily transformed by the mind from something strictly material to something musical. Then it is united into a music work with aesthetic qualities and value.

Whether or not one is tempted to accept this idea that the musical material will be formed on two levels, one has to admit that it illuminates a main problem of modern, scientific music theory. The terms of music theory do not represent unambiguous physical entities. Rather they are defined in respect to perceptions and sometimes they are used in accordance with a blurred mixture of sensations and perceptions. This is the result of the marriage between old traditions of music education and an urge to base music theory on scientific empiricism and a scientific realism. This should have resulted in a psychology of music that admitted the complete translation of old music terms into the language of science, but it did not.

A general problem with this scientific theory of music was the ambition to present law-like propositions of more or less universal relevance. There had to be something that transcended cultural and stylistic conventions. Historical music theory was usually normative, but conventional. In some way modern music theory wants be definite. It has not just wanted to determine the effects of music on the human mind, once and for all times. It has also sought to formulate the ultimate presuppositions of music making. Evidently there are physical as well as psychological principles that govern the possibilities of music making as well as listening. But as long as music is primarily considered to be a form of expression in human cultures, the aesthetically
valuable in music is as dependent on these physical and psychological principles as the communication in a natural language is dependent on the phonetic resources of the language.
References


A Woman's (Theoretical) Work

Marion A. Guck

While still a graduate student in music theory, I began to notice that informal, figurative language conveyed and encouraged a vital engagement with music in ways that I missed in many analytical texts. I came to this realization from a typical music-theoretical position of skepticism about the value of metaphorical language that was shaken by a student in an analysis class. Initially enthusiastic and talkative about her sense of how pieces go, she eventually became silent and sullen. I attributed the change to my discouraging her from using language that I found imprecise and embarrassingly personal, but that she apparently found vividly informative.

My dissertation (1981a) examines ways that metaphorical descriptions of musical works “provide information about pieces that is not available through technical language.”¹ I taped three groups of musicians in conversations centered on a passage from a recording of a Chopin prelude, under circumstances that discouraged what I thought of as technical description and that thereby encouraged metaphorical descriptions. Then I asked the conversants what about the piece’s structure inclined them to say the so-called non-technical things they had.² I learned that metaphoric descriptions are associated with the musical sound-world in complex ways; more importantly, I learned that such descriptions articulate aspects of the sound-world and of experiences with the sound-world that technical descriptions cannot.

I’m still working from what those people taught me. My subsequent work has often examined how a characterization of the effect of a passage, for example, that I hear it as portentous, can be understood as induced by the particulars of the music’s sounds.³ Recently I have become increasingly concerned with portraying how a piece’s passage in time feels to me, and with examining the ways that individuals, most often analysts, are involved with

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¹ Two published papers (1981b and 1991) incorporate ideas and material from this work.
² My methodology’s reliance on informants developed from linguistic study I also undertook in graduate school. The outcome bears some resemblances to ethnomusicological studies and some ethnomusicologists have expressed an interest in it.
³ The example is drawn from “Rehabilitating the Incorrigible” (Guck, 1994b).
musical works. In all of my work, I have argued explicitly and by example that any analysis of music derives necessarily from personal experience of music and that analyses benefit from the overt representation of the analyst in the text. To tease out the epistemological commitments of this assertion will be one of the tasks of this paper; it intertwines with another task, to relate the work to my gender and history. This latter task requires further introduction.

As these interests became clearer to me, I began to speculate that my work is, in some sense, informed by my gender situation, and I determined eventually to examine this conjecture. Now, having achieved a modest degree of feminist awareness, I propose to reflect upon how my work is “a woman’s work,” a woman who has chosen to position her intellectual work within the discipline of music theory.4

To say that my work is a woman’s work might mean either of two things: that it derives from my experience growing up and being treated as a woman, or that the work itself has qualities that are gendered feminine.

The impression that my work derives from my experience as a woman has been supported by noticing a similar willingness to address the less clear-cut aspects of perception in Elaine Barkin’s (1975 and 1978) analyses of Webern and Berger, and in Judy Lochhead’s (1979) application of phenomenological method to musical analysis. Lately, I’ve been heartened by Claire Boge’s (1990 and 1992) examination of the use of figurative language to help students become analysts, heartened by the attention she gives both to the role the language plays in understanding and to what she learns from teaching. But I am in danger of sounding essentialist.

“Essentialists” believe that certain traits are natural attributes of men, for example, rationality, in opposition to traits considered natural to women, for example, emotionalism. Essentialism tends to reinforce the view that women are by nature capable of only certain kinds of things, thereby continuing to justify limiting women’s possibilities or our relegation to a “special sphere” (essentialism also limits men’s possibilities, though the results are less obviously pernicious). It tends to facilitate the denigration of what women are thought of as doing naturally because masculine traits are more highly valued than feminine traits; on the other hand, it labels women who do different sorts of work “unnatural.” By contrast, constructivists hold that women’s and men’s “natures” are not, in fact, natural but rather are culturally determined: inculcated in the course of childrearing and reinforced throughout life. What is cultural can be changed; what is natural cannot. My position is thoroughly constructivist.

If I think my work has important features in common with that of several other women working in music theory, I also can’t fail to notice that there

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4 Autobiography is a frequent source for the theorizing of feminist writers. See, for example, Miller 1991.
are men doing similar work. J.K. Randall and Benjamin Boretz are probably more notorious than Barkin for their evocative analyses, and Boretz, especially, has long been an important influence on my thinking. Edward T. Cone’s *The Composer’s Voice* is almost twenty years old—I’m ashamed to say that I didn’t know it in graduate school. My current work overlaps that of Fred Maus and my colleagues Robert Snarrenberg and Roland Jordan. Roger Graybill’s research is, like Boge’s, centered on issues raised by his teaching. The qualities I sense in common with the work of these men may gender the work feminine.

“Gendering” is the ascription of masculine or feminine quality to some pair of opposed entities or concepts. For instance, mind and culture are construed at the present time in this culture as masculine, body and nature as feminine. These qualities are rarely identified explicitly as masculine or feminine; usually the ascription can be inferred by the treatment received by work that is gendered feminine. In a recent study of research by a varied group of sixty-two women, Nadya Aisenberg and Mona Harrington (1988) notice that there is always a small group of men who do work like that of the women they talked with and they point out an interesting fact about it: the work always holds a minority position, whatever the field.

Fred Maus (1993) has argued in “Masculine Discourse in Music Theory,” that music-theoretical discourse is gendered. He illustrates this gendering using a set of four paired terms developed in John Rahn’s (1979) “Aspects of Musical Explanation.” He shows that “in each pair, the term that refers to mainstream professional discourse [that is, “digital,” “time-out,” “top-down” or “concept-driven,” and “theory of piece”] is also the term that associates with masculine gender.” The other term in each pair, “analog,” “in-time,” “bottom-up” or “data-driven,” and “theory of experience,” is gendered feminine. Of two of the pairs, Maus says that “experience-oriented accounts tend toward personal chronicle (gendered feminine), piece-oriented accounts tend toward statement of results and impersonality (gendered masculine). An opposition between “concepts” and “data” is likely to recall gendered oppositions between form and matter, mind and body.” He points out that music-theoretical work in general could be gendered feminine due to the centrality of the relatively passive, receptive act of “listening attentively,” and he proposes that the power relation of this feminine role might be reversed by concept-driven explanation that “would situate the individual composition as an instance of [a] general theory, thereby giving the theory a sort of controlling power over the composition.”

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6 The gendering of Rahn’s paired terms is discussed in the section of Maus’s paper headed “Some Gendered Oppositions” (269–70). The feminine gendering of listening and thus “the theorist’s relation to music” is discussed as the first point in the section headed “Some Gendered Figurative Language” (271–5).
may be designed to mask an underlying femininity; doing work that is gendered feminine, for example, focused on the individual rather than generalizing and thereby considered subjective rather than objective, may be more than usually problematic in this discipline. If such work is actually done by a woman, feminine-gendered qualities can be causally connected to her essential, and inferior, feminine “nature.”

The work that I do can be gendered feminine along the axes Maus draws using Rahn’s paired terms: it can be classified “analog,” “in-time,” “data-driven” (“bottom-up” is not equivalent for me), and “theory of experience.” I will explore some of the implications of that gendering when the work is in fact, done by a woman. Specifically, I will look at some reactions to the work and I will try to relate my research inclinations to my cultural conditioning to life as a woman (whose expectations were also limited by the fact that I grew up white but working class).

It will sometimes be unclear whether I am speaking of my work as gendered or as owing its particular character to my experience as a woman. The unclarity is intentional: gendering happens not only to work but also to people. My experience includes learning and living a gender role. In my particular case, that role is conflicted, as I will describe shortly. I have tried to be clear about my constructivist position, but trying to be clear has made it evident to me that notions of the masculine and feminine that tend to reinforce the conflation are so deep-rooted in our language that it is difficult to avoid asserting traditional gender associations unless they are explicitly denied in every instance. I may not always have succeeded.

Two other mistakes keep tempting me, as well. One is the mistaken notion that, by presenting this paper, I must be offering my work as an example of women’s work generally. The legitimate sources for this mistake can be found in the belief that my work has been shaped in significant respects by my experience as a woman, and the recognition, from conversations with others, similarly situated women, that my experience is, in some respects (for example, my descriptions of graduate school), similar to theirs.

Aisenberg and Harrington (1988), though also uneasy about making general claims about women’s work, offer modest support for this belief. They find “common patterns,” “not … absolute qualities marking the work of all women, but … propensities.” They see these commonalities as deriving from the common experience of women who set out on a quest for professional authority in a cultural climate still significantly defined by the marriage plot. This experience sets up perspectives and interests that unavoidably affect how women carry out their intellectual work—from the academic disciplines they enter, to the questions they ask and, often, to the values they apply in reaching answers to those questions (85–6).
Aisenberg and Harrington locate the cause of the difference of women’s work not in women’s nature but in women’s experience. The language they use implies that that experience is characterized by conflict between culturally defined ideas about men and women, specifically in life expectations shaped by a masculine-gendered quest plot as opposed to a feminine-gendered marriage plot. Women who choose the research path in academic life place themselves in a conflicted position relative to the masculine-feminine opposition as it is constructed in this culture. I dare say that many women find these oppositions, as they exist within ourselves, terribly painful. And our work, or mine, at least, can be an effort to integrate warring elements. Below I will allude to conflicting inclinations to think “like a man,” that is, in music theory’s analytical mode, and to think (or feel) “like a woman,” by attending carefully to my personal experience. The two are not, after all, in opposition; they illuminate each other, make a more satisfying whole. They can be integrated.

Oppositional thinking is the other error into which I fall, most strikingly when I reify a notion of normal music theory in opposition to work like mine. This habit arises when I place my work, or when someone else places it, in the context of the discipline of music theory; for example, I appear in the tenth anniversary bibliography of the Society for Music Theory under the heading “New Research Paradigms” (Rahn 1989). However, if my work represents a new paradigm, it is not by opposing or supplanting an old music theory-and-analysis, but by enlarging its scope.

Virginia Woolf has described how work done by a woman can be misvalued when it proposes “new paradigms”:

> When a woman comes to write a novel, she will find that she is perpetually wishing to alter the established values—to make serious what appears insignificant to a man, and trivial what is to him important. And for that, of course, she will be criticized; for the critic of the opposite sex will be genuinely puzzled and surprised by an attempt to alter the current scale of values, and will see in it not merely a difference of view, but a view that is weak, or trivial, or sentimental because it differs from his own (1972 [1929], 146; quoted in Spender 1989, 38, italics added).

Woolf implies that women who write novels try to change what counts, what gets attention. I have been trying to change what counts as music theory. In particular, I have been trying to change the rule of discourse by which we music theorists agree that we will not talk about our personal—emotional and physical—involvement in musical works, and I have been trying to

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7 In fiction, the quest plot centers around the male protagonist’s search for an identity or position from which he can act in the world; the marriage plot centers around the female protagonist’s attracting a husband with whom she can settle into a home. These stories, often repeated in fiction, crystallize cultural expectations about the ideal course of life for men and women.
change it because thereby we can augment and enrich the qualities that we can ascribe to works and the involvement we have with them.

Woolf also implies that work that changes values can be misvalued because it is misunderstood. Words may have different meanings and values for practitioners of different so-called paradigms of music theory. If I’m concerned about whether I can make myself understood, this concern is based on experience. I can exemplify with two referees’ reports on a paper (1993), “Taking Notice: A Response to Kendall Walton.”

One referee took issue with this statement from my text: “analysis is a continual engagement in refining our understanding of how pieces sound.” He or she quoted it in order to ask who could argue with it. I’d like to think that my statement is uncontroversial, but the analyses I find in print don’t often seem to take “understanding how pieces sound” as their point. So I’ve become suspicious that charges that what I assert is obvious, or not new, or what everyone knows and agrees to, are meant to discourage me from bringing up the subject of musical experience by hinting that I’ve made a fool of myself. The referee’s response reminds me of Woolf’s observation that “the critic … will see in [the work] not merely a difference of view, but a view that is weak or trivial, or sentimental.”

The paper I cited a moment ago responds to the philosopher Kendall Walton’s (1993) account of the motivation for analysis and of the analytical process, an account with which I agree. Walton proposes that understanding music has two successive aspects: first, one responds to something “as music,” and, then, one articulates what elicits that response. For example, we might hear a change of harmony simultaneous with the resolution of a suspension as welcome change, in one case, or as deflection, in another; we might then examine what we heard in the music that created the particular effect. This two-staged introspective process is not merely the means to some further explanatory end, however, but is “itself our objective or a large part of it.” Thus Walton assumes that being moved by the music is the sort of understanding that music theorists start with. Unfortunately, though it may well be the sort of understanding we start with, it is something we rarely articulate explicitly.

I responded to Walton by providing an example of what analysis might look like under his account of it. The example enacted the ways in which specific interactions in the opening of the Adagio of Mozart’s A-Major Piano Concerto, K. 488, induced a particular mental state in me, a momentary disorientation at the sound of the D major triad that comes in the eighth measure, which I described as “a disconcerting impression of giving way, but to something surprisingly welcome.” A second referee reacted to the analysis by saying

the responder’s account … is insensitive to all the cultural cues of dolorous chromatic music which were already well established in the first quarter of
the 17th century (and survived virtually intact in such genres as the instrumental siciliano). Far from being incongruous, the chromatic motions and embellishments are necessary and expected components of this music.

It seems that my values are so different from this referee’s as “to make serious what appears insignificant … and trivial what is … important.” He or she calls me “insensitive to … the cultural cues … of dolorous chromatic music”—as if the Adagio exhibited an agreed-upon dolorousness apart from my or anyone’s experience of it. *But recognizing cultural cues is not the same as hearing dolor.* My concern is my interaction with the music understood as an individual, an active agent in our conversation. Treating the piece primarily as a token of a general type or practice will not elucidate that concern.8

Furthermore, this referee implies that, if I’d paid more attention to the cultural cues, I wouldn’t have needed to explain why the “chromatic motions and embellishments” are there because I would have known they were not incongruous. But I never called the chromaticism “incongruous.” And its being normal for this kind of music doesn’t obviate the need to go into it. I wasn’t trying to explain why the chromaticism is there, I was trying to describe and account for the Adagio’s ability to act upon and change me. I think that the referee, reading from a firm grounding in a different set of values, can’t see what I’m doing and sees instead only that I do badly what he or she would have done.

To summarize, authorized speech about music doesn’t endorse recognition of personal musical experience. As Judy Lochhead (1982) has pointed out, theorists tend to replace the individual and that individual’s ear by the eye directed at the score. I, and others with similar concerns, insist that accounts of musical works acknowledge the relationship between piece and perceiver, and examine how that relationship is created, which means considering not just the musical “structure” evident in the score but how the piece affects the listener—how, for example, a piece might disorient a listener. In feminist terms, terms derived from an account of feminist objectivity by Donna Haraway, *omission of musical experience from analysis places the analyst in a position of seeing without ever being seen, of authority beyond observation; inclusion of musical experience makes both piece and analyst visible, makes the individual analyst’s location and perspective evident, and thereby makes the analyst personally responsible for analytical choices.*9

Let me return to the account of my work in order to look into some aspects of gendering or gender stereotyping and the conflicts these can cause.

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8 My paper was subsequently submitted to the *Journal of Musicology*, which published it.

I’ve had another eye-opening encounter with a student recently. In my sophomore class, my students and I had been looking at species counterpoint lines they had written and the tune in front of us at that moment pleased us. To make the point that simple species lines can be musical, I asked them what they liked about this one and was getting fairly routine answers until a student said that it was foreboding. My reaction surprised me: I said that it was musical because it could be heard as foreboding.

An odd thing to say, I thought, even a dangerous thing, and yet I find that I believe it: if a tune doesn’t have qualities like foreboding, qualities that pull at body and soul, then I’m not sure it’s music. Why am I uncomfortable saying this? To tell the truth, because I’m a music theorist: I was trained to think about musical structure, and my job, in this class, is to teach voice-leading and harmony, not banal sentimentalism.

So, though I’ve spent more than ten years thinking about how we can interpret musical sounds as seeming foreboding and what we get out of doing so, I still catch myself thinking that talking about music as “structure”—a public edifice in sound whose properties can be agreed upon because they can be seen—is better, or at least safer, than talking about music in the private, subjective, feeling world—the world traditionally assigned to women in our culture and traditionally despised by objectivity-seeking disciplines like music theory. My research and public papers affirm their origins in that world when they insist that the private and so-called subjective be brought into and accorded respect by the public (professional) theory world.

But, it’s not surprising that that old ideal of structure still catches at me. My internal conflict between masculine-gendered intellectual standards and feminine-gendered emotional awareness is longstanding. I was raised to be an intellectual by a doting father. My ability to think analytically (like a man?) is terribly important to me—it was my means of escape from the confining drudgery that “women’s work” seemed like as I dusted the house and washed the dishes as a child.¹⁰ Nor is it surprising that I keep trying to assert the value of the women’s world that I, nevertheless, absorbed—that dark and dirty work women have been confined traditionally to taking care of, including the murky work of feeling. We overlook it…

…Just as we music theorists overlook, bypass, how the music feels to us. Sure, we may have felt these things as children, but we’ve outgrown them, gone to work. We needn’t say such childish-womanish things any longer: they’re obvious, they can be assumed, they’re naïve.

These are the same deficiencies asserted or implied more than once by a journal editor about analyses that ostentatiously went through, did not pass over, how the sounds seemed to me—how they tugged at body and soul, what their musical qualities were. Read aloud to a Society for Music Theory

¹⁰ The inevitability of a life of drudgery may have seemed particularly acute, given my family’s economic limitations.
audience, the analyses were commended, but, submitted for publication in a lasting form, they are merely bad poetry about things that don’t need to be said.\textsuperscript{11}

And how did I get to be doing this work anyway? I mentioned my analysis student, but I couldn’t have noticed her if I hadn’t already had a disposition to question my training. To tell the truth, I had become bored with analysis as I knew it.

I am reasonably good at analysis, reasonably good even at remaking it to suit my needs, but even before I’d finished graduate school, it began to seem confining. Sometimes I think that thinking like a man began to feel just as confining as cleaning the house had. And I didn’t have a very strong commitment to a standard career path: graduate school had taught me that success along that route was by no means assured, as it had been when I was younger. Like many women, I got to graduate school and suddenly found that I was dumb, not able to speak, and, if I believed the reaction elicited when I tried to speak, not able to think. My intelligence was not marked by “normal,” which is to say masculine-gendered, behaviors and interests. So I didn’t have a big investment in high-status work. On the other hand, my status as outsider (to invoke Aisenberg and Harrington), gave me the freedom to take issues seriously that I noticed by paying attention to personal relationships with music, expressed in everyday ways of talking. I wasn’t deterred from asking questions less likely to yield to “rational” approaches.

I noticed that other women in my department also seemed to face difficulties and to have different ideas about theory. I think of Marianne Kielian-Gilbert, who, throughout those years, persisted in asking questions about the smallest details of the language we use and pursued a vision of music theory that stubbornly refused to demystify her relation to music, as she courageously continues to do. She still finds her subjects in examining those things: making theory in cracks that no one else seems to see and across spaces no one else thinks of connecting.\textsuperscript{12} Perhaps I thought that I might as well follow her example and pursue my peculiar interests. And we have both survived, in some ways even succeeded.

The music theory that I came to know in graduate school, and since, often seems to me sterile and conventional. Perhaps the analytical process is meant to bring individuals into the closer and more complete contact that I seek with a musical work, but usually by the time the analytical text is completed,  

\textsuperscript{11}Joseph Kerman (1991) notes the same disparity between recent adventuresome programs of the American Musicological Society and the still relatively conservative publication record of the \textit{Journal of the American Musicological Society} in “American Musicology in the 1990s.” I am grateful to Fred Maus for bringing this citation to my attention.

that intimacy and engagement have disappeared. Training in music analysis often seems to be directed toward acquiring a skill, which will assure the practitioner of producing a competent analysis of any piece in the proper repertoire—it is meant to ensure professional success. That is what I infer from the way that Schenker’s work has been transformed from an exemplary effort to model the individuality he found in each piece to a standardized and standardizing tool. Analytical methodology seems rarely to be meant to tap or to feed a passion for music. It only rarely seems to be meant to draw on individual creativity and insight. It seems designed to communicate mastery rather than involvement.

But I have been speaking as though my work stands in opposition to traditional analysis, when in fact my purpose is to enlarge its scope by writing from a position of involvement. Early in this paper I reified a distinction between technical and non-technical modes of speech. That distinction can be useful, but it’s misleading: at the Society for Music Theory conference in Oakland I spoke about the fact that any analytical text integrates technical and non-technical vocabularies—that the so-called non-technical language is used systematically enough to create, in every analysis, an account of the author’s sense of involvement with musical works (Guck 1994a). However, texts can be written or read in ways that highlight or downplay involvement. Most theorists, I think, are more comfortable downplaying it through choices of moderate, relatively conventional language. I am inclined to highlight involvement, usually by integrating vivid, response-depicting language with more traditional, analytical language. One might say that I have been trying to integrate what I’ve reified as my inclination to think like a man—analytically—with my desire to think like a woman—in terms of personal experience.

And doing this work gives great pleasure: I make myself all ears, give myself up to the piece, let it carry me away. When the sounds of the piece end, I carry myself back by asking “just what was that like? What could I tell someone else so that they too could hear that effect?” I play it at the piano, I make up momentary dances—I give myself up to it now as it has become part of me. I try to sense the qualities that the musical events create and I describe them, like my student did, as qualities like foreboding.

It’s not comfortable work, because there are no guarantees about what will happen—finding the words is always difficult and the fit is never perfect; on the other hand, I end up hearing things I hadn’t known to notice before. But what’s behind it all is pleasure: the pleasure of contact with the music, the pleasurable adventure of mind-exploration in order to find the words and shape them into something that can reach—achieve contact with—someone else who might also get pleasure from the experience.

For some years I have thought of this process as similar to interpreting the behavior of another person, and so I was pleased recently to discover a theory of knowledge that elaborates relationship with another person as a gen-
eral epistemological model. Lorraine Code (1991), in a book entitled *What Can She Know?*, points out the obvious fact that the intellectual skills through which we know people are crucial and ubiquitous. Her epistemic model is based on the model of friendship between two individuals, a relatively equal relationship requiring a balance between the separateness and interdependence of the individuals involved. Her model of knowledge thus incorporates notions of relation with the individual studied, respect for that individual, and focus of attention on the individual’s particular character.\(^{13}\)

On this view, the musical object becomes the musical individual; the process of analysis is the means to become increasingly well-acquainted with that individual. The analytical text is a characterization of that individual and of a relationship one might have with it. Theories grow out of individual experiences of knowing and contribute to them. Essential to Code’s theory is gathering information through attentive listening, a point of particularly close contact with the way music analysts learn.

Code gives an account of features of the research practice of three women scientists, Anna Brito, Rachel Carson, and Barbara McClintock, that “differentiates them from malestream ‘normal science’:

Their work is marked by (i) a respect that resists the temptation to know primarily in order to control. … The work is (ii) oriented toward letting the “objects” of study speak for themselves; hence it is wary of imposing preconceived ideas and theoretical structures. Yet … it is an attitude aware of the constraints of theory-ladenness and thus governed by reflexive, self-critical imperatives. The approach is (iii) nonreductive, adding to the first two features a recognition of an irreducible complexity. … In all of the features there is (iv) a sense of the knowing subject’s position in, and accountability to, the world she studies. That sense manifests itself in a mode of observation that is immersed and engaged, not manipulative, voyeuristic, or distanced. Finally, and implicated in all of these features, is (v) a concern to understand difference, to accord it respect, hence to overcome temptations to dismiss it as theoretically disruptive, aberrant, cognitively recalcitrant (150–51).

These attitudes bear affinities with the work I’ve done throughout my career. In my dissertation research my purpose was to discover how figurative language enriched people’s musical understanding. To do this I had to devise a situation in which I got, as much as possible, out of their way; and then my analysis of their conversations had to trace the patterns of thought evident in their speech with each other. I had to listen to the individuals, and discover the “system,” so to speak, from what they gave me—I had to be receptive, watchful of my own preconceptions, respectful of their intentions.

Since then, my analyses have grown out of a way of listening to music that follows much the same procedure and adheres to the same values as those I’ve just described: receptivity, concern to let the piece speak, care to

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\(^{13}\) For an account of this model, see especially chapters 3–4 of *What Can She Know?*.
try to let it determine the theoretical account even if I have to invent something to get there, even if it doesn’t fit the categories I have been familiar with or category-type distinctions at all. Largely what I learn is that the music-theoretical categories tend to separate the continuous sounding into distinct, bounded event types, which for some purposes is perfectly all right. But, if I want to portray, for example, the continuousness of sounding, or to incorporate its nonpitch aspects, I have to create a different way of speaking.

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I have argued, with Fred Maus, that music-theoretical practice and discourse is gendered. The sort of theory that Maus, some other women and men, and I do can be gendered feminine. What is gendered feminine is also usually valued less than what is gendered masculine. Most men aren’t going to do it; those who do risk having their work overlooked, marginalized because it is subliminally taken as feminine. Women who do such work take an even greater risk because the fact that they are women reinforces the feminine status, and therefore the devaluation, of the work.

I’ve talked about my work as different from traditional or conventional music theory and about being an outsider. I may sound bitter about it, but I don’t think I am. I would like to see inroads made from the margins to the center, not to replace the old center but to create a more integrated and balanced music theory for the sake of refining and enriching what we can notice and take pleasure in.

But, if I’m not bitter, I do get angry when my work is dismissed as immature or naïve. And, having returned to Virginia Woolf’s subject, I do want to shake up music theory’s values. I would like to make it difficult to forget that music theory is an inductive process that begins with, and ideally returns to, attentive listening. What I think we do as theorists, ideally, is to notice what we are hearing and, based on what we notice, change our hearing—refine it, enrich it. That is an individual process, though it can be influenced by what others have learned to notice: it is both interesting and useful to hear about how some individual has changed her hearing. Therein lies the public, intersubjective, professional part of the theorist’s work.

It is unusual in music theory to speak overtly, as I have, about personal experience. In music theory it’s just not done to speak personally. Public presentations are usually rhetorically dissociated from the cares, concerns, and particular perceptions of the individual, though life as a musician, and

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14 The most unequivocal examples of the marginalization of men for work perceived as feminine (personal, subjective) are Boretz and Randall. Maus discusses this issue in footnote 23 of “Masculine Discourse.”
indeed each analysis, grows out of a personal relationship with individual pieces. Yet, whenever someone stands at a podium or publishes a paper, she or he is telling us about her or his personal perspective on the musical world, however rhetorically covered the first-person perspective may be and however much the subject seems to be defined by disciplinary conventions. When you sit down to write or stand up to speak, you are asking your audience to pay attention to you, even if you want to show how Schenker’s methodology has uncovered a consistently occurring pattern of harmony and voice leading in composer X’s string quartets. You are telling your audience about your particular theoretical and musical commitments, cares, concerns, interests. Even if you think these concerns are shared by most of the people in your audience, they’re shared individual by individual. This paper may be more obvious about presenting a personal philosophy, but it’s not different in doing so.
References


Musical Intuition and the Status of Tonal Theory as Cognitive Science

Mark DeBellis

What do we hear when we listen to music? Rich and detailed answers to this question are found in Fred Lerdahl and Ray Jackendoff’s *Generative Theory of Tonal Music* (1983; hereafter *GTTM*), one of the best-known and most important contributions to cognitive science in the field of music cognition. *GTTM* is a comprehensive theory of a listener’s mental representations of tonal music, ostensibly from a computationalist perspective (Jackendoff 1987). In this article I want to ask: What is the nature of the mental representations *GTTM* postulates? Where do they lie in our geography of the mind, in relation to belief, perception, and language? Are they conscious or unconscious, conceptual or nonconceptual? Is the correct account of their content a computational one? And what sort of knowledge can we have of them? Addressing these questions will help us to see what sort of theory *GTTM* is, what it attempts to explain, and where it fits in the emerging paradigm of cognitive science. *GTTM*’s authors—Jackendoff most fully, and it is his book *Consciousness and the Computational Mind* I shall mainly draw on here—have provided a metatheoretical framework for *GTTM* that might well be regarded as authoritative. But I want to argue that that account leaves important questions unanswered, or dealt with only in an unsatisfactory way.

Essentially, my main contention will be that what Jackendoff’s story leaves out is any account of *GTTM*’s role, or the role of its analytical vocabulary, in communication. Hence it leaves out any explanation of the ability to put *GTTM*’s mental representations into words, hence, of the ability of a music theorist to arrive at the theory, or of a reader of their book to comprehend it. Curiously, the theory’s informal invocation of *musical intuition* points the way to the problem, although the implications of the notion are never acknowledged. Hence, the meta-account leaves unintelligible *GTTM*’s own existence, how the theory can be conceived of, stated, communicated, or known. This by itself is an important omission in a theory, such as Jackendoff’s, that purports to be a general account of the architecture of the

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mind. But the problem goes deeper: the omission gives us a false picture of \textit{GTTM} since, as I shall argue, it is essential to understanding \textit{GTTM} and its concomitant notions of mental representation that its role in communication between analytically-minded listeners be taken into account.\footnote{Thus what I am invoking is \textit{GTTM}'s role in communication of a certain sort, couched in \textit{GTTM}'s analytical vocabulary. Of course much communication about music goes on in other, e.g., emotive or gestural, terms.} In large part, I shall argue, these problems stem from Jackendoff's idiosyncratic philosophical framework. I should like to suggest an alternative construal of the theory that does not leave things so much of a mystery (or, at least, locates the mysteries elsewhere).

Once we have a more satisfactory picture of the nature of the mental representations the theory postulates, and of their roles in it, we will be in a position to appreciate some interesting issues about knowledge of musical structure, and about a certain kind of self-knowledge, namely, knowledge that one hears a piece as having this or that structure. Those are matters I shall take up toward the end.

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\textit{GTTM} postulates five levels of mental representation of music: musical surface, grouping level, metrical level, time-span level, and prolongational level (Jackendoff 1987, 217–32). The surface comprises pitch and duration (218); the metrical level, a pattern of strong and weak beats (221); the time-span level, ornamentation or elaboration of structurally important events (225); and the prolongational level, a pattern of musical tension and release (229).

To get an idea of what the theory looks like (this now is introduction, without prejudice to the philosophical issues), see the graph in Figure 1 (from Jackendoff 1987, 226). It is a time-span reduction of the opening of Mozart’s Sonata K. 331, representing the musical surface as being generated by a process of successive decoration from simpler to more complicated structures. The simplest level, (e), is that of a triad or chord, where C$\sharp$ is the highest note; in level (d) the triad is elaborated by a second chord, supporting a neighboring motion in the melody from C$\sharp$ to B; and so on, until we get the music as it actually sounds. A tree diagram indicates the structural significance of each pitch, where pitches introduced on simpler levels are higher in the tree.
A somewhat simpler example is that of grouping structure, from the scherzo of Beethoven’s Sonata Op. 2, No. 2 (Figure 2, from Lerdahl and Jackendoff 1983, 15). The brackets show grouping at several levels: at the most detailed level it is one bar + one bar + two bars, repeated; at the next level the two one-bar segments form a two-bar unit, balancing the next two bars (which carry over unmodified to that level); on the third level the structure is four bars balanced by four bars, comprising antecedent and consequent phrases.
A central claim of GTTM is that the experienced listener—someone who is familiar with a musical idiom but who need not have studied music or know any music theory (Lerdahl and Jackendoff 1983, 3)—attributes a structure to the piece at each of the five levels: musical surface, grouping, metrical, time-span, and prolongational levels (Jackendoff 1987, 214, 218, 232). GTTM’s notion of the experienced listener is modeled after that of the ideal speaker-hearer in linguistics (1987, 214), and hence the listener’s attribution of musical structure may be thought of as analogous to a language user’s attribution of structural properties (ambiguity, being related as active and passive, and so on) to sentences. Such attribution of structure is, according to GTTM, an essential part of the experienced listener’s understanding of music, and is the central kind of mental representation with which I will be concerned here. (Often, what will be of interest will be the more abstract, hierarchical levels, other than the musical surface.) Essentially, the background problem of this paper is to make coherent sense of the relation between these graphs, these pictures or diagrams, and what’s in somebody’s head.

Now, within GTTM, there is in addition a grammar, a set of well-formedness and preference rules (Lerdahl and Jackendoff 1983, 9) that assign structural descriptions to a piece and thereby predict what mental representations an experienced listener will have. (This musical grammar is modeled to some extent on those of generative syntax in linguistics, hence a “generative” theory.) I mention the grammar mainly to exclude it. The focus in what follows is not on the grammar, but on what the grammar generates or predicts: the listener’s representations of musical structure.

The term “mental representation for music” (Jackendoff 1987, 218) is unwieldy, and potentially ambiguous. What we want is the notion of a mental or psychological state corresponding to a structural description of GTTM, where being in that state constitutes the attribution of musical structure in the specified way. Let me introduce the term R-state to denote a such state. Thus, to be in any particular R-state is to enjoy a mental representation answering to a structural description of GTTM, and moreover to do so in a way that constitutes attribution of that structure to the music. The question is, now, what kind of states are R-states? What are their properties, and what notion of mental representation do they exemplify? To answer these ques-
tions, let us turn first to Lerdahl and Jackendoff, to see what they say about this and to see what role mental representations play in their theory.

An oft-repeated claim of _GTTM_, one that Lerdahl and Jackendoff clearly take to be important, is that representations other than those of the musical surface are unconscious: they write, for example, that _GTTM_ “address[es] the formal organization that experienced listeners _unconsciously attribute_ to a piece of music and the principles by which they determine this organization” (Jackendoff 1987, 214, emphasis mine). To avoid terminological confusion, let me point out two conditions often taken to be criterial for application of the words “conscious of.” First, what is available to verbal report is something of which one is conscious (hence, availability to verbal report is a sufficient condition for consciousness of something, though not a necessary one). The second condition on consciousness is that there is something “it is like” to have it—consciousness in the phenomenological sense. The view that emerges, in Jackendoff’s writings in particular, is that the relevant states are unconscious by both criteria. In connection with the first condition, he claims:

> [M]usical representations do not lead ultimately to the construction of conceptual structures. Since it is the presence of conceptual structures that makes verbalization possible, the musical response in large part simply cannot be verbalized. … [N]one of [the levels of musical representation in _GTTM_] translates into conceptual structure (1987, 237).

Thus, on Jackendoff’s view, structure of the kind that is specified in _GTTM_ is inaccessible to verbal report. And while he does postulate a level of conceptual structure, which he calls “conceptual structure$_2$” (1987, 313–6), which is the product of introspection and leads to verbal reports, conceptual structure$_2$ cannot describe the musical representations of _GTTM_ in any satisfactory way: “conceptual structure$_2$ is relatively rudimentary: it does not preserve anywhere near the full range of distinctions that exist in the [musical representations] themselves” (316). The informational flow from surface to structural representations and conceptual structure$_2$, respectively, are independent from one another (313–4); the latter two are not connected in any way. Hence there is nothing, on Jackendoff’s account, that would make possible verbal reports describing specific musical representations.

2 The terminological problem to be avoided here arises from the fact that Jackendoff (1987) uses “conscious” specifically for the phenomenological sense, but, as will be clear, he thinks musical representations fail the verbal-reportability criterion as well. These claims for unconscious status are to be distinguished from the following: (1) mental processes, as opposed to their products, are unconscious (45); (2) one is not aware of mental structures, as distinguished from what those structures represent (see Jackendoff 1992, 89); (3) the relevant levels do not exhibit consciousness in the sense of self-consciousness, i.e., second-order reflective awareness (Jackendoff 1987, 6); (4) we are not conscious of the rules of musical grammar (see below). Jackendoff is quite clear on all of these points.
Jackendoff takes pains to limn a phenomenological sense of “conscious of” and to stress that his interest is, above all, in consciousness in this sense. This sense is the operative one when he writes, “the perception of music … involves the unconscious construction of abstract musical structures, of which the events of the musical surface (the sequence of notes and chords) are the only audible part” (1992, 126, emphasis mine). According to Jackendoff, it is a “fact that one does not experience four simultaneous hierarchies while listening to music” (1987, 293). Thus, he argues, conscious awareness of music is located exclusively in the musical surface, not in the four other levels. (In the case of language, Jackendoff makes the analogous claim that a hearer is consciously aware, in the phenomenological sense, only of phonological properties, not syntactic ones (1987, 287–9); more on this later.) Notice, incidentally, that the claim is not this: that we’re not conscious of following the rules of the musical grammar. The concern here is not with unconscious knowledge of rules. The claim is rather that we’re not conscious of, as it were, the output of the grammar, the structural representations it generates.

So far, so good. The notion of mental representation that is emerging is a very familiar one in cognitive science: that of unconscious mental representations postulated for their explanatory value. And, as Jackendoff indicates, those representations are to be conceived of in the context of a computational theory of mind, on which mental representations in the brain encode information (where information content can be specified independently of physical instantiation) and are operated on by processes similar to those of computer algorithms (1987, 15–6).

From this description, moreover, a reader familiar with contemporary debates in cognitive science may feel a tug toward construing GTTM along the following lines. On some views of cognitive science, a cognitive theory need not import our ordinary, mentalistic conception of ourselves and our mental life, one that advert to beliefs, desires, conscious experiences, and so on, mental states for which verbal reports are taken to be a central source of evidence. On such a vision of cognitive science, folk-psychological notions are to be supplanted by ones more scientifically respectable—on some views, computational (argued for, in various forms, by Churchland 1979, Stich 1983, and Churchland 1986, and discussed by Dennett 1987). The reader may be apt to assimilate GTTM to this picture, or, at the very least, to ask whether it conforms to it. To be sure, Jackendoff nowhere himself explicitly endorses a conception of cognitive science hostile to folk psychology, as far as I know (cf. 1992, 169–72). But the question is about the treatment of music specifically, and it could still be the case that GTTM satisfies a certain conception of cognitive theory even if theories in other areas cannot. Given the characterization of the theory on the table so far, then—unconscious representations, inaccessibility to verbal report, the computational theory of mind—it is germane to ask how close GTTM comes to fit-
ting the profile of a cognitive theory in which folk-psychological notions are eschewed in favor of computational ones.

As I say, so far, so good. But things start to unravel when we ask: what are the data? How is the theory confirmed? How, in particular, is the ascription, to a listener, of a particular R-state (corresponding to a particular structural description) confirmed? One might have thought that such confirmation is always quite indirect—that the data thereby explained are behavior at a substantial remove from the states that do the explaining—but that is not so. One route of confirmation, with which I will be mainly concerned, is demonstrated by Jackendoff (1987). (What is central in the examples to follow is the appeal to “musical intuition.”)

[Fig. 3] gives the intuitively correct grouping structure for the opening of Mozart’s G minor symphony. … That these are not gratuitous formal impositions on the music is demonstrated by [Figure 4], which illustrates two incorrect ways of grouping the passage. One clearly cannot hear the passage as broken up in the fashion indicated in [Figure 4a]. Although [Figure 4b] segments the passage correctly into smallest-scale groups, the aggregation of small groups into larger ones is strongly in violation of musical intuition. In other words, our unconscious understanding of music enables us intuitively to choose a hierarchical segmentation, and therefore there must be means for mentally representing it (1987, 219; Figures 3 and 4 from pages 219–20).

And, in connection with an example we looked at earlier, Jackendoff writes,
The best way to understand [time-span reduction] is to attempt to hear the successive musical lines in rhythm. If the analysis is correct, each line should sound like a natural simplification of the previous one. …

As in the case of grouping, it is useful to present an example of an incorrect reduction [Figure 5], in order to show that real musical intuitions are at stake. … It should be intuitively clear that the incorrect reduction sounds “less like the piece” (1987, 225–7; Figure 5 from page 228).

Figure 5. Alternative time-span reductions, Mozart, K. 331, mm. 7–8. Reprinted from Jackendoff 1987, by permission of MIT Press.

One further kind of example is relevant. GTTM draws analogies between musical grouping and visual grouping (1983, 40–2; Figure 6 from 42). The way the shapes are seen as grouped together is similar to the way the notes are heard as grouped together; the visual patterns are perceived as similar to the auditory patterns.
The examples just given—I shall call them “illustrative examples”—are highly significant. They capitalize on and demonstrate the possibility of a listener’s confirming, from a first-person perspective, that he or she attributes a certain structure—one of the sort that is given in a structural description of GTTM—to the music. (This is consistent, of course, with the possibility of other kinds of confirmation.) In this kind of first-person confirmation, one recognizes that a given structural description correctly specifies one’s psychological state; one sees that the structure it designates is in fact the structure one attributes to the piece. The question, of course, is how this sort of recognition is possible.

But by now something should strike us as being a little bit odd. There is a serious tension between what Jackendoff says is possible and what he and Lerdahl actually do: if musical representations do not lead to verbalization, how were they able to show us (and how were we able to tell) what the “correct” ones are? And if GTTM’s levels, other than the surface, do not make their way to conscious awareness, how can it be that one passage will “sound like a natural simplification” of another, or that a musical passage will be perceivably similar in grouping to a visual pattern? If we can see which reductions are correct and which incorrect, how can the relevant mental representations be unconscious?3

Let’s try to sort things out—to take inventory. To begin with, there are the data for the theory, the things it is trying to explain. One important category of data consists of verbal behavior and its products: a listener’s saying, for example, that one passage sounds like a natural simplification of another, or that an auditory grouping resembles a visual one, or that a tree diagram corresponds to how one hears a piece. GTTM’s structural descriptions belong to

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3 That is, unconscious in Jackendoff’s sense. There is, I think, something important about GTTM’s representations he is trying to put his finger on, which he misdiagnoses as inaccessibility to conscious awareness. Later I will attempt a better diagnosis. I think the term “unconscious” is quite misleading here.
this category. But in addition to verbal behavior there are, I take it, mental or psychological states that underlie such behavior and are expressed through it; and I take it that these states, which comprise another important category of data for the theory, are the “musical intuitions” to which Lerdahl and Jackendoff appeal in the illustrative examples. But there is a curious omission in Jackendoff’s theory: for all the informal reference to intuitions, their nature and role in the architecture of the mind are never accounted for. They remain an unexplained residue. It remains to ask, then, what such intuitions could be and what kinds of mental or psychological states play the role of such intuitions, and to draw out the implications of this for our understanding of GTTM as cognitive science.

Now, possibly over and against the musical intuitions we have the R-states. I say “possibly” because it is never completely clear whether, on Lerdahl and Jackendoff’s view, intuitions (or certain intuitions) and musical representations are the same thing (see 1983, 3–4), constituting the explananda for which the musical grammar is explanans, or whether the representations—or R-states, as I am calling them—are supposed to lie behind and explain the intuitions, and hence are distinct from them (whereby R-states are explanans, intuitions explananda). So what we need to get clearer on is the relationship between intuitions and R-states, and what motivation there is for, or against, identifying them with one another.

Well, whatever else is true, intuitions must be conscious, by both verbal and phenomenological criteria. They are available to verbal report since reporting them is precisely what Lerdahl and Jackendoff do in the illustrative examples. (Moreover, a reader of GTTM is able to see that a description is in accord with intuition.) And to deny that intuitions are conscious in the sense of phenomenological awareness would, it seems to me, reflect a pointlessly restrictive conception of awareness. Tension and resolution, the closure at the end of a phrase or section—such structural properties are phenomenologically salient in as full-blooded a sense as anyone could want. How, moreover, are we able to recognize the equivalence, as Lerdahl and Jackendoff ask us to, of an auditory grouping to a visual one? Only by virtue of our awareness of similar patterns in each can they appear similar to us. Consider, too, phenomena of aspect switches, or ambiguity—in music, passages such as the beginning of Mozart’s D-minor Fantasia, which can be heard as

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4 Thus, here, “(musical) intuition” will be restricted to mental or psychological states of the kind that underlie and are expressed in verbal reports. Possibly Lerdahl and Jackendoff use the term more widely, but, if so, I will not attempt to take account of that wider usage.

5 This way of putting the issue owes to Raffman (1993, 49). Raffman allows that GTTM’s mental representations (which she calls “structural descriptions,” taking them to be mental tokens thereof) are, at certain levels, conscious (29) and verbalizable (31). Yet in her discussion of the explanatory structure of the theory, structural descriptions are limited to unconscious ones (49). This is needlessly restrictive: if there are conscious structural descriptions, surely their role should be considered.
subdivided either into threes or twos. The music sounds different from one hearing to another, showing that metrical structure is reflected in awareness.

Jackendoff makes the argument in the case of language that “the units of syntax—nouns, verbs, prepositional phrases, and so on—do not present themselves to awareness at all. We do not hear syntactic categories …” (1987, 288). Rather, all that is present to awareness, in his view, is phonological structure as comprised of words and possibly syllables. This too seems to me to be simply mistaken, at least in any phenomenologically robust sense of the terms “aware” and “hear.” It is evident from a phenomenological standpoint that sentences present themselves to us as having a syntactic structure, as built up, at least to a first approximation, out of things like noun phrases, prepositional phrases, dependent clauses, and so on. We hear them as having such a structure (cf. Harman 1973, 89–91; Stich 1971, 496; see also Fodor 1983, 53–7; but note that the type of structure phenomenologically attributed need not be the same as that given in a correct linguistic theory). This becomes apparent when we compare different readings of ambiguous sentences such as “Visiting relatives can be boring.” It follows that fairly abstract qualities may be present to awareness—ones that cannot be conveyed unambiguously by, say, mental images along the lines envisioned by Hume and other empiricist philosophers. Possibly, Jackendoff’s restrictive conception of what is present to awareness is motivated by an assumption that awareness must be imagistic in this way. If so, that assumption is not compulsory.

Musical intuitions, then, are conscious by both criteria: being available to production or recognition of a correct verbal report, and conscious in the phenomenological sense. Now, given that Jackendoff claims that musical representations other than surface ones are unconscious in both senses, it looks like we ought to distinguish them, or R-states, from intuitions. On that picture, R-states—themselves unconscious—somehow lie behind, and explain, the conscious intuitions. But no sooner do we suggest this picture than we must see it is untenable. R-states cannot be unconscious. For the author or reader of GTTM is able, reliably enough, to recognize the correctness of an R-state ascription: that ability is what the illustrative examples trade on. Patently, those states are conscious by the verbal reportability criterion. (Perhaps it is not particularly easy to put them into words, or perhaps it’s easier to recognize correct reports than to generate them, but all that’s a far cry from calling the states “unconscious.”) And if the claim is that the R-states do not themselves support awareness, but lead reliably to intuitions that do—in opposition to the simpler claim that the R-states themselves support awareness—this is a distinction without a difference. Why not just call the R-states conscious? It is hard to see what the point would be of postulating states that are themselves not conscious, but reliably bring about other states that are, unless more is said to indicate the rationale or evidence for the distinction.
Thus, in light of the fact that GTTM is susceptible to first-person confirmation, the conception of R-states as unconscious is insupportable (in the verbal and phenomenological senses of “conscious”; and if some other sense ought to apply, it is not apparent in Jackendoff’s text). Thus, R-states, contrary to advertisement, are not plausibly construed as unconscious computational states of the brain. Rather, they must be taken to be conscious mental states, tied to language and perceptual awareness.

In fact, it looks like the thing to do is simply to collapse the distinction between intuitions and R-states. For if we keep the distinction, R-states must be seen as lurking behind the intuitions, somehow explaining them. But it is hard to see how this would genuinely explain anything. There is simply not enough independent grasp on R-states, versus intuitions, to give substance to an explanatory connection between them. We simply haven’t much handle on the notion of attributing a structure, beyond that of having the relevant intuition (or, perhaps, being disposed to have it), in a way that would give some point to explaining one thing in terms of the other. R-states and intuitions, for all that has been said so far, are isomorphic. What is the point, then, of postulating duplicate realms? We would need a reason, and we have seen none. Nevertheless, I shall resist collapsing the distinction between the two for the time being, since a motivation for it may emerge as we proceed.

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What is going on when the illustrative examples work—when they succeed in demonstrating what they do? At a minimum, it seems to me, the following must be true. The reader/listener must have an adequate idea (though not necessarily a full theoretical understanding) of what the analysis is saying—what musical structure it designates—and thereby must be able to detect a certain matching relation between his hearing and the analysis (by virtue, presumably, of detecting the appropriate relation between the hearing and the structure the analysis designates). The relevant ability would thus depend on a certain kind of cognitive sensitivity, evidenced by the ability to pick out matching analyses: but possession of that ability would not entail that the listener thinks of hearing and analysis as matching, or that he hypostatizes hearings, or perhaps even that he is self-reflective enough to think of himself as a perceiver or conscious subject.

But typically a listener who can pick out analyses that correspond to her hearing will have conceptual resources that transcend the minimal level just

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6 I would apply much the same criticism to Raffman’s view (1993, 50) that musical feelings (of strong and weak beats, etc.) are explained by unconscious “structural descriptions.” The feelings she cites are so articulate, so close to the structural descriptions themselves, that the distinction between them dissolves.
indicated. One will be able to think of oneself as *hearing a passage as a such-and-such* (where an analysis or analytical description, e.g., “expansion of the subdominant,” takes the place of “such-and-such”), and to think of others as hearing the passage in that way, and to think of oneself and someone else as hearing the passage in the same way. One will thus have a certain conception of a certain class of mental states enjoyed by oneself and others, states one typically attributes via expressions of the form “… hears passage as a such-and-such.” I call such expressions *hearing ascriptions* (though often, in practice, only the analysis is given, “hears” being understood). Such a conception of hearing—hearing a piece or passage along the lines of a certain analysis—plays an essential role in music-theoretic communication, where analyst and reader understand an analysis as expressing the analyst’s way of hearing a piece, or as recommending how reader should hear it. (Which is not to say that musical analysis is always limited to this role.) It is precisely this ability to conceive of our own and others’ hearing in analytical terms that Jackendoff’s account fails to explain.

By way of further illustration, consider Lerdahl and Jackendoff’s distinction between “structural importance” and “surface salience” (1983, 108). Structural importance determines which events belong to simpler reductional structures, and is not to be conflated with mere surface prominence. But then the requirement that a reduction must sound “like the piece” (1987, 227) must be qualified, crucially, as *sounding like the piece in respect of structurally important events*, not sounding like it merely in respect of salient surface events. What is it, now, that I understand to be going on in you, when I take you to hear a reduction as sounding like the piece in a “structural” respect?

Jackendoff’s meta-account has no real answer to this, and no satisfactory answer is possible, it seems to me, given his assumptions. His account does not satisfactorily account for our understanding of what it is for you and I to pick out the same way of hearing by the same words. For on Jackendoff’s conception of semantics, the meaning of a word is a mental structure, and he rejects any conception of semantics on which the latter would tell us how representations, mental or otherwise, relate to the world (1987, 128–33). Rather, the operative notion—what functions as a primitive in his philosophical view—is that of intentionality as a relation between mental states and the world “as experienced” (128). But on what it is to be related to this rather than that world-as-experienced, Jackendoff’s account is silent. He leaves unanalyzed the notion of being related to a world-as-experienced, yet that notion is at the very heart of the problem of intentionality.

It should be noted that Jackendoff’s conception of mind, though computational in some sense, is not the sort of computational account that exemplifies the “syntactic” theory of mind advocated by Stich (1983, 149 ff.), or one dealing only with “uninterpreted formal symbols” (Pylyshyn 1980, 161, quoted in Stillings et al. 1995, 359, see 356–60). Jackendoff’s account presupposes full-blooded intentionalistic notions (or, at any rate, as full-blooded
as a “world-as-experienced” implies). But the core notion of intentionality, when all is said and done, is simply left unanalyzed. (Conceptual structures are built out of elements according to Jackendoff’s theory, and of course that is analysis in a sense. But what goes unaccounted for, crucially, is the intentionality of the elements.)

And though Jackendoff invokes functionalism as the operative conception of the mind (1987, 15), the notion of content is never cashed out in a functionalistic way here: what it is to have this content, rather than that, is not analyzed in functionalistic terms, either information-theoretic (i.e., wide-functionalist, Dretske 1981, Stalnaker 1984, Harman 1987) or narrow-causal (discussed in Stich 1983, 48). A wide-functionalist account would bring in causal relations to the world, but that is exactly the sort of realist semantics Jackendoff eschews. Nor does Jackendoff provide any narrow-causal account, i.e., one in terms of internal causal relationships in the head (or organism), of what it is to, for example, hear a note as structural or salient.

Thus, although Jackendoff does not explicitly say so, what seems to be presupposed, by default, is that we understand what it is for you and I to have type-identical mental representations “from our own case”: what I think, when I think of you as hearing a piece as having a certain grouping structure, is that you have this sort of hearing, i.e., a hearing of the sort that I am having right now. Wittgenstein’s criticism of this conception of our understanding of the mental states of others is well known and, I think, exactly on point: absent a conceptual link to behavior, I can have no understanding of what it is for your mental structure to be the same as, or different from, mine (Wittgenstein 1958; discussed in Dancy 1985, 71–3). Jackendoff’s metatheory provides no account, then, of the distinction between the listener’s following a rule and his merely thinking he is following a rule, as he decides whether the illustrative examples correspond to his hearing.

Thus, what is absent from Jackendoff’s metatheory is a basis for understanding the sort of communication that goes on between music analysts, or between music analysts and their readers, since it cannot account for what it is for them to speak a public language. We should recognize here that GTTM’s vocabulary and conceptual repertoire are not cut from whole cloth, made to order for cognitive science, but derive from a pre-existing tradition of musical analysis in which self-reflective musicians communicate with one another about what and how they hear. Oddly, this well-established tradition of verbalization about what one hears—which, depending on how one draws the lines, goes back at least a couple of centuries (Koch 1787/1793)—has no clear place in Jackendoff’s metatheory. GTTM is not just a theory in cogni-

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7 Jackendoff does suggest at one point that conceptual semantics can be supplemented by a story about how mental representations relate to the world (simpliciter, as contrasted with a world-as-experienced; 1987, 132). What he apparently has in mind is an information-theoretic account, on which the content of mental representations derives from that of sensory input. But he does not develop this idea within his own theory.
Musical Intuition

That GTTM’s mental representations are ones about which listeners can communicate, constrains in certain ways what conception we should take of those representations and their contents. For these will be states characteristic of a language user, ones linked to verbal behavior. And because GTTM inherits its conception of mental representation, at least in large part, from the music analyst’s notion of hearing, that conception (GTTM’s, not just ours looking in) is a conception of mind of a sort characteristically possessed by a language user. Where one has such a conception, one’s notion of the intentional properties of a mental state is parasitic on one’s grasp of semantic properties of language: one’s understanding of what way of hearing it is, for something that is a hearing of an event as structurally important or surface-salient, is parasitic on one’s understanding of the terms “structure” and “salient” (or terms synonymous with those). And to see that GTTM inherits such a conception of mind is crucial to understanding what sort of theory it is. It is not cognitive theory of a kind that eschews common-sense mentalistic notions, since the conception of mind it inherits is indeed folk-psychological, specifically, that of the music-analytical folk.

Two points, before we go further, to fend off possible misunderstanding. First, my remarks are not directed toward criticism of GTTM itself. What I am urging, rather, is that we should arrive at a construal of GTTM different from Jackendoff’s: GTTM needs a more philosophically satisfactory meta-theory. Second, I am not arguing that GTTM lacks “operational definitions” for its terms. I am not advocating philosophical or psychological behaviorism, which would take too rigid a view of the relationship between the truth conditions for a psychological description and the possible evidence for it. My point, rather, is that Jackendoff’s meta-account does not tell us enough about a certain kind of evidence for GTTM’s psychological descriptions, and does not work out the implications, of the fact that it is evidence, for what sort of theory we should take GTTM to be and what conception of mental representation it implies. Most peculiarly, his account points to such evidence without officially making room for it (as we would expect a comprehensive account of mind, like his, to do). The fact that ways of testing GTTM experimentally have to be, and are being, developed by cognitive psychologists (Deliège 1987, Deliège and Sloboda 1997) should not lead us into thinking that the theory can, in one phase, be stated and mean what it means in complete independence from what is to count as possible evidence, and

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8 There is a fairly sharp contrast with the case of language here: it is not generally taken to be a constraint on syntactic theories in language that competent speaker-hearers be able to recognize correct structural descriptions in the terms of the theory in question. Structural descriptions are “in the data” to a far greater degree in GTTM than in syntactic theory.
that then, once it is given in this way, experiments can be devised to test it. Rather, GTTM already has bound up with it certain things that count as evidence for it, namely musical intuitions, and any satisfactory construal of what the theory is saying must take this into account. (And to take the concept of a kind of thing to be specified in part by reference to what those things are causally related to, and hence what is evidence about them—as I do—is not to embrace strict behaviorism, but a more moderate functionalism.)

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GTTM, as well as the larger music-analytical language to which it owes its lineage, need interpretation. What is needed is an account of the semantics of music-analytical language, together with an account of the nature of the psychological states involved in music perception (including an account of their semantic properties, if any). We will want to know about the semantic properties of terms such as “duple meter,” “hears passage x in duple meter,” “sounds duple,” etc.: at the very least, we will want an account at the level of reference or satisfaction, an account that tells us what sort of things such terms refer to or under what kinds of circumstances they are satisfied. And we will want an account of what sort of psychological state hearing a passage in duple meter is, in particular, whether it is a state that has semantic properties, or intentionality. What is more, we will want our account to elucidate the relation between the semantic properties possessed by the words and those possessed by the psychological states. In what follows, I hope to set us down the road to such an account.

We may start with this question. Which is the more basic notion: that of hearing a passage in a certain way, or that of a passage’s being a certain way? Which is conceptually and/or semantically prior to the other? (Cf. Smith 1986, Peacocke 1983, 28–30.) Depending on the way we think those questions should be answered, we will incline toward one or the other of the following two main stances toward matters of semantics and intentionality; I call these representationalism and sensationalism, respectively. The first of these takes it that a psychological state involved in the perception of music has a semantic value—that it has a content. On the representationalist view, then, a perception of musical structure—for example, perceiving a passage as being in duple meter—represents the music as being a certain way. To say it represents the music as being a certain way is to be understood as implying that it is intelligible to speak of the music’s being, or not being, that way; hence, representationalism conceives of a representation as having a truth
condition and therefore evaluable as true or false. What is sketched here is a view one may take toward one type of psychological state or another, e.g., GTTM’s intuitions or R-states. “Perceive” serves as a neutral term in this regard. Of course, one may be a representationalist with respect to some kinds of states in music but not others, but I will not explore that option here. The main alternative I see here, which I call sensationalism, does not take musical perception to be representational in character, but rather thinks of it in analogy with, say, pain.

What relation does each of these views, respectively, take to hold between an analysis of GTTM, i.e. a structural description, and a corresponding psychological state, i.e. intuition or R-state? The representationalist has a conception of the semantics of music-analytical language on which “is duple” is explanatorily prior to “hears x as duple” (or “sounds duple”). For the representationalist, “is duple” refers to a property of passages, or sounds, out in the world; “hears as duple” refers to a type of state in the head. The representationalist holds, moreover, that an analysis gives the content of the corresponding mental state. On the representationalist view, analysis and psychological state are each representations of a musical passage—symbolic and mental representations, respectively. The analysis represents the passage as being a certain way, and that specification is put to work as a characterization of the psychological state in turn, to say how it represents the passage as being. In this way, one representation is used to characterize the representational power of another. (This is a “secondary” use of the representation: in the first instance representations represent things, not other representations of those things.) The relation of analysis to psychological state that obtains on the representationalist view may be seen as an instance of a wider phenomenon, that of “giving the meaning,” or content, of a representation, and in this respect like belief attribution, indirect quotation, and giving the meaning of a sentence or term via another.

Cf. Peacocke, who requires a “correctness” condition (1992, 64–6). More properly, what is represented as being a certain way are sounds, that is, one has a representation as of sounds—certain events in the environment—having certain properties. Note that one’s experience may be hallucinatory or imagined—there may be no such sounds—yet representational. A hallucination or imaginary hearing has content in that it represents how the world would have to be in order to make the representation true. Jackendoff apparently overlooks this point in arguing that hallucinations and mental images cannot be accommodated by a semantics that accounts for meaning in terms of relations to the world (1987, 19–20).

Pace intentionalists about pain such as Armstrong (1968). The opposition between representationalism and sensationalism in this respect can be characterized, more precisely, in terms of how type-identity conditions for the relevant states are to be drawn. Are those condition to be given in terms of some representational content attributed to the states? The representationalist says yes, the sensationalist no.

Here we, as interpreters, “go native”: we adopt the object language for purposes of mentalistic description. (To say that “duple” gives the content of state S is to mention the word, not to use it, but nonetheless we are thereby committed to the correctness of a characterization of the content of S in the terms of the object language.)
The form of words that conveys a representationalist view most transparently is one in which a terminological distinction is made between a predicate “F” applied to musical passages and “hears x as an F” predicated of listeners. But things are not always so neat: musicians often speak of hearing a passage in duple meter (without the “as”), and it is possible for words or symbols do double duty, to refer variously to passage-types or hearing-types.

The sensationalist, in contrast, regards the relevant features of musical perception in a way that does not attribute to them a representational content. On a natural and plausible formulation of the view, to be in a psychological state of a given type—to hear an event as a strong beat, for example—is to have an experience with certain sensational properties, in Christopher Peacocke’s terminology (1983, 5); it is not a matter of representing the music as being any particular way. On such a view it would be natural to take expressions in the language of GTTM—“strong beat,” say—to refer to sensational properties of this sort. What is basic on this conception is the listener’s being in some subjective state—how one “hears,” in at least a loose sense of the term—rather than the music’s being some way or other.

Jackendoff’s stance with respect to the issue of representationalism versus sensationalism is rather hard to make out. On the one hand he writes, “one is not conscious of [an information] structure but rather of the aspect of the world that the structure represents.” One naturally reads this as expressing a representationalist position, since presumably we are entitled to suppose that either the world has the aspect attributed to it or it does not, whence the attribution is true or false. But on the other hand he asserts, “it hardly makes sense to say that the representations one constructs in response to hearing a performance of the Eroica are true or false” (Jackendoff 1992, 165), and that is inconsistent with representationalism. The problem that faces us in attempting to locate his view within the given dichotomy is that he does not share the assumption that representation entails having a truth condition. But fundamentally, what “hardly makes sense” is the notion of something’s being a representation at all—of its characterizing the Eroica as being any particular way—without there being a concomitant notion of the Eroica’s being that way, and hence of the representation’s being true or false. The notion of a representation’s content cannot be intelligibly separated from the notion of what would make it true, i.e., from its truth conditions. Jackendoff wants to have it that (at least some) representations do not have truth conditions, which, as far as I can see, is to abandon the notion of representation in those

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12 Though it is not obligatory: one might instead take such terms to refer to dispositional properties of passages to cause listeners to have experiences with certain sensational properties.

13 Jackendoff 1988, 209; the view is similar to that of Harman to be discussed. In a revised version this reads, “one is conscious … rather of that aspect of the perceived world that the structure encodes” (1992, 89). For reasons I shall not go into, the change does not affect the point made above.
cases. In any event, Jackendoff adduces no real argument in support of the claim that musical representations cannot be true or false. What he does adduce is an argument that propositions about musical structure, like linguistic structure, are mind-, or society-, or culture-dependent, but as he acknowledges elsewhere, it does not follow from this that such matters do not admit of truth or falsity: propositions about “ownership, marriage, tenure, or baseball” have truth conditions (1987, 131). At any rate, on balance, I read Jackendoff as a sensationalist.

To restate, then: on a representationalist view of the perception of musical structure, a listener’s psychological state represents the music as having that structure, whereas the sensationalist attributes no representational content to the state. How are we to decide the issue? Consider the experience of such features as grouping, metrical organization, tension and resolution, and so on: patently, we are aware of is restricted to features of the intentional object, rather than the intrinsic properties of our mental representations (or other non-representational properties of our minds or brains). Short of doing neuroscience, we are aware of represented, not representing, qualities (Harman 1990 calls the latter “mental paint”). This is precisely what the sensationalist must deny in the case of musical perception: the sensationalist must claim that, in hearing structure, we are aware of features other than represented ones. A sensationalist (of Jackendoff’s stripe) will thus take the musical structure we hear not to be something that can belong to a musical passage or sounds external to us, but will locate that property in the head: it is the mental representation that is, as it were, the bearer of structure.

A prima facie problem for the sensationalist is that such features as cadential closure, metrical organization, tension and resolution, and so on are phenomenally located in the music: they are “phenomenally objective” (Beardsley 1981, 37). Structural features coalesce with the field of objects and properties that are represented in our experience; but it is hard to see how they could do so if such features belong to mental representations inside our heads (and are not themselves represented). The sensationalist has two choices here: either he can tell a projectivist story, on which we “gild” our representation of pitches and rhythms (which for the sake of argument I as-

14 Note that the claim that some representations do not have truth conditions is much stronger than the methodological tack Jackendoff takes elsewhere, that semantics ought to be conceived of as the study of internal symbol systems rather than truth conditions or relations between symbols and the world (1987, 128–33). Given, though, that facts about complex social interactions help to determine the truth conditions for sentences about baseball, it is not clear how a theory restricted to internal symbol manipulation is going to be able to tell us much about what it is in virtue of which those sentences mean what they mean (for it will be apt to presuppose, rather than explicate, the relevant intentionalistic notions), nor is it clear why mention of such (socially-determined) truth conditions would not enter into an illuminating semantical account of those sentences.
sume are represented) with subjective, non-represented qualities (cf. Hume 1975 [1777], Appendix 1, quoted in Boghossian and Velleman 1989, 96). Or he can embrace sensationalism across the board, denying that features we ordinarily take to be represented—shape, motion, extension, and so on—are in fact represented. This would be in effect to deny that there is any such thing as mental representation, at least in the sense I have adumbrated.

Now perhaps neither projectivism, nor an across-the-board sensationalism, can be refuted outright. But there is a much more straightforward, representationalist view on which, first, such features as grouping, meter, and tension are features of the music (where this is taken to be something outside of our heads), and, second, our experience represents the music as having those features. Such a view is plausible on phenomenological grounds and, moreover, is compatible with a plausible semantical story about music-analytical language that takes into account how those terms are learned and used. Terms such as “duple meter” are introduced as monadic predicates that apply to musical passages, not to persons or persons-at-a-time. Musicians learn to apply those terms in contexts of perceptual acquaintance with those passages (though not only in perceptual contexts)—they train their ears. Such terms come to have reasonably determinate extensions: musicians learn that “Twinkle Twinkle Little Star” gets the term “duple meter” and “My Country ’tis of Thee” “triple,” that Baroque dance movements are usually binary and da capo arias ternary, and so on. Thus, in the first instance, terms for musical structure are terms for properties of passages, not for psychological or subjective states; only subsequently does there arise a secondary use of such terms in connection with perception and experience, as in talk of hearing a passage as duple. Considerations of simplicity, economy, and plausibility militate in favor of this representationalist view, versus projectivism or any version of sensationalism.

Thus I favor a representationalist account of music-analytical terms, of the psychological states such terms can be used to identify, and of the relation between words and psychological states. On this account, a term “$F$” refers to a property of musical passages; the correlative term “hears … as an $F$” picks out a psychological state type; and states of that type refer to, are representations as of, the property $F$ (i.e., the same property to which “$F$” refers). (“Property,” here and elsewhere, should be regarded as a promissory note for a level of content to be discussed in the next section.)

Given this view, it is natural to hold that some of the states germane to musical perception here are perceptual beliefs. The notion of perceptual belief I have in mind here is a functionalist one: a perceptual belief (say a visual belief) of the sort, Here’s a brown cow, is one that is typically caused by brown cows in appropriate ways in appropriate circumstances, that makes me disposed to say that a brown cow is before me, and so on. A perceptual (here, auditory) belief of the sort, Here’s a passage in duple meter, is one that is typically caused by duple-meter passages in appropriate ways in ap-
propriate circumstances, that makes me disposed to label the passage as dup-
ple, and so on.¹⁵

What do GTTM’s psychological states—intuitions and R-states—turn out
to be on this picture? Let’s start with intuitions. It seems to me overwhelm-
ingly plausible that at least some of the “musical intuitions” to which Ler-
dahl and Jackendoff refer, and to which the illustrative examples appeal, are
perceptual beliefs.¹⁶ In cases such as those of the illustrative examples, there
is, correlative to the analysis “duple meter,” a certain kind of perceptual be-
belief, one of the (auditory) sort Here’s a passage in duple meter, on which our
recognition of the analysis as corresponding to our hearing typically de-
pends. I will call a perceptual belief that corresponds to an analysis in this
way a “matching belief” (i.e., one that matches the analysis). My claim is
that when we see that “duple meter” correctly describes the way we hear the
passage, this depends, crucially, on our having a matching belief.

One virtue of this story is that it opens the way to a satisfactory account of
what is going on in the illustrative examples. Plausibly, a correct account
would go something like this: When an analysis (“duple meter”) seems to
you to capture how you hear a passage, typically (1) the analysis represents
the passage as being in duple meter; (2) you have a matching belief, which is
a perceptual belief that the passage you are hearing is in duple meter; (3) you
recognize, in some way or other, the congruence of what the matching belief
represents and what the analysis represents (because you understand what
the analysis says about the passage, you know you are disposed to express
your belief in the same words, etc.). Though much needs to be filled in in
this account, it is at least intelligible in broad outline, since it is grounded in
mechanisms of public language.

Let us turn now to the R-states, the psychological states that according to
GTTM embody cognition of musical structure. As representationalists, we
will want to say that R-states, like matching beliefs, have a content; and we
will take it that an analysis that specifies a certain type of R-state gives its
content: thus, the content of a listener’s attribution of duple meter to the mu-
ic is given by the words “duple meter,” or by an equivalent graph of GTTM.
But are such states perceptual beliefs? Are they, moreover, indistinguishable
from and therefore to be identified with matching beliefs? Or should we

¹⁵ Harman 1973, 1987. In the above, “passage” should be read, more strictly, as “sounds in the
environment.” Another point: as Peacocke (1983, 5–6) reminds us, a distinction needs to be
made between the notion of an experience with a certain content and that of a judgment with
that content, since one may fail to take one’s experience at face value (as when one views the
Müller-Lyer diagram knowing it is an illusion). That is true, but since those considerations are
remote for present purposes, I will not worry about the complication. Normally, listeners do
not fail to take their experience at face value, and hence they do have the sorts of perceptual
beliefs I invoke here.

¹⁶ This parallels Stich’s view of the “intuitions evoked by linguists … [as] perceptual judg-
ments” (1971, 496).
think of R-states as giving rise to matching beliefs, as related to them causally?

Before we pursue these questions further, it will be useful to return briefly to the issue of what sort of cognitive theory GTTM is and what notions of mental representation it trades on. We noted earlier that GTTM’s conception of mind—of hearing—is inherited, in large part, from music analysis. It is, by and large, a conception of hearing that an analytically inclined listener, a participant in that language community, will have. Now, whereas earlier I invoked functionalism merely to import the notion of a perceptual belief, let me invoke it again as a theory of the listener, as a reconstruction of his concepts of the mental. And let me invoke representationalism once more, for if its account of the semantics of music-analytical terms such as “duple meter” is correct, so far as it goes, then it is a correct account, so far as it goes, of what someone who understands those terms understands them to mean. If we put these together, we will reconstruct the listener’s notion of hearing a passage as in duple meter as one defined in terms of causal relations to duple-meter passages and duple-meter labelling: it will be the notion of a perceptual belief as the philosopher reconstructs it, or at least one close to it. Such a conception of the psychological is of a sort internal to a linguistic practice, in which talk about objects and events becomes extended to talk about thought and perception of objects and events; it is a conception on which the notion of the intentionality ascribed to a hearing has a conceptual dependence on the notion of the semantic properties of the words (e.g., “duple”) used to index, and give expression to, that hearing.17

This, I submit, is the conception of hearing, of mental representation, that GTTM carries with it in connection with R-states that the listener can self-ascribe. And, patently, the notion of content thereby imported is a common sense, folk-psychological notion.18 Thus, to reiterate a point made earlier, GTTM is not cognitive science of any sort that eschews folk-psychological notions of content in favor of computational or other scientifically progressive constructs; a common sense conception of mental content is, rather, presupposed in the theory from the start.

17 This view is indebted to Sellars (1963, 186–9). Sellars claims, moreover, that one conceives of intentional states on the model of language; thus the semantical categories of language are prior to the intentionality of mind (180). Note that the conception of content expressed above is consistent with the possibility of ascribing content of some other kind, e.g., information-theoretic content (Woodfield 1986), to mental representations of music; it is just that GTTM does not ascribe content of some other kind.

18 It could be objected that many of the concepts of music theory in terms of which such content is ascribed are unknown to most people. But it does not follow from this that the attendant conception of intentionality is not folk-psychological, because what is relevant is how such notions are put to work toward intentionalistic description, not the degree of familiarity of the theoretical notions themselves.
Let us return now to the question of the relation between intuitions and R-states, in particular, the intuitions we have called matching beliefs. (Recall that the matching belief for the analysis “F” is a perceptual belief, of the form Here’s an F, in virtue of which one is disposed to recognize that the analysis correctly describes one’s hearing). Are we to maintain a distinction between R-states and matching beliefs, taking the relation between them to be a causal one, or—if they are for all intents and purposes isomorphic—should we simply identify them with one another? Earlier we could make no headway with this; here I shall suggest a motivation for distinguishing them.

Lerdahl and Jackendoff’s illustrative examples, and their analyses in general, have a highly interesting property. They are not only true (as I assume them to be), but illuminating. An analysis of GTTM is a correct description of one’s hearing and at the same time goes beyond it: it is revelatory of one’s hearing. It articulates, in a way that conveys new information, the intentional object of one’s hearing (see Walton 1993).

It is not simply that the analysis gets you to hear in new ways. Often it does, to be sure, and that is surely an important function of musical analysis. But what I am calling attention to now is something else: the analysis purports to capture, and typically does capture, a way of hearing that one enjoyed prior to encountering the analysis; and at the same time it illuminates that way of hearing. Forthwith, I will let R-states play the role of those pre-existing hearings (since it is convenient to have them play this role), and shall speak interchangeably of R-states and hearings.

The situation then is this: you are presented with an analysis—say a graph of GTTM that represents the passage as being in duple meter—that seems to capture your hearing and at the same time seems revelatory of it. (For orthographic convenience I will pretend that the analysis consists of the words “duple meter,” but in what follows the reader should substitute one of GTTM’s metrical grids for those words, at least mentally.) By hypothesis, you are in an R-state of the type specified by the analysis: you hear the passage as being in duple meter. And, as hypothesized earlier, you have a matching belief—a belief that the passage you are hearing is in duple meter—on which your recognition of the analysis as correct depends.

What I want to argue now is that, in a situation such as this, the matching belief (call it “B”) and the R-state (call it “R”) do not have the same conceptual content and are therefore distinct from one another.

I shall assume here that the analysis (“duple meter”) has a conceptual content, built up out of the concepts expressed by its words and symbols (concepts of meter, strong and weak beats, and so on). I assume that B has the
same conceptual content as the analysis, and that the thought\textsuperscript{19} \textit{That passage is in duple meter} has that conceptual content as well. I do not assume that \( R \) has a conceptual content; but, if it does, then I assume it functions as a belief in one’s cognitive economy (this last assumption is dispensable, but convenient).\textsuperscript{20}

I take it to be a constraint on conceptual content that if you believe that \( x \) is \( F \), and if the thought that \( x \) is \( G \) is informative to you, then \( F \) and \( G \) cannot be the same concept.\textsuperscript{21} (A Fregean notion of cognitive significance is put to work here as a condition on concept individuation; cf. Frege (1980 [1892]).)

Here then is the crux of the matter. It is possible for you to be in state \( R \)—to hear a passage as being in duple meter—and for the thought \textit{That passage is in duple meter} to be informative to you. Even as you are attentively following a piece, tapping along with it—so if \textit{GT TM’s description} ever applies to anyone, it applies to you—it can be informative to you \textit{that it is duple meter} you are tapping. Now either \( R \) has a conceptual content or it does not. If it does have a conceptual content, then, by the principle stated, that content is distinct from that of the thought \textit{That passage is in duple meter}, hence distinct from that of \( B \). If \( R \) does not have a conceptual content, it cannot have the same conceptual content as \( B \). Hence, either way, \( B \) and \( R \) do not have the same conceptual content. They are conceptually inequivalent.\textsuperscript{22}

That the matching belief has the same conceptual content as the analysis is part of the explanation of why we accept the analysis as correct; that matching belief and R-state are conceptually inequivalent is part of the explanation of why the analysis is illuminating, of why we have an “Aha!” experience. In the course of recognizing that the analysis is correct, we make a certain inference or mental transition from R-state to matching belief; hence the two cannot be the same state. Now it might be argued, in response, that the location of the interest has been misidentified, in the following way. A distinction can be drawn between first-order thoughts, such as \textit{That passage is in duple meter}, and second-order thoughts, such as I hear that \textit{passage as being in duple meter}. It might be suggested that the analysis is illuminating only because the second-order thought is informative to someone in state \( R \), and moreover that the first-order thought is not informative. But that would be a mistake. A conceptual inequivalence obtains between hear-

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\textsuperscript{19} As distinguished from the perceptual belief.

\textsuperscript{20} One reason it is necessary to specify this is in order to disarm the following complication. A perceiver does not always take his experience at face value (see n. 13), so, for example, one can see a pair of lines as unequal but, thinking them to be a Müller-Lyer diagram, not believe them to be unequal, and then find it informative that they \textit{are} unequal. I shall leave this complication aside.

\textsuperscript{21} This is derived, with alteration, from Peacocke 1992, 2. Note that what must be informative here is that \( x \) is \( G \), not that the sentence \textit{‘x is G’} is true. (If you don’t already understand ‘\( G \),’ it could be informative to you that ‘\( x \) is \( G \)’ is true even though you already believe that \( x \) is \( F \).)

\textsuperscript{22} Note that this does not imply that either has a conceptual content.
ings and thoughts at the first-order level: that is precisely what the previous argument showed.

This point about conceptual inequivalence leads, I think, to some illumination of why one might be tempted to call GTTM’s musical representations, or other musical perception, “unconscious,” as Jackendoff does. Someone who is in a certain R-state but does not have the matching belief B fails to represent structural features of the music via the music-theoretic concepts that enter into B’s conceptual content (and are expressed in the analysis), and hence fails to be conscious of the structure via those concepts. So, in some sense, one fails to be conscious (is “unconscious”) of the structure. But it does not follow, from the fact that we are not conscious of those structural features via those concepts, that we are not conscious of them in any way at all, or that we are not conscious of them via other concepts. To speak of such representations as “unconscious” simpliciter, then, is apt to be misleading at best, and—if one means to imply that they do not involve awareness in any way at all—simply incorrect, it seems to me, in the case of GTTM’s representations. I suggest, at any rate, that this is the source of Jackendoff’s misdiagnosis: he conflates not being aware of a structure via certain music-theoretic concepts with not being aware of it at all.

It is interesting to note that a version of the paradox of analysis arises here. The analysis, we have said, is a linguistic representation of the musical passage; the hearing is a mental, perceptual representation of it. Each bears information about the musical passage. Now on the one hand, one wants the analysis to be faithful to the hearing. It would seem that the analysis is faithful to the hearing just insofar as it hews to the information borne by the latter; and insofar as it strays from that information, it fails to capture the hearing, either through introduction of a foreign element, or through omission. So it looks like what we want is for the analysis to bear just the same information about the music that the hearing does. (Of course, there will be much information borne by the auditory experience—concerning timbral qualities, for instance—that the analysis is not intended to capture. Such information is excluded from the requirement.)

On the other hand, one wants the analysis to be illuminating. Music theory journals are filled with such analyses, and what sustains their interest is that one feels one has gained insight into one’s hearing. Contrast this with the case of ordinary statements about belief such as “I believe that Bill Clinton is a Democrat”: there is no comparable “Aha!” there, no sense that, thanks to the report, one is enabled to appreciate more fully just what one’s belief about Clinton is, comparable to the way musical analysis enables one

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23 Conversely, some of the information in the analysis, e.g., concerning specific pitch locations, is not meant to enter into the scope of the ascription. The presence of such information is not to be regarded as counting against the correctness of the analysis.
to appreciate more fully what one’s hearing is. But then the analysis bears new information, information not already contained in the hearing. The paradox, then, is that the analysis must be true to the hearing and at the same time go beyond it. This is reminiscent of, or a version of, paradoxes of conceptual analysis and paraphrase. In the example “A bachelor is an eligible unmarried male,” if analysans and analysandum are the same concept then the analysis must be trivial and uninformative, but if they are not then the analysis must be incorrect (Langford 1942, Ackerman 1992).

I suggest that here we should distinguish two levels of content (which, in the above, were conflated as “information”), one of which is preserved or captured from hearing to analysis, the other of which is not. The analysis gives the content of the hearing (the R-state) on the first level, though not on the second. Contents on the second level, I have already suggested, are to be individuated by a Fregean criterion of cognitive significance. But what is the first level, the level at which the analysis gives the content of the hearing? Some philosophers, such as Stalnaker (1984, 23), have suggested that for an adequate account of perception and thought it is sufficient to have a notion of mental content that distinguishes alternative possibilities but cuts no finer; let me call that a coarse-grained notion of content. (Equivalently, we can say that a coarse-grained notion does not distinguish between necessarily equivalent alternatives; for such contents, necessary equivalence entails identity.)

But it is evident that the level of content ascribed by music analysis must be finer-grained than this. We have all listened to the steady, intrinsically undifferentiated ticking of a watch or a metronome, hearing the ticks sometimes in twos, sometimes in threes. Those experiences, which correspond to different metrical representations, do not represent different alternative possibilities in Stalnaker’s sense: any possible world consistent with one experience is consistent with the other. And the point holds for many other examples of structural organization in music, for example, hearing a passage as an elaboration of one Schenkerian background structure or another (see Schenker, 1979 [1935]). Therefore, perceptual organization cannot be captured via coarse-grained content. Musical phenomena show, in this way, the general need for a notion of perceptual content that cuts finer than necessary equivalence. I’m not altogether sure how surprised we should be by this result, but one might have supposed that, in a certain central, phenomenal sense of “looks,” the way the cup looks, when it looks half empty, is no different from the way it looks when it looks half full. Whether or not this is true about the looks of cups and their contents, such a notion has little to

24 But the case is importantly different, too, from another kind where the “Aha!” response is also absent, one where a person utterly fails to recognize a rule of syntax that, according to a presumably correct linguistic theory, he or she knows unconsciously. I am indebted to Martin Davies for discussion of this point. Hearings lie at an interesting midway point between the obvious and the unrecognizable.

25 The example is discussed more fully in DeBellis 1995, 50–1.
recommend it in the musical domain, where, on the contrary, differences between hearings indistinguishable at a coarse-grained level are often both phenomenologically salient and artistically significant.

Of course, a classic problem for a coarse-grained conception of the content of thought as well as perception, such as Stalnaker’s, is the problem of equivalence: how someone can believe, for example, that two plus two is four but not that there are infinitely many prime numbers, given that both are necessarily true and hence necessarily equivalent. Stalnaker’s solution is to let the work be done by sentential attitudes: to characterize the different mathematical beliefs as beliefs about sentences (73–4). However plausible this approach may be in the case of mathematical belief, it is not plausible in the music-perceptual case, at least if we assume that Lerdahl and Jackendoff are correct when they claim that GTTM furnishes a correct mental description of the ordinary listener who knows no music theory. Such a listener can have none of the sentential attitudes, in this case attitudes toward GTTM’s structural descriptions, that would do the work here.

Hence, the level at which GTTM characterizes the content of an R-state is neither that of conceptual content, since analysis and hearing differ at that level, nor coarse-grained content, since non-correcting analyses and hearings, such as a triple-meter analysis and a duple-meter hearing, may well agree at that level. The level at which hearing is captured by analysis is located at, as it were, an intermediate level, that of a fine-grained structure that nonetheless admits of more than one route of cognitive access (DeBellis 1995, 52–3).

Let me turn at last to the issue of self-knowledge, specifically, that of the sort exemplified by I hear that passage as in duple meter. I claim that when we encounter an analysis that captures how we hear, and is illuminating, we have, or come to have, a certain reflective, second-order belief, which we express by a sentence such as “I hear that passage as in duple meter” (or perhaps one on its surface less tendentiously representationalist; again, a GTTM graph should be mentally substituted for “duple meter”). This belief, I take it, constitutes ascription to oneself of an R-state of a particular type. Let me call such a belief a second-order intuition. In having such a belief, or more properly in expressing it, one employs music-theoretical concepts (e.g., “duple”) to ascribe to oneself a cognitive state that, by the argument given above, does not itself contain those concepts as constituents. This is of course a variant on a familiar theme, which is that in belief ascription there need only be an approximate relation between the concepts employed by the ascriber and those employed by the subject to whom the belief is ascribed (Woodfield 1982). Many examples may be cited, varying in degree of conceptual remoteness: ascribing beliefs about arthritis to the medically misinformed, ascribing beliefs about elms to the botanically innocent, or ascribing beliefs to animals. But in music, there is a new twist: one makes such an ascription to oneself. In music, the analyst reconceptualizes (or, perhaps,
conceptualizes *simpliciter*) one of his own states, capturing its content via concepts not employed in it.

Perhaps the most salient and interesting issue here is epistemological. When I take myself to hear a passage as being in duple meter—to be in that R-state—what sort of self-knowledge is this? We might well wonder, of course, whether it is knowledge at all, especially in the case of our apparent access to our own past states of hearing, prior to doing the analysis. Now that my hearing has been clarified and articulated, I hear (and I can see that I hear) the piece as duple; but how do I know that this applies to my past, pre-analytical, possibly confused and obscure perception? In view of GTTM’s strong intuitive plausibility I am inclined to think that these are cases of knowledge, but it is fair to say that that this is a distinctive kind of self-knowledge that has not received the sort of close philosophical scrutiny that it deserves. What makes it distinctive is, in part, the fact that concepts that serve to specify the first-order content are not themselves constituents of that content. This sort of self-knowledge of intentional states has tended to be overlooked in the philosophical literature, I think. It is often claimed, for example, that the self-ascription of intentional states has an epistemically privileged status. But the claim is most plausible when the concepts used to specify the first-order state themselves belong to its conceptual content (for instance, if I ascribe to myself the belief *He’s a jaywalker* via *I believe that he’s a jaywalker*—where the concept *jaywalker* occurs in both). In the music-analytical cases, in which the specifying concepts do not belong to the state specified, how do we know how to make such a specification of our psychological states via concepts foreign to them, and how do we know when we’ve gotten it right? To say—as Jackendoff’s framework invites us to—that some translation between “languages of the mind” is going on here seems true enough. But how do we know how to make the translation? If we can recognize that analysis and hearing correspond in the relevant way, what faculty is it that compares utterances in the two “languages” and recognizes one to be a correct translation of the other? And what sort of knowledge is it that such a faculty draws on?²⁶

Let me suggest, without argument, that in the epistemological account a crucial role will be played by a mental transition, on the listener’s part, from

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²⁶ As always, parallel questions can be asked about knowledge of the perceived syntactic structure of sentences. But, as was pointed out earlier, theories in linguistics—in contrast to GTTM—do not place anything like comparable weight on a speaker-hearer’s judgments about the correctness of structural descriptions posed in the terms of the theory. Thus it is not clear where to look, in the case of language, for the richly detailed, articulate working-out of intuitions exhibited by GTTM in music. Linguistic theories seem to allow, to a much greater extent than GTTM, for the possibility that the syntactic structure a sentence appears to have is not the structure it actually has (where what we mean by the latter is the structure the best overall theory tells us it has). Linguistic theory does not assume that the appearances are everywhere a reliable guide to the reality of linguistic structure. As far as attribution of structure is concerned, GTTM distances itself from the appearances much less, if at all.
the hearing (R-state) to a corresponding first-order belief, That’s duple (which earlier we called a “matching belief”). The question is what to say about that transition. We can take the relationship to be merely causal, or we can take the transition to be an inference, whereby the relationship is logical as well, and the category of justification applies. If it is an inference, is it a justified inference? If it is justified, then it seems that the inference would contain an additional premise that would have to be among the things we know, a premise of the form: any passage of that kind (where the demonstrative picks out the perceptual mode of presentation under which the passage is presented in the R-state) is a duple-meter passage. Now, it may well be plausible that we know some premise of that form, but it is hard to see how that knowledge could be empirical. For it would not rest upon an enumeration of cases in which passages of that (perceptually presented) kind are invariably found to be duple-meter passages; nor is it clear how it could rest on empirical support in some less direct way, say through inference to the best explanation. What seems inescapable, then, is that we rely here on a certain kind of a priori knowledge about musical structure, knowledge that mediates between structure as perceptually presented and structure as represented via music-theoretical concepts. The inference from R-state to matching belief, and, in turn, the knowledge that one hears a passage in a certain theoretically specified way, rest upon this a priori knowledge.

Figure 7.

An analogy to mathematical knowledge seems to me to be illustrative and suggestive.27 Suppose you see a circle (Figure 7), and thereby have a visual experience of that shape. To characterize the shape represented in your experience, you might have recourse to a conceptual28 representation such as “set of points equidistant from its center,” or an equation such as “x^2 + y^2 = z^2.” This is parallel to the use of the concept of meter to characterize the metrical organization presented in one’s hearing of a passage. In both cases, the conceptual representation can convey new information to someone who has the perceptual experience being characterized: it can be informative to someone that the shape represented in his experience determines a certain equation.

27 The connection was suggested to me by Zlatan Damnjanovic and Robin Jeshion.
28 Here I use “conceptual” and “perceptual” as a convenient way of referring to the representations in question: the one that is perceptual, I call “perceptual,” and the other, which is not perceptual but is conceptual, I call “conceptual.” However, “perceptual” does not imply “not conceptual”: I do not imply that no concepts whatever enter into the perceptual representation.
just as it can be informative to someone that the metrical organization he
hears is a grouping into twos. At the same time, the relationship between
conceptual and perceptual representations is a priori: it is a priori that a
shape of the kind visually presented satisfies the equation, just as it is a pri-
ori that the structure presented in auditory perception is an organization into
twos or threes. (In both cases, we work out the equivalence via measur-
ment: by a coordinate system in geometry, by counting—ONE, two, ONE,
two—in music.) But the cases are not entirely equivalent either: the Carte-
sian coordinate system is arguably more remote from our subjective visual
experience, from the way things are presented to us, than are concepts of
meter and counting to musical experience. Though there is a conceptual in-
equivalence in both cases, the gap is greater in the Cartesian-coordinate case
(or so it seems to this musician).

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I want to conclude with some reflections about the role of concepts in music
perception, in particular, about whether hearings (R-states) have conceptual
content. What should we say is the relationship between a hearing and the
concepts expressed in a corresponding analysis? One possible view is that
the hearing does have a conceptual content, and that musical analysis ac-
complishes much the same thing as a classical conceptual analysis like
“Bachelor =df eligible unmarried male”: the musical analysis, on this view,
analyzes the concepts in the hearing in much the same way that “eligible
unmarried male” analyzes “bachelor.”

Figure 8. Diamond, or square rotated 45°?

But an alternative view would be that the hearing does not have a concep-
tual content—that what is being analyzed is some kind of nonconceptual
content.29 To flesh out this suggestion in one possible way, consider Figure
8, which can be seen either as a diamond or as a square rotated 45 degrees
(Peacocke 1992, 75). What is the difference between those visual experi-
ences? Peacocke suggests, following Palmer (1983), that the phenomol-

29 This may be suggested, perhaps, by Jackendoff’s terminological distinction between con-
ceptual structure and other kinds of representations, but I suspect that he and I do not draw the
conceptual/nonconceptual distinction in the same way.
ogical difference amounts to a difference in what symmetries are represented in one’s visual experience. (Peacocke notes here that, on the relevant notion of representation, representing such symmetries does not require the subject to possess the concept *symmetrical*.) Such symmetries are, for Peacocke, a level of nonconceptual content. It would be natural to entertain the possibility, then, that it is at this level that the musical structure treated in *GTTM* is mentally represented. But here’s the rub: listeners—or some of them, anyway—are supposed to have a certain kind of self-knowledge, knowledge of how they hear; and we will want to see our way clear to taking this to be an *inference* from perceptual states, to supposing that somehow the listener can *work out*, starting from hearing a passage as duple, *that he is hearing it as duple*. And that, it seems to me, forces the organization represented in the hearing into what John McDowell (1994) calls the realm of “spontaneity,” that of concepts. If one can carry out a translation from perceptually presented structure to (music-theoretic) concepts of structure, then the perceptual representation one is starting with must have a conceptual content rich enough to sustain that inference. For one is reasoning, and reasoning is always via concepts.

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To sum up, *GTTM* and the mental representations it postulates are a locus for many interesting philosophical questions. Despite the label of computationalism, *GTTM*’s notion of mental content is best understood not along computational lines, but instead in terms of the familiar, folk-psychological notion of perceptual belief. We are apt to recognize this once we appreciate the representational character of musical perception, and once we see that the theory’s evidential support presupposes a public language in which listeners can communicate about their perception. *GTTM*’s perceptual reports, and the intuitions behind them, are not conceptually equivalent to the hearings they report, although they attribute content at a fine-grained level sensitive to perceptual organization. The outstanding question is epistemological: what sort of knowledge is it that allows us to ascribe hearings to ourselves via music-theoretical concepts? The signature moment for the music theorist is

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30 Thus, the most we can say is that the representation of musical structure is *weakly* nonconceptual, in the sense given in DeBellis 1995, ch. 2—that the representation is indeed conceptual, though the concepts that enter into it are distinct from the music-theoretic concepts at issue. And here is where the analogy to Peacocke’s diamond-shape example breaks down. If we wanted to maintain the analogy, we would say that the perception of the diamond shape involves a conceptual representation (though one not containing the concept *symmetrical*) that serves as premise to the conclusion that one’s experience involves certain symmetries. But the important disanalogy between the examples is that Palmer’s theory does not stand or fall on its being intuitively plausible to a perceiver that her experience involves such symmetries.
when he recognizes that *that* perceptual organization, the one specified in the analysis, is present in his hearing of the piece. To understand what makes that recognition *knowledge* is the real epistemological challenge.\(^{31}\)

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1. What Kind of Theory is Music Theory?

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Studies in North-European Archaeology
Music theory has long aligned itself with the sciences – particularly with physics, mathematics, and experimental psychology – seeking to cloak itself in the mantle of their epistemological legitimacy. This affinity, which was foreshadowed in music’s inclusion in the medieval quadrivium alongside geometry, astronomy, and arithmetic, is evident throughout the history of music theory from the scientific revolution onward. Yet, as eager as music theorists have been to claim the epistemological privilege accorded the sciences, they have also been wary of limitations attending such an alliance.

This collection of essays will provide a historical as well as a philosophical and aesthetic context regarding music theory’s relationship to science. The thirteen authors work in a range of fields and traditions including music theory, musicology, and philosophy.